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"The EITC and the Extensive Margin: A Reappraisal."

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September 22, 2020
Via Zoom
Time: 2:00 – 3:50 p.m. EST
Week 5

SCHEDULE FOR FALL 2020 NYU TAX POLICY COLLOQUIUM

(All sessions meet online on Tuesdays, from 2:00 to 3:50 pm EST)

1. Tuesday, August 25 – Steven Dean, NYU Law School. "A Constitutional Moment in Cross-Border Taxation."

2. Tuesday, September 1 – Clinton Wallace, University of South Carolina School of Law. "Democratic Justice in Tax Policymaking."

3. Tuesday, September 8 – Natasha Sarin, University of Pennsylvania Law School. "Understanding the Revenue Potential of Tax Compliance Investments."

4. Tuesday, September 15 – Adam Kern, Princeton Politics Department and NYU Law School. "Illusions of Justice in International Taxation."

5. Tuesday, September 22 – Henrik Kleven, Princeton Economics Department. "The EITC and the Extensive Margin: A Reappraisal."

6. Tuesday, September 29 – Leandra Lederman, Indiana University Maurer School of Law. "Of Risks and Remedies: Best Practices in Tax Rulings Transparency."

7. Tuesday, October 6 – Daniel Shaviro, NYU Law School. "What Are Minimum Taxes, and Why Might One Favor or Disfavor Them?"

8. Tuesday, October 13 – Steve Rosenthal, Urban-Brookings Tax Policy Center. "Tax Implications of the Shifting Ownership of U.S. Stock."

9. Tuesday, October 20 – Michelle Layser, University of Illinois College of Law. "How Place-Based Tax Incentives Can Reduce Economic Inequality."

10. Tuesday, October 27 – Gabriel Zucman, University of California, Berkeley. "The Rise of Income and Wealth Inequality in America: Evidence from Distributional Macroeconomic Accounts."

11. Tuesday, November 10 – Owen Zidar, Princeton Economics Department. "The Tax Elasticity of Capital Gains and Revenue-Maximizing Rates."

12. Tuesday, November 17 – Abdoulaye Ndiaye, NYU Stern Business School. "Redistribution With Performance Pay."

13. Tuesday, November 24 – Lilian Faulhaber, Georgetown Law School. "Searching for Coherence: The Overuse of Excess Returns and Excess Profits."

14. Tuesday, December 1 – Erin Scharff, Arizona State Sandra Day O'Connor College of Law. "Revisiting Local Income Taxes."

The EITC and the Extensive Margin: A Reappraisal*

Henrik Kleven
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Abstract

This paper reconsiders the impact of the Earned Income Tax Credit (EITC) on labor supply at the extensive margin. I investigate every EITC reform at the state and federal level since the inception of the policy in 1975. Based on event studies comparing single women with and without children, or comparing single mothers with different numbers of children, I show that the only EITC reform associated with clear employment increases is the expansion enacted in 1993. The employment increases in the mid-late nineties are very large, but they are influenced by the confounding effects of welfare reform and a booming macroeconomy. Based on different approaches that exploit variation in these confounders across household type, space and time, I show that the employment effects align closely with exposure to welfare reform and the business cycle. Single mothers who were unaffected by welfare reform (but eligible for the EITC) did not respond. Overall and contrary to consensus, the case for sizable extensive margin effects of the EITC is fragile. I highlight the presence of informational frictions, widely documented in the literature, as a natural explanation for the absence of extensive margin responses.

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1 Introduction

Anti-poverty policy in the United States has changed fundamentally over the last four to five decades. One of the most profound changes has been the expansion of support to the working poor through the Earned Income Tax Credit (EITC) along with the downsizing of traditional cash welfare to the poorest segments of the population. This transition from welfare state to workfare state is illustrated in Figure 1, which plots the number of EITC recipients and the number of welfare recipients over time. The EITC has taken over as the main source of cash support and now dwarfs cash welfare by a factor of almost six. While this shift has distributional consequences, a key motivation has been to encourage labor supply at the extensive margin. Traditional welfare has long been blamed for keeping families out of the workforce (e.g. Murray 1984), while the EITC is supposed to draw them in. This paper revisits the extensive margin effects of the EITC.

The large literature on the EITC features a striking degree of consensus. Most authors agree that the program has had sizable extensive margin impacts, particularly on single mothers. Starting with important contributions by Eissa & Liebman (1996) and Meyer & Rosenbaum (2001), the literature has focused mostly on the federal EITC reforms in the 1980s and 1990s and relied on difference-in-differences approaches using variation by the presence and number of children. The most striking evidence brought forward in support of extensive margin responses is the sharp rise in the employment of single mothers, especially single mothers with two or more children, following the large 1993 EITC expansion for these family types (Hotz *et al.* 2006; Meyer 2010).

Given the importance of the historical change in welfare state design and the current proposals to further expand the EITC, it is critical that we have a correct understanding of its effects. The conclusions from the EITC literature are also central to a wider narrative regarding the impact of tax incentives on the extensive margin (see e.g., Blundell & MaCurdy 1999; Chetty *et al.* 2013a). In this paper, I do three things to shed new light on the EITC. First, I take a long-run perspective and consider every EITC reform since its inception in the mid-1970s, including all federal and state reforms. Second, I study all reforms in an event study framework, investigating the dynamics of labor supply changes before and after legislated reforms. Third, I carefully investigate the role of confounding factors, especially for the 1993 reform which underpins much of the consensus. I am far from the first to think about confounders in the 1990s, but I will try to offer some fresh

perspectives.

Like most of the existing literature, I use Current Population Survey (CPS) data and focus on the labor supply of single mothers.[1] For these women, the extensive margin incentives created by the EITC are unambiguously positive. I start from a long-run perspective, documenting the evolution in labor force participation of single women with and without children over the last 50 years. The observed patterns are striking. The participation rates for these two groups have evolved in parallel over this entire period, except for the mid-late 1990s. The participation rate for those with children was about 14 percentage points lower than for those without children in the late 1960s. This gap was about the same 25 years later, in the early 1990s. Then single mothers closed the entire gap in about five years, after which the two groups reverted to parallel trends. The dramatic increase for single mothers followed the 1993 EITC expansion, but the long-run series highlight that this period was an anomaly. If the EITC was important for the extensive margin in the 1990s, then why did it not narrow the gap in other time periods? There were federal EITC reforms in 1975, 1986, 1990 and 2009 along with numerous state EITC reforms between 1984-2018, which significantly increased the incentive to enter the labor market for single mothers.

Turning to event studies of each individual EITC reform further highlights how unusual the 1993 reform looks. Apart from this reform episode, event studies are essentially flat around the various EITC expansions at the federal and state levels. Any compelling narrative regarding the EITC and the extensive margin must reconcile the starkly different patterns around 1993 and elsewhere. One view is that the 1993 reform was different simply by virtue of being bigger and size matters in a world with optimization frictions (Chetty *et al.* 2011; Chetty 2012). A problem with this interpretation is that the 1993 reform was bigger only for those with two or more children, and yet employment also increased substantially for those with one child. Another view is that the anomalous patterns of the 1990s were driven, not by the EITC, but by confounding factors. The importance of such factors has been discussed in the literature (e.g., Ellwood 2000; Meyer & Rosenbaum 2001; Blank 2002; Grogger 2003; Fang & Keane 2004), with authors arguing that the EITC was in fact a major component of what happened in the 1990s even if it wasn't solely responsible.

What were the main confounders? First, there was the collapse of traditional cash welfare as illustrated by the caseload reductions in Figure 1. These caseload reductions were linked to welfare reform, both the federal welfare reform act of 1996 and the numerous state welfare reforms ("wel-

[1] While the existing literature has relied primarily on the March supplement of the CPS, I use linked March and monthly files. The larger dataset is very useful for precision in some of the more demanding event study specifications.

fare waivers") implemented between 1992-96. These policy changes put constraints on welfare receipt through aspects such as time limits, work requirements, training and job search activities. Because of the close link between welfare receipt and number of children, welfare reform produced extensive margin incentives correlated with the EITC. Second, the economy was booming at this time and the favorable business cycle may have had heterogeneous effects on single women with and without children. In particular, welfare reform and the business cycle are likely to interact: In a situation where single mothers are pushed off welfare, the labor market dynamics will depend on the tightness of the labor market at the time of the reform. I will argue that the dramatic labor market changes in the mid-late 1990s were driven primarily, if not exclusively, by welfare reform aided by the strong economy.

I conduct a number of exercises that highlight the importance of welfare reform over the EITC. I start by considering event studies around the 1993 reform by family size. The extensive margin effects are strictly increasing in the number of children: In the years following the 1993 reform, employment increased by about 10pp for single women with one child, 15pp for single women with two children, 20pp for single women with three children, and close to 30pp for those with four or more children. This pattern of heterogeneity is consistent with welfare reform due to the aforementioned link between AFDC benefits and the number of children.[2] Conversely, the patterns are not consistent with the EITC, which was increased sharply for all families with two or more children and only modestly for families with one child (relative to those without children). This would predict a sharp divergence in the effects between those with one and two children and little divergence elsewhere.

Is it possible that the fanning-out of effects by family size is driven by heterogeneity in extensive margin elasticities? To probe this question, I convert the effects into elasticities assuming that they are driven entirely by the EITC. This analysis produces elasticities that are enormous overall and sharply increasing in family size. The average elasticity across all single mothers equals 2, while the elasticity for those with, say, four or more children is about 5. Elasticities of such magnitudes are not credible, implying that any argument that all or most of the employment effects in the 1990s were driven by the EITC must be far off. To put it differently, if we assume sizable but *potentially* reasonable elasticities, then a very small fraction of what happened in the 1990s was driven by the EITC. This highlights the challenges of using the 1990s to learn about the effects of the EITC. We

[2] As I will show, AFDC participation levels prior to welfare reform and the subsequent AFDC participation drops were strongly increasing in the number of children.

are trying to pick out a potential EITC effect in a pattern that is mostly not about the EITC.

Besides the number of children, the age of the youngest child is a strong predictor of AFDC participation (see also Grogger 2002, 2003; Looney & Manoli 2016). The pre-reform level of AFDC participation and its ensuing fall are strongly declining in the age of the youngest child and close to zero for those with older children. As a result, the age of the youngest child can be used as a proxy for welfare treatment, allowing for a comparison of employment effects after the 1993 EITC expansion by intensity of welfare treatment. These employment effects are monotonically declining in the age of the youngest child. Among those with EITC eligible children above 9 years of age, the effects are virtually zero. Building on this idea, I also present an analysis that uses the age of the youngest child together with the number of children and other demographic variables to predict a probability of pre-reform AFDC participation, and consider employment effects by this measure of welfare treatment intensity.[3] There are no employment effects in the lowest deciles of predicted AFDC participation.

The final test focuses on the years 1994-1996, i.e. after EITC reform but before federal welfare reform. During these years, the main confounders are due to welfare waivers and the business cycle, both of which can be potentially controlled for using state-level variables. Augmenting the EITC event study specification with controls for the effect of state welfare waivers (allowed to vary by year and children) and controls for state-level unemployment (allowed to vary by children) renders the employment effects between 1994-1996 small and statistically insignificant. In other words, absorbing the variation that can be explained by waivers and unemployment changes a series that look like an event study of the 1993 EITC expansion into a series that look like an event study of the 1996 welfare reform. This holds across different samples and specifications.

To summarize, the different analyses I present suggest that the dramatic employment increases in the nineties were driven by welfare reform aided by a favorable business cycle. Of course, it would be difficult to prove conclusively that these increases were completely unrelated to the EITC, even if the empirical patterns do not point in that direction. The magnitude of the labor market changes in the 1990s may swamp any EITC-related pattern as I demonstrate through simple simulation exercises. This puts the onus on EITC reforms that are less confounded. When

[3]Because the analysis is based on EITC eligible children — defined as children living at home who are younger than 18 or between 18-23 and still in education — the number of children and the age of the youngest child are correlated (see also Looney & Manoli 2016). Having more children living at home implies that the last-born tends to be younger. As a result, the univariate analyses of heterogeneity by number of children and age of the youngest child partially overlap, whereas the multivariate analysis of heterogeneity by the probability of AFDC participation exploits the joint predictive power of the two.

investigating the long-run evolution of labor supply for single mothers and doing event studies of other EITC reforms, it is hard to conclude that the EITC has played any major role for the historic extensive margin changes in the U.S. In fact, when merging all of the EITC reforms at the state and federal level in a stacked event study, I obtain a precisely estimated zero.

How do we reconcile the observational findings presented here with the many randomized evaluations finding extensive margin effects of work incentives?[4] The bulk of these randomized evaluations have considered the impact of welfare reform treatments such as time limits, work requirements and various financial incentives, often bundled together. These are precisely the aspects that featured prominently in both TANF reform and state welfare waivers, and in fact many of the experiments were demonstration projects for statewide waivers. The findings of extensive margin effects in this literature (e.g. Kline & Tartari 2016) are consistent with the arguments in this paper. As for in-work transfers like the EITC, Card & Hyslop (2005) evaluate an earnings subsidy to welfare recipients in Canada — finding effects in the short run, but not in the long run — but this policy was structured quite differently from the EITC. More importantly, the EITC is different from experimental settings in terms of salience, information, administration and claiming. There is a considerable amount of evidence that most potential recipients are either unaware of the EITC or have a limited understanding of the schedule, eligibility and claiming (e.g., Bhargava & Manoli 2015). Given these aspects, the EITC is not an *a priori* likely candidate for finding large effects on labor supply at the intensive or extensive margins.[5] I come back to these issues below.

The rest of the paper is organized as follows. Section 2 reviews the related literature. Section 3 describes the policy context and the data. Section 4 provides descriptive evidence on the evolution of the extensive margin for single women over the last fifty years. Section 5 presents a series of exercises trying to understand the historic changes in the nineties. Section 6 presents a combined analysis of all the EITC expansion at the federal and state level. Section 7 concludes.

[4] See Grogger & Karoly (2005) for a comprehensive review of the early literature, and Card & Hyslop (2005), Kline & Tartari (2016) and Miller *et al.* (2018) for more recent experimental evaluations.

[5] While the informational and psychological frictions associated with the EITC are widely acknowledged in the literature, they have been used almost exclusively as an explanation for not observing any *intensive* margin responses. However, theoretical models of intensive and extensive margin responses (see Kleven & Kreiner 2006) imply that such frictions are equally important for the extensive margin.

2 Literature Review

An enormous body of work has examined the labor market impacts of the EITC and welfare reform. This section discusses a number of closely related papers, referring those interested in a more exhaustive summary to one of the many excellent reviews of the literature.[6]

Much of the EITC literature has focused on the extensive margin responses by single mothers, using Current Population Survey (CPS) data and variation from the federal EITC expansions in the 1980s and 1990s. These papers consider difference-in-differences approaches that rely on EITC variation by the presence and/or numbers of children. The early study by Eissa & Liebman (1996) finds sizable employment effects based on comparing single women with and without children before and after the 1986 EITC expansion. Meyer & Rosenbaum (2001) expand the time period to include the 1986, 1990 and 1993 tax reforms, and take a more structural approach based on modeling income taxes and welfare parameters from AFDC, Food Stamps and Medicaid. They estimate that tax changes (including the EITC) account for over 60 percent of the employment increase of single mothers between 1984-96. Hotz *et al.* (2006) use administrative panel data from California, covering families on welfare during the period 1991-2000. Their empirical strategy exploits the differential EITC expansion for families with two or more children relative to families with one child following the 1993 reform. They find sizable employment effects. Gelber & Mitchell (2011) confirm the qualitative findings from these earlier studies using PSID data from 1975-2004. Hoynes & Patel (2018) present event studies of the 1993 reform, comparing single women with zero, one and two or more children. Their focus is one poverty effects, but their online appendix shows employment effects as well. In a recent study of the 1975 introduction of the EITC, Bastian (2018) estimates large effects on maternal employment based on comparing women (single and married together) with and without children.

There are some exceptions to the consensus view described above. Cancian & Levinson (2005) examine Wisconsin's large EITC supplement to families with three or more children, and find no effects on labor supply at the extensive margin. In a paper analyzing the experience effects of employment, Looney & Manoli (2016) emphasize a point closely related to one of the analyses presented in this paper. They highlight that much of the variation in employment by family size (i.e., by number of minor children living at home) is really variation by the age of the youngest child, and that the age of the youngest child is strongly related to AFDC receipt prior to welfare

[6]The EITC literature has been reviewed by Hotz & Scholz (2003), Eissa & Hoynes (2006), Meyer (2010) and Nichols & Rothstein (2015), while the welfare reform literature has been reviewed by Blank (2002) and Grogger & Karoly (2005).

reform and to the drop in welfare receipt through the 1990s.[7]

A very different approach to estimating labor supply responses to the EITC has been proposed by Chetty *et al.* (2013b). Based on the idea that responding to the EITC requires knowledge of the EITC, they estimate behavioral responses by comparing zip-codes that vary by EITC knowledge. Their proxy for knowledge is based on bunching by self-employed individuals around the first kink of the EITC. They back out extensive margin responses by comparing event studies around the birth of the first child in high- and low-bunching areas. Women are more likely to continue working after child birth in high-bunching ("EITC") areas than in low-bunching ("no-EITC") areas. This approach yields a modest extensive margin elasticity of 0.2 using data from after year 2000. Since their proxy for knowledge is increasing over time, the elasticity obtained in this way should be smaller in the 1980s and 1990s.

In the experimental literature, there are relatively few examples of EITC-style policies. Card & Hyslop (2005) study a temporary earnings subsidy to welfare recipients in Canada. Similar to the EITC, the transfer was conditional on working. But the policy was otherwise different because it was time-limited (available for maximum of 3 years) and because eligibility required finding work within 12 months of random assignment, creating a very strong short-term incentive for working in order to obtain the option of any future transfers. Such a program creates intertemporal substitution incentives and is related to the Frisch elasticity (see Chetty *et al.* 2013a). Recently, Miller *et al.* (2018) provides evidence from the Paycheck Plus demonstration in New York City, an EITC-style policy for low-income workers without children. They find relatively modest employment effects.

The paper is also related to the literature on welfare reform, including many observational and experimental studies (see Blank 2002; Grogger & Karoly 2005). Because the federal government required experimental evaluations of the state waiver programs that preceded nationwide TANF reform, there is a substantial body of experimental work assessing programs that involve features like time limits, work requirements, welfare-to-work training, and financial incentives. In general, the literature has found significant extensive margin effects. For example, Grogger & Michalopoulos (2003) provide evidence on the impact of time limits using Florida's Family Transition Program. Leveraging the randomization of a bundle of policies (including time limits) combined with the insight that the impact of time limits should be declining in the age of the youngest child, they find

[7] Outside the EITC context, some papers show zero-responses in labor supply. A recent paper by Martinez *et al.* (2018) use a large and salient two-year income tax holiday in Switzerland to estimate Frisch elasticities along the intensive and extensive margins. They find a precisely estimated zero elasticity at the extensive margin, despite the fact that the Frisch elasticity is an upper bound on the Hicksian elasticity in standard life-cycle models (e.g. Chetty 2012).

large effects of time limits on welfare use. Their findings are broadly consistent with results using the age of the youngest child presented here. More recently, Kline & Tartari (2016) study the extensive margin impacts of Connecticut's Jobs First Program, a randomized waiver demonstration that included a TANF-style bundle of changes such as time limits, work requirements, family caps and earnings disregards (see also Bitler *et al.* 2006). They find substantial effects on women's labor supply at the extensive margin.

There are relatively few studies that directly compare the effects of EITC and welfare reform in the nineties. Ellwood (2000) argued that the independent effects of the EITC, welfare reform, and the strong economy are very difficult to separate due to their overlapping timing. While it is hard to disagree with this general sentiment, I will argue that a number of patterns in the data are strongly suggestive. Grogger (2003) estimates the separate effects of time limits, the remaining waiver/TANF bundle, and the EITC. Using a specification that attributes variation by family size to the EITC rather than to welfare, he argues that the EITC may have been the single most important explanation for the rise in employment among single mothers. As I argue, by putting constraints on a cash benefit that vary by family size, the treatment effect of waiver and TANF initiatives should be allowed to vary by family size, and this changes the evaluation of the comparative effects of EITC and welfare reform. Finally, Fang & Keane (2004) provide a detailed study of all the different policy parameters and macroeconomic variables that may have influenced the behavior of single mothers. Their estimates imply that the rise in employment between 1993-2002 can be explained in roughly equal proportions by the EITC, welfare reform (work requirements and time limits, respectively), and the macro economy.

Of crucial importance to interpretation is the literature on EITC knowledge. In their study of extensive margin responses to the 1986 EITC expansion, Eissa & Liebman (1996) cite evidence from interviews conducted in 1993 showing "virtually no awareness" of the credit among potential recipients (Eissa & Liebman 1993; Olson & Davis 1994). A number of subsequent studies have documented the presence of substantial frictions related to the awareness, understanding, and claiming of the EITC (Romich & Weisner 2000; Smeeding *et al.* 2000; Ross Phillips 2001; Berube *et al.* 2002; Maag 2005; Kopczuk & Pop-Eleches 2007; Jones 2010; Chetty & Saez 2013; Mead 2014; Bhargava & Manoli 2015). For example, Maag (2005) reports that only 58% of low-income families "had heard about the EITC" in a nationally representative sample from 2002. What is more, the understanding of how the schedule is designed is even weaker. Among the families interviewed by Romich & Weisner (2000), most people had heard of the EITC, but virtually no one knew that

they needed to earn a certain amount to maximize the credit. Chetty *et al.* (2013b) show that their proxy for knowledge — the degree of bunching by the self-employed at the first EITC kink — has been increasing over time and was relatively limited in the mid-nineties.[8]

While these informational and psychological frictions are widely acknowledged, they have been used mostly to explain the absence of *intensive* margin responses. The presumption is, it appears, that extensive margin responses can be based solely on knowing about the existence of a tax refund, while intensive margin responses require detailed understanding of the schedule. This view is not consistent with economic theory, nor with basic introspection. Theoretical models predict that the extensive margin decision depends on taxes and transfers at the desired level of earnings, i.e. the intensive and extensive margin decisions are inter-dependent (see e.g., Kleven & Kreiner 2006; Eissa *et al.* 2006, 2008). This inter-dependence is particularly strong for nonlinear incentives like the EITC. In a model with fixed costs of working (due to for example child care), the EITC is an incentive to start working around the refund-maximizing earnings range (for the relevant family size), not in any earnings range. Absent information about where the relevant earnings range is located and the size of the credit in that range, the extensive margin response is not credible.

3 Institutional Background and Data

3.1 The Earned Income Tax Credit

Since its inception in 1975, the Earned Income Tax Credit (EITC) has grown to become the largest cash transfer program in the United States in terms of the number of recipients and revenue costs. The EITC is a refundable tax credit and eligibility depends on having earned income, creating a positive incentive at the extensive margin. As shown in Panel A of Figure 2, the credit amount is a function of household earnings and the number of qualifying dependents (children).[9] For each family size, the credit schedule features a phase-in range, a refund-maximizing plateau range, and a phase-out range. The generosity of the schedule is increasing in family size, with the largest possible credit for families with three or more children. As of 2018, their maximum annual credit

[8] From outside the economics literature, Mead (2014) argues that the EITC was not responsible for moving single mothers into work in the 1990s. Besides the survey evidence on EITC knowledge, his argument is that the academic consensus is inconsistent with the experiences of welfare officials and administrators "on the ground." Based on interviews with welfare officials in Wisconsin and New York, he concludes that those dealing directly with welfare recipients did not think the EITC played any role in the initial decision by welfare mothers to start working.

[9] An EITC qualifying dependent is a relative who is under the age of 19 (24 for full-time students) or permanently disabled, and reside with the tax filer for at least half the year.

equals about $6,400 and is reached at annual earnings in the range of $14,300-18,700.

The parameters of the EITC have been revised substantially over time. Table A.I in the online appendix documents the full set of EITC parameters for each family size and each year between 1975-2018.[10] To simplify, Panel B of Figure 2 focuses on the evolution of just one key parameter — the maximum possible credit — across different family sizes. The credit amount in each year is reported in 2018 US Dollars. As shown in the figure, there have been five federal reforms of the EITC: the introduction in 1975 and the expansions enacted in 1986, 1990, 1993 and 2009.[11] The largest changes are the 1975 EITC introduction for all families with children and the 1993 EITC expansions for families with two or more children. The figure highlights that the most natural research design for studying the impacts of the EITC is to compare different family sizes around the reform episodes. For the 1993 reform in particular, it is natural to compare those with two or more children to those with one child or without any children. This comparison comes with potential placebo checks, because there is relatively little variation between those with one vs zero children and no variation between those with three or more vs two children.

In addition to the federal EITC described above, many states have introduced their own EITC supplements. A total of thirty states instituted EITC supplements between 1984-2018. The details of these state programs are described in Table A.II in the appendix. With the exception of California and Minnesota, state EITCs are specified as a percentage of the federal EITC. In some states the supplement is small, while in others it is quite large. The majority of state EITCs are refundable, just like the federal credit, but some of them are nonrefundable. Overall, when accounting for both the state and federal EITC, there has been a very substantial and ongoing expansion of support to the working poor through the last four decades. I will explore the potential impacts of all of these EITC expansions in this paper.

3.2 Welfare Reform

It would be impossible to assess the EITC without considering the potentially confounding effects from the rest of the welfare system. In particular, the EITC expansion enacted in 1993 and implemented between 1994-96 — the biggest expansion in the history of the program — coincided with dramatic changes to traditional cash welfare. The Personal Responsibility and Work Oppor-

[10] The set of parameters includes the phase-in rate, the maximum credit, the phase-out rate, and the location of the kink points.

[11] These EITC changes were legislated as part of the Tax Reduction Act of 1975, the Tax Reform Act of 1986 (TRA86), the Omnibus Budget Reconciliation Acts of 1990 and 1993 (OBRA90 and OBRA93), and the American Recovery and Reinvestment Act of 2009 (ARRA).

tunity Reconciliation Act of 1996 (PRWORA) implemented welfare reform at the national level, replacing Aid to Families with Dependent Children (AFDC) with the more restrictive Temporary Assistance for Needy Families (TANF). However, this welfare reform did not represent a sudden and unanticipated departure from past policy. It was the culmination of state-led welfare reform efforts starting in the late 1980s and accelerating through the first part of the 1990s. These state reforms were implemented under the heading of *welfare waivers*, federal approvals for states to change their welfare programs. While waiver-based reforms had been possible since the early 1960s, they became very popular in the 1990s and set the stage for national reform. I will highlight the key features of state and federal welfare reform below, but refer to Grogger & Karoly (2005) for an exhaustive description of the history and details of these policy changes.

The state waivers involved all the key elements that would later be implemented on a national scale through TANF reform, including time limits, work requirements, and financial incentives to work. Some waivers were pilot programs that affected only a few counties or a subset of welfare recipients, while other waivers were statewide welfare reforms. Between 1987-92, a total of thirty waivers were approved by the federal government, although none of these were statewide reforms. Between 1992-96, under the first term of the Clinton administration, another eighty-three waivers were approved and many of these were statewide reforms. In total, thirty-five states received approval for statewide legislation through waivers. Table A.III in the appendix documents the approval and implementation dates of all statewide waivers.

There were six main types of waiver policies (see e.g., Council of Economic Advisors 1997, 1999; Department of Health and Human Services 1999). *Termination time limits* introduced upper bounds on the length of time that a family or its adult members could receive welfare benefits, even if they were otherwise eligible. These time limits varied from state to state, but a number of places set their limit to 24 months. *Work requirement time limits* imposed mandatory work requirements that kicked in when recipients reached a certain time limit.[12] Furthermore, building on the Job Opportunities and Basic Skills Training Program (JOBS) instituted in 1988, JOBS waivers strengthened the rules regarding participation in education, training, and job search activities. There were two such waivers. *JOBS exemptions* allowed states to eliminate or reduce the exemption of families with young children from JOBS. While previously parents with children under the age of 3 (or under the age of 6 if the state could not guarantee child care) had been exempt from JOBS,

[12]Henceforth, I refer to termination time limits simply an "time limits" and to work requirement time limits as "work requirements".

these waivers required parents with very young children (sometimes as young as 12 weeks) to participate in the program. *JOBS sanctions* allowed states to impose harsher sanctions for failure to comply with JOBS requirements. Many states introduced the possibility of suspending the entire family's AFDC grant after a period of non-compliance. Motivated by a perception that welfare benefits were contributing to single motherhood and out-of-wedlock births, *family caps* were used to eliminate or reduce the benefit increase for existing AFDC recipients who had additional children. Finally, *earnings disregards* provided stronger financial incentives to work by disregarding earnings up to a level (such as the federal poverty line) in the calculation of benefit claw back.

The era of waiver-based reform culminated in national reform through PRWORA, signed into law in August 1996. This reform consolidated the AFDC and JOBS programs into the TANF program, which included all the key waiver elements just described. States had considerable latitude in designing their own TANF program under some federal guidelines. For example, it was a federal requirement that states impose a time limit of no more than 60 months, but they were free to choose stricter limits. States that had implemented time limits through waivers could keep the same limit (and many states did) or adjust it up or down. States without any time limit had to introduce one. As a result, TANF extended the waiver-type elements that had already been implemented in many states to the remaining states. There was, and still is, considerable variation in the specific program parameters across states.[13]

Most of the policy changes implemented under waivers and PRWORA did not directly reduce statutory benefit levels, but they imposed much harsher constraints on receiving those benefits. Table A.IV in the appendix shows the maximum monthly benefit by state and number of children in 1993. The benefit is strictly increasing in the number of children in every state. By itself, this implies that the treatment effect of welfare reform is increasing in the number of children. For example, the cost of introducing a time limit on benefit receipt will be greater in a large family than in a small family due to their different benefit levels. Regardless of benefit levels, there is another more fundamental reason why the treatment effect of welfare reform is increasing in the number of children. In a world with fixed costs of work per child (e.g. due to child care), single mothers with more children are less likely to work are more likely to be on welfare, all else equal. Hence, their welfare participation rates will be higher at baseline (a prediction borne out in the data), making them more treated by welfare reform. This is important to keep in mind when interpreting the empirical evidence by family size.

[13] See Grogger & Karoly (2005) for a detailed documentation of TANF parameters in each state.

3.3 Data

The analysis is based on the Current Population Survey (CPS). I combine data from the basic monthly files and from the Annual Social and Economic Supplement (ASEC), or "March files". Using the monthly and March files together give much more data than using the March files alone, which is what previous papers in the literature have done.[14] I restrict the dataset to include the monthly files from 1989-2018 and the March files from 1968-2017. Even though the monthly files go back to 1976, they do not allow for accurately identifying the presence or number of children prior to 1989. I focus on the sample of single women aged 20-50.[15] These restrictions result in a sample of 4,674,064 individual-month observations across survey years 1968-2018. Appendix B provides a detailed description of the CPS data, including the link between monthly and March files, variable definitions and measurement.

The CPS allows for different ways of measuring labor supply at the extensive margin. One distinction is between employment and labor force participation, where the latter includes unemployed people who are actively searching for work or only temporarily laid off. While employment is the more relevant outcome for welfare calculations, participation has the advantage of not moving with transitions between employment and unemployment and is therefore less sensitive to the business cycle. The other distinction is between annual and weekly measures of the extensive margin. Annual measures are preferred in principle due to fact the EITC is based on annual earnings.[16] However, focusing on weekly measures offer three key advantages. First, the weekly measures are available in both the monthly and the March files, whereas the annual measures are available only in the March files. Hence, the weekly measures give much more statistical power. Second, questions about work activities during the previous week likely involve less measurement error than questions about work or earnings during the previous year. Third, the weekly measures largely avoid issues with nonresponses in the CPS. There are very few nonresponses in the weekly labor market and demographic variables, whereas there is significant nonresponse in the annual income variables in the March files. This issue has become more severe over time. As a result, a significant fraction of earnings observations in the CPS are imputed based on a procedure described in appendix section B. I refer to Bollinger *et al.* (2017) for a detailed analysis of this issue.

[14] The main advantage of the March supplement is that it contains detailed information on annual income variables during the previous year. I use the March files alone for analyses that requires annual income information.

[15] The definition of "single" includes never married, separated, divorced, and widowed.

[16] On the other hand, the AFDC/TANF program — also very central for the analysis in this paper — is administered at the monthly level and thus depends on monthly earnings.

Figure A.I in the online appendix shows the long-run evolution of all four extensive margin measures. There are level differences between these measures — annual measures are higher than weekly measures and participation is higher than employment — but the time trends are very similar. The employment series fluctuate more than the participation series due to the business cycle effects mentioned above. While this would be an argument for focusing on the participation outcome, and I do start out by considering this outcome in the next section, the main analyses that follow consider employment as the baseline outcome. This is consistent with the focus of the existing literature. I will show results for weekly employment in the baseline specifications, but present results for the three alternative measures in the appendix. None of the main results I present vary fundamentally with the choice of extensive margin measure, but the annual-level analyses are more noisy as they rely exclusively on the March files.

Table 1 provides descriptive statistics in the estimation sample of single women aged 20-50, pooled across all years. Single mothers are somewhat older, have less education, and employment/participation than single women without children. These level differences are unsurprising and, of course, by themselves do not invalidate the difference-in-differences analyses presented here. Figure A.II in the appendix shows the earnings distributions of single mothers, split between low education (high school and below) and high education (some college and above). Earnings are normalized using the first kink point of the EITC in each year (so that the kink is located at zero). While the distribution of the highly-educated is shifted to the right, the graph shows that, in the sample of single mothers, both those with low and high education tend to have earnings within the EITC eligible range. I consider the full sample of single mothers as a baseline, but present results for low-educated single mothers as a robustness check.

4 EITC and the Extensive Margin: The Long View

It is useful to start from a long-run perspective, describing the extensive margin changes for single women over the last fifty years. Figure 3 shows the labor force participation rates of single women with children (blue series) and single women without children (black series) between 1968-2018. Given children is an eligibility requirement for the EITC, we can think of these series as treatment and control groups for assessing the effects of the program.[17] The patterns are very striking. Over half a century, all of the action in the participation rate of single mothers relative to that of single

[17] To clarify, childless families could not receive any EITC until 1993 and only a very modest credit thereafter (see Figure 2).

childless women took place during a single spell in the mid-late 1990s. Outside this spell, the two groups have evolved in parallel. At the beginning of the period, in 1968, the gap in labor force participation between the two groups was equal to 14.3 percentage points. A quarter of a century later, in the early 1990s, the gap was about the same. Then the labor force participation of single mothers rose dramatically in the mid-late 1990s, closing the entire 14 percentage gap in just a few years. After this, the two groups went back to parallel trends — but now at the same levels — and have stayed that way to this day. The graph highlights just how extraordinary the late 1990s were in the history of the U.S. labor market.

What explains this long-run evolution? To think about the role of the EITC and other factors, Figure 4 compares the long-run series to the timing of the five federal EITC reforms (Panel A) and the confounders from welfare reform and the macroeconomy (Panel B). Starting with Panel A, it shows that the EITCs claim to sizable extensive margin effects relies on two concurrent events: the EITC expansion enacted in 1993 (which increased tax refunds between 1994-1996) and the historic increase in the participation rate of single mothers in the mid-late 1990s. At the same time, the figure brings out a major puzzle. If EITC expansion drove the extensive margin increases in the late 1990s, then why do we not see any such effect around the other reforms? The three federal EITC reforms prior to 1993 did not lead to any closing of the gap between single women with and without children.[18] In addition, thirty states introduced EITC supplements between 1984-2018, adding to the puzzle of why there is no action outside the critical period in the 1990s.

Any compelling EITC narrative must reconcile the starkly different patterns in the 1990s and elsewhere. Did the EITC expansion in the mid-1990s have a different impact, because it was bigger and perhaps better advertised than the other reforms? Or were the changes in the 1990s driven by other confounding factors, or by an interaction between the EITC and these other factors? Panel B highlights the two main confounders in the 1990s. First, there was welfare reform: the period of waiver-based reform between 1992-96, culminating in national reform in 1996 (see section 3.2 for details). Second, there was the macroeconomy: the economy was booming during the Clinton era, with the national unemployment rate (dashed series) falling from about eight to four percent between 1992-2000.

Why would the macroeconomy impact single women with and without children differently? The figure shows that, outside the 1990s, there is no strong correlation between unemployment

[18]The EITC expansion in 2009 did not lead to any visible effect either, but because this expansion applied only to families with three or more children, its effect may be missed when considering all single mothers. I present long-run series by different family sizes below.

fluctuations and the gap in labor force participation between the two groups. However, this is not inconsistent with the business cycle having an effect in the 1990s, because of the interaction with the policy shocks happening at that time. Regardless of which policy is primarily responsible for the effects (EITC or welfare reform), the labor market dynamics around the time of the reforms will depend on the tightness of the labor market. For example, when single mothers get pushed off welfare, their ability to find work will depend on the availability of jobs at the time. Hence, the short-term effects of welfare reform will almost surely interact with the business cycle. Moreover, in the presence of employment hysteresis (see e.g., Yagan 2019), the fact that welfare reform happened in a strong economy may play a role even for the longer-term effects.

The long-run series discussed above are based on a specific measure of the extensive margin — weekly labor force participation — and it is worth checking if the empirical patterns are different for alternative measures of the extensive margin. Figure A.III in the online appendix compares all four extensive margin measures available in the CPS data: participation and employment at either the weekly or annual level.[19] The difference between participation and employment is that the former includes unemployed people who are actively searching for work or temporarily laid off. The figure shows that the main insights are robust to the choice of extensive margin measure. If anything, the employment gaps show even less sign of shrinking in the 25 years leading up to the reforms in the 1990s. It is also worth noting that, in general, the employment series are bumpier than the participation series. This is because employment is more sensitive to the business cycle as it is directly affected by movements between employment and unemployment. Figure A.IV shows the evolution of the same four outcomes, but restricted to the sample of those with low education (high school or less). This is motivated by the idea that low-educated single mothers tend to have lower earnings and are therefore more treated by the EITC. The broad patterns are again the same, although there is some shrinking in the participation gaps — but not in the employment gaps — in the decades leading up to the 1990s.

Further insight can be gained by splitting the sample of single mothers into different family sizes. Figure 5 shows the long-run evolution in labor force participation for single women with zero, one, two, and three or more children. The broad pattern is the same as before: the different groups have trended similarly over half a century, except for the mid-late 1990s.[20] However, while

[19] See section 3.3 for details. The annual participation series is shorter than the others, because this measure was not recorded in CPS data until the 1976 March files (i.e., pertaining to calendar year 1975).

[20] These series are somewhat noisier than the aggregate series. This is partly because the long-run graphs — but not the analysis of the 1990s in the next section — are based on the March files alone rather than the linked March and monthly files. As mentioned in section 3.3 and described in the data appendix B, the monthly files are used only for

all groups of single mothers increase their labor force participation in the 1990s, the magnitude is strongly increasing in family size. Consider those with three or more children: their participation rate increases by a staggering 23 percentage points over six years, about twice as much as for those with two children. This difference is puzzling under the EITC narrative, because the tax credit expansion was the same for those with two and three children. It is of course possible that larger families have larger extensive margin elasticities and therefore respond more strongly to the same incentive, but this line of reasoning leads to other puzzles: if single women with three or more children are much more elastic, then why do they not increase participation after the 2009 EITC expansion (targeted specifically to them) or after the 1986 and 1990 expansions? Their baseline level was higher in 2009 and there was the Great Recession — both of which might explain the lack of a response — but such arguments do not apply to the 1986 or 1990 reforms. Overall, the long-run evidence suggests that whatever happened to large female-headed families in the late 1990s was something unique to that time period.

Another interesting aspect of the 1990s is that participation rose substantially even for those with just one child. This runs counter to the argument that the EITC had larger effects in the 1990s simply because the 1993 expansion was much larger than the other expansions. While this was true for families with two or more children, it was not true for families with one child. As shown in Figure 2, the increase in the maximum refund for one-child families was not exceptionally large on its own, and it was actually quite modest relative to the concurrent increase for childless families. As a result, the sizable extensive margin increases for single women with one child relative to those without children during the 1990s provide *prima facie* evidence of confounders.

Figures A.V-A.VI in the appendix investigate the robustness of these patterns to the alternative extensive margin measures and to focusing on the low-educated. For each outcome and sample, the stylized pattern is the same: what happened in the nineties was unique to this time period, it happened across all groups of single mothers, and it was monotonically increasing in the number of children. This points to a treatment that was both unique to the 1990s and strictly increasing in family size. Welfare reform satisfies both of these, whereas the EITC satisfies neither. To dig deeper, the next section moves from the long-run descriptive perspective to a detailed econometric analysis of the 1990s.[21]

post-1989 analyses due to problems with accurately identifying the presence and number children in the CPS monthly files before that time.

[21] Although my main focus is on single mothers, I consider married women in Appendix Figure A.VII. This figure shows labor force participation rates of married women between 1968-2018, considering comparison groups based either on the presence of children or on spousal earnings (using that the EITC is based on family income). The patterns

5 EITC and the Extensive Margin: Making Sense of the 1990s

This section investigates the changes in the 1990s from different angles that will shed light on the role of the EITC vs other factors. The 1990s can be analyzed using the linked March and monthly CPS files, which give more data and precision than when using the March files alone.[22] In particular, the larger dataset allows for flexible event study specifications and granular heterogeneity analyses without loosing too much precision.

5.1 Event Studies: 1993 Reform vs Other Reforms

I start by putting the data into an event study framework, comparing the 1993 EITC expansion to the other federal EITC reforms. This exercise is not very different from the descriptive evidence presented above — comparing those with and without children over time — but a formal event study approach has several advantages. It allows me to present graphs that highlight the dynamics around each reform more clearly, and it allows me to control for changing demographics and to provide standard errors. Consider the following specification:

$$P_{imt} = \sum_j \alpha_j \cdot Year_{j=t} + \beta \cdot Kids_i + \sum_{j \neq -1} \gamma_j \cdot Year_{j=t} \cdot Kids_i + \boldsymbol{X_i}\boldsymbol{\phi} + \nu_{imt}, \quad (1)$$

where the outcome P_{imt} is an indicator for individual i working or participating in month m of year t. The right-hand side includes dummies for each year, a dummy for having kids, the interaction between year and kids dummies, and a vector of demographic controls $\boldsymbol{X_i}$. The interaction term omits the year just before the reform (denoted by $j = -1$), so that the difference-in-differences coefficient γ_t can be interpreted as the extensive margin effect in year t relative to the pre-reform year. As a baseline, I consider effects on weekly employment without any demographic controls, and then investigate the robustness of the results to alternative extensive margin measures and to controlling for demographics.

