The EITC and the Extensive Margin: A Reappraisal*

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Abstract

This paper reappraises the impact of the Earned Income Tax Credit (EITC) on labor supply at the extensive margin for single mothers. I investigate every EITC reform at the state and federal level since the inception of the policy. Apart from the federal 1993 reform, EITC expansions have not had any clear and significant effects on employment. The 1993 reform is associated with large employment effects, but these effects align more closely with confounding changes from welfare reform and the macroeconomy than with the EITC. I conduct a comprehensive analysis of the robustness of the EITC null result to model uncertainty.

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1 Introduction

Anti-poverty policy in the United States has changed fundamentally over the last fifty years. 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. The transition from welfare state to workfare state is illustrated in Figure A.1, which shows that the EITC program now dwarfs cash welfare by a factor of almost 30 in terms of the number of recipients. A key motivation for this policy shift 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.

A large literature on the EITC features a striking degree of consensus. Most authors argue that the program has had sizable effects on extensive margin labor supply, especially for single mothers. Starting with the important contributions by Eissa and Liebman (1996) and Meyer and 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 in support of extensive margin responses is the large increase in the employment rate of single mothers, especially single mothers with two or more children, following the 1993 EITC expansion for these family types (e.g., Meyer 2010; Hoynes and Patel 2018).

Given the importance of the historical change in welfare state design and the recurring 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 at the extensive margin (see e.g., Blundell and MaCurdy 1999; Chetty, Guren, Manoli, and Weber 2013). In this paper, I do four things to shed new light on the EITC. First, I take a long-run perspective and consider every EITC reform at the federal and state level since the inception of the program in 1975. Second, I study all reforms in an event study framework, investigating the dynamics of labor supply changes before and after legislated reforms. Third, I provide a comprehensive study of model uncertainty, comparing prior EITC estimates to the distribution of estimates under different empirical models. Fourth, I carefully investigate the role of confounding

factors, especially for the 1993 reform which underpins much of the consensus. I am not the first to investigate the role of confounders in the 1990s, but I provide new and transparent analyses of the issue.

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 historical 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.

Following the descriptive long-run evidence, I present event studies of EITC reforms at the federal and state levels. For federal reforms, the analyses are based on difference-in-differences specifications comparing single women with and without children, or single women with different numbers of children. For state reforms, the analyses are based on either difference-in-differences specifications comparing single mothers in states with and without EITC reform or triple-differences specifications comparing single women with and without children in states with and without EITC reform. Apart from the 1993 federal reform, the event studies are essentially flat around the various EITC expansions. This holds in the sample of all single mothers and in samples of single mothers more strongly treated by the EITC. For the 1993 federal reform, on the other hand, the estimated effects are consistently positive and large.

Empirical designs that define treatment status based on fertility rest on strong assumptions, because the impact of children on labor market outcomes is very large (see Kleven, Landais, and Søgard 2019; Kleven 2023). This creates significant imbalance in the levels of pre-reform outcomes

[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.

for treatment and control groups. Investigating pre-trends is useful for assessing the validity of the difference-in-differences approach and these generally look good, but concerns remain that contemporaneous shocks affect treatment and control groups differently. Hence, the central challenge for estimating causal impacts lies in specifying an empirical model that avoids bias from confounding shocks. This paper takes an agnostic approach to the issue of model uncertainty, estimating EITC impacts across a wide range of specifications. These specifications consider different reform episodes, different samples, different comparison groups, different extensive margin measures, and different control variables. Allowing for all possible permutations of specification choices yield hundreds of treatment impact estimates. Leaving aside the 1993 federal reform, the estimates are symmetrically distributed around zero. I show that prior estimates are strong outliers in the distribution of possible estimates. For the 1993 reform, on the other hand, the distribution of estimates is shifted to the right and has a mean elasticity of 0.63.

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, Friedman, Olsen, and Pistaferri 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. A more natural interpretation 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 and Rosenbaum 2001; Blank 2002; Grogger 2003; Fang and 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 in the 1990s? First, there was welfare reform. This includes a large number of state welfare reforms (so-called welfare waivers) between 1992-96 and the federal welfare reform act of 1996. These policy changes introduced constraints on welfare receipt such as time limits and work requirements, pushing single mothers off cash welfare and producing extensive margin incentives correlated with the EITC.[2] Second, there was a booming macroeconomy. Business cycles may have heterogeneous effects on single women with and without children, especially at a time where welfare reform puts strong pressure on single mothers to find employment. Third, changes to social norms may have played a role as well. I provide evidence that attitudes

[2] As I discuss in the paper, estimates from other EITC reforms (especially the 1986 reform) may also be subject to upward bias from contemporaneous changes to the welfare system.

towards welfare receipt and work changed dramatically in the 1990s — the passage of welfare reform arguably grew out of those attitude changes — and such social norms could have had an independent effect on behavior.

I provide a detailed investigation of confounders in the 1990s, showing that the empirical patterns align more closely with confounders than with EITC reform. Let me outline some of the key findings. First, simulation exercises reveal that the fraction of the employment increase for single mothers attributable to the 1993 EITC expansion is small even under large extensive margin elasticities. In other words, within any reasonable range of tax elasticities, the patterns around the 1993 reform must be driven mostly by confounders and not by the EITC. Second, the extensive margin effects for single mothers following the 1993 reform are strongly increasing in the number of children.[3] Such fanning-out by family size is exactly consistent with welfare reform, because pre-reform welfare participation and therefore welfare treatment was strongly increasing in family size. EITC reform, on the other hand, is not consistent with this pattern. Third, using a rich set of demographic variables to predict welfare treatment intensity, I show that the extensive margin effects in the 1990s are driven precisely by those most treated by welfare reform.[4] EITC-eligible women who were only weakly affected by welfare reform did not increase labor supply at the extensive margin. Finally, I show that the employment effects between 1994 and 1996 — after the 1993 EITC reform, but before federal welfare reform — can be explained by state welfare waivers and the business cycle. Controlling for business cycle effects, non-waiver states did not see any significant increase in the employment of single women with children relative to those without prior to federal welfare reform.

To summarize, the results in this paper suggest that the EITC consensus is fragile. Leveraging a multitude of EITC reforms and empirical analyses, I find that the estimates of treatment impacts are clustered around zero, except for the strongly confounded 1993 reform. A detailed investigation of the 1993 reform reveals that the empirical patterns align more closely with welfare changes than with EITC changes. Prior estimates are outliers in the distribution of estimates due to the reform episodes and specification choices on which they relied.

Can these findings be reconciled with the broader literature on labor supply responses to eco-

[3] After the 1993 reform, the employment rate of single mothers (relative to single childless women) increased by about 10pp for those with one child, 15pp for those with two children, 20pp for those with three children, and almost 30pp for those with four or more children.

[4] To predict welfare reform intensity, I use that pre-reform welfare participation rates (and the ensuing drop in welfare caseloads) are strongly related to demographics such as the number of children, the age of the youngest child, the age of the mother, race, and state.

nomic incentives? Let us consider quasi-experimental studies and randomized evaluations in turn. While my conclusions are at odds with prior quasi-experimental studies of EITC reform, they are not necessarily at odds with quasi-experimental studies of tax reform more broadly. After decades of research, we have surprisingly little evidence showing clear and sizeable effects of taxes on real labor supply.[5] It is telling that the meta study of extensive margin elasticities by Chetty, Guren, Manoli, and Weber (2013) cites only nine studies of (Hicksian) elasticities, most of which are relatively old and not based on modern quasi-experimental designs. Studies that do find sizeable behavioral responses to tax reform mostly come from the literature on taxable income responses at the intensive margin, driven by avoidance and evasion behavior (Saez 2010; Saez, Slemrod, and Giertz 2012). Therefore, prior EITC studies are arguably stronger outliers in the literature on behavioral responses to taxes than the study presented here.

In contrast to quasi-experimental studies, a number of randomized evaluations do find extensive margin effects of work incentives. Importantly, almost all of these evaluations consider 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 federal welfare reform and state welfare waivers, and in fact most of the trials were demonstration projects for waiver-based reform (Grogger and Karoly 2005; Kline and Tartari 2016).[6] Finding extensive margin effects in such welfare trials is consistent with the arguments regarding welfare reform presented here. As for in-work benefits like the EITC, we have very few randomized evaluations. The main exception is Card and Hyslop (2005). They 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 very differently from the EITC. More broadly, the EITC is different from experimental settings in terms of salience, information, administration, and claiming. A considerable amount of evidence shows that most potential recipients are either unaware of the EITC or have a limited understanding of its complicated schedule, eligibility and claiming (e.g., Bhargava and Manoli 2015). Given these aspects, the EITC is not an *a priori* likely candidate for finding large effects on labor supply.[7]

[5] Conversely, we *do* have clear examples of precisely estimated zeros. A recent paper by Martinez, Saez, and Siegenthaler (2021) use a large and salient two-year income tax holiday in Switzerland to estimate the (Frisch) elasticity of extensive margin labor supply, finding a precisely estimated zero effect.

[6] Grogger and Karoly (2005) provide a comprehensive review of these waiver demonstration trials. Kline and Tartari (2016) provide a recent evaluation of Connecticut's Jobs First Program, a waiver demonstration that included time limits, work requirements, and financial incentives. They find substantial extensive margin labor supply effects.

[7] While the informational and psychological frictions associated with the EITC program are widely acknowledged in the literature, they have been used mostly as an explanation for not observing any *intensive* margin responses. However,

The notion that EITC and welfare reform are associated with different labor supply elasticities is inconsistent with standard economic models in which "a dollar is a dollar." In such models, each worker responds to a total effective tax rate, including all tax and welfare incentives, according to a single elasticity. This does not allow for the possibility that EITC and welfare reform — by using fundamentally different policy levers and by being administered in fundamentally different ways — are likely associated with different informational and psychological frictions and may also give rise to different social multiplier effects. These aspects are discussed in the paper. Broadly speaking, my findings call for taking a richer behavioral and sociological approach to studying labor supply responses than the simple neoclassical view (see also Saez 2021; Kleven 2023).

The rest of the paper is organized as follows. Section 2 reviews the related literature. Section 3 describes the policy context and data. Section 4 presents long-run historical evidence on extensive margin labor supply for single women. Sections 5 and 6 provide quasi-experimental evidence on the impact of EITC reforms at the federal and state levels. Section 7 dissects the empirical patterns of the 1990s, trying to separate EITC effects from the effects of confounding factors. Section 8 concludes.

2 Literature Review

An enormous body of work has examined the labor market impacts of EITC and welfare reform. This section discusses a subset of closely related papers, referring those interested in a more exhaustive summary to one of the many surveys of the literature.[8]

Most of the EITC literature has focused on 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 and Liebman (1996) finds sizable employment effects based on comparing single women with and without children before and after the 1986 EITC expansion. Meyer and 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 the EITC and other

theoretical models of intensive and extensive margin responses (see e.g., Kleven and Kreiner 2006) imply that such frictions are equally important for the extensive margin.

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

tax changes account for over 60 percent of the employment increase of single mothers between 1984-96. Hotz, Mullin, and Scholz (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 and Mitchell (2011) confirm the qualitative findings from these earlier studies using PSID data from 1975-2004. Hoynes and Patel (2018) present event studies of the 1993 reform. Their focus is on poverty effects, but their online appendix shows employment effects as well. Bastian (2020) estimates sizable employment effects of the 1975 EITC introduction based on comparing all women (single and married) with and without children.

Schanzenbach and Strain (2020) replicate findings from an earlier version of this paper (Kleven 2020) and consider alternative specifications. They argue that there is robust evidence of positive effects of the EITC on extensive margin labor supply. However, I show that their estimates are strong outliers in the distribution of estimates across a wide range of specifications.

There are some exceptions to the consensus view described above. Cancian and 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 studying experience effects of employment, Looney and Manoli (2016) make a point closely related to one of the analyses presented in this paper. They show that most of the variation in employment by family size (i.e., by number of minor children living at home) reflects variation by age of the youngest child, and that age of the youngest child is strongly related to AFDC receipt prior to welfare reform and therefore to welfare treatment intensity.

A different approach to estimating labor supply responses to the EITC has been proposed by Chetty, Friedman, and Saez (2013). 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 child birth 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 0.19 at the average level of EITC knowledge between 2000-2005. Given their proxy for knowledge has increased significantly over time, their study suggests an average elasticity close to zero around the major EITC reforms in the 1980s

and 1990s.

In the experimental literature, there are relatively few examples of EITC-style policies. Card and 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, Guren, Manoli, and Weber 2013). Recently, Miller, Katz, Azurdia, Isen, Schultz, and Aloisi (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 also contributes to the literature on welfare reform, including both observational and experimental studies (see Blank 2002; Grogger and Karoly 2005). Because the federal government required experimental evaluations of state waiver programs, there is a substantial body of experimental work assessing the impact of those programs. These trials generally bundled a range of treatments such as time limits, work requirements, welfare-to-work training, and financial incentives. The literature finds significant effects of welfare reform on extensive margin labor supply, consistent with the arguments in this paper. For example, Grogger and Michalopoulos (2003) provide evidence on the impact of time limits using Florida's Family Transition Program. Leveraging the randomization of a bundle of policies combined with the assumption that the impact of time limits declines in the age of the youngest child, they find large effects of time limits on welfare use. Kline and Tartari (2016) study the extensive margin impacts of Connecticut's Jobs First Program, a randomized waiver demonstration that included time limits, work requirements, family caps, and earnings disregards. They find substantial effects on extensive margin labor supply.

There are relatively few studies that attempt to quantify the relative contribution of EITC and welfare reform to the employment increase of single mothers in the 1990s. Meyer and Rosenbaum (2001) estimate that the EITC accounts for 35% of the increase in employment between 1992 and 1996, with a more modest role for welfare reform. Similarly, Grogger (2003) estimates that the EITC explains 34% of the employment increase between 1993 and 1999, while welfare reform explains a much smaller share. Finally, Fang and 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 and 2002 can

be explained in roughly equal proportions by the EITC, welfare reform, and the macroeconomy.

Of crucial importance to interpretation is the literature on EITC knowledge. In their study of extensive margin responses to the 1986 EITC expansion, Eissa and Liebman (1996) cite evidence from interviews conducted in 1993 showing "virtually no awareness" of the credit among potential recipients (Eissa and Liebman 1993; Olson and 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 and Weisner 2000; Smeeding, Ross Phillips, and O'Connor 2000; Ross Phillips 2001; Berube, Kim, Forman, and Burns 2002; Maag 2005; Kopczuk and Pop-Eleches 2007; Jones 2010; Chetty and Saez 2013; Mead 2014; Bhargava and 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 and 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, Friedman, and Saez (2013) 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 extremely limited in the mid-nineties.[9]

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 intuition. 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 and Kreiner 2006; Eissa, Kleven, and Kreiner 2006, 2008). This inter-dependence is particularly strong for non-linear 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.

[9]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.

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 spent. The EITC is a refundable tax credit and eligibility depends on having earned income, creating a positive labor supply incentive at the extensive margin. Figure 1 illustrates the federal EITC schedule in 2018 (Panel A) and the expansion of EITC generosity between 1968-2018 (Panel B). The credit amount is a nonlinear function of household earnings and the number of qualifying dependents (children).[10] 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. Panel B shows the maximum possible EITC credit across different family sizes over time.[11] There have been five federal reforms of the EITC: the introduction in 1975 and the expansions enacted in 1986, 1990, 1993 and 2009.[12] The largest changes are the 1975 EITC introduction to all families with children and the 1993 EITC expansion to families with two or more children.

To assess the impact of these EITC expansions on the extensive margin incentives of single mothers, Figure 2 shows the evolution of their average tax rates over the period 1968-2018. The tax rates include state income taxes, federal income taxes, and federal payroll taxes.[13] The calculations assume that women enter the labor market at the first kink of the federal EITC (in each year and for each number of children), i.e. where the EITC refund is maximized and the entry incentive is the strongest. Average tax rates on single mothers have fallen enormously over this period, between 45 and 55 percentage points depending on the number of children. All of this fall can be attributed to the five federal EITC reforms, and especially to the 1975, 1993, and 2009 reforms. The impact of the 1986 and 1990 reforms on the tax incentives of single mothers is relatively modest.

In addition to the federal EITC, many states have introduced EITC supplements. A total of

[10] 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.

[11] The maximum credit is shown in 2018 US Dollars. Table A.1 in the online appendix documents the full set of EITC parameters for each family size and each year between 1975-2018.

[12] 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).

[13] The calculations are based on NBER's tax simulation model (TAXSIM). Full details are provided in section C of the online appendix.

thirty states instituted EITC supplements between 1984-2018.[14] Most state EITCs are specified as percentages of the federal EITC, implying that they have the exact same structure as the federal program. Table A.2 in the appendix provides details on all state EITC reforms, including their impact on the average tax rate of single mothers. While a number of state reforms did not change tax rates by very much, others introduced sizable changes (such as the reforms in Connecticut, Washington D.C., Michigan, New Jersey, and New York). The state reforms are useful for studying labor supply by introducing an additional source of variation: Besides comparing single women with and without children (or with different numbers of children), they allow for comparing states with and without EITC supplements.

3.2 Welfare Reform

It would be impossible to assess the EITC without considering the confounding effects from the rest of the welfare system. In particular, the 1993 EITC expansion coincided with dramatic changes to traditional cash welfare. The Personal Responsibility and Work Opportunity 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). Importantly, 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. I will highlight the key features of state and federal welfare reform below, but refer to Grogger and Karoly (2005) for an exhaustive description of the history and details of these policy changes.

Between 1992 and 1996, thirty-eight states received approvals for statewide legislation through waivers.[15] There were six main types of waiver policies. *Termination time limits* introduced upper bounds on the length of time that any family could receive welfare benefits. *Work requirement time limits* imposed mandatory work requirements after a certain amount of time on benefits.[16] 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

[14] Appendix Figure A.2 provides maps that illustrate the roll-out of state EITCs across the U.S.

[15] See Council of Economic Advisors (1997, 1999) and Department of Health and Human Services (1999).

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

activities. There were two such waivers. *JOBS exemptions* allowed states to eliminate or reduce the exemption of families with young children from the program. *JOBS sanctions* allowed states to impose harsher sanctions for failure to comply with the program. *Family caps* were used to eliminate or reduce benefit increases for existing AFDC recipients who had additional children. Finally, *earnings disregards* provided stronger financial incentives to work by disregarding earnings up to a level in the calculation of benefit claw back. Table A.3 in the appendix shows the approval and implementation dates of all statewide waivers.

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 TANF programs 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 and many did. States without any time limit had to introduce one. As a result, TANF extended the waiver-type elements that had already been implemented in a number of states to the remaining states.

Welfare reform did not primarily change statutory benefit levels, but imposed much harsher constraints on receiving those benefits by strengthening ordeals and enforcement. This increased the incentives for single mothers to move from welfare into work and mechanically kicked some of them off welfare. Importantly, the treatment intensity was not uniform across single mothers, but increased in the number of children. The main reason is that, prior to welfare reform, AFDC participation rates were sharply increasing in family size. This is shown in Figure A.3 of the online appendix. Therefore, as welfare reform restricted benefit access for all family types, the implications were much stronger for larger families as they were more reliant on benefits to begin with. The monotonic relationship between number of children and treatment intensity separates welfare reform from EITC reform. In this paper, I use family size along with other demographic variables that predict pre-reform welfare participation and thus treatment intensity to tease apart the impacts of EITC and welfare reform in the 1990s.[17]

[17] Another strategy would be to use variation in pre-reform benefit amounts across states. The cost of restricting benefit access through ordeals and enforcement is larger where the level of benefits is larger. However, Figure A.4 shows that such a strategy is not feasible due to the absence of a first stage. Panel A plots pre-reform welfare participation rates against pre-reform maximum monthly benefits across states, while Panel B plots post-reform drops in welfare participation rates against pre-reform maximum monthly benefits across states. The figure shows that state variation in pre-reform benefit levels is uncorrelated with pre-reform welfare participation rates, and therefore does not predict post-reform drops in welfare participation rates. This implies that variation in the dollar amounts of benefits across states is swamped by other factors that vary across states (including other program parameters, demographic composition, fixed costs of work, preferences, information, welfare norms/culture, etc.). The absence of a first stage implies that 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". Merging the monthly and March files give much more data than using the March files alone, which is what previous papers in the literature have done.[18] I restrict the dataset to include the monthly files from 1989-2019 and the March files from 1968-2019. Even though the monthly files go back to 1976, they do not allow for accurately identifying the presence and number of children prior to 1989. I focus on the sample of single women aged 20-50.[19] These restrictions result in a sample of 4,809,195 individual-month observations across survey years 1968-2019. Appendix B provides a detailed description of the CPS data.

Extensive Margin Measures: The CPS allows for four different ways of measuring labor supply at the extensive margin: Employment at the weekly or annual levels, and labor force participation at the weekly or annual levels. As shown in Figure A.5 of the appendix, the four extensive margin measures are highly correlated over time. I use weekly employment as the baseline outcome, but reproduce all the main analyses using the other outcomes as well. Given most of the existing literature has focused on annual employment, it is worth outlining the conceptual trade-offs in the choosing the extensive margin measure.

First, the difference between employment and labor force participation is that the latter includes unemployed people who are actively searching for work or only temporarily laid off. While employment is more relevant for welfare calculations, labor force participation has the advantage of not moving with transitions between employment and unemployment. This makes participation less vulnerable to the confounding effects of the business cycle. Second, the difference between annual and weekly measures is that the latter, besides capturing extensive margin responses, may also capture intensive margin responses (working more or less weeks over the year, conditional on working). Given the existing literature finds small or zero intensive margin responses to the EITC, this difference between annual and weekly measures seems quantitatively trivial.[20] That is, given the evidence on intensive margin responses *and* absent any statistical differences between the two

variation in welfare benefit levels is not useful for assessing the impact of welfare reform in the 1990s.

[18]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.

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

[20]Chetty, Friedman, and Saez (2013) *do* find evidence of intensive margin responses to the EITC, but the average response is small due to offsetting effects from the phase-in and phase-out regions.

measures, we would expect to get the same results from both.

However, there are important statistical advantages from using weekly measures. First, the weekly measures are available in both the monthly and the March files, while the annual measures are available only in the March files. Weekly measures therefore 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, while there is significant nonresponse in the annual income variables in the March files. As a result, a significant fraction of earnings observations in the CPS are based on imputations.[21] For these three reasons, weekly measures are preferred in terms of measurement and statistical power.

Comparison Groups: The empirical design exploits the two key determinants of EITC eligibility: children and earnings. The existing literature has compared *all* single women with and without children or *low-educated* single women with and without children. I use all single women as my baseline sample, but reproduce the main results for low-educated single women in the appendix. The goal of using education to restrict the sample is to capture families with low earnings capacity and therefore stronger EITC treatment. However, cutting the sample by education level raises two issues: (i) proxying for earnings capacity using only education is relatively crude given the many other demographic variables that affect earnings, and (ii) using a fixed education cutoff introduces potential selection bias due to the increase in education over time.[22] To avoid both of these issues, I take an approach based on fixed quantiles of predicted earnings. Specifically, I predict earnings conditional on working based on the following regression implemented on the sample of single women with positive earnings:

$$Y_i = \alpha_a + \beta_n + \gamma_y + \delta_e + \zeta_r + \eta_s + \nu_i, \tag{1}$$

[21] These imputations are described in appendix section B. Bollinger, Hirsch, Hokayem, and Ziliak (2019) provides a detailed analysis of the issue.

[22] Consider the typical education cutoff used in the literature, single women with high school degree or less. At the beginning of my sample period (1968), 87% (59%) of single women with children (without children) were low-educated by this definition. At the end of my sample period (2018), only 44% (27%) of single women with children (without children) were low-educated. Hence, using a fixed education cutoff is associated with substantial changes in sample selection that differ between treatment and control groups.

where Y_i is the earnings of woman 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 (4 categories), and state s.[23] The estimated coefficients are used to predict earnings $\hat{Y}_i$ in the full sample of single women (workers and non-workers), providing a virtually continuous measure of EITC treatment intensity. I then investigate the impact of the EITC in different quantiles of the predicted earnings distribution. These quantiles (such as the bottom quartile or bottom half of the distribution) are selected within each year to ensure that the sample selection is consistent through time.

Descriptive Statistics: Table 1 provides descriptive statistics for single women with children (columns (1)-(3)) and single women without children (columns (4)-(6)). In each group, outcomes are shown for the full sample and for subsamples with low education (high school degree or less) or low predicted earnings (bottom quartile of the distribution).[24] For single women with children, average earnings conditional on working equal $22,186 in the full sample, $15,313 in the low-education sample, and $10,922 in the low-earnings sample. Contrasting women in the low-education and low-earnings samples, we see that the latter are almost as low educated, younger, more African-American, and that they have more and younger children. Appendix Figure A.6 shows the earnings distribution of single mothers in the three samples, normalizing earnings by the first kink of the EITC in each year (so that the kink is located at zero). In all samples, the vast majority of single mothers have earnings within the EITC range: 73.3% in the full sample, 83.2% in the low-education sample, and 93.6% in the low-earnings sample. Results for the full sample can be interpreted as intention-to-treat (ITT) estimates, while results for the low-earnings sample are close to treatment-on-the-treated (TOT) estimates.

4 EITC and the Extensive Margin: The Long View

I start from a long-run perspective, documenting the extensive margin changes for single women over the last fifty years. Panel A of Figure 3 shows the labor force participation rates of single women with and without children between 1968-2018. Given EITC eligibility depends on children,

[23] The binning of these dummy variables is as follows: age of woman (20-24, 25-29, 30-34, 35-39, 40-44, 45-50), number of 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 (below high school, high school degree, some college, college degree and above), and race (white, black, Asian, and other).

[24] All samples include single women in the age group 20-50 and pool data from survey years 1968-2019. Earnings are reported in 2018 USD.

we can think of these series as treatment and control groups for evaluating the program.[25] The patterns are striking. Over half a century, all of the action in the participation rate of single mothers relative to that of single 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 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 how exceptional the 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, Panel B of Figure 3 compares the long-run series to the timing of the five federal EITC reforms and the confounders from welfare reform and the macroeconomy. It is clear that the EITC's main claim to sizable extensive margin effects relies on two correlated events: the EITC expansion enacted in 1993 and the ensuing increase in the participation rate of single mothers. At the same time, the figure brings out a major puzzle. If EITC expansion drove the extensive margin increases in the 1990s, then why do we not see any such effect around the other reforms? The three EITC reforms prior to 1993 did not lead to any closing of the gap between single women with and without children, nor did the EITC expansion in 2009. As discussed above, while the 1993 tax reform introduced very large tax cuts on single mothers, so did the 1975 and 2009 tax reforms.

Any compelling EITC narrative must reconcile the starkly different patterns observed in the 1990s and elsewhere. The missing link could be the two confounders emphasized here. First, there was welfare reform: waiver-based state reforms between 1992-96, culminating in national reform in 1996. Second, there was the macroeconomy: the economy was booming during the Clinton era, with the national unemployment rate falling steadily between 1992-2000. Why would the macroeconomy impact single women with and without children differently? Apart from the 1990s, there is no clear correlation between unemployment and the participation gap between the two groups. This is not inconsistent with the business cycle having an effect in the 1990s, however, because of the interaction between the business cycle and welfare reform. As single mothers get pushed off welfare, their ability to find work will depend on the tightness of the labor market around the time of the reform. What is more, any short-run interaction between welfare reform

[25] Childless families could not receive any EITC until 1993 and only a very modest credit thereafter (see Figure 1).

and the business cycle may persist in the long run due to employment hysteresis (e.g., Yagan 2019).

The long-run series discussed above show weekly participation rates in the sample of all single women. Figures A.7-A.9 in the appendix replicate the analysis for all four extensive margin measures (participation and employment at the weekly and annual levels) and three samples (all single women, low-educated single women, and single women with low predicted earnings).[26] The figures show that the qualitative patterns are robust to the choice of extensive margin measure and sample.[27] In the low-educated sample, there is some shrinking of the extensive margin gap between single women with and without children in the decades leading up to the 1993 reform. However, as discussed above, cutting the sample by education introduces sample selection bias due to the large increase in education levels over time, making it more appropriate to cut the sample by within-year quantiles of predicted earnings. In the low-earnings sample, there is a *widening* of the gap between single women with and without children over time. That is, zooming in on a sample more strongly treated by the EITC does not reconcile the contrast between the 1990s and other periods. If anything, it makes the contrast even sharper.

Further insight can be gained by splitting the sample of single mothers into different family sizes. Figure 4 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. However, while all groups of single mothers increased 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?[28]

[26] See section 3.3 for details. Low-educated single women are defined as those with a high school degree or less. Single women with low predicted earnings are defined as those below the median of the within-year distribution of predicted earnings. The annual participation series is shorter than the other series, because this measure was not recorded in CPS data until the 1976 March files (pertaining to calendar year 1975).

[27] 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.

[28] Figures A.10-A.12 in the appendix replicate the long-run series by family size for the different extensive margin measures and samples. The stylized patterns are robust across outcomes and samples.

To conclude, the long-run evidence suggests that the dramatic changes for large female-headed families in the mid-late 1990s were driven by factors that were unique to that time period *and* strictly increasing in family size. Welfare reform satisfies both; EITC reform satisfies neither.

5 The Impact of Federal EITC Reforms

5.1 Event Studies of Federal EITC Reforms

This section presents event studies of the five federal EITC reforms in 1975, 1986, 1990, 1993, and 2009.[29] The baseline specification uses weekly employment as the outcome and all single women as the sample, but alternative outcomes and samples will be considered below. The empirical strategy is a difference-in-differences (DiD) design comparing single women with and without children around each reform.[30] The event study specification is the following:

$$\begin{aligned} P_{imt} &= \sum_j \alpha_j \cdot Year_{j=t} + \beta \cdot Kids_i + \sum_{j \neq -1} \gamma_j \cdot Year_{j=t} \cdot Kids_i \\ &\quad + \boldsymbol{X_i \phi} + \eta \cdot U_{st} + \theta \cdot U_{st} \cdot Kids_i + \nu_{imt}, \end{aligned} \tag{2}$$

where P_{imt} is an indicator for individual i working 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, along with controls for demographics and the business cycle. By omitting the year just before the reform (indexed as -1) in the interaction term, the DiD coefficients γ_t can be interpreted as the extensive margin effect in year t relative to the pre-reform year. The vector of demographic controls $\boldsymbol{X_i}$ includes dummies for the age of the woman (6 categories), the age of the youngest child (7 categories), education (4 categories), race (4 categories), and state.[31] To adjust for business cycle confounders, the specification adds controls for the aggregate unemployment rate in state s and year t, U_{st}, interacted with kids. Specifically, U_{st} is a *demeaned* unemployment rate, i.e. the actual unemployment rate in state s and year t minus the average unemployment rate in state s over

[29] Because the CPS monthly files can be used only from 1989 (see section 3.3), to ensure that each reform is analyzed using consistent data through time, I use March files alone for reforms that occurred before 1993 and March and monthly files combined for reforms that occurred from 1993 onwards. The same split is made in the analysis of state EITC reforms in the next section.

[30] The 2009 reform is an exception. There I compare single women with three or more children to single women without children, because this EITC expansion was targeted specifically to families with 3+ children. Alternative comparison groups (such as 3+ vs 2 children) give similar results, but because single women with 1 or 2 children were strongly treated by non-EITC aspects of 2009 reform (see Figure 2), the comparison groups used here are more natural.

[31] The binning of these dummies is the same as for the earnings regression (1) described above.

the event study window. This captures the idea that business cycle variation reflects movements in unemployment relative to its "structural" level.[32]

In addition to event studies of each reform separately, I present stacked event studies of several reforms combined. Interacting the specification in (2) with indicators for each reform episode k, I obtain DiD coefficients γ_t^k for event year t and reform k. Stacked DiD coefficients are calculated as $\bar{\gamma}_t = \sum_{k=1}^{K} \frac{\hat{\gamma}_t^k}{K}$, where K is the number of reforms included. These coefficients give the average effect across different reforms, weighing each reform equally. In the stacked event studies, I always exclude the 1990 reform as a separate reform episode due to its close proximity to the 1986 reform (given the event study window used, any effect of the 1990 reform will be captured by the 1986-reform coefficients). I show results from two versions of the stacked event study, one that excludes the confounded 1993 reform and one that includes it.

The first set of results is presented in Figure 5. The figure plots DiD coefficients $\hat{\gamma}_t$ for the 1975 reform (Panel A), the 1986 and 1990 reforms (Panel B), the 1993 reform (Panel C), the 2009 reform (Panel D), federal reforms stacked excluding the 1993 reform (Panel E), and federal reforms stacked including the 1993 reform (Panel F). The panels also report the average three-year effect of each reform (with its standard error in parenthesis). The 3-year window is chosen to avoid overlap between reforms.

The results confirm the main insight from the long-run descriptive analysis presented above, namely that the patterns around the 1993 EITC expansion are exceptional. Taken at face value, the 1993 event study looks compelling: pre-trends trends are roughly parallel, employment starts increasing in the treatment group relative to the control group after the reform, and a large and statistically significant effect builds up over time. The average three-year effect (1994-96) on the employment rate equals 1.29pp. This precedes the passage of national welfare reform in August 1996, but coincides with the period of waiver-based state welfare reforms in 1992-96. The effect grows much larger in the late 1990s, following the passage of national welfare reform. No other EITC reform is associated with such patterns. The event studies are either flat around the reform implementations (1975 and 1986) or falling (1990 and 2009). As a result, the stacked event studies are also flat and this holds even when including the confounded 1993 reform. The average 3-year effect is negative and statistically insignificant in both stacked event studies.

Table 2 presents estimates from different specifications and reforms. Results are shown for

[32]Similar results are obtained when using first differences in unemployment rates ΔU_{st}. Similar results are also obtained when using raw unemployment levels, except around the 1975 reform.

weekly employment and annual employment, and for specifications without controls, with only demographic controls, and with both demographic and unemployment controls. The baseline results from the preceding figure correspond to those in column (3) of the table. All effects are averages over three post-reform years. Besides showing reduced-form effects of each tax reform on employment rates, the table converts the effects into extensive margin elasticities with respect to the net-of-tax rate.

The extensive margin elasticity is defined as

$$\varepsilon \equiv \frac{\Delta P / P}{\Delta (1-\tau) / (1-\tau)}, \tag{3}$$

where $\Delta P / P$ is the percentage effect of the reform on the employment rate and $\Delta (1-\tau) / (1-\tau)$ is the percentage effect of the reform on the average net-of-tax rate.[33] Both the numerator and denominator of the elasticity are measured based on DiDs comparing treatment and control groups. To simplify the calculation of elasticities across many reforms and over a long time period, I use the average tax rate series presented in Figure 2 to get the denominator. This implies that two approximations are made. First, while the calculations account for the entire tax system, they ignore the implicit tax from the welfare system. This overstates the *baseline* net-of-tax rate $1-\tau$. Second, the calculations assume that single women enter the labor market at the first kink of the federal EITC, i.e. where the EITC refund is maximized. This overstates the *change* in the net-of-tax rate $\Delta (1-\tau)$ induced by EITC reform, because some workers enter at higher earnings levels where the EITC expansions were weaker. Therefore, the two approximations have offsetting effects on the denominator of the elasticity.[34] Full details of the tax simulations and elasticity calculations are provided in appendix section C.

Table 2 confirms the main insight from the event study graphs. Except for the 1993 reform, none of the estimates are positive *and* statistically significant at the conventional 5% level. Across all 30 estimates from outside the 1993 reform, two are positive and significant at the 10% level, while all others are either negative or statistically insignificant even at the 10% level. Two additional points are worth highlighting. First, while the estimates for the 1993 reform are sizable across all specifications, they decline substantially as controls are added. For example, the elasticity of weekly employment goes from 0.53 without any controls to 0.22 with controls for demographics

[33] This elasticity definition corresponds to the literature on optimal taxation and welfare measurement (see e.g., Saez 2002; Kleven and Kreiner 2005; Eissa, Kleven, and Kreiner 2006, 2008).

[34] Results for the 1993 reform (available upon request) show that, in practice, the two approximations roughly cancel out, so that the elasticities presented here provide good approximations of the exact elasticities.

and the business cycle. This is a concern for causal inference, because there is no guarantee that these controls are enough. For example, the state-level unemployment controls included in (2) do not address national business cycle effects present in all states. Second, there is large variation in the elasticity estimates based on the 1986 reform, from -0.48 to +0.75 across specifications. The reason is that the 1986 reform introduced only small tax rate changes for single mothers, implying that even modest bias in the reduced-form estimates translates into large bias in the elasticity estimates. In general, small tax variation is not ideal for credibly identifying elasticity parameters.

In the following sections, I delve deeper into the implications of specification choice and the connection between my results and the prior literature.

5.2 Treatment Effects by Predicted Earnings

Estimations based on the full sample of single women do not account for the fact that the EITC program is means-tested. This may attenuate the effects as some single mothers are ineligible (due to having earnings above the EITC exhaustion point) and others are eligible for only minor subsidies (due to having earnings in the upper part of the EITC range). Figure A.6 in the appendix shows earnings distributions in different samples of single mothers. As can be seen in the figure, most but not all single mothers have earnings within the EITC range, consistent with the possibility of some attenuation in the full sample. To put it differently, estimations for the full sample give intention-to-treat (ITT) effects rather than treatment-on-the-treated (TOT) effects.[35] To see if this distinction matters here, I investigate responses among single women with low predicted earnings, estimated using equation (1) described above.

Figure 6 shows event studies for single women below the median of predicted earnings, while Table 3 shows average three-year effects across different specifications for the same sample. Apart from the sample, these exhibits are constructed exactly as those presented above. Restricting the sample to single mothers more strongly treated by the EITC does not change the main insights. The effects of the 1993 reform (which includes the confounding effects of welfare reform) are larger in this sample, but there are still no clear effects around any of the other reforms. The stacked event studies (even when including the 1993 reform) are virtually flat around the reform experiments, with a slightly negative and statistically insignificant effect during the first three post-reform years.

[35] Whether the object of interest should be ITT or TOT effects depends on the goal of the study. If the goal is to understand the impact of the EITC program on single mothers as a whole, ITT effects are more relevant. The fact that means-testing reduces the overall effect of the program should be reflected in the estimates. Conversely, if the goal is to estimate parameters that may be externally valid for counterfactual policy experiments, TOT effects are more relevant.