The first set of results is presented in Figure 6. This shows event studies around the 1975 EITC introduction (Panel A), the 1986 and 1990 EITC expansions (Panel B), the 1993 EITC expansion (Panel C), and the 2009 EITC expansion for families with three or more children (Panel D). These

do not point to any clear extensive margin responses for married women (even in the 1990s).

[22] As discussed in section 3.3 and in the data appendix, the recording of children in the monthly files is accurate (and consistent with the March files) only from 1989 onwards. As a result, all analyses using data prior to 1989 are based on the March files alone, while those using data only from after 1989 are based on the linked March-monthly files.

are run separately and each graph shows the estimated DiD coefficients $\hat{\gamma}_t$ over time. While Panels A-C compares single mothers with and without children, Panel D compares single mothers with three or more children to those without children.[23]

The figure highlights just how exceptional the patterns around the 1993 reform are. Taken at face value, the 1993 event study looks very compelling: the trends are roughly parallel in the years before the reform, the treatment group starts increasing relative to the control group after the reform, and a large and statistically significant DiD effect builds up over time. No other reform is associated with such patterns. The event studies are essentially flat around the 1975 introduction (which was large) and around the 1986 and 1990 expansions. The event study around the 2009 expansion shows a sizable negative effect, which is presumably due to the confounding effects of the Great Recession in 2008-09. The negative DiD effects obtained during this macroeconomic downturn highlights the problems with interpreting the positive DiD effects during the upturn in the 1990s.

Figure A.VIII in the appendix repeats this analysis with demographic controls to see if the patterns are driven partly by compositional changes in the group of single mothers over time. Specifically, the controls include dummies for the age of the woman (six categories), the age of the youngest child (seven categories), and education level (three categories).[24] The resulting patterns are very similar to those based on raw means. There is one exception worth highlighting, however. Controlling for demographics in the 1993 event study strengthens a feature that was also present (but subtle) in the baseline graph, namely that the inflection point in the DiD series happens a year too early, in 1992. The year 1992 coincides with the turn of the business cycle and the beginning of statewide welfare waivers (as shown in Figure 4), suggesting that the simple DiD estimates around 1993 may substantially overstate the effects of the EITC.

Figure A.IX and A.X in the appendix reproduce the analysis with demographic controls for the alternative extensive margin outcomes: weekly participation and annual employment. The patterns are overall similar when considering these other outcomes, but annual employment is noisier as it is based on the March files alone. The main difference relates to the DiD series of annual employment around the 1986 and 1990 reforms. This series shows an increase between 1987

[23] That is, Panel D is based on an extension of equation (1) that includes separate dummies for 1, 2, and 3+ children, plotting the DiD coefficients on single mothers with 3+ children. Changing the control group to those with one or two children (or their combination) does not affect the results qualitatively.

[24] The binning of these dummy controls is as follows: age of woman (20-24, 25-29, 30-34, 35-39, 40-44, 45-50), age of youngest child (0-1, 2-3, 4-6, 7-9, 10-13, 14-17, 18+), and education (below high school, high school degree, some college and above). Unless otherwise specified, the same binning is used in subsequent analyses.

and 1989, consistent with finding positive effects when comparing years before the 1986 reform to years in the late 1980s as in Eissa & Liebman (1996).[25] While this could be an effect of the EITC, the evidence is far from conclusive. Not only does the increase start a year too late (which might be explained by frictions), but most of the effect goes away between 1990 and 1991, i.e. after the 1990 reform had enacted further EITC expansions. More broadly, and as discussed in the previous section, there is no narrowing of the annual employment gap between single women with and without children over the 25 years leading up to the 1993 reform, despite the substantial EITC increases over this period.[26] The employment series is quite bumpy from year to year (due to business cycles and other time-varying factors), but there is no longer-run convergence.

Table 2 presents DiD estimates for all five federal EITC reforms across different specifications. The table shows results for employment and labor force participation (weekly measures), for all single mothers and low-educated single mothers (high school and below), and for regressions with and without demographic controls. The estimates represent the average effect over the first three years after each reform. The three-year window is chosen because it avoids overlap between the 1990 and 1993 EITC reforms, and between the 1993 EITC reform and PRWORA. The results are very robust and consistent with the interpretations above. Apart from the 1993 reform, all estimates are either small and statistically insignificant or they are negative. By contrast, the 1993 reform is associated with large and strongly significant DiD estimates that hardly vary across specifications. The estimates are virtually the same for the employment and participation outcomes, and for specifications with and without demographic controls. The estimates are larger in the low-educated sample than in the full sample, but the difference is small: a positive effect of roughly 3.5pp for low-educated single mothers versus roughly 3pp for all single mothers.

To conclude, the empirical patterns that follow the 1993 EITC expansion represent a historic anomaly independently of the outcome, sample, and specification. A natural interpretation, therefore, is that these patterns are driven by confounders that are unique to the 1990s: welfare reforms at the state and federal level, perhaps interacted with the sharp upturn in the US macroeconomy. The following sections investigate these confounders from a variety of angles.

[25] The main specifications in Eissa & Liebman (1996) are based on comparing average annual employment between 1984-86 and 1988-90.

[26] See Panel D of Figure A.III.

5.2 1993 Reform: Heterogeneity by Family Size

This section presents event studies by family size. The long-run descriptive analysis included this dimension of heterogeneity, but using the linked March and monthly files allows me to produce cleaner and more granular evidence for the 1993 reform. The results are based on an extension of equation (1) in which the dummy for having any kids is replaced by separate dummies for having one, two, three, or four or more kids. The graphs presented below plot DiD coefficients γ_t^n for each year t and each number of children n. These coefficients capture the effect in each year relative to the pre-reform year, and for each family size relative to single women without children.

Figure 7 shows results from specifications without demographic controls (Panel A) and with demographic controls (Panel B). In both panels, there is a clear fanning-out of employment effects by number of children. Consider first the raw patterns in Panel A. In the decade following the 1993 reform, the employment rate increased by about 10pp for single women with one child, 15pp for single women with two children, 20pp for single women with three children, and close to 30pp for those with four or more children. Because the baseline level of employment is declining in family size, the fanning-out is even more dramatic in percentage terms. For example, single mothers with four or more children almost doubled their employment rate, from about 30% to about 60%. Panel B of the figure shows that some of the fanning-out can be explained by changes in the demographic composition of single mothers. Including detailed demographic controls, reduces the effect for each group, but by less for small families than for large families. The fanning-out is still strong, however, with increases in the employment rate ranging from about 8pp to 25pp across the four family sizes.[27,28]

These patterns of heterogeneity are puzzling under the hypothesis that they are driven by the EITC. As described in section 3.1, the 1993 EITC expansion was modest for families with one child (relative to those without children), while it was very large for families with two or more children. By itself, this implies that the divergence in the DiD series should occur primarily between one and two children. The DiD series for one child should be relatively flat, while the series for three and four plus children should feature little additional divergence. Conversely, the observed

[27] Note that, while demographic controls reduce the fanning-out by number of children, they had virtually no impact on the average employment effects discussed in the previous section. This is because demographic controls matter mostly for single women with three or more children, who represent a small fraction of the sample: about 80% of single mothers have one or two children, about 13% have three children, and the remaining 7% have four or more children. These numbers have remained fairly constant for a long time.

[28] Figure A.XI in the appendix repeats the analysis for labor force participation. If anything, the fanning-out by number of children is even stronger in the participation outcome than in the employment outcome.

heterogeneity is natural if the effects are driven by welfare reform. As described in section 3.2, AFDC benefits were increasing in family size — each child triggered additional benefits — and pre-reform caseloads were strongly increasing in the number of children.[29] Hence, the treatment effect of welfare reform — from both waivers and PRWORA — is strictly increasing in family size.

It is useful to zoom in on the years 1994-96, before national welfare reform. It is potentially easier to isolate the impact of the EITC during these years, although welfare waivers and the business cycle still pose a threat to identification. If anything, the evidence is even more puzzling during these years, because single mothers with two children do not begin increasing employment relative to those with one child until later.[30] In other words, in the years prior to PRWORA, there is divergence between each family size except where we should see it. This provides *prima facie* evidence that waivers and the business cycle by themselves were sufficient to swamp any EITC-related pattern.

5.3 1993 Reform: Elasticities and Simulated Responses

To better understand the magnitude of the extensive margin changes in the 1990s, I convert the estimated DiD effects into extensive margin elasticities. The elasticities will be calculated under the (extreme) assumption that the effects are driven solely by the EITC. This exercise will clarify if it is conceivable that the observed effects, including their heterogeneity across family sizes, are driven exclusively or primarily by the EITC.

The extensive margin elasticity with respect to the net-of-tax rate on labor force participation is defined as follows

$$\varepsilon \equiv \frac{\Delta P / P}{\Delta \left(1-\tau\right) / \left(1-\tau\right)}, \tag{2}$$

where P is the probability of employment or participation and τ is the participation tax rate, i.e. the average tax rate on earnings when accounting for taxes paid and benefits lost upon entry into the labor market. This way of defining the elasticity corresponds to the literature on optimal taxation and welfare measurement (Saez 2002; Kleven & Kreiner 2005; Eissa *et al.* 2006, 2008). Assuming that the 1993 event study effects are driven by the EITC, I relate the total DiD effect ΔP to the change in the participation tax rate $\Delta\tau$ implied by the EITC expansion alone. The baseline net-

[29] Figure A.XII in the appendix shows AFDC/TANF participation rates over the 1990s by number of children. The pre-reform participation rates — and the ensuing drops in those rates — are strongly increasing in family size.

[30] This delay cannot be explained by the gradual phase-in of the EITC expansion for families with two or more children, because even the 1994 expansion on its own was much larger than the total expansion for families with fewer children (see Figure 2).

of-tax rate $1-\tau$, on the other hand, accounts for the combined implications of taxes and welfare benefits. I calculate this tax rate by combining the CPS data with NBER's tax simulation model (TAXSIM) and a welfare benefit calculator that accounts for AFDC and Food Stamp benefits. The tax-benefit simulations are outlined below and the full technical details are provided in Appendix C. While τ and $\Delta\tau$ vary at the individual level, the elasticity ε will be calculated by relating the average treatment effect in the numerator to an average incentive change in the denominator.

Calculating the tax parameters in equation (2) requires information about earnings conditional on working. While the earnings of those who are working are observed in the CPS March files, the latent earnings of those who are not working are not directly observable. Therefore, the first step of the analysis is to predict earnings for non-workers. This is done by running the following regression on the sample of single women with positive (pre-reform) earnings:

$$Y_i = \alpha_a + \beta_n + \gamma_y + \delta_e + \zeta_r + \lambda_s + \eta_{ae} + \theta_{ne} + \vartheta_{ye} + \nu_i, \tag{3}$$

where Y_i is earnings of worker i and the right-hand side includes fixed effects for the age of the woman a (6 categories), the number of children n (7 categories), the age of the youngest child y (7 categories), education e (4 categories), race r (white, non-white), state s, as well as a set of second-level interactions between education, age, number of children, and age of the youngest child. The regression is estimated using pre-reform data (1991-93) for workers, and the estimated coefficients are then used to predict earnings for non-workers. The measurement of tax parameters is based on predicted earnings for non-workers and actual earnings for workers.

Two conceptual issues come up in this type of exercise. First, while the earnings predictions account for selection on observables, they do not account for selection on unobservables. The results are very robust to alternative specifications of the earnings regression, either more parsimonious or richer specifications. The different specifications predict that the majority of single mothers enter the labor market around the peak of the EITC schedule (where the subsidy is largest), which is also what economic theory predicts.[31] Second, I calculate the tax parameters in the denominator of equation (2) using the full sample of workers and non-workers. Both groups are included because, in general, extensive margin effects may be driven either by higher entry rates among

[31] Panel A of Figure A.XIII shows the predicted earnings distribution in the full sample of single mothers (working and non-working) estimated from equation (3). The kink points of the 1993 EITC schedule are demarcated by the vertical dashed lines. The mode of the earnings distribution is located between the first and second kink points of the EITC (where the refund is maximized) and only a small fraction of workers are predicted to locate above the EITC exhaustion point. Panel B of the figure shows the distribution of participation tax rates implied by the predicted earnings distribution.

non-workers or by lower exit rates among workers. In theory, the elasticity should be based on the tax parameters of the *marginal* workers, i.e. those close to the indifference margin of entry or exit. By including inframarginal women among both workers and non-workers, there will be offsetting selection effects. These issues related to unobserved earnings and selection are endemic to estimations of extensive margin elasticities.

The results are displayed in Table 3. The first three columns show predicted earnings and tax parameters, the next three columns show employment effects, and the last three columns show participation effects. Each statistic is shown for all single mothers in the first row and separately by number of children in the following rows. The change in the net-of-tax rate ($\Delta(1-\tau)$) and the changes in the employment and participation rates (ΔP) represent difference-in-differences between the treatment and controls groups. For the tax rate changes, the DiDs are calculated at full phase-in of the EITC expansion in 1996. For the extensive margin effects, the DiDs represent 10-year effects as shown in the event studies in Figure 7 (employment) and Figure A.XI (participation).

The following insights emerge from the table. First, the change in the net-of-tax rate is strictly increasing in family size even though the EITC schedule itself does not vary after two children. This is because single mothers with more children are predicted to enter at lower earnings levels where the EITC subsidy is larger. Second, the elasticities are in general very large. Across all single mothers, the elasticity of employment equals 2.0 while the elasticity of participation equals 1.7.[32] Third, the elasticities are strongly heterogeneous across family sizes. The elasticities for large families are huge, with employment elasticities of 2.7 for those with three children and 5.1 for those with four or more children. That is, despite accounting for the larger EITC incentive among larger families (due to variation in the schedule as well as in predicted earnings), the implied elasticities are much larger for these families.[33]

To summarize, assuming that all of the extensive margin increases after the 1993 reform were driven by the EITC implies elasticities that are extremely large. Although extensive margin elasticities are not structural parameters, we do have a sense of a reasonable range from the vast labor supply literature. The elasticities reported here fall outside any reasonable range. Hence, the view

[32] As described in Appendix C, the EITC-induced tax change $\Delta(1-\tau)$ is calculated assuming 100% take-up of the EITC conditional on eligibility. Given incomplete take-up, $\Delta(1-\tau)$ is upward biased and, as a result, the large elasticities presented here tend to be conservative (i.e., conservative given the thought experiment whereby the EITC is assumed to drive the entire extensive margin effect).

[33] The estimates in Table 3 are based on the raw event study specification without demographic controls. Table A.V in the appendix repeats the exercise based on specifications with demographic controls. Adjusting for demographics produces elasticities that are somewhat smaller — especially for the largest family sizes — but the broad findings are the same.

that the EITC was exclusively or mostly responsible for the changes in the 1990s must be very far off.

Instead of calculating elasticities, a related approach is to simulate how much can be explained by the EITC assuming a (potentially) reasonable elasticity. In a static labor supply model where single mothers respond according to the constant extensive margin elasticity ε, we have

$$\Delta P_t = \varepsilon \cdot \frac{\Delta\left(1-\tau_t\right)}{1-\tau_{93}} \cdot P_{93}, \tag{4}$$

where ΔP_t is the extensive margin response in year t relative to the pre-reform year 1993, $\Delta\tau_t$ is the reform-induced tax rate in change in year t relative to 1993 (which features some time variation due to the gradual phase-in of the reform), while τ_{93} and P_{93} are baseline values in 1993.[34] Calculating the net-of-tax rate change $\Delta\left(1-\tau_t\right)$ as the reform-induced change for the treatment group relative to the control group, the implied time path of ΔP_t from (4) can be compared to the observed DiD impacts from the event studies.

This exercise is presented in Figure 8.[35] The figure assumes an extensive margin elasticity of 0.3, corresponding roughly to the preferred estimate in the meta study by Chetty *et al.* (2013a).[36] Even under a sizable elasticity of 0.3, only a small fraction of the observed changes in the nineties would be driven by the EITC: 13% for single women with one child, 19% for single mothers with two children, 11% for single mothers with three children, and 6% for those with four or more children. I am not arguing for using an elasticity of 0.3 — in fact, I will argue that the elasticity with respect to EITC-induced changes is closer to zero — but these simulations give a clear sense of how large the changes in the nineties were. Any statement that the EITC was a major component of what happened in the nineties is implicitly based on unrealistically large elasticities.

5.4 1993 Reform: Heterogeneity by Welfare Treatment Intensity

The next two sections investigate the impact of welfare reform (waivers and PRWORA) on the extensive margin patterns in the 1990s. Such an investigation requires variation in welfare reform treatment across individuals. I start by considering two different proxies for welfare treatment

[34] Of course, the simple static model underlying equation (4) may not be realistic. The point of this exercise, however, is to provide a sanity check on the magnitudes in the data, and for such an exercise, the static, iso-elastic model is a natural benchmark to consider.

[35] This figure is based on event study specifications without demographic controls. The corresponding figure with demographic controls is presented in Figure A.XIV in the appendix.

[36] They show that the average extensive margin elasticity across nine studies is equal to 0.25.

intensity. The first proxy is the age of the youngest child. It turns out that the level of welfare participation prior to waivers and PRWORA and the ensuing drop in welfare participation are strongly related to the age of the youngest child. The second proxy is a predicted probability of pre-reform welfare participation using the age of the youngest child, the number of children, and other demographic variables. Both of these analyses will show that the extensive margin effects in the 1990s are closely aligned with the strength of welfare treatment.

Due to the granularity of the analysis in this section, rather than showing event studies in each cut of the data, I summarize the effects using a standard DiD specification with a post-reform dummy. Specifically, I consider specifications of the following form

$$\begin{aligned} P_{imt} &= \alpha \cdot Post_t + \sum_j \beta_j \cdot Welfare_{j=c} + \sum_j \gamma_j \cdot Post_t \cdot Welfare_{j=c} \\ &\quad + \eta \cdot U_{st} + \theta \cdot U_{st} \cdot Kids_i + \lambda_s + \boldsymbol{X_i}\boldsymbol{\phi} + \nu_{imt}, \end{aligned} \tag{5}$$

where P_{imt} is an indicator equal to one if individual i is employed in month m of year t, $Post_t$ is an indicator equal to one in the years after the 1993 reform, and $Welfare_{j=c}$ is an indicator equal to one if the individual belongs to welfare treatment category c. The welfare treatment categories are based either on the age of the youngest child (7 bins) or on a predicted AFDC probability (10 deciles) as described below. In either case, the omitted category is having no children, so that the welfare category variable subsumes the kids dummy in the previous specifications. The coefficient γ_c represents the average DiD effect (over the post period) for single mothers in welfare category c relative to single women without children. I consider effects over two post-periods: a three-year period (avoiding years after PRWORA) and a ten-year period. I start from the raw DiD effects γ_c obtained from the first line of equation (5), and then consider the implications of the controls in the second line. These controls absorb the effects of state-level business cycles and demographics. Specifically, U_{st} is the aggregate unemployment rate in state s and year t, the effect of which is allowed to vary by children, and λ_s is a state fixed effect.

The results are presented in Figure 9. The left panels show heterogeneity by age of the youngest child, while the right panels show heterogeneity by probability of AFDC participation. Consider first the results for age of the youngest child.[37] The top panel shows that this variable is a very strong predictor of welfare participation before the reform — the younger is the child, the higher

[37] As elsewhere, I include only EITC-eligible children and consider the following categories for the age of the youngest child: 0-1, 2-3, 4-6, 7-9, 10-13, 14-17, and 18+. Consistent with the EITC rules, the 18+ category includes children aged 18-23, who are living at home and are still in full-time education.

is participation — which in turn predicts the drop in welfare participation after the reform. The relationship between pre-reform participation levels and post-reform participation drops across bins of the age of the youngest child is almost perfectly linear and has a slope of 0.69. Among single mothers with children aged 0-1, pre-reform AFDC participation was about 50 precent and the subsequent fall was close to 40 percentage points. By contrast, single mothers with older children had very low AFDC participation rates and, as a result, were virtually unaffected by welfare reform.

The middle panel shows raw DiD estimates by age of the youngest child over three years (solid black) and over ten years (dashed black) together with pre-reform AFDC participation rates (solid red). The extensive margin effects are strongly and monotonically declining in the age of the youngest child. The three-year effects fall to about zero (and become statistically insignificant) for those whose youngest child is older than 13 years, while the ten-year effects fall to zero when the youngest child is older than 17. These estimates include any confounding effects of the business cycle and changing demographics. As shown in the bottom panel, controlling for these factors reduces the treatment effect at each age and therefore strengthens the results. The three-year effects are non-positive above age 9 and the ten-year effects are non-positive above age 13.

Table 4 shows estimated impacts in two age groups (0-13 years and 14+ years) across different specifications and outcomes. The main insight is very robust: while the extensive margin effects are very large for single women with younger children, there are no effects for those with older children. Among those with children above age 13, the three-year effects are either small and statistically insignificant or they are negative. The ten-year effects are significant only when no controls are included. Once unemployment controls are added, there is no longer any effect.

Because the analysis is based on EITC eligible children — that is, children living at home who are below age 18 or below age 24 if still in education — the age of the youngest child is correlated with the number of children. Having more children implies that the last-born tends to be younger. As a result, the analysis using the age of the youngest child overlaps with the previous analysis using the number of children. I now move to a multivariate analysis using the joint predictive power of the age of the youngest child, the number of children, and other demographic variables. Specifically, I estimate the probability of pre-reform welfare participation based on the following specification

$$AFDC_i = \alpha_a + \beta_n + \gamma_y + \zeta_r + \lambda_s + \nu_i, \tag{6}$$

where an indicator for receiving welfare benefits, $AFDC_i$, is regressed on fixed effects for the age of the mother a, the number of children n, the age of the youngest child y, race r, and state s.[38] The binning of these demographic variables is the same as before. The regression is run on the CPS March files (which contains information on welfare receipt) in the pre-reform year, 1993. From this regression, I predict the probability of AFDC participation for each single mother in the estimation sample, and define an indicator for being in different deciles of the distribution of these AFDC probabilities.

The results are shown in the right panels of Figure 9. The top panel shows that pre-reform welfare participation varies widely across the distribution, from 8% to more than 60%, and is strongly related to the post-reform welfare participation drop. The relationship between pre-reform levels and post-reform drops is stronger in this specification than when using only the age of the youngest child. The next panels show DiD impacts on employment over three years and ten years by deciles of the predicted AFDC probability. The employment impacts are strongly increasing in the AFDC probability. In the raw data, the ten-year effect is about 20pp in the top decile, but only 2pp in the bottom decile. Adding controls for business cycle effects and demographic changes makes the estimates smaller: the ten-year effect is now 13pp in the top decile and virtually zero in the bottom decile. In fact, there are no significant ten-year effects in the bottom two deciles and no significant three-year effects in the bottom four deciles. Table 5 shows average effects in the bottom two deciles and in the top eight deciles of welfare treatment intensity across different specifications. In the bottom deciles, there are no significant three-year effects in any specification and no significant ten-year effects once controls are added.

To summarize, the large extensive margin increases in nineties were driven by single mothers who had high AFDC participation prior to welfare reform and were therefore strongly treated by waivers and PRWORA. Single mothers with low pre-reform AFDC participation did not respond. These results are consistent with welfare reform, but harder to reconcile with EITC reform.

Building on these insights, it is useful to consider a different outcome variable: the fraction of single mothers who are either employed or on AFDC/TANF (henceforth referred to as the "employment-welfare rate"). This is useful because the EITC and welfare reform are expected to affect this outcome differently. Cutting welfare pushes people from welfare into work or into

[38] Equation (6) does not include education even though this variable would help with predicting AFDC participation. Education is excluded because it is a relatively direct proxy for earnings conditional on working and therefore for EITC eligibility. As a result, while using education information would strengthen my results, this is misleading as highly-educated single mothers who did not participate in welfare (thus being untreated by welfare reform) were also less likely to be eligible for the EITC (thus being untreated by EITC reform as well).

searching for work. If everyone finds jobs, the employment-welfare rate will be unaffected. Otherwise, it will decrease. By contrast, expanding the EITC provides work incentives to people from all non-working states, including those in the AFDC/TANF program, those in other social assistance programs, and those receiving no social assistance. If people respond to the EITC, the employment-welfare rate will increase.

The results are presented in the online appendix. Figure A.XV illustrates the approach in the full sample of single women. Panel A shows the basic DiD comparing the employment series for single women with and without children around the 1993 reform, while Panel B adds AFDC/TANF participants to the employment series for single mothers.[39] Strikingly, once welfare recipients are included, there is no visible treatment effect in the 1990s. That is, the employment effects are fully accounted for by movements from welfare to employment, with no additional effects coming from any other non-employed state. Figure A.XVI-A.XVIII shows that this is true in subsamples that vary by number of children, age of the youngest child, and states with high and low welfare caseload drops. While the employment increase varies greatly across subsamples, the employment-welfare rate series is always flat (or slightly falling) across time. In each subsample and year, the extensive margin increase corresponds essentially to a movement between AFDC/TANF and employment. These patterns are again consistent with welfare reform, but puzzling under the EITC narrative. This narrative requires that, while the EITC had sizable effects on people inside the AFDC program, people outside the program were unresponsive to the EITC. This would be an unusual pattern of heterogeneity, even if not inconceivable.[40]

5.5 1993 Reform: Controlling for Waivers and the Business Cycle

The confounding factors that make it difficult to identify the impact of the 1993 EITC expansion operate at both the state and national levels. This section focuses specifically on state-level factors — welfare waivers and state business cycles — as these can be estimated. Waivers were introduced in some states but not others, and the timing of their approval and implementation varied from place to place (see section 3.2 for details). Similarly, while the economy was strong across the country, the labor market implications of the booming economy varied spatially. Exploiting the variation in these factors across time and space, I investigate how much of the 1990s employment increase for

[39] To avoid double counting, I add only AFDC/TANF participants who are not also employed.

[40] The reason why the pattern is not inconceivable is selection. It is possible that single women in other social assistance programs — in particular, Disability Insurance (DI) and Supplemental Security Income (SSI) — and those receiving no social assistance are a selected sample of people who are unresponsive to taxes.

single mothers can be explained by waiver-based reform along with falling unemployment. I focus primarily on the years 1994-96, i.e. after the EITC reform but before nationwide TANF reform took full effect in 1997. After the implementation of TANF, welfare confounders are operating at a national scale and there is no quasi-experimental approach to separate welfare from the EITC. We still have the cross-sectional variation in welfare treatment intensity analyzed above, but there is no separate event study approach for the EITC and TANF reforms.

The results in this section are based on variants of the following specification:

$$\begin{aligned} P_{imt} &= \sum_j \alpha_j \cdot Year_{j=t} + \beta \cdot Kids_i + \sum_{j \neq -1} \gamma_j \cdot Year_{j=t} \cdot Kids_i \\ &\quad + \sum_j \delta_j \cdot Year_{j=t} \cdot Waiver_{sj} + \sum_j \zeta_j \cdot Year_{j=t} \cdot Kids_i \cdot Waiver_{sj} \\ &\quad + \eta \cdot U_{st} + \theta \cdot U_{st} \cdot Kids_i + \lambda_s + \boldsymbol{X_i \phi} + \nu_{imt}, \end{aligned} \tag{7}$$

where the first line is the basic DiD event study of the 1993 reform, while the second and third lines control for welfare waivers, the business cycle, and demographics. Specifically, $Waiver_{st}$ is an indicator equal to one if state s has approved a statewide waiver in year t. The waiver indicator is interacted with a kids dummy and a full set of year dummies. That is, the impact of waivers is allowed to vary between those with and without children (as only the former are eligible for welfare) and from year to year. To the extent that waiver states saw larger employment increases for single mothers after the introduction of waivers than non-waiver states, this effect gets absorbed in the waiver controls and reduces the DiD coefficients γ_t. Similarly, if states with larger drops in (aggregate) unemployment saw larger employment increases for single mothers, this gets absorbed in the unemployment controls and further reduces the DiD coefficients.

It is useful to highlight a few conceptual points about the specification. First, since it controls only for state variation, any national confounders are still absorbed in the DiD coefficients. The strong macroeconomy of the mid-late 1990s was a national phenomenon, and PRWORA affected all states after its enactment in August 1996. Moreover, most states without any major waiver reform introduced smaller pilot programs, and it is therefore conceivable that both treatment and control states were affected by welfare reform prior to PRWORA. For these reasons, even if the state-level controls in (7) had no impact on the estimates, this would not necessarily imply that confounders are not affecting the estimates. Second, while there were six main types of waiver policies, these are lumped together in a single any-waiver dummy. Below I consider an extension that allows for separate coefficients on different types of waivers. Third, the waiver indicator may

switch on either at the time of waiver approval or at the time of waiver implementation (see Table A.III for for approval and implementation dates of each state waiver). The baseline specification is based on approval dates, but I consider a robustness check based on implementation dates. Finally, the impact of waivers is allowed to vary over time through the interaction with year dummies. This specification choice reflects that waivers are unlikely to have the same impact after 1996 when TANF introduced waiver-style policies across the country. That is, because the waiver dummies capture the impact of having a waiver *relative to not having one*, their impact is likely to be smaller (or zero) after the implementation of TANF.[41]

The first set of results is presented in Figure 10. Panel A compares the raw DiD series to a specification that includes unemployment controls, but no other controls.[42] Panel B compares the raw DiD series to a specification with both unemployment and waiver controls. The figure shows that both of these controls reduce the extensive margin effects after 1993. While the unemployment controls reduce the estimates throughout the mid-late 1990s, waivers reduce the estimates just before TANF reform in 1997. This is natural given that most statewide waivers were approved in 1995 and 1996, just before TANF reform. The striking feature of Panel B is that the state-level controls change the DiD series from what looks like a compelling event study of the 1993 EITC reform to what looks like an event study of the 1996 TANF reform. This finding is consistent with welfare reform (in combination with the macroeconomy) being the main driver of the extensive margin effects.

The appendix provides a number of robustness checks. Figure A.XIX repeats the analysis for different outcomes (employment vs participation) and samples (all women vs low-educated women). None of these aspects affect the results in any significant way. Figure A.XX investigates heterogeneity by number of children. The results are particularly strong for families with two children, where the event study adjusted for business cycle and waiver effects is completely flat until 1996, the year before PRWORA takes full effect. The results are a little less strong for families with three or more children, but qualitatively similar. Figures A.XXI and A.XXII vary the specification of waivers. The two figures consider the same waiver specifications, but the first includes both waiver and unemployment controls while the other includes only waiver controls. In each

[41] As described in section 3.2, there is variation in the setting of TANF parameters across states (such as in the length of time limits). To the extent that the stringency of TANF parameters is correlated with pre-TANF waivers, the waiver dummies may still have explanatory power after the implementation of TANF. It remains the case, however, that their impact is unlikely to be the same before and after national welfare reform.

[42] To clarify, I always introduce unemployment controls together with state fixed effects. This ensures that the business cycle controls capture only within-state variation in unemployment over time as opposed to also cross-state variation in structural unemployment levels.

figure, the baseline specification of waivers (in Panel A) is compared to specifications where the any-waiver indicator is replaced by indicators for each waiver type (Panel B), where the date of implementation is used instead of the date of approval (Panel C), and where the time-varying waiver controls are replaced by basic before-after waiver controls (Panel D). The graphs produce two main insights. The first is that waivers on their own reduce the estimates mainly in 1996; the other is that the estimates are very robust to specification, except for the before-after waiver dummy. The second insight is related to the first: because the impact of waivers has a very specific timing, introducing a basic before-after dummy has little impact.[43] For the reasons discussed above, a before-after waiver dummy is likely to be attenuated due to the introduction of TANF.

Table 6 summarizes the estimated effects of the 1993 reform after three years (before PRWORA) and after ten years (after PRWORA) with waiver and business cycle controls under different specifications. The table consider different extensive margin outcomes, different education levels, and different sets of controls. The table demonstrates the robustness of the main message: while the raw estimates are very large, the three-year effects become small and statistically insignificant as soon as unemployment and waivers controls are added. Of course, the ten-year effects remain significant as these include the impact of national welfare reform.

Like the results in the previous section, the analysis presented here points to the crucial role of welfare reform and the macroeconomy in the 1990s. The results show that the extensive margin effects between 1994-96 — the years that offer the best chance of separately identifying any EITC effects — can be explained by a combination of waiver-based reform and the business cycle. These results highlight the fragility of EITC estimates that rely on variation in the 1990s.

6 EITC and the Extensive Margin: Putting Everything Together

Having analyzed the 1993 reform in detail, I consider all EITC reforms at the state and federal level together in a stacked event study. Combining all the reforms increases precision and is arguably better identified than an event study of any individual reform. In particular, because the time series of employment is bumpy from year to year (due to business cycles and other time-varying factors), event studies of individual reforms may easily create the illusion of either positive or negative effects depending on the timing of the reform. Combining reforms with different timing alleviates this problem.

[43] The small impact of a before-after waiver dummy is consistent with results presented in an online appendix to Hoynes & Patel (2018). They include a post-waiver indicator and find that it does not significantly impact their results.

6.1 State EITC Supplements

This section presents evidence from state introductions of EITC supplements. The roll-out of these supplements is illustrated in Figure 11 and the details of each program are provided Table A.II. A total of 30 states instituted EITC supplements between 1986-2018, and I restrict attention to the 27 states that have maintained their supplement for at least 3 years.[44] Except for California and Minnesota, the state EITCs are specified as percentages of the federal EITC. They are refundable credits in most states, but non-refundable in some. An empirical advantage of these policies is that they generate variation across space, in addition to the variation across family size considered above.

The analysis is implemented by creating a synthetic control state for each state with an EITC supplement, and then running a stacked event study comparing treatment and synthetic control states around the time of EITC introductions. The synthetic control states are constructed from the states that never had a supplement, matching on the level of the employment rate in each of the five pre-reform years. Table A.VI shows the composition of each synthetic state, and the details of the approach and implementation are provided in section D of the online appendix.

In the first specification, the estimation is implemented in the sample of single women with children. Here the empirical strategy is a difference-in-differences estimation that compares different states over time, conditioning on children. The results are shown in Figure 12. Panel A of the figure includes all 27 reforms and shows no effect of the EITC. The treatment and control states track each other in the pre-reform years (by construction) and continue to do so in the post-reform years. A concern with the analysis in Panel A is that many of the state supplements are quite small, making it difficult to detect any effect. Panel B therefore restricts attention to 12 "large reforms", specifically reforms that introduced *refundable* supplements equal to *at least 10%* of the federal EITC.[45] Focusing on large EITC reforms does not alter the main result: states that introduce EITC supplements do not see employment increases relative to states without such supplements.

As a robustness check, Figure A.XXIII in the appendix shows results from a specification that exploits the variation both across states and across single women with and without children within

[44] I drop Hawaii and South Carolina as they introduced their EITC supplements too recently (in 2018). I drop Washington as they enacted, but de facto never implemented their EITC supplement.

[45] To obtain a set of relatively sharp policy experiments, I also require that the state EITC reached 10% within three years of enactment. This excludes states that have sizable and refundable credits, but where this level has been reached too incrementally for a compelling event study design. The 12 states with large EITC reforms are California (2015), Connecticut (2011), Dist. of Columbia (2000), Kansas (1998), Massachusetts (1997), Michigan (2008), Minnesota (1991), New Jersey (2000), New Mexico (2007), New York (1994), Vermont (1988), and Wisconsin (1989).

states. Here the empirical strategy is a triple-differences estimation that compares treatment and control states conditional on children relative to those same states conditional on no children. The figure shows that this refinement has no substantive impact on the empirical patterns. There is still no effect of state EITC supplements on employment.

6.2 Stacked Event Study: All State and Federal Reforms

The final piece of the investigation combines the state and federal reforms in a stacked event study framework. I include 31 reforms in the analysis: the 27 state reforms analyzed in the previous section along with the 4 federal reforms enacted in 1975, 1986, 1993, and 2009. The 1990 reform is not included as a separate event due to its close proximity in time to the (larger) 1986 and 1993 reforms. All federal reforms are weighted equally, while state reforms are weighted according to the state's share of the national population in 2016. Therefore, although there are many more state reforms than federal reforms, all of the state reforms together weigh less than a single federal reform. The qualitative results are robust to alternative weighting schemes (e.g, equal weights for all reforms).

Normalizing the first year after each reform to zero, I consider an event time window that runs from -5 to +5. There are two exceptions to this: California's 2015 reform allows for an event time window running only to +2, while the federal 1993 reform is truncated at event time +2 for identification reasons. The latter specification choice is done to avoid overlap between the 1993 EITC reform and the 1996 TANF reform, which would otherwise confound the estimates. I include controls for welfare waivers and the business cycle (specified as in equation 7) to deal with the confounders already at play during event times 0 to +2 for the 1993 reform.

Two additional specification points are worth noting. First, the estimates are based on comparing single women with and without children, except for the 2009 federal reform targeted to families with three or more children. For this reform, the treatment indicator is based on having at least three children (relative to zero children). Second, in order to combine the state and federal reforms within one event model, the effect of the state reforms is based on comparing single women with and without children within each state. That is, in contrast to the synthetic control approach considered above, the spatial variation in EITC supplements is not used. The full details regarding the specification are provided in section E of the appendix.

The results are presented in Figure 13. Panel A shows the federal reforms alone, Panel B shows the state reforms alone, while Panel C combines the federal and state reforms. The federal reforms

— and by implication the federal and state reforms combined — feature small positive post-reform coefficients, but at the same time there is a pre-trend. There is no inflection point in the series after the reforms and therefore no visible EITC effect. To make this clearer, Panel D adjusts for linear, group-specific trends estimated on the pre-reform data. This pre-trend adjustment makes the DiD series completely flat. Each panel provides an estimate of the average EITC effect over the post-reform period (event times 0 to 5) along with the confidence band in parenthesis. The estimates are close to zero and precisely estimated: in Panel D, we can rule out effects on employment larger than 0.8 percentage points across all 31 reforms. Finally, Figure A.XXIV in the appendix repeats the analysis in the sample of single women with low education (high school degree or less). The main finding is the same: the EITC effect is a tightly estimated zero.

7 Conclusions

In this paper I have analyzed every EITC reform at the state and federal level within one modern empirical framework. From this comprehensive and long-run perspective, the EITC has not had any clear effects on labor supply at the extensive margin. Apart from the expansion enacted in 1993, EITC reforms are not associated with increases in the employment of single mothers relative to single women without children. The 1993 reform, on the other hand, is associated with very large employment increases, but these increases align closely with the confounding effects of welfare reform and a booming macroeconomy. Exploiting variation in these confounders across household type, space and time, I have shown that the effects are driven exactly by those affected most strongly by welfare reform and the business cycle. A stacked event study of all EITC reforms, controlling for welfare reform and the business cycle, produces a precisely estimated zero.

There are two interpretations of this null result. One is that the extensive margin elasticity with respect to taxes is small, and perhaps especially the elasticity with respect to a highly nonlinear tax refund like the EITC. The other is that, even if the elasticity is not zero in general, the informational and psychological frictions specific to the EITC have reduced its impact. These frictions may be related to the complex nonlinear schedule or to the procedures for filing taxes and claiming the credit. As reviewed above, a number of studies have documented that most potential recipients are either unaware of the EITC or have a limited understanding of the schedule and eligibility requirements.[46] Given these frictions, the EITC is not an *a priori* likely candidate for finding large

[46] See e.g., Romich & Weisner (2000), Smeeding *et al.* (2000), Ross Phillips (2001), Maag (2005), Chetty & Saez (2013), and Bhargava & Manoli (2015).

labor supply impacts.

While these frictions are widely acknowledged in the literature, they have been used mainly to explain the absence of *intensive* margin responses. The reasoning seems to be that extensive margin responses can be based solely on knowing about the existence of a tax refund without understanding the specifics of the schedule and eligibility rules. Even ignoring the fact that many potential recipients are unaware of the EITC, this argument is surprising. Economic theory predicts that intensive and extensive margin decisions are interdependent, and this interdependence is particularly strong for nonlinear incentives (see Kleven & Kreiner 2006; Eissa *et al.* 2006, 2008). The EITC is an incentive to enter the labor market around the refund-maximizing earnings range, which is relatively narrow and vary by family size. Without precise information about the location of the refund-maximizing earnings range and the size of the credit in that range, the extensive margin response is not very credible.

By contrast, welfare reform is not subject to these concerns about salience and information. The debate about welfare reform and welfare culture was extremely prominent in the nineties, as exemplified by Bill Clinton's famous campaign pledge to "end welfare as we know it." State waivers and national TANF reform imposed drastic changes on *existing* program participants, so information was essentially automatic. What is more, the changes to welfare were largely related to ordeals and enforcement, the impact of which is more mechanical than financial incentives. They included lifetime limits on welfare receipt, work requirements, community service and training, and those who did not satisfy these requirements could get kicked off the welfare rolls. These initiatives stand in sharp contrast to the complex tax refund incentives introduced by the relatively unknown EITC program.

These findings do not necessarily imply that the EITC is a bad policy. Absent labor supply responses at either the extensive or intensive margins, the EITC is a non-distortionary transfer to the working poor.[47] The optimality of such a transfer depends on the social welfare function and on who pays for it. For example, if the EITC is financed by a lump-sum tax on all individuals and if the government puts a larger weight on the working poor than on the average individual, then the policy is socially optimal. This is the reasoning underlying the results in Saez (2002).[48]

[47] Of course, this paper has not shown the absence of intensive margin responses to the EITC. The literature has struggled to find any clear evidence of intensive margin responses to the policy, perhaps because of the frictions just discussed. Chetty *et al.* (2013b) estimate significant intensive margin responses to the EITC *conditional on knowledge*, which is still consistent with small average responses across informed and uninformed taxpayers.

[48] In Saez (2002), the optimal EITC (defined as a negative tax rate on participation) reflects a trade-off between the equity gain just described and the efficiency *loss* from extensive margin responses to an EITC that make people work too much.

Conversely, if transfers to the working poor are financed by reducing welfare benefits to the poor, then the equity effect is negative under standard social preferences. In this case, the desirability of the EITC requires social preferences that put more weight on the working poor than on the non-working poor, even if the latter are worse off. Leaving aside these optimal tax considerations, the political argument for the EITC has relied heavily on its supposed employment effects. The empirical findings presented here do not lend support to this argument.

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TABLE 1: DESCRIPTIVE STATISTICS

	Single Women		
	All	With Children	Without Children
	(1)	(2)	(3)
Age	32.05	34.34	30.68
Age of Youngest Child	7.51	7.51	.
Number of Children	0.67	1.79	0.00
High School & Below	0.41	0.55	0.33
Some College	0.34	0.32	0.36
College Degree	0.24	0.13	0.31
Employment Rate	0.73	0.68	0.75
Labor Force Participation Rate	0.78	0.76	0.80
Welfare Participation Rate	0.20	0.39	0.03
Earnings	23,519	21,365	24,984
Observations	4,674,064	1,746,591	2,927,473

Notes: This table shows means of demographics, labor market variables, and earnings (in 2018 USD) for single women. Column (1) shows all single women, column (2) shows single women with EITC-eligible children, while column (3) shows single women without EITC-eligible children. EITC-eligible children are defined as children under 19 (24 if in full-time education) and living at home for at least half of the year. Employment and labor force participation rates are based on respondents' activities last week. The sample includes single women aged 20-50 using the March and monthly CPS files combined, pooling all years from 1968 to 2018.

TABLE 2: THREE-YEAR EFFECTS OF FEDERAL EITC REFORMS ON SINGLE MOTHERS

	Employment Rate (pp)		Participation Rate (pp)	
	(1)	(2)	(3)	(4)
Panel A: 1975 Reform				
All Educations	-0.713	-0.678	0.708	0.706
	(1.377)	(1.338)	(1.330)	(1.297)
Low-Education	-0.118	-0.248	1.087	0.991
	(1.687)	(1.654)	(1.617)	(1.582)
Panel B: 1986 Reform				
All Educations	-1.028	-0.178	-1.571	-0.877
	(1.084)	(1.036)	(1.002)	(0.969)
Low-Education	-0.411	0.293	-0.879	-0.339
	(1.439)	(1.405)	(1.334)	(1.309)
Panel C: 1990 Reform				
All Educations	-0.347	-1.608	-1.115	-2.330**
	(1.068)	(1.005)	(0.998)	(0.947)
Low-Education	-0.170	0.925	-0.804	0.075
	(1.479)	(1.430)	(1.391)	(1.347)
Panel D: 1993 Reform				
All Educations	3.060***	2.949***	3.055***	2.965***
	(0.508)	(0.475)	(0.467)	(0.440)
Low-Education	3.538***	3.627***	3.604***	3.592***
	(0.758)	(0.716)	(0.711)	(0.674)
Panel E: 2009 Reform				
All Educations	-3.554***	-3.920***	-2.162**	-2.490***
	(1.074)	(1.030)	(0.966)	(0.932)
Low-Education	-2.170	-2.786*	-2.001	-2.476*
	(1.486)	(1.443)	(1.374)	(1.337)
Demographic Controls:		X		X

Notes: This table shows DiD estimates of the effects of the five federal EITC reforms after three years. The estimates are obtained from a modified version of equation (1) in which the year dummies are collapsed into a post dummy, equal to one for the three years after each reform. Panels A-D are based on comparing single women with children to those without children, while Panel E is based on comparing single women with 3+ children to those without children. The columns show results for different outcomes (weekly employment and weekly participation) and different controls, while the rows show results for the full sample and for those with low education (defined as having a high school degree or less). The sample includes single women aged 20-50. Panels A-C use the March CPS files alone, while Panels D-E use the March and monthly files combined. Robust standard errors are clustered at the individual level.

TABLE 3: EXTENSIVE MARGIN ELASTICITIES WHEN IGNORING CONFOUNDERS

	Earnings and Tax Parameters			Employment Effects			Participation Effects		
	Earnings	τ	$\Delta(1-\tau)$	P	ΔP	ε	P	ΔP	ε
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Any Children:	14,685	0.283	0.077	0.606	0.130	**2.0**	0.690	0.129	**1.7**
1 Child:	16,197	0.338	0.034	0.680	0.085	**2.4**	0.756	0.083	**2.1**
2 Children	14,703	0.280	0.105	0.610	0.138	**1.6**	0.697	0.137	**1.4**
3 Children:	12,110	0.192	0.124	0.474	0.196	**2.7**	0.570	0.202	**2.3**
4+ Children:	8,327	0.067	0.159	0.300	0.262	**5.1**	0.400	0.252	**3.7**

Notes: This table shows estimates of the extensive margin elasticities based on the 1993 reform, assuming that the entire DiD effect between 1993-2003 can be attributed to the EITC. Columns (1)-(3) show predicted earnings and tax parameters, columns (4)-(6) show employment effects, and columns (7)-(9) show participation effects. Each statistic is shown for all single mothers in the first row and separately by number of children in the following rows. The earnings measure in column (1) is based on predicted earnings for non-workers (estimated using equation 3) and actual earnings for workers. The changes in employment and participation rates (ΔP) as well as the EITC-induced change in the net-of-tax rate ($\Delta(1-\tau)$) represent difference-in-differences comparing single women with and without children. The elasticities in columns (6) and (9) are calculated using equation (2). See section C in the appendix for additional details.

TABLE 4: EFFECTS OF THE 1993 EITC REFORM BY AGE OF YOUNGEST CHILD

	Employment Rate (pp)			Participation Rate (pp)		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Average 3-Year Effects (Before PRWORA)						
Youngest Aged 0-13	4.516***	3.771***	2.161***	4.335***	3.723***	2.259***
	(0.396)	(0.403)	(0.383)	(0.365)	(0.371)	(0.355)
Youngest Aged 14+	0.557	-0.032	-1.580**	0.176	-0.327	-1.718***
	(0.662)	(0.662)	(0.638)	(0.599)	(0.599)	(0.579)
Panel B: Average 10-Year Effects (After PRWORA)						
Youngest Aged 0-13	11.595***	9.523***	7.611***	11.072***	9.210***	7.492***
	(0.322)	(0.346)	(0.330)	(0.296)	(0.318)	(0.307)
Youngest Aged 14+	2.533***	0.560	-1.151**	2.316***	0.514	-1.017**
	(0.531)	(0.542)	(0.527)	(0.479)	(0.490)	(0.478)
Controls:						
State Fixed Effects		X	X		X	X
Unemployment × Kids		X	X		X	X
Demographics			X			X

Notes: This table shows effects of the 1993 reform by the age of youngest child across different specifications and outcomes. The estimates are obtained from versions of equation (5) where the welfare category variable corresponds to bins of the age of the youngest child. Panel A shows 3-year effects (before PRWORA) while Panel B shows 10-year effects (after PRWORA). Each panel shows the effects among mothers with youngest children aged 0-13 and youngest children aged 14+. The different columns show different outcomes (weekly employment and weekly participation) and specifications with different controls: no controls in columns (1) and (4), adding unemployment-by-kids and state fixed effects in columns (2) and (5), and finally adding demographics in columns (3) and (6). The sample includes single women aged 20-50 using the March and monthly CPS files combined. Robust standard errors are clustered at the individual level.

TABLE 5: EFFECTS OF THE 1993 EITC REFORM BY PROBABILITY OF AFDC PARTICIPATION

	Employment Rate (pp)			Participation Rate (pp)		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Average 3-Year Effects (Before PRWORA)						
Deciles 3-10 of AFDC Prob.	4.375***	3.826***	2.137***	4.188***	3.728***	2.191***
	(0.398)	(0.405)	(0.379)	(0.366)	(0.373)	(0.352)
Deciles 1-2 of AFDC Prob.	0.505	-0.114	-1.074*	0.339	-0.170	-1.078*
	(0.645)	(0.647)	(0.624)	(0.586)	(0.589)	(0.569)
Panel B: Average 10-Year Effects (After PRWORA)						
Deciles 3-10 of AFDC Prob.	11.484***	9.598***	7.445***	10.972***	9.264***	7.325***
	(0.323)	(0.348)	(0.329)	(0.298)	(0.321)	(0.305)
Deciles 1-2 of AFDC Prob.	2.340***	0.790	-0.012	2.325***	0.827*	0.088
	(0.519)	(0.531)	(0.514)	(0.470)	(0.481)	(0.468)
Controls:						
State Fixed Effects		X	X		X	X
Unemployment × Kids		X	X		X	X
Demographics			X			X

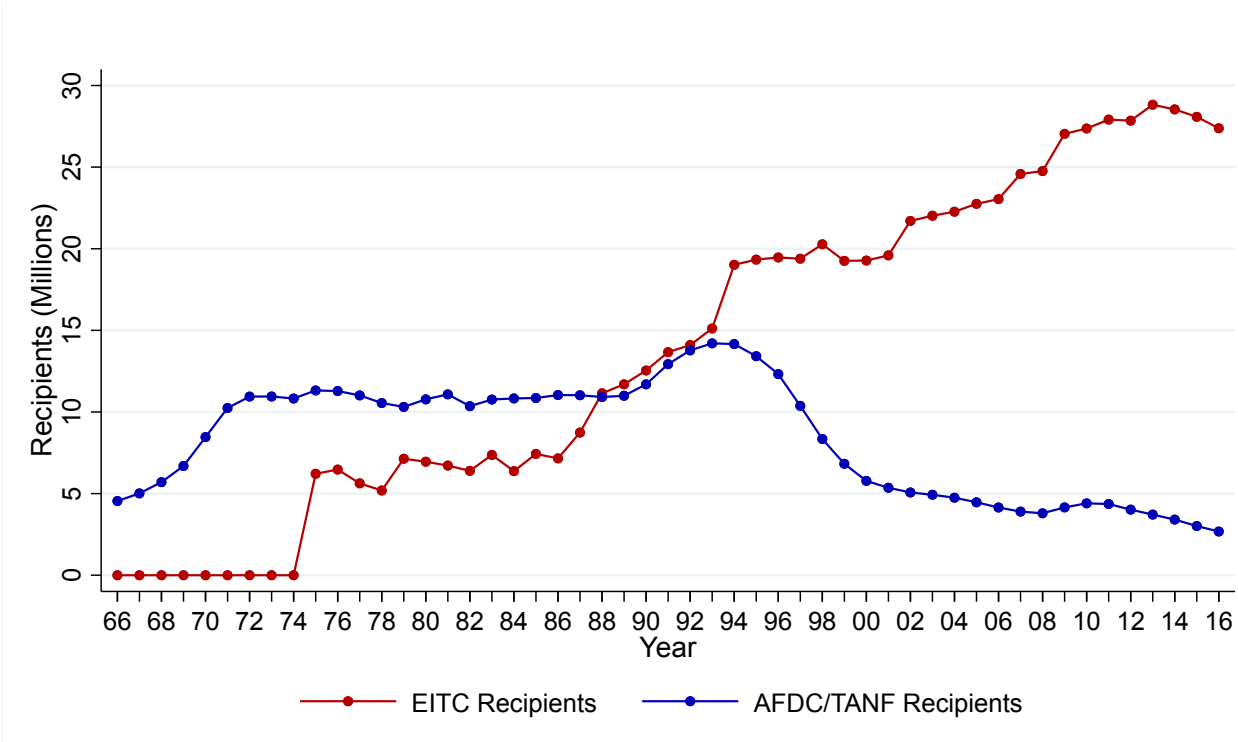
Notes: This table shows effects of the 1993 reform by probability of AFDC participation across different specifications and outcomes. The estimates are obtained from versions of equation (5) where the welfare category variable is estimated from (6). Panel A shows 3-year effects (before PRWORA) while Panel B shows 10-year effects (after PRWORA). Each panel shows the effects in the bottom two deciles and in the top eight deciles of the predicted probability of AFDC participation. The different columns show different outcomes (weekly employment and weekly participation) and specifications with different controls: no controls in columns (1) and (4), adding unemployment-by-kids and state fixed effects in columns (2) and (5), and finally adding demographics in columns (3) and (6). The sample includes single women aged 20-50 using the March and monthly CPS files combined. Robust standard errors are clustered at the individual level.

TABLE 6: EFFECTS OF THE 1993 EITC REFORM WITH WAIVER AND BUSINESS CYCLE CONTROLS

	Employment Rate (pp)			Participation Rate (pp)		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Year-3 Effects (Before PRWORA)						
All Educations	4.684***	-0.053	0.836	5.074***	-0.027	0.795
	(0.671)	(1.076)	(0.996)	(0.614)	(0.996)	(0.930)
Low-Education	5.494***	-0.105	1.060	5.923***	-0.205	0.893
	(1.017)	(1.614)	(1.511)	(0.950)	(1.527)	(1.431)
Panel B: Year-10 Effects (After PRWORA)						
All Educations	13.009***	9.753***	9.017***	12.877***	9.550***	9.005***
	(0.648)	(1.036)	(0.982)	(0.590)	(0.945)	(0.904)
Low-Education	16.050***	12.976***	12.021***	16.120***	13.501***	12.837***
	(1.003)	(1.633)	(1.563)	(0.935)	(1.527)	(1.468)
Controls:						
State Fixed Effects		X	X		X	X
Unemployment × Kids		X	X		X	X
Welfare Waivers × Kids		X	X		X	X
Demographics			X			X

Notes: This table shows effects of the 1993 reform across different samples, specifications, and outcomes. The estimates are obtained from versions of equation (7). Panel A shows 3-year effects (before PRWORA) while Panel B shows 10-year effects (after PRWORA). Each panel shows the effect among single women of all education levels and single women with a high school education or below. The different columns show different outcomes (weekly employment and weekly participation) and specifications with different controls: no controls in columns (1) and (4), adding unemployment-by-kids and state fixed effects in columns (2) and (5), and finally adding demographics in columns (3) and (6). The sample includes single women aged 20-50 using the March and monthly CPS files combined. Robust standard errors are clustered at the individual level.

FIGURE 1: LONG-RUN EVOLUTION OF EITC AND CASH WELFARE

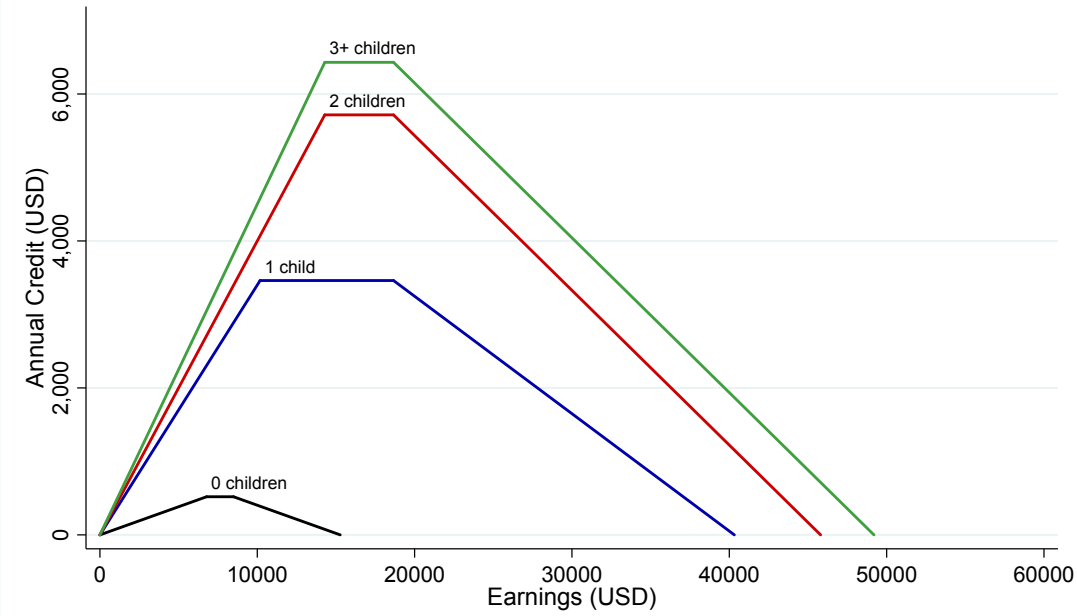


Source: Internal Revenue Service (EITC) and Department of Health and Human Services (AFDC/TANF).

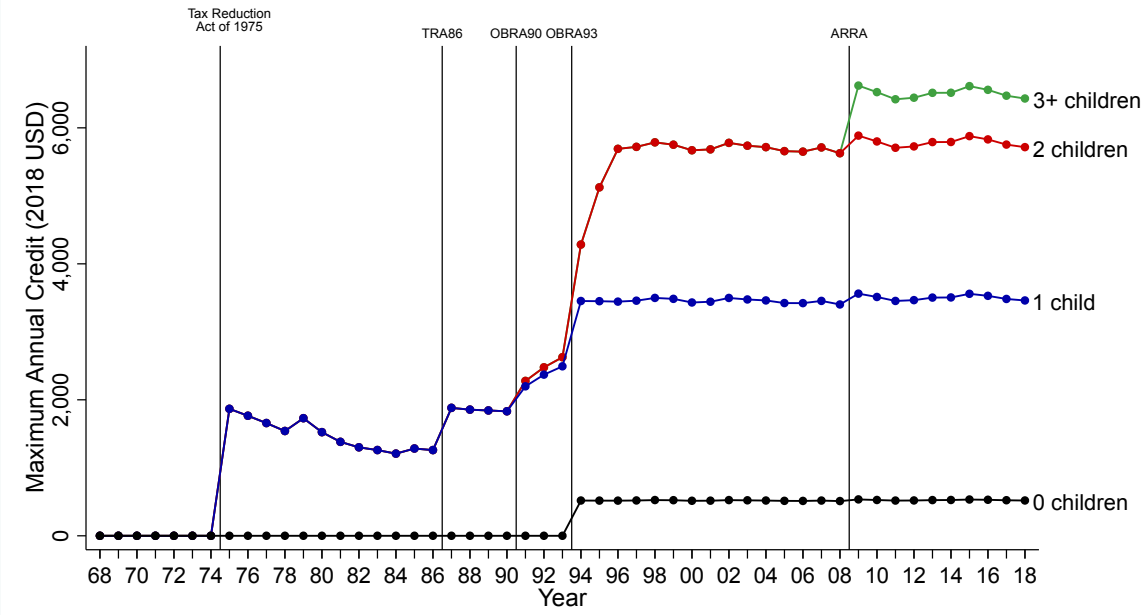
Notes: The red series show the annual number of federal EITC recipients between 1966-2016. The blue series show the average monthly number of Aid to Families with Dependent Children (AFDC) recipients between 1966-1996, and the average monthly number of Temporary Assistance for Needy Families (TANF) recipients between 1997-2016.

FIGURE 2: EITC PARAMETERS

A: EITC Schedule in 2018

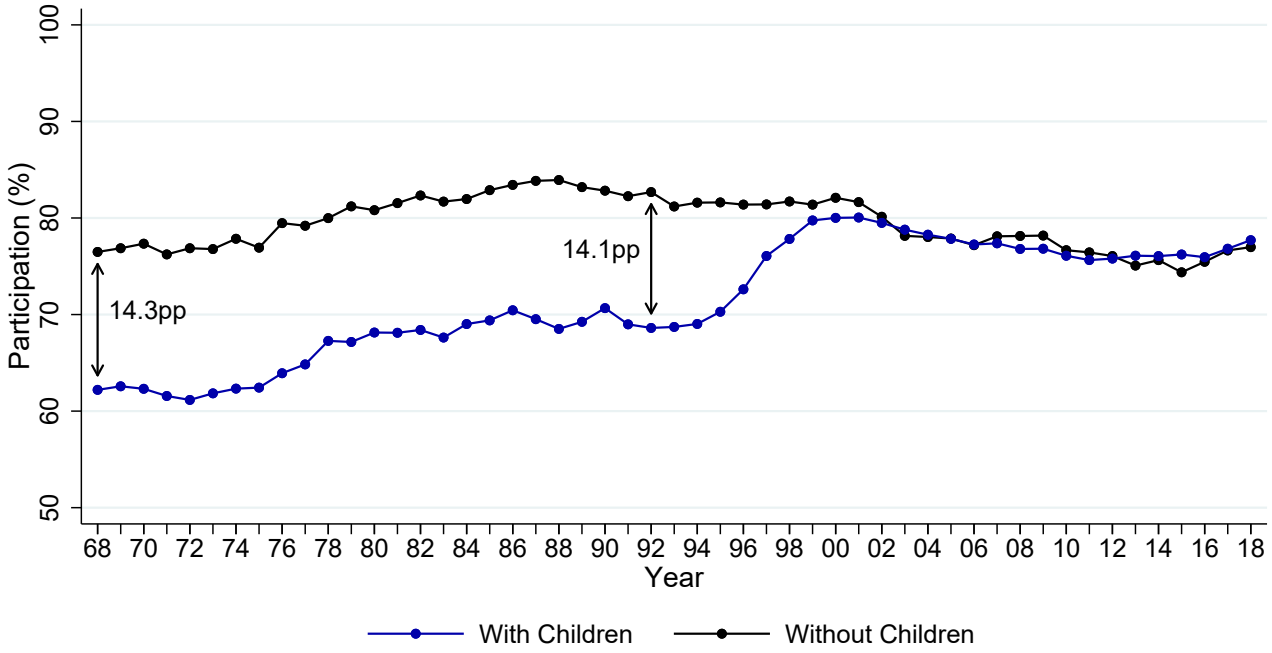


B: EITC Maximum Credit Over Time



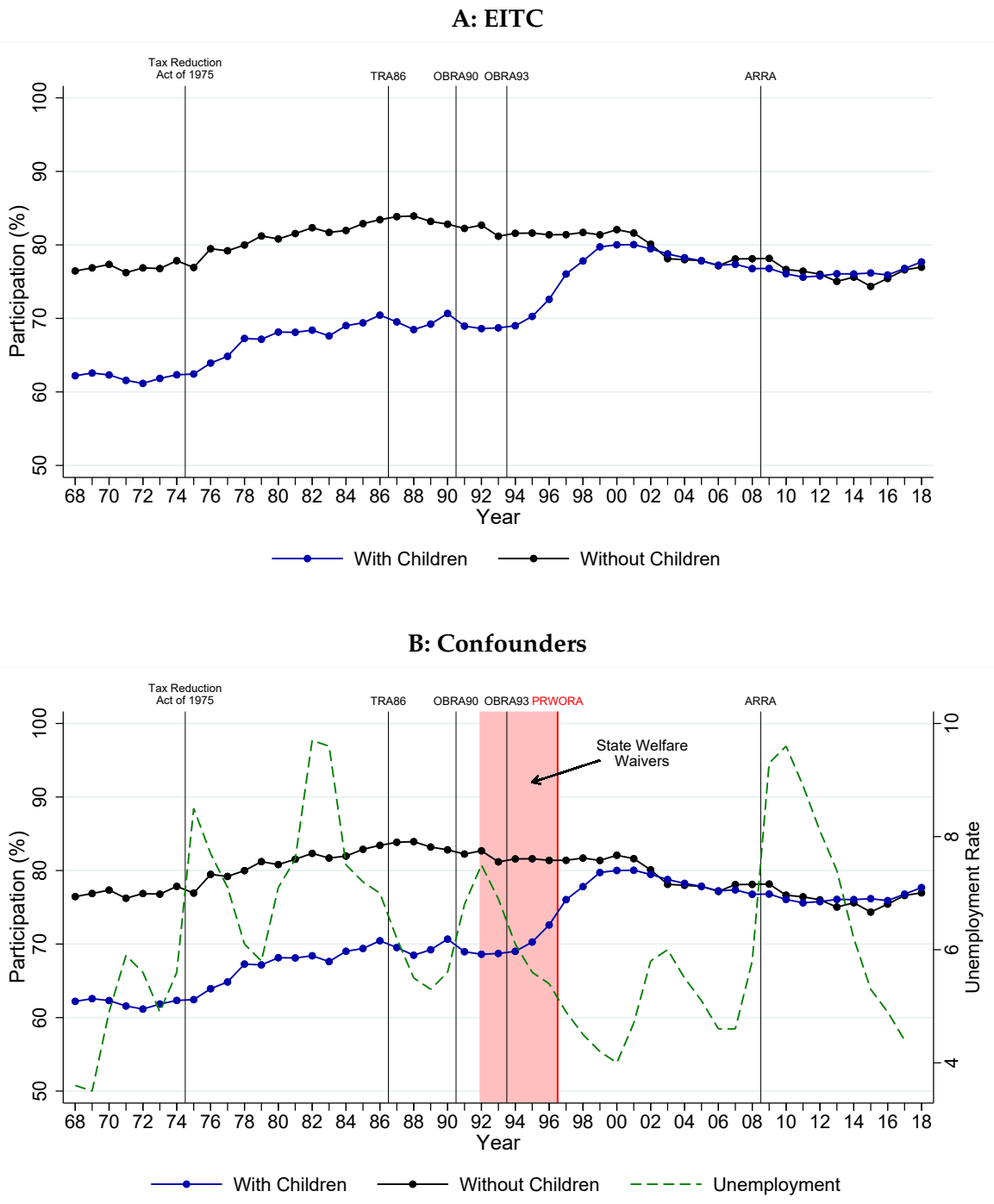
Notes: This figure shows federal EITC parameters for different family sizes. Panel A shows the 2018 EITC schedule as a function of total family earnings for families with 0, 1, 2, and 3+ EITC-eligible children. Panel B shows the maximum annual credit for families with 0, 1, 2, and 3+ EITC-eligible children between 1968 and 2018, in 2018 USD.

FIGURE 3: FIFTY YEARS OF LABOR FORCE PARTICIPATION FOR SINGLE WOMEN



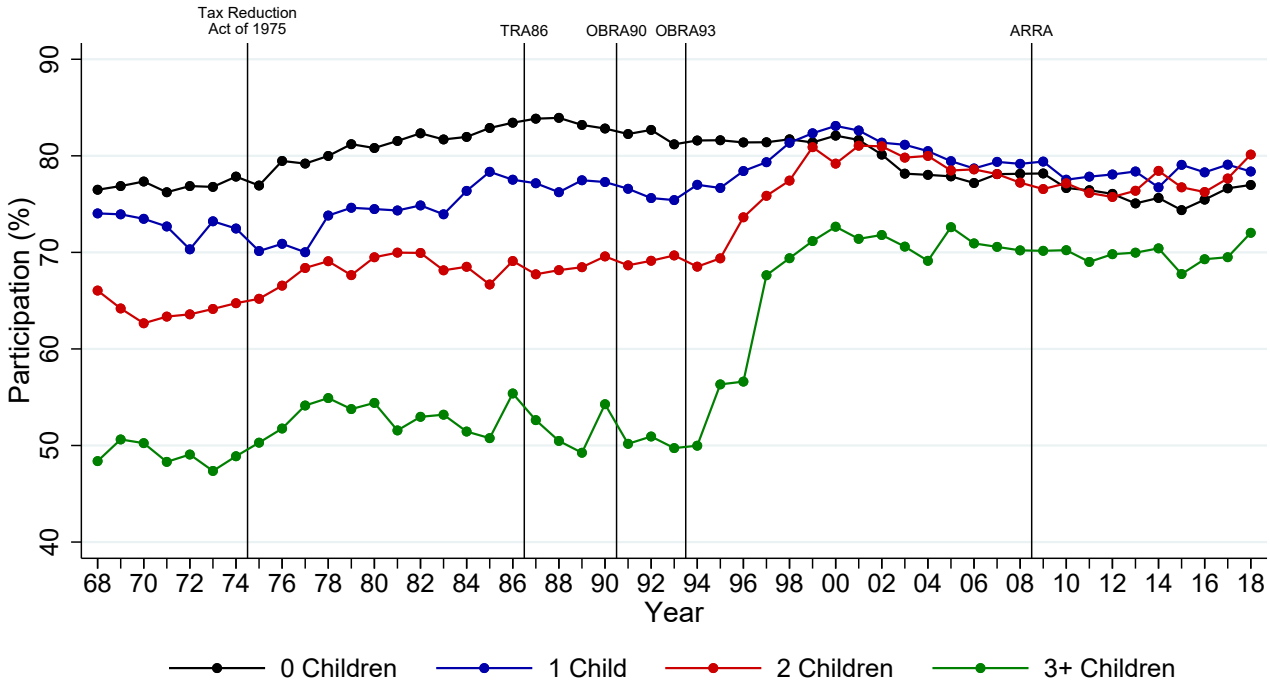
Notes: This figure shows the weekly labor force participation rate of single women with and without children between 1968 and 2018. The sample includes single women aged 20-50 using the March CPS files.

FIGURE 4: WHAT EXPLAINS THE LONG-RUN EVOLUTION?



Notes: This figure shows the weekly labor force participation rate of single women with and without children between 1968 and 2018. Panel A marks the timing of the five federal EITC reforms by vertical black lines. Panel B adds the timing of statewide welfare waivers (red shaded area), federal welfare reform (red line), and the national unemployment rate (green dashed line). The sample includes single women aged 20-50 using the March CPS files.

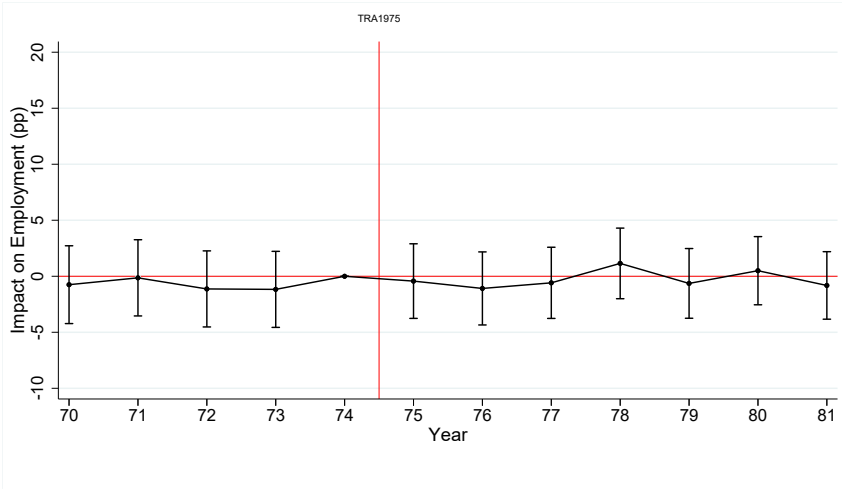
FIGURE 5: FIFTY YEARS OF PARTICIPATION FOR SINGLE WOMEN, BY FAMILY SIZE



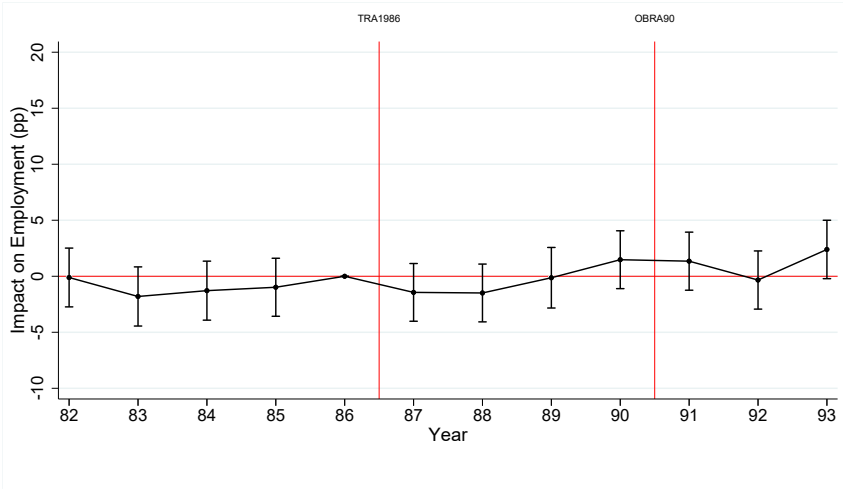
Notes: This figure shows the weekly labor force participation rate of single women with 0, 1, 2, and 3+ children between 1968 and 2018. The timing of the five federal EITC reforms are marked by vertical black lines. The sample includes single women aged 20-50 using the March CPS files.

FIGURE 6: DID EVENT STUDIES OF ALL FEDERAL EITC REFORMS

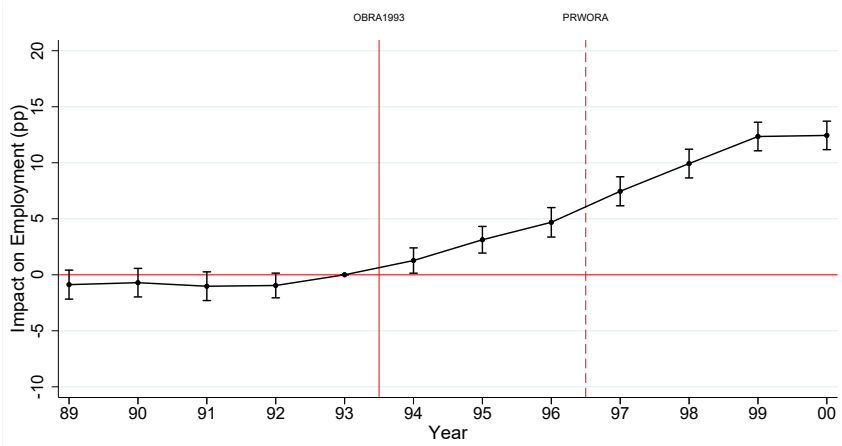
A: 1975 Reform, With vs Without Children



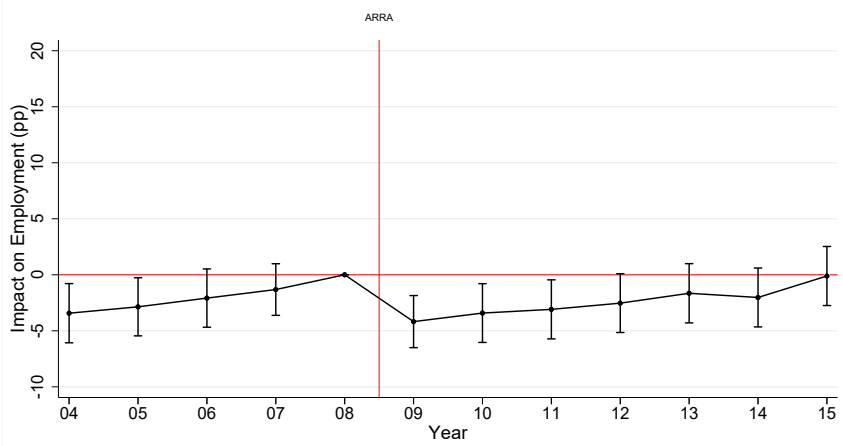
B: 1986 and 1990 Reforms, With vs Without Children



C: 1993 Reform, With vs Without Children

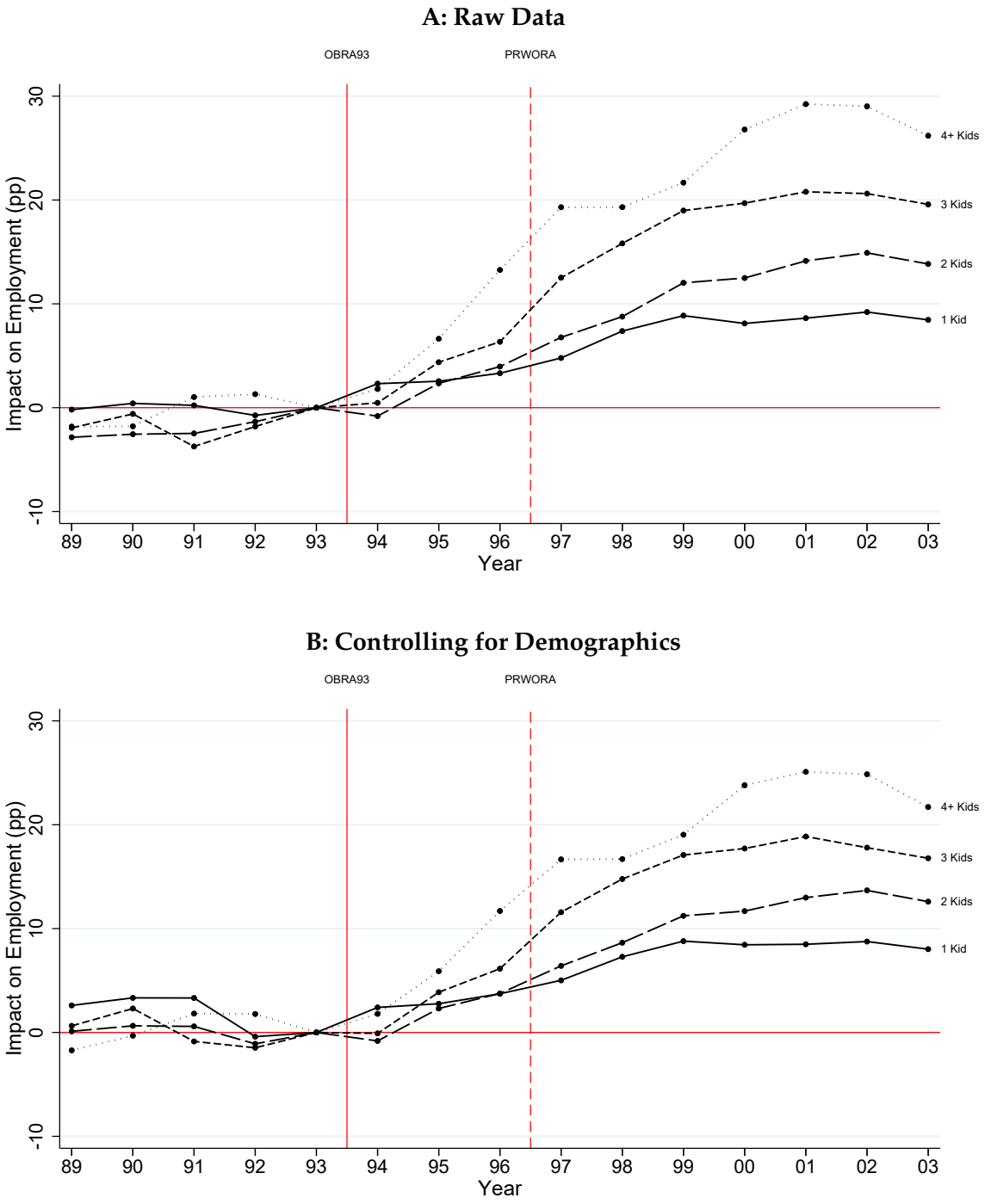


D: 2009 Reform, 3+ vs Without Children



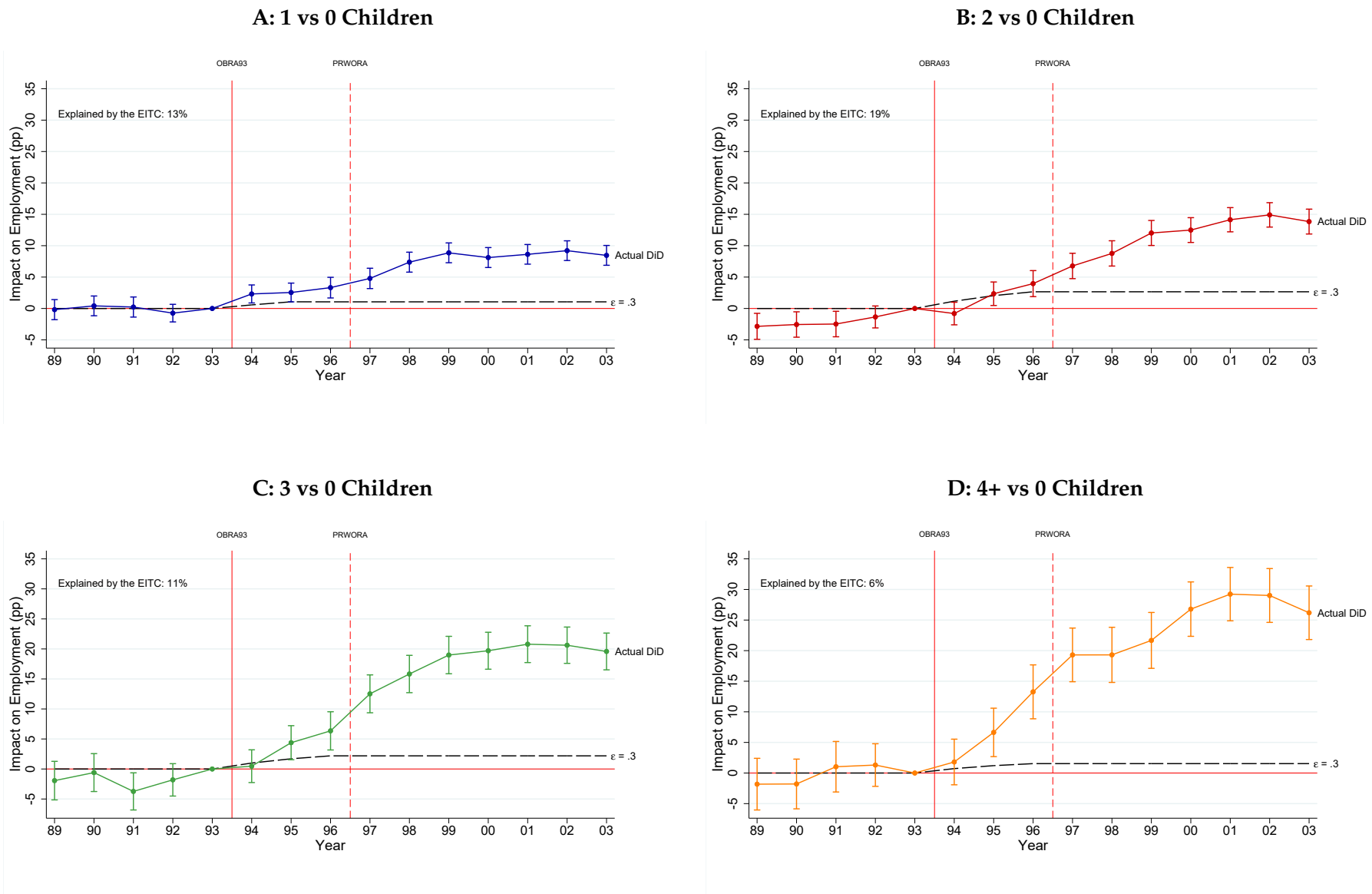
Notes: This figure shows DiD event studies for the five federal EITC reforms. The graphs plot estimates of γ_t based on specification (1) without demographic controls. Panels A-C are based on comparing single women with and without children, while Panel D is based on comparing single women with 3+ children to those without children. In each panel, the difference in the pre-reform year is normalized to zero. The dependent variable is weekly employment. The sample includes single women aged 20-50. Panels A-B use the March CPS files alone, while Panels C-D use the March and monthly files combined. The 95% confidence intervals are based on robust standard errors clustered at the individual level.