Is the absence of EITC effects due to the median sample split being too coarse to capture the subset of women who are responding to the EITC? I investigate this possibility in Figure 7, which plots three-year effects of federal EITC reforms by decile of predicted earnings. The estimates are based on stacked DiD specifications including either all reforms (red dots) or all reforms apart from 1993 (black dots). Panel A shows estimates from a specification without controls, while Panel B adds controls for demographics and state-level unemployment rates. As can be seen from the figure, the profile of DiD coefficients is flat around zero across the distribution of predicted earnings. This holds with and without controls and whether or not the 1993 reform is included. There is no earnings decile where the effect is positive and statistically significant. If the EITC had an effect on extensive margin labor supply, we would expect the profile to be declining in predicted earnings and positive at the bottom.[36]

5.3 Model Uncertainty

Estimating causal EITC impacts is challenging because of imperfections in the empirical design and survey data used. This creates uncertainty about how to specify an empirical model that avoids bias from confounding shocks and measurement error. The preceding sections considered the implications of different estimation samples, extensive margin measures, and control variables, arguing that the main insights are robust to changing the model in these dimensions. However, the specifications presented constitute just a small subset of the possible specifications. In this section, I take a more exhaustive approach to model uncertainty by presenting estimates from a wide range of specifications that vary the estimation sample, extensive margin outcome, and controls. Specifically, I consider all possible permutations of four samples (all single women, low-educated single women, single women below the median of predicted earnings, and single women below the 25th percentile predicted earnings), four outcomes (employment and participation at the weekly and annual levels), and four specifications of controls (no controls, basic demographic controls, rich demographic controls, and rich demographic controls plus unemployment controls).[37] With the seven reform experiments considered here (five federal reforms and two stacked specifications),

[36] Figures A.13-A.16 in the online appendix show that these insights are robust to the extensive margin outcome used. The annual measures feature a positive and statistically significant effect in decile 4 of predicted earnings, but not in deciles 1-3. Given the large number of estimates presented in these figures (160 estimates), it is not surprising that some are statistically significant. The main take-away from these figures is that the declining profile of coefficients that would be consistent with EITC effects is not present in any of the specifications.

[37] The basic demographic controls include age of the woman, age of the youngest child, and education. The rich demographic controls add race and state to the set. All controls are specified as dummies for different bins of the demographic variable, using the same binning as in equation (2) described above.

this amounts to a total of 432 specifications.

The online appendix provides event studies for all 432 specifications.[38] Figure 8 summarizes the data by showing the distribution of estimates across specifications. These are estimates of average three-year effects of each reform experiment. Panel A shows reduced-form effects on employment or participation rates in percentage points, while Panel B shows the implied elasticities with respect to the average net-of-tax rate. The gray bars depict the estimate distribution without the 1993 reform, while the blue bars depict the estimate distribution for the 1993 reform. These two distributions are fundamentally different. The non-1993 distribution is symmetric around zero, with a mean reduced-form effect of -0.32pp and a mean elasticity of -0.04. The 1993 distribution is shifted to the right, with a mean reduced-form effect of 4.02pp and a mean elasticity of 0.63. These distributions cannot both be right unless the underlying tax elasticities were somehow much larger in the mid-late 1990s than in any other period. A more plausible interpretation is that the 1993 distribution is upward biased due to the confounding effects of welfare reform and the macroeconomy.

Another way of illustrating model uncertainty is by way of a specification curve analysis as presented in Figure 9. This figure shows the curve of elasticity estimates, ranked from low to high, across all combinations of reform experiments and specifications. Leaving out the confounded 1993 reform, the figure includes 304 different elasticity estimates (black dots) and their 95% confidence intervals (gray bars). The underlying specification for each estimate is indicated below the graph. The striking finding is that less than a dozen specifications (3.6% of all specifications) yield estimates that are positive *and* statistically significant. Arguing for a positive effect of the EITC on extensive margin labor supply therefore requires taking a stand on specifications that are strong outliers in the distribution of specifications. Specifically, the only way to get large and statistically significant elasticities is to focus on the 1986 tax reform and annual employment (but *not* annual participation), combined with certain samples and specification of controls.[39] These outlier estimates have very large confidence intervals, preventing us from ruling out elasticities anywhere between marginally above zero to around 2. Conversely, my baseline specification is placed right in the middle of the specification curve and is much more precise. Given most of the estimates

[38] See Figures A.17-A.80.

[39] Relying on the 1986 reform to obtain large elasticities is not compelling, because this reform was relatively small in terms of the change in the average tax rate on single mothers (see Figure 2). In general, estimating elasticities based on small tax variation is not convincing for two reasons: (i) it is hard to accurately detect the response to small incentives in noisy data and (ii) any small bias in the reduced-form estimate (the numerator of the elasticity) translates to large bias in the elasticity estimate (due to the small denominator).

are concentrated around zero and are statistically insignificant, an agnostic view on model uncertainty leads one to conclude that the impact of the EITC on extensive margin labor supply has been negligible.

5.4 Comparison with Existing Estimates

It is useful to compare the distribution of estimates across specifications to existing estimates in the literature. Therefore, Figure 10 compares the distribution of elasticities from Figure 8B (leaving out the confounded 1993 reform) to elasticities implied by the reduced-form estimates in Eissa and Liebman (1996) and Schanzenbach and Strain (2020).[40] These two papers are chosen for several reasons: they are representative of the consensus in the EITC literature, they consider other reforms than 1993, and they provide reduced-form estimates that are easily converted into elasticities.[41] I use estimates from the preferred specifications in these papers, both of which consider effects on annual employment for either all single mothers or low-educated single mothers.[42] Full details on the conversion of their reduced-form estimates into elasticities are provided in Table A.4 of the online appendix.

In Figure 10, estimates based on Eissa and Liebman (1996) are demarcated by black vertical lines, while estimates based on Schanzenbach and Strain (2020) are demarcated by blue vertical lines. Apart from the 2009 reform (the two lines furthest to the left), their estimates are strong positive outliers in the distribution of estimates. The elasticities range from 0.30 to 1.66, all in the upper tail of the distribution. The largest estimates are those based on the 1986 reform for the sample of low-educated single women, an elasticity of 1.34 in Eissa and Liebman (1996) and 1.66

[40] Schanzenbach and Strain (2020) is a comment on a previous version of this paper (Kleven 2020).

[41] These and other papers in the EITC literature do not convert their reduced-form estimates into elasticities. Elasticity calculations are useful for several reasons, including the fact that they provide a normalized measure of responsiveness that can be compared across reforms and allows for assessing the plausibility of magnitudes (see Kleven 2021).

[42] For Eissa and Liebman (1996), I use their estimates reported in Table III (column 5) for all single women and Table IV (columns 1 and 2) for low-educated single women. Their paper shows effects on single mothers with less than high school degree and with high school degree separately. To ensure comparability with the low-education sample considered here (high school degree or less), I calculate a weighted average of their two estimates using the pre-reform fractions of low-educated single mothers with less than high school and a high-school degree, respectively. Eissa and Liebman (1996) consider four-year effects of the 1986 reform (comparing 1984-1986 to 1988-1990), as opposed to the average three year-effects shown in Figures 8-10. For Schanzenbach and Strain (2020), I use their estimates reported in Table 3 (column 2). These correspond to the estimates presented here: average three-year effects in the sample of either all single women or low-educated single women, defined as those with high school degree or less. I focus on their estimates for the 1975, 1986, 1990, and 2009 reforms. Both papers provide estimates of the reduced-form impact on the employment rate, ΔP. When calculating the percentage effects on the employment rate $\Delta P/P$ and on the average net-of-tax rate $\Delta(1-\tau)/(1-\tau)$, I replicate as closely as possible the estimations samples of each paper based on the sample selection criteria described. The elasticity conversions are done consistently with the elasticities presented above, i.e. using the average tax rate series in Figure 2 in which single women are assumed to enter the labor market at the first kink of the EITC (for each year and family size).

in Schanzenbach and Strain (2020). Elasticities of such magnitudes are arguably outside the realm of the possible. The reason why the 1986 tax reform can yield such large elasticity estimates is that this reform introduced only small changes in the average tax rates of single mothers, implying that even modest bias in the reduced-form estimates blows up to large bias in the elasticity estimates. As discussed above (see footnote 39), it is challenging to accurately estimate elasticities based on small identifying variation in tax rates.[43]

6 The Impact of State EITC Reforms

6.1 Event Studies of State EITC Reforms

In this section I consider a different source of variation: state EITC reforms. A total of 27 states have introduced and maintained an EITC supplement for a sufficiently long period of time to conduct an empirical study.[44] The introduction of state EITC supplements provide useful variation by allowing for comparing single mothers in states with and without an EITC, thus being immune to confounders that impact single mothers across states. State EITC reforms are analyzed using a synthetic control approach: For each state with an EITC supplement, I create a synthetic control state from the pool of states that never had an EITC supplement. I then run a stacked event study comparing treatment and synthetic control states around state EITC introductions.

Not all of the state EITC reforms provide useful quasi-experiments. I exclude three states as their EITC introduction was offset by other state tax changes, thereby *increasing* the total average tax rate on single women with children relative to those without.[45] I exclude another six states due to small sample sizes, less than 100 single mothers per year around the reforms.[46] Including these reforms in the analysis creates considerable noise in the estimates. These restrictions leave a total of 18 state EITC reforms. Some of these reforms were small, while others were quite large. I therefore

[43] Chetty, Guren, Manoli, and Weber (2013) converts the Eissa-Liebman estimate for all single mothers into an elasticity of 0.30, much smaller than the elasticity of 0.71 in Table A.4. This is despite the fact that they use a larger reduced-form estimate in the numerator of the elasticity (taken from column 4 of Table III in Eissa and Liebman 1996). The reason for their smaller elasticity is that they use a much larger denominator, which includes the total change in net earnings from taxes, welfare benefits, and Medicaid between 1984 and 1990. This gives a denominator of 13.2% instead of the 4.2% used here. Using the total incentive change from taxes, welfare benefits, and Medicaid is problematic, because the research design of Eissa and Liebman (1996) is supposed to identify the causal impact of the 1986 Tax Reform Act. In other words, such an elasticity calculation can only be correct if the Eissa-Liebman estimates are fully confounded by welfare and Medicaid changes. This point highlights that EITC estimates may be confounded by welfare changes even in the 1980s.

[44] Table A.2 lists 30 states with an EITC supplement. But the state of Washington never funded or paid out the credit, while Hawaii and South Carolina introduced their supplements only in 2018.

[45] These states are Indiana, Ohio, and Oregon. See Table A.2.

[46] These states are Iowa, Maryland, Minnesota, Rhode Island, Vermont, and Wisconsin.

compare specifications based on all 18 reforms to specifications that focus on the 10 largest reforms as measured by the reform-induced change in the state average tax rate on single women with children relative to those without.[47] As can be seen in Table A.2, these reforms reduced the average tax rate on treatments relative to controls by between 3.05pp and 8.32pp. These tax calculations include all state income taxes (not just the EITC) and assume that women enter the labor market at the first kink of the federal EITC.

To run the synthetic control analysis, the CPS data is collapsed to state-by-year observations for single women with and without children separately. For each treatment state, a synthetic control state is constructed (from among those without an EITC supplement) by matching on the level of the outcome variable in each of the five pre-reform years.[48] I consider two empirical approaches. The first is a difference-in-differences approach that compares treatment and control states in the sample of single mothers alone. The second is a triple-differences approach that compares treatment and control states for single women with children relative to those without. The full details of these specifications are provided in section E of the online appendix.

Figure 11 shows four event studies: difference-in-differences vs triple-differences (left vs right panels) and all reforms vs the ten largest reforms (top vs bottom panels). As above, I start by considering the sample of all single women and using weekly employment as the extensive margin measure. The figure shows clearly that there is no effect of state EITC supplements in any of the four event studies. 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. This is despite the fact that the tax variation is sizable, a reduction in the average tax rate of about 6pp across the ten largest reforms.

6.2 Model Uncertainty

As in the analysis of federal reforms, I investigate how sensitive the analysis of state reforms is to model uncertainty. The preceding section showed four different specifications, but focused on one estimation sample (all single women) and extensive margin outcome (weekly employment). This section shows results for all possible permutations of four samples (all single women, low-educated single women, and single women below the median or 25th percentile of predicted earnings) and four outcomes (employment and participation at the weekly or annual levels) in each of

[47] These are the EITC reforms in Colorado, Connecticut, District of Columbia, Kansas, Massachusetts, Michigan, Nebraska, New Jersey, New Mexico, and New York.

[48] This is done separately for single women with children and single women without children. Allowing for separate synthetic control states for the two groups improves pre-trends.

the four different specifications (difference-in-differences and triple-differences for all reforms or the largest reforms alone). This gives a total of 64 specifications.

The online appendix presents event studies for all 64 specifications.[49] Figure 12 summarizes the findings by showing the distribution of reduced-form estimates (Panel A) and elasticity estimates (Panel B). These are estimates of average three-year effects of state reforms. As can be seen from the figure, the estimates are symmetrically distributed around zero, with the mean being slightly negative. What is more, the event study graphs in appendix reveal that the positive outliers do not in general come from the specifications one would expect: some of the largest estimates are based on *all* reforms and the sample of *all* single women, i.e. specifications where the average tax treatment is relatively small. Overall, this analysis suggests that state EITC supplements, even when zooming in on the largest ones, have had no impact on extensive margin labor supply for single mothers.

7 Dissecting the 1990s

The large employment shift experienced by single mothers following the federal EITC expansion in 1993 is a historical anomaly. No other EITC reform at the state or federal level is associated with such labor supply changes. Because of the confounding effects of welfare reform and the business cycle in the mid-late 1990s, it would be natural to conclude that we should ignore the 1993 reform as a means to identifying the impact of the EITC. This reform is not a clean quasi-experiment. Nevertheless, it is interesting to dive deeper into the empirical patterns of the 1990s in an attempt to tease apart the EITC from confounding factors. There are two reasons for this. First, a complete story about the EITC and extensive margin labor supply ought to reconcile the zero effect from outside the 1993 reform with the large increases observed after the 1993 reform. Second, because the employment increases for single mothers in the 1990s were so large and historically unique, understanding what happened is important in its own right. Therefore, in this section, I present a number of analyses intended to show if these employment increases could be consistent with a zero effect of the EITC. The analyses suggest that they could: the employment increases align more closely with welfare reform and business cycle variation than with EITC variation. Given the fundamentally confounded nature of the variation, however, it is important to note that such an undertaking can never be conclusive. If the sole purpose is to identify the impact of the EITC, it is

[49] See Figures A.81-A.96.

better to focus on the many other reforms analyzed above.

7.1 A Needle in a Haystack

The first exercise provides a simple reality check of the data. Here I present simulations showing how much of the employment increases in the mid-late 1990s could be explained by the EITC under different elasticity scenarios. The results will show that trying to detect EITC impacts using the 1993 expansion is akin to looking for a needle in a haystack. The employment changes are so large that, even under sizable tax elasticities, almost all of the variation must be driven by non-tax factors.

From the definition of the elasticity ε in equation (3), the extensive margin response to the 1993 tax reform can be written as

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

where ΔP_t is the response in year t relative to the pre-reform year 1993, $\Delta\left(1-\tau_t\right)$ is the reform-induced change in the net-of-tax rate in year t relative to 1993, while τ_{93} and P_{93} are baseline values in 1993.[50] Calculating $\Delta\left(1-\tau_t\right)$ as the difference-in-differences between treatment and control groups, the implied time path of ΔP_t can be compared to the observed difference-in-differences impacts from the event studies.

Given the elasticity, the main input into equation (4) is the percentage effect of the reform on the net-of-tax rate, $\frac{\Delta(1-\tau_t)}{1-\tau_{93}}$. In the elasticity estimations presented above, two approximations were made when calculating this entity: the average tax rate did not account for the welfare system, and single women were assumed to enter the labor market at the first kink of the federal EITC. These approximations were made to sidestep data constraints when calculating elasticities across many reforms over a long time period. In the simulation exercise presented here, I take an exact approach that accounts for (i) taxes paid *and* benefits lost in the calculation of net-of-tax rates and (ii) the entire distribution of earnings conditional on working. The tax calculations are based on NBER's tax simulation model (TAXSIM), while the benefit calculations are based on a model of Aid to Families with Dependent Children (AFDC) and Food Stamps (FS). The full details of these calculations are provided in appendix sections C and D.

To calculate tax rates on participation for all single women, earnings conditional on working are

[50] Note that, while the elasticity formula in (3) is a definition, the expression in (4) is a model: it assumes a static labor supply model in which single women respond according to the constant elasticity ε in each period t. The static, iso-elastic model is obviously a simplification, but a natural benchmark to consider given the objective of this exercise.

set equal to observed earnings for workers and predicted earnings for non-workers. The earnings prediction is based on equation (1) run on a pre-reform sample of single women with positive earnings, using the estimated parameters to predict earnings for non-workers. Combining these earnings measures with the tax-benefit simulation model gives the reform-induced change in the net-of-tax rate on participation, $\frac{\Delta(1-\tau_t)}{1-\tau_{93}}$. While the baseline participation tax rate τ_{93} includes *all* taxes on labor income (federal income taxes, federal payroll taxes, and state income taxes) as well as the implicit tax from welfare benefits, the *change* in the participation tax rate $\Delta\tau_t$ accounts only for *federal income taxes* (including but not limited to the EITC). Because the goal is to simulate the impact of the federal tax reform act of 1993, the tax rates from federal payroll taxes, state income taxes, and welfare benefits are held constant at their 1993 levels. The calculation of federal income taxes in each year t is based on 1993 earnings (adjusted for inflation) to isolate the mechanical effect of the reform on tax rates.

Figure 13 compares actual and simulated DiD event studies of the 1993 reform. The actual DiD series plot estimates $\hat{\gamma}_t$ from equation (2) without any controls, while the simulated DiD series plot ΔP_t calculated from equation (4) under an extensive margin elasticity of either 0.25 (short dashes) or 0.5 (long dashes). The smaller of the two elasticities corresponds to the preferred estimate in the meta study by Chetty, Guren, Manoli, and Weber (2013). Results are shown for all single mothers (Panel A) and single mothers with low predicted earnings (Panel B). The main take-away from the figure is that, even if tax elasticities were very large, the 1993 tax reform would account for just a small part of the employment increase observed in the mid-late 1990s. Among those most strongly treated by the EITC (Panel B), only 10% (19%) of the employment increase can be explained by the 1993 tax reform under an elasticity of 0.25 (0.50). Hence, most of the variation *must* be driven by confounders, whatever they are. This makes it virtually impossible to identify EITC impacts using the 1993 reform: if the true elasticity were 0.25, we would need to find controls that can absorb 90% of the variation and we can never be confident that we have found the right controls.[51]

7.2 Was Welfare Reform Big Enough to Explain the Effects?

The AFDC/TANF program always served fewer families than the EITC program (see Figure A.1). It is therefore natural to ask if welfare reform could feasibly account for the employment increases in the 1990s, or if the program was just too small. The answer to this question depends, not on

[51] Figures A.97-A.98 in the appendix present simulation results by number of children in both the full sample and the low-earnings sample, showing that the identification challenges described here are even stronger in larger families.

welfare caseload *levels*, but on welfare caseload *changes*. To see if the caseload changes were big enough to explain the effects, I consider a different outcome: the fraction of single women who are either employed or on AFDC/TANF. I refer to this outcome as the "employment+welfare rate".[52] Given welfare caseloads were falling through the 1990s, the employment+welfare rate will show less of an effect than the employment rate. However, if there is still a positive effect in this outcome, then the caseload drops were too small to account for the entire employment effect, leaving something to explain for non-welfare factors.

The results are shown in Figures A.99-A.100 of the appendix (for the full sample and by number of children). These figures compare event studies of the 1993 reform for the employment rate and the employment+welfare rate. While the event study in employment shows large effects in every sample, the event study in employment+welfare is flat or slightly falling in every sample. In other words, once the effect of AFDC/TANF caseload changes is neutralized, there is no remaining treatment effect in the 1990s. This implies that welfare reform *was* large enough to explain the effects. In fact, movements between AFDC/TANF and employment account almost exactly for the employment increases year by year through the 1990s.

These results are more than just an accounting exercise, because EITC and welfare reform are predicted to affect the employment+welfare rate differently. Cutting welfare pushes people from welfare into work or into searching for work. If everyone finds work, the employment+welfare rate will be unaffected, while otherwise it will decrease. 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 should increase. The patterns shown here are therefore consistent with welfare reform and inconsistent with EITC reform.[53]

[52] Because single women without children are ineligible for AFDC/TANF, there is no distinction between the employment rate and the employment+welfare rate for this group. For single women with children, to avoid double counting people who are both employed and on welfare, I add only AFDC/TANF participants who are not also employed.

[53] A potential concern with this analysis is that welfare receipt is measured with error in the CPS data due to non-responses as described in appendix section B.1.3 (see also Meyer, Mok, and Sullivan 2015 and Meyer and Mittag 2019 for a detailed analysis). To address this issue, Figure A.101 replicates the analysis using administrative data on welfare caseloads. Because the publicly available administrative data does not provide caseloads by marital status, this robustness check is done for *all* women. The employment rate increased substantially in the 1990s even for mothers as a whole, albeit less than for single mothers alone. Crucially, consistent with the results for single mothers using CPS data, the results for all mothers using administrative data show that the extensive margin effect disappears when considering the employment+welfare rate. The DiD event study is flat or slightly falling. The full details of the analysis are provided in appendix section F.

7.3 Fanning-Out by Family Size

The difficulty of separating the effects of EITC and welfare reform in the 1990s is that their timing overlapped and that both of them treated single mothers. This difficulty may be addressable, however, because the two policies introduced different treatment variation *within* the group of single mothers. The following sections investigate if such variation can be used to tease apart the two effects. I start by focusing on variation by number of children. The 1993 EITC expansion was larger for families with two children than for families with one child, but the same across families with two or more children (see Figures 1-2). This implies that any EITC-driven divergence in the employment effects should occur primarily between one and two children, with little additional divergence between two and three or more children. State and federal welfare reform restricted benefit access across the board, implying that single mothers who were initially more reliant on welfare benefits were treated more strongly. Because pre-reform welfare participation was sharply increasing in the number of children, so was the post-reform drop in welfare participation (see Figure A.3). In other words, welfare treatment intensity was strictly increasing in family size and welfare-driven divergence in the employment effects should therefore occur at all family sizes.

Figure 14 shows event studies of the 1993 reform by number of children (1, 2, 3, and 4+). The figure plots DiD estimates $\hat{\gamma}_t^n$ from an extension of equation (2) with separate dummies for each number of children n, without any controls in Panel A and with demographic controls in 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 years 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 almost 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 4+ 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 demographic composition. The fanning-out is still strong, however, with increases in the employment rate ranging from about 8pp to 25pp across family sizes. Appendix Figures A.102-A.103 reproduce the analysis for the other extensive margin measures, showing that the fanning-out by family size is a robust feature of the data. These patterns of heterogeneity are consistent with a welfare reform interpretation, but harder to explain under an EITC interpretation.[54]

[54] If extensive margin elasticities were increasing in family size, this would generate a fanning-out of employment

7.4 Impacts by Welfare Treatment Intensity

The analysis of heterogeneity by number of children is based on the idea of using variation in welfare treatment intensity (relative to EITC treatment intensity) within the group of single mothers to tease apart the impacts of welfare and EITC reform. This section pursues this idea in greater depth, considering two alternative proxies for welfare treatment 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 study graphs, I summarize the effects using a DiD specification with a post-reform dummy. Specifically, I consider specifications of the following form

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

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 on either the age of the youngest child (7 bins) or a predicted AFDC probability (10 deciles). In either case, the omitted category is having no children, so that the welfare category variable subsumes the kids dummy from the previous specifications. The coefficient γ_c represents the average DiD effect for single mothers in welfare category c relative to single women without children over a specified time horizon. I consider two different time horizons: a three-year horizon (avoiding years after PRWORA) and a ten-year horizon. I start from raw DiD effects obtained from the first line of equation (5), and then consider the implications of adding the controls in the second line. These controls absorb the effects of demographics and state-level business cycles, specified as

responses even to the EITC. However, heterogeneity in tax elasticities is not a plausible explanation for the empirical patterns documented here simply because the amount of heterogeneity is too large. If the fanning-out of employment effects is interpreted solely through the lens of the EITC, the extensive margin elasticity would have to be around 2.5 for single mothers with three children and close to 6 for single mothers with four or more children. These results are available upon request (see also Kleven 2020 for similar elasticity calculations).

in equation (2) above.

The results are presented in Figure 15. 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.[55] The top panel shows that this variable is a strong predictor of welfare participation before the reform — the younger is the child, the higher 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 percent 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 of the average employment effect over three years (solid black) and ten years (dashed black) by age of the youngest child. It also shows the average drop in the welfare participation rate over ten years (dashed red), estimated from equation (5) using an indicator for welfare receipt as the outcome variable. The extensive margin effect is monotonically declining in the age of the youngest child. The three-year effect falls to about zero (and becomes statistically insignificant) for those whose youngest child is older than 13 years, while the ten-year effect falls to zero when the youngest child is older than 17. These estimates include any confounding effects of changing demographics and the business cycle. 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 effect disappears above age 6 and the ten-year effect disappears above age 13.

Because the analysis is based on EITC-eligible children — 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 imply that the last-born tends to be younger. As a result, the analysis using age of youngest child overlaps with the previous analysis using number of children. I now move to a multivariate analysis using the joint predictive power of the age of youngest child, the number of children, and other demographic variables. Specifically, I estimate

[55] 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.

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.[56] The binning of these demographic variables is the same as before. The regression is run on the CPS March files from 1994, 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 presented in the right panels of Figure 15. 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 plot the average employment effect over three years and ten years by decile of predicted AFDC probability. The employment effect is strongly increasing in the AFDC probability. In the raw data, the ten-year effect is almost 20pp in the top decile, but only 2pp in the bottom decile. Adding controls for demographics and state-level business cycles makes the estimates smaller: the ten-year effect is about 12pp in the top decile and negative 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.[57]

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.

[56] 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).

[57] Appendix Figures A.104-A.107 reproduce the analysis for all four extensive margin outcomes, showing that the results are robust.

7.5 The Impact of Welfare Waivers

The confounding effects of welfare reform operate at both the state level (waivers) and the national level (PRWORA). The preceding sections used heterogeneity in welfare treatment intensity in an attempt to control for both, but such patterns of heterogeneity are subject to endogeneity concerns. This section asks how much of the employment increase in the 1990s can be explained by welfare waivers using a more quasi-experimental approach. Waivers were introduced in some states but not others, creating plausibly exogenous variation in welfare benefits. The main focus is on the years 1994-96 — after the 1993 EITC reform, but before nationwide TANF reform — where we have the best chance of separating the effects of welfare and EITC reform.

To investigate the confounding effect of welfare waivers, I conduct event studies of the 1993 EITC reform separately for states that ever approved statewide waiver legislation and for those that did not. As shown in Table A.3 of the appendix, there were 13 states without any statewide waiver legislation: Alabama, Alaska, District of Columbia, Kansas, Kentucky, Louisiana, Nevada, New Mexico, New York, Oklahoma, Pennsylvania, Rhode Island, and Wyoming. Figure 16 shows event studies for all non-waiver states (blue series) and all waiver states (black series). These series plot DiD coefficients from specification (2), implemented separately on the two groups of states, controlling for demographics and state-level business cycles.

The figure shows that the employment effect is much smaller in non-waiver states than in waiver states in the years before nationwide TANF reform. In fact, the non-waiver effect is close to zero and statistically insignificant between 1994-1996. Following the implementation of TANF reform, non-waiver states begin to catch up to waiver states and converge to the same long-run effect. As a result, restricting attention to non-waiver states changes the DiD series from what looks like an event study of the 1993 EITC reform to what looks like an event study of the 1996 TANF reform. These patterns are exactly consistent with welfare reform being the main driver of the employment increases of single mothers in the 1990s. Figure A.108 replicates the analysis for different extensive margin outcomes (weekly vs annual employment) and for different estimation samples (all single women, low-educated single women, and single women with low predicted earnings). There is some variation across the different specifications, but the fundamental qualitative insight is robust. If anything, the results are even stronger when considering annual employment, and they survive in the low-educated and low-earnings samples.

7.6 Changing Social Norms

Another possibility is that social norms regarding welfare receipt changed in the 1990s. The public debate certainly changed, featuring increasingly derogatory rhetoric and racial stereotypes to describe welfare mothers.[58] As society became less tolerant of welfare mothers, their willingness to work may have increased above and beyond any effect of economic incentives. Conceptually, there are two effects of social norms to consider: (i) Norm changes may have precipitated welfare reform (and EITC reform) and at the same time changed behavior, leading researchers to attribute an impact of norms to policies; (ii) welfare reform may have been perceived as a social injunction that changed norms, creating a social multiplier effect of the reform. In the latter case, there would be no bias in the reduced-form impact of the reform (leaving aside other confounders), but there would be bias in the inferred elasticity with respect to the net-of-tax rate.

Figure A.110 in the appendix suggests that social norms may have been a confounding factor in the 1990s. Using Google Ngrams, the figure traces the use of loaded terms to describe welfare recipients in books published in the United States since 1970. Panel A considers the term *welfare queen*, a derogatory phrase used to describe women on welfare.[59] Panel B considers the terms *undeserving poor* and *deserving poor*. The graphs demonstrate that the use of negative language surrounding welfare receipt and poverty exploded in the 1990s, consistent with important changes to social norms and culture. Therefore, trying to understand the historic labor supply shift for single mothers in the 1990s solely through the lens of economic incentives may be too narrow.

8 Conclusion

The EITC program has been widely praised for boosting labor supply at the extensive margin, especially for single mothers. This paper reappraises the conventional wisdom, analyzing every EITC reform at the state and federal level since the inception of the policy in 1975. Based on a wide range of approaches and specifications, I show that the EITC has not had any clear effects on extensive margin labor supply. Apart from the federal 1993 reform, EITC expansions are not associated with employment increases for single mothers. The 1993 reform, on the other hand, is associated with large employment increases, but these increases align closely with confounding changes from welfare reform and the macroeconomy. Exploiting different sources of variation in

[58] As an example of the tenor of the debate, Figure A.109 shows an (infamous) cover of *The New Republic* on August 12, 1996, just before PRWORA was signed into law by President Bill Clinton.

[59] The phrase was coined in the Chicago Tribune in 1974 and later popularized by Ronald Reagan.

these confounders, I show that the effects are driven exactly by those affected most strongly by welfare reform and the business cycle.

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 have limited knowledge of the EITC program.[60] Given these frictions, the EITC is not an *a priori* likely candidate for finding large 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 if we ignore that many potential recipients were in fact unaware of the EITC at the time of the main historical reforms, the argument is still surprising. Economic theory predicts that intensive and extensive margin decisions are interdependent, and this interdependence is particularly strong for nonlinear incentives (see Kleven and Kreiner 2006; Eissa, Kleven, and Kreiner 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

[60] See e.g., Romich and Weisner (2000), Smeeding, Ross Phillips, and O'Connor (2000), Ross Phillips (2001), Maag (2005), Chetty and Saez (2013), and Bhargava and Manoli (2015).

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.[61] 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).[62] 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.

[61] This paper does not investigate intensive margin responses to the EITC. In general, the literature has struggled to find any clear evidence of intensive margin responses to the program, perhaps because of the frictions just discussed. Chetty, Friedman, and Saez (2013) is an exception in the literature, arguing that EITC eligibles do respond along the intensive margin once information frictions are accounted for.

[62] 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.

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TABLE 1: DESCRIPTIVE STATISTICS

	Single Women With Children			Single Women Without Children		
	All	Low Education	Low Predicted Earnings	All	Low Education	Low Predicted Earnings
	(1)	(2)	(3)	(4)	(5)	(6)
Weekly Employment Rate	0.68	0.60	0.53	0.75	0.66	0.68
Annual Employment Rate	0.73	0.66	0.63	0.81	0.74	0.78
Earnings	22,186	15,313	10,923	25,924	18,545	13,082
Age	34.37	33.26	25.51	30.67	32.15	21.55
Fraction White	0.68	0.67	0.58	0.79	0.75	0.79
Fraction Black	0.26	0.27	0.34	0.14	0.18	0.14
Number of Children	1.79	1.89	2.12	0.00	0.00	0.00
Age of Youngest Child	7.51	7.11	3.11	.	.	.
High School & Below	0.55	1.00	0.79	0.33	1.00	0.42
College Degree	0.13	0.00	0.00	0.31	0.00	0.00
Observations	1,787,348	979,702	447,099	3,010,586	993,063	765,901

Notes: This table shows means of demographic and labor market variables among single women with and without EITC-eligible children, respectively, where EITC-eligible children are those under the age of 19 (24 if in full-time education) and living at home for at least half of the year. The columns consider different samples: all single women, single women with low education (high school degree or below), and single women with low predicted earnings (bottom quartile of the within-year distribution of predicted earnings, estimated using equation 1). All samples include single women aged 20-50 using the March and monthly CPS files combined, pooling all years from 1968 to 2019. Earnings are reported in 2018 USD.

TABLE 2: DiD EFFECTS OF FEDERAL EITC REFORMS ON SINGLE MOTHERS

ALL SINGLE WOMEN

	Weekly Employment Rate			Annual Employment Rate		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: 1975 Reform						
Effect of Reform (pp)	-0.71	-0.83	0.33	-0.42	-0.66	1.89
	(1.38)	(1.33)	(1.61)	(1.23)	(1.19)	(1.45)
Elasticity	-0.10	-0.12	0.05	-0.05	-0.08	0.24
	(0.19)	(0.19)	(0.23)	(0.15)	(0.15)	(0.18)
Panel B: 1986 Reform						
Effect of Reform (pp)	-1.03	-0.11	-0.70	1.50	1.77*	1.09
	(1.07)	(1.01)	(1.03)	(0.98)	(0.94)	(0.96)
Elasticity	-0.48	-0.05	-0.33	0.63	0.75*	0.46
	(0.50)	(0.47)	(0.48)	(0.41)	(0.39)	(0.40)
Panel C: 1990 Reform						
Effect of Reform (pp)	-0.35	-1.73*	-0.39	0.00	-2.53***	-1.84*
	(1.07)	(1.00)	(1.04)	(0.98)	(0.94)	(0.98)
Elasticity	-0.13	-0.65*	-0.15	0.00	-0.86***	-0.62*
	(0.40)	(0.37)	(0.39)	(0.33)	(0.32)	(0.33)
Panel D: 1993 Reform						
Effect of Reform (pp)	3.06***	2.81***	1.29***	4.39***	3.72***	2.66***
	(0.51)	(0.47)	(0.49)	(0.98)	(0.92)	(0.96)
Elasticity	0.53***	0.49***	0.22***	0.67***	0.57***	0.41***
	(0.09)	(0.08)	(0.08)	(0.15)	(0.14)	(0.15)
Panel E: 2009 Reform						
Effect of Reform (pp)	-3.55***	-4.02***	-3.01***	-1.25	-2.02	-0.58
	(1.07)	(1.03)	(1.14)	(1.70)	(1.67)	(1.86)
Elasticity	-0.70***	-0.79***	-0.59***	-0.23	-0.37	-0.11
	(0.21)	(0.20)	(0.22)	(0.31)	(0.30)	(0.34)
Panel F: Federal Reforms Stacked, Without 1993						
Effect of Reform (pp)	-1.41**	-1.67***	-0.94	-0.04	-0.86	0.14
	(0.57)	(0.54)	(0.62)	(0.62)	(0.61)	(0.69)
Elasticity	-0.33**	-0.39***	-0.22	-0.01	-0.18	0.03
	(0.13)	(0.13)	(0.15)	(0.13)	(0.13)	(0.15)
Demographic Controls:		X	X		X	X
Unemployment Controls:			X			X

Notes: This table reports DiD estimates of the effects of the five federal EITC reforms separately (Panels A-E) and stacked together (Panel F). These are three-year effects, estimated from a modified version of equation (2) in which the first three year dummies after each reform have been collapsed into a post dummy. Panels A-D are based on comparing single women with and 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 and annual employment) and for different controls. The rows show the reduced-form effect of each reform and the implied elasticity with respect to 1 - τ as defined in equation (3). The sample includes all single women aged 20-50. Panels A-C use March CPS files alone, while Panels D-E use March and monthly CPS files combined. Robust standard errors are clustered at the individual level.