FIGURE 7: A FANNING-OUT BY NUMBER OF CHILDREN



Notes: This figure shows DiD event studies for the 1993 reform by number of EITC-eligible children (1, 2, 3, 4+). The graphs plot DiD coefficients γ_t based on an extension of specification (1) that includes dummies for each family size. Hence, each series shows the difference between single mothers with a given number of children and single women without children, normalized to zero in 1993. Panel A shows raw estimates, while panel B controls for demographic composition: dummies for the age of the woman (six categories), dummies for the age of the youngest child (seven categories), and dummies for education (three categories). The dependent variable is weekly employment. The sample includes single women aged 20-50 using the March and monthly CPS files combined.

FIGURE 8: ACTUAL VS SIMULATED RESPONSES TO THE 1993 REFORM

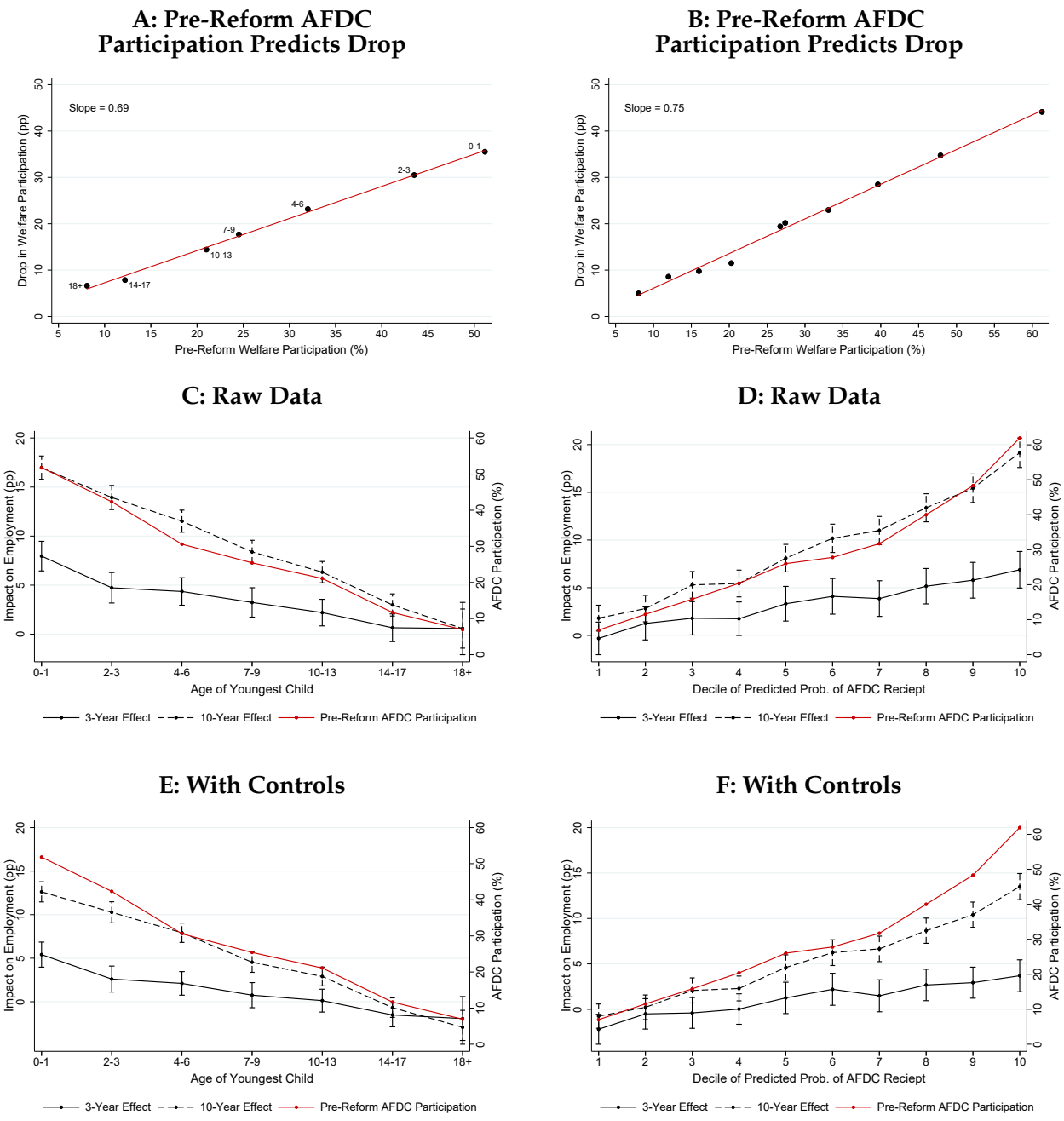


Notes: This figure shows actual and simulated DiD event studies for the 1993 reform, by number of EITC-eligible children. The actual DiD series plot DiD coefficients γ_t based on an extension of specification (1) with separate dummies for each family size. The specification does not include demographic controls and the dependent variable is weekly employment. The simulated DiD series (black dashed lines) plot ΔP_t calculated from equation (4), assuming an elasticity of 0.3. See section C in the appendix for additional details. The fraction explained by the EITC equals the simulated DiD estimate in 2003 divided by the actual DiD estimate in 2003. The sample includes single women aged 20-50 using the March and monthly CPS files combined. The 95% confidence intervals are based on robust standard errors clustered at the individual level.

FIGURE 9: EFFECTS OF THE 1993 EITC REFORM BY WELFARE TREATMENT INTENSITY

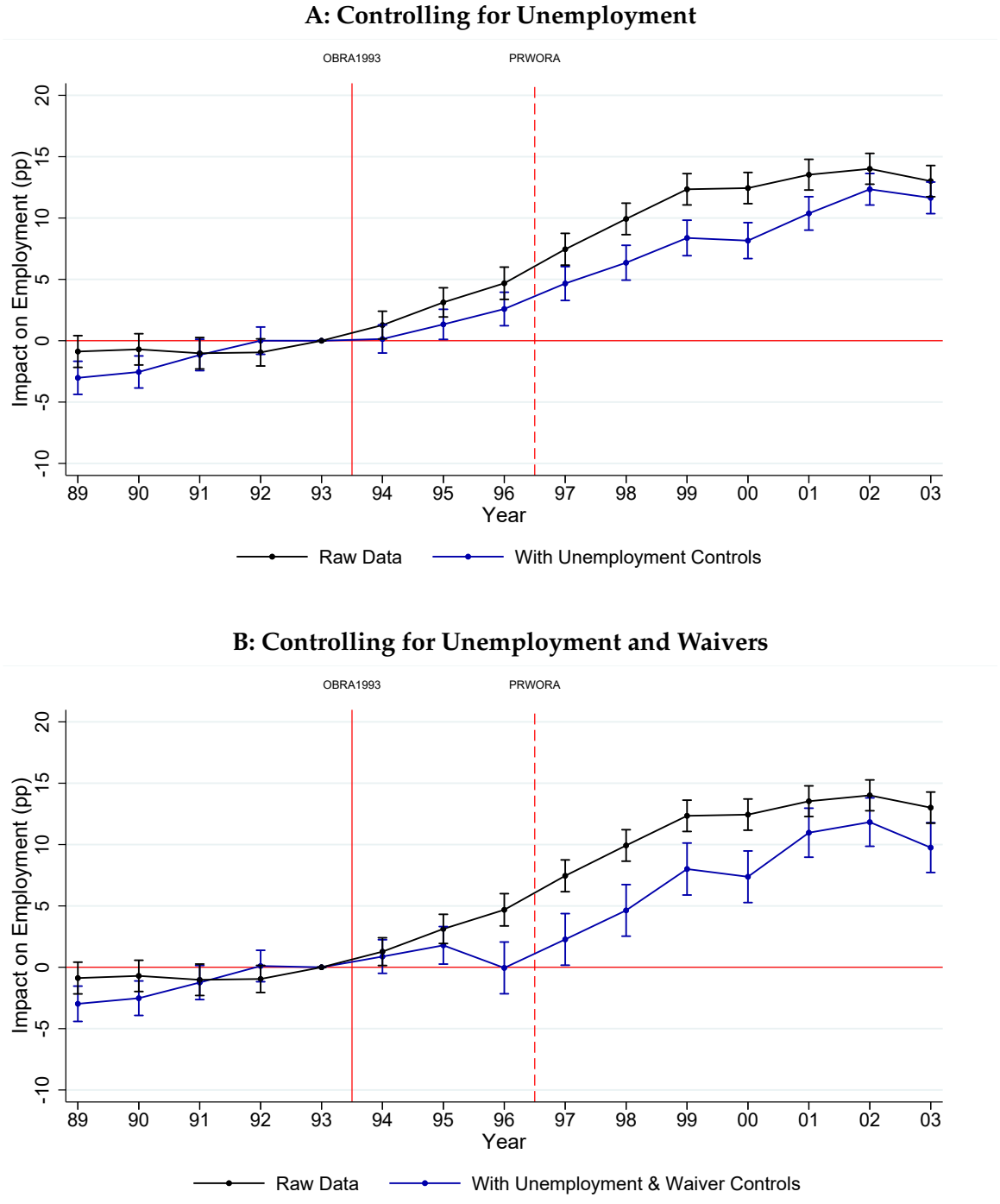
BY AGE OF YOUNGEST CHILD

BY PROBABILITY OF AFDC PARTICIPATION



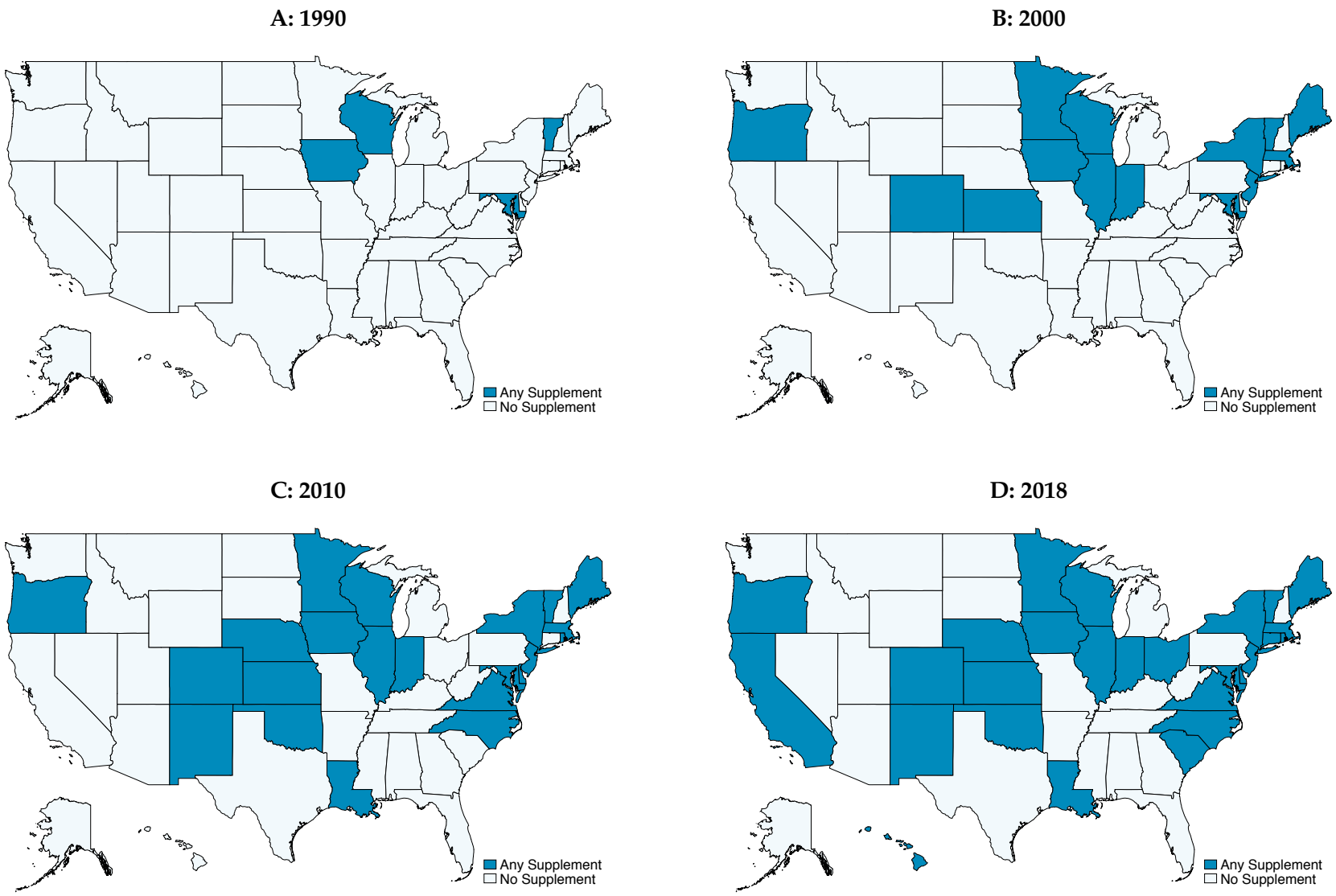
Notes: This figure shows DiD effects of the 1993 reform by welfare treatment intensity, estimated from equation (5). The left panels use the age of youngest child as a proxy for welfare treatment, while the right panels use deciles of a predicted probability of AFDC receipt (estimated from eq. 6) as the proxy for welfare treatment. The top row plots the level of AFDC participation before the reform (1993) against the drop in welfare participation after the reform (1993-2003) across bins of the welfare treatment proxy. The next rows plot the 3-year effects (solid black) and 10-year effects (dashed black) by welfare treatment intensity in the raw data (middle row) and after controlling for business cycle effects and demographics (bottom row). The 3-year and 10-year effects correspond to the coefficient on a 1994-1996 dummy and a 1994-20003 dummy, respectively, The dependent variable is weekly employment. The sample includes single women aged 20-50 using the March and monthly CPS files combined. The 95% confidence intervals are based on robust standard errors clustered at the individual level.

FIGURE 10: HOW MUCH CAN BE EXPLAINED BY THE BUSINESS CYCLE AND WAIVERS?



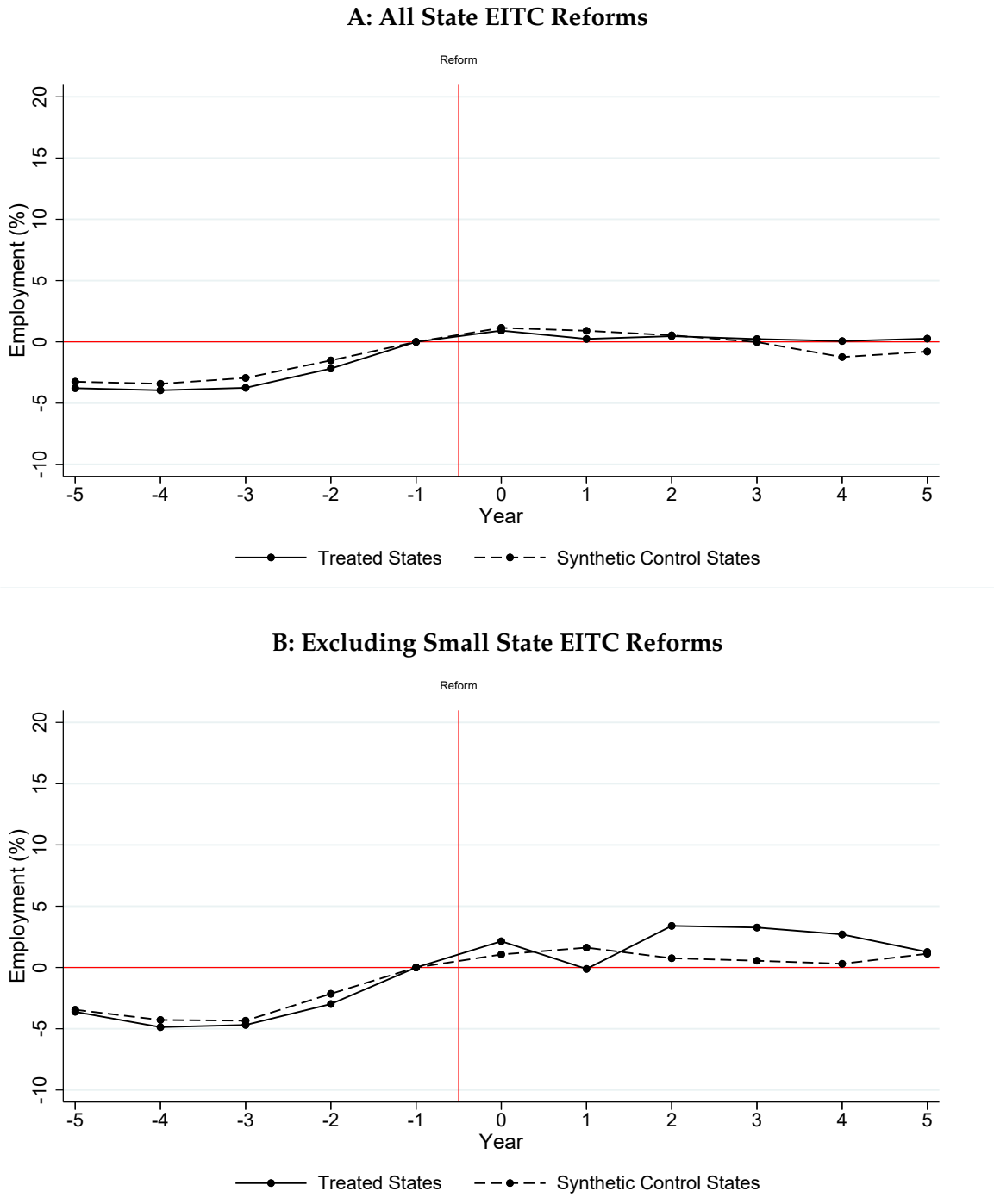
Notes: This figure shows DiD event studies for the 1993 reform with controls for unemployment and waivers. The graphs plot DiD coefficients γ_t based on equation (7). In both panels, the black series show the raw DiD without controls. In panel A, the blue series includes controls for state unemployment by kids and state fixed effects. In panel B the blue series adds controls for waivers by kids. The dependent variable is weekly employment. The sample includes single women aged 20-50 using the March and monthly CPS files combined. The 95% confidence intervals are based on robust standard errors clustered at the individual level.

FIGURE 11: STATES WITH EITC SUPPLEMENTS



Notes: This figure shows which states had an EITC supplement (dark blue) and which states did not have a supplement (light blue) in 1990, 2000, 2010, and 2018.

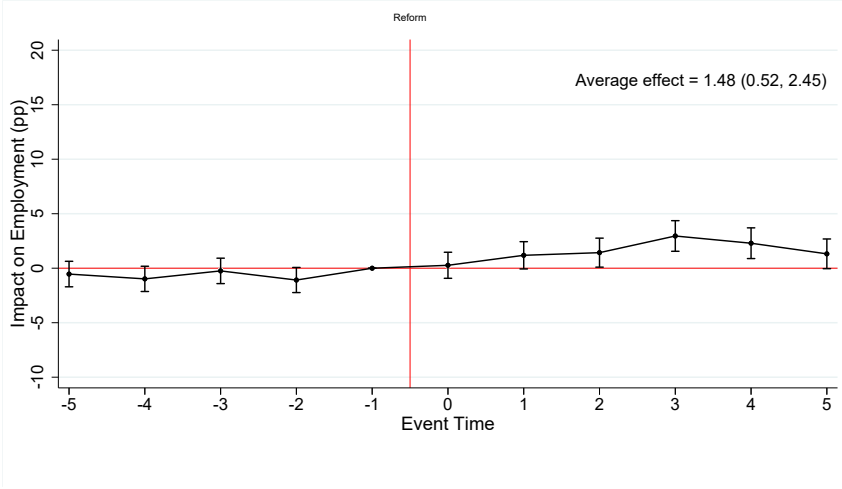
FIGURE 12: SYNTHETIC CONTROL ANALYSIS OF STATE EITC REFORMS



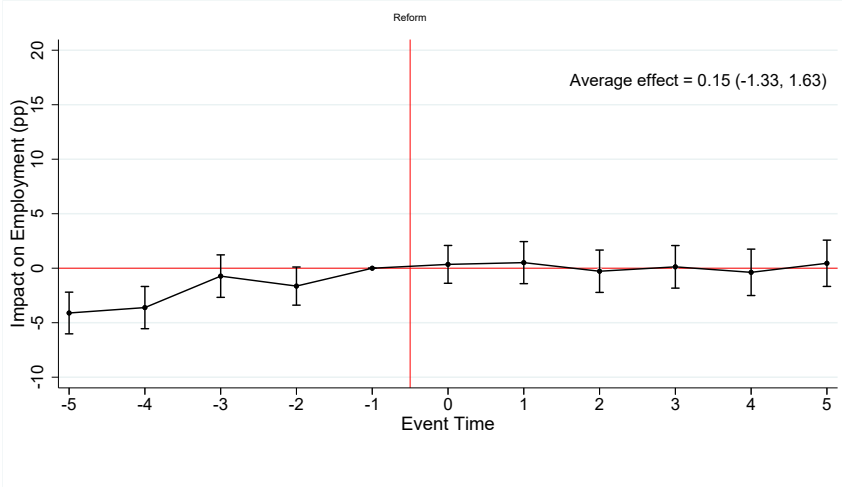
Notes: This figure shows stacked event studies of state EITC reforms using a synthetic control approach. The graphs plot employment rates for single mothers in treatment and synthetic control states across event time, normalized to zero in the last pre-reform year (event time -1). Panel A includes all states that instituted an EITC supplement before 2015, while panel B includes only states that instituted a "large" EITC supplement. Large supplements are defined as refundable credits equal to at least 10% of the federal credit. In both panels, the synthetic control states are constructed from states that never instituted an EITC supplement. For each treatment state, a synthetic control state is constructed by matching on the employment rate in the five pre-reform years. Table A.VI shows the make-up of each synthetic state. For states with supplements enacted before 1993 the sample is based on March CPS files alone, while for states with supplements enacted after 1993 the sample is based on and March and monthly CPS files combined. The outcome is weekly employment and the sample includes single mothers aged 20-50. See section D in the appendix for additional details.

FIGURE 13: STACKED EVENT STUDIES OF ALL STATE AND FEDERAL EITC REFORMS

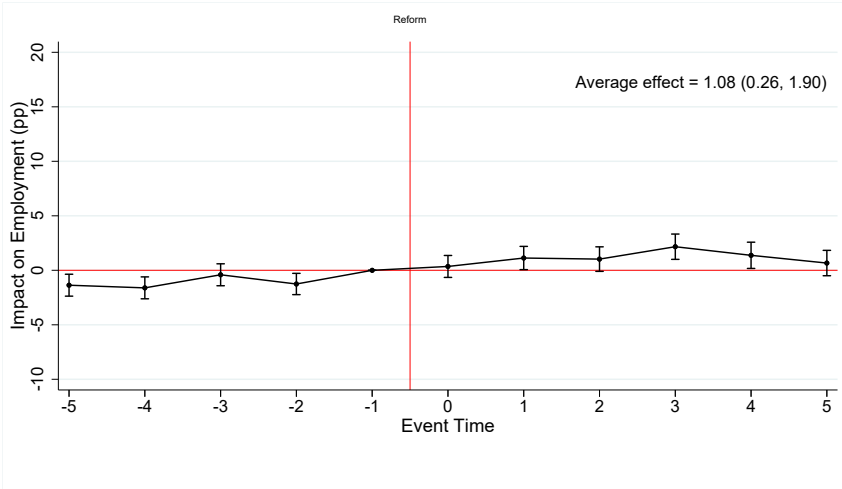
A: Federal Reforms



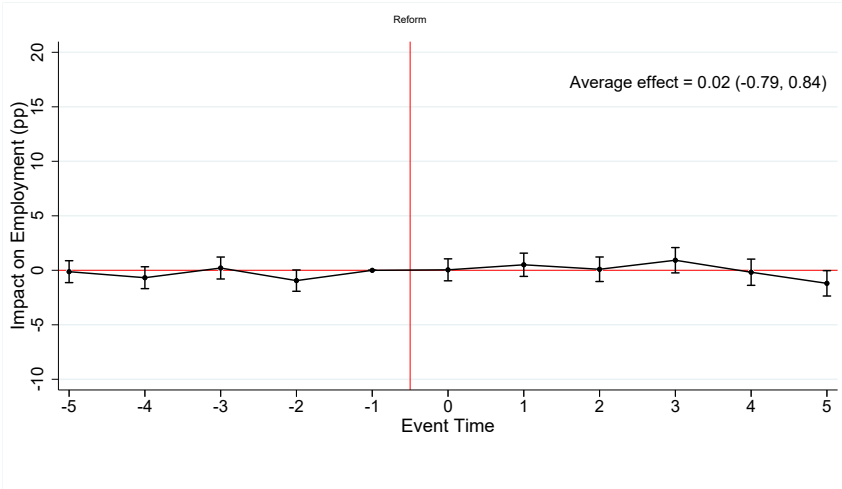
B: State Reforms



C: State and Federal Reforms



D: State and Federal Reforms, Adjusted for Pre-Trends



Notes: This figure shows stacked event studies of all state and federal EITC reforms. The graphs plot DiD coefficients based on comparing single women with and without children across event time, normalized to zero in the pre-reform year (event time -1). The specifications control for demographics, and for the impact of waivers and unemployment around the 1993 federal reform (i.e., the controls in equation (7), interacted with an OBRA93 indicator). Panel A includes all federal reforms, Panel B includes all state reforms. while Panel C includes all state and federal reforms together. Panel D is similar to panel C, but adjusts for group-specific linear pre-trends. Each panel reports the average effect across the post-reform years, with 95% confidence intervals in parentheses. For reforms enacted before 1993 the sample is based on the March CPS files alone, while for reforms enacted after 1993 the sample is based on the March and monthly CPS files combined. The outcome variable is weekly employment and the sample consists of single women aged 20-50. See section E in the appendix for additional details.

Online Appendix (Not for Publication)

A Supplementary Figures and Tables

TABLE A.I: HISTORICAL EITC PARAMETERS

Year	Family Size	Phase-in Rate (%)	First Kink	Max Credit	Second Kink	Phase-out Rate (%)	Exhaustion Point
1975-78	all	10	4,000	400	4,000	10.00	8,000
1979-84	all	10	5,000	500	6,000	12.50	10,000
1985-86	all	11	5,000	550	6,500	12.22	11,000
1987	all	14	6,080	851	6,920	10.00	15,432
1988	all	14	6,240	874	9,840	10.00	18,576
1989	all	14	6,500	910	10,240	10.00	19,340
1990	all	14	6,810	953	10,730	10.00	20,264
1991	1 child	16.7	7,140	1,192	11,250	11.93	21,250
	2+ children	17.3	7,140	1,235	11,250	12.36	21,250
1992	1 child	17.6	7,520	1,324	11,840	12.57	22,370
	2+ children	18.4	7,520	1,384	11,840	13.14	22,370
1993	1 child	18.5	7,750	1,434	12,200	13.21	23,050
	2+ children	19.5	7,750	1,511	12,200	13.93	23,050
1994	0 children	7.65	4,000	306	5,000	7.65	9,000
	1 child	26.3	7,750	2,038	11,000	15.98	23,755
	2+ children	30	8,425	2,528	11,000	17.68	25,296
1995	0 children	7.65	4,100	314	5,130	7.65	9,230
	1 child	34	6,160	2,094	11,290	15.98	24,396
	2+ children	36	8,640	3,110	11,290	20.22	26,673
1996	0 children	7.65	4,220	323	5,280	7.65	9,500
	1 child	34	6,330	2,152	11,610	15.98	25,078
	2+ children	40	8,890	3,556	11,610	21.06	28,495
1997	0 children	7.65	4,340	332	5,430	7.65	9,770
	1 child	34	6,500	2,210	11,930	15.98	25,750
	2+ children	40	9,140	3,656	11,930	21.06	29,290
1998	0 children	7.65	4,460	341	5,570	7.65	10,030
	1 child	34	6,680	2,271	12,260	15.98	26,473
	2+ children	40	9,390	3,756	12,260	21.06	30,095
1999	0 children	7.65	4,530	347	5,670	7.65	10,200
	1 child	34	6,800	2,312	12,460	15.98	26,928
	2+ children	40	9,540	3,816	12,460	21.06	30,580

Year	Family Size	Phase-in Rate (%)	First Kink	Max Credit	Second Kink	Phase-out Rate (%)	Exhaustion Point
2000	0 children	7.65	4,610	353	5,770	7.65	10,380
	1 child	34	6,920	2,353	12,690	15.98	27,413
	2+ children	40	9,720	3,888	12,690	21.06	31,152
2001	0 children	7.65	4,760	364	5,950	7.65	10,710
	1 child	34	7,140	2,428	13,090	15.98	28,281
	2+ children	40	10,020	4,008	13,090	21.06	32,121
2002	0 children	7.65	4,910	376	6,150	7.65	11,060
	1 child	34	7,370	2,506	13,520	15.98	29,201
	2+ children	40	10,350	4,140	13,520	21.06	33,178
2003	0 children	7.65	4,990	382	6,240	7.65	11,230
	1 child	34	7,490	2,547	13,730	15.98	29,666
	2+ children	40	10,510	4,204	13,730	21.06	33,692
2004	0 children	7.65	5,100	390	6,390	7.65	11,490
	1 child	34	7,660	2,604	14,040	15.98	30,338
	2+ children	40	10,750	4,300	14,040	21.06	34,458
2005	0 children	7.65	5,220	399	6,530	7.65	11,750
	1 child	34	7,830	2,662	14,370	15.98	31,030
	2+ children	40	11,000	4,400	14,370	21.06	35,263
2006	0 children	7.65	5,380	412	6,740	7.65	12,120
	1 child	34	8,080	2,747	14,810	15.98	32,001
	2+ children	40	11,340	4,536	14,810	21.06	36,348
2007	0 children	7.65	5,590	428	7,000	7.65	12,590
	1 child	34	8,390	2,853	15,390	15.98	33,241
	2+ children	40	11,790	4,716	15,390	21.06	37,783
2008	0 children	7.65	5,720	438	7,160	7.65	12,880
	1 child	34	8,580	2,917	15,740	15.98	33,995
	2+ children	40	12,060	4,824	15,740	21.06	38,646
2009	0 children	7.65	5,970	457	7,470	7.65	13,440
	1 child	34	8,950	3,043	16,420	15.98	35,463
	2 children	40	12,570	5,028	16,420	21.06	40,295
	3+ children	45	12,570	5,657	16,420	21.06	43,279
2010	0 children	7.65	5,980	457	7,480	7.65	13,460
	1 child	34	8,970	3,050	16,450	15.98	35,535
	2 children	40	12,590	5,036	16,450	21.06	40,363
	3+ children	45	12,590	5,666	16,450	21.06	43,352
2011	0 children	7.65	6,070	464	7,590	7.65	13,660

Year	Family Size	Phase-in Rate (%)	First Kink	Max Credit	Second Kink	Phase-out Rate (%)	Exhaustion Point
	1 child	34	9,100	3,094	16,690	15.98	36,052
	2 children	40	12,780	5,112	16,690	21.06	40,964
	3+ children	45	12,780	5,751	16,690	21.06	43,998
2012	0 children	7.65	6,210	475	7,770	7.65	13,980
	1 child	34	9,320	3,169	17,090	15.98	36,920
	2 children	40	13,090	5,236	17,090	21.06	41,952
	3+ children	45	13,090	5,891	17,090	21.06	45,060
2013	0 children	7.65	6,370	487	7,970	7.65	14,340
	1 child	34	9,560	3,250	17,530	15.98	37,870
	2 children	40	13,430	5,372	17,530	21.06	43,038
	3+ children	45	13,430	6,044	17,530	21.06	46,227
2014	0 children	7.65	6,480	496	8,110	7.65	14,590
	1 child	34	9,720	3,305	17,830	15.98	38,511
	2 children	40	13,650	5,460	17,830	21.06	43,756
	3+ children	45	13,650	6,143	17,830	21.06	46,997
2015	0 children	7.65	6,580	503	8,240	7.65	14,820
	1 child	34	9,880	3,359	18,110	15.98	39,131
	2 children	40	13,870	5,548	18,110	21.06	44,454
	3+ children	45	13,870	6,242	18,110	21.06	47,747
2016	0 children	7.65	6,610	506	8,270	7.65	14,880
	1 child	34	9,920	3,373	18,190	15.98	39,296
	2 children	40	13,931	5,572	18,190	21.06	44,648
	3+ children	45	13,930	6,269	18,190	21.06	47,955
2017	0 children	7.65	6,670	510	8,340	7.65	15,010
	1 child	34	10,000	3,400	18,340	15.98	39,617
	2 children	40	14,040	5,616	18,340	21.06	45,007
	3+ children	45	14,040	6,318	18,340	21.06	48,340
2018	0 children	7.65	6,780	519	8,490	7.65	15,270
	1 child	34	10,180	3,461	18,660	15.98	40,320
	2 children	40	14,290	5,716	18,660	21.06	45,802
	3+ children	45	14,290	6,431	18,660	21.06	49,194

Notes: This table shows federal EITC parameters by family size since the introduction of the program in 1975. The phase-in rate corresponds to the increase in the tax credit for each additional dollar of income. The first kink point is the minimum income needed to maximize the credit. The maximum credit is largest possible EITC amount a family can receive. The second kink point is the maximum income allowed before the credit begins to phase out. The phase-out rate is the reduction in the tax credit for each additional dollar of income above the second kink point. The exhaustion point is the income level at which the EITC is completely phased out.

TABLE A.II: STATE EITC SUPPLEMENTS

State	Year Instituted	At Introduction: % of Fed. Credit	At Introduction: Type	Current: % of Fed. Credit	Current: Type
Alabama	.	.	.	.	.
Alaska	.	.	.	.	.
Arizona	.	.	.	.	.
Arkansas	.	.	.	.	.
California[1]	2015	n/a	R	n/a	R
Colorado[2]	1999	10	R	10	R
Connecticut	2011	25	R	23	R
Delaware	2006	20	NR	20	NR
Dist. of Columbia	2000	25	R	40	R
Florida	.	.	.	.	.
Georgia	.	.	.	.	.
Hawaii	2018	.	.	20	NR
Idaho	.	.	.	.	.
Illinois	2000	5	NR	18	R
Indiana[3]	1999	n/a	NR	9	R
Iowa	1990	6.5	NR	15	R
Kansas	1998	10	R	17	R
Kentucky	.	.	.	.	.
Louisiana	2008	3.5	R	5	R
Maine	2000	5	NR	5	R
Maryland[4]	1987	50	NR	28	R
Massachusetts	1997	10	R	23	R
Michigan	2008	20	R	6	R
Minnesota[5]	1991	n/a	R	n/a	R
Mississippi	.	.	.	.	.
Missouri	.	.	.	.	.
Montana	.	.	.	.	.
Nebraska	2005	8	R	10	R
Nevada	.	.	.	.	.
New Hampshire	.	.	.	.	.
New Jersey	2000	17.5	R	37	R
New Mexico	2007	10	R	10	R
New York	1994	20	R	30	R
North Carolina[6]	2008	5	R	.	.

State	Year Instituted	At Introduction: % of Fed. Credit	At Introduction: Type	Current: % of Fed. Credit	Current: Type
North Dakota	.	.	.	.	.
Ohio	2013	10	NR	10	NR
Oklahoma	2002	5	R	5	NR
Oregon[7]	1997	5	NR	8	R
Pennsylvania	.	.	.	.	.
Rhode Island[8]	1986	25	NR	15	R
South Carolina	2018	125	NR	125	NR
South Dakota	.	.	.	.	.
Tennessee	.	.	.	.	.
Texas	.	.	.	.	.
Utah	.	.	.	.	.
Vermont	1988	28	R	36	R
Virginia	2006	20	NR	20	NR
Washington[9]	2008	n/a	n/a	n/a	n/a
West Virginia	.	.	.	.	.
Wisconsin[10]	1989	5/25/75	R	4/11/34	R
Wyoming	.	.	.	.	.

Notes: This table shows the years in which state EITC supplements were instituted, along with their parameters 3 years after introduction ("at introduction") and in 2018 ("current"). The notation R and NR refers to whether the credit is refundable or non-refundable.

1. California's EITC is not a percentage of the federal EITC, but is based on an independent schedule similar in structure to the federal schedule. In 2018, the maximum California credit was equal to 45 percent of the corresponding maximum federal credit for families with 0, 1, 2, and 3+ children.

2. Colorado's original EITC was contingent upon the state having surplus revenue. In 2015, legislation was enacted that made the credit permanent. Before 2015, it was only paid out between 1999 and 2001.

3. Until 2002, Indiana's EITC was not a percentage of the federal EITC, but was based on an independent schedule similar in structure to the federal schedule. In 2003, Indiana's credit was respecified to be 6 percent of the federal credit and became refundable.

4. Maryland also offers a 50% non-refundable credit that taxpayers can choose in place of the refundable credit.

5. Minnesota's EITC is not a percentage of the federal EITC, but is based on an independent schedule similar in structure to the federal schedule. In 2018, the maximum Minnesota credit was equal to 25, 30, 35, and 31 percent of the maximum federal credit for families with 0, 1, 2, and 3+ children, respectively.

6. North Carolina's credit was eliminated from 2014.

7. Oregon's EITC is 11% of the federal credit for families with children under three.

8. While Rhode Island explicitly enacted a state EITC in 1986, they already had an implicit EITC from the introduction of the federal credit in 1975. This is because, at that time, the Rhode Island income tax was assessed as a percentage of the federal income tax.

9. Washington enacted a state EITC in 2008, but the credit has never been funded or paid out.

10. Wisconsin introduced a non-refundable EITC already in 1984, which was repealed in 1986 and reinstituted in 1989 as a refundable credit. Wisconsin's credit varies by family size. The numbers shown in the table correspond to the credit for 1, 2, and 3+ eligible children, respectively.

TABLE A.III: APPROVAL AND IMPLEMENTATION DATES OF STATEWIDE WAIVERS

State	Termination Time Limits		Work Requirement Time Limits		JOBS Exemptions		JOBS Sanctions		Family Caps		Earnings Disregard	
	Appr	Impl	Appr	Impl	Appr	Impl	Appr	Impl	Appr	Impl	Appr	Impl
Alabama												
Alaska												
Arizona	5-95	11-95				11-95	5-95	11-95	5-95	11-95		
Arkansas									4-94	7-94		
California			9-95	9-95					8-96		10-92	12-92
Colorado												
Connecticut	12-95	1-96			8-94	1-96	8-94	1-96	12-95	1-96	8-94	1-96
Delaware	5-95	10-95	5-95	10-95	5-95	10-95	5-95	10-95	5-95	10-95	5-95	10-95
Dist. of Columbia												
Florida		2-94			6-96				6-96			2-94
Georgia							11-93	1-94	11-93	1-94	6-94	
Hawaii	8-96	2-97			6-94	2-97					8-96	2-97
Idaho					8-96		8-96					
Illinois					9-95		9-95	10-95	9-95	12-95	11-93	11-93
Indiana	12-94	5-95			12-94	5-95	12-94	5-95	12-94	5-95		
Iowa	8-93	10-93			8-93	10-93	8-93	10-93			8-93	10-93
Kansas												
Kentucky												
Louisiana												
Maine					6-96							
Maryland					8-96	10-96	8-96	10-96	8-95	3-96	8-96	10-96
Massachusetts			8-95	11-95	8-95	11-95	8-95	11-95	8-95	11-95	8-95	11-95
Michigan			8-92		10-94	10-94	10-94	10-94			8-92	10-92
Minnesota												
Mississippi									9-95	10-95		
Missouri			4-95				4-95	6-95				

State	Termination Time Limits		Work Requirement Time Limits		JOBS Exemptions		JOBS Sanctions		Family Caps		Earnings Disregard	
	Appr	Impl	Appr	Impl	Appr	Impl	Appr	Impl	Appr	Impl	Appr	Impl
Montana			4-95	2-96	4-95	2-96	4-95	2-96			4-95	2-96
Nebraska	2-95	10-95			2-95	10-95	2-95	10-95	2-95	10-95	2-95	
Nevada												
New Hampshire			6-96		6-96		6-96				6-96	
New Jersey					7-92	10-92	7-92	10-92	7-92	10-92	7-92	
New Mexico												
New York												
North Carolina	2-96	7-96			2-96	7-96	2-96	7-96	2-96	7-96		
North Dakota								7-96				10-96
Ohio	3-96						3-96	7-96			3-96	7-96
Oklahoma												
Oregon	3-96	7-96			7-92	2-93	3-96	7-95				
Pennsylvania												
Rhode Island												
South Carolina	5-96				5-96		5-96		5-96			
South Dakota			3-94	6-94			3-94	6-94				
Tennessee	7-96	10-96			7-96	9-96	7-96	9-96	7-96	9-96	7-96	9-96
Texas	3-96	6-96			3-96	6-96	3-96	6-96				
Utah					10-92	1-93	10-92	1-93			10-92	1-93
Vermont			4-93	7-94	4-93	7-94	4-93	7-94			4-93	7-94
Virginia	7-95	7-95	7-95	7-95	7-95	7-95	7-95	7-95	7-95	7-95	7-95	7-95
Washington	9-95	1-96										
West Virginia							7-95	2-96				
Wisconsin			9-96	9-96	8-95	1-96	8-95	1-96	6-94	1-96		
Wyoming												

Source: Department of Health and Human Services, Assistant Secretary for Planning and Evaluation (1997). *Setting the Baseline: A Report on State Welfare Waivers*.

Notes: This table shows dates of approval and implementation for the six main types of statewide welfare waivers. For waivers that were rolled out at the county level, dates of implementation correspond to the date the first county implemented the reform.

TABLE A.IV: MAXIMUM MONTHLY AFDC BENEFITS IN 1993 FOR SINGLE MOTHERS, BY NUMBER OF CHILDREN

	Monthly Benefit in 1993 (2018 USD)						
State	One Child	Two Children	Three Children	Four Children	Five Children	Six Children	Seven Children
Alabama	238	285	337	391	438	499	547
Alaska	1427	1604	1781	1958	2136	2313	2490
Arizona	478	603	726	850	975	1098	1222
Arkansas	282	355	429	497	575	648	721
California	852	1055	1256	1432	1609	1767	1925
Colorado	488	620	751	891	1027	1135	1243
Connecticut	822	1010	1187	1357	1536	1733	1915
Delaware	469	587	707	825	945	1064	1183
Dist. of Columbia	573	730	891	1027	1208	1385	1531
Florida	419	527	633	740	846	954	1060
Georgia	408	487	573	657	712	772	817
Hawaii	982	1237	1493	1748	2004	2259	2513
Idaho	436	551	664	779	891	1006	1121
Illinois	466	638	719	845	947	997	1050
Indiana	398	500	601	704	805	907	1008
Iowa	627	740	860	952	1060	1164	1270
Kansas	612	746	864	970	1076	1182	1288
Kentucky	341	396	495	579	653	728	728
Louisiana	240	330	407	481	549	612	679
Maine	542	726	914	1098	1284	1470	1656
Maryland	497	636	766	888	977	1098	1208
Massachusetts	845	1006	1161	1321	1484	1644	1802
Michigan	645	798	978	1145	1376	1508	1640
Minnesota	759	924	1079	1211	1343	1477	1592
Mississippi	167	209	250	292	334	375	417
Missouri	407	507	594	674	749	824	893
Montana	553	697	841	985	1130	1272	1418

State	Monthly Benefit in 1993 (2018 USD)						
	One Child	Two Children	Three Children	Four Children	Five Children	Six Children	Seven Children
Nebraska	509	633	756	879	1003	1126	1249
Nevada	500	605	709	813	916	1020	1124
New Hampshire	836	956	1065	1170	1310	1420	1583
New Jersey	560	737	848	959	1070	1176	1265
New Mexico	492	620	749	876	1004	1133	1262
New York	813	1003	1194	1390	1536	1755	1913
North Carolina	410	473	516	563	606	648	671
North Dakota	579	711	871	989	1091	1163	1237
Ohio	485	593	732	857	954	1065	1182
Oklahoma	436	563	699	817	935	1053	1157
Oregon	686	799	982	1147	1312	1460	1607
Pennsylvania	549	700	864	1024	1164	1309	1453
Rhode Island	780	963	1098	1234	1390	1529	1686
South Carolina	276	348	417	488	558	629	700
South Dakota	660	747	831	918	1004	1090	1173
Tennessee	247	321	393	459	530	600	671
Texas	275	320	384	427	494	535	610
Utah	577	719	841	958	1055	1105	1157
Vermont	911	1093	1234	1390	1491	1661	1809
Virginia	401	506	603	712	756	756	756
Washington	765	949	1116	1286	1461	1687	1868
West Virginia	349	433	542	626	718	801	829
Wisconsin	765	900	1074	1230	1331	1441	1527
Wyoming	556	626	678	782	886	999	1112
Median	509	636	756	888	1004	1105	1222

Source: U.S. House of Representatives, Committee on Ways and Means (1993). *Green Book: Background Material and Data on Programs within the Jurisdiction of the Committee on Ways and Means*.

Notes: This table shows the AFDC benefit for families with zero countable income by number of children and state in 1993. For states whose benefits vary across counties, the most generous benefit is listed. All families include 1 adult caretaker.

TABLE A.V: EXTENSIVE MARGIN ELASTICITIES WHEN IGNORING CONFOUNDERS
DEMOGRAPHIC CONTROLS

	Earnings and Tax Parameters			Employment Effects			Participation Effects		
	Earnings	τ	$\Delta(1-\tau)$	P	ΔP	ε	P	ΔP	ε
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Any Children:	14,685	0.283	0.077	0.606	0.113	**1.8**	0.690	0.115	**1.6**
1 Child:	16,197	0.338	0.034	0.680	0.079	**2.2**	0.756	0.079	**2.0**
2 Children	14,703	0.280	0.105	0.610	0.124	**1.4**	0.697	0.126	**1.2**
3 Children:	12,110	0.192	0.124	0.474	0.164	**2.3**	0.570	0.175	**2.0**
4+ Children:	8,327	0.067	0.159	0.300	0.211	**4.1**	0.400	0.208	**3.0**

Notes: This table shows estimates of the extensive margin elasticities based on the 1993 reform, assuming that the entire DiD effect between 1993-2003 (controlling for demographic changes) can be attributed to the EITC. Columns (1)-(3) show predicted earnings and tax parameters, columns (4)-(6) show employment effects, and columns (7)-(9) show participation effects. Each statistic is shown for all single mothers in the first row and separately by number of children in the following rows. The earnings measure in column (1) is based on predicted earnings for non-workers (estimated using equation 3) and actual earnings for workers. The changes in employment and participation rates (ΔP) as well as the EITC-induced change in the net-of-tax rate ($\Delta(1-\tau)$) represent difference-in-differences comparing single women with and without children. The elasticities in columns (6) and (9) are calculated using equation (2). See section C in the appendix for additional details.

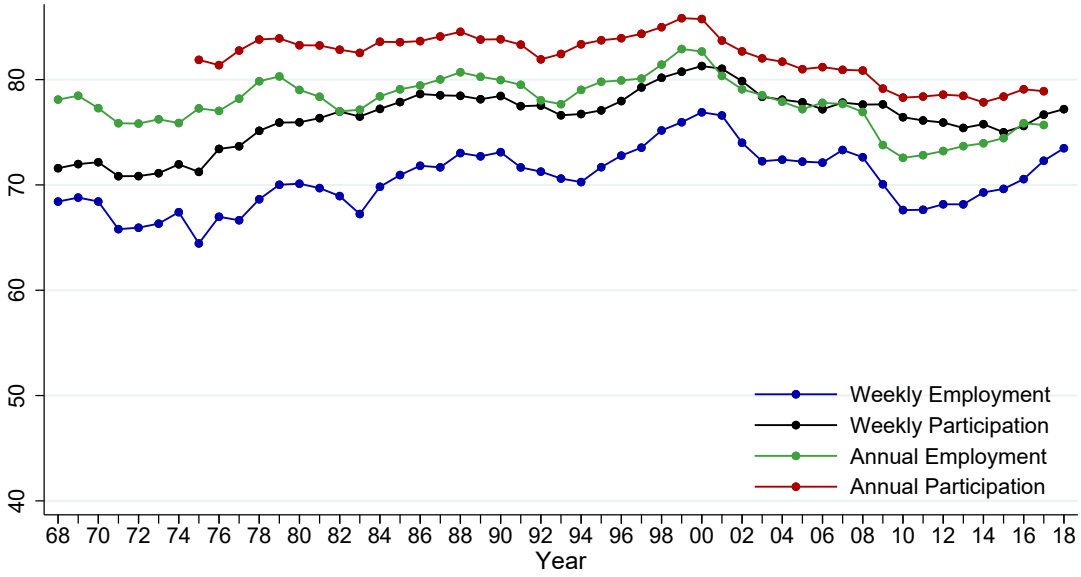
TABLE A.VI: SYNTHETIC EITC STATES

Treated State	Synthetic State
California	49.4% KY, 27.5% UT, 17.8% WV, 4.0% TN, 1.3% WY
Colorado	69.4% SD, 30.6% ID
Connecticut	34.0% SD, 31.3% ND, 27.5% ID, 7.2% NH
Delaware	67.6% MT, 20.7% WY, 8.7% NV, 3.0% UT
Dist. Of Columbia	86.2% WV, 13.8% PA
Illinois	49.2% WA, 17.8% MT, 14.9% PA, 12.1% AK, 5.9% MO
Indiana	71.8% NH, 24.6% UT, 3.3% WV, 0.3% WY
Iowa	51.4% PA, 21.4% MT, 18.2% TX, 8.9% AL
Kansas	52.9% ID, 27.5% UT, 19.5% ND
Louisiana	58.5% MS, 22.4% GA, 19.1% AK
Maine	50.1% UT, 30.0% MT, 18.2% AK, 1.3% AZ, 0.4% ID
Maryland	56.3% MO, 26.5% NV, 17.1% TN
Massachusetts	59.1% PA, 30.4% WV, 10.5% TN
Michigan	46.6% TX, 26.1% AL, 17.6% AK, 9.8% WV
Minnesota	35.9% PA, 23.8% AL, 17.3% WA, 16.8% MT, 6.2% TX
Nebraska	37.9% NH, 30.6% WY, 22.5% UT, 9.0% MT
New Jersey	80.7% PA, 19.3% MO
New Mexico	48.4% AR, 20.0% KY, 15.1% TN, 14.7% FL, 1.8% MS
New York	87.3% WV, 12.7% PA
North Carolina	34.0% SD, 29.5% MS, 17.1% PA, 12.0% AR, 7.4% FL
Ohio	48.7% ID, 27.7% WV, 22.9% UT, 0.8% NV
Oklahoma	44.1% SD, 26.7% WV, 15.7% AK, 12.0% UT, 1.6% WY
Oregon	9.9% SD, 9.6% TN, 9.4% ND, 7.1% UT, 6.1% WV, 57.9% Other
Rhode Island	52.1% WV, 19.9% WA, 14.0% MS, 13.6% TN, 0.4% WY
Vermont	34.6% AR, 33.9% AZ, 31.5% PA
Virginia	80.3% NH, 11.1% WY, 8.6% UT
Wisconsin	43.1% PA, 29.9% FL, 27.0% MT

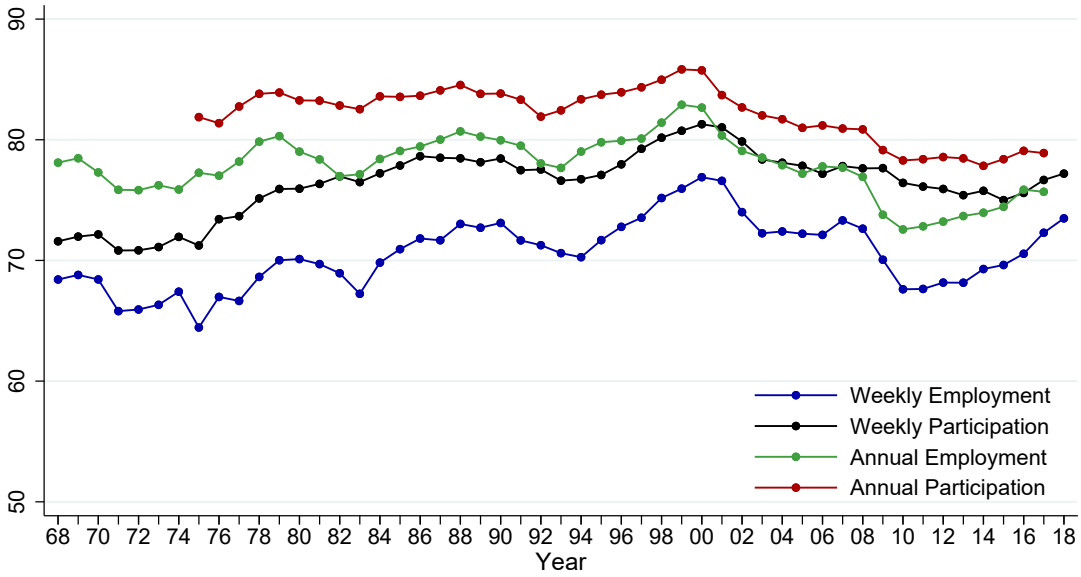
Notes: This table shows how synthetic EITC states in figure (12) are constructed. Each synthetic state is calculated as a linear combination of the set of control states. Values are independently rounded and may not add up to 100%. For synthetic states with more than six control states, remaining states are grouped into an "other" category.

FIGURE A.I: EXTENSIVE MARGIN MEASURES

A: All Women, 20-50

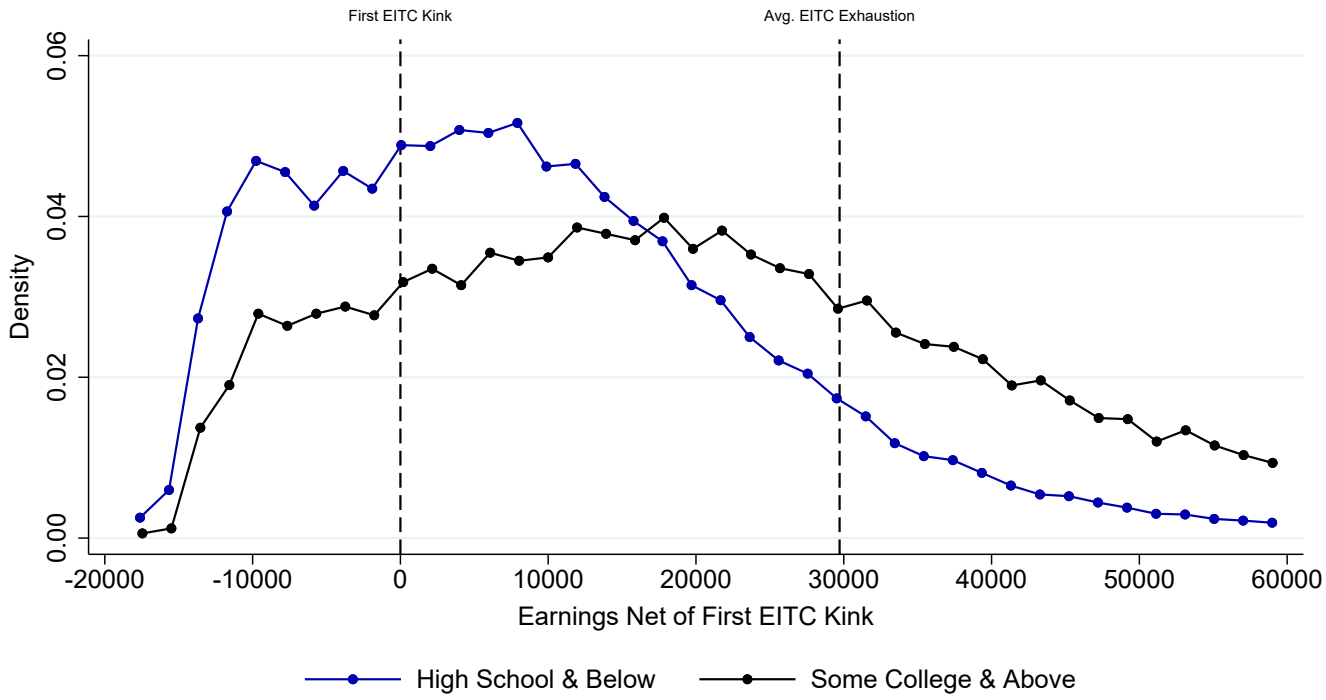


B: Single Women, 20-50



Notes: This figure shows the weekly employment, weekly participation, annual employment, and annual participation of women (panel A) and single women (panel B) between 1968 and 2018. See section B.1 for additional details. The sample includes women aged 20-50 using the March CPS files.

FIGURE A.II: EARNINGS DISTRIBUTION OF SINGLE WOMEN WITH CHILDREN

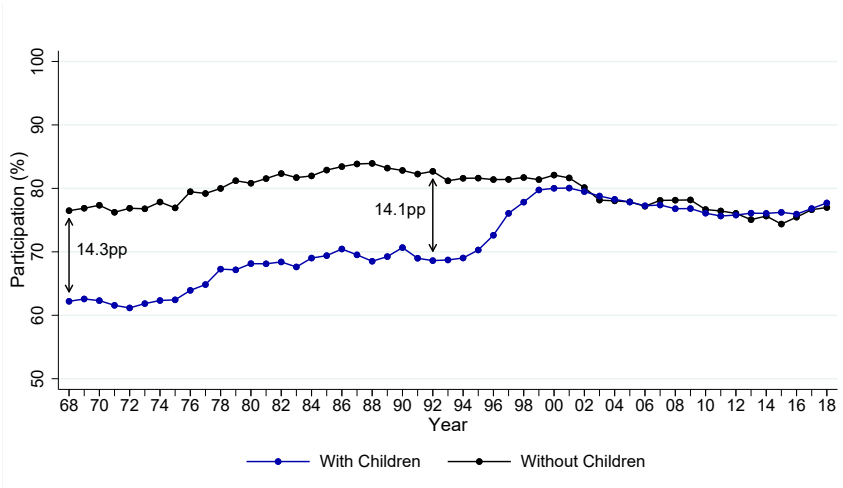


Notes: This figure plots the pooled earnings distribution, from 1975 to 2018, net of the first EITC kink for single mothers of different education levels. All dollar values are in 2018 USD. EITC kink is measured in the same year as earnings. The average EITC exhaustion line corresponds to the average point of EITC exhaustion, relative to the first EITC kink, across all years and observations in the sample. Distributions are divided into 40 quantiles and are plot separately for women with a high school degree or less and with any college education and above. The sample includes single women aged 20-50 using March CPS files alone.

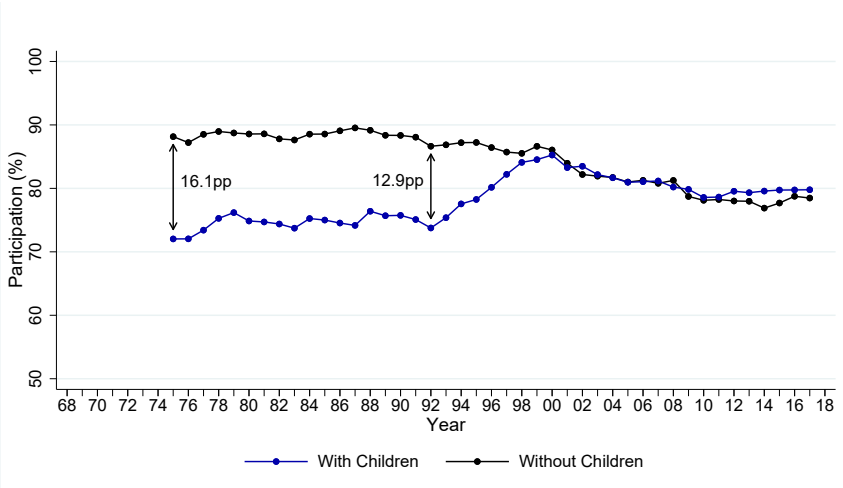
FIGURE A.III: FIFTY YEARS OF LABOR FORCE PARTICIPATION FOR SINGLE WOMEN

DIFFERENT EXTENSIVE MARGIN MEASURES

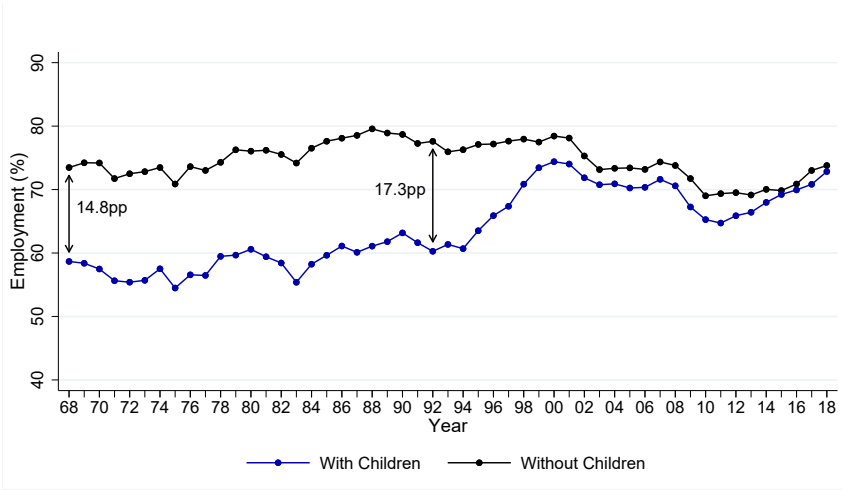
A: Weekly Participation



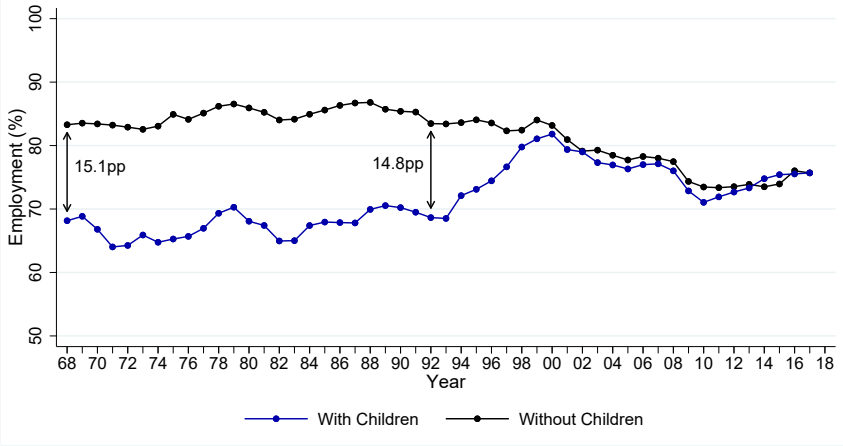
B: Annual Participation



C: Weekly Employment



D: Annual Employment

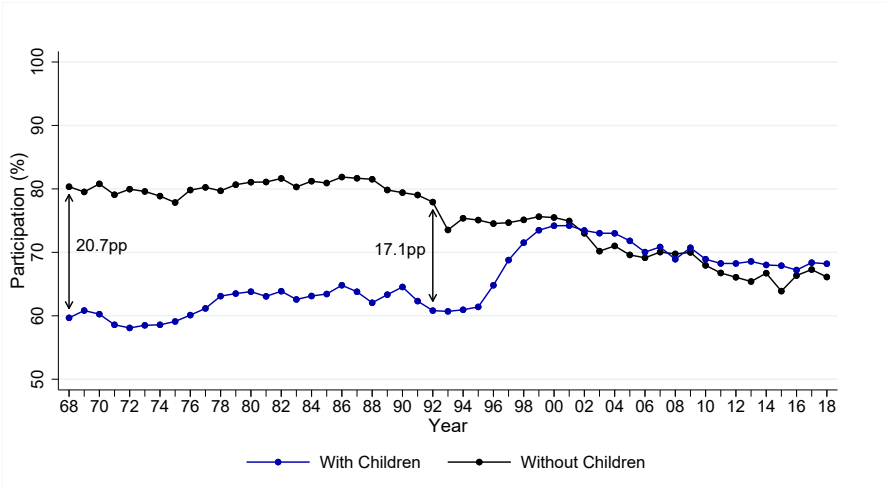


Notes: This figure shows the weekly participation, annual participation, weekly employment, and annual employment rates rate of single women with and without children between 1968 and 2018. The sample includes single women aged 20-50 using the March CPS files.

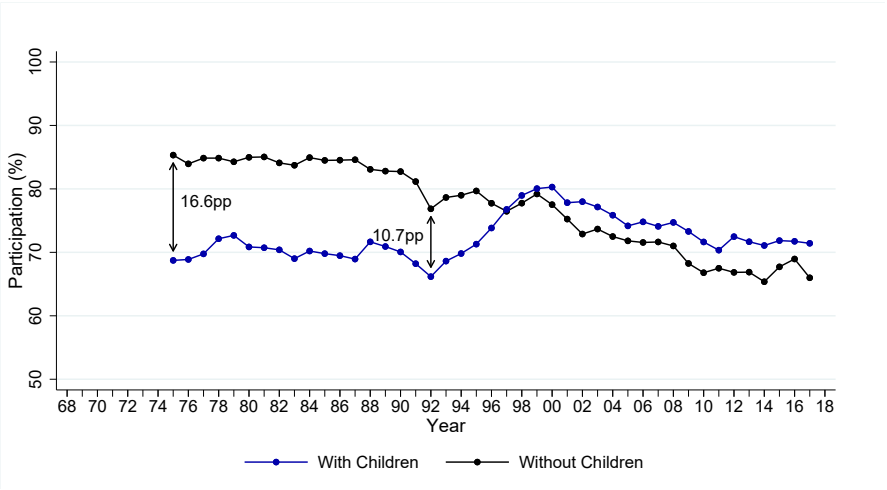
FIGURE A.IV: FIFTY YEARS OF LABOR FORCE PARTICIPATION FOR SINGLE WOMEN

DIFFERENT EXTENSIVE MARGIN MEASURES, LOW-EDUCATED

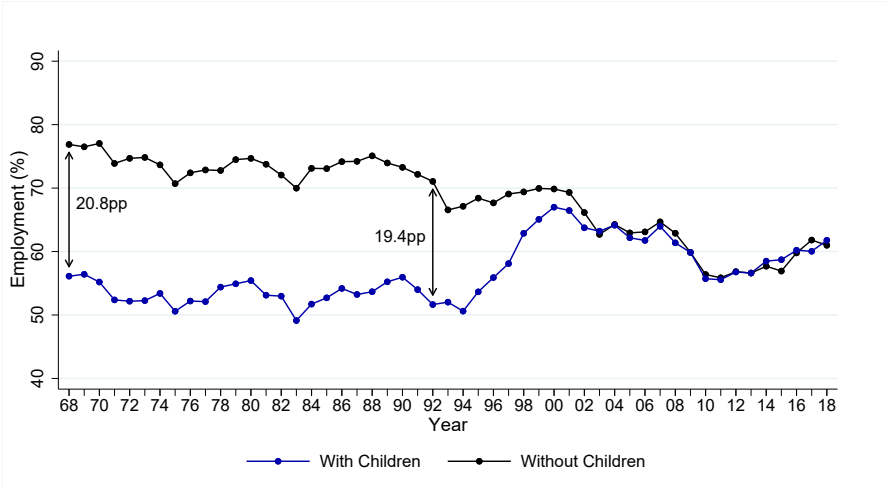
A: Weekly Participation



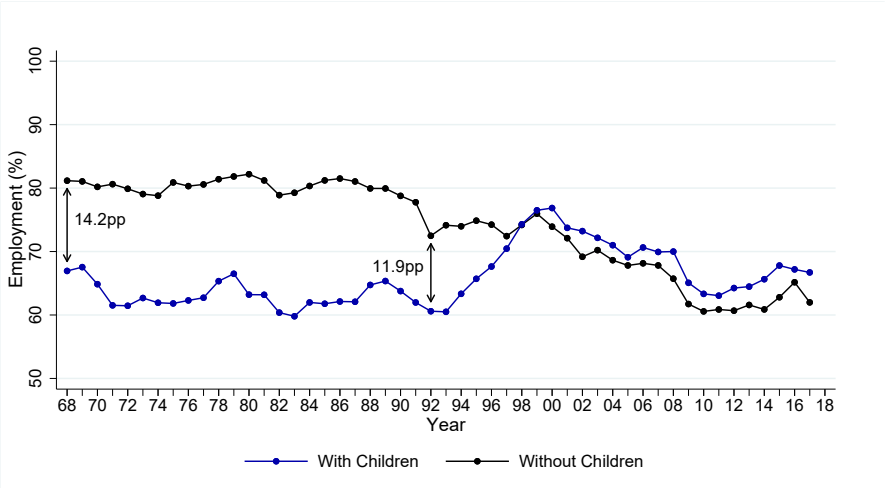
B: Annual Participation



C: Weekly Employment



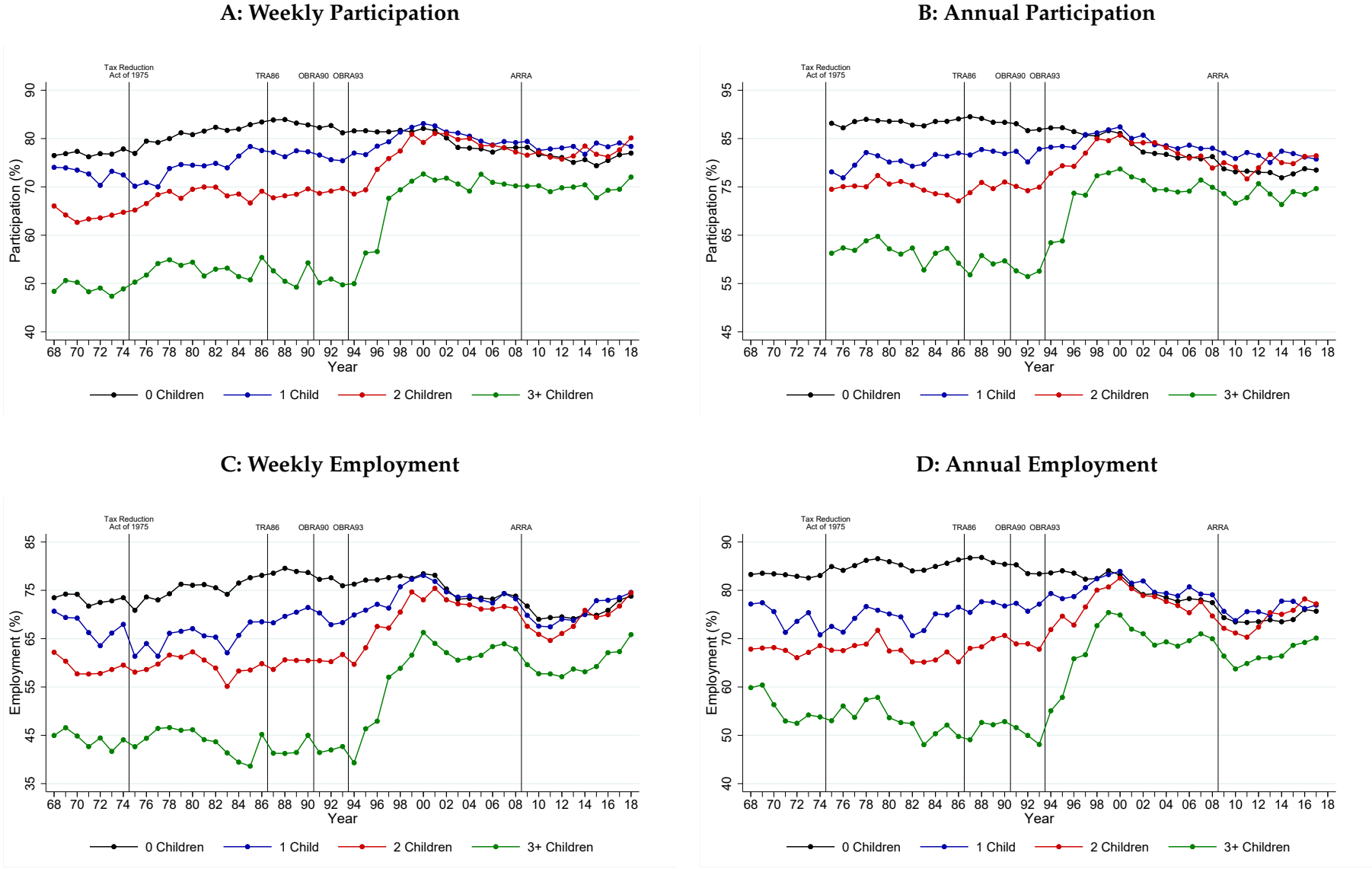
D: Annual Employment



Notes: This figure shows the weekly participation, annual participation, weekly employment, and annual employment rates rate of single women with and without children with a high school degree or less between 1968 and 2018. The sample includes single women aged 20-50 using the March CPS files.

FIGURE A.V: FIFTY YEARS OF PARTICIPATION FOR SINGLE WOMEN, BY FAMILY SIZE

DIFFERENT EXTENSIVE MARGIN MEASURES

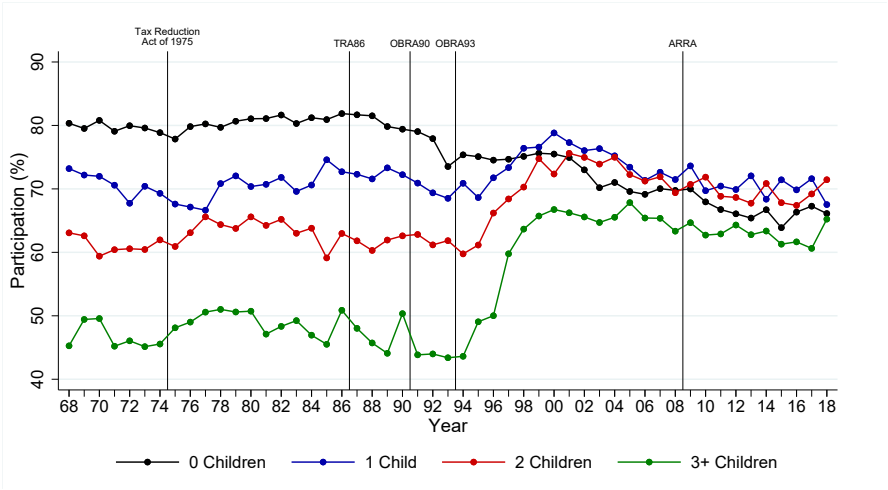


Notes: This figure shows the weekly participation, annual participation, weekly employment, and annual employment rates rate of single women with 0,1, 2, and 3 or more children between 1968 and 2018. The sample includes single women aged 20-50 using the March CPS files.

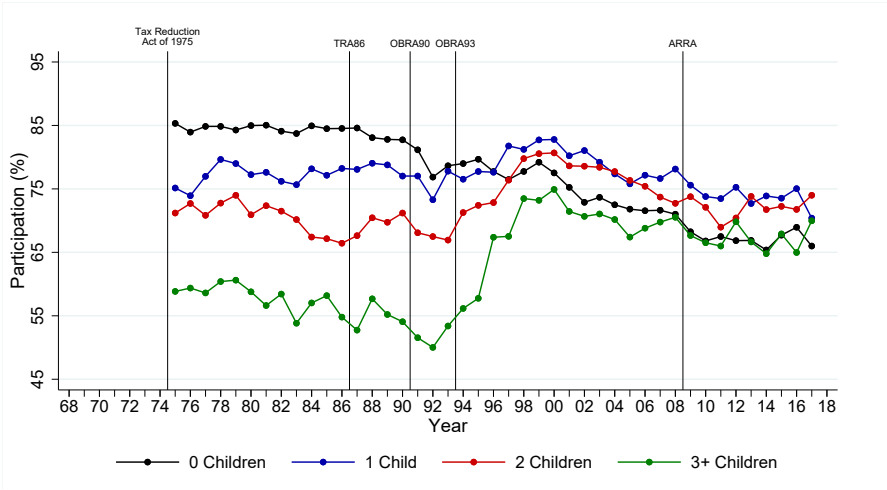
FIGURE A.VI: FIFTY YEARS OF PARTICIPATION FOR SINGLE WOMEN, BY FAMILY SIZE

DIFFERENT EXTENSIVE MARGIN MEASURES, LOW-EDUCATED

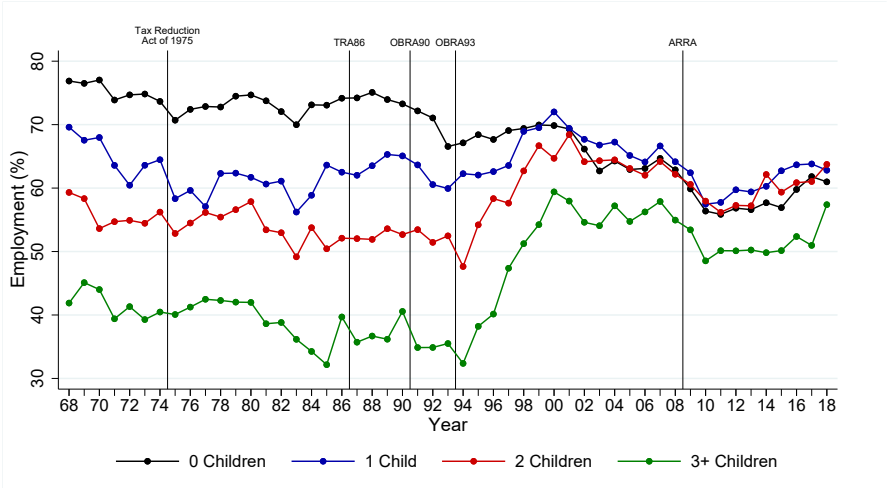
A: Weekly Participation



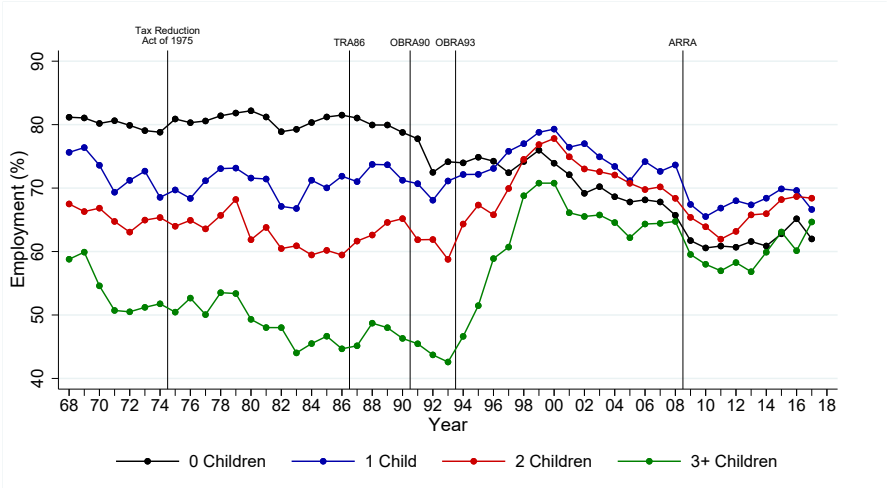
B: Annual Participation



C: Weekly Employment

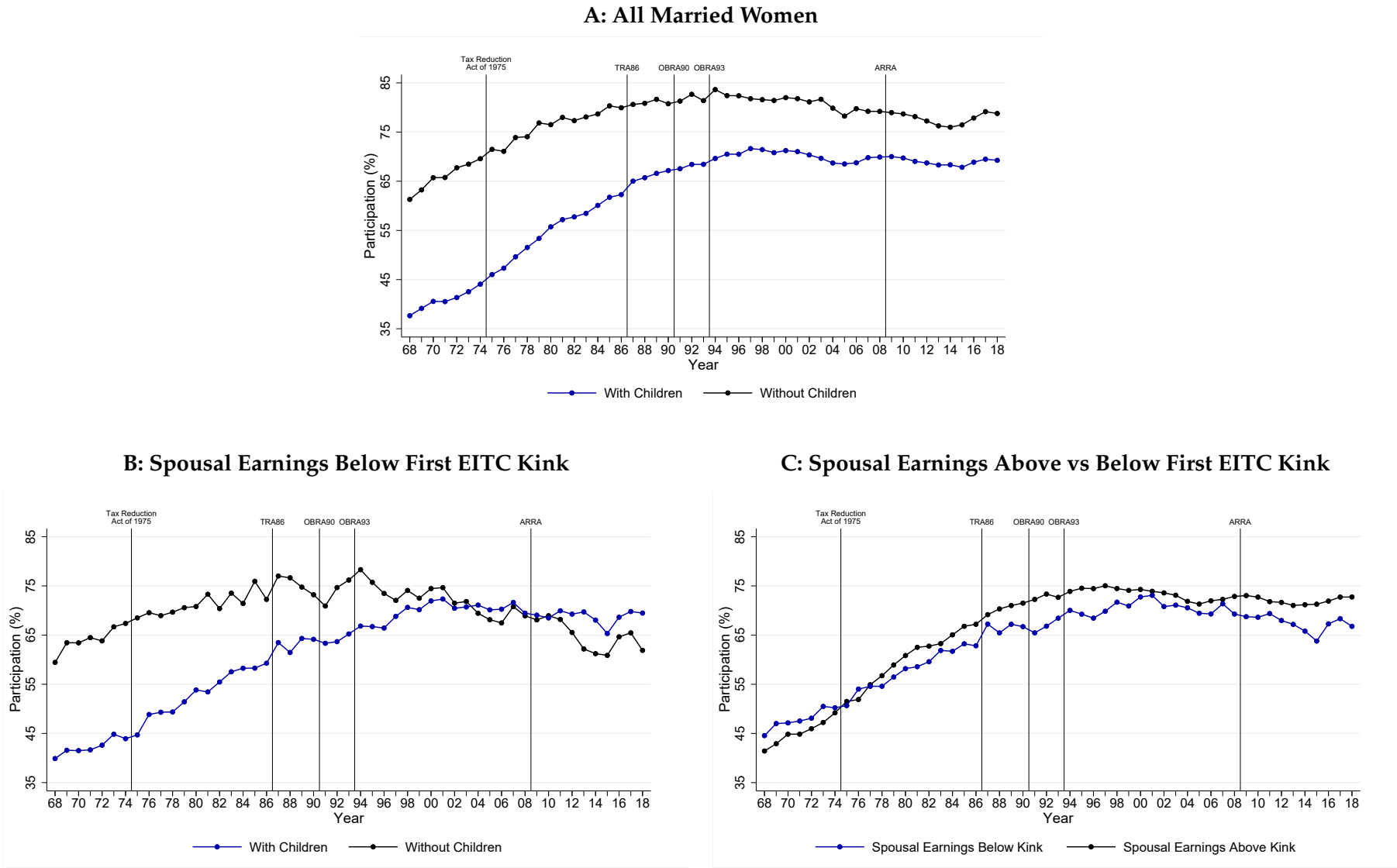


D: Annual Employment



Notes: This figure shows the weekly participation, annual participation, weekly employment, and annual employment rates rate of single women with 0, 1, 2, and 3 or more with a high school degree or less between 1968 and 2018. The sample includes single women aged 20-50 using the March CPS files.

FIGURE A.VII: FIFTY YEARS OF PARTICIPATION FOR MARRIED WOMEN

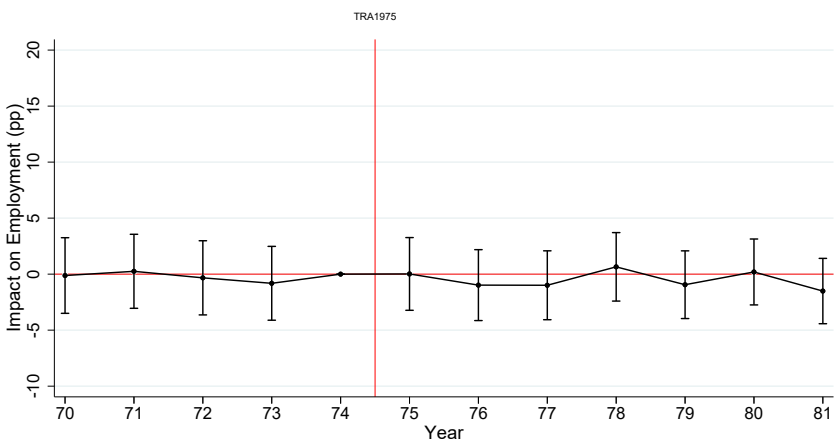


Notes: This figure shows weekly labor force participation of married women between 1968 and 2018. The sample includes married women aged 20-50 using the March CPS files. Panel A compares married women with and without children. Panel B conditions on having spousal earnings below the first EITC kink (for one-child families), and again compares married women with and without children. Panel C conditions on having children, and compares married women with spousal earnings below and above the first EITC kink. Because the EITC is based on joint household income, having spousal earnings below or above the first EITC kink determines whether the policy creates a positive or negative extensive margin incentive. The fact that the two participation series in Panel C track each other over time suggests that there has been no extensive margin impact on married women.