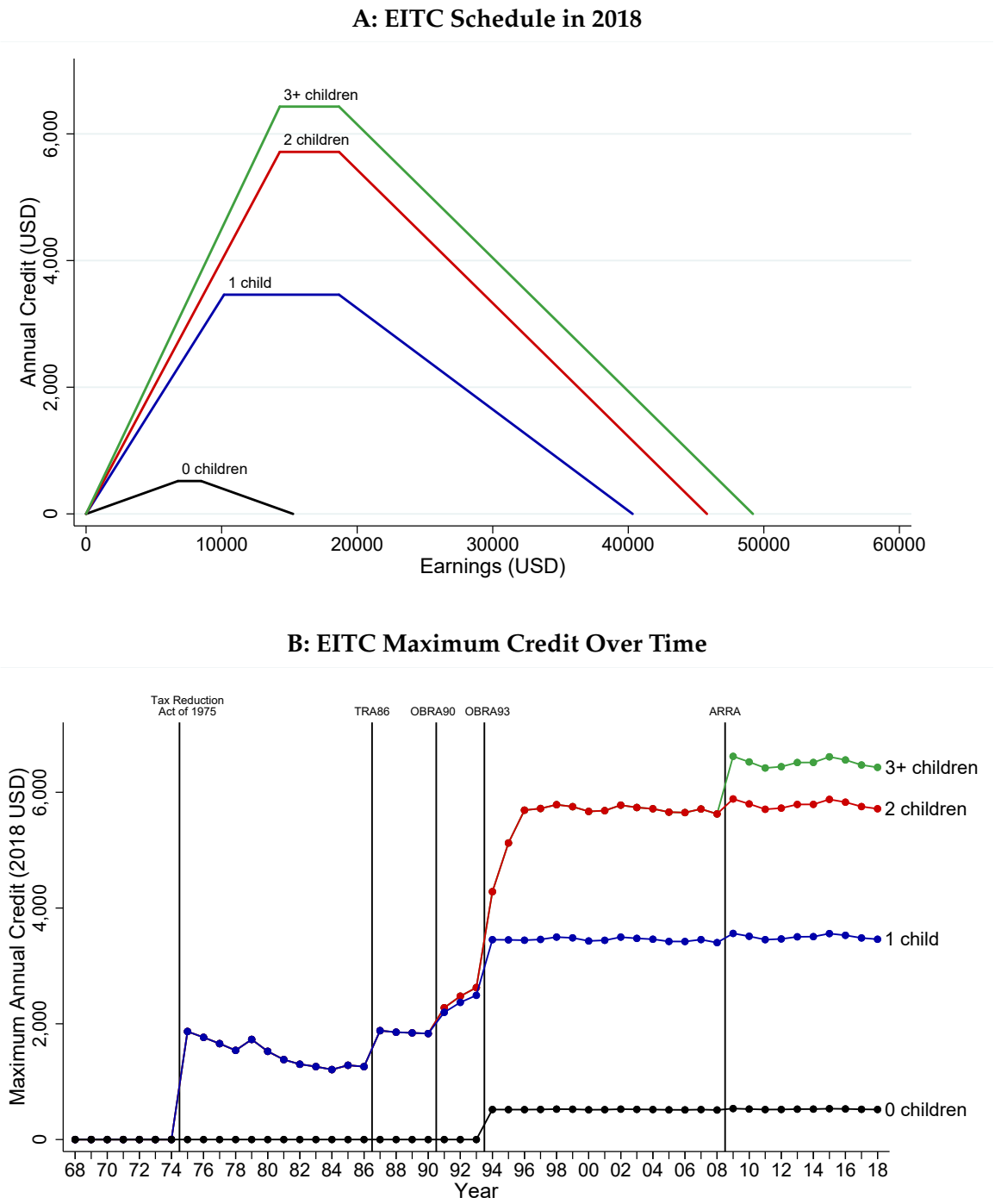
TABLE 3: DID EFFECTS OF FEDERAL EITC REFORMS ON SINGLE MOTHERS

SINGLE WOMEN IN BOTTOM HALF OF PREDICTED EARNINGS

	Weekly Employment Rate			Annual Employment Rate		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: 1975 Reform						
Effect of Reform (pp)	1.12	1.01	0.05	0.06	-0.62	3.43
	(2.00)	(1.94)	(2.36)	(1.78)	(1.74)	(2.14)
Elasticity	0.19	0.17	0.01	0.01	-0.09	0.47
	(0.34)	(0.33)	(0.40)	(0.25)	(0.24)	(0.30)
Panel B: 1986 Reform						
Effect of Reform (pp)	-0.42	0.14	-0.86	3.14**	3.24**	2.23
	(1.59)	(1.53)	(1.57)	(1.45)	(1.40)	(1.44)
Elasticity	-0.26	0.09	-0.53	1.57**	1.61**	1.11
	(0.98)	(0.94)	(0.97)	(0.72)	(0.70)	(0.72)
Panel C: 1990 Reform						
Effect of Reform (pp)	-0.79	-1.84	-0.23	-2.64*	-4.18***	-3.62**
	(1.60)	(1.53)	(1.59)	(1.48)	(1.43)	(1.48)
Elasticity	-0.36	-0.84	-0.11	-0.97*	-1.54***	-1.34**
	(0.73)	(0.70)	(0.73)	(0.55)	(0.53)	(0.55)
Panel D: 1993 Reform						
Effect of Reform (pp)	4.17***	3.67***	1.72**	7.43***	6.69***	5.42***
	(0.75)	(0.71)	(0.74)	(1.51)	(1.43)	(1.50)
Elasticity	0.78***	0.69***	0.32**	1.12***	1.00***	0.81***
	(0.14)	(0.13)	(0.14)	(0.23)	(0.21)	(0.23)
Panel E: 2009 Reform						
Effect of Reform (pp)	-2.98**	-3.32**	-2.65*	-0.44	-1.21	0.42
	(1.34)	(1.30)	(1.48)	(2.18)	(2.14)	(2.48)
Elasticity	-0.63**	-0.71**	-0.57*	-0.08	-0.23	0.08
	(0.28)	(0.28)	(0.32)	(0.42)	(0.41)	(0.47)
Panel F: Federal Reforms Stacked, Without 1993						
Effect of Reform (pp)	-0.77	-1.00	-0.92	0.03	-0.69	0.62
	(0.82)	(0.79)	(0.90)	(0.87)	(0.85)	(0.98)
Elasticity	-0.22	-0.28	-0.26	0.01	-0.16	0.14
	(0.23)	(0.22)	(0.25)	(0.20)	(0.20)	(0.23)
Demographic Controls:		X	X		X	X
Unemployment Controls:			X			X

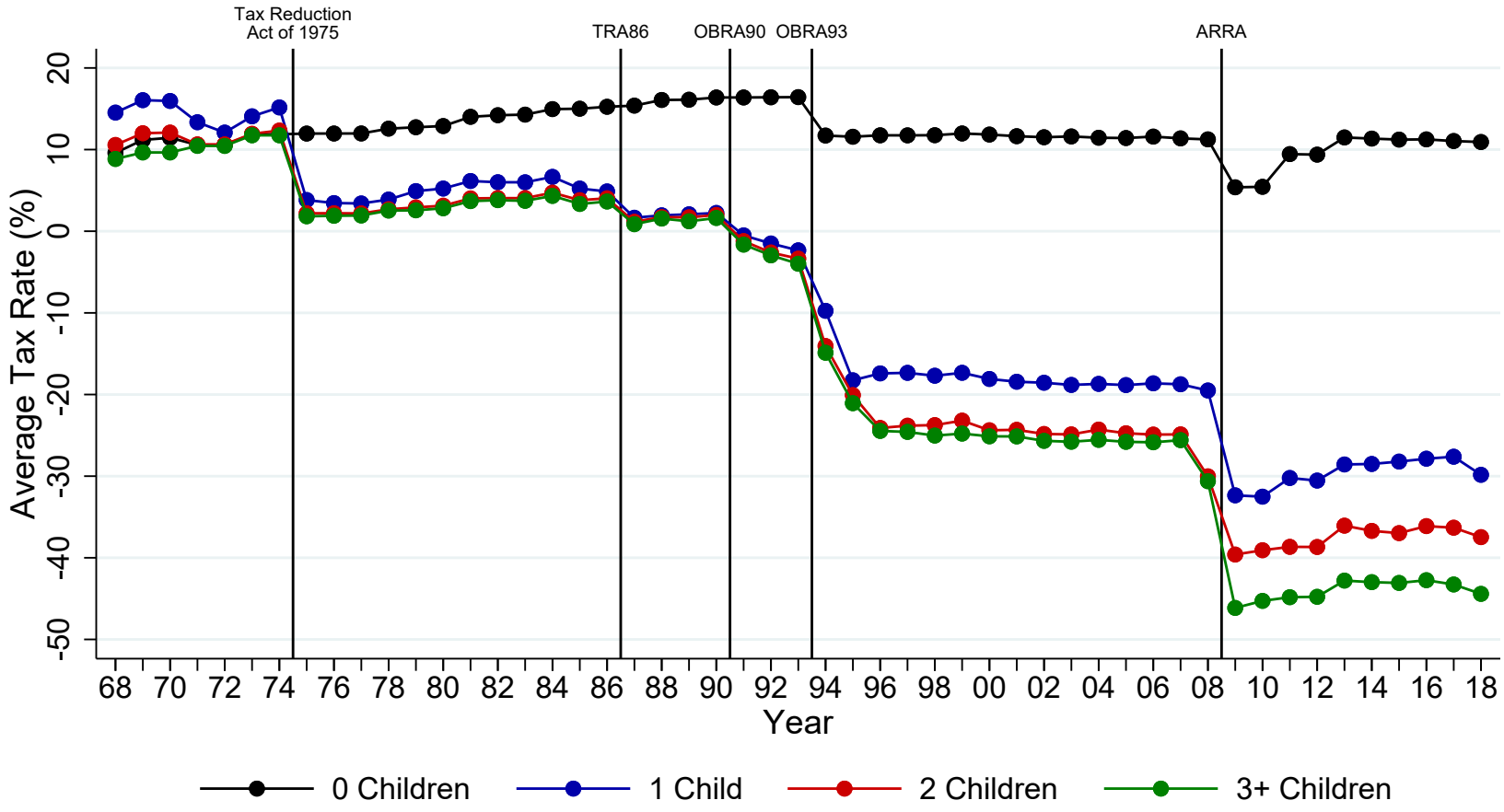
Notes: This table is constructed exactly as Table 2, but focusing on a sample of single women with low predicted earnings. Specifically, the sample includes single women in the bottom half of the within-year distribution of predicted earnings, estimated using equation (1).

FIGURE 1: FEDERAL EITC PARAMETERS



Notes: This figure shows federal EITC parameters for families with different numbers of EITC-eligible children. Panel A shows the EITC schedule in 2018 as a function of earnings for families with 0, 1, 2, and 3+ children. Panel B shows the maximum annual credit (in 2018 USD) for families with 0, 1, 2, and 3+ children over the period 1968-2018. The five federal EITC reforms are marked by vertical lines.

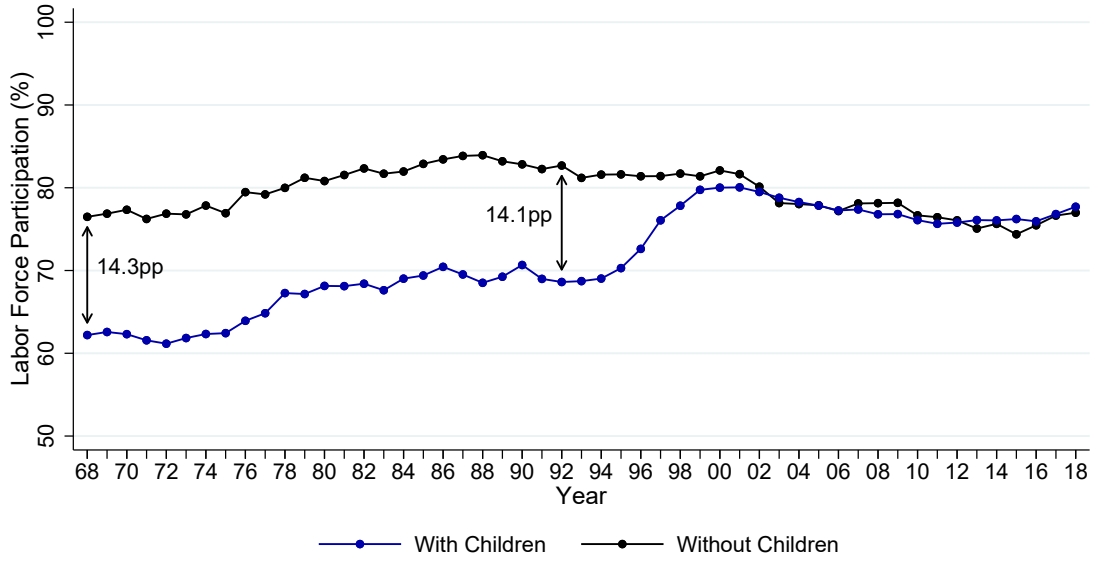
Figure 2: Average Tax Rates on Single Women Over Time



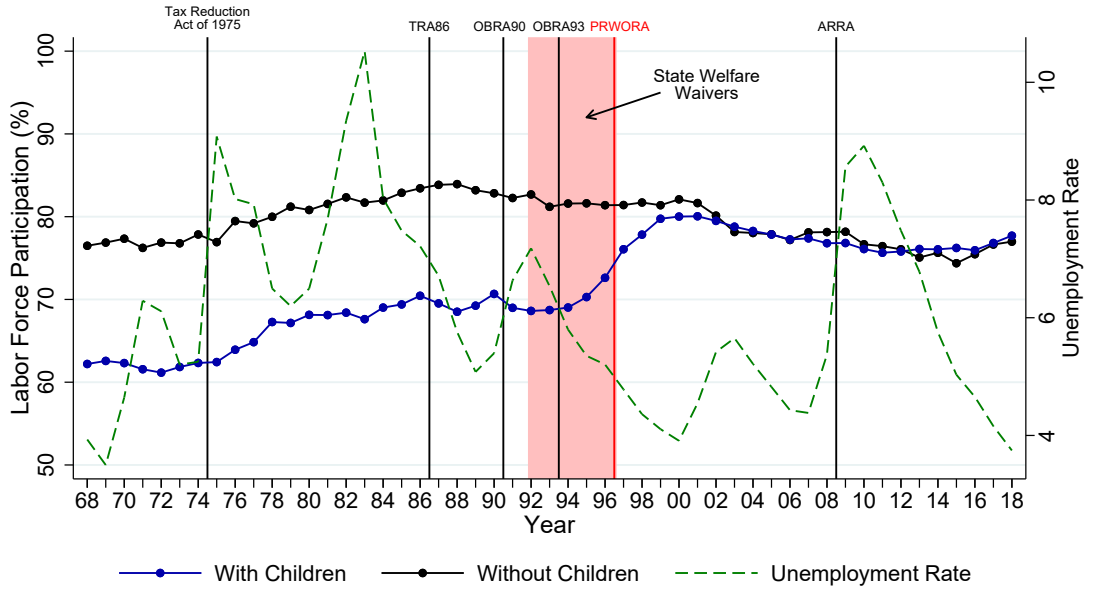
Notes: This figure shows average tax rates for single women with different numbers of EITC-eligible children over the period 1968-2018. The tax rates include all state income taxes, federal income taxes, and federal insurance contributions (FICA). The calculations assume that single women enter the labor market at the first kink of the federal EITC (for each year and number of children separately), i.e. where the EITC refund is maximized. The five federal EITC reforms are marked by vertical lines. Full details of the tax simulations are provided in section C of the online appendix.

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

A: Raw Patterns

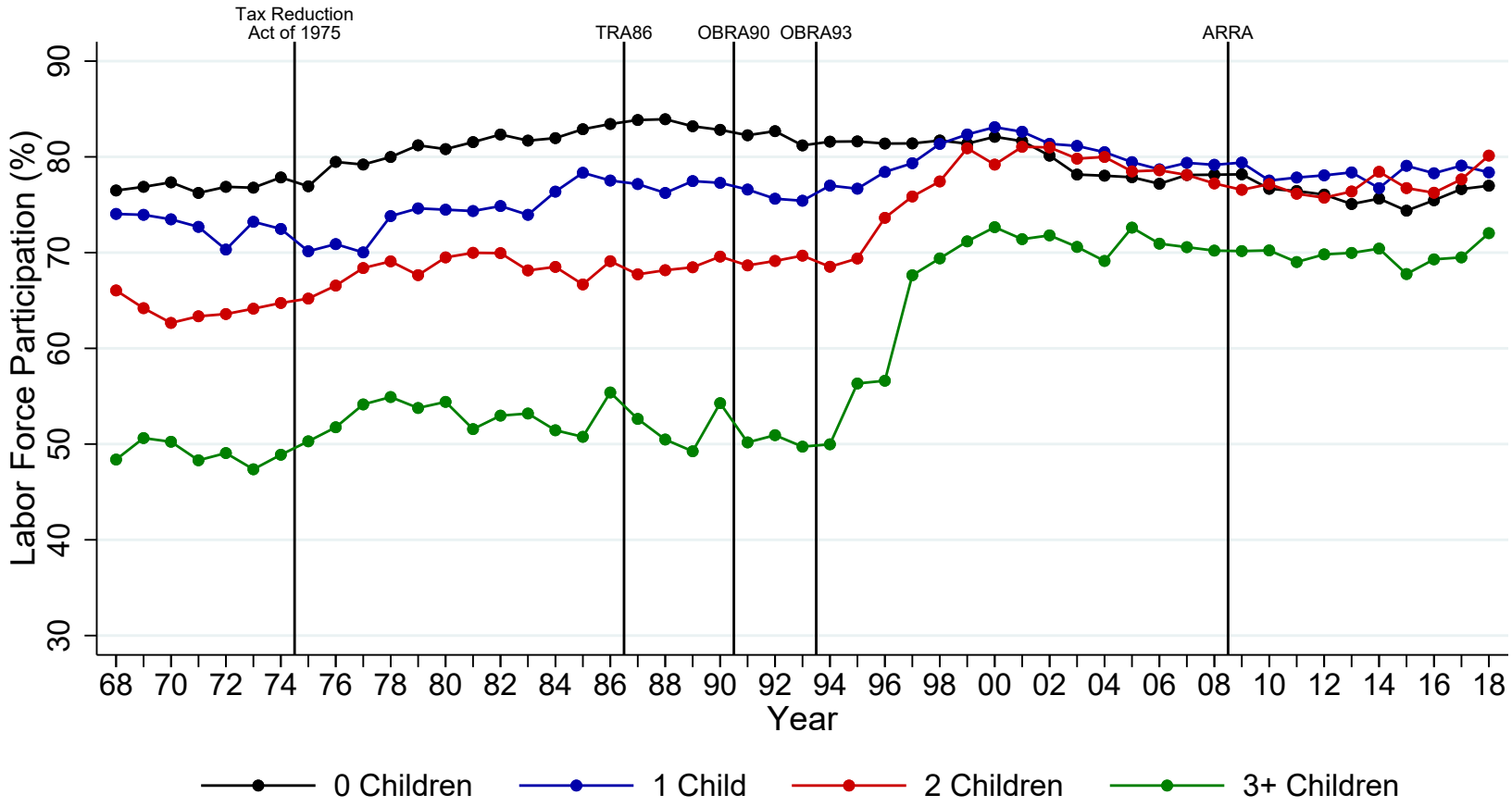


B: EITC Reforms and Confounders



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

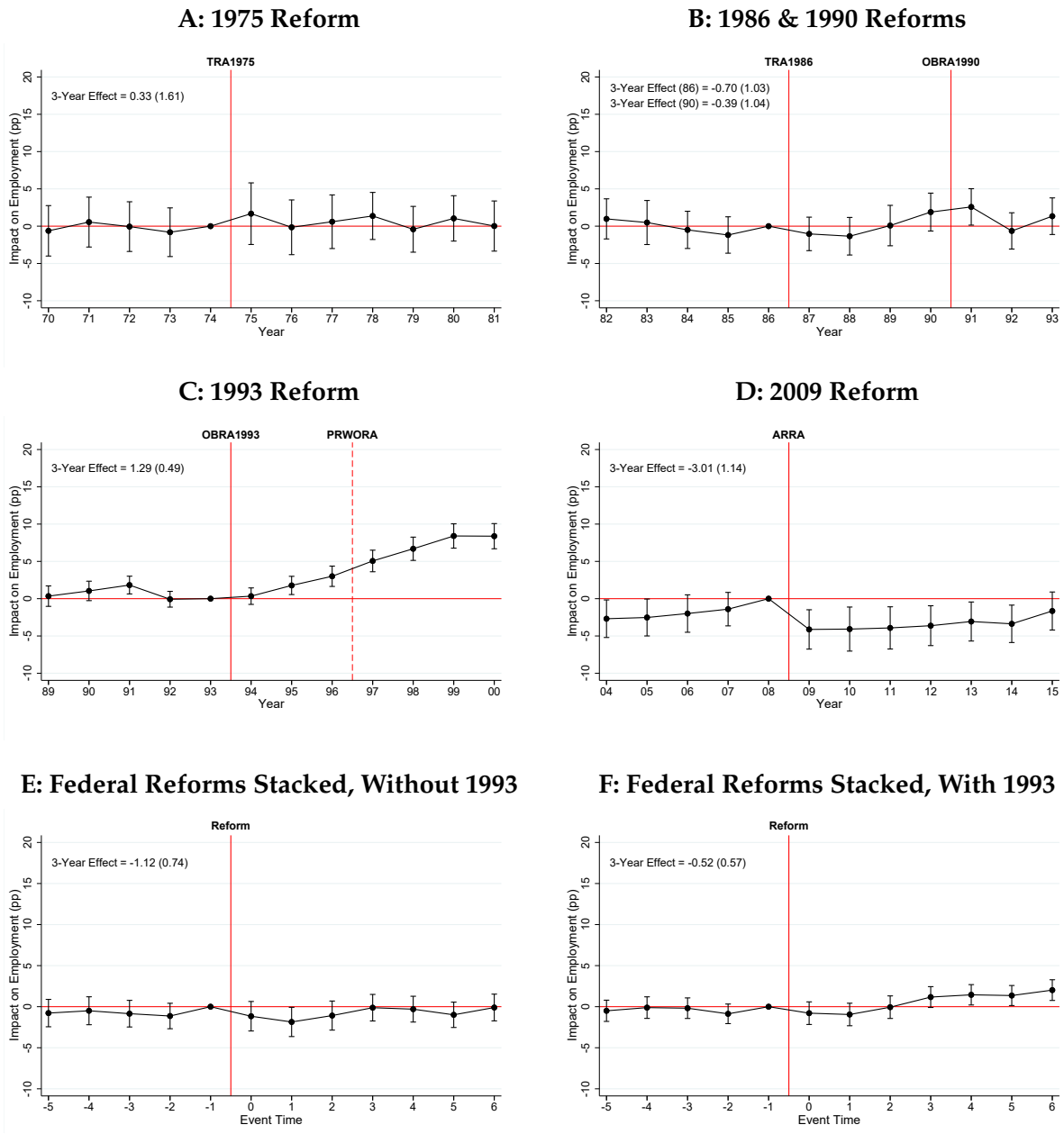
FIGURE 4: FIFTY YEARS OF LABOR FORCE PARTICIPATION FOR SINGLE WOMEN, BY NUMBER OF CHILDREN



Notes: This figure shows labor force participation rates 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 lines. The outcome is weekly participation and the sample includes all single women aged 20-50 using the March CPS files.

FIGURE 5: DID EVENT STUDIES OF FEDERAL EITC REFORMS

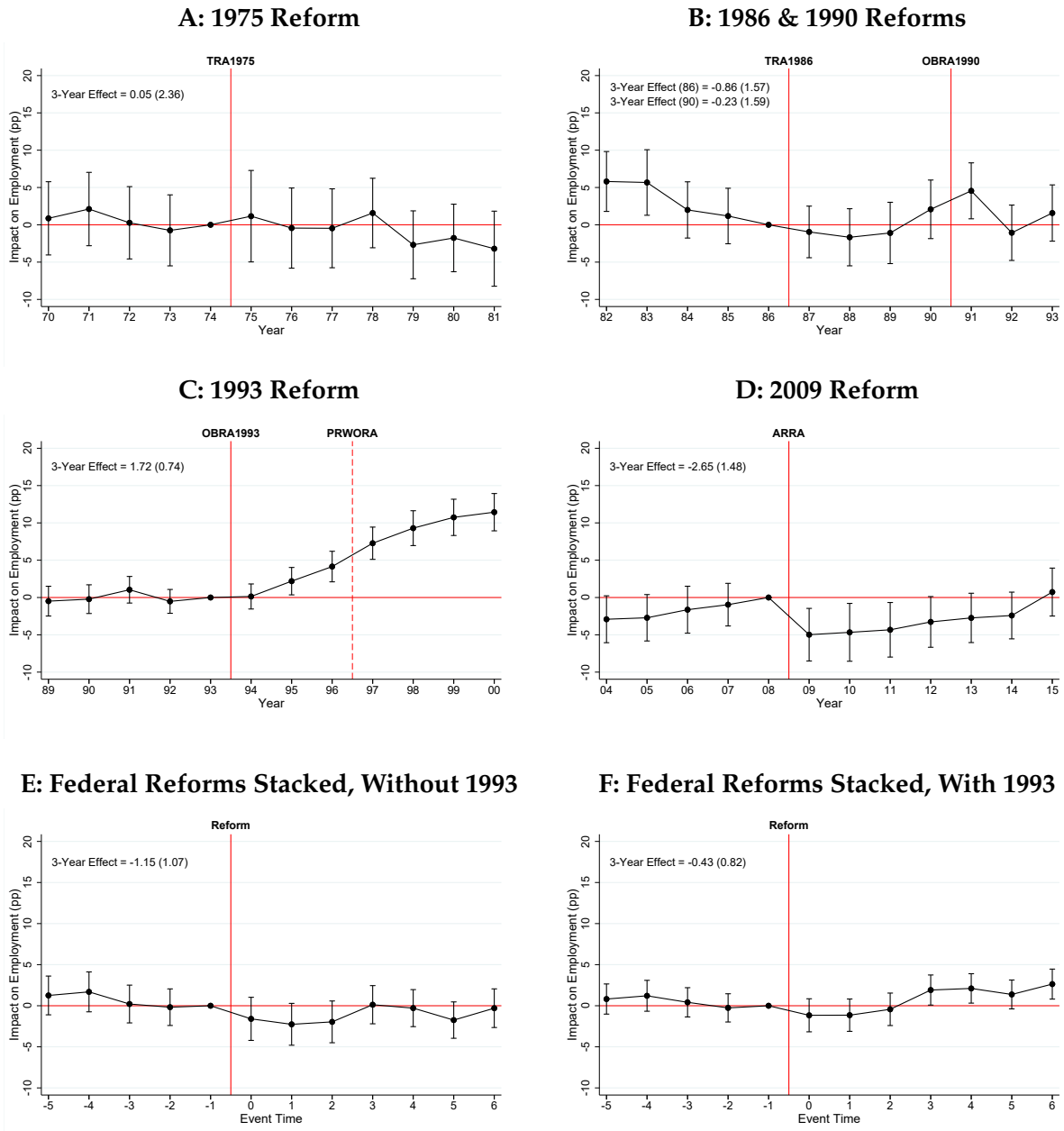
ALL SINGLE WOMEN, WEEKLY EMPLOYMENT



Notes: This figure presents DiD event studies of the five federal EITC reforms separately (Panels A-D) and stacked together (Panels E-F). Panels A-D plot estimates of the yearly impact $\hat{\gamma}_t$ based on specification (2). Panels E-F plot estimates of the average yearly impact $\bar{\gamma}_t = \sum_{k=1}^{K} \frac{\hat{\gamma}_t^k}{K}$, where $\hat{\gamma}_t^k$ is the estimated impact in event year t for reform episode $k = 1, ..., K$. Two stacked event studies are shown, one that excludes the confounded 1993 reform and one that includes it. The impact of each reform is based on comparing single women with and without children, except for the 2009 reform which is based on comparing single women with 3+ children to those without children. The dependent variable is weekly employment. The sample includes all single women aged 20-50. Early reforms (1975, 1986, 1990) are analyzed using the March CPS files alone, while later reforms (1993, 2009) are analyzed using the March and monthly CPS files combined. The 95% confidence intervals are based on robust standard errors clustered at the individual level. Each panel reports the average three-year DiD effect, with standard errors in parentheses.

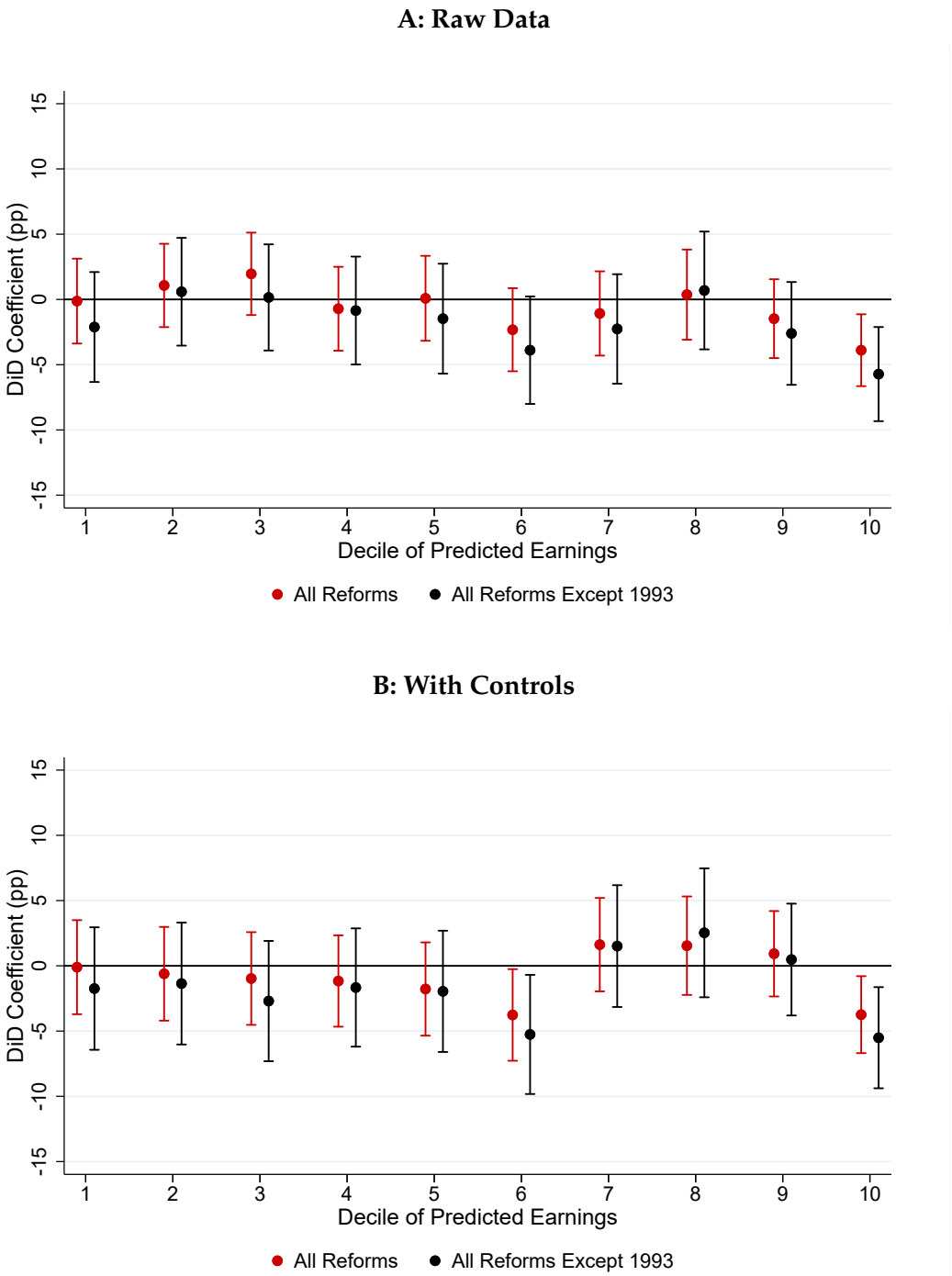
FIGURE 6: DID EVENT STUDIES OF FEDERAL EITC REFORMS

SINGLE WOMEN IN BOTTOM HALF OF PREDICTED EARNINGS, WEEKLY EMPLOYMENT



Notes: This figure is constructed exactly as Figure 5, but focusing on a sample of single women with low predicted earnings. Specifically, the sample includes single women in the bottom half of the within-year distribution of predicted earnings, estimated using equation (1).

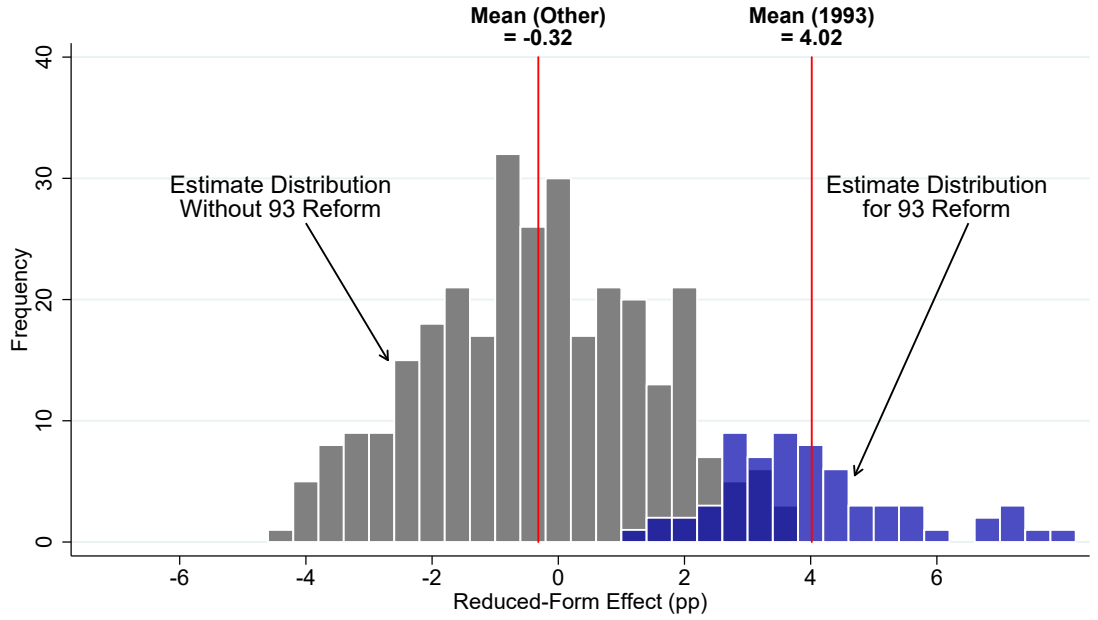
FIGURE 7: EFFECTS OF FEDERAL REFORMS STACKED BY DECILE OF PREDICTED EARNINGS
WEEKLY EMPLOYMENT



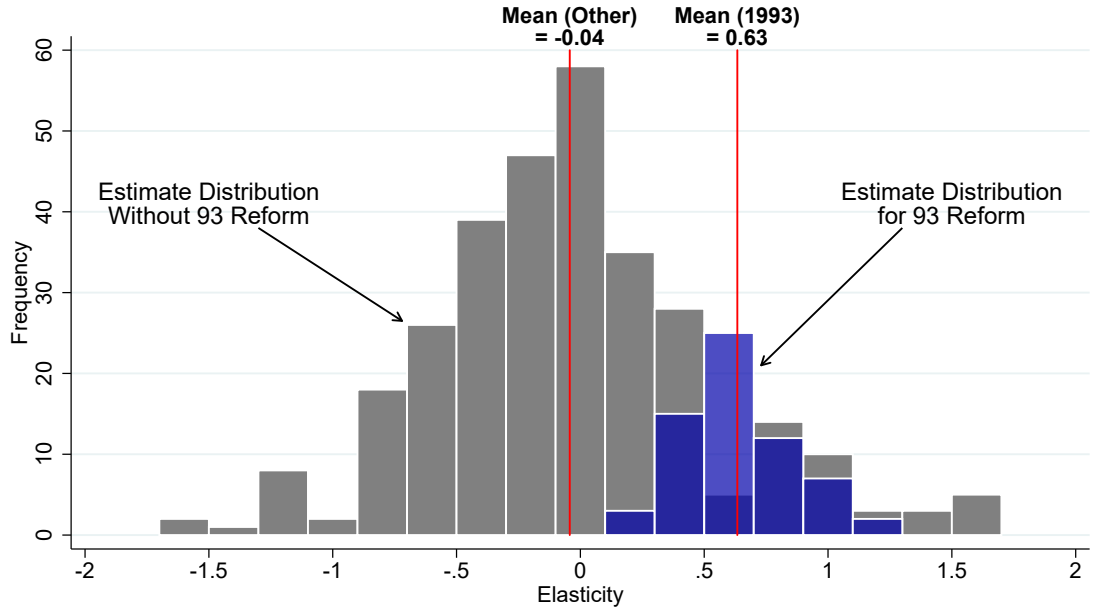
Notes: This figure shows estimates of three-year effects of federal EITC reforms by decile of predicted earnings. The estimates are based on stacked DiD specifications including either all reforms (red dots) or all reforms apart from 1993 (black dots). For each reform, predicted earnings are estimated by running specification (1) on three years of pre-reform data for working single women, using the coefficients to predict earnings for all single women in all years. The sample is then split by deciles of predicted earnings within each year. Panel A shows estimates from a specification without any controls, while Panel B adds controls for demographics and state-level unemployment rates (as in equation 2). The dependent variable is weekly employment. The sample includes all single women aged 20-50. The 95% confidence intervals are based on robust standard errors clustered at the individual level.

FIGURE 8: EFFECTS OF FEDERAL EITC REFORMS ACROSS ALL SPECIFICATIONS

A: Distribution of Reduced-Form Estimates

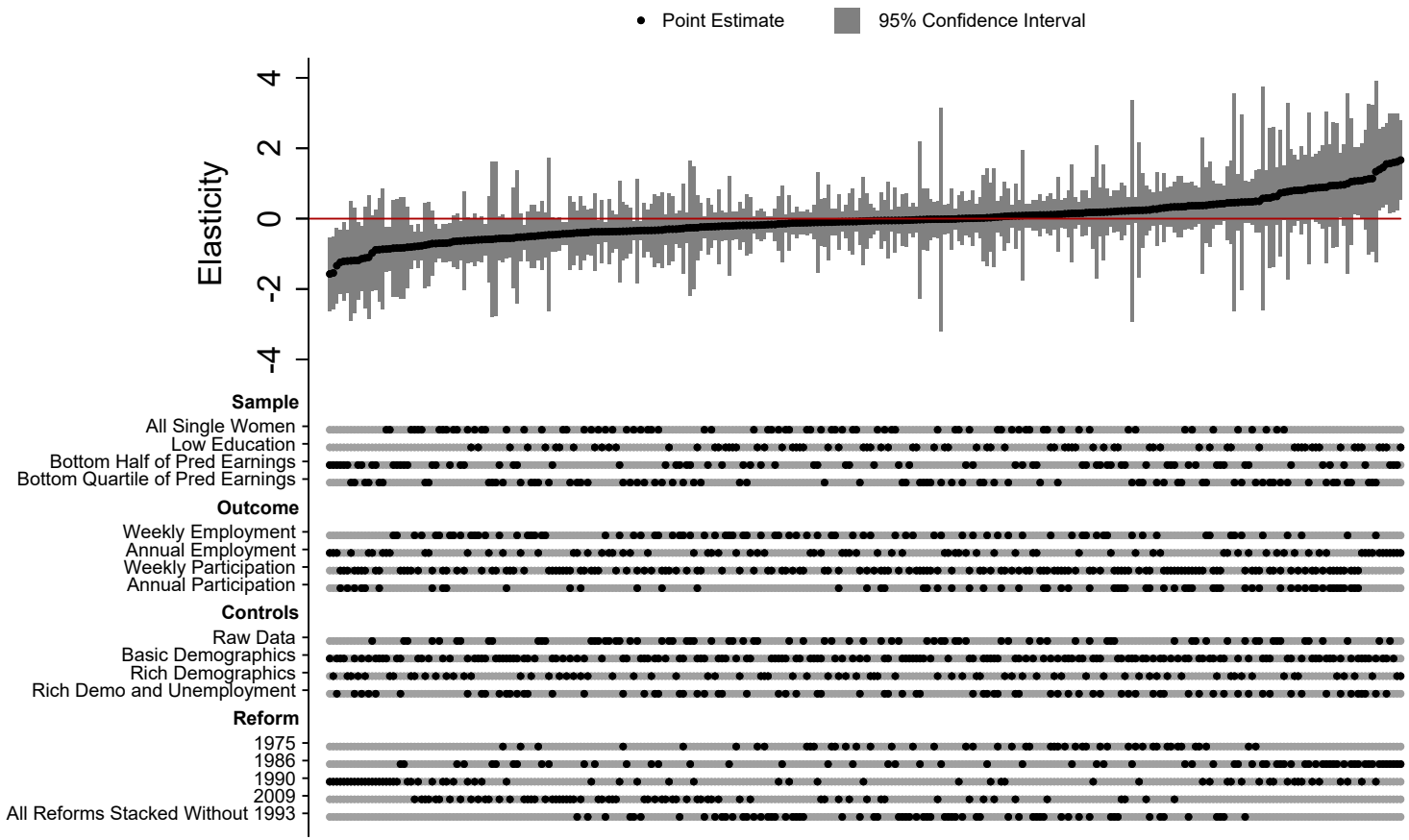


B: Distribution of Elasticity Estimates



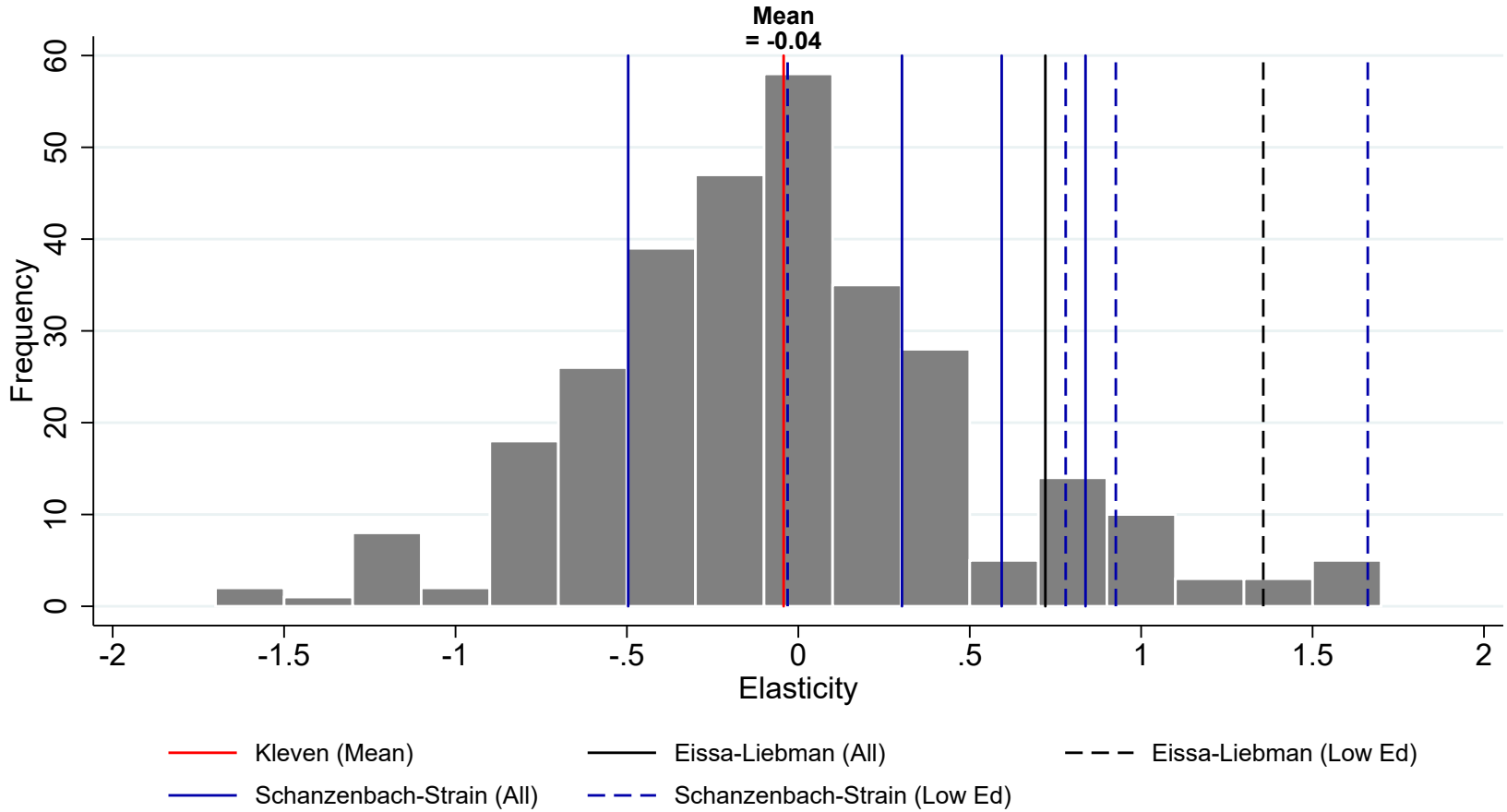
Notes: This figure shows the distribution of estimated effects of federal EITC reforms across 432 different specifications (permutations of different reform experiments, extensive margin measures, samples, and control variables). Panel A shows reduced-form effects on employment or participation rates (average three-year effects), while Panel B shows the implied extensive margin elasticities with respect to $1 - \tau$. Each panel shows two estimate distributions: all estimates excluding those based on the confounded 1993 reform (gray bars) and estimates based on the 1993 reform (blue bars). The mean of each distribution is marked by a vertical line. The distribution excluding 1993 estimates is centered around zero (with a mean elasticity of -0.04), whereas the distribution of 1993 estimates is shifted to the right (with a mean elasticity of 0.63).