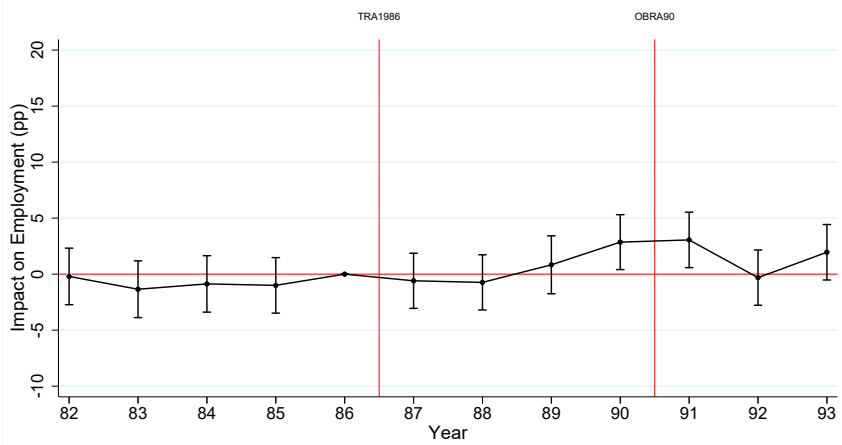
FIGURE A.VIII: DID EVENT STUDIES OF FEDERAL ALL EITC REFORMS

WEEKLY EMPLOYMENT (DEMOGRAPHIC CONTROLS)

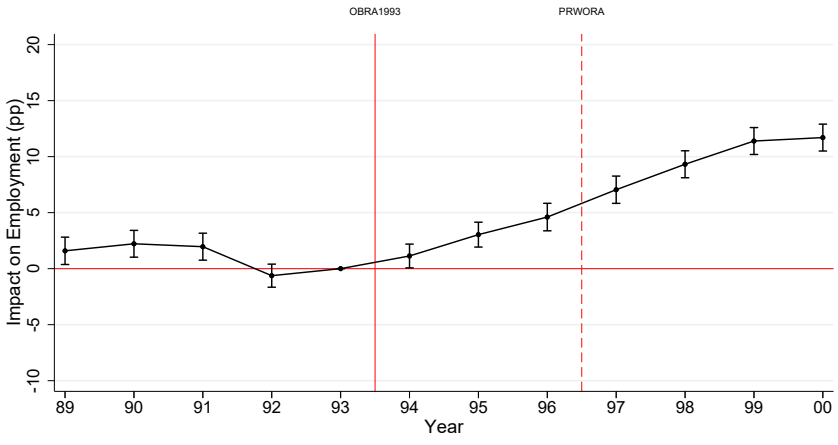
A: 1975 Reform, With vs Without Children



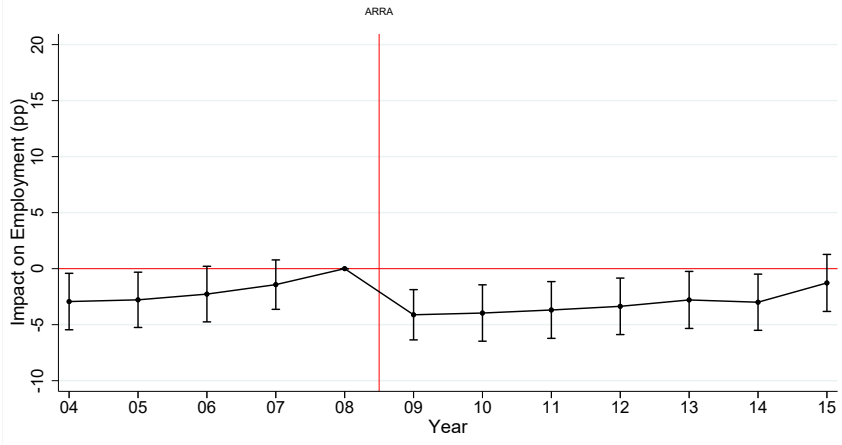
B: 1986 and 1990 Reforms, With vs Without Children



C: 1993 Reform, With vs Without Children



D: 2009 Reform, 3+ vs Without Children

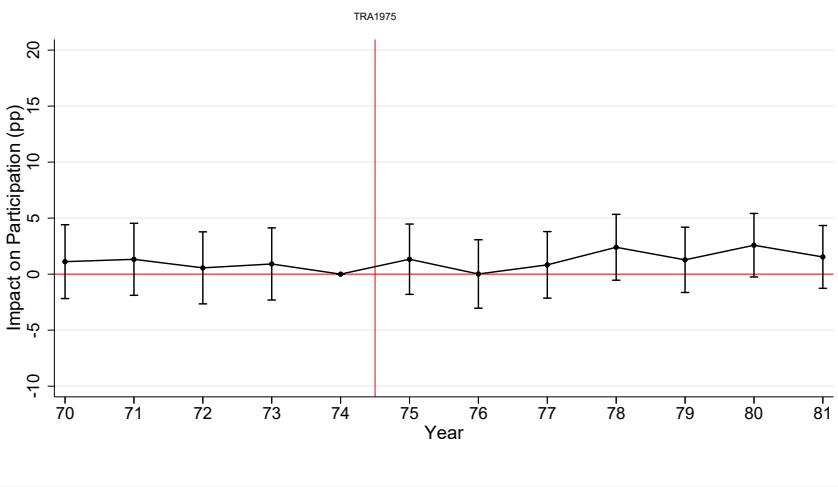


Notes: This figure shows DiD event studies for the five federal EITC reforms. The graphs plot estimates of γ_t based on specification (1) that includes controls for demographic composition: dummies for the age of the woman (six categories), dummies for the age of the youngest child (seven categories), and dummies for education (three categories).. Panels A-C are based on comparing single women with and without children, while Panel D is based on comparing single women with 3+ children to those without children. In each panel, the difference in the pre-reform year is normalized to zero. The dependent variable is weekly employment. The sample includes single women aged 20-50. Panels A-B use the March CPS files alone, while Panels C-D use the March and monthly files combined. The 95% confidence intervals are based on robust standard errors clustered at the individual level.

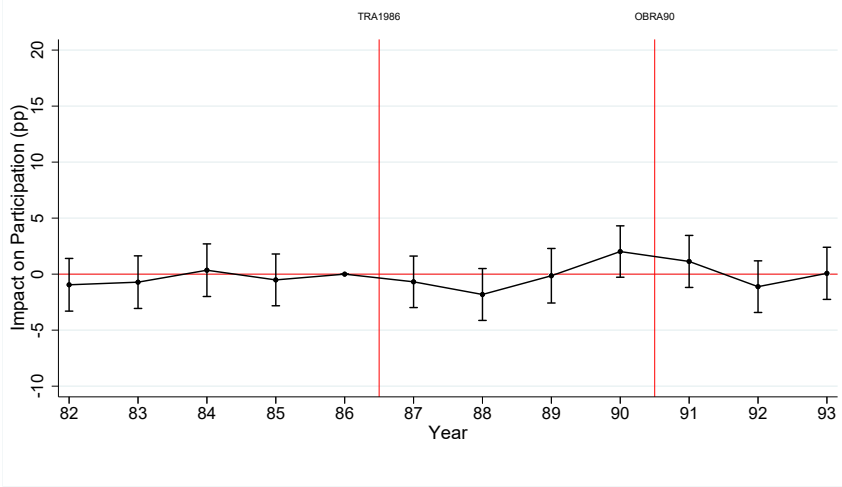
FIGURE A.IX: DID EVENT STUDIES OF FEDERAL ALL EITC REFORMS

WEEKLY PARTICIPATION (DEMOGRAPHIC CONTROLS)

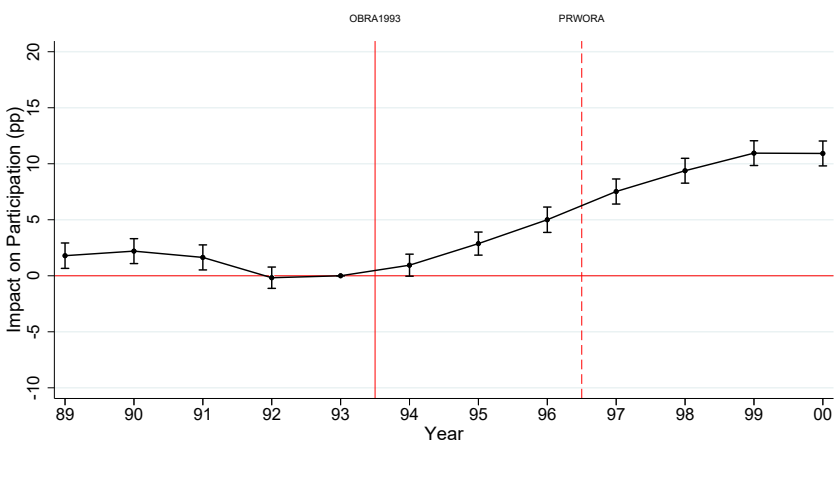
A: 1975 Reform, With vs Without Children



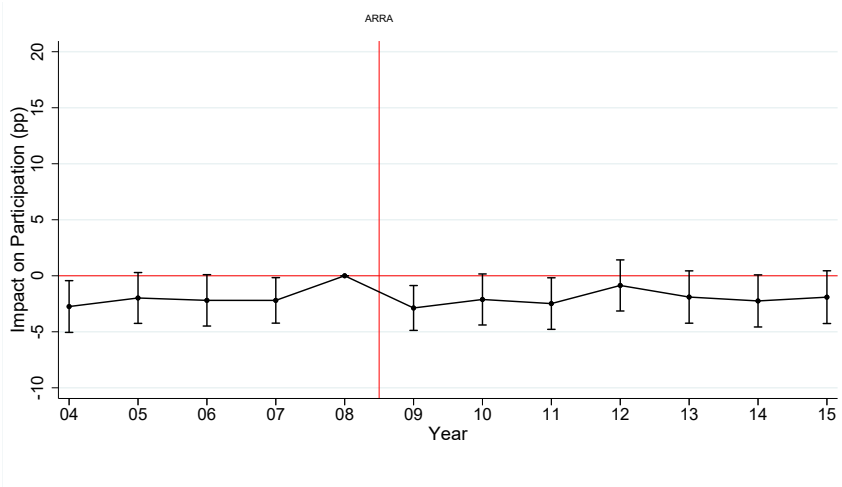
B: 1986 and 1990 Reforms, With vs Without Children



C: 1993 Reform, With vs Without Children



D: 2009 Reform, 3+ vs Without Children

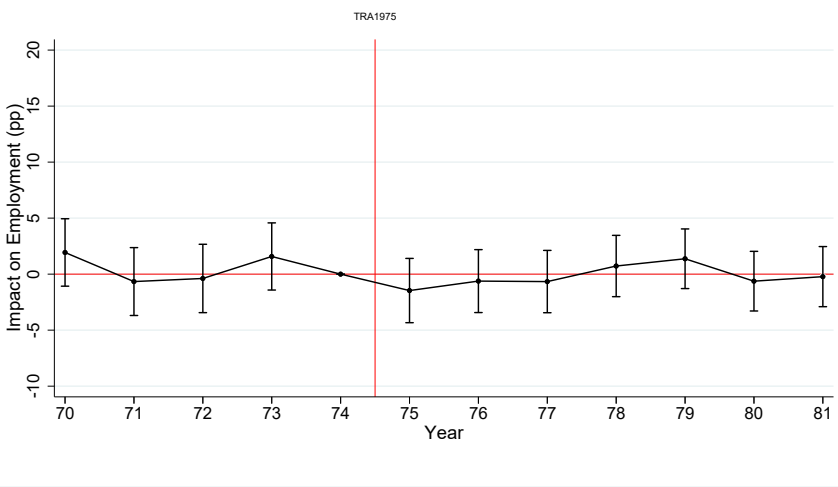


Notes: This figure shows DiD event studies for the five federal EITC reforms. The graphs plot estimates of γ_t based on specification (1) that includes controls for demographic composition: dummies for the age of the woman (six categories), dummies for the age of the youngest child (seven categories), and dummies for education (three categories).. Panels A-C are based on comparing single women with and without children, while Panel D is based on comparing single women with 3+ children to those without children. In each panel, the difference in the pre-reform year is normalized to zero. The dependent variable is weekly participation. The sample includes single women aged 20-50. Panels A-B use the March CPS files alone, while Panels C-D use the March and monthly files combined. The 95% confidence intervals are based on robust standard errors clustered at the individual level.

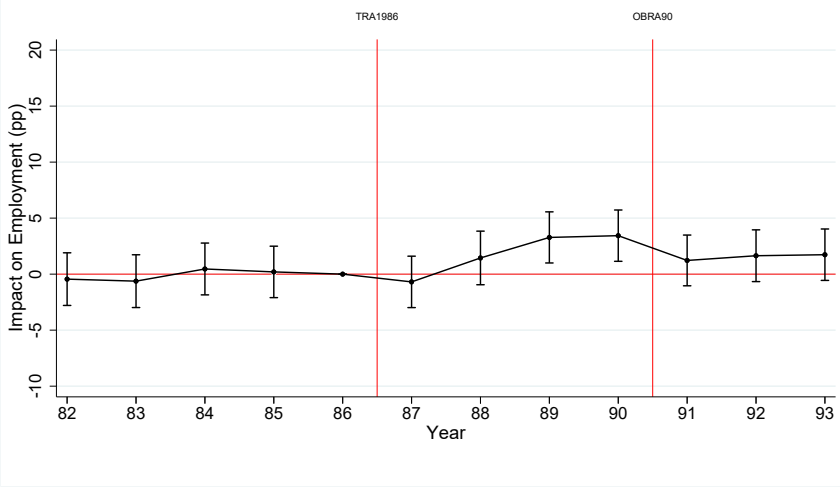
FIGURE A.X: DID EVENT STUDIES OF FEDERAL ALL EITC REFORMS

ANNUAL EMPLOYMENT (DEMOGRAPHIC CONTROLS)

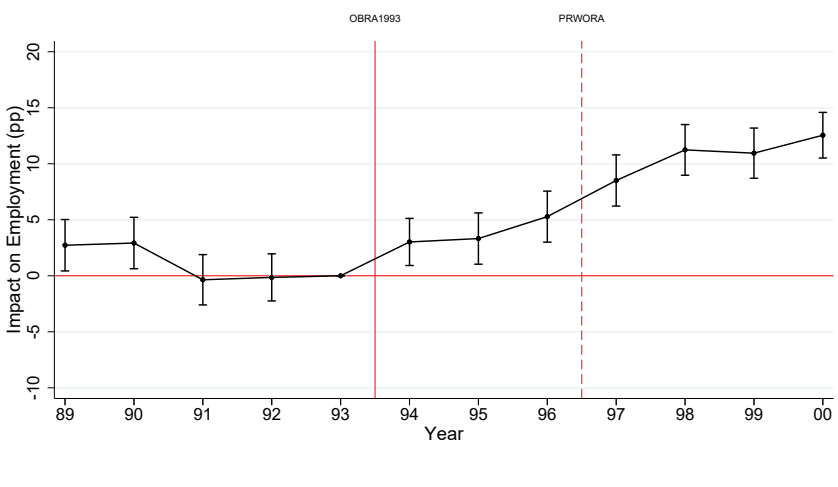
A: 1975 Reform, With vs Without Children



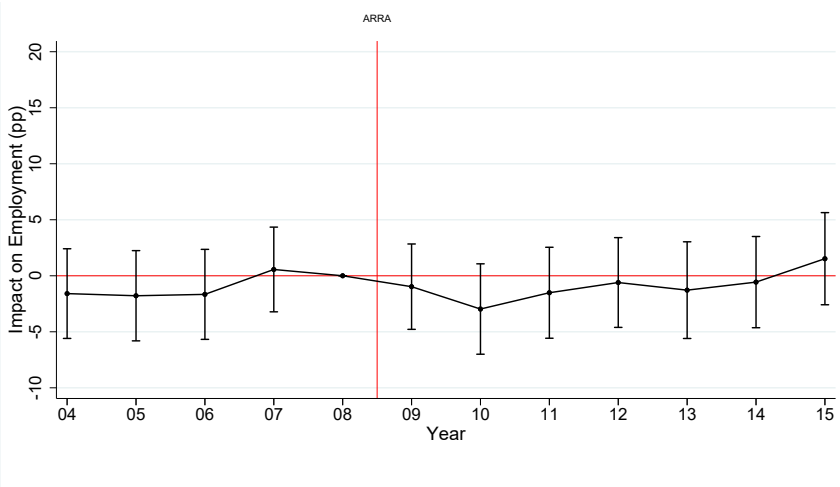
B: 1986 and 1990 Reforms, With vs Without Children



C: 1993 Reform, With vs Without Children



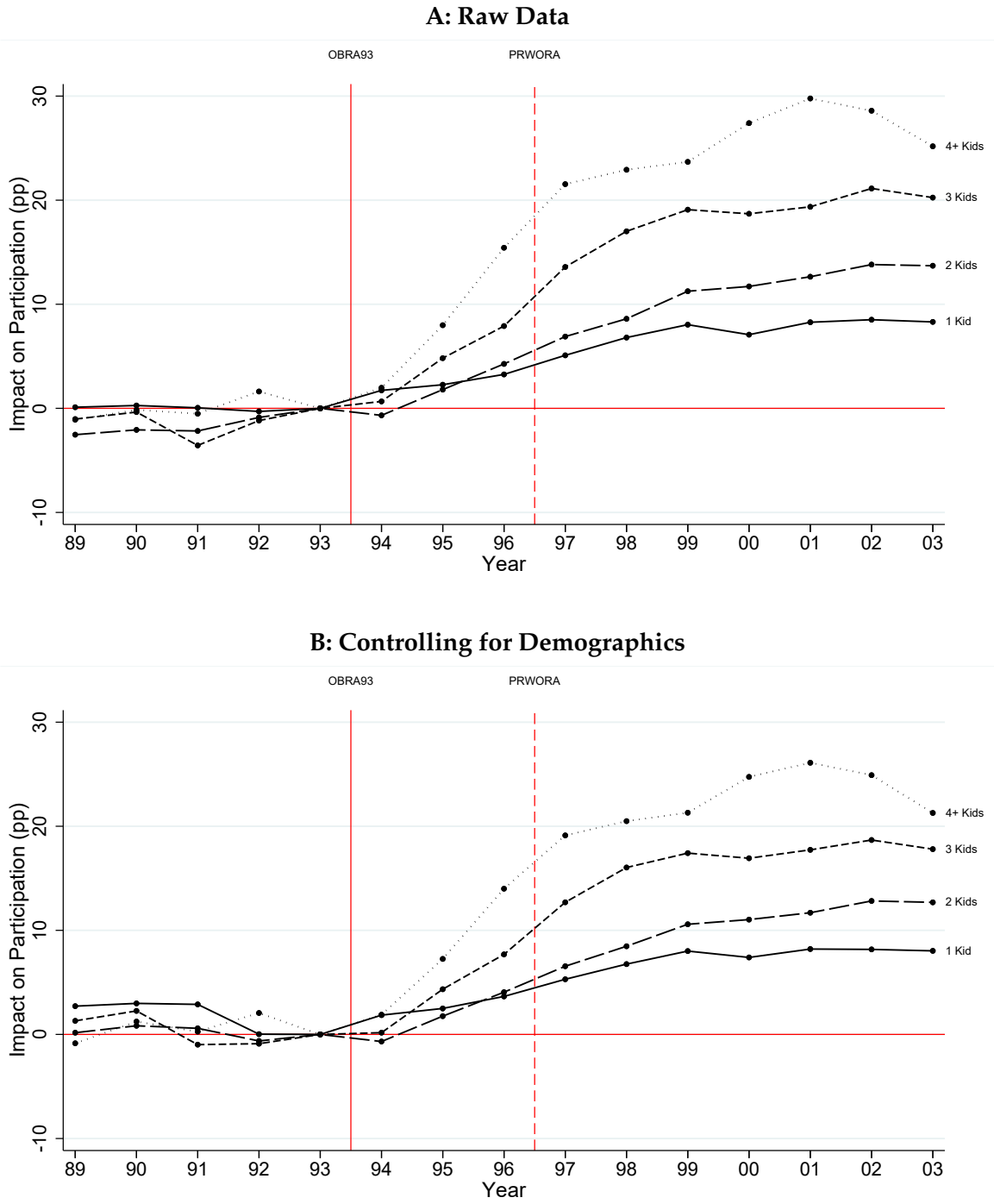
D: 2009 Reform, 3+ vs Without Children



Notes: This figure shows DiD event studies for the five federal EITC reforms. The graphs plot estimates of γ_t based on specification (1) that includes controls for demographic composition: dummies for the age of the woman (six categories), dummies for the age of the youngest child (seven categories), and dummies for education (three categories).. Panels A-C are based on comparing single women with and without children, while Panel D is based on comparing single women with 3+ children to those without children. In each panel, the difference in the pre-reform year is normalized to zero. The dependent variable is annual employment. The sample includes single women aged 20-50 using the March CPS files alone. The 95% confidence intervals are based on robust standard errors clustered at the individual level.

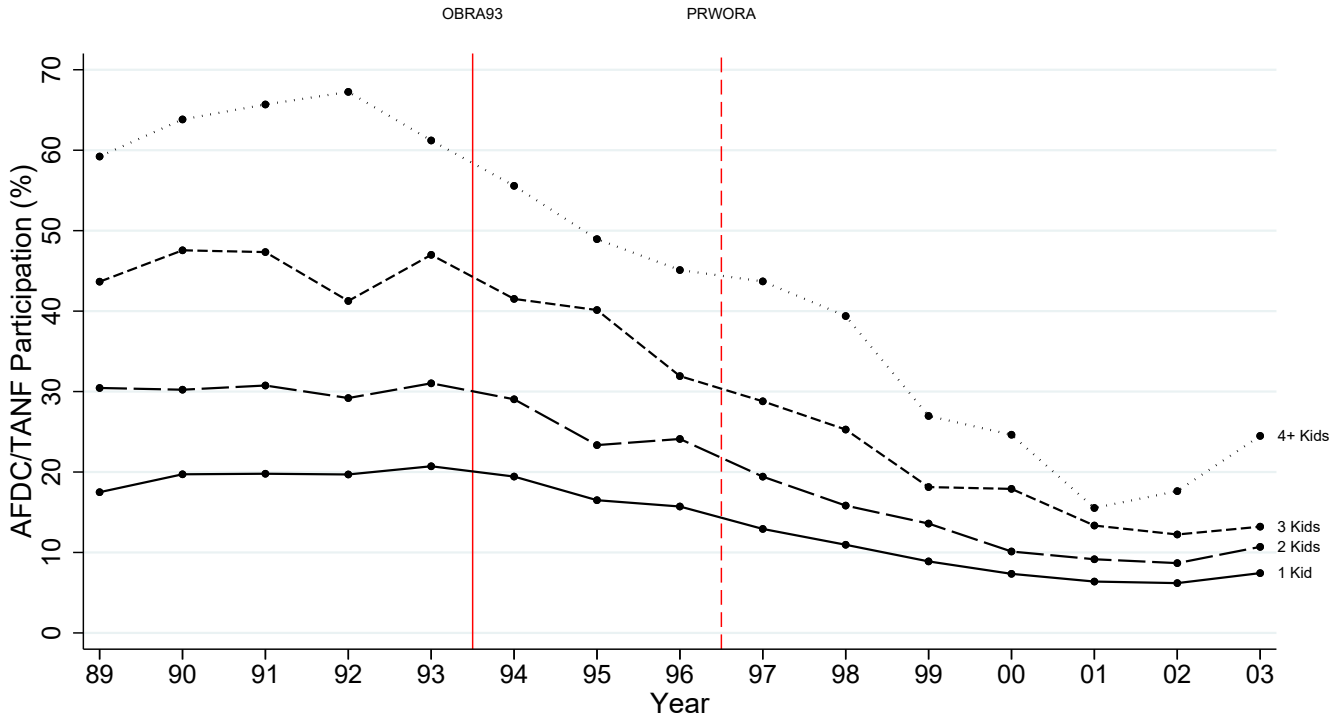
FIGURE A.XI: A FANNING-OUT BY NUMBER OF CHILDREN

PARTICIPATION



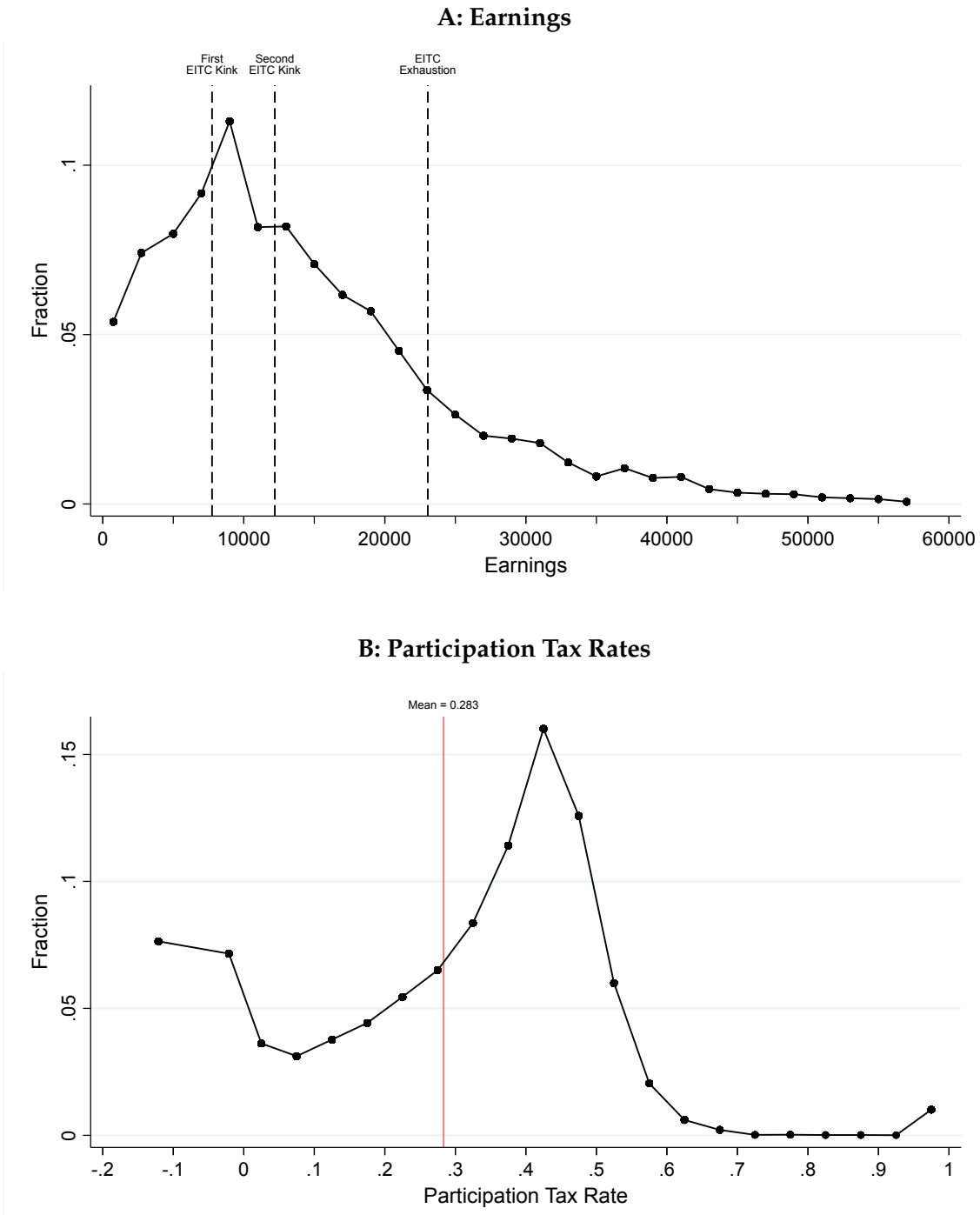
Notes: This figure shows DiD event studies for the 1993 reform by number of EITC-eligible children (1, 2, 3, 4+). The graphs plot DiD coefficients γ_t based on an extension of specification (1) that includes dummies for each family size. Hence, each series shows the difference between single mothers with a given number of children and single women without children, normalized to zero in 1993. Panel A shows raw estimates, while panel B controls for demographic composition: dummies for the age of the woman (six categories), dummies for the age of the youngest child (seven categories), and dummies for education (three categories). The dependent variable is weekly participation. The sample includes single women aged 20-50 using the March and monthly CPS files combined.

FIGURE A.XII: AFDC/TANF PARTICIPATION RATES BY NUMBER OF CHILDREN



Notes: This figure shows event studies for the 1993 reform by number of EITC-eligible children (1, 2, 3, 4+). The graphs plot DiD coefficients γ_t based on an extension of specification (1) without demographic controls that includes dummies for each family size. The dependent variable is annual AFDC/TANF participation. The sample includes single women aged 20-50 using the March files alone.

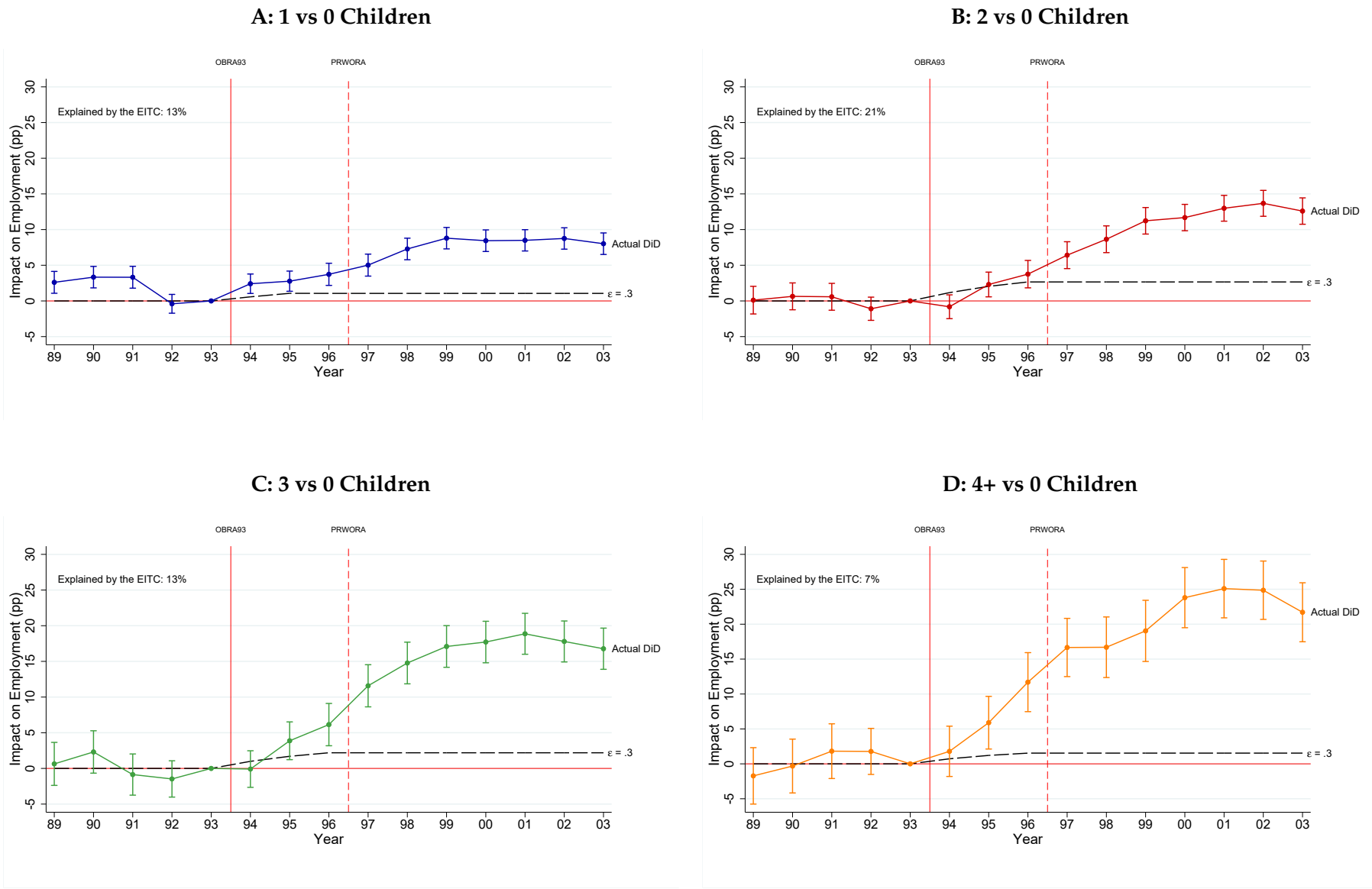
FIGURE A.XIII: DISTRIBUTIONS OF EARNINGS AND PARTICIPATION TAX RATES IN 1993



Notes: This figure shows the distribution of earnings and participation tax rates among single mothers. Panel A shows the earnings distribution using predicted earnings for non-workers (estimated from equation 3) and actual earnings for workers. Panel B shows the distribution of participation tax rates. The sample includes single mothers aged 20-50 using March CPS files from 1992-94 (corresponding to tax years 1991-93).

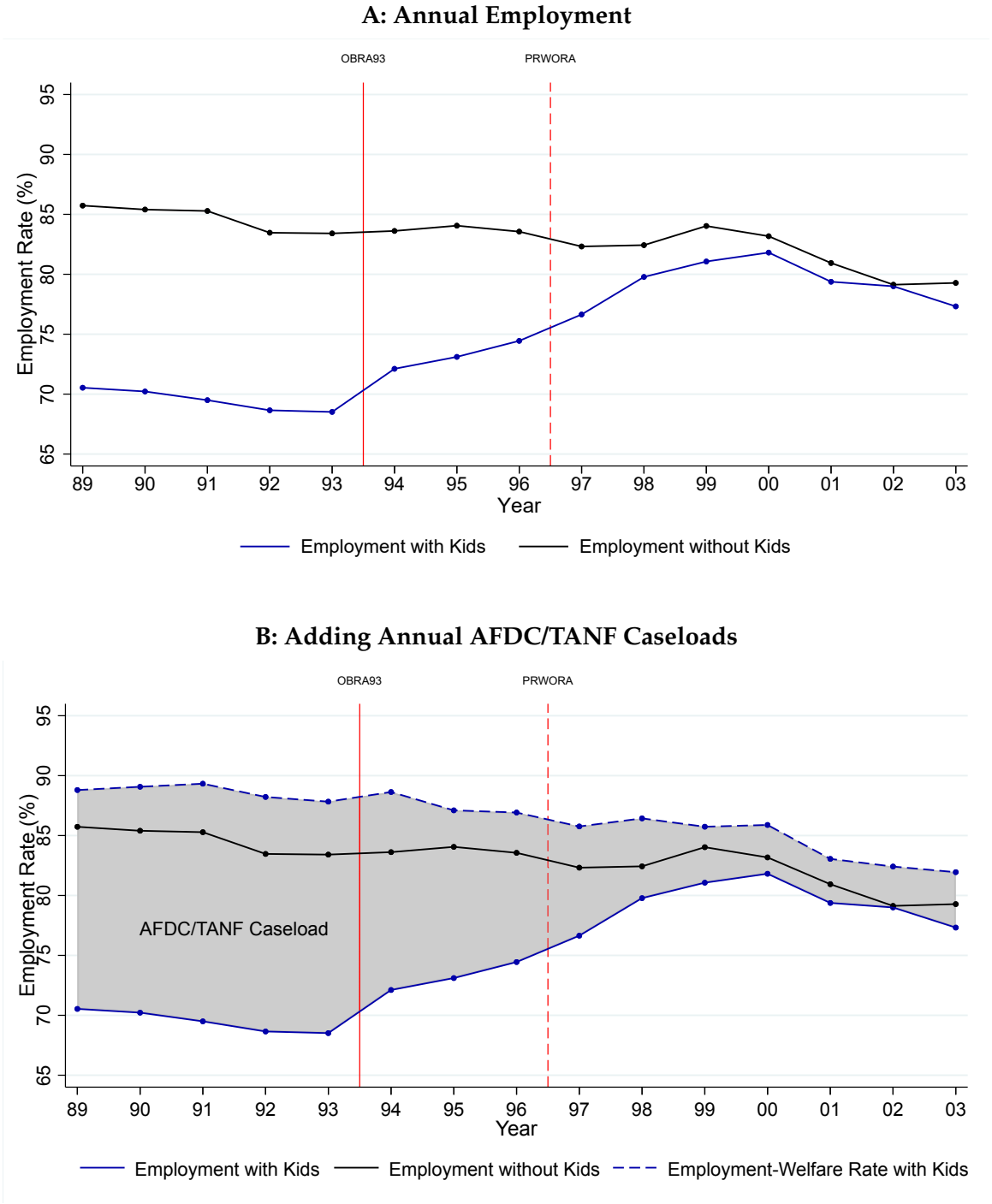
FIGURE A.XIV: ACTUAL VS SIMULATED RESPONSES TO THE 1993 REFORM

DEMOGRAPHIC CONTROLS



Notes: This figure shows actual and simulated DiD event studies for the 1993 reform, by number of EITC-eligible children. The actual DiD series plot DiD coefficients γ_t based on an extension of specification (1) with separate dummies for each family size and controlling for demographics. The specification does not include demographic controls and the dependent variable is weekly employment. The simulated DiD series (black dashed lines) plot ΔP_t calculated from equation (4), assuming an elasticity of 0.3. See section C in the appendix for additional details. The fraction explained by the EITC equals the simulated DiD estimate in 2003 divided by the actual DiD estimate in 2003. The sample includes single women aged 20-50 using the March and monthly CPS files combined. The 95% confidence intervals are based on robust standard errors clustered at the individual level.

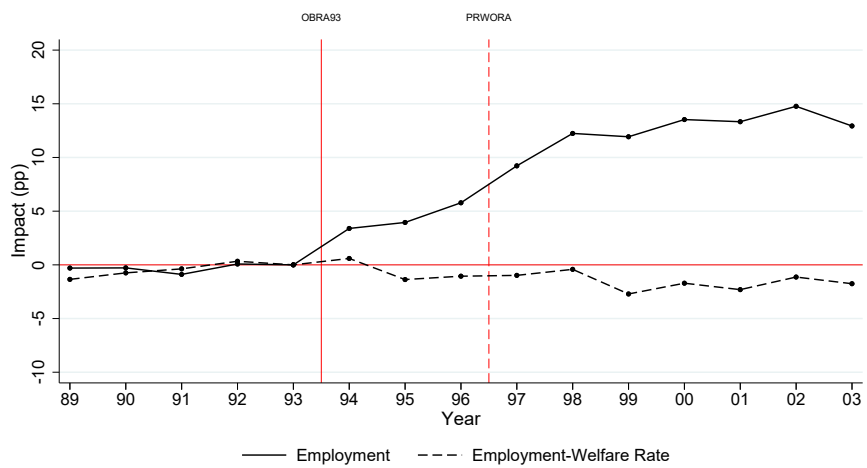
FIGURE A.XV: EMPLOYMENT VS AFDC/TANF CASELOADS



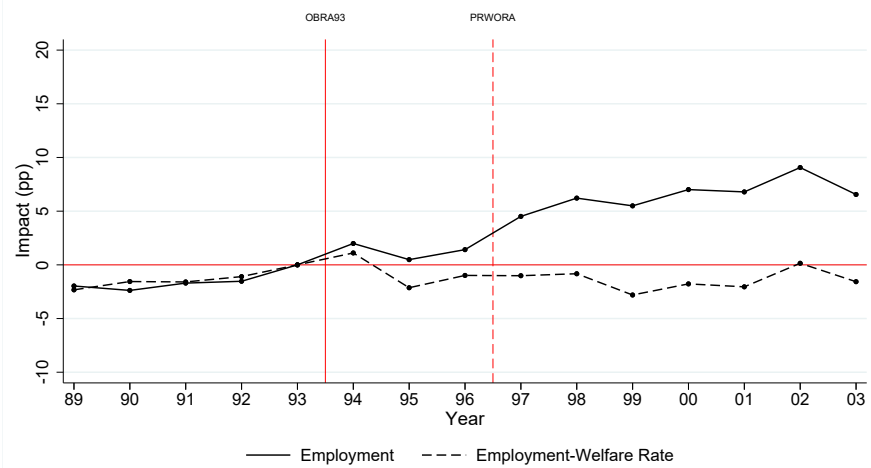
Notes: This figure shows the employment rate (Panel A) and the "employment-welfare rate" (Panel B) for single women with and without children. The employment-welfare rate equals the combined fraction of those who are employed and/or participants in the AFDC/TANF program. For single women without children, AFDC/TANF participation is zero and hence there is no distinction between the employment and employment-welfare rates for this group. The outcome variables are measured at the annual level. The sample includes single women aged 20-50 using March CPS files.

FIGURE A.XVI: EMPLOYMENT VS AFDC/TANF CASELOADS, BY NUMBER OF CHILDREN

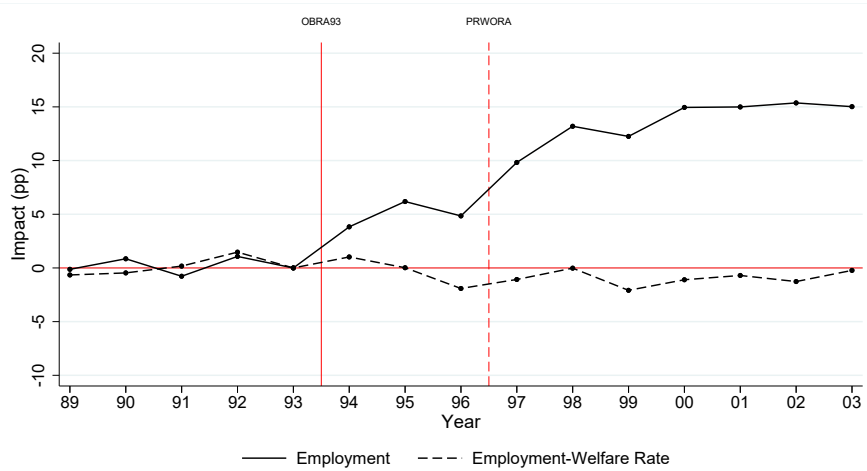
A: With vs Without Children



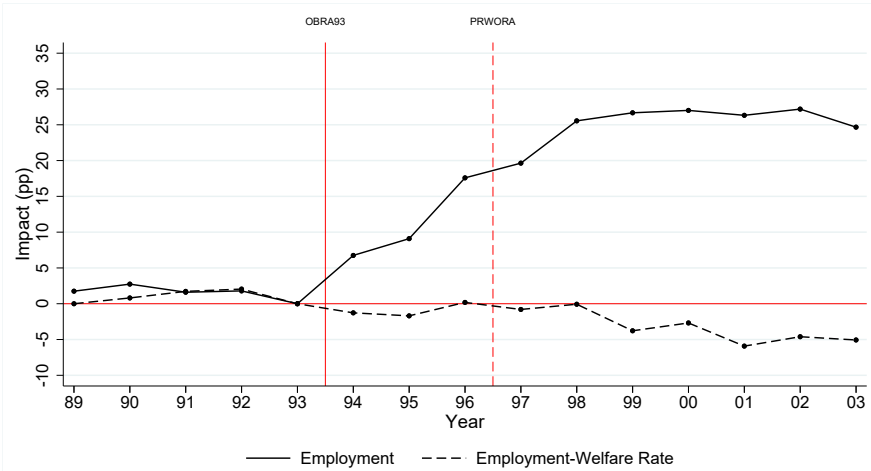
B: 1 vs 0 Children



C: 2 vs 0 Children



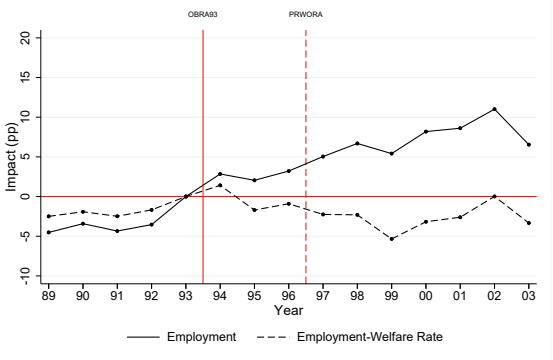
D: 3+ vs 0 Children



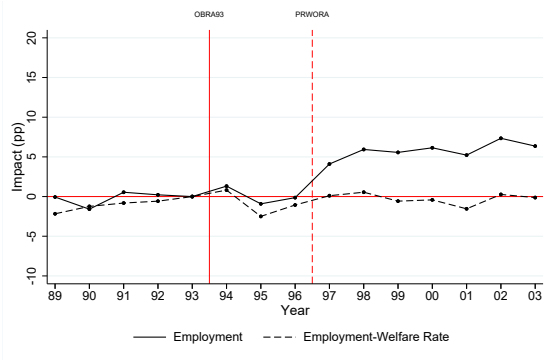
Notes: This figure shows DiD event studies of the 1993 reform by number of children for two different outcomes: the employment rate (solid line) and the employment-welfare rate (dashed line). The estimates are based on an extension of equation (1) that includes separate dummies for each number of children (1, 2, and 3+). The specification does not include demographic controls. The outcome variables (employment or employment/welfare) are measured at the annual level. The sample includes single women aged 20-50 using March CPS files.

FIGURE A.XVII: EMPLOYMENT VS AFDC/TANF CASELOADS, BY NUMBER OF CHILDREN AND AGE OF YOUNGEST CHILD

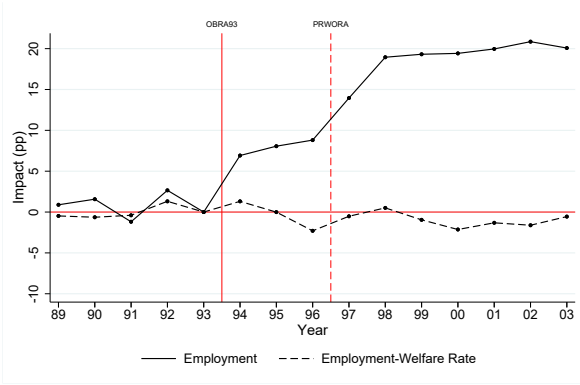
A: 1 Child, Youngest Aged 0-7



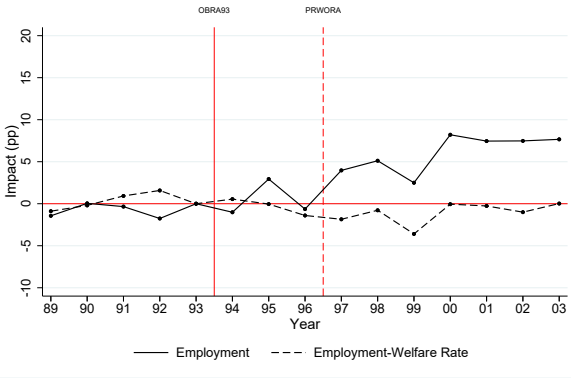
B: 1 Child, Youngest Aged 8-18



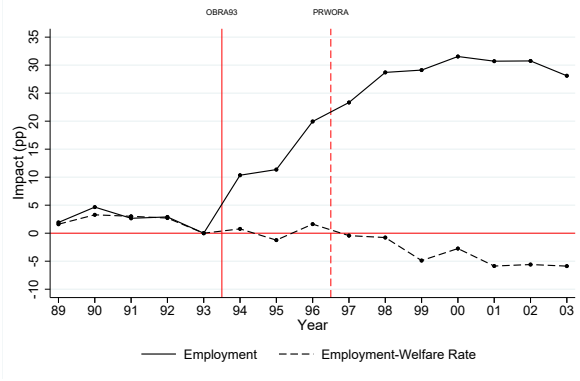
C: 2 Children, Youngest Aged 0-7



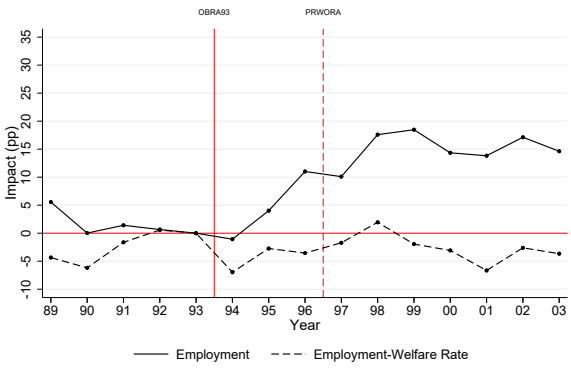
D: 2 Children, Youngest Aged 8-18



E: 3+ Children, Youngest Aged 0-7

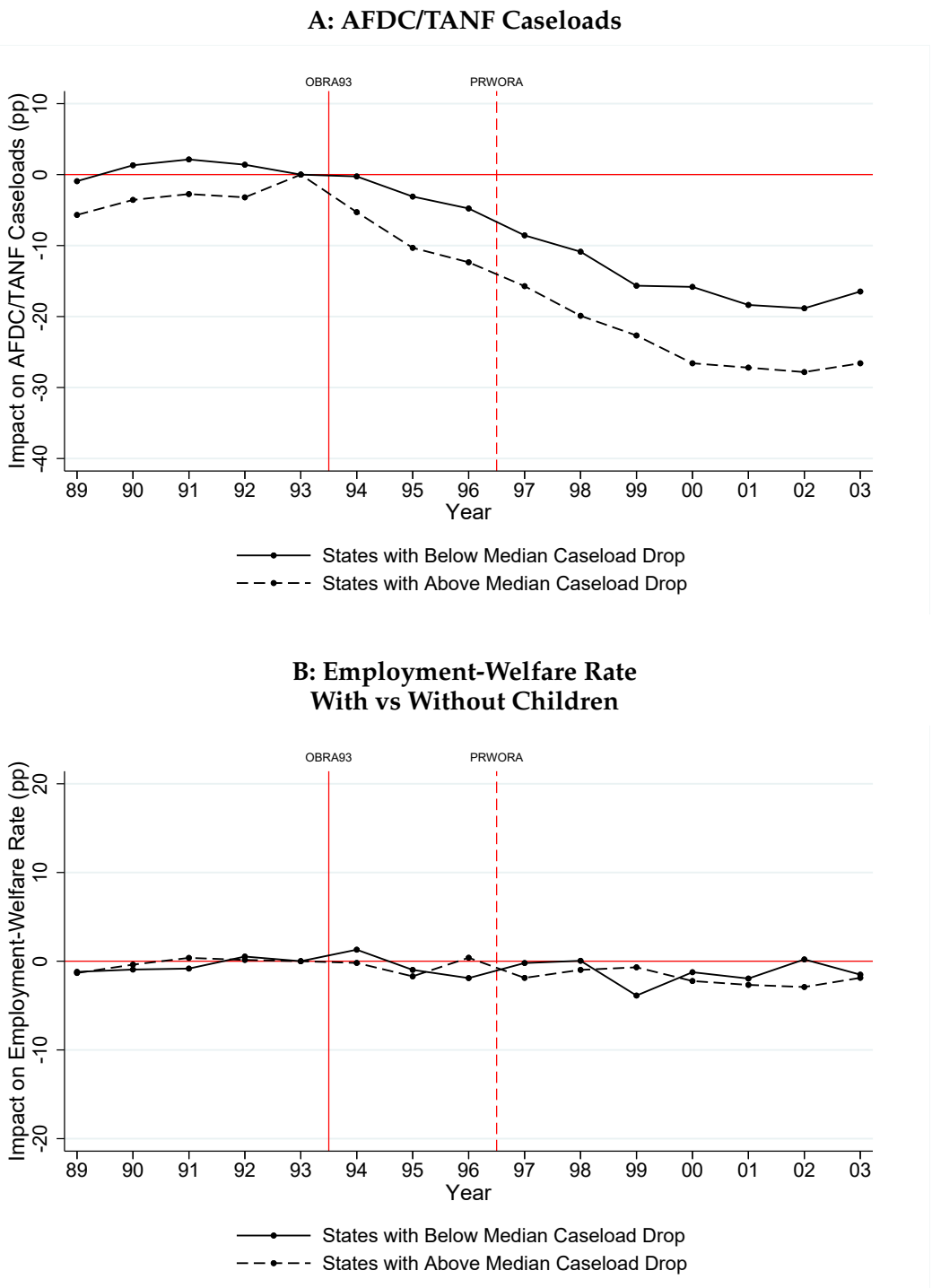


F: 3+ Children, Youngest Aged 8-18



Notes: This figure shows DiD event studies of the 1993 reform by family size and age of youngest child for two different outcomes: the employment rate (solid line) and the employment-welfare rate (dashed line). The estimates are based on an extension of equation (1) that includes separate dummies for each number of children (1, 2, 3+), age of youngest child (0-7 and 8-18), and their interaction. The specification does not include demographic controls. The outcome variables (employment or employment/welfare) are measured at the annual level. The sample includes single women aged 20-50 using March CPS files.

FIGURE A.XVIII: EMPLOYMENT VS AFDC/TANF CASELOADS, BY STATE

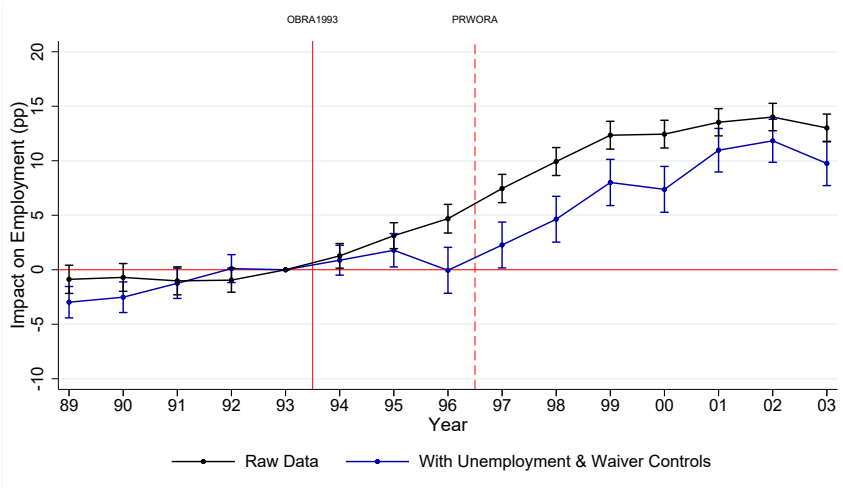


Notes: This figure shows DiD event studies of the 1993 reform, split by states with below and above median AFDC/TANF caseload drops between 1993-2000. Panel A plots the AFDC/TANF caseload series for the two groups of states, while Panel B plots the employment-welfare rate series. The estimates are based on an extension of equation (1) that interacts each variable with a dummy for being in a state with an above-median drop in AFDC/TANF caseload. The outcome variables are measured at the annual level. The sample includes single women aged 20-50 using March CPS files.

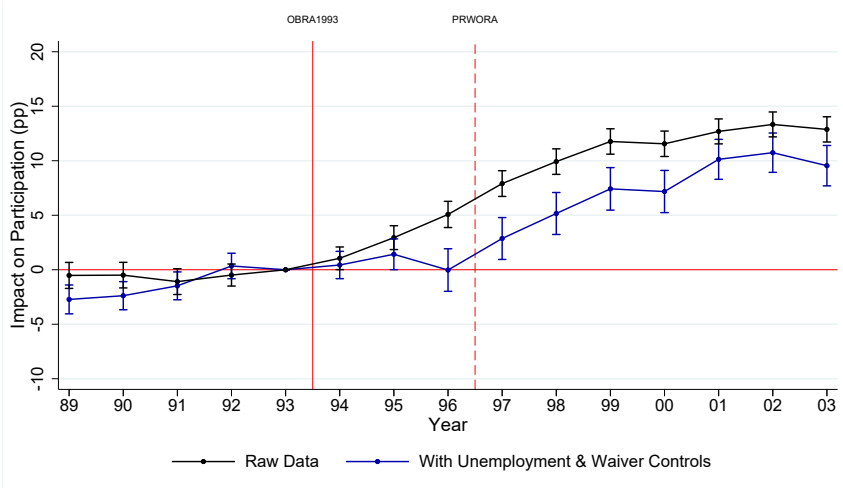
FIGURE A.XIX: HOW MUCH CAN BE EXPLAINED BY THE BUSINESS CYCLE AND WAIVERS?

VARYING THE OUTCOME AND SAMPLE

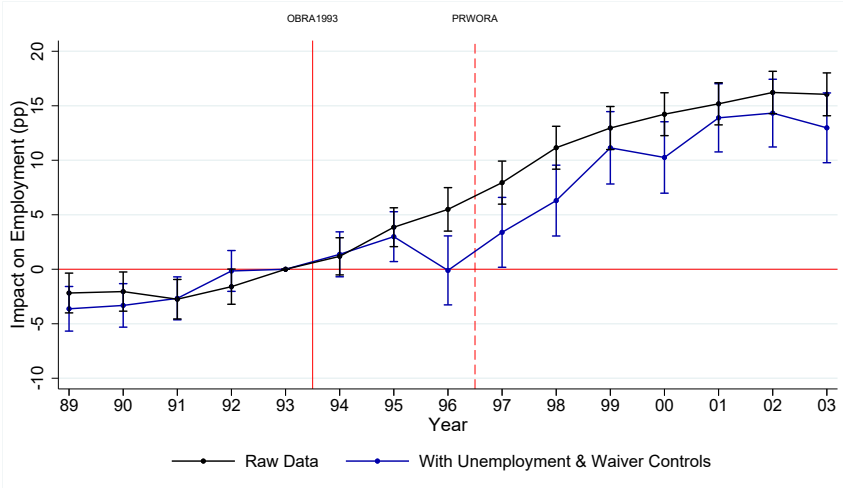
A: Employment of All Single Mothers (Baseline)



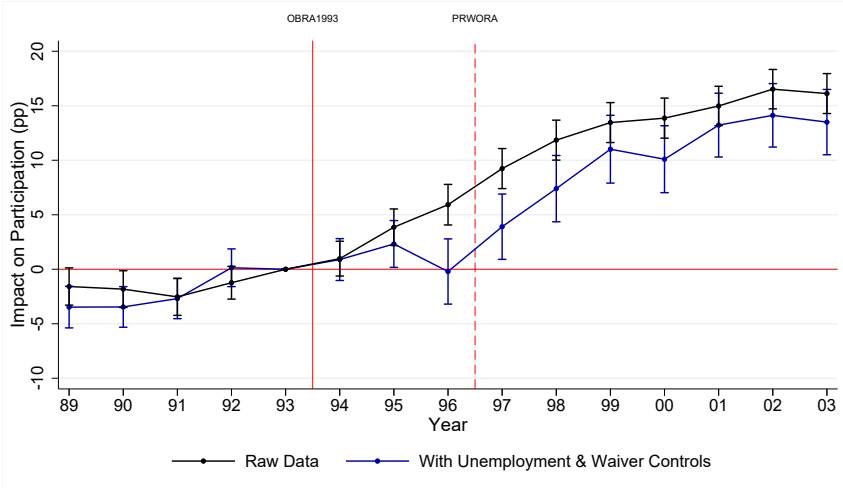
B: Participation of All Single Mothers



C: Employment of Low-Educated Single Mothers



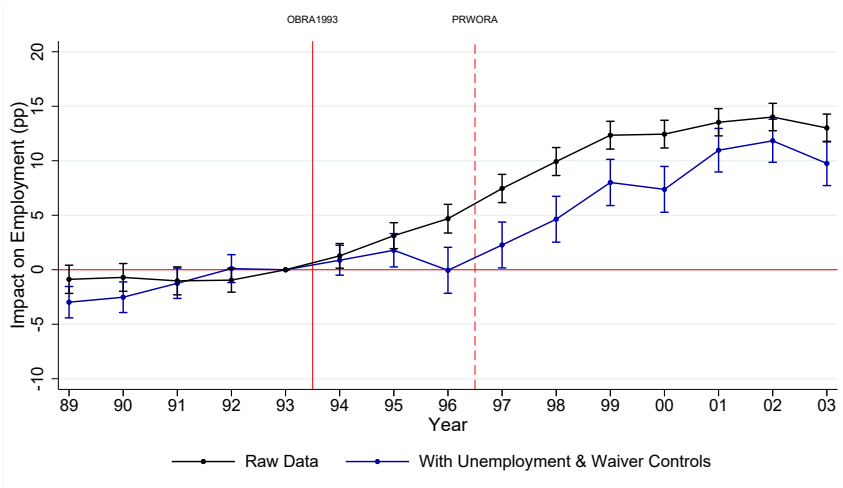
D: Participation of Low-Educated Single Mothers



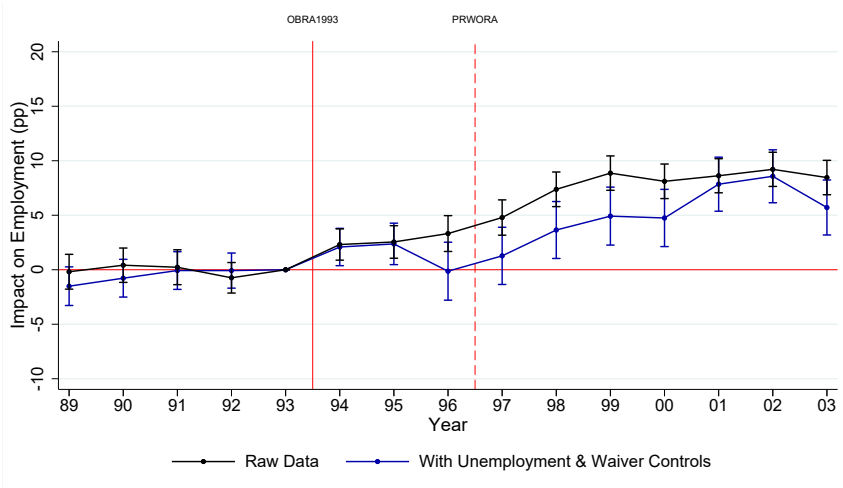
Notes: This figure shows DiD event studies for the 1993 reform with controls for unemployment and waivers using different samples and outcomes. The graphs plot DiD coefficients γ_t based on equation (7). The top row shows results for all single mothers while the bottom row shows results for single mothers with a high school education or below. The left column shows results for weekly employment while the right panel shows results for weekly participation. In both panels, the black series show the raw DiD without controls. The sample includes single women aged 20-50 using the March and monthly CPS files combined. The 95% confidence intervals are based on robust standard errors clustered at the individual level.

FIGURE A.XX: HOW MUCH CAN BE EXPLAINED BY THE BUSINESS CYCLE AND WAIVERS?
BY NUMBER OF CHILDREN

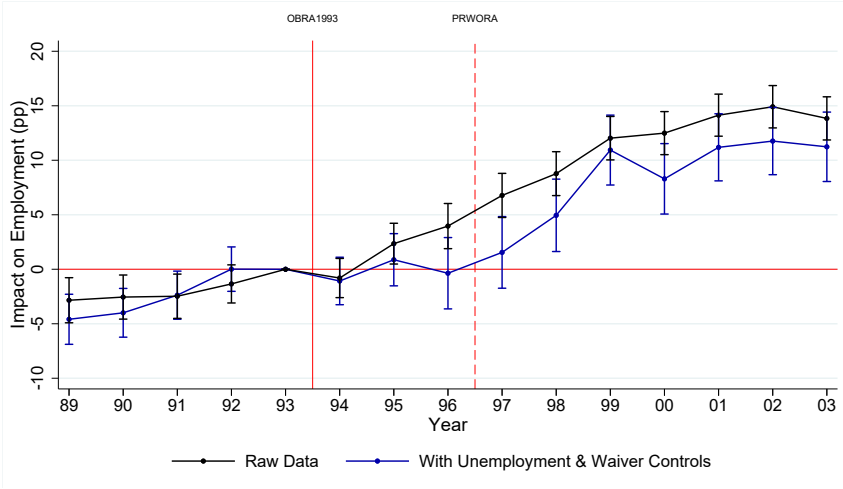
A: With vs Without Children (Baseline)



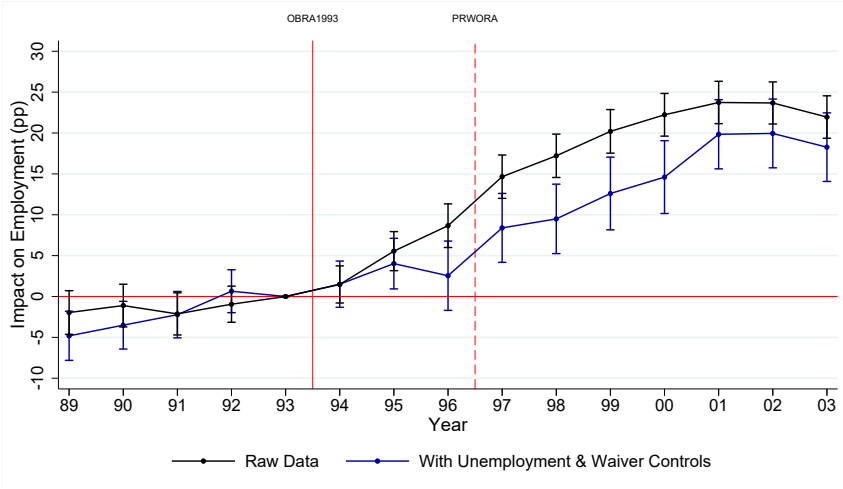
B: With 1 vs 0 Children



C: With 2 vs 0 Children



D: With 3+ vs 0 Children

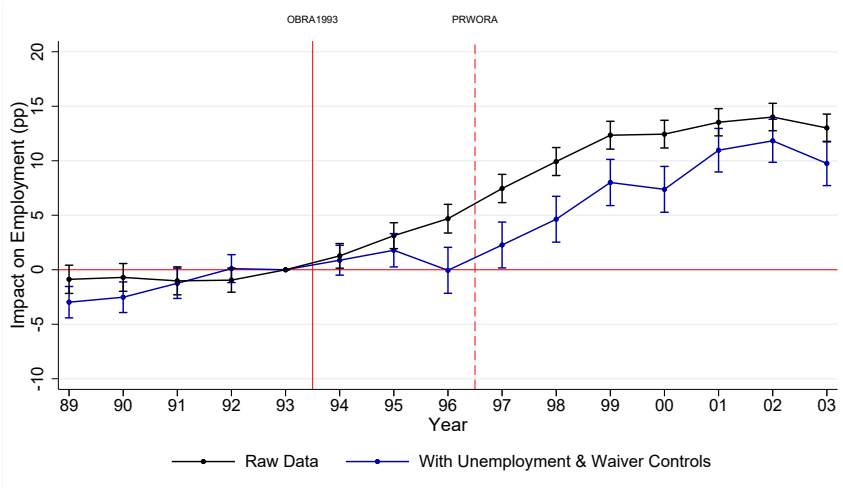


Notes: This figure shows DiD event studies for the 1993 reform with controls for unemployment and waivers by number of EITC-eligible children (any, 1, 2, and 3+). The graphs plot DiD coefficients γ_t based on an extension of equation (7) that includes dummies for each family size. In both panels, the black series show the raw DiD without controls. The dependent variable is weekly employment. The sample includes single women aged 20-50 using the March and monthly CPS files combined. The 95% confidence intervals are based on robust standard errors clustered at the individual level.

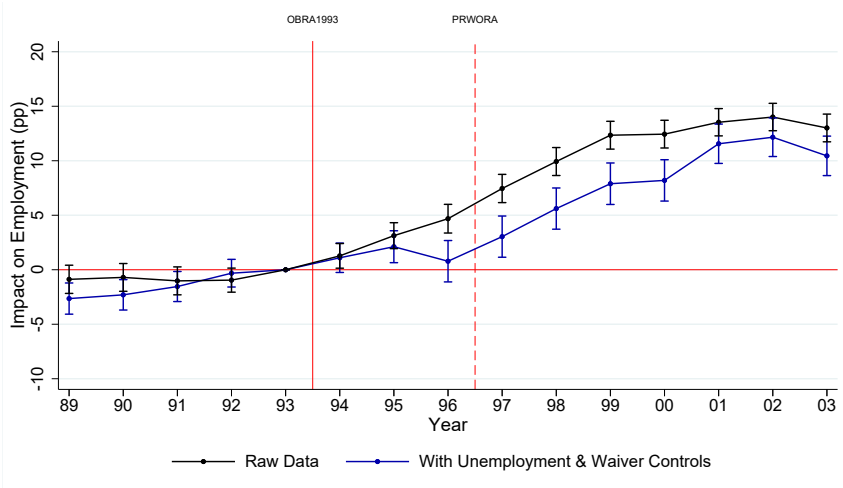
FIGURE A.XXI: HOW MUCH CAN BE EXPLAINED BY THE BUSINESS CYCLE AND WAIVERS?

VARYING THE SPECIFICATION OF WAIVERS

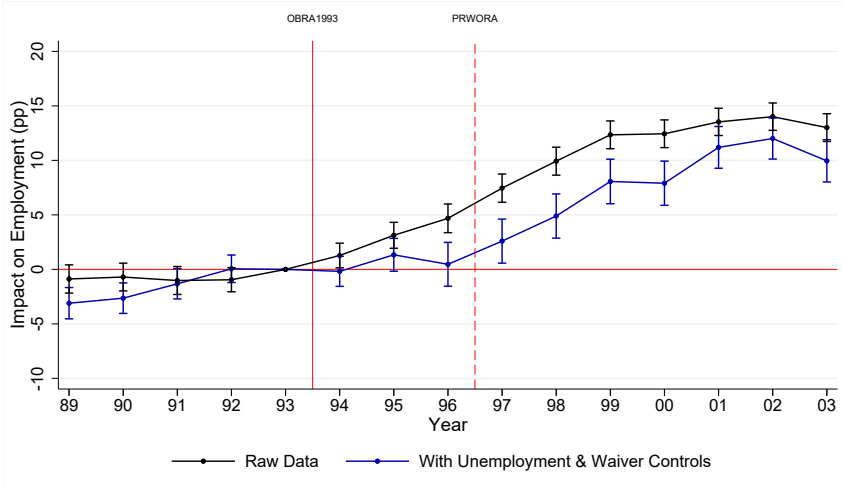
A: Baseline Specification



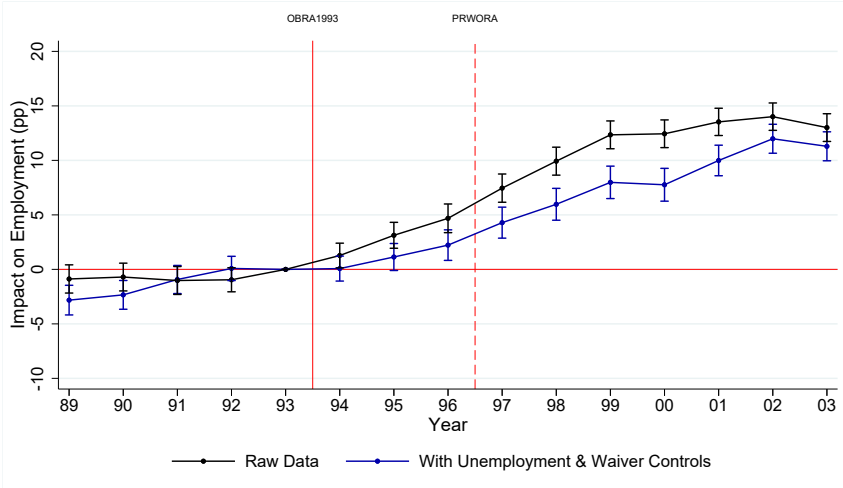
B: Separate Indicators for the Six Waiver Types



C: Using Date of Implementation



D: Post-Waiver Indicator

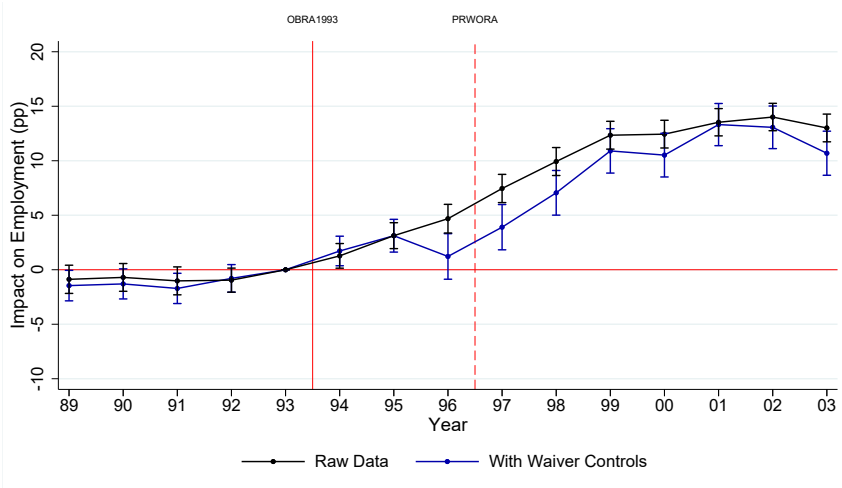


Notes: This figure shows DiD event studies for the 1993 reform with controls for unemployment and waivers. The graphs plot DiD coefficients γ_t based on equation (7). The black series show the raw DiD without controls. In panel A, the blue series controls for unemployment by kids, state fixed effects, and waivers by kids (based on the waiver approval date). Panel B is based on an extension of equation (7) where the any waiver dummy is replaced by separate dummies for each of the six waiver types. Panel C is similar to panel A, but uses the date of waiver implementation rather than approval. Panel D uses a similar specification to panel A, but does not interact the any waiver dummy with yearly indicators. The dependent variable is weekly employment. The sample includes single women aged 20-50 using the March and monthly CPS files combined. The 95% confidence intervals are based on robust standard errors clustered at the individual level.

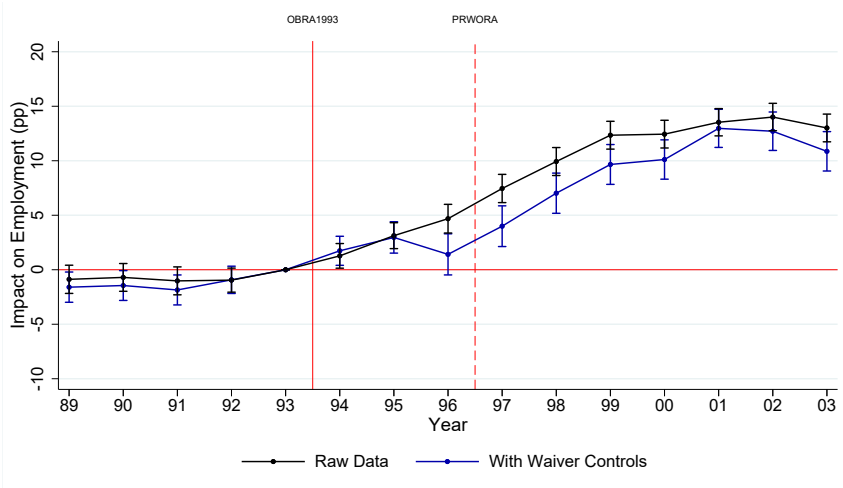
FIGURE A.XXII: CONTROLLING ONLY FOR WELFARE WAIVERS

VARYING THE SPECIFICATION OF WAIVERS

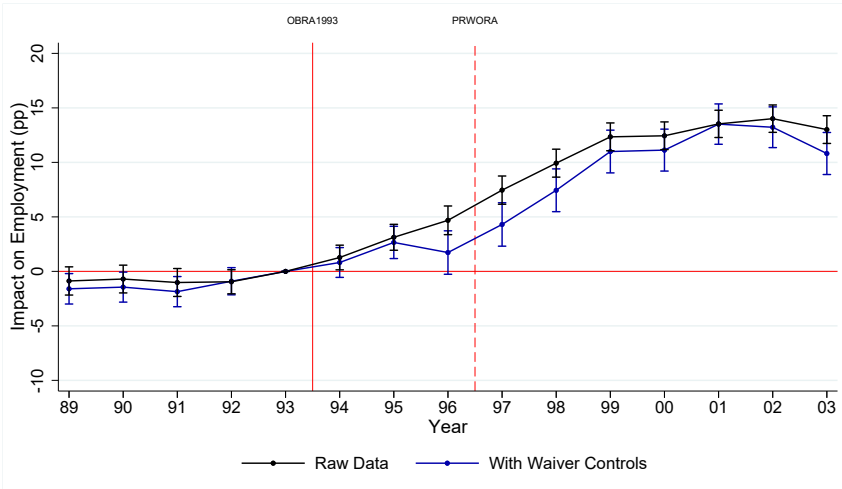
A: Baseline Specification



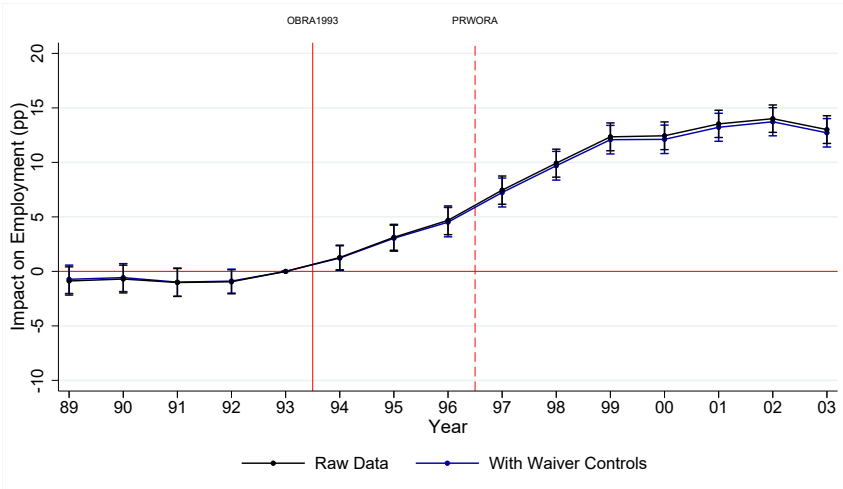
B: Separate Indicators for the Six Waiver Types



C: Using Date of Implementation



D: Post-Waiver Indicator

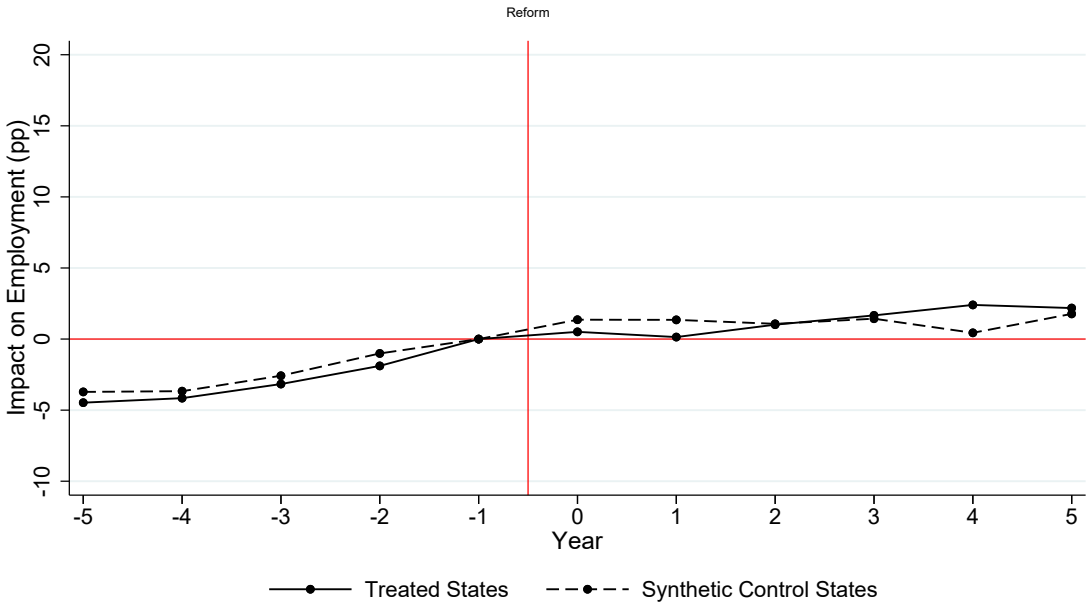


Notes: This figure shows DiD event studies for the 1993 reform with controls for unemployment and waivers. The graphs plot DiD coefficients γ_t based on equation (7) without state unemployment controls. In all panels, the black series show the raw DiD without controls. In panel A, the blue series includes controls for state fixed effects and waivers by kids based on the date of waiver approval. Panel B is based on an extension of equation (7) where the any waiver dummy is replaced by separate dummies for each of the six waiver types. Panel C is similar to panel A, but uses the date of waiver implementation rather than approval. Panel D uses a similar specification to panel A, but does not interact the any waiver dummy with yearly indicators. The dependent variable is weekly employment. The sample includes single women aged 20-50 using the March and monthly CPS files combined. The 95% confidence intervals are based on robust standard errors clustered at the individual level.

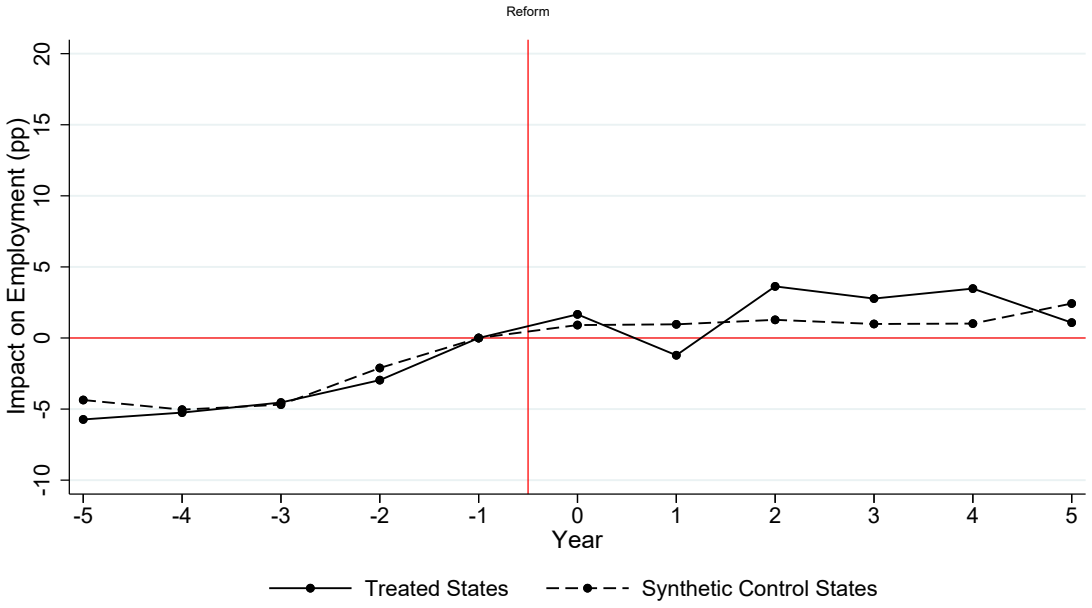
FIGURE A.XXIII: A SYNTHETIC CONTROL ANALYSIS OF STATE EITC REFORMS

COMPARING SINGLE WOMEN WITH AND WITHOUT CHILDREN

A: All State EITC Reforms



B: Excluding Small State EITC Reforms

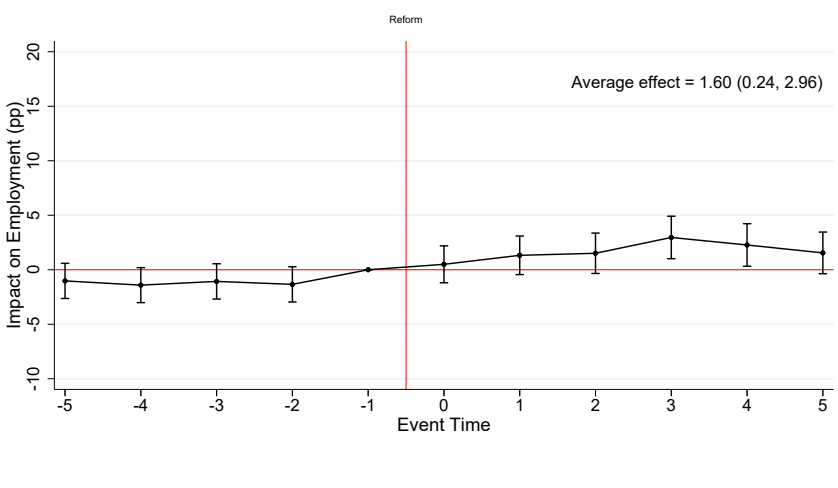


Notes: This figure shows stacked event studies of state EITC reforms using a synthetic control approach. The graphs plot employment rates for single women with children relative to single women without children in treatment and synthetic control states, respectively, normalized to zero in the pre-reform year. This is a triple-differences approach using variation across states and by the presence of children within states. Panel A includes all states that instituted an EITC supplement before 2015, while panel B includes only states that instituted a "large" EITC supplement. Large supplements are defined as refundable credits equal to at least 10% of the federal credit. In both panels, the synthetic control states are constructed from states that never instituted an EITC supplement. For each treatment state, a synthetic control state is constructed by matching on the employment rate in the five pre-reform years. Table A.VI shows the make-up of each synthetic state. For states with supplements enacted before 1993 the sample is based on March CPS files alone, while for states with supplements enacted after 1993 the sample is based on and March and monthly CPS files combined. The outcome is weekly employment and the sample includes all single women aged 20-50. See section D in the appendix for additional details.