FIGURE 9: SPECIFICATION CURVE ANALYSIS FOR FEDERAL EITC REFORMS



Notes: This figure shows elasticity estimates for federal EITC reforms (leaving out the confounded 1993 reform) across all specifications (304 permutations of different reform experiments, extensive margin measures, samples, and control variables). The elasticity estimates are shown by black dots (ranked from low to high), with the 95% confidence intervals in gray. The underlying specification for each estimate is indicated below the graph. Only a few specifications at the top (3.6% of all specifications) yield estimates that are positive *and* statistically significant.

Figure 10: Distribution of Elasticity Estimates Across Specifications vs Prior Estimates



Notes: This figure compares the distribution of elasticities across all specifications and reforms (leaving out the 1993 reform) to the elasticities implied by reduced-form estimates for those same reforms in Eissa and Liebman (1996) (black vertical lines) and Schanzenbach and Strain (2020) (blue dashed lines). The reduced-form estimates are taken from Table III and Table IV in Eissa and Liebman (1996), and from Table 3 in Schanzenbach and Strain (2020). Both papers use annual employment as the extensive margin outcome, and they consider treatment effects on all single mothers (labeled "All" in the figure) and on low-educated single mothers (labeled "Low" in the figure). Full details on the conversion of the reduced-form estimates in these papers into elasticities can be found in Table A.4 of the online appendix.

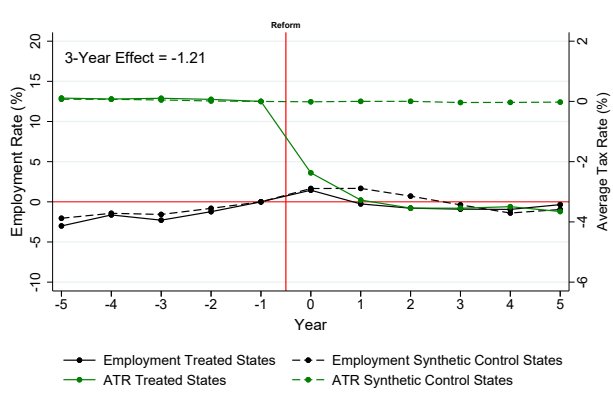
FIGURE 11: SYNTHETIC CONTROL ANALYSIS OF STATE EITC REFORMS

ALL SINGLE WOMEN, WEEKLY EMPLOYMENT

All Reforms Stacked

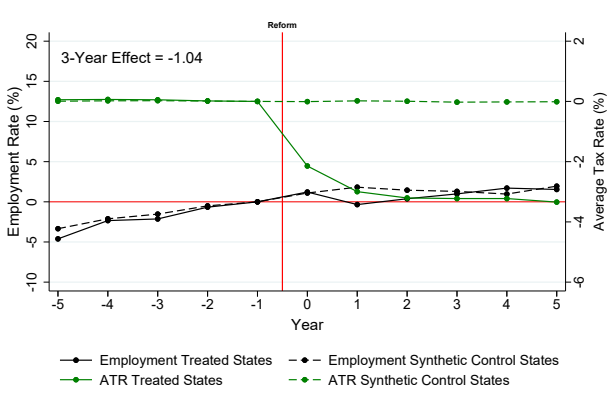
A: Difference-in-Differences

Treated vs Control States (With Kids)



B: Triple-Differences

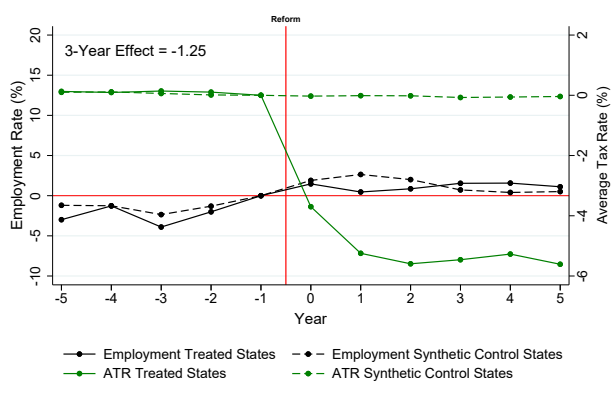
Treated vs Control States (With vs Without Kids)



Ten Largest Reforms Stacked

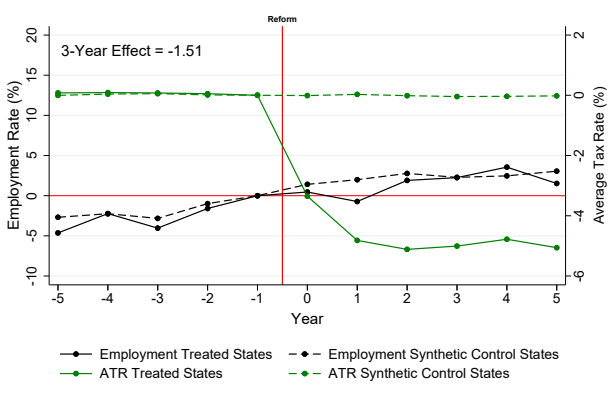
C: Difference-in-Differences

Treated vs Control States (With Kids)



D: Triple-Differences

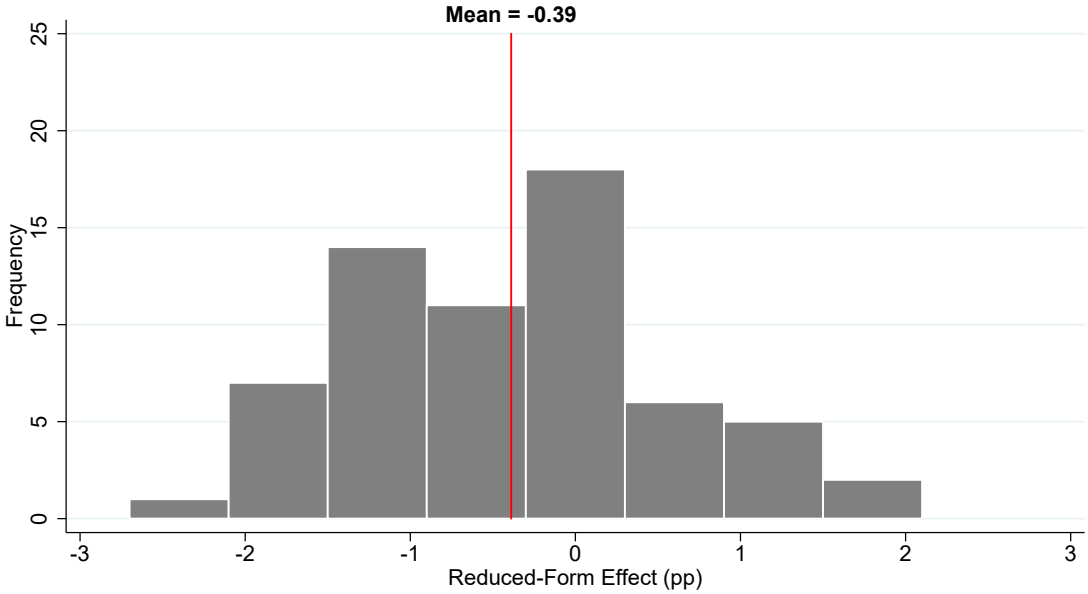
Treated vs Control States (With vs Without Kids)



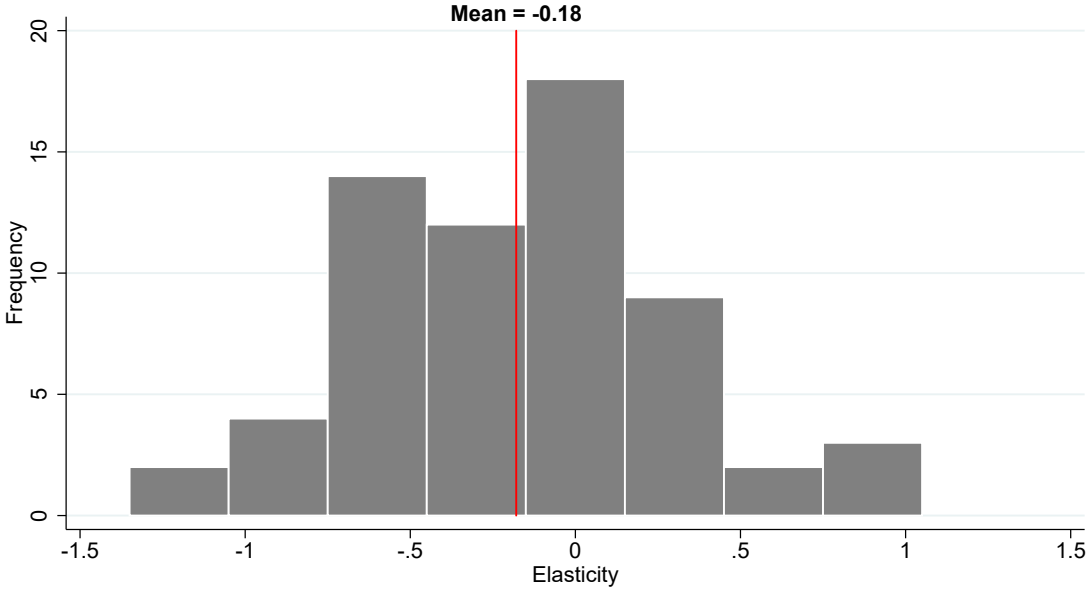
Notes: This figure shows stacked event studies of state EITC reforms using a synthetic control approach. Panels A and C are based on a difference-in-differences approach comparing states with and without EITC reforms for single women with children. Panels B and D are based on a triple-differences approach comparing states with and without EITC reforms for single women with children relative to those without children (in these panels, each series show differences between single women with and without children). The average tax rate (ATR) series include only state income taxes, and they are calculated by setting earnings equal to the first kink of the federal EITC (for each year and number of children), The top row includes all state reforms except for Iowa, Maryland, Minnesota, Rhode Island, Vermont, and Wisconsin (because of small sample sizes) and Indiana, Ohio, and Oregon (because other state tax changes offset the EITC incentive). The bottom row focuses on the ten largest state EITC reforms, i.e. those where the reform-induced ATR reduction for single women with children (relative to those without) was the largest. These are the reforms in Colorado, Connecticut, District of Columbia, Kansas, Massachusetts, Michigan, Nebraska, New Jersey, New Mexico, and New York. Reforms enacted before 1993 are analyzed based on the March CPS files alone, while reforms enacted after 1993 are analyzed based on the March and monthly CPS files combined. The sample includes all single women aged 20-50. The details of each state EITC reform are provided in appendix Table A.2, and the full details of the synthetic control approach are provided in appendix section E.

FIGURE 12: EFFECTS OF STATE EITC REFORMS ACROSS ALL SPECIFICATIONS

A: Distribution of Reduced-Form Estimates



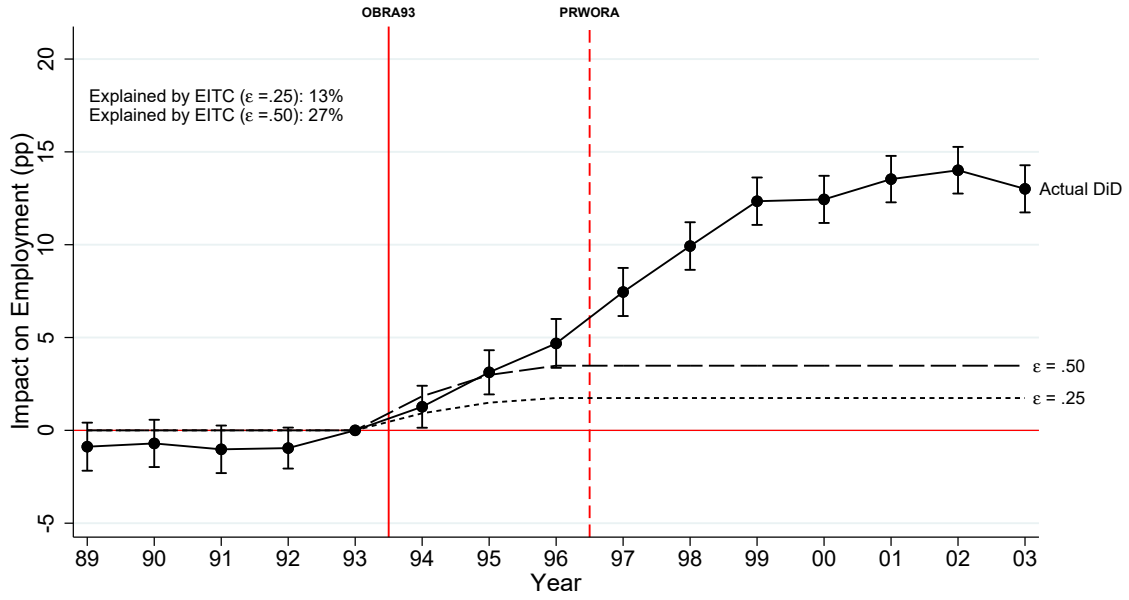
B: Distribution of Elasticity Estimates



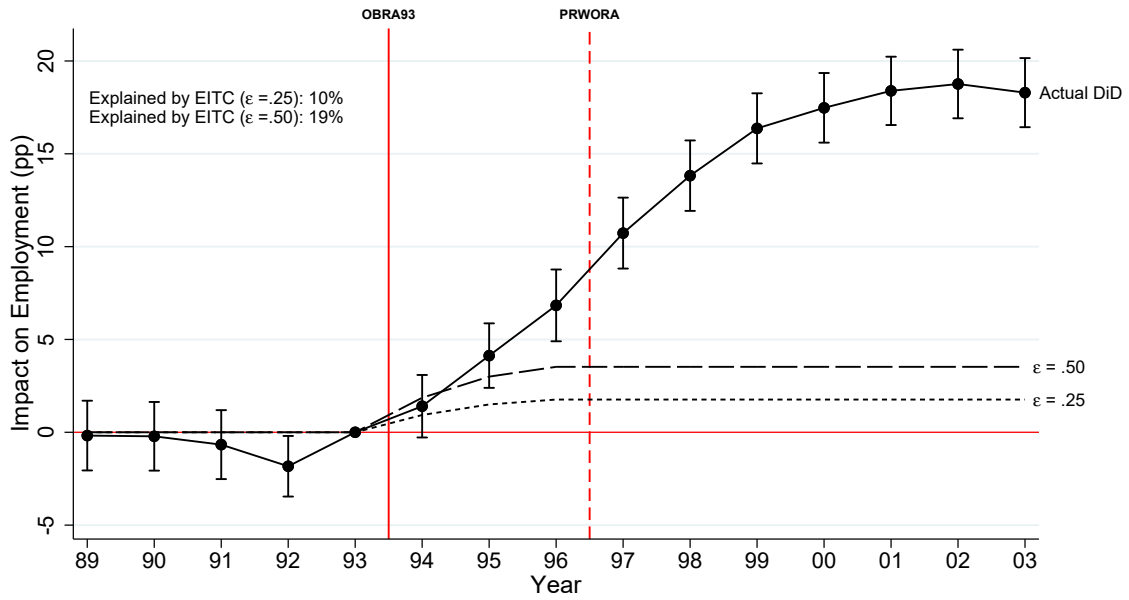
Notes: This figure shows the distribution of estimated effects of state EITC reforms across 64 different specifications. These are permutations of different reform experiments (all reforms stacked and ten largest reforms stacked), empirical strategies (difference-in-differences and triple-differences), extensive margin measures, and samples. Panel A shows reduced-form effects on employment or participation rates (average three-year effects), while Panel B shows the implied extensive margin elasticities with respect to $1 - \tau$. Reforms enacted before 1993 are analyzed based on the March CPS files alone, while reforms enacted after 1993 are analyzed based on the March and monthly CPS files combined. The sample includes single women aged 20-50.

FIGURE 13: ESTIMATED VS SIMULATED RESPONSES TO THE 1993 EITC REFORM
WEEKLY EMPLOYMENT

A: All Single Women



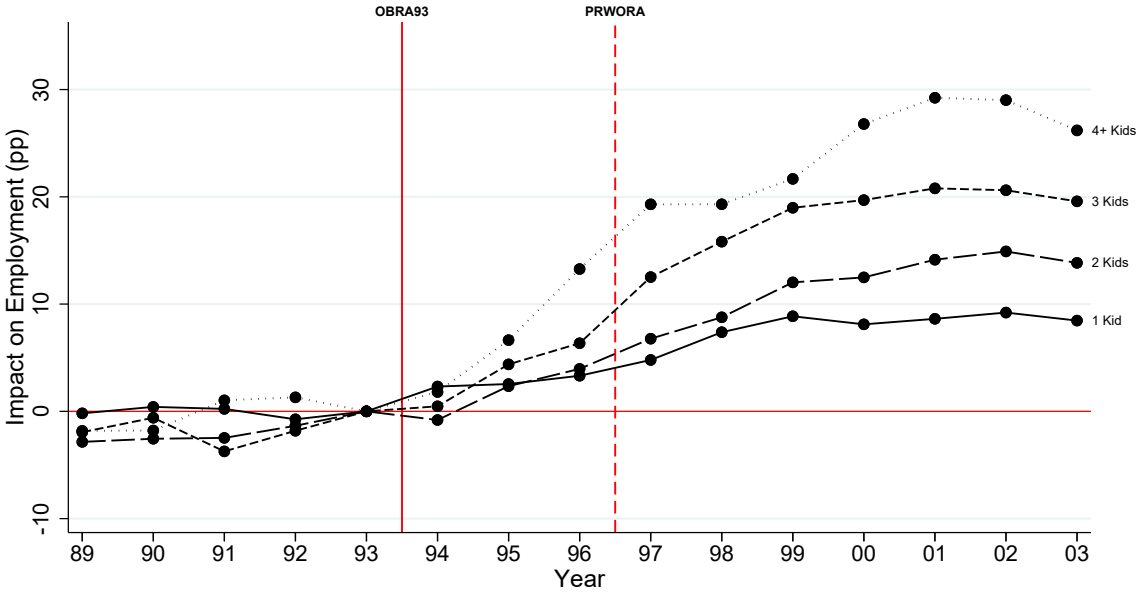
B: Single Women in Bottom Half of Predicted Earnings



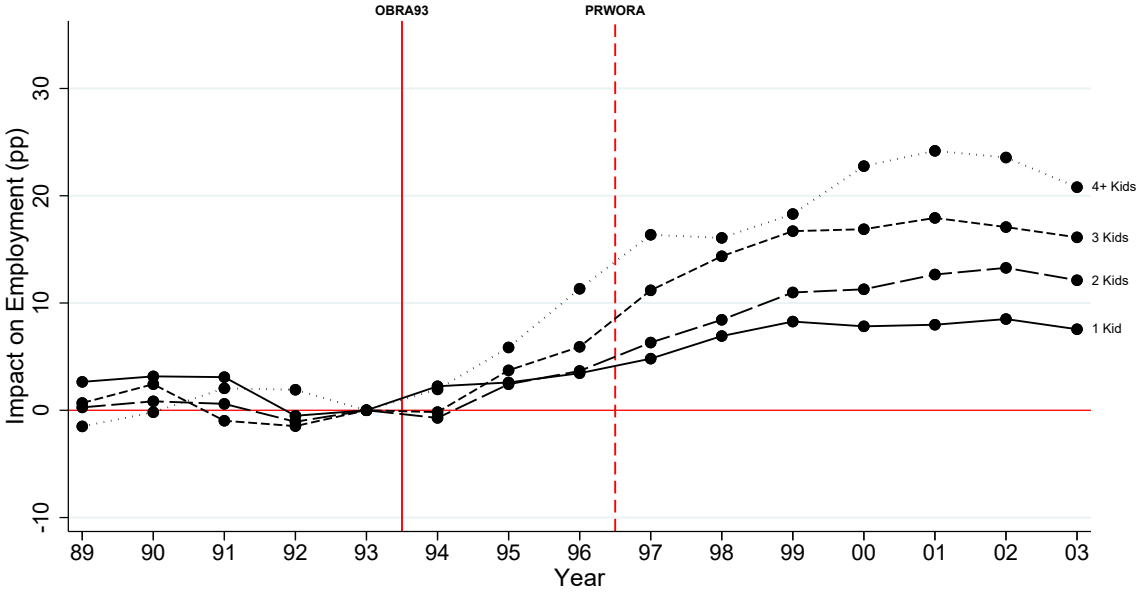
Notes: This figure shows actual and simulated DiD event studies of the 1993 reform. The actual DiD series plot estimates $\hat{\gamma}_t$ from specification (2) without any controls. The simulated DiD series plot ΔP_t calculated from equation (4), assuming an elasticity of 0.25 (short dashes) or 0.5 (long dashes). Panel A shows results for all single women, while Panel B shows results for single women in the bottom half of predicted earnings, estimated based on specification (1). Each panel also shows the fraction of the employment increase over the post-reform period that can be explained by the 1993 tax reform (including but not limited to the EITC expansion) under the two elasticity scenarios. Among the most treated women (Panel B), only 10-19% of the employment increase in the 1990s can be attributed to the EITC even under the sizable tax elasticities assumed. The extensive margin outcome is weekly employment, and 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 14: FANNING-OUT BY NUMBER OF CHILDREN
ALL SINGLE WOMEN, WEEKLY EMPLOYMENT

A: Raw Data



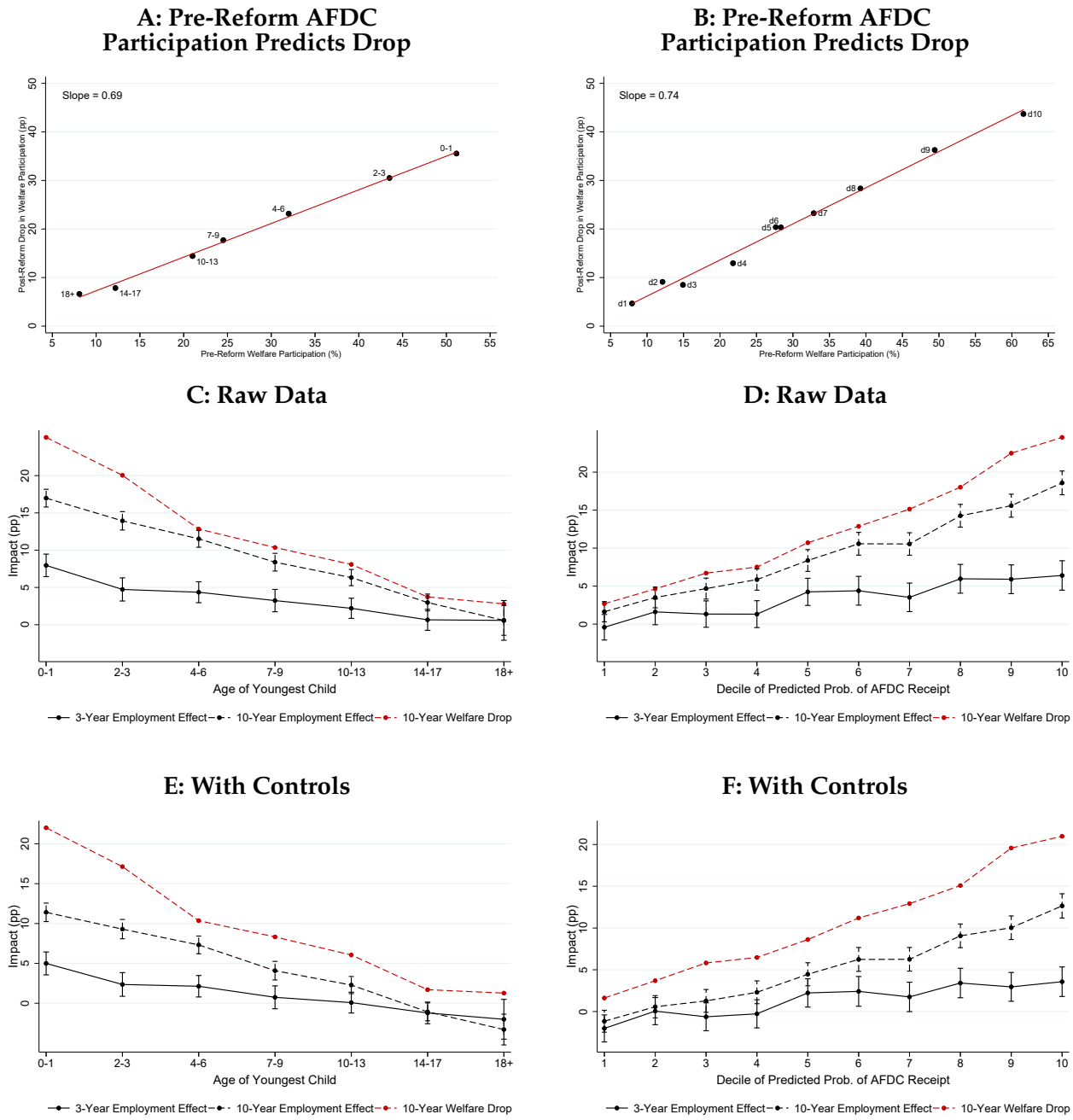
B: Controlling for Demographics



Notes: This figure shows DiD event studies of the 1993 reform by number of EITC-eligible children (1, 2, 3, 4+). The graphs plot estimates $\hat{\gamma}_t^n$ from an extension of specification (2) with separate dummies for each number of children n. Hence, each series shows the difference between single women with n children and single women without children, normalized to zero in 1993. Panel A shows raw estimates, while panel B controls for demographics. The extensive margin outcome is weekly employment. The sample includes single women aged 20-50 using the March and monthly CPS files combined.

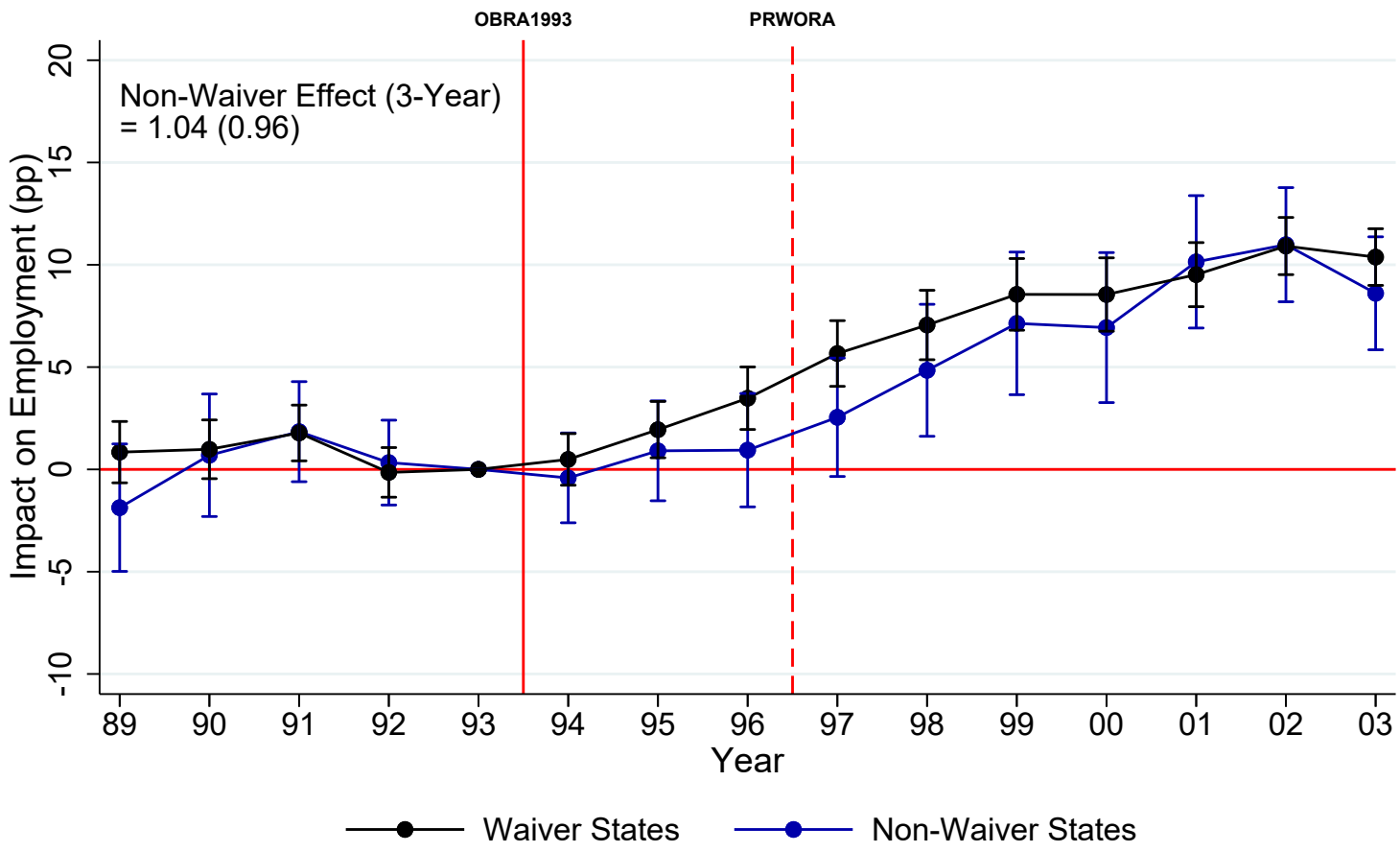
FIGURE 15: EFFECTS OF THE 1993 EITC REFORM BY WELFARE TREATMENT INTENSITY
ALL SINGLE WOMEN, WEEKLY EMPLOYMENT

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 welfare participation rates before the reform (1993) against the drop in welfare participation rates after the reform (from 1993 to 2003) across bins of the welfare treatment proxy. The next rows plot 3-year employment effects (solid black), 10-year employment effects (dashed black), and 10-year welfare drops (dashed red) by welfare treatment intensity in the raw data (middle row) and after controlling for demographics and business cycle effects (bottom row). These are average effects based on post-reform dummies for either 1994-1996 or 1994-2003 dummies. The extensive margin outcome 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 16: HOW MUCH CAN BE EXPLAINED BY WELFARE WAIVERS?
ALL SINGLE WOMEN, WEEKLY EMPLOYMENT



Notes: This figure shows DiD event studies of the 1993 reform for waiver states (black series) and non-waiver states (blue series). Specifically, the series show estimates of the DiD coefficient γ_t from specification (2), implemented separately on states that ever approved statewide waiver legislation and those that did not. Both series include controls for demographics and unemployment. From Table A.3 in the appendix, there were 13 states without any statewide waiver legislation: Alabama, Alaska, District of Columbia, Kansas, Kentucky, Louisiana, Nevada, New Mexico, New York, Oklahoma, Pennsylvania, Rhode Island, and Wyoming. The extensive margin outcome 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.

Online Appendix (Not for Publication)

A Supplementary Figures and Tables

TABLE A.1: FEDERAL EITC PARAMETERS OVER TIME

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.2: STATE EITC REFORMS

State	Year Instituted	% of Fed. Credit	Type	Reform-Induced ATR Changes (pp) Any Children	1 Child	2 Children	3+ Children
Alabama	.	.	.	.	.	.	.
Alaska	.	.	.	.	.	.	.
Arizona	.	.	.	.	.	.	.
Arkansas	.	.	.	.	.	.	.
California[1]	2015	n/a	R	-0.70	-1.02	-0.40	-0.56
Colorado[2]	1999	10	R	-3.05	-2.74	-3.56	-3.06
Connecticut	2011	25	R	-8.32	-6.97	-8.74	-11.09
Delaware	2006	20	NR	-0.14	-0.10	-0.23	-0.14
Dist. of Columbia	2000	25	R	-7.15	-6.39	-7.09	-8.71
Florida	.	.	.	.	.	.	.
Georgia	.	.	.	.	.	.	.
Hawaii	2018	20	NR	.	.	.	.
Idaho	.	.	.	.	.	.	.
Illinois	2000	5	NR	-0.98	-1.03	-1.15	-0.35
Indiana[3]	1999	n/a	NR	2.36	2.31	2.53	1.84
Iowa	1990	6.5	NR	-0.19	-0.44	0.16	-0.14
Kansas	1998	10	R	-3.62	-3.05	-4.34	-3.90
Kentucky	.	.	.	.	.	.	.
Louisiana	2008	3.5	R	-1.23	-1.24	-1.11	-1.42
Maine	2000	5	NR	-0.04	-0.02	-0.08	-0.02
Maryland[4]	1987	50	NR	-1.27	-1.34	-1.18	-1.30
Massachusetts	1997	10	R	-3.35	-3.07	-3.79	-3.46
Michigan	2008	20	R	-6.43	-5.81	-6.60	-7.53
Minnesota[5]	1991	n/a	R	-2.88	-3.08	-2.69	-2.93
Mississippi	.	.	.	.	.	.	.
Missouri	.	.	.	.	.	.	.
Montana	.	.	.	.	.	.	.
Nebraska	2005	8	R	-3.29	-2.96	-3.71	-3.66
Nevada	.	.	.	.	.	.	.
New Hampshire	.	.	.	.	.	.	.
New Jersey	2000	17.5	R	-6.23	-5.75	-6.84	-6.89
New Mexico	2007	10	R	-3.54	-3.00	-3.86	-4.11
New York	1994	20	R	-6.16	-5.41	-6.78	-6.90
North Carolina[6]	2008	5	R	-2.07	-1.99	-2.03	-2.40

State	Year Instituted	% of Fed. Credit	Type	Reform-Induced ATR Changes (pp) Any Children	1 Child	2 Children	3+ Children
North Dakota	.	.	.	.	.	.	.
Ohio	2013	10	NR	0.05	0.02	0.01	0.17
Oklahoma	2002	5	R	-1.36	-1.31	-1.21	-1.72
Oregon[7]	1997	5	NR	0.04	-0.13	0.18	0.18
Pennsylvania	.	.	.	.	.	.	.
Rhode Island[8]	1986	25	NR	-0.22	-0.30	-0.03	-0.03
South Carolina	2018	63	NR	.	.	.	.
South Dakota	.	.	.	.	.	.	.
Tennessee	.	.	.	.	.	.	.
Texas	.	.	.	.	.	.	.
Utah	.	.	.	.	.	.	.
Vermont	1988	28	R	-4.09	-4.11	-4.14	-3.92
Virginia	2006	20	NR	-0.02	0.01	-0.04	-0.04
Washington[9]	2008	n/a	n/a	-0.00	-0.00	-0.00	-0.00
West Virginia	.	.	.	.	.	.	.
Wisconsin[10]	1989	5/25/75	R	-4.30	-0.56	-4.04	-13.38
Wyoming	.	.	.	.	.	.	.

Notes: This table shows details on state EITC reforms. The first three columns show the years in which state EITC supplements were instituted, along with their parameters 3 years after introduction. The notation R and NR refers to whether the credit is refundable or non-refundable. The last four columns show average tax rate changes induced by state EITC reforms for single women with children relative to single women with no children (allowing for a 3-year phase-in). The average tax rates in this table include only state taxes and are calculated by setting earnings equal to the first kink of the federal EITC.

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.3: 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	9-97	10-92	12-92
Colorado												6-94
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												4-94
Mississippi									9-95	10-95		10-95
Missouri			4-95				4-95	6-95			1-93	7-94

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	7-92	2-93				
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: Table B in Department of Health and Human Services (1999). Footnotes to Table B are used to modify certain cells of the table.

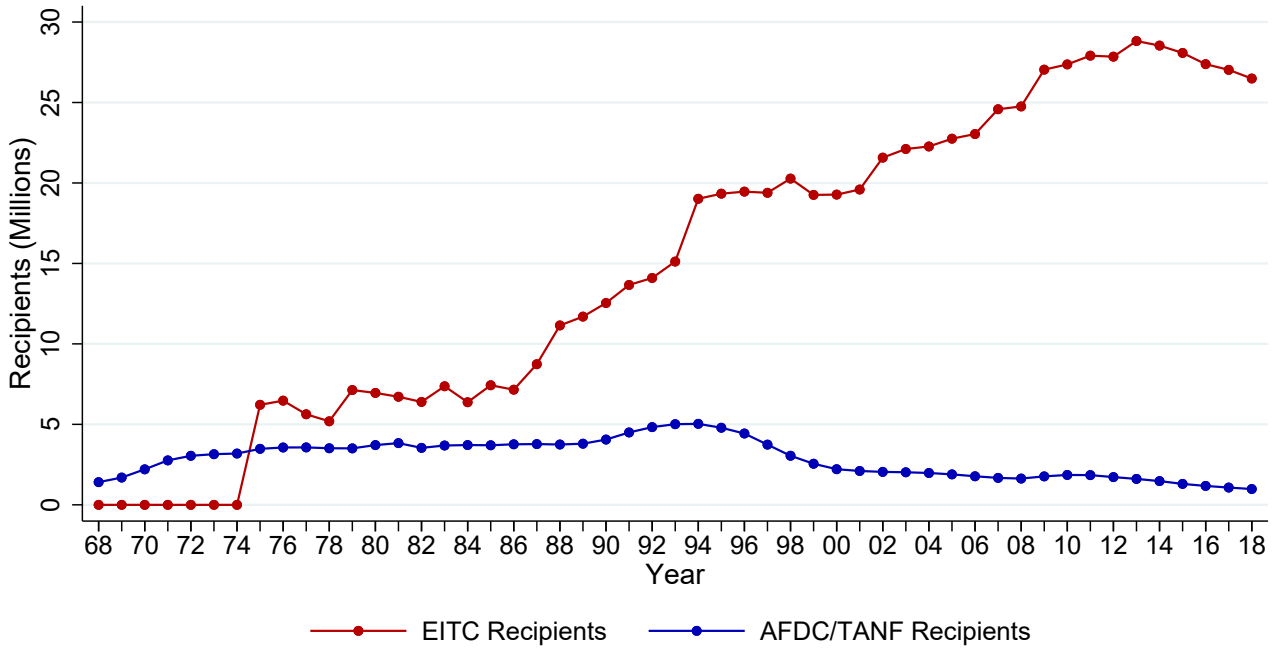
Notes: This table shows dates of approval and implementation for the six main types of statewide welfare waivers. For waivers that were rolled out at the county level, dates of implementation correspond to the date the first county implemented the reform.

Table A.4: Converting Prior Estimates Into Elasticities

Sample	Reform	Employment Effect (pp)	$\frac{\Delta P}{P}$	$\frac{\Delta(1-\tau)}{1-\tau}$	Elasticity
Panel A: Eissa and Liebman (1996)					
All Single Mothers	1986	0.022	0.030	0.042	0.71
Low-Educated Single Mothers	1986	0.038	0.057	0.043	1.34
Panel B: Schanzenbach and Strain (2020)					
All Single Mothers	1975	0.067	0.103	0.124	0.84
Low-Educated Single Mothers	1975	0.071	0.115	0.124	0.93
All Single Mothers	1986	0.014	0.021	0.035	0.59
Low-Educated Single Mothers	1986	0.037	0.060	0.036	1.66
All Single Mothers	1990	0.009	0.013	0.042	0.30
Low-Educated Single Mothers	1990	0.021	0.032	0.042	0.78
All Single Mothers	2009	-0.015	-0.022	0.044	-0.50
Low-Educated Single Mothers	2009	-0.001	-0.001	0.043	-0.03

Notes: This table converts reduced-form estimates from Eissa and Liebman (1996) and Schanzenbach and Strain (2020) into elasticities with respect to $1-\tau$. These papers estimate the effects of federal EITC reforms on annual employment for all single mothers and low-educated single mothers, respectively. For Eissa and Liebman (1996), I use their estimates reported in Table III (column 5) for all single women and Table IV (columns 1 and 2) for low-educated single women. As they report separate estimates for those below high school and those at the high school level, the Eissa-Liebman estimate for low-educated single mothers shown above is a weighted average of their two estimates using the pre-reform (1984-1986) fractions of low-educated single mothers at each level. The Eissa-Liebman estimates represent four-year effects of the 1986 reform (comparing 1984-1986 to 1988-1990). For Schanzenbach and Strain (2020), I use their estimates reported in Table 3 (column 2). These are average three-year effects in the sample of either all single women or low-educated single women, defined as those with high school or less. I focus on their estimates for the 1975, 1986, 1990, and 2009 reforms, leaving out the confounded 1993 reform. Both papers give estimates of the effect on the employment rate ΔP (shown in the second column above). When calculating the percentage change in the employment rate $\Delta P / P$ and in the average net-of-tax rate $\Delta\left(1-\tau\right) / \left(1-\tau\right)$, I replicate as closely as possible the estimations samples of each paper. The change in the average net-of-tax rate in the denominator of the elasticity is calculated to correspond to the reduced-form estimate in the numerator of the elasticity. Specifically, it is a difference-in-differences calculation comparing treatment and control groups over the same time horizon as the reduced-form effect: the total change between 1984-1986 and 1988-1990 for Eissa-Liebman and the average change over the first three post-reform years for Schanzenbach-Strain. This tax term is based on the tax rate series in Figure 2. These tax rates account for all income and payroll tax provisions, and they assume that single women enter the labor market at the first kink of the federal EITC (for each year and family size).

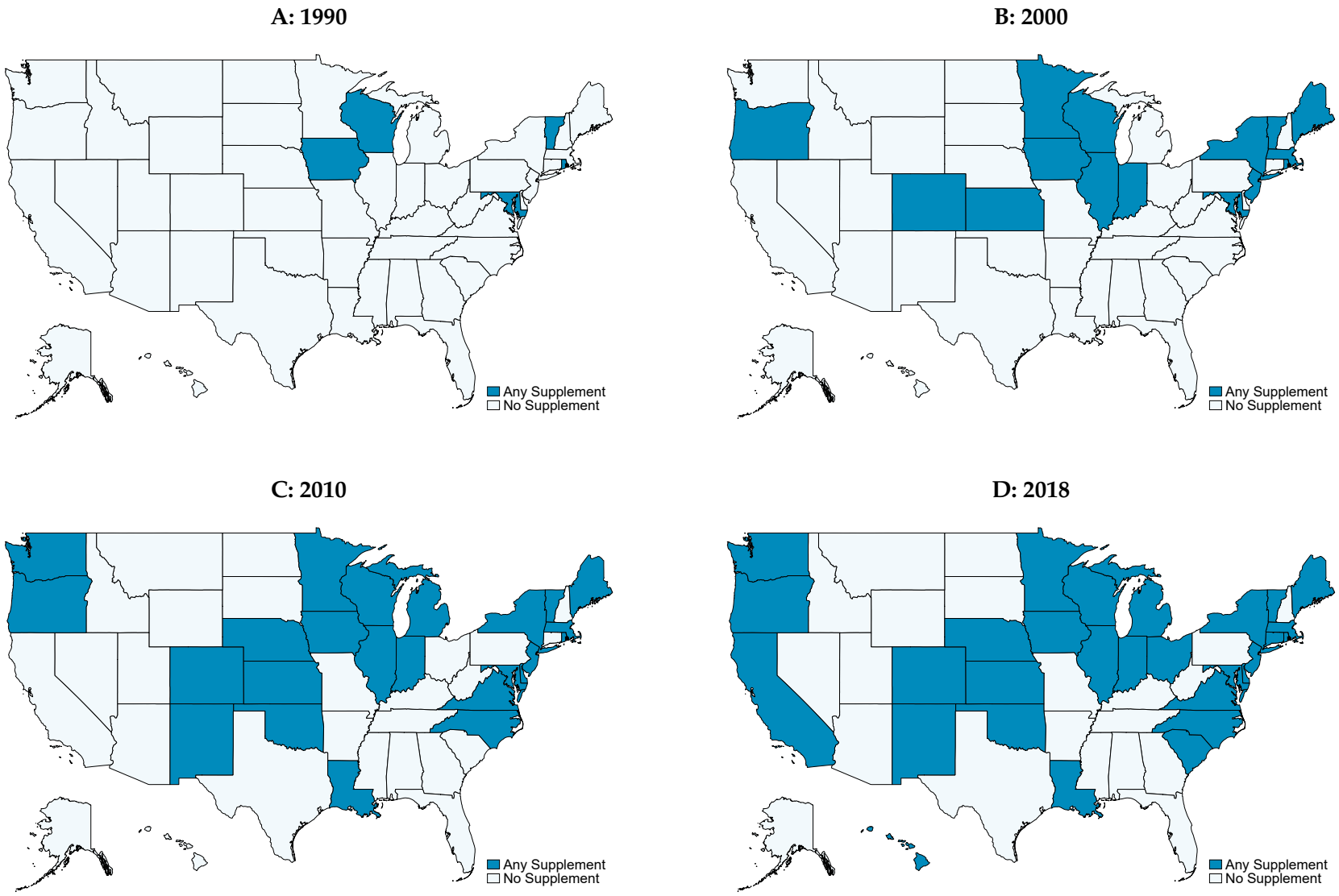
Figure A.1: Long-Run Evolution of EITC and Cash Welfare Recipients



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

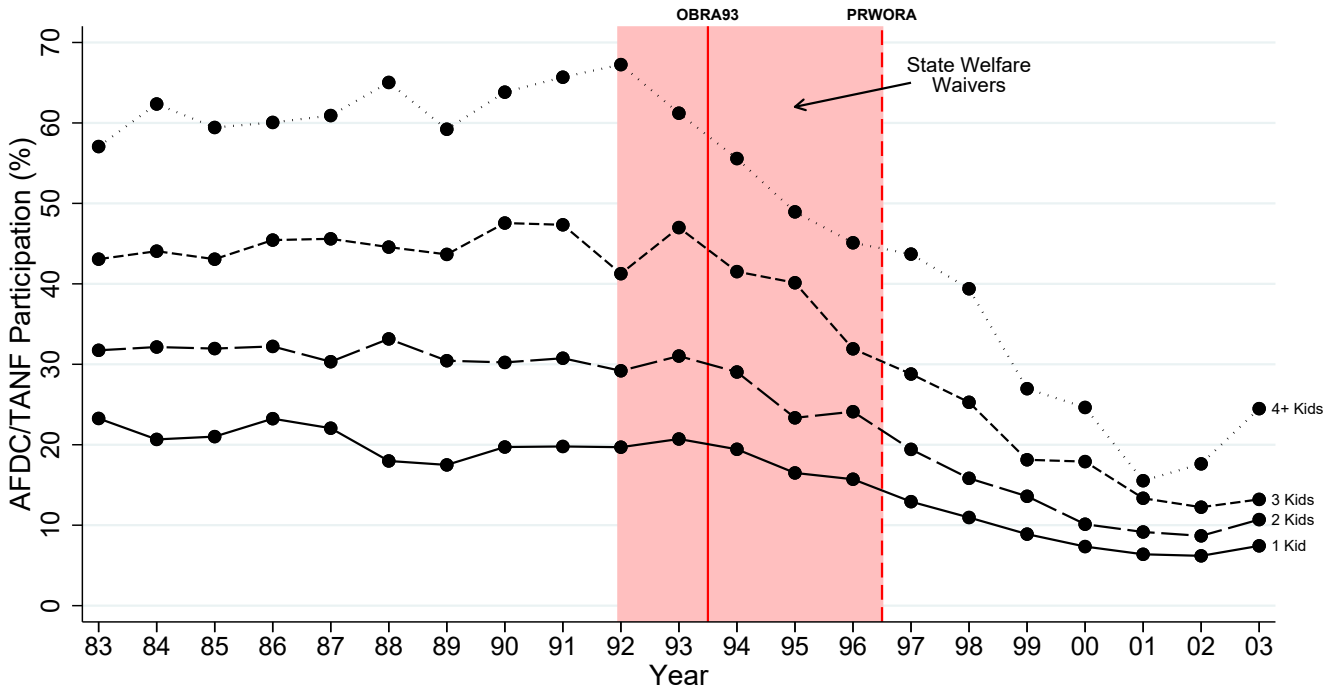
Notes: The red series show the number of families receiving the federal EITC between 1968-2018. The blue series show the number of families receiving Aid to Families with Dependent Children (AFDC) between 1968-1996 and Temporary Assistance for Needy Families (TANF) between 1997-2018.

Figure A.2: States with EITC Supplements



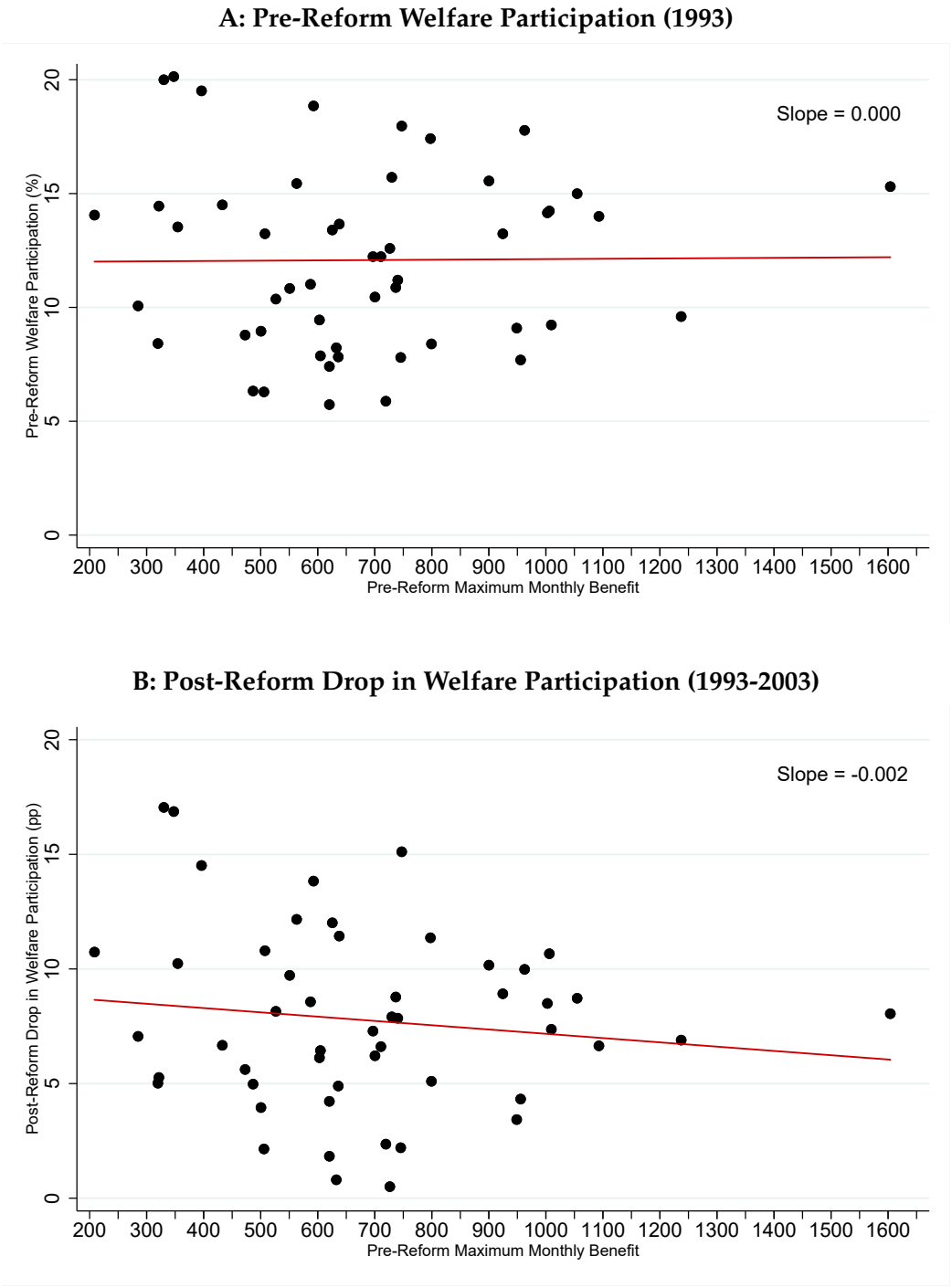
Notes: The maps show which states had an EITC supplement (dark blue) and which states did not (light blue) in 1990, 2000, 2010, and 2018.

FIGURE A.3: AFDC/TANF PARTICIPATION RATES FOR SINGLE MOTHERS BY NUMBER OF CHILDREN



Notes: This figure shows AFDC/TANF participation rates for single mothers by number of EITC-eligible children (1, 2, 3, 4+) over the period 1983-2003. The 1993 federal EITC reform is marked by the solid red line, the 1996 federal welfare reform is marked by the dashed red line, and statewide welfare waiver reforms are marked by the red shaded area.

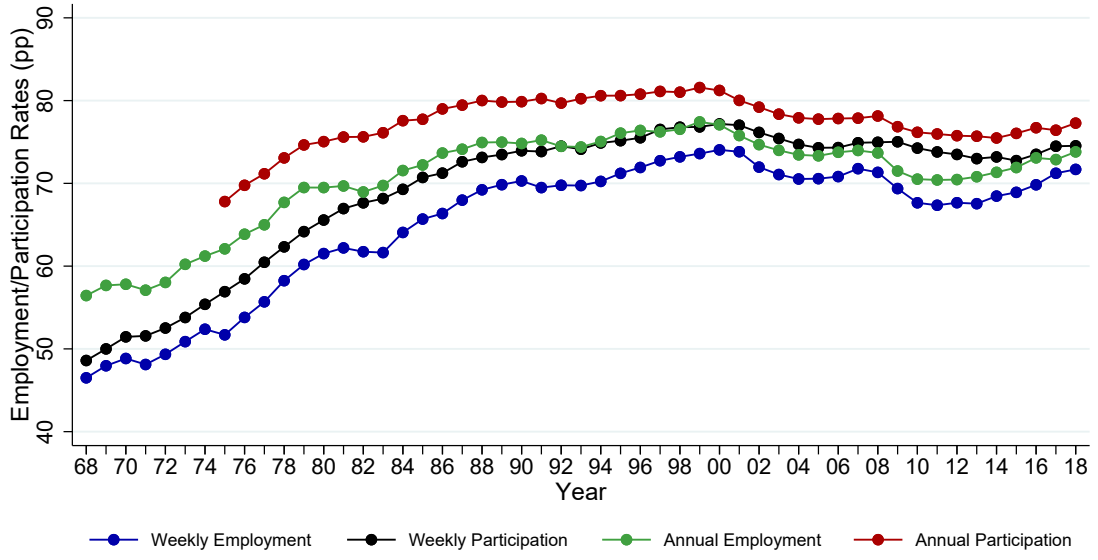
Figure A.4: Welfare Participation vs Benefit Levels Across States



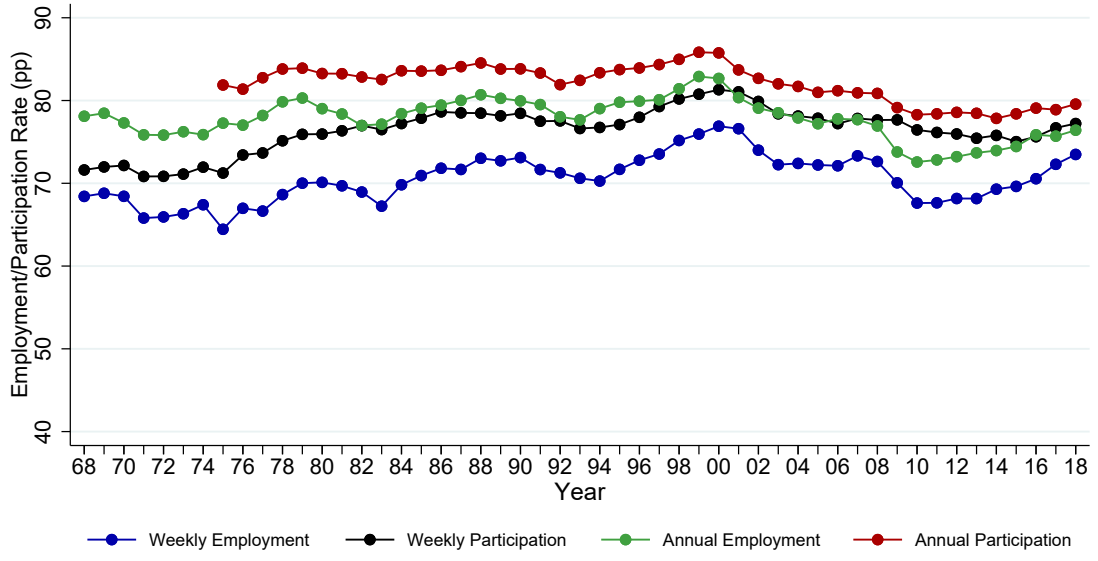
Notes: This figure investigates the empirical relationship between welfare participation rates and welfare benefit levels across US states in the 1990s. Panel A plots pre-reform welfare participation rates against pre-reform maximum monthly benefits across states, while Panel B plots post-reform drops in welfare participation rates against pre-reform maximum monthly benefits across states. The pre-reform year (1993) is before both EITC and AFDC/TANF reforms, while the post-reform period (1993-2003) includes both EITC and AFDC/TANF reforms. The maximum monthly benefit levels (measured in 2018 USD) are those applying to single women with two children. The sample includes single women aged 20-50 using the March files. The figure shows that state variation in pre-reform benefit levels is uncorrelated with pre-reform welfare participation rates (Panel A), and therefore does not predict post-reform drops in welfare participation rates either (Panel B). This absence of a first stage implies that state variation in welfare benefit levels is not useful for estimating the impact of welfare reform in the 1990s.

Figure A.5: Extensive Margin Measures

A: All Women, 20-50

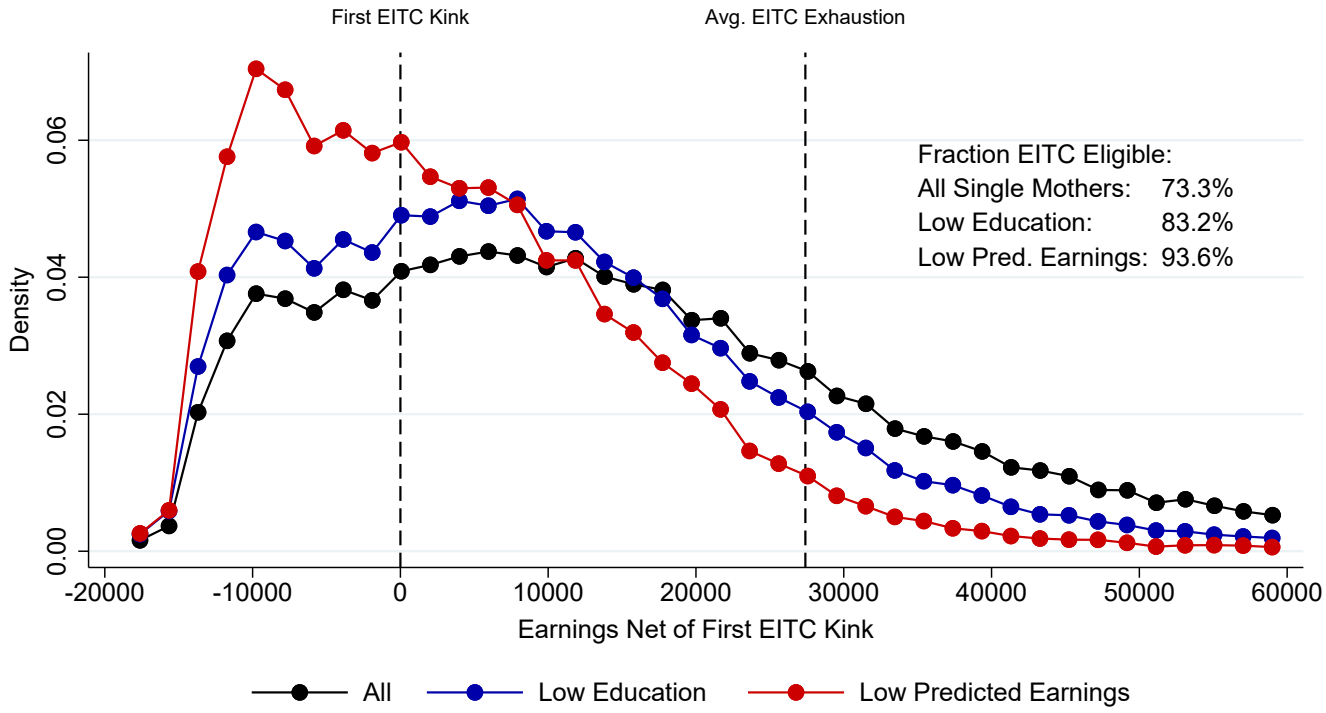


B: Single Women, 20-50



Notes: This figure shows the evolution of weekly employment, weekly participation, annual employment, and annual participation between 1968-2018. Panel A considers all women (panel A) aged 20-50, while Panel B considers single women aged 20-50. The samples are based on the March CPS files. See section B.1 for additional details.

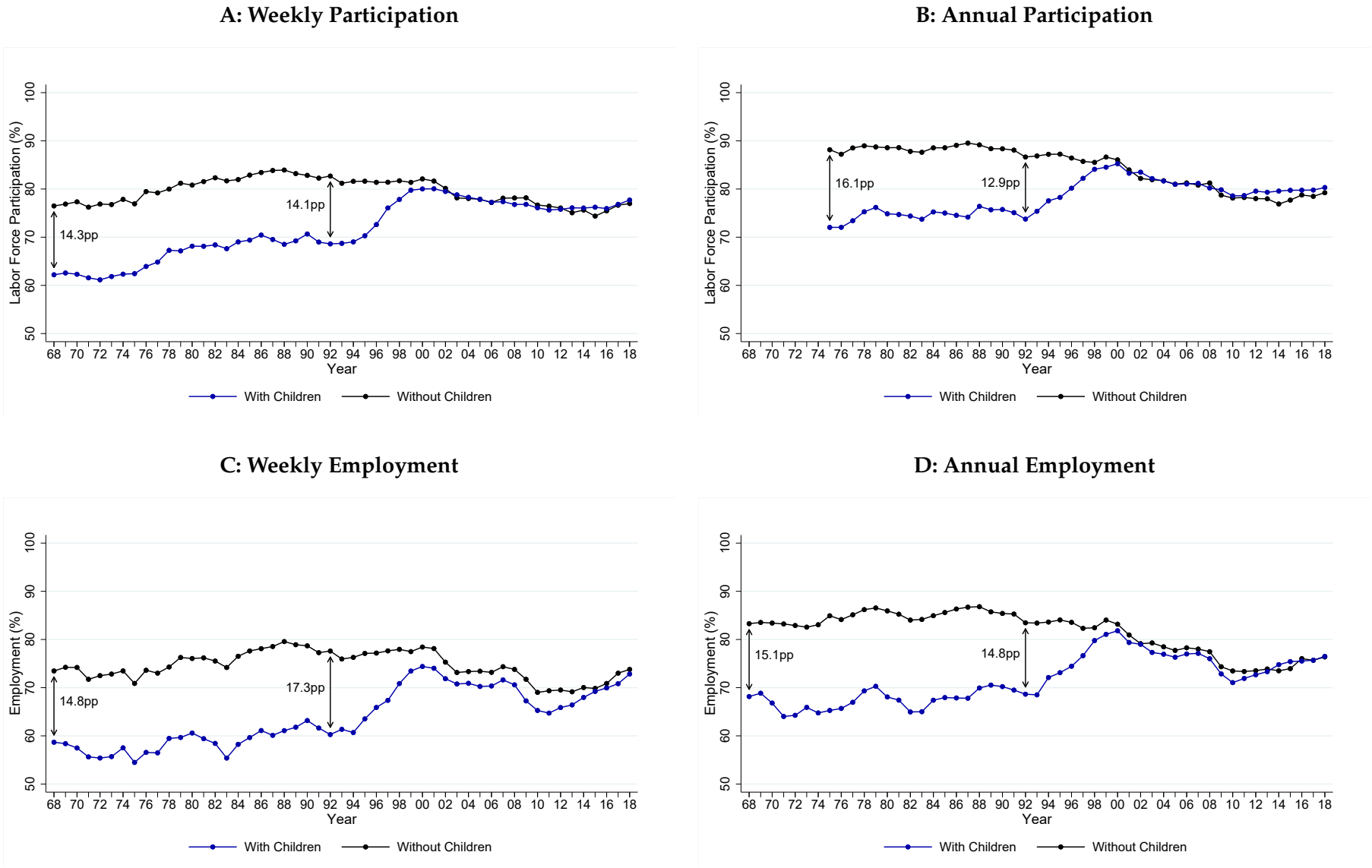
FIGURE A.6: EARNINGS DISTRIBUTIONS FOR SINGLE MOTHERS



Notes: This figure shows earnings distributions in different samples of single mothers. The distributions are based on pooled earnings data from 1975-2018, where earnings in each year are measured relative to the first EITC kink in that year. Hence, having earnings at the first EITC kink corresponds to zero. The average EITC exhaustion point (again, relative to the first kink) is calculated as an average across all years and observations. The distributions are shown for all single mothers, low-educated single mothers, and single mothers with low predicted earnings. Low-educated single mothers are those with a high school degree or less, while low-earning single mothers are those in the bottom quartile of the within-year distribution of predicted earnings, estimated from equation (1). For each of the three samples, the figure reports the fraction who are EITC eligible (i.e., below the EITC exhaustion point). The samples include single mothers aged 20-50 using the March CPS files. All dollar values have been inflation adjusted to 2018.

Figure A.7: Fifty Years of Labor Force Participation for Single Women

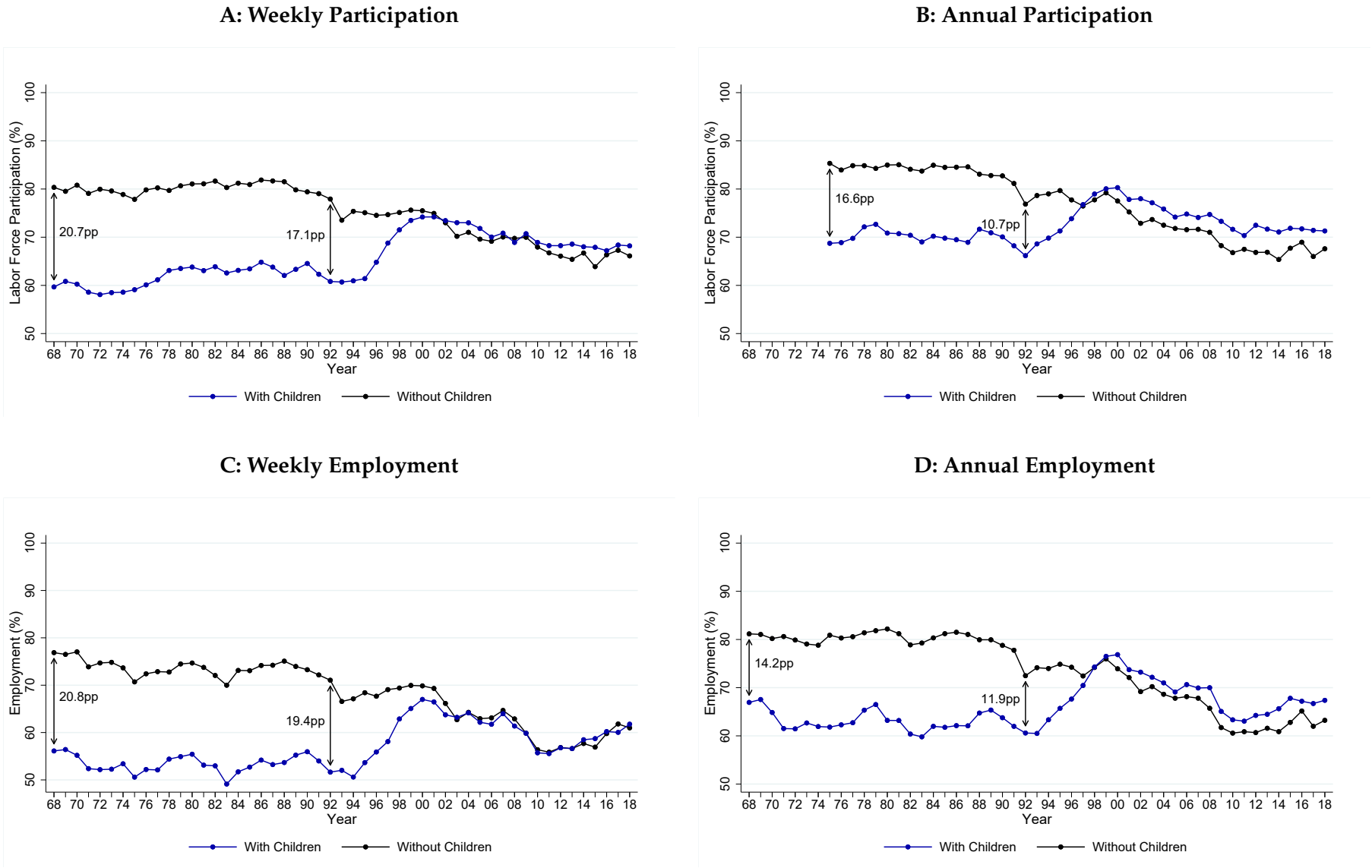
Different Extensive Margin Measures, All Single Women



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

Figure A.8: Fifty Years of Labor Force Participation for Single Women

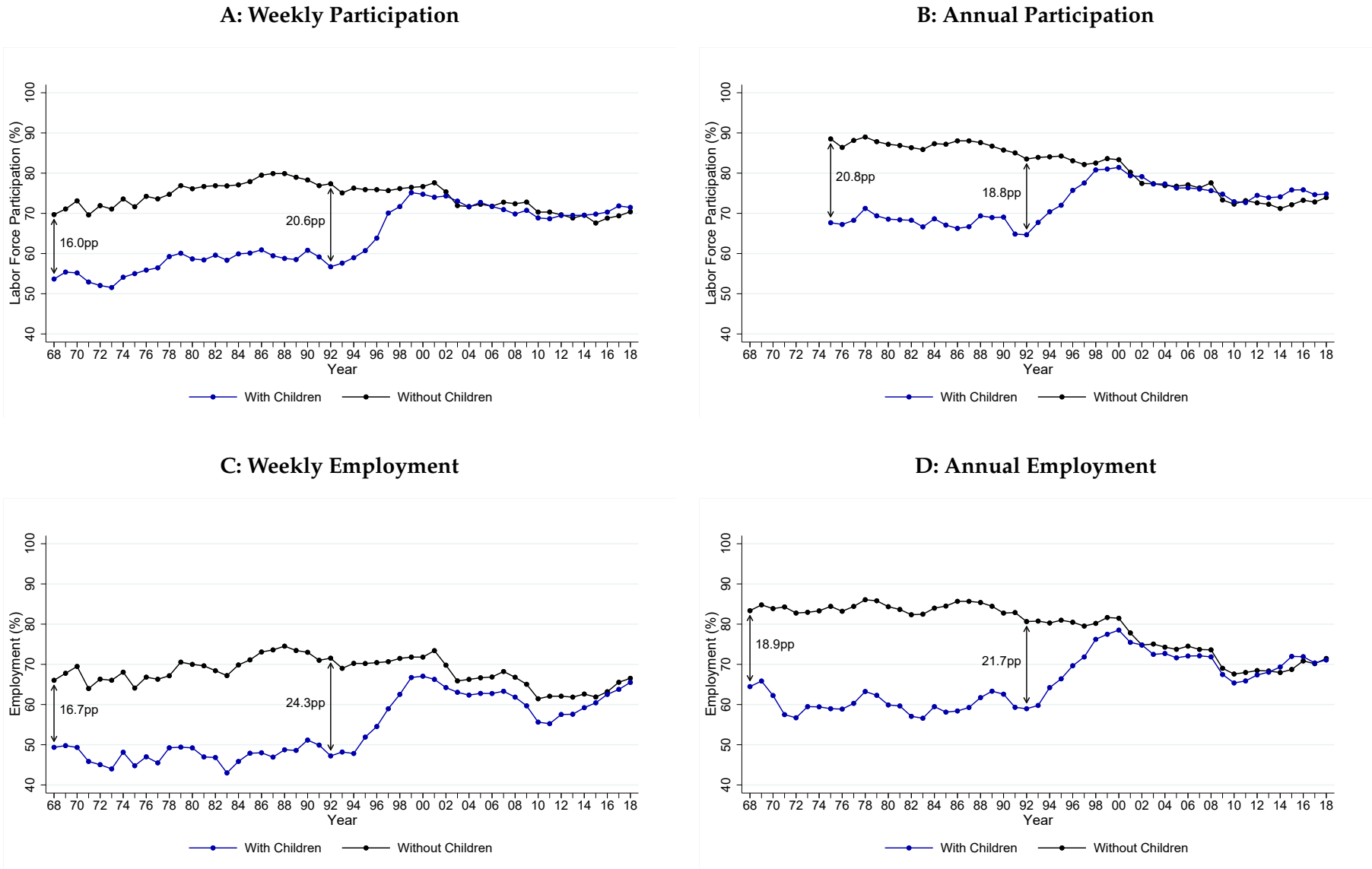
Different Extensive Margin Measures, Low-Educated Single Women



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

Figure A.9: Fifty Years of Labor Force Participation for Single Women

Different Extensive Margin Measures, Single Women in the Bottom Half of Predicted Earnings

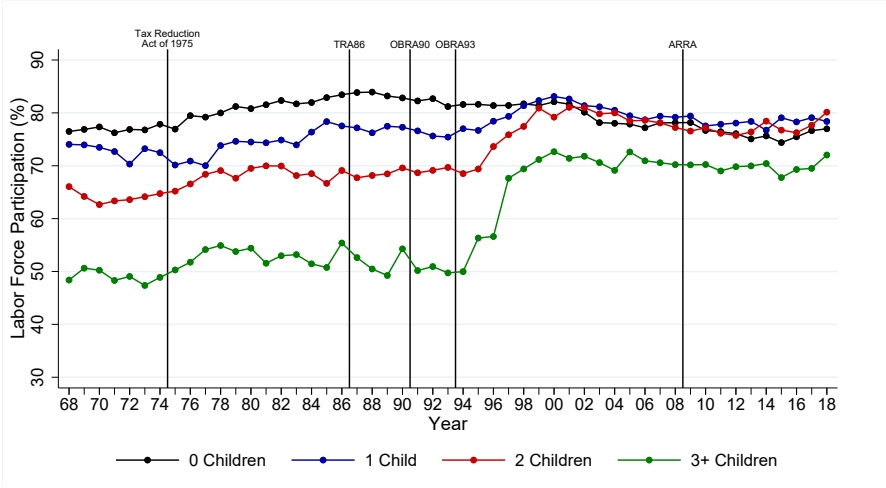


Notes: This figure shows the weekly participation, annual participation, weekly employment, and annual employment rates of single women with and without children between 1968 and 2018. The sample includes single women with low predicted earnings aged 20-50 using the March CPS files. The low earnings sample includes single women in the bottom half of the within-year distribution of predicted earnings, estimated using equation (1).

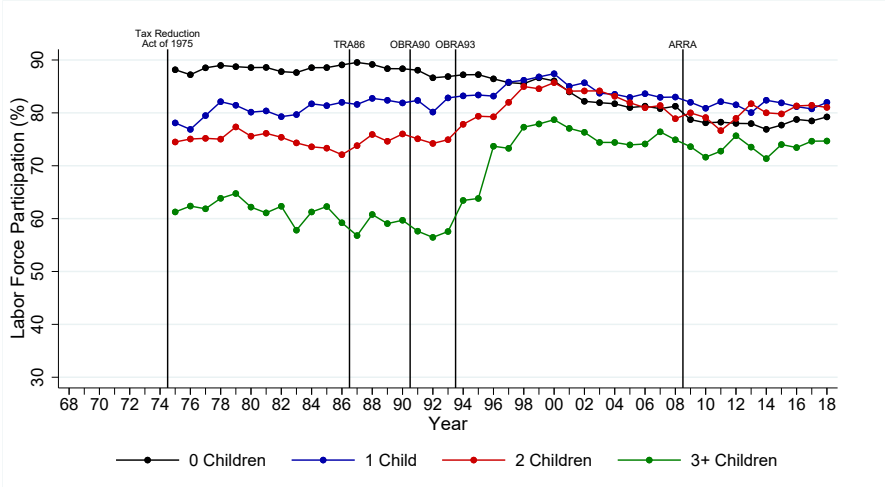
Figure A.10: Fifty Years of Participation for Single Women, by Number of Kids

Different Extensive Margin Measures, All Single Women

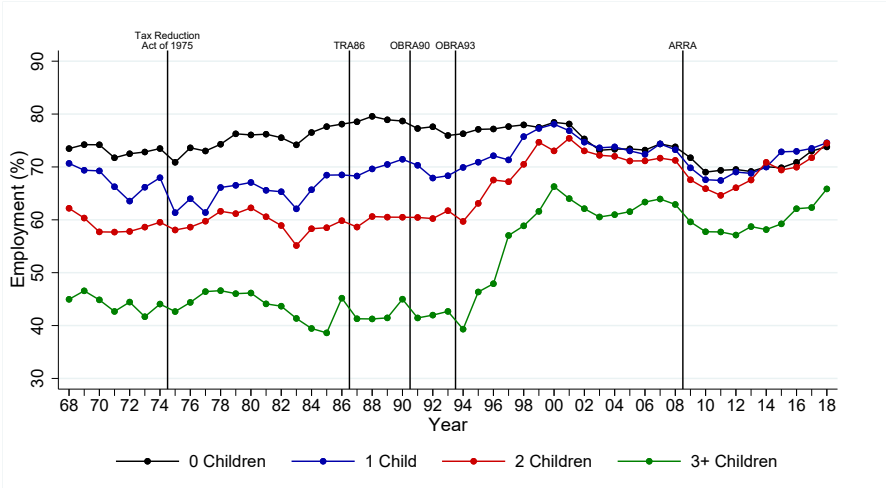
A: Weekly Participation



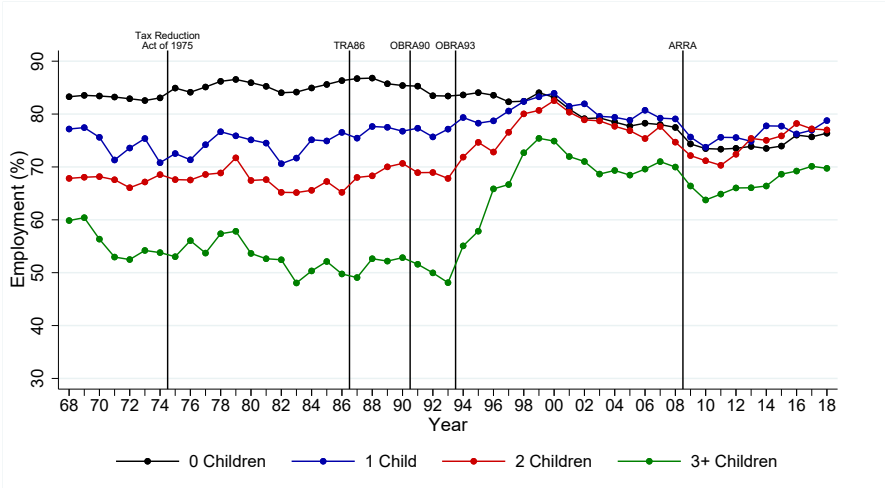
B: Annual Participation



C: Weekly Employment



D: Annual Employment

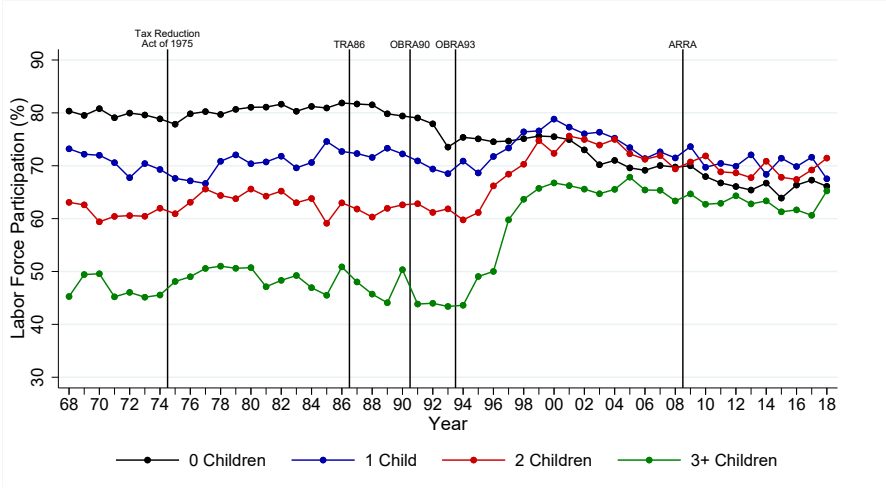


Notes: This figure shows the weekly participation, annual participation, weekly employment, and annual employment rates of single women with 0, 1, 2, and 3 or more children between 1968 and 2018. The sample includes all single women aged 20-50 using the March CPS files.

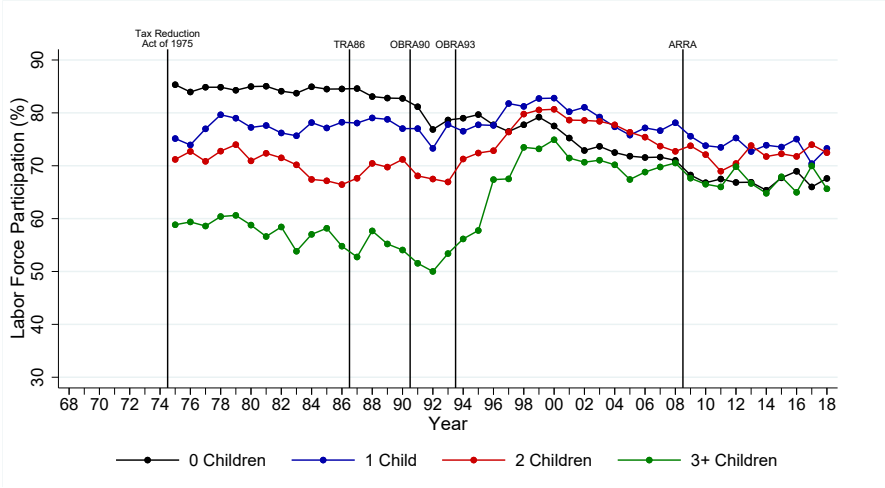
Figure A.11: Fifty Years of Participation for Single Women, by Number of Kids

Different Extensive Margin Measures, Low-Educated Single Women

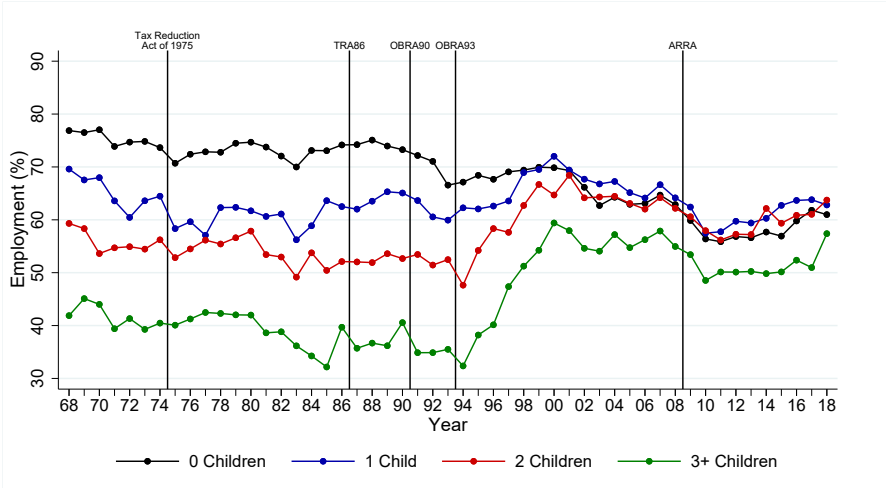
A: Weekly Participation



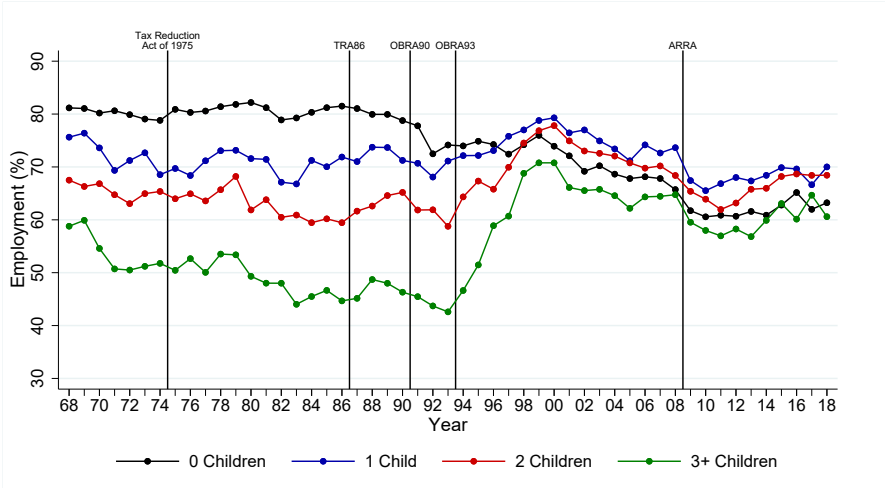
B: Annual Participation



C: Weekly Employment



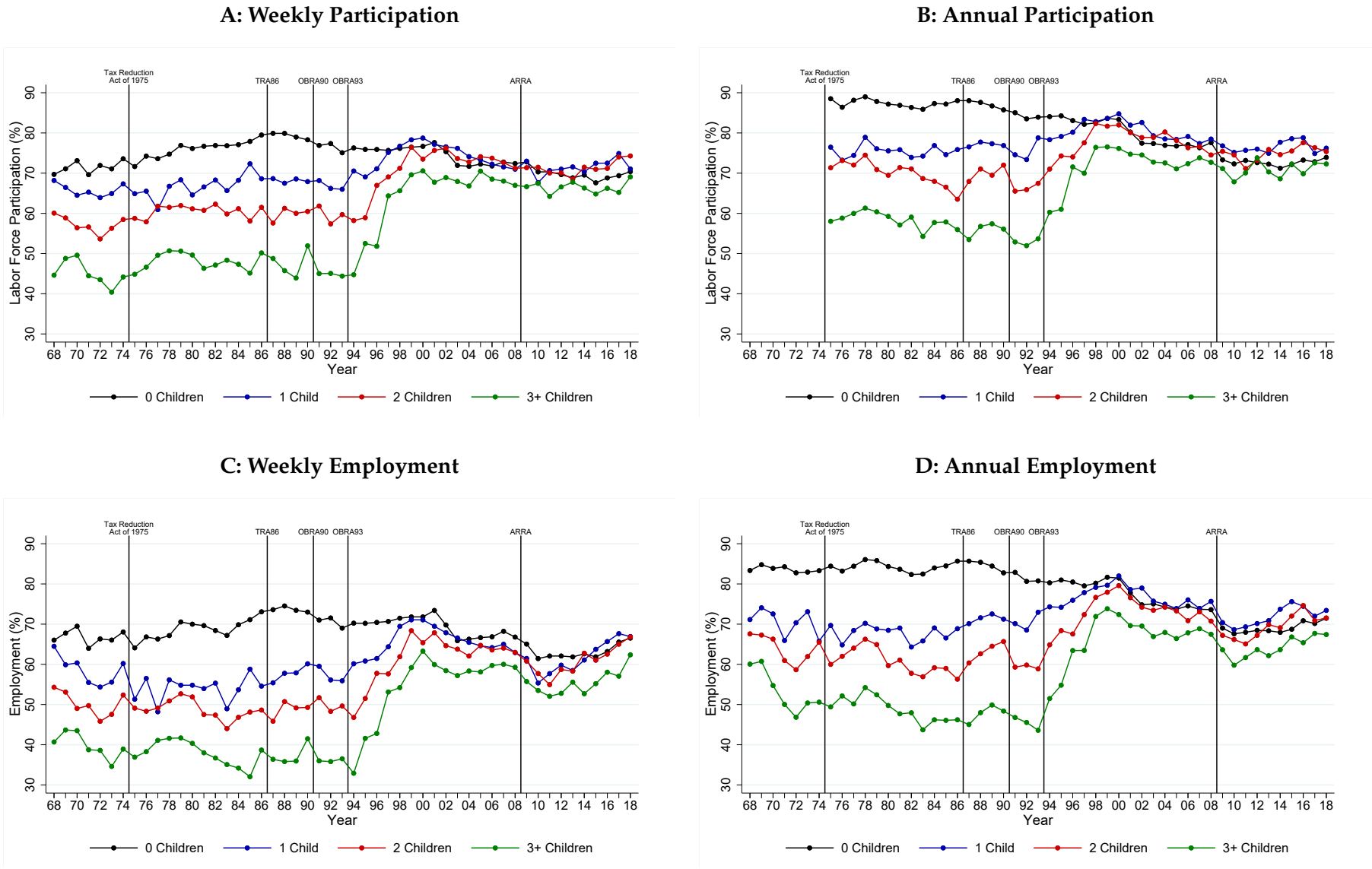
D: Annual Employment



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 between 1968 and 2018. The sample includes low-educated single women (those with a high school degree or less) aged 20-50 using the March CPS files.

Figure A.12: Fifty Years of Participation for Single Women, by Number of Kids

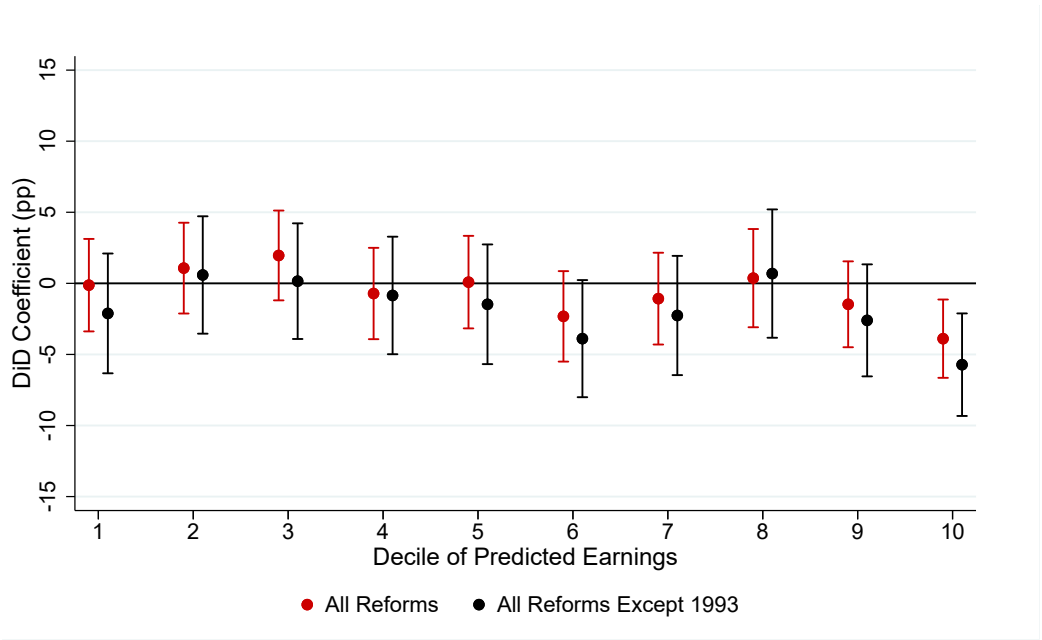
Different Extensive Margin Measures, Single Women in the Bottom Half of Predicted Earnings



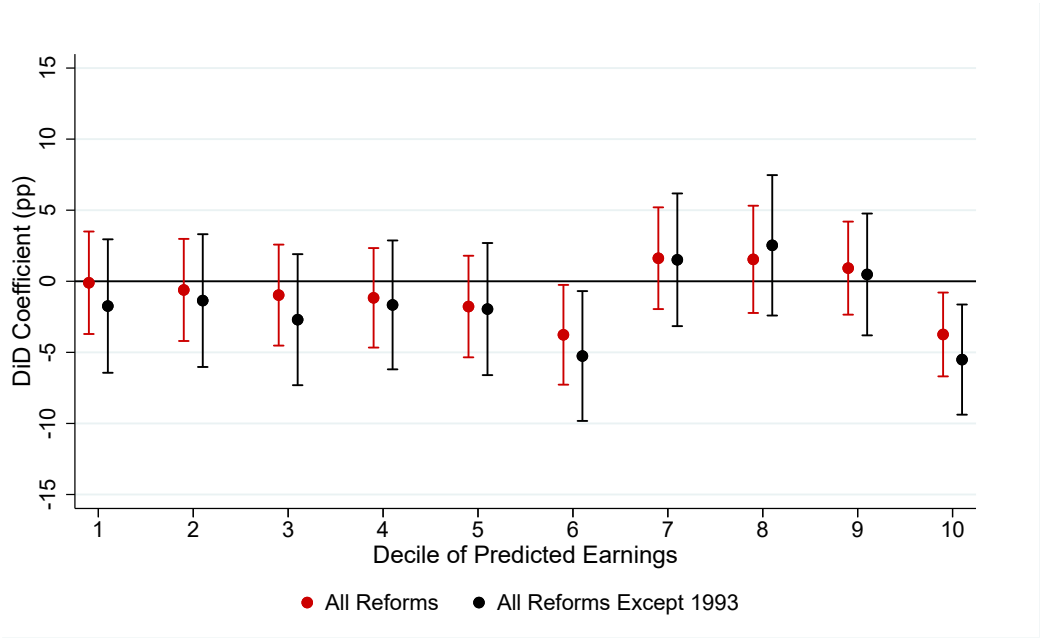
Notes: This figure shows the weekly participation, annual participation, weekly employment, and annual employment rates rate of single women with 0, 1, 2, and 3 or more between 1968 and 2018. The sample includes single women with low predicted earnings aged 20-50 using the March CPS files. The low earnings sample includes single women in the bottom half of the within-year distribution of predicted earnings, estimated using equation (1).

FIGURE A.13: EFFECTS OF FEDERAL REFORMS STACKED BY DECILE OF PREDICTED EARNINGS
WEEKLY EMPLOYMENT

A: Raw Data



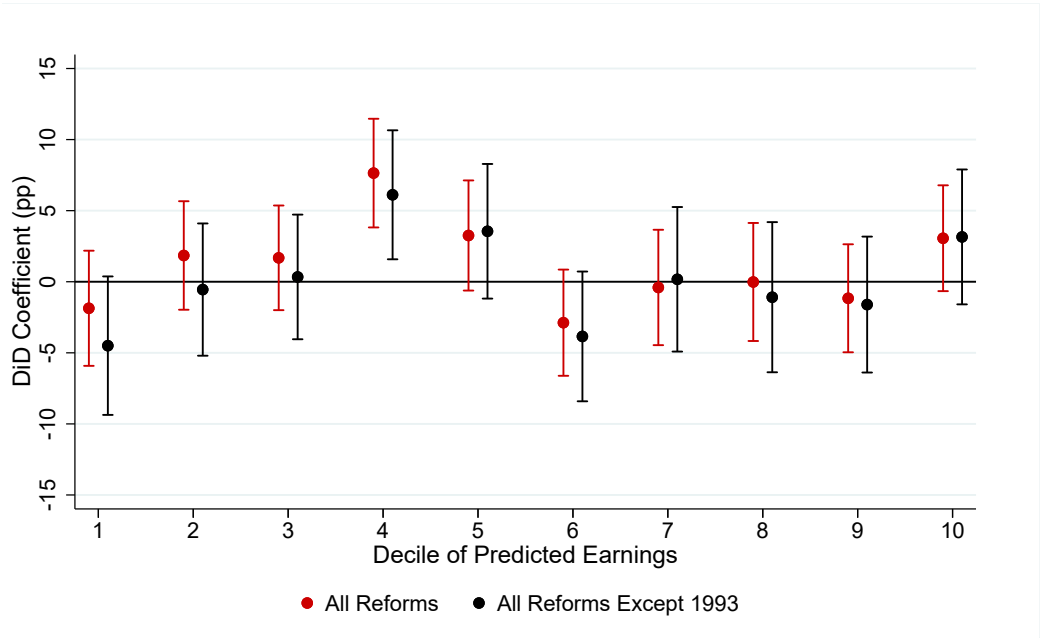
B: With Controls



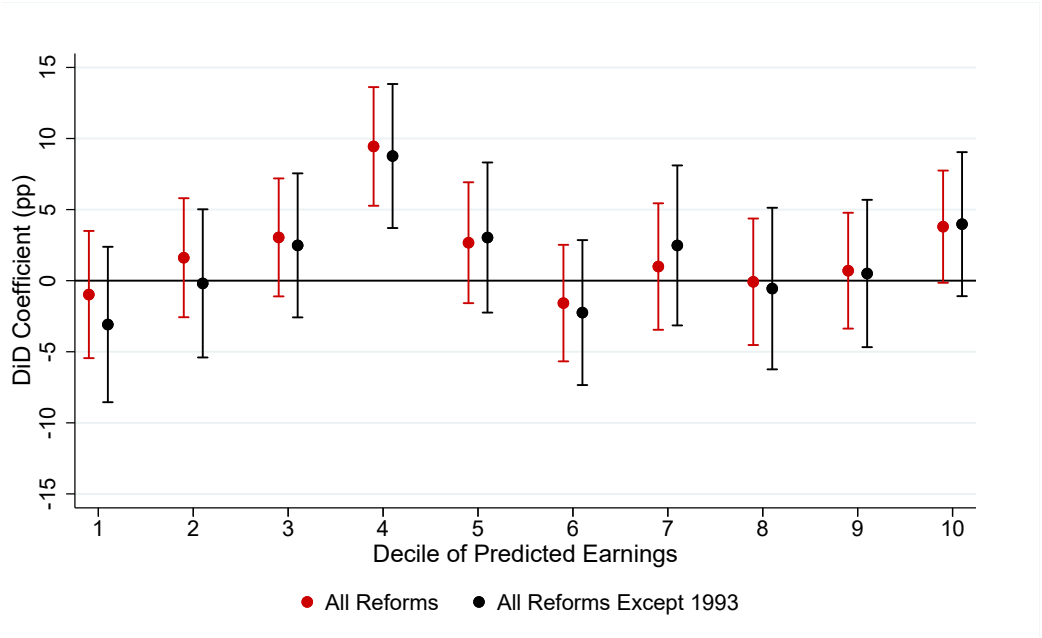
Notes: This figure is exactly the same as Figure 7 in the main text.

FIGURE A.14: EFFECTS OF FEDERAL REFORMS STACKED BY DECILE OF PREDICTED EARNINGS
ANNUAL EMPLOYMENT

A: Raw Data



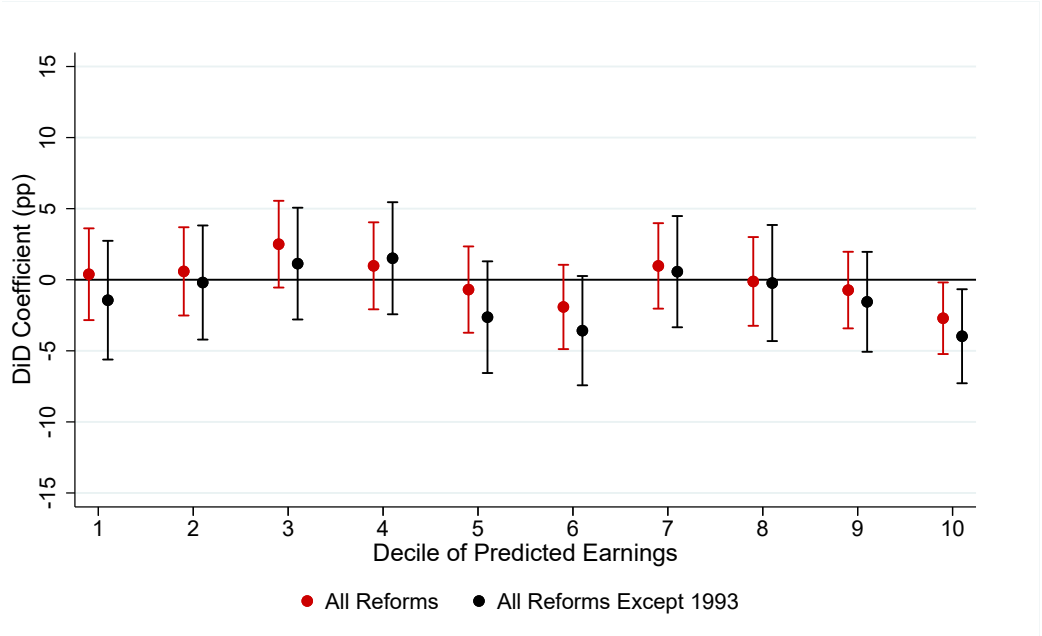
B: With Controls



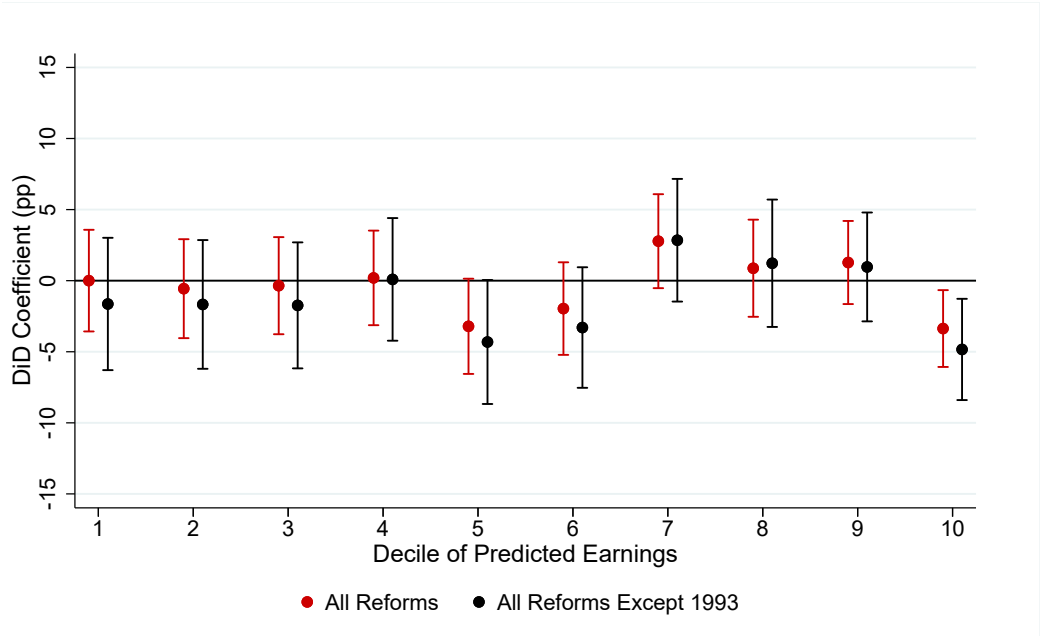
Notes: This figure is constructed exactly as Figure 7 in the main text, but using annual employment as the extensive margin measure.

FIGURE A.15: EFFECTS OF FEDERAL REFORMS STACKED BY DECILE OF PREDICTED EARNINGS
WEEKLY PARTICIPATION

A: Raw Data



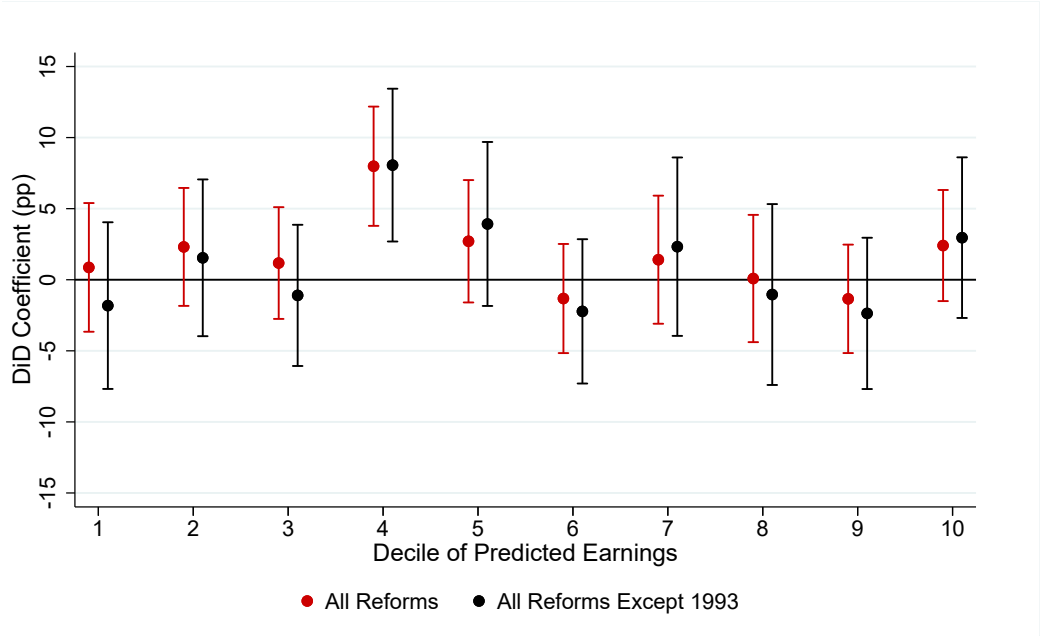
B: With Controls



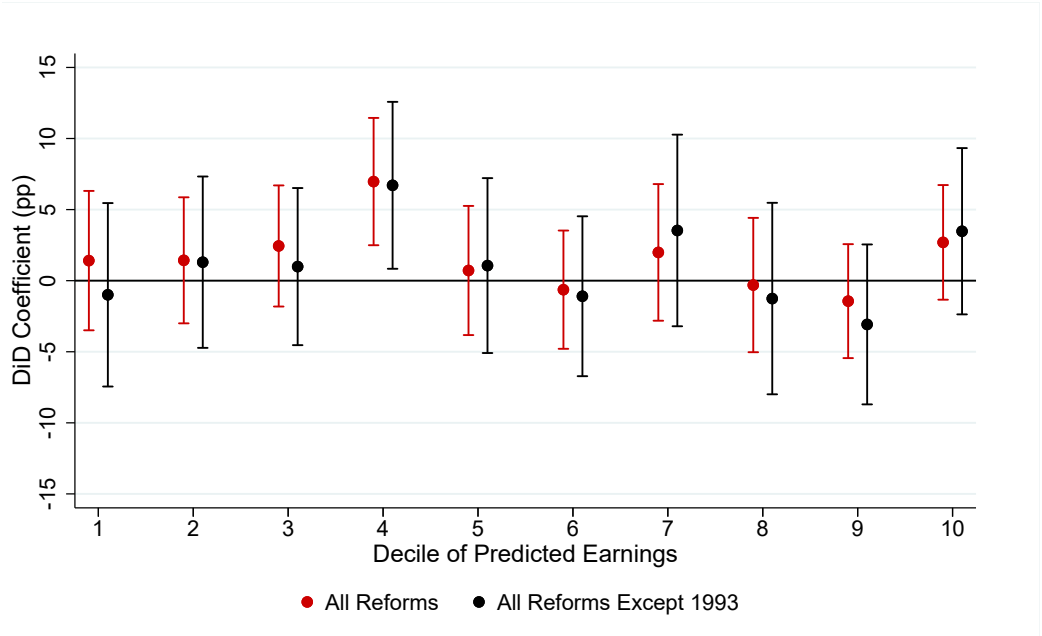
Notes: This figure is constructed exactly as Figure 7 in the main text, but using weekly participation as the extensive margin measure.

FIGURE A.16: EFFECTS OF FEDERAL REFORMS STACKED BY DECILE OF PREDICTED EARNINGS
ANNUAL PARTICIPATION

A: Raw Data

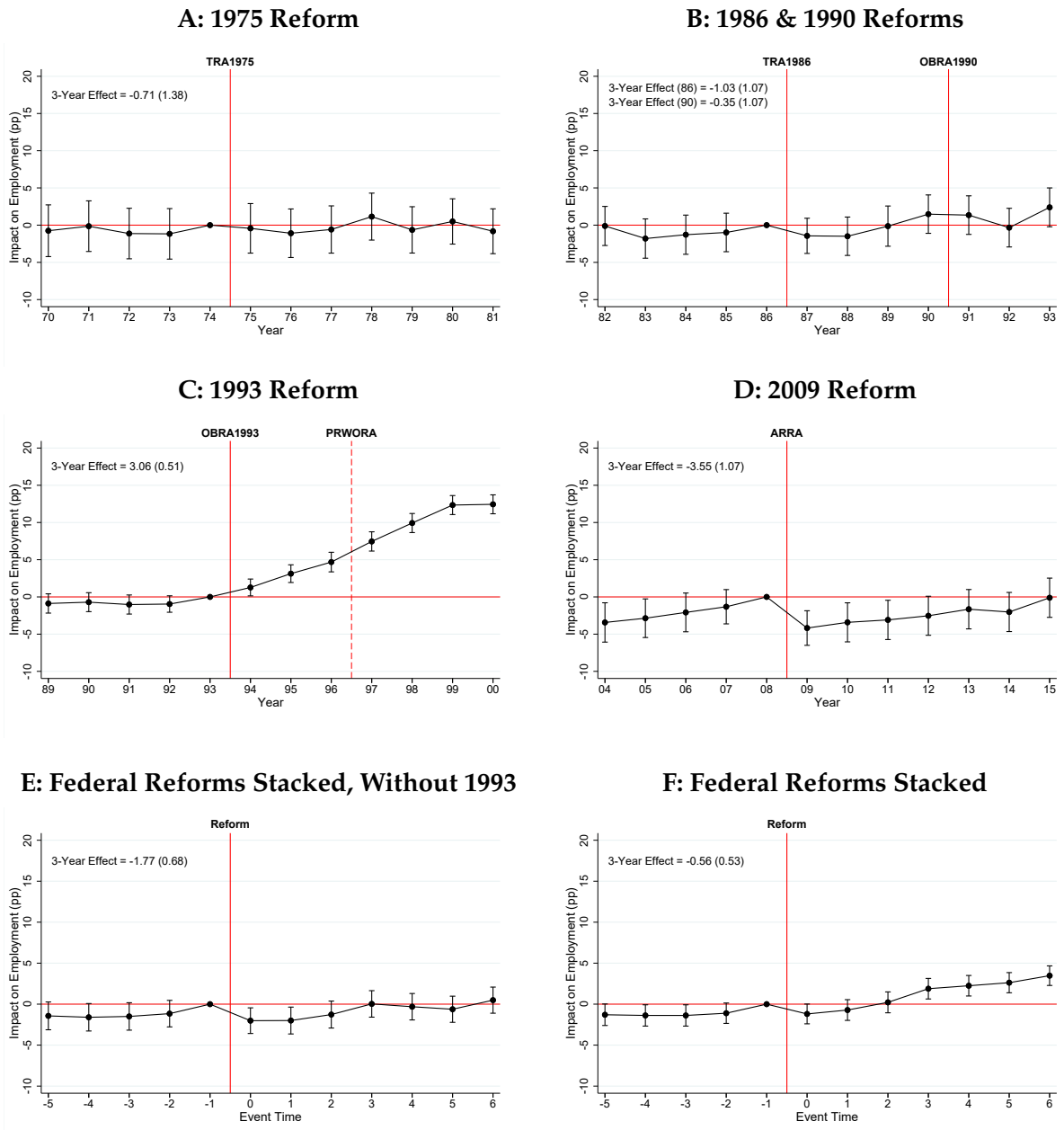


B: With Controls



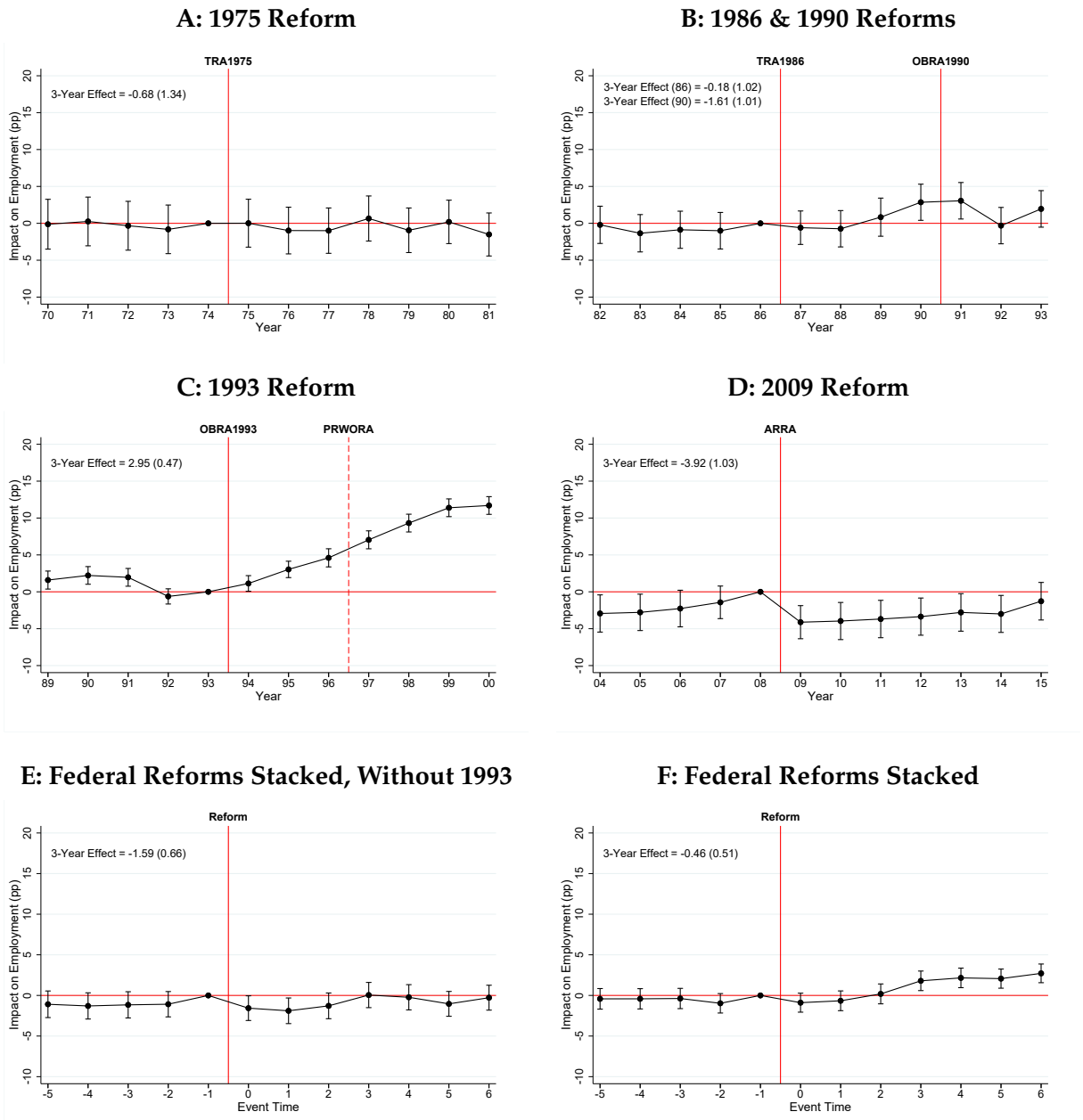
Notes: This figure is constructed exactly as Figure 7 in the main text, but using annual participation as the extensive margin measure.

FIGURE A.17: DID EVENT STUDIES OF FEDERAL EITC REFORMS
ALL SINGLE WOMEN, WEEKLY EMPLOYMENT
RAW DATA



Notes: This figure is constructed exactly as Figure 5 in the main text, except for the specification of control variables. The figure is based on specification (2) without any controls, the sample of all single women, and using weekly employment as the extensive margin measure.

FIGURE A.18: DID EVENT STUDIES OF FEDERAL EITC REFORMS
ALL SINGLE WOMEN, WEEKLY EMPLOYMENT
CONTROLS FOR BASIC DEMOGRAPHICS

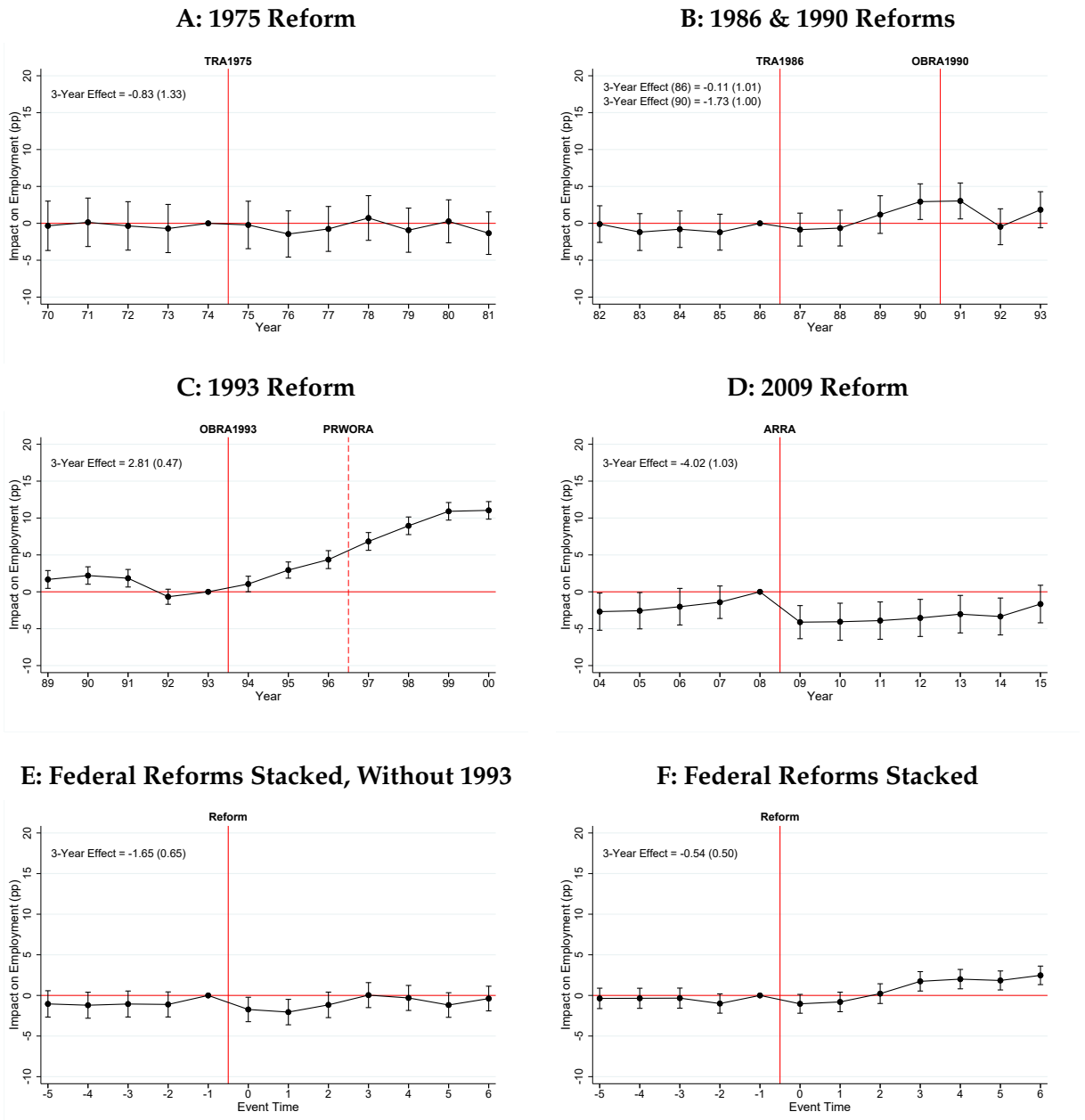


Notes: This figure is constructed exactly as Figure 5 in the main text, except for the specification of control variables. The figure is based on specification (2) with controls for basic demographics only, the sample of all single women, and using weekly employment as the extensive margin measure.

FIGURE A.19: DID EVENT STUDIES OF FEDERAL EITC REFORMS

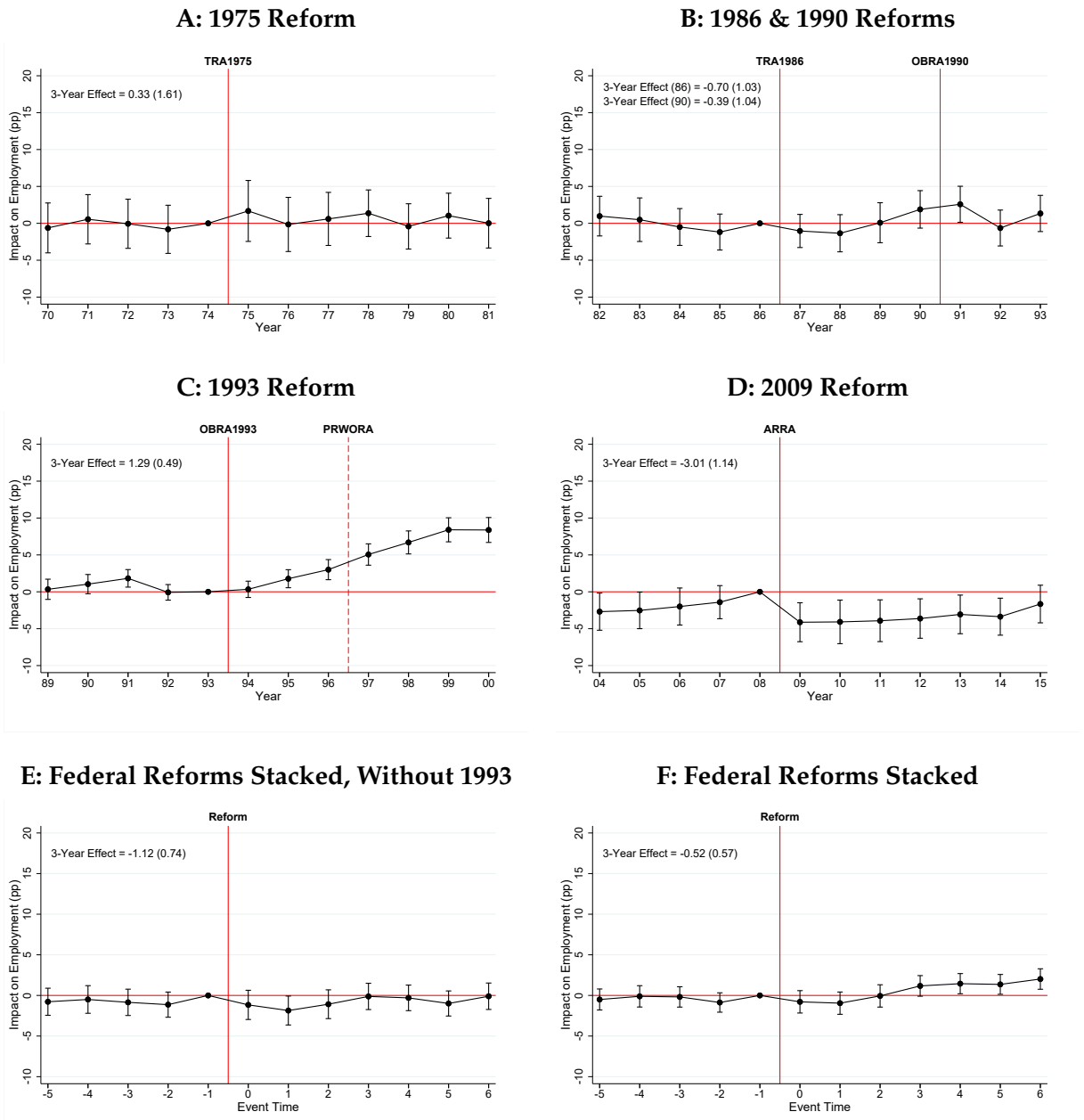
ALL SINGLE WOMEN, WEEKLY EMPLOYMENT

CONTROLS FOR RICH DEMOGRAPHICS



Notes: This figure is constructed exactly as Figure 5 in the main text, except for the specification of control variables. The figure is based on specification (2) with controls for rich demographics only, the sample of all single women, and using weekly employment as the extensive margin measure.

FIGURE A.20: DID EVENT STUDIES OF FEDERAL EITC REFORMS
ALL SINGLE WOMEN, WEEKLY EMPLOYMENT
CONTROLS FOR RICH DEMOGRAPHICS AND UNEMPLOYMENT

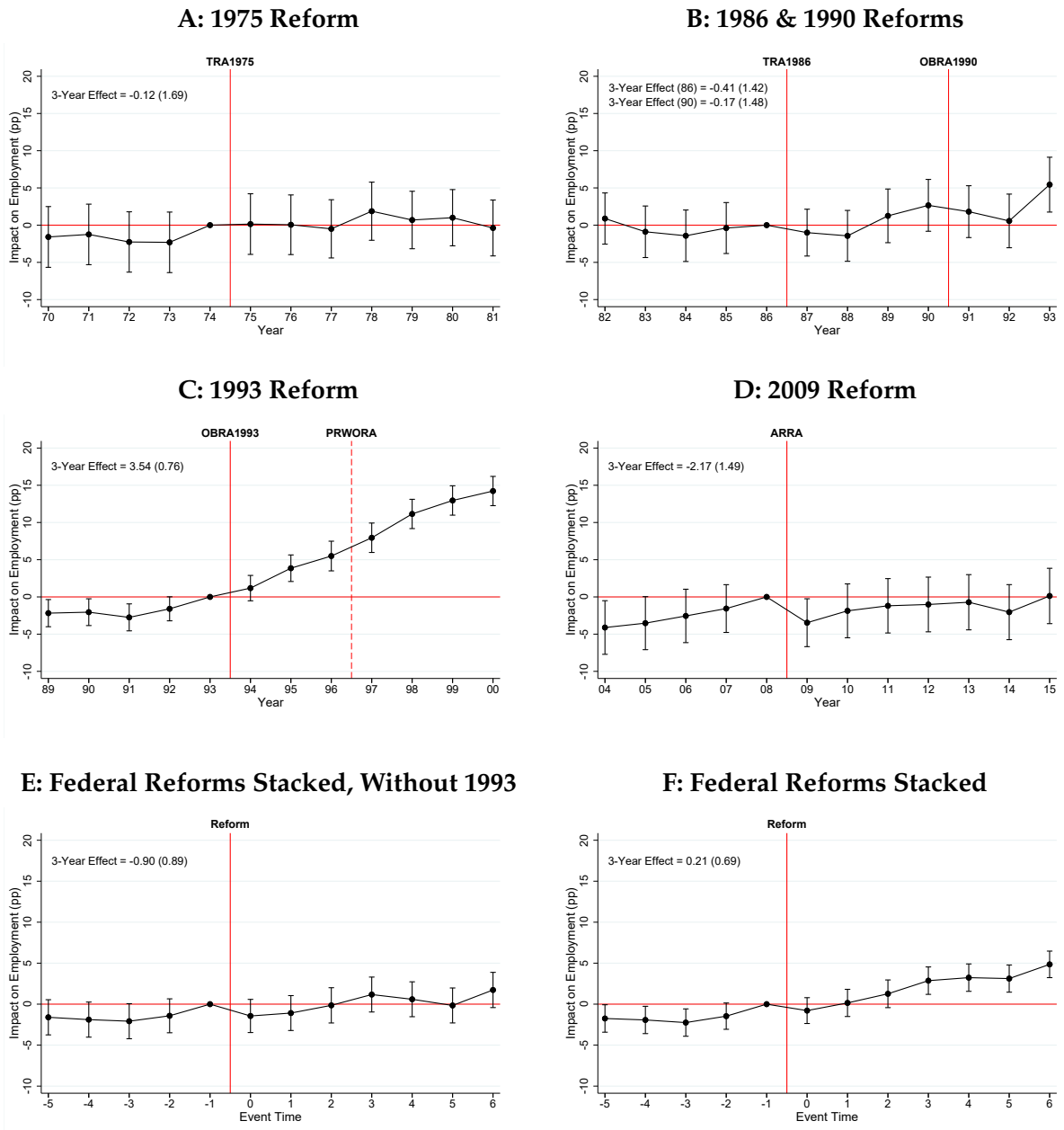


Notes: This figure is exactly the same as Figure 5 in the main text. The figure is based on specification (2) with controls for rich demographics and unemployment, the sample of all single women, and using weekly employment as the extensive margin measure.

FIGURE A.21: DID EVENT STUDIES OF FEDERAL EITC REFORMS

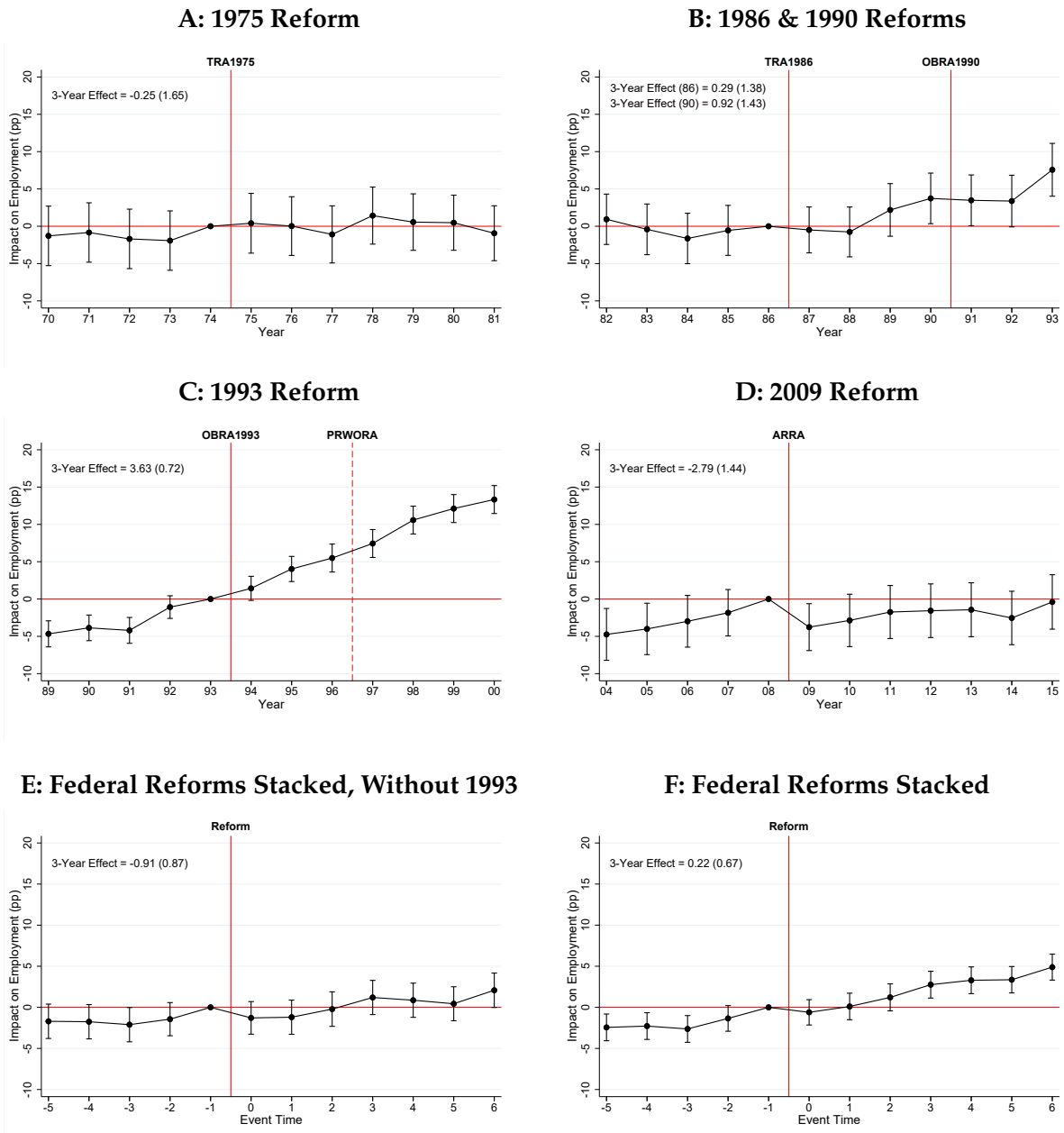
LOW-EDUCATED SINGLE WOMEN, WEEKLY EMPLOYMENT

RAW DATA



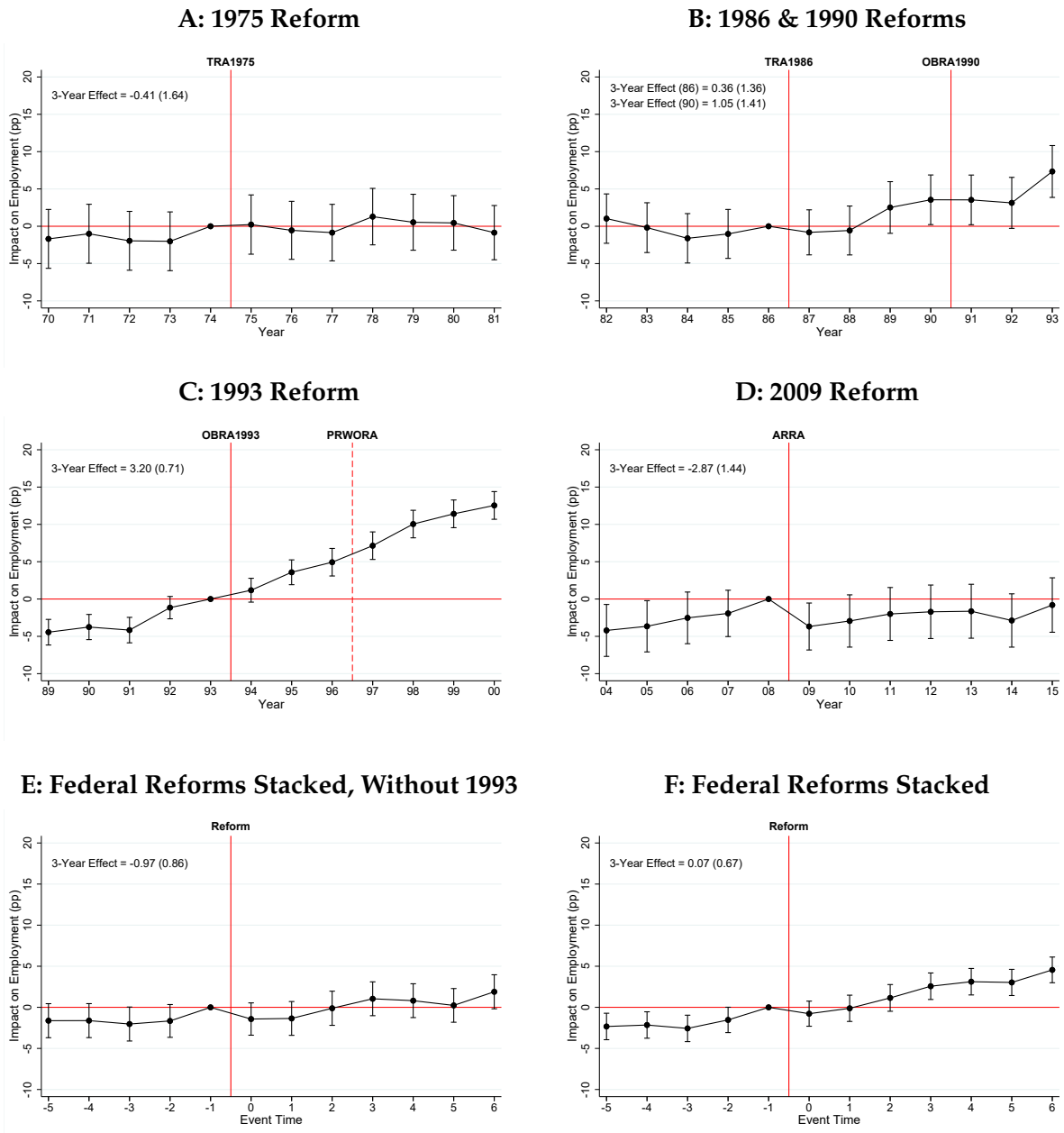
Notes: This figure is constructed exactly as Figure 5 in the main text, except for the specification of control variables and estimation sample. The figure is based on specification (2) without any controls, a sample of low-educated single women (those with a high school degree or less), and using weekly employment as the extensive margin measure.

FIGURE A.22: DID EVENT STUDIES OF FEDERAL EITC REFORMS
LOW-EDUCATED SINGLE WOMEN, WEEKLY EMPLOYMENT
CONTROLS FOR BASIC DEMOGRAPHICS



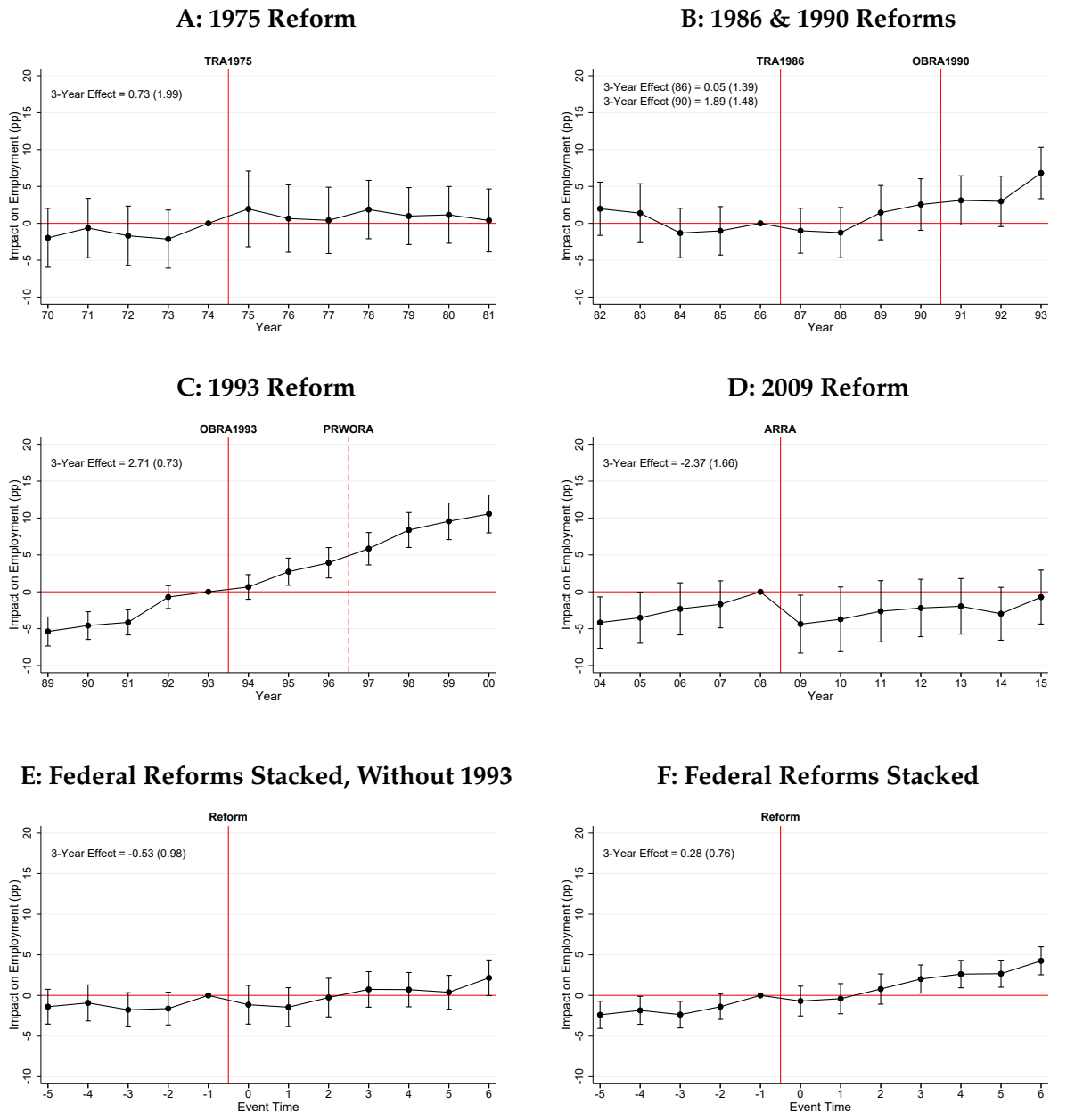
Notes: This figure is constructed exactly as Figure 5 in the main text, except for the specification of control variables and estimation sample. The figure is based on specification (2) with controls for basic demographics only, a sample of low-educated single women (those with a high school degree or less), and using weekly employment as the extensive margin measure.

FIGURE A.23: DID EVENT STUDIES OF FEDERAL EITC REFORMS
LOW-EDUCATED SINGLE WOMEN, WEEKLY EMPLOYMENT
CONTROLS FOR RICH DEMOGRAPHICS



Notes: This figure is constructed exactly as Figure 5 in the main text, except for the specification of control variables and estimation sample. The figure is based on specification (2) with controls for rich demographics only, a sample of low-educated single women (those with a high school degree or less), and using weekly employment as the extensive margin measure.

FIGURE A.24: DID EVENT STUDIES OF FEDERAL EITC REFORMS
LOW-EDUCATED SINGLE WOMEN, WEEKLY EMPLOYMENT
CONTROLS FOR RICH DEMOGRAPHICS AND UNEMPLOYMENT

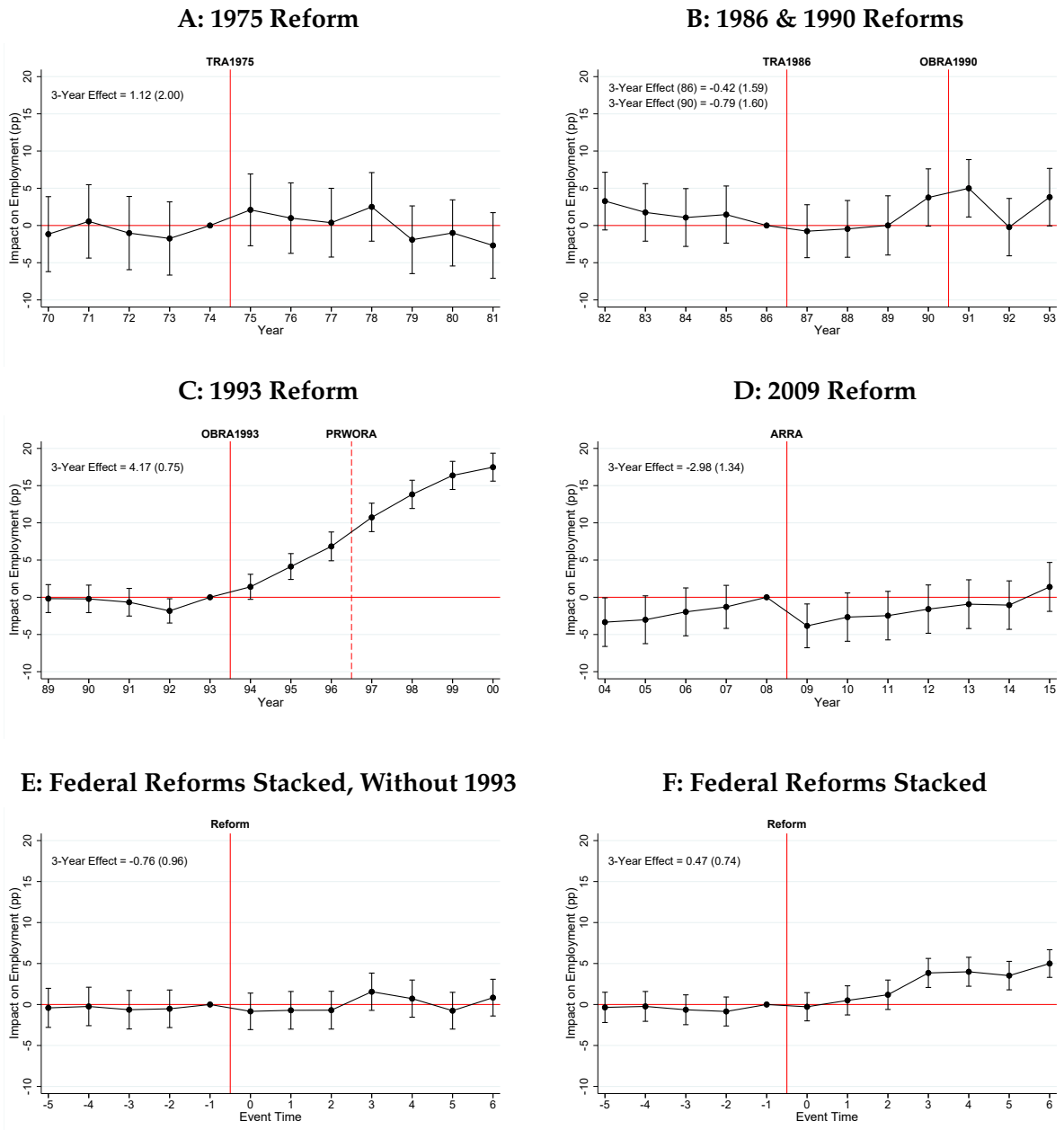


Notes: This figure is constructed exactly as Figure 5 in the main text, except for the estimation sample. The figure is based on specification (2) with controls for rich demographics and unemployment, a sample of low-educated single women (those with a high school degree or less), and using weekly employment as the extensive margin measure.

FIGURE A.25: DID EVENT STUDIES OF FEDERAL EITC REFORMS

SINGLE WOMEN IN BOTTOM HALF OF PREDICTED EARNINGS, WEEKLY EMPLOYMENT

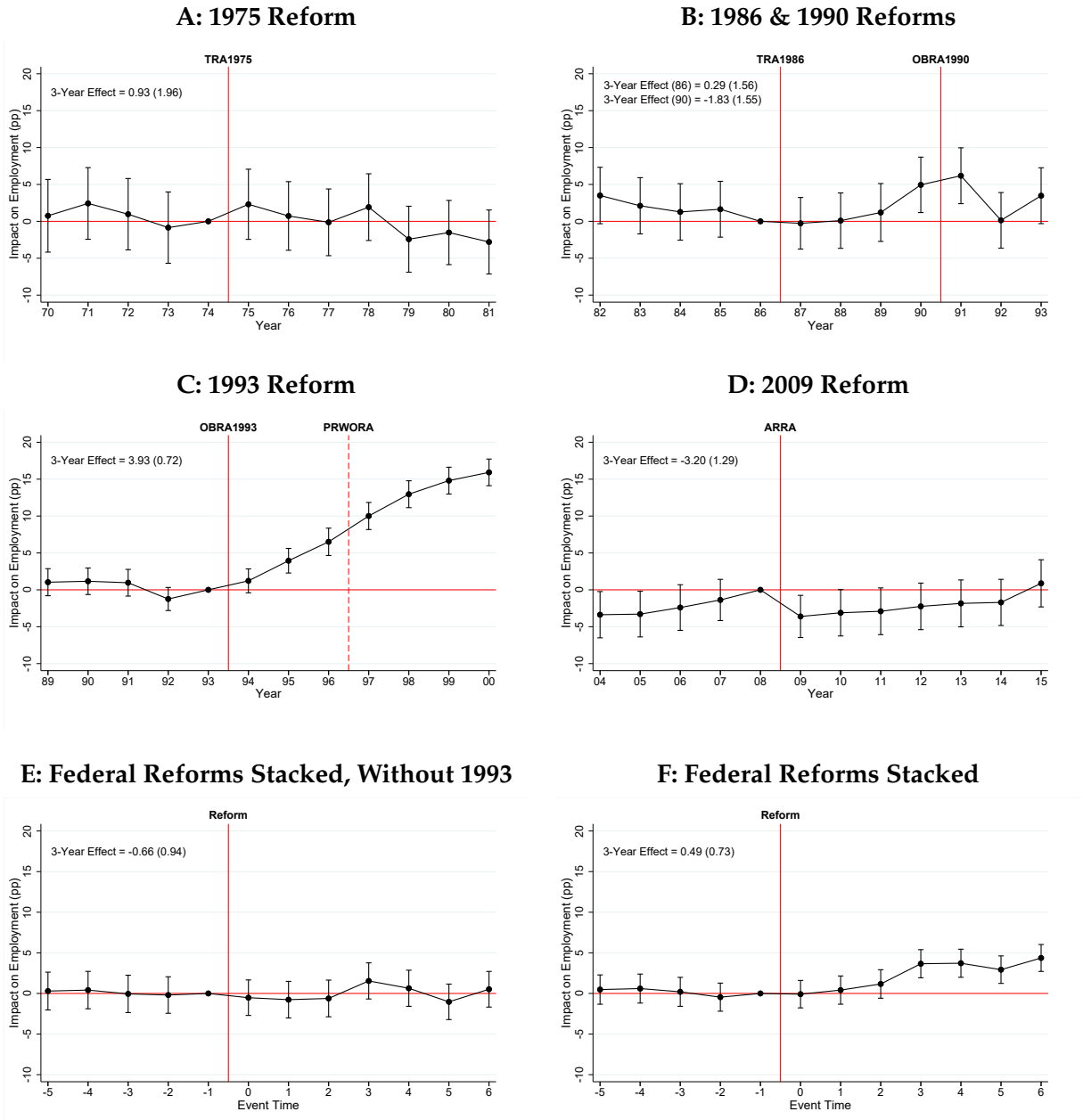
RAW DATA



Notes: This figure is constructed exactly as Figure 5 in the main text, except for the specification of control variables and the estimation sample. The figure is based on specification (2) without any controls, a sample of single women with low predicted earnings, and using weekly employment as the extensive margin measure. The low-earnings sample includes single women in the bottom half of the within-year distribution of predicted earnings, estimated using equation (1).

FIGURE A.26: DID EVENT STUDIES OF FEDERAL EITC REFORMS

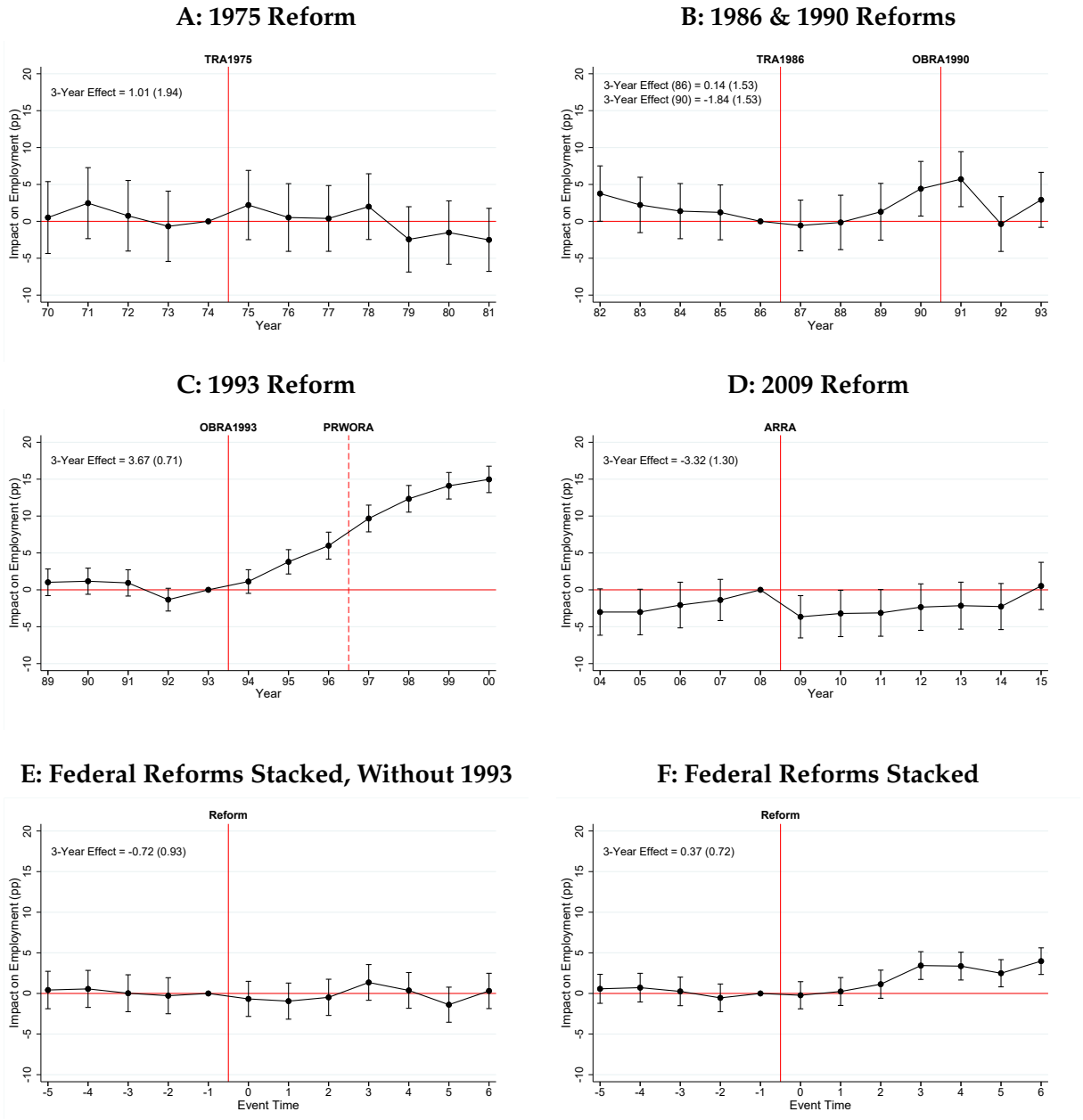
SINGLE WOMEN IN BOTTOM HALF OF PREDICTED EARNINGS, WEEKLY EMPLOYMENT
CONTROLS FOR BASIC DEMOGRAPHICS



Notes: This figure is constructed exactly as Figure 5 in the main text, except for the specification of control variables and the estimation sample. The figure is based on specification (2) with controls for basic demographics only, a sample of single women with low predicted earnings, and using weekly employment as the extensive margin measure. The low-earnings sample includes single women in the bottom half of the within-year distribution of predicted earnings, estimated using equation (1).

FIGURE A.27: DID EVENT STUDIES OF FEDERAL EITC REFORMS

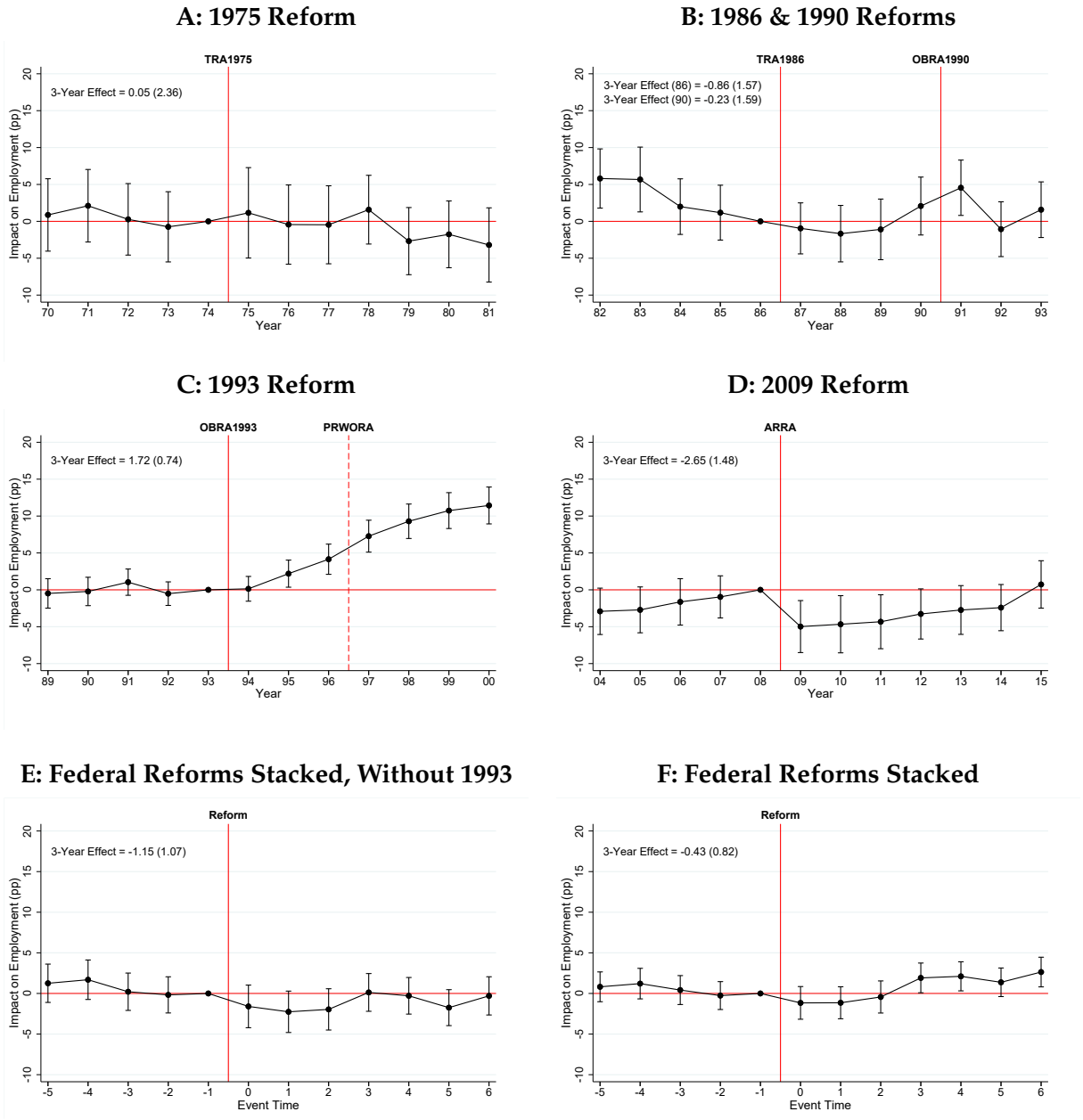
SINGLE WOMEN IN BOTTOM HALF OF PREDICTED EARNINGS, WEEKLY EMPLOYMENT
CONTROLS FOR RICH DEMOGRAPHICS



Notes: This figure is constructed exactly as Figure 5 in the main text, except for the specification of control variables and the estimation sample. The figure is based on specification (2) with controls for rich demographics only, a sample of single women with low predicted earnings, and using weekly employment as the extensive margin measure. The low-earnings sample includes single women in the bottom half of the within-year distribution of predicted earnings, estimated using equation (1).

FIGURE A.28: DID EVENT STUDIES OF FEDERAL EITC REFORMS

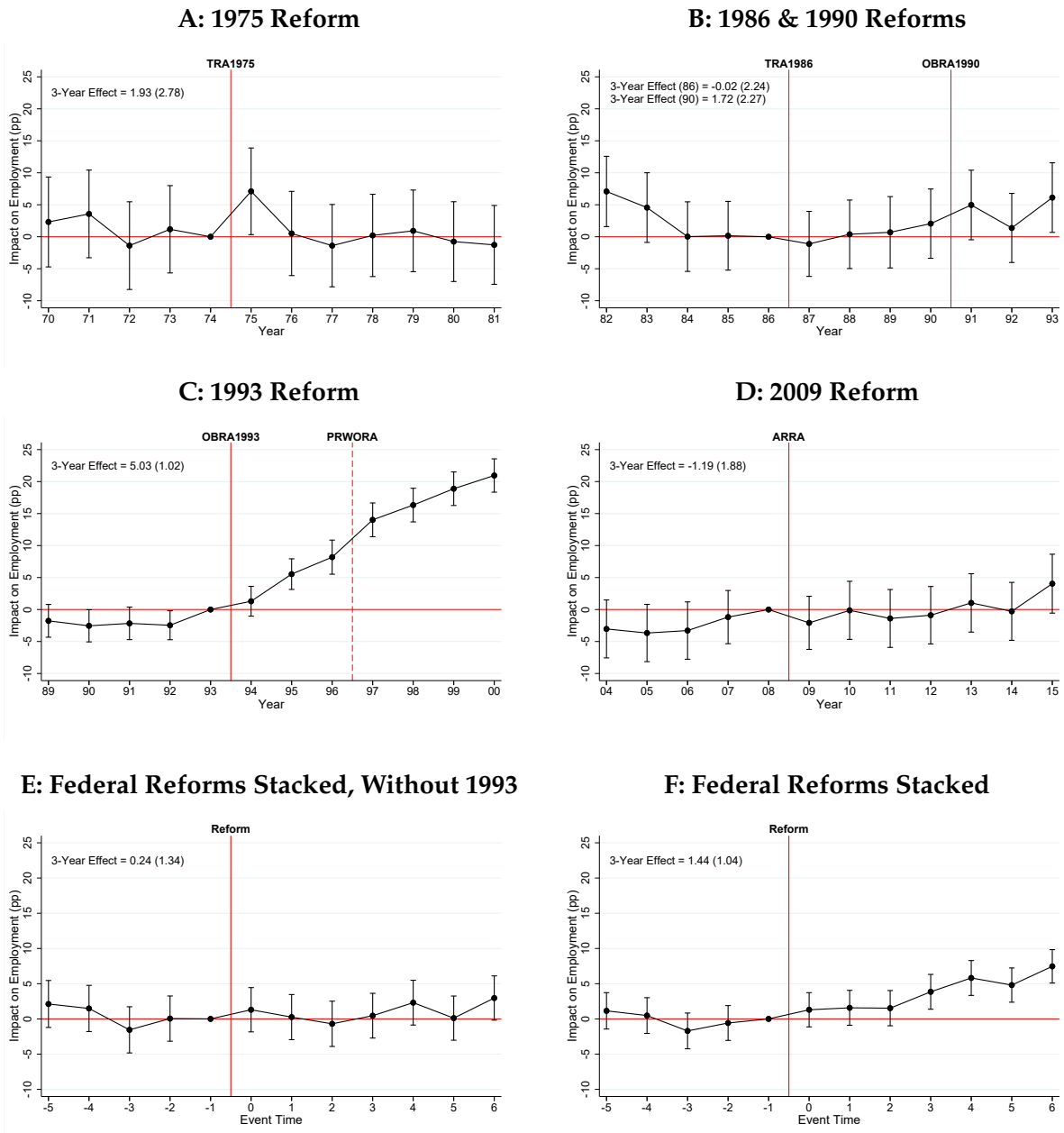
SINGLE WOMEN IN BOTTOM HALF OF PREDICTED EARNINGS, WEEKLY EMPLOYMENT
CONTROLS FOR RICH DEMOGRAPHICS AND UNEMPLOYMENT



Notes: This figure is constructed exactly as Figure 5 in the main text, except for the estimation sample. The figure is based on specification (2) with controls for rich demographics and unemployment, a sample of single women with low predicted earnings, and using weekly employment as the extensive margin measure. The low-earnings sample includes single women in the bottom half of the within-year distribution of predicted earnings, estimated using equation (1). This figure also corresponds to Figure 6 in the main text.

FIGURE A.29: DID EVENT STUDIES OF FEDERAL EITC REFORMS

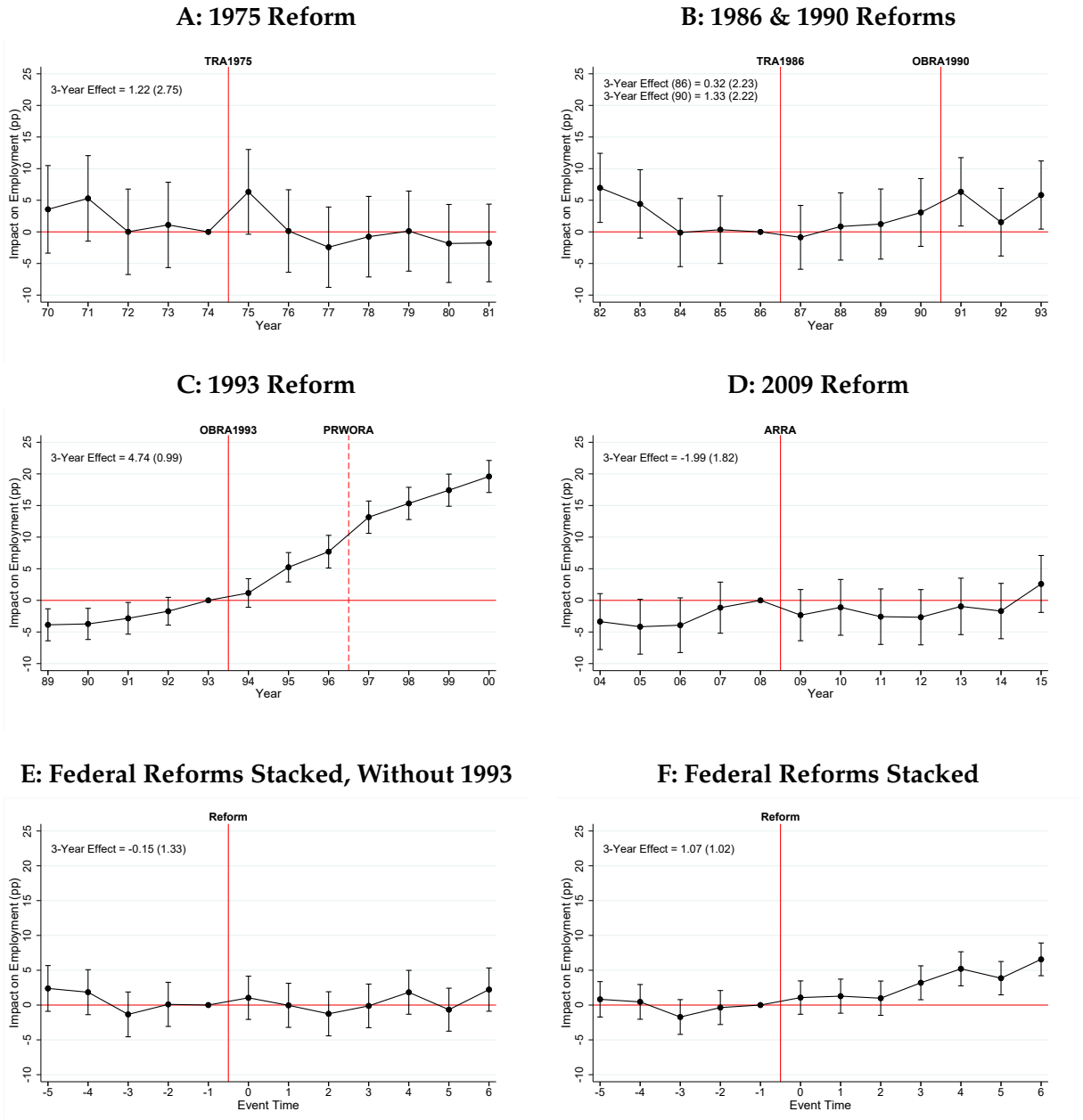
SINGLE WOMEN IN BOTTOM QUARTILE OF PREDICTED EARNINGS, WEEKLY EMPLOYMENT
RAW DATA



Notes: This figure is constructed exactly as Figure 5 in the main text, except for the specification of control variables and the estimation sample. The figure is based on specification (2) without any controls, a sample of single women with low predicted earnings, and using weekly employment as the extensive margin measure. The low-earnings sample includes single women in the bottom quartile of the within-year distribution of predicted earnings, estimated using equation (1).

FIGURE A.30: DID EVENT STUDIES OF FEDERAL EITC REFORMS

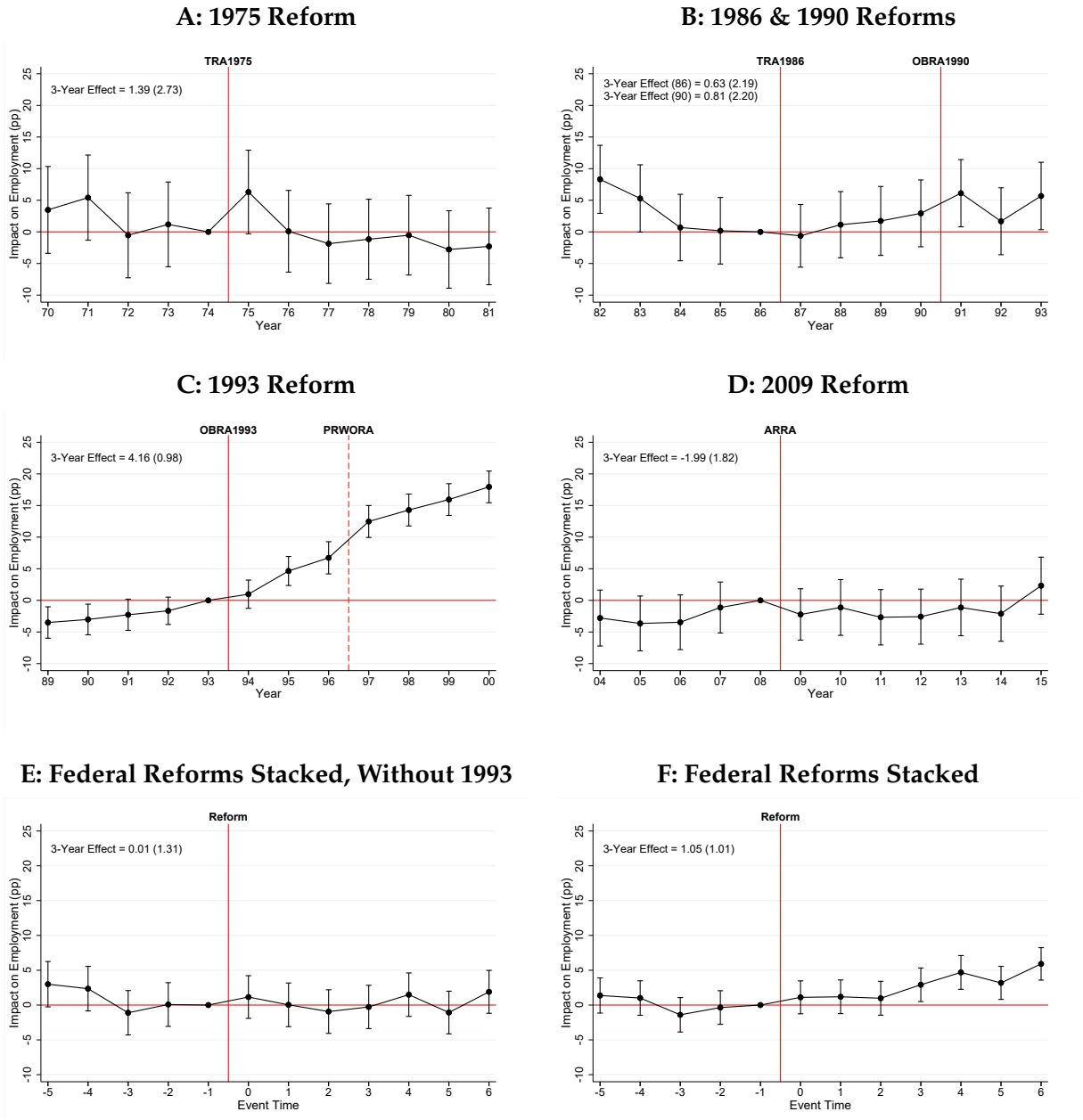
SINGLE WOMEN IN BOTTOM QUARTILE OF PREDICTED EARNINGS, WEEKLY EMPLOYMENT
CONTROLS FOR BASIC DEMOGRAPHICS



Notes: This figure is constructed exactly as Figure 5 in the main text, except for the specification of control variables and the estimation sample. The figure is based on specification (2) with controls for basic demographics only, a sample of single women with low predicted earnings, and using weekly employment as the extensive margin measure. The low-earnings sample includes single women in the bottom quartile of the within-year distribution of predicted earnings, estimated using equation (1).

FIGURE A.31: DID EVENT STUDIES OF FEDERAL EITC REFORMS

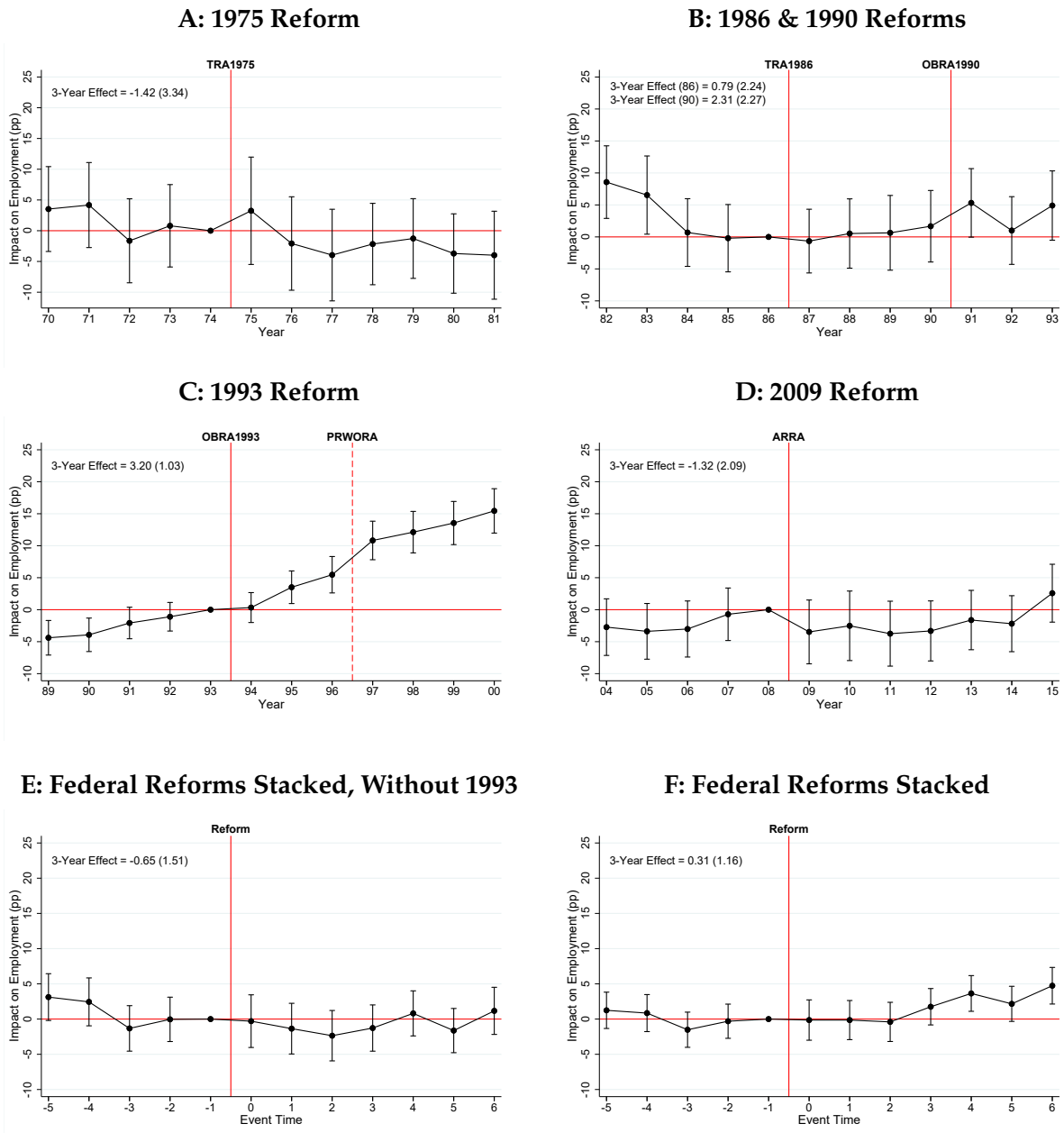
SINGLE WOMEN IN BOTTOM QUARTILE OF PREDICTED EARNINGS, WEEKLY EMPLOYMENT
CONTROLS FOR RICH DEMOGRAPHICS



Notes: This figure is constructed exactly as Figure 5 in the main text, except for the specification of control variables and the estimation sample. The figure is based on specification (2) with controls for rich demographics only, a sample of single women with low predicted earnings, and using weekly employment as the extensive margin measure. The low-earnings sample includes single women in the bottom quartile of the within-year distribution of predicted earnings, estimated using equation (1).

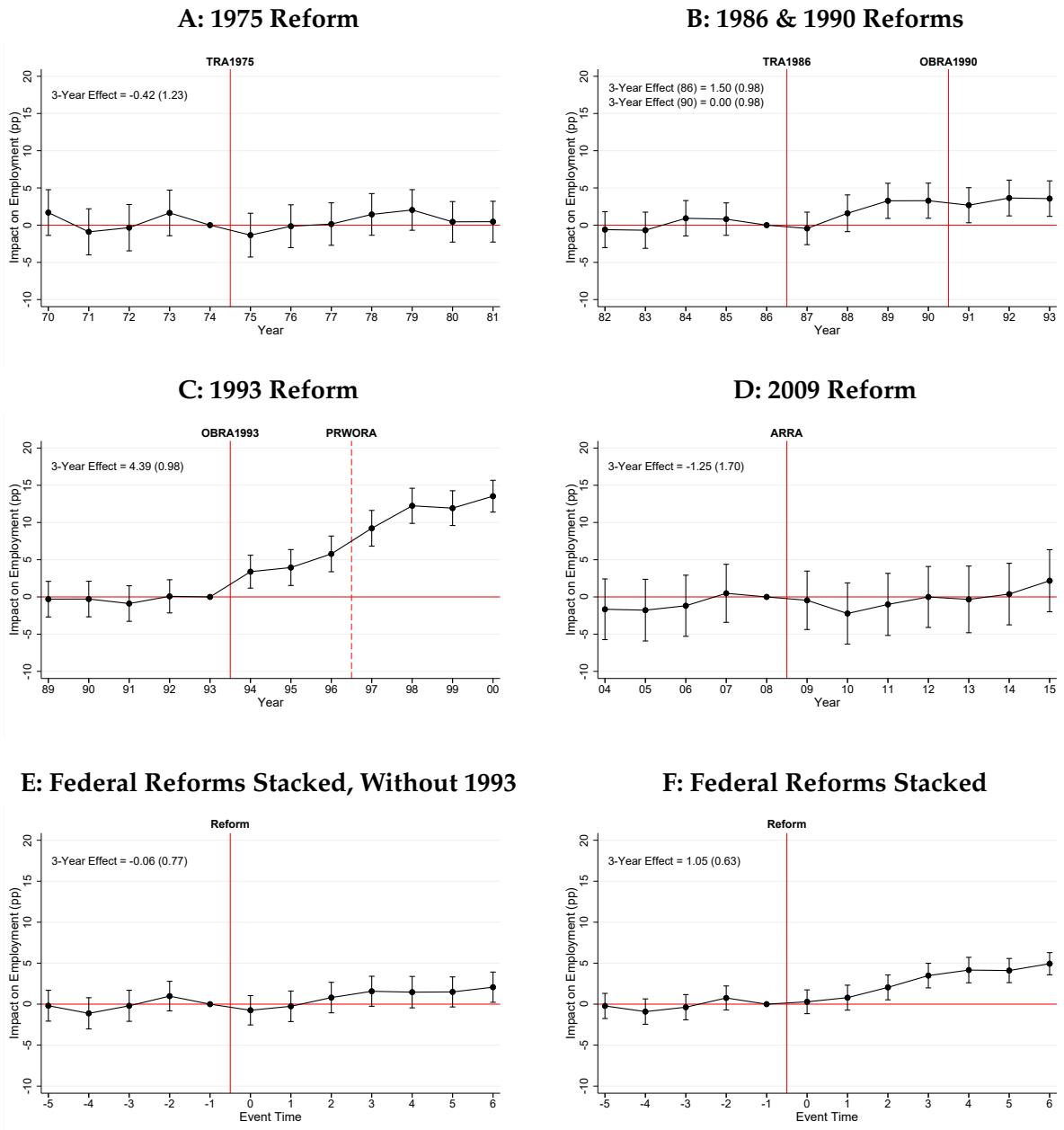
FIGURE A.32: DID EVENT STUDIES OF FEDERAL EITC REFORMS

SINGLE WOMEN IN BOTTOM QUARTILE OF PREDICTED EARNINGS, WEEKLY EMPLOYMENT
CONTROLS FOR RICH DEMOGRAPHICS AND UNEMPLOYMENT



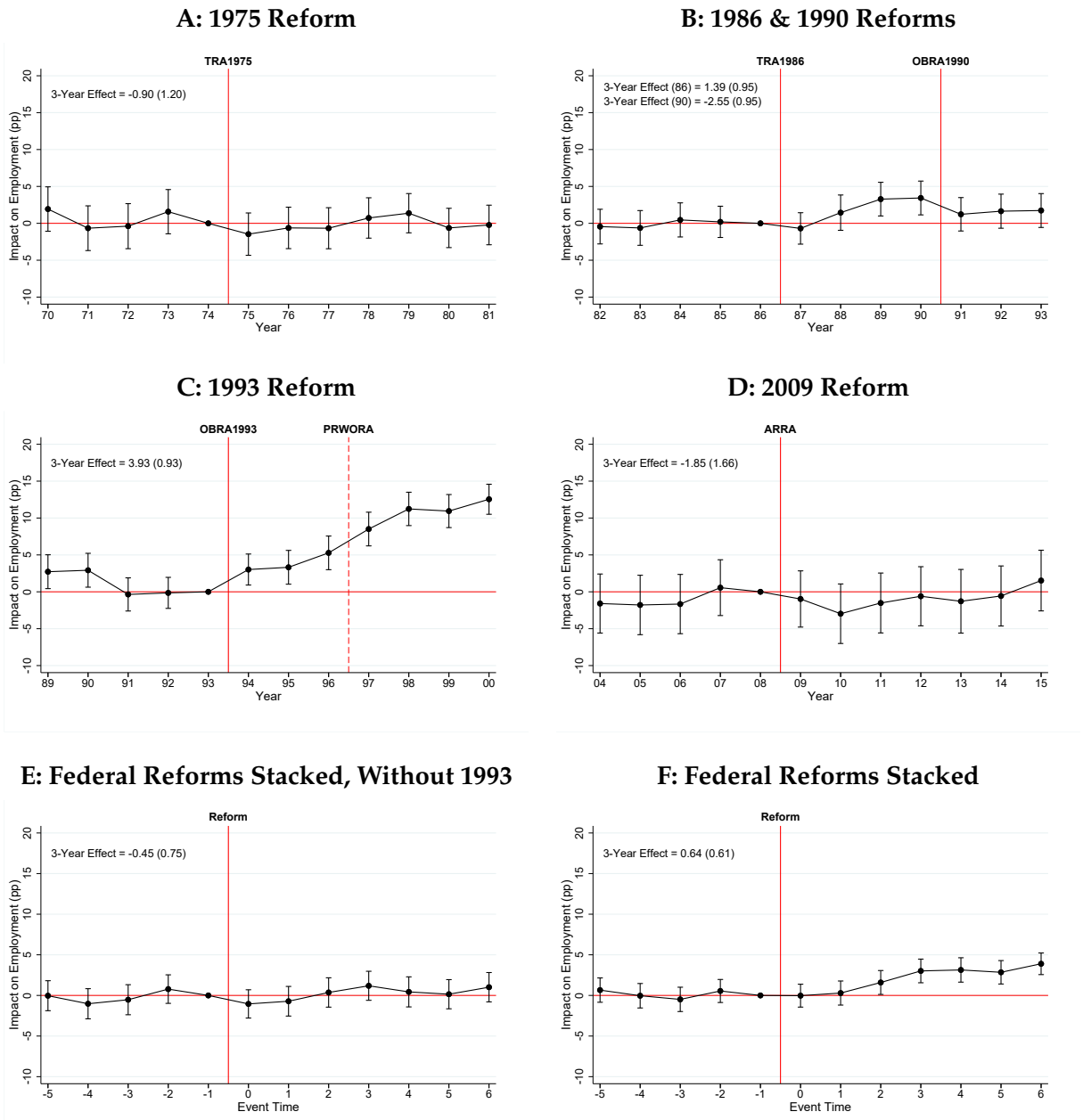
Notes: This figure is constructed exactly as Figure 5 in the main text, except for the estimation sample. The figure is based on specification (2) with controls for rich demographics and unemployment, a sample of single women with low predicted earnings, and using weekly employment as the extensive margin measure. The low-earnings sample includes single women in the bottom quartile of the within-year distribution of predicted earnings, estimated using equation (1).

FIGURE A.33: DID EVENT STUDIES OF FEDERAL EITC REFORMS
ALL SINGLE WOMEN, ANNUAL EMPLOYMENT
RAW DATA



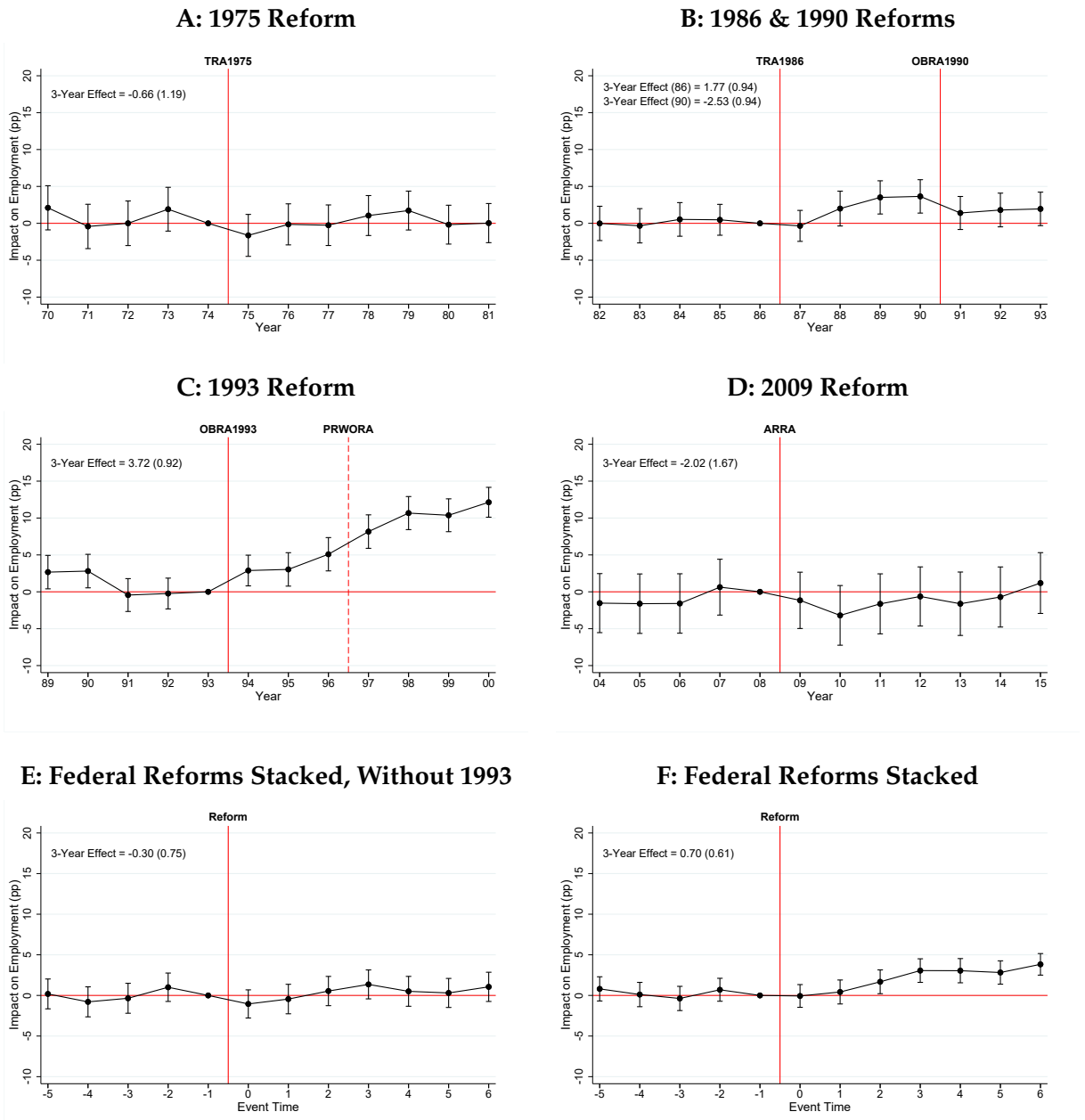
Notes: This figure is constructed exactly as Figure 5 in the main text, except for the specification of control variables and the extensive margin measure. The figure is based on specification (2) without any controls, the sample of all single women, and using annual employment as the extensive margin measure.

FIGURE A.34: DID EVENT STUDIES OF FEDERAL EITC REFORMS
ALL SINGLE WOMEN, ANNUAL EMPLOYMENT
CONTROLS FOR BASIC DEMOGRAPHICS



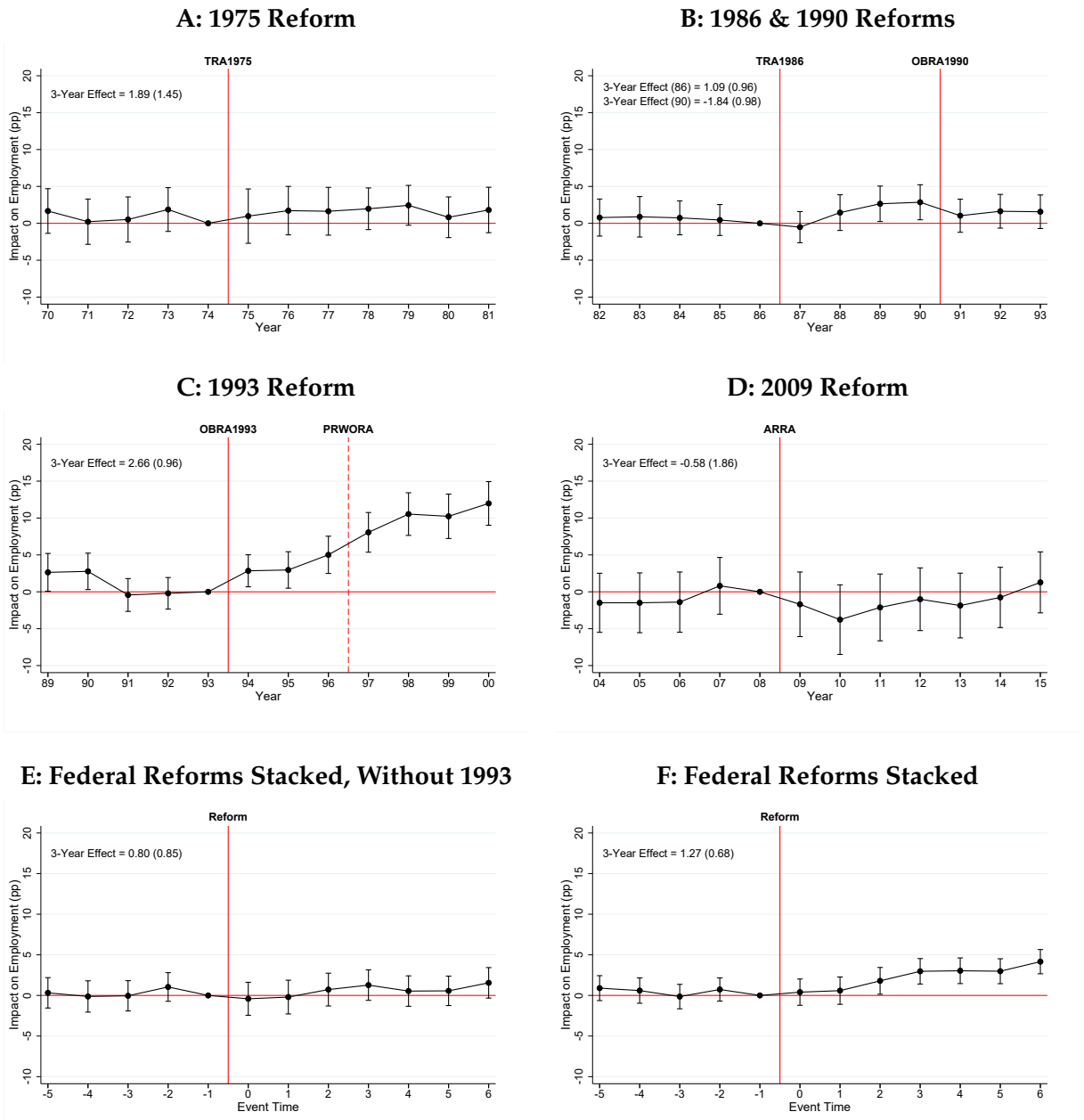
Notes: This figure is constructed exactly as Figure 5 in the main text, except for the specification of control variables and the extensive margin measure. The figure is based on specification (2) with controls for basic demographics only, the sample of all single women, and using annual employment as the extensive margin measure.

FIGURE A.35: DID EVENT STUDIES OF FEDERAL EITC REFORMS
ALL SINGLE WOMEN, ANNUAL EMPLOYMENT
CONTROLS FOR RICH DEMOGRAPHICS



Notes: This figure is constructed exactly as Figure 5 in the main text, except for the specification of control variables and the extensive margin measure. The figure is based on specification (2) with controls for rich demographics only, the sample of all single women, and using annual employment as the extensive margin measure.

FIGURE A.36: DID