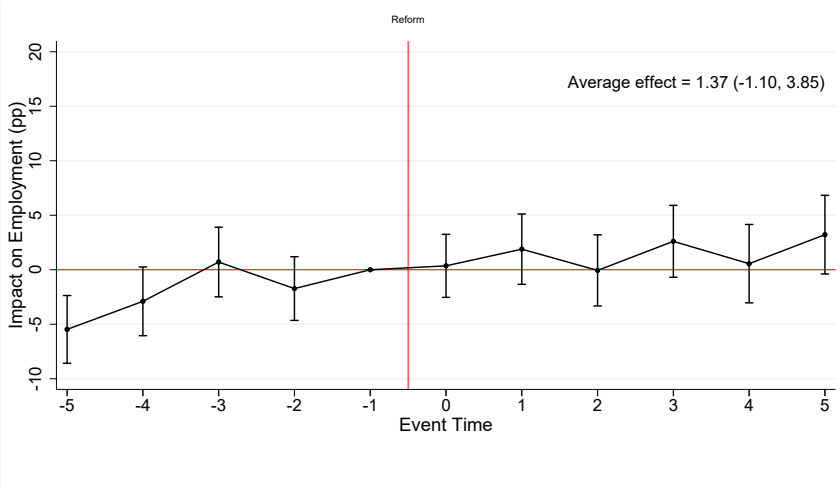
FIGURE A.XXIV: STACKED EVENT STUDIES OF ALL STATE AND FEDERAL EITC REFORMS

EMPLOYMENT OF LOW-EDUCATED SINGLE WOMEN

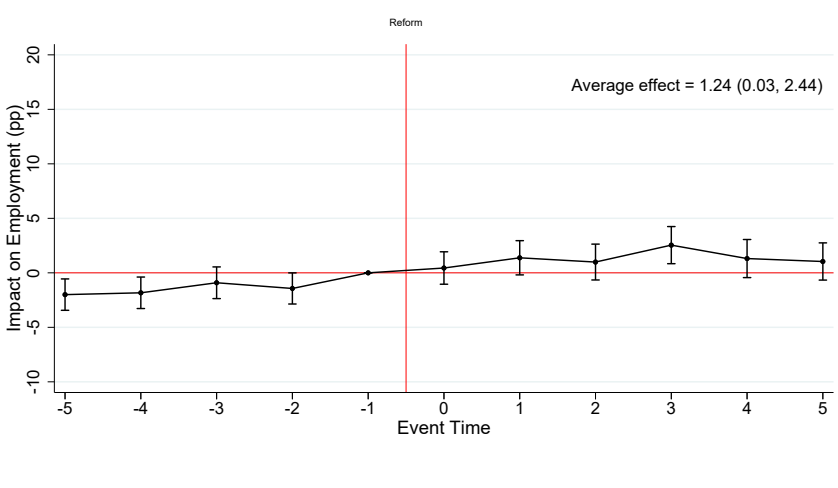
A: Federal Reforms



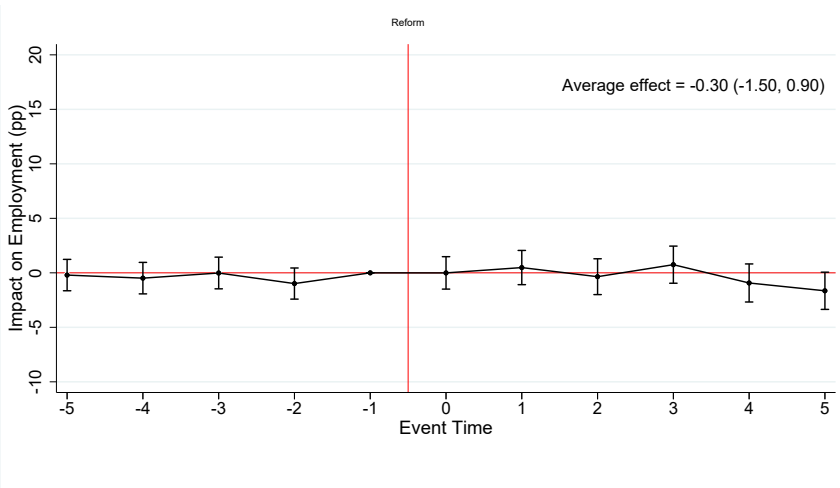
B: State Reforms



C: State and Federal Reforms



D: State and Federal Reforms, Adjusted for Pre-Trends



Notes: This figure shows stacked event studies of all state and federal EITC reforms. The graphs plot DiD coefficients based on comparing single women with and without children across event time, normalized to zero in the pre-reform year (event time -1). The specifications control for demographics, and for the impact of waivers and unemployment around the 1993 federal reform (i.e., the controls in equation (7), interacted with an OBRA93 indicator). Panel A includes all federal reforms, Panel B includes all state reforms. while Panel C includes all state and federal reforms together. Panel D is similar to panel C, but adjusts for group-specific linear pre-trends. Each panel reports the average effect across the post-reform years, with 95% confidence intervals in parentheses. For reforms enacted before 1993 the sample is based on the March CPS files alone, while for reforms enacted after 1993 the sample is based on the March and monthly CPS files combined. The outcome variable is weekly employment, and the sample consists of single women aged 20-50 with a high school degree or less. See section E in the appendix for additional details.

B Data Description

B.1 Current Population Survey (CPS)

The CPS is made up of two main components: the Basic Monthly Survey and topical Supplements. In most cases, supplement samples are limited to individuals who participate in the Basic Monthly Survey. The Annual Social and Economic Supplement (ASEC) — the "March files" — is an exception. It includes an oversample of respondents from other months who are not scheduled to receive the March Basic Monthly CPS. The ASEC is the most commonly used supplement of the CPS due to its long history, large sample size, and detailed information on annual income and social assistance. I use the Basic Monthly and the ASEC CPS files, extracted from IPUMS at https://cps.ipums.org/cps/.

The Basic Monthly CPS uses a sample rotation scheme whereby households are included in the sample for four consecutive months, excluded for eight, and then return for another four months before leaving the sample permanently. Due to the 4-8-4 sampling pattern, individuals in the CPS show up a total of eight times over a 16 month period. Despite this panel element of the CPS, most researchers use the survey as a repeated cross-section. While it is impossible to link respondents between the Basic Monthly samples and the ASEC oversample, it is possible to link respondents across the monthly samples alone. IPUMS has greatly simplified this process by creating the variable CPSIDP. CPSIDP is a combination of the year a household enters the sample, the month a household enters the sample, a within-month household ID, and a within-month person ID. It allows users to uniquely identify and track respondents across all Basic Monthly samples. CPSIDP is not available for respondents in the ASEC oversample, however, and it is therefore not possible to link respondents from the ASEC oversample to their observations in the Basic Monthly Survey.[49] More detail about how the unique ID is constructed given these constraints is described in section B.1.4.

I use the Basic Monthly files from 1989-2018 and the ASEC files from 1968-2018.[50] Although the monthly files are available from 1976, they do not allow for accurate identification of the presence and number of children in a household prior to 1989. To identify children in the CPS, I rely on the IPUMS variable RELATE. For each observation in a household, this variable identifies the relationship to the household head. Prior to 1989, the only RELATE categories available in the

[49] See Flood & Pacas (2016) for a more comprehensive explanation for why the ASEC oversample respondents cannot be linked to their observations in the other months.

[50] I exclude the March Basic file, because all respondents in the March Basic sample are included in ASEC.

monthly files are "householder," "spouse," "other relative," and "other non-relative." The ASEC files, on the other hand, have more consistent categories for RELATE over time and, importantly, these categories include "child." The absence of the "child" category in the monthly files results in a substantial undercounting of children relative to the ASEC files. Hence, I use the Basic Monthly files (combined with ASEC files) from 1989 onwards, and the ASEC files on their own before this time.

B.1.1 Extensive Margin Measures

I consider all four extensive margin measures available in the CPS: weekly employment, weekly participation, annual employment, and annual participation. Weekly measures are based on respondents' activities during the last week and are available in all Basic Monthly and ASEC files. Annual measures are based on respondent's activities during the last year and are only available in the ASEC.

Weekly Measures: Individuals' weekly employment and participation statuses are determined on the basis of answers to a series of questions relating to their activities during the preceding week. Upon answering these questions, respondents are grouped into eleven categories: "armed forces," "at work," "has a job, not at work last week," "unemployed, experienced worker," "unemployed, unexperienced worker," "housework," "unable to work," "school," "other," "unpaid, less than 15 hours," and "retired." Respondents classified as "at work" include those who either did any work for pay or profit or worked for at least fifteen hours without pay in a family business or farm. Respondents classified as "has a job, not at work last week" include those who did not work during the previous week but who acknowledged having a job or business from which they were temporarily absent (e.g. due to illness, vacation, or labor dispute). Individuals who do not fall into the above two categories but who reported either being temporarily laid off or actively searching for work are classified as unemployed.[51] Respondents who do not fall into any of the above categories are classified as not in labor force and distributed among the remaining six categories: "housework," "unable to work," "school," "other," "unpaid, less than 15 hours," and "retired."

Annual Measures: These are determined on the basis of questions in the ASEC pertaining to respondents' activities last year. The annual measures of employment and participation are based on different questions than the weekly measures. Annual employment is determined based on

[51] Respondents were considered to be actively searching for work if they were either looking for work as their major activity during the previous week (for 1962 through 1993) or answered yes to a question about whether they had been looking for work in the past four weeks (for 1994 onwards).

respondents' earnings last year. Respondents with positive earnings are classified as employed last year. Annual participation is based on the number of weeks a respondent was either working or searching last year. Respondents who either worked or were looking for work for one or more weeks last year are classified as having participated last year.

B.1.2 Historical Changes to the CPS

In January 1994, the questions regarding labor force status (which underlie the weekly measures of employment and participation) underwent certain changes. A primary motivation for this redesign was to better classify individuals engaged in informal or intermittent activities. The redesign included a number of changes, all of which are explained in detail in Cohany *et al.* (1994). I focus here on the changes most pertinent to my analysis.

Prior to 1994, respondents were asked an "ice-breaker question" about their main activity during the preceding week. The question took the form "what were you doing most of last week? were you keeping house/working/in school or something else?" where the choice of prompt depended on the respondent's age and sex. The Bureau of Labor Statistics (BLS) concluded that this question led to an underreporting of women in part-time work. Additionally, respondents who indicated that they did not have a job were asked a follow-up question of the form "why were you absent from work last week?" Due to its open-ended nature, this question may have led to underreporting of respondents who were temporarily laid off. Beginning in 1994, these questions were redesigned to have more specific wording and fewer open-ended responses. For example, the initial "ice-breaker question" was replaced with a question asking if the respondent did any work for pay or profit last week. Similarly, respondents who indicated they did not have a job were asked whether they were laid off and if the layoff was temporary.

To asses the impact of this redesign on estimates of labor force participation, the BLS ran a parallel survey from July 1992 through December 1993 that interviewed households using the new survey questions. Cohany *et al.* (1994) examine the differences between official CPS and parallel survey estimates in a variety of metrics. They find that for women aged 20 and above, the weekly employment rate was 55.1% in the official CPS and 55.8% in the parallel survey, a difference of only 0.7 percentage points. The weekly participation rate was 58.5% in the official CPS and 59.6% in the parallel survey, a difference of 1.1 percentage points. When including controls for state of residence, race, and hispanic origin, these differences drop to 0 and 0.1 percentage points, respectively. These differences are too small to have any substantial effect on the analysis. In any case,

the difference-in-differences design reduces this issue even further, or eliminates it entirely, by including year fixed effects.

B.1.3 Nonresponse in the CPS

The CPS is subject to two types of nonresponse: noninterview households and item nonresponse. Noninterview occurs when a household refuses to participate in the survey altogether and is especially common in March, corresponding to the delivery of the ASEC. In the Basic Monthly CPS, noninterview is accounted for by distributing the weights of noninterview households among interviewed households. In the ASEC, noninterview is accounted for by imputing missing values.

The second source of data loss, item nonresponse, occurs when respondents refuse to answer specific questions within the survey. To compensate for item nonresponse (and for noninterview in the ASEC), the BLS imputes missing values using one of three methods. First, if possible, missing values are inferred from other characteristics of a respondent or other respondents within the same household. For example, if a respondent has a missing value for race, it is assigned based the race of other household members. These edits, known as relational edits, are most commonly used for demographic variables. Next, if relational edits are not possible, longitudinal edits are made. Longitundial edits exploit the panel nature of CPS data and use respondent's entries from previous months to fill in missing values. Labor force items are typically imputed using longitudinal edits. Finally, if neither of the above are possible, the CPS uses a "hot-deck" imputation method. The "hot-deck" method assigns a missing value from a record with similar characteristics, called the hot deck. Hot decks are made up of demographic characteristics such as age, race, sex, occupation and educational attainment. The specific characteristics that make up a hot deck vary depending on which variable is being imputed.

How common is nonresponse in the CPS? Historically, nonresponse in the CPS was very modest, but it has grown significantly over time (see e.g., Meyer *et al.* 2015; Bollinger *et al.* 2017; Jones & Ziliak 2019). Household non-interview rates have risen from 7-9 percent in 2004 to 13-15 percent in 2017.[52]

As for item nonresponse, two points are worth mentioning. First, item nonresponse is much smaller for demographic and labor force variables than it is for earnings. In 2018, only 0.45% of the respondents in the estimation sample have imputed labor force status and 3.1% have imputed

[52]These statistics have been retrieved from https://www.census.gov/prod/2006pubs/tp-66.pdf and https://www.census.gov/programs-surveys/cps/technical-documentation/methodology/non-response-rates.html.

demographics (marital status, age, or race), compared to 17.9% with imputed earnings. Second, the degree of item nonresponse in earnings has increased over time. While 17.9% of respondents have imputed earnings in 2018, this number was only 10.9% in 1970.[53] The significant degree of nonresponse and imputation in the earnings variable is another argument for using the weekly measures of extensive margin labor supply, as I do here.

B.1.4 Sample and Variables

I restrict the sample to single women (never married, separated, divorced, or widowed) aged 20-50.[54] I drop observations with a zero, negative, or missing weight (wgt), missing state FIPS code ($stfips$), or missing educational attainment ($educ$). The difference-in-differences analyses are based on comparing single women with EITC eligible children (treatment group) to single women without recorded children (control group). The control group includes both those who never had any children and those whose children do not live at home. A small fraction of single women with EITC *ineligible* children living at home are dropped from the sample.[55] These restrictions leave me with a sample of 4,858,644 individual-month observations across survey years 1968-2018.

Unless otherwise specified, variables in the ASEC and monthly files are defined in the same way. Variables based on income and welfare participation are only available in the ASEC.

- Unique household ID (hid): This variable is my best attempt at a unique identifier for each household in the CPS. In the monthly files, households can be uniquely identified and tracked across subsequent months using IPUMS variable CPSID. In the ASEC files, the variable CPSID is unavailable so I instead identify households using a combination of IPUMS variables YEAR and SERIAL. As a result, respondents in the ASEC cannot be linked to those in the monthly files and the variable $hhid$ only uniquely identifies households in the monthly and ASEC files separately.

- Unique person ID (id): This variable is a unique combination of $hhid$ and IPUMS variable PERNUM, which uniquely identifies individuals within a household.

[53] The growth in item nonresponse rates has also been quite large for other income variables, including income from social assistance programs. See Meyer *et al.* (2015) and Meyer & Mittag (2019) for an investigation of item nonresponse bias in questions pertaining to social assistance receipt.

[54] Except for one analysis in the online appendix in which I consider a sample of married women.

[55] For example, this includes women with children who recently turned 19 and are not full-time students. The reason for dropping these observations (as opposed to assigning them to the control group) is that most of them would have been EITC eligible in the recent past and are therefore borderline cases between the treatment and control groups. In any case, assigning them to the control group does not make much of a difference to any of the results.

- Number of eligible children ($nechild$): This variable identifies the number of EITC eligible children a respondent has. An ETIC eligible child is defined as a household member who is either under 19 or who is under 24 and a full time student (EMPSTAT = 33). Using a combination of household ID ($hhid$) and IPUMS variables MOMLOC and POPLOC, I link respondents to their biological and adoptive children. I then look to the age and education associated with each child's observation to establish whether the child is EITC eligible. For more detail on how to link respondents to their children, see https://cps.ipums.org/cps-action/variables/MOMLOC#description_section.

- Age of youngest child ($ageyc$): The minimum age of all EITC eligible children. Takes on a value of 99 if respondent has no children.

- Single ($single$): Takes on a value of one if the respondent is separated (MARST = 3), divorced (MARST = 4), widowed (MARST = 5), or never married (MARST = 6), a value of zero if the respondent is married with spouse present (MARST = 1) or married with spouse absent (MARST = 2), and is missing otherwise.

- Age (age): this variable is taken from the IPUMS age variable and is top-coded at 90.

- Gender ($female$): takes on a value of one if IPUMS variable SEX = 2 and zero otherwise.

- Education Level ($edlevel$): takes on a value of one if the respondent has less than a HS education (IPUMS variable EDUC = 2-72), a value of two if the respondent has a HS diploma or equivalent (EDUC = 73), and a value of three if the respondent has more than a HS education (EDUC = 80-125).

- Low-educated ($lowed$): takes on a value of one if respondent has less than a HS education ($edlevel = 1 - 2$) and zero otherwise.

- Alternate low-educated ($lowedA$): takes on a value of one if respondent has less than a HS education ($edlevel = 1 - 2$) and zero if the respondent has a a college degree or above ($EDUC = 91 - 125$).

- AFDC receipt ($afdc\_annual$): takes on a value of one if respondent receives AFDC/TANF (SRCWELFR = 1) or both AFDC/TANF and another type of welfare (SRCWELFR = 3), takes on a value of zero if respondent doesn't receive welfare (SRCWELFR = 0) or receives only another type of welfare (SRCWELFR = 2), and is missing otherwise.

- Weekly employment (emp): takes on a value of one if respondent is in the armed forces (EMPSTAT = 1), working (EMPSTAT = 10), or has a job but is not at work (EMPSTAT = 12), a value of zero if respondent is unemployed (EMPSTAT = 20-22), or not in the labor force (EMPSTAT = 30-36), and is missing otherwise.

- Weekly participation (lfp): takes on a value of one if the respondent is in the armed forces (EMPSTAT = 1), working (EMPSTAT = 10), has a job but is not at work (EMPSTAT = 12), or is unemployed (EMPSTAT = 20-22), a value of zero if the respondent is not in the labor force (EMPSTAT = 30-36), and is missing otherwise.

- Annual employment (emp_annual): takes on a value of one if person had positive earnings last year (IPUMS variable INCWAGE > 0), zero if they had zero earnings last year.[56]

- Annual participation (lfp_annual): takes on a value of one if the respondent worked (WKSWORK1) or looked for work (NWLOOKWK) for at least one week last year and takes on a value of zero if the respondent didn't look for work at all last year (WKSWORK1 = 0 and NWLOOKWK = 0), and is missing otherwise.

- Income ($wsal$): the $wsal$ variable comes from the IPUMS variable INCWAGE. Values of 9999999 and 9999998 are recoded to be missing.

- Person weight (wgt): in the ASEC this variable is equal to the IPUMS variable ASECWT; in the monthly files this variable is equal to the IPUMS variable WTFINL.

- State unemployment rate ($st\_unemployed$): the state unemployment rate is calculated by dividing the number of unemployed respondents (EMPSTAT = 20-22) by the number of respondents in the armed forces (EMPSTAT = 1), working (EMPSTAT = 10), or with a job but not at work (EMPSTAT = 12) in a given state.

B.2 Supplementary Data

Data on state welfare waivers comes from the Department of Health and Human Services (HHS).[57] I follow HHS and consider only major statewide waivers in the following six categories: termination time limits, work requirement time limits, JOBS exemptions, JOBS sanctions, family caps, and

[56] When using the annual employment variables ($lfply$ and $emply$) as outcomes, I substitute $year$ for $year - 1$ to reflect the fact that the employment measure refers to the previous year.

[57] Health and Human Services, Assistant Secretary for Planning and Evaluation, Setting the Baseline: A Report on State Welfare Waivers. Retrieved from https://aspe.hhs.gov/report/setting-baseline-report-state-welfare-waivers.

earnings disregards. I use either the dates of approval or the dates of implementation to create a state-by-year dataset that contains indicators for each waiver type that are equal to 1 in all years post-approval (or post-implementation) and 0 otherwise. I define the first post-approval year to be the year of approval, no matter the time of year the waiver was approved. The any-waiver indicator is equal to one if any statewide waiver was in effect in that year.

Data on federal EITC parameters come from the Tax Policy Center.[58] Data on state EITC parameters come from the Tax Policy Center,[59] the National Bureau of Economic Research (NBER),[60] and various state-specific sources.

[58] Tax Policy Center. "Earned Income Tax Credit Parameters, 1975-2018." Retrieved from https://www.taxpolicycenter.org/statistics/eitc-parameters.

[59] Tax Policy Center. "State EITC as Percentage of the Federal EITC" Retrieved from https://www.taxpolicycenter.org/statistics/state-eitc-percentage-federal-eitc.

[60] NBER. "State EITC provisions 1977-2016." Retrieved from https://users.nber.org/∼taxsim/state-eitc.html.

C Elasticity Calculations

The extensive margin elasticity with respect to the net-of-tax rate on labor force participation is defined in equation (2).

The numerator of the elasticity ($\Delta P/P$) is calculated as the difference-in-differences between single women with and without children (or between different numbers of children) after 10 years. Specifically, ΔP corresponds to the coefficient estimate $\hat{\gamma}_{2003}$ in equation (1), while P is the baseline employment or participation rate in 1993. The values of ΔP and P for each family size are shown in Table 3.

The denominator of the elasticity ($\Delta\left(1-\tau\right)/\tau$) is calculated based on a pre-reform sample of single women (years 1991-1993), predicted earnings from the specification in (3), and a tax-benefit calculator. The tax parameters τ and $\Delta\tau$ are calculated at the individual level, but the denominator of ε is based on the population averages. The following subsections describe the details of the calculation of τ and $\Delta\tau$.

C.1 Baseline Net-of-Tax Rate

Tax Simulations:

Calculating the baseline net-of-tax rate $1-\tau$ in the full sample requires a measure of earnings conditional on working for both workers and non-workers. I use actual observed earnings for workers and predicted earnings from (3) for non-workers. Based on these earnings measures, I simulate tax liabilities from state income taxes, federal income taxes, and payroll taxes using NBER's tax simulation model (TAXSIM). TAXSIM requires information on income, dependents, and demographics.[61] The following list describes the mapping between TAXSIM variables (shown in parentheses) and CPS variables:

- Marital status ($mstat$) is set as "single or head of household" (corresponding to a value of one) for all observations.
- Age ($page$) is equal to the variable age described in appendix section B.1.4.
- Number of dependents ($depx$) uses IPUMS variable NCHILD, which corresponds to the number of own children at home.

[61] The full list of TAXSIM inputs is listed online at https://users.nber.org/~taxsim/taxsim27/

- Number of children under 13 with eligible child care expenses ($dep13$) is equal to number of children at home that are under 13. Uses variables NCHILD (described above) and $ageyec$ (described in appendix section B.1.4).
- Number of children under 17 for the entire tax year ($dep17$) is equal to number of children at home that are under 17. Uses variables NCHILD (described above) and $ageyec$ (described in appendix section B.1.4).
- Number of qualifying children for EITC ($dep18$) is equal to variable $nechild$ (described in appendix section B.1.4).
- Earnings from wages and salary ($pwages$) is equal to observed earnings for workers (variable inc, described in appendix section B.1.4) and predicted earnings for non-workers (estimated from the earnings regression in eq. 3). The earnings regression includes dummies for the age of the woman (20-24, 25-29, 30-34, 35-39, 40-44, 45-50), number of EITC eligible children (0, 1, 2, 3, 4, 5, 6+), age of youngest child (0-1, 2-3, 4-6, 7-9, 10-13, 14-17, 18+), education level (below high school, high school degree, some college, college degree), race (white, non-white), state of residence, and two-way interactions between education, age, number of children, and age of youngest child. The specification is weighted by CPS weights and run on the sample of single women with positive earnings in the pre-reform years, 1991-1993 (all adjusted to 1993 USD). Using the parameters from this regression, earnings are predicted for non-workers. Those with predicted earnings below zero are dropped (only 7 observations).
- Dividends ($dividends$) comes from IPUMS variable INCDIVID. INCDIVID indicates how much pre-tax income the respondent received from stocks and mutual funds.
- Interest received ($intrec$) comes from IPUMS variable INCINT. INCINT indicates how much pre-tax income the respondent received from interest on saving accounts, certificates of deposit, money market funds, bonds, treasury notes, IRAs, and/or other investments which paid interest.
- Other property income ($otherprop$) comes from IPUMS variable INCRENT. INCRENT indicates how much pre-tax income the respondent received from rent (after expenses), from charges to roomers or boarders, and from money paid by estates, trusts, and royalties.
- Gross Social Security benefits ($gssi$) comes from IPUMS variable INCSS. INCSS indicates how much pre-tax income the respondent received from Social Security payments.

- Unemployment insurance (ui) comes from IPUMS variable INCUNEMP. INCUNEMP indicates how much pre-tax income the respondent received from state or federal unemployment compensation, Supplemental Unemployment Benefits (SUB), or union unemployment or strike benefits.

- Age and wage of spouse ($sage$, $swages$) are set to zero as the sample only includes single women.

- Other income ($stcg$, $ltcg$, $mortgage$, $nonprop$, $pensions$, $rentpaid$, $proptax$, $otheritem$, $childcare$) are set to zero as they are not observed in the CPS.

For each individual, tax liability is simulated when working and when not working. Hence, TAXSIM is run twice, once where earnings from wages and salary is set equal to actual/predicted earnings (as described above) and once where earnings is set equal to zero. Based on the simulations of state income tax, federal income tax, and payroll tax liabilities, the average tax rate ATR can be calculated as follows

$$ATR = \frac{(fed_1 - fed_0) + (st_1 - st_0) + FICA}{pwages}$$

where fed_1 and fed_0 (st_1 and st_0) are the federal (state) income tax liabilities when working and not working, respectively, and $FICA$ is the payroll tax liability (Federal Insurance Contributions Act tax) when working.

Welfare Simulations:

In addition to taxes, the participation tax rate also accounts for welfare benefits that are lost when entering the labor market. Annual benefits from AFDC and Food Stamps when not working (B_0) are calculated as

$$B_0 = (maxAFDC + maxFS) \cdot 12$$

where $maxAFDC$ and $maxFS$ denote the maximum monthly benefits from AFDC and Food Stamps (in 1993), which vary by family size and by state.

For those who are working, benefits from AFDC ($AFDC_1$) and Food Stamps (FS_1) are clawed back as earned income increases. However, part of earned income is disregarded when determining benefit levels. Specifically, monthly benefits are calculated as follows

$$
\begin{aligned}
AFDC_1 &= \min\left(PaymentStandard - NetIncomeAFDC, maxAFDC\right) \cdot \mathbf{I}_{AFDC} \\
FS_1 &= \left(maxFS - 0.3 \cdot NetIncomeFS\right) \cdot \mathbf{I}_{FS},
\end{aligned}
$$

where the indicators $\mathbf{I}_{AFDC}$ and $\mathbf{I}_{FS}$ are equal to one when the calculated AFDC and Food Stamp benefits are non-negative and equal to zero otherwise. The net income measures ($NetIncomeAFDC$ and $NetIncomeFS$) are defined as follows

$$
\begin{aligned}
NetIncomeAFDC &= \max\Big(GrossIncome - WorkExp - ChildCareExp \cdot n \\
&\quad - EarningsDisregardAFDC, 0\Big) \\
NetIncomeFS &= \max\Big(GrossIncome - ShelterCosts - ChildCareExp \cdot n \\
&\quad - StandardDeduction - EarningsDisregardFS, 0\Big).
\end{aligned}
$$

The following list describes the components of the monthly benefit calculation:

- Payment Standard ($PaymentStandard$) denotes a baseline AFDC benefit level (before claw-back) used to compute benefits for working individuals in 1993. The payment standard varies by family size and by state. It is typically equal to $maxAFDC$, but some states set $PaymentStandard$ to a level above $maxAFDC$.

- Gross Income ($GrossIncome$) includes monthly wage and salary income as well as unearned income (e.g., interest income). Unearned income is assumed to be zero for all individuals in the sample. Therefore, the monthly gross income equals monthly wages and salary ($Earnings$).

  - For individuals who are observed working, $Earnings$ are calculated by dividing annual wage and salary income ($pwages$) by the number of months worked ($MonthsWorked$). The variable $MonthsWorked$ is calculated from the IPUMS variable WKSWORK1, which corresponds to the number of weeks worked. For individuals who are not working, $Earnings$ are calculated based on their predicted annual earnings conditional on working (from the regression in eq. 3) and an imputed value of $MonthsWorked$. The imputation of $MonthsWorked$ for non-workers is based on the average value for workers in different cells of family size n.

- Work Expenses Disregard ($WorkExp$) denotes work-related expenses such as transportation costs. Following Fajgelbaum *et al.* (2020), $WorkExp$ are set equal to the maximum disregard in 1993 ($90).

- Child Care Expenses Disregard ($ChildCareExp$) denotes child care expenses incurred per child. Following Fajgelbaum *et al.* (2020), $ChildCareExp$ are set equal to the maximum disregard in 1993 ($175 per child for AFDC and $160 per child for Food Stamps).

- AFDC Earnings Disregard ($EarningsDisregardAFDC$) denotes an earned income disregard, which provides AFDC recipients a financial incentive to seek and maintain employment. It is equal to $\$30 + 0.33 \cdot Earnings$ per month for the first 4 months worked and $30 per month for all subsequent months.

- Standard Deduction ($StandardDeduction$) is a monthly deduction to account for basic costs, which varies by state.

- Food Stamps Earnings Disregard ($EarningsDisregardFS$) denotes an earned income disregard which provides Food Stamps recipients a financial incentive to seek and maintain employment. It is equal to $0.2 \cdot Earnings$.

- Shelter Deduction ($ShelterCosts$) is an allowance for excess housing costs. It is equal to housing expenses (such as rent and utilities) exceeding 50 percent of "net income," up to a maximum. Here, net income is computed as $Earnings$ net of the standard deduction ($StandardDeduction$) and the earnings disregard ($EarningsDisregardFS$). Following Fajgelbaum *et al.* (2020), it is assumed that households living in rented properties incur housing expenses equal to 30 percent of monthly earnings, whereas households living in owned properties are assumed to have no allowance for excess housing costs. Property ownership is determined using the IPUMS variable OWNERSHP, which indicates whether the household rented or owned its housing unit. The maximum shelter cost deduction varies by state.

The annual benefits from AFDC and Food Stamps when working (B_1) are calculated as follows

$$\begin{aligned} B_1 &= AFDC_1 \cdot MonthsWorked + maxAFDC \cdot (12 - MonthsWorked) \\ &\quad + FS_1 \cdot MonthsWorked + maxFS \cdot (12 - MonthsWorked) \end{aligned}$$

When working, individuals receive benefits $AFDC_1$ and FS_1 each month, and in the remaining months they receive the maximum benefits $maxAFDC$ and $maxFS$.

Participation Tax Rate:

Finally, the participation tax rate can be calculated as follows

$$\tau = ATR + takeup\_rate \cdot \frac{B_0 - B_1}{pwages},$$

where $takeup\_rate$ scales welfare benefits to account for incomplete benefit take-up among eligibles. Based on the evidence presented in Blank & Ruggles (1996), Blank (2001), and Currie (2006), the average take-up rate is set equal to 70%. The participation tax rate is top-coded at 0.99.

C.2 Change in Tax Rate

To calculate the average change in the participation tax rate, $\Delta(1-\tau)$, I focus exclusively on the changes implied by the EITC expansion. For each individual, I calculate a pre-reform EITC subsidy (1993 rules) and a post-reform EITC subsidy (1996 rules) using baseline earnings in 1993.[62] The EITC is a function of earnings ($pwages$), family size n, and year t. It is calculated as follows

$$EITC = \begin{cases} pwages \cdot phase\_in_{nt} & \text{if } pwages < k1_{nt} \\ maxcredit_{nt} & \text{if } k1_{nt} \leq pwages \leq k2_{nt} \\ maxcredit_{nt} - phase\_out_{nt} \cdot (pwages - k2_{nt}) & \text{if } k2_{nt} \leq pwages \leq exhaust_{nt} \\ 0 & \text{if } pwages > exhaust_{nt} \end{cases}$$

where $phase\_in$ and $phase\_out$ denote the EITC phase-in and phase-out rates, $maxcredit$ is the maximum possible EITC refund, $k1$ and $k2$ are the first and second kink points of the EITC, while $exhaust$ is the point of EITC exhaustion. The change in the net-of-tax rate equals the difference between the pre- and post-reform EITC credits (1993 vs 1996, under baseline earnings $pwages$), $EITC_{96} - EITC_{93}$, divided by baseline earnings $pwages$. Labeling this difference $\Delta(1-\tau_n)$ for family size n, the tax rate change that enters into the elasticity formula (2) equals the difference-in-differences $\Delta(1-\tau) = \Delta(1-\tau_n) - \Delta(1-\tau_0)$. This calculation assumes 100% take-up of the EITC conditional on eligibility. Given incomplete take-up, the net-of-tax rate change $\Delta(1-\tau)$ will be

[62] The calculation of the post-reform EITC under baseline earnings uses 1993 earnings measured in 1996 USD.

upward biased and the elasticity ε is therefore conservative.

D Synthetic Control Approach

This analysis is implemented by creating a synthetic control state for each state with an EITC supplement, and then running a stacked event study comparing treatment and synthetic control states around state EITC introductions. Table A.II lists all states with an EITC supplement and provides key details about those supplements.

D.1 Constructing synthetic controls

To run the synthetic control analysis, CPS data is collapsed into state-by-year observations. The data is collapsed separately for single women with and without children. I consider an event study window from five years before to five years after each reform. Since the monthly files can only be used from 1989 onwards, to ensure that each reform has a consistent dataset across the event window, the analysis uses the March CPS files alone for reforms that occurred before 1993 and the March and monthly CPS files combined for reforms that occurred from 1993 onwards. Event time is set to zero in the first year after the introduction of the EITC supplement.

I focus on the 27 states that implemented and maintained an EITC supplement for at least 3 years.[63] For each of these treatment states, a synthetic control state is constructed from states that never had a supplement, matching on the level of the employment rate in each of the five pre-reform years. Table A.VI shows the makeup of each synthetic state. Synthetic control regressions are run using the stata command `synth`.[64]

In the main specification, synthetic control regressions are run on the sample of single women with children. Hence, the empirical strategy is a difference-in-differences comparing different states over time, conditioning on children. As a robustness check, I consider a triple-differences specification that also exploits the variation between those with and without children within states.

D.2 Stacked Event Study

Having obtained measures of employment in treatment and synthetic control states, a stacked event study specification is used to estimate the average effect across all state EITC reforms. The event study is based on the following specification

[63] A total of 30 states have instituted a supplement (see Table A.II). But the state of Washington never funded and paid out the credit, while Hawaii and South Carolina instituted their supplements only in 2018.

[64] See http://fmwww.bc.edu/RePEc/bocode/s/synth.html for documentation on the `synth` command.

$$P_{st} = \sum_j \alpha_j \cdot Event_{j=t} + \beta \cdot Treat_s + \sum_{j \neq -1} \gamma_j \cdot Event_{j=t} \cdot Treat_s + \nu_{st}, \quad (8)$$

where P_{st} is the employment rate in state s at time t, $Event_{j=t}$ is an indicator for event time t, and $Treat_s$ is an indicator for treatment states. In Figure 12, the treatment series (solid line) corresponds to the coefficient $\hat{\alpha}_t + \hat{\gamma}_t$, while the synthetic control series (dashed line) corresponds to $\hat{\alpha}_t$. Two different samples of treatment states are considered. In panel A of Figure 12, the treatment sample includes all 27 states that implemented and maintained a supplement for at least three years. In panel B, the treatment sample includes only the 12 states with "large EITC reforms", defined as the introduction of *refundable* supplements that reached at least 10% of the federal EITC within three years of enactment.[65]

Figure A.XXIII is based on a similar analysis, but it adds the variation from children (within states) to the variation across states in a triple-differences design. Specifically, the analysis is based on the following specification

$$\begin{aligned} P_{kst} = & \sum_j \alpha_j \cdot Event_{j=t} + \beta \cdot Kids_k + \gamma \cdot Treat_s + \delta \cdot Kids_k \cdot Treat_s \\ & + \sum_{j \neq -1} \zeta_j \cdot Event_{j=t} \cdot Kids_k + \sum_{j \neq -1} \eta_j \cdot Event_{j=t} \cdot Treat_s \\ & + \sum_{j \neq -1} \theta_j \cdot Event_{j=t} \cdot Kids_k \cdot Treat_s + \nu_{kst} \end{aligned} \quad (9)$$

where P_{kst} is the employment rate for those with kids status k (with or without) in state s at time t. Here, the treated series (solid line) corresponds to the coefficient $\hat{\zeta}_t + \hat{\theta}_t$, while the synthetic control series (dashed line) corresponds to $\hat{\zeta}_t$.

[65] The 12 states with large EITC reforms are California (2015), Connecticut (2011), Dist. of Columbia (2000), Kansas (1998), Massachusetts (1997), Michigan (2008), Minnesota (1991), New Jersey (2000), New Mexico (2007), New York (1994), Vermont (1988), and Wisconsin (1989).

E Stacked Event Study of All Federal and State Reforms

The purpose of the stacked event study analysis is to capture the average effect across all 31 EITC reforms at the state and federal level.[66] The analysis is implemented by converting calendar time around each reform into event time (with $t = 0$ denoting the first year after each reform), and then stacking the CPS data for the 31 events. The window around each reform runs from event time -5 to +5, except for the federal reform in 1993 (OBRA93) where the window runs from event time -5 to +2.[67] This avoids overlap between the 1993 EITC reform and TANF reform, which we cannot control for. Given the monthly files can be used only from 1989, to ensure that each reform uses consistent data across time, I use March CPS files alone for reforms that occurred before 1993 and March and monthly CPS files combined for reforms that occurred from 1993 onwards.

The stacked event study is based on the following specification

$$\begin{aligned} P_{imt} \quad = \quad & \sum_j \alpha_j \cdot Event_{j=t} + \beta \cdot Kids_i + \sum_{j \neq -1} \gamma_j \cdot Event_{j=t} \cdot Kids_i \\ & + \mathbf{I}_{\text{OBRA93}} \cdot \Big(\sum_j \delta_j \cdot Event_{j=t} \cdot Waiver_{sj} + \sum_j \zeta_j \cdot Event_{j=t} \cdot Kids_i \cdot Waiver_{sj} \\ & + \eta \cdot U_{st} + \theta \cdot U_{st} \cdot Kids_i + \lambda_s \Big) + \boldsymbol{X_i \phi} + \nu_{imt}, \end{aligned} \tag{10}$$

where the first line is the basic DiD event study specification, and the second and third lines add controls for welfare waivers, the business cycle, and demographics. The waiver and business cycle controls are switched on only for the 1993 federal reform ($\mathbf{I}_{\text{OBRA93}} = 1$) where they are necessary for identification. For the 2009 federal reform, the dummy variable $Kids_i$ is an indicator for having 3+ children (relative to 0 children), while for all other reforms it is an indicator for having any children. Notice also that, for the stacked event study, the state reforms are analyzed by comparing single women with and without children within each state. This is similar to the DiD analyses of federal reforms elsewhere in the paper, but different from the more involved synthetic control approach to state reforms in section 6.1 (which exploits variation both by the presence of children and across different states).

The results of this analysis are presented in Figure 13 in the main text and Figure A.XXIV in the appendix. Panel A of these figures is based on running specification (10) for the federal reforms

[66] These are the 27 state EITC reforms analyzed in section 6.1 along with four federal reforms: the 1975, 1986, 1993, and 2009 reforms. I exclude the 1990 reform as a separate event because of its small size and close proximity in time to the 1986 and 1993 reforms.

[67] The other exception is the state reform enacted in California in 2015. This reform allows for an event time window running only to +2.

alone, Panel B runs (10) for the state reforms alone, while Panel C runs (10) for the federal and state reforms combined. Because there is a gradual pre-trend in Panel C, the final panel considers an extension of (10) that adjusts for this pre-trend.

The pre-trend adjustment is done based on estimating linear, group-specific trends using only pre-reform data. This consists of two steps. First, using pre-reform data (event time -5 to -1), I run a version of equation (10) in which the event time dummies $Event_{j=t}$ in the first line are replaced by a continuous event time variable t. Hence, this specification includes the same control variables as the specification of interest. This yields pre-trend coefficients for those with and without kids, $\hat{\vartheta}_{k=1}$ and $\hat{\vartheta}_{k=0}$. Second, I residualize the outcome variable P_{imt} using these coefficients, i.e. $\hat{P}_{imt} \equiv P_{imt} - \hat{\vartheta}_{k=0} \cdot t - \hat{\vartheta}_{k=1} \cdot t \cdot Kids_i$. Equation (10) is then run using $\hat{P}_{imt}$ as the outcome.

The individual reforms are reweighted to adjust for changes in CPS sample size over time. In an unweighted stacked regression, later reforms would tend to weigh more heavily than earlier reforms due to the increase in CPS samples over time. To adjust for this, the CPS weights are rescaled such that all federal reforms are weighted equally and all state reforms are weighted according to their share of the national population in 2016. Specifically, the rescaling factor for each reform equals $\frac{1}{s/pop_{2016}}$ where s is the number of sample observations for a given reform and pop_{2016} is the national or state population in 2016, depending on whether the observation is from a federal or state reform. This rescaling factor is multiplied by the individual CPS weights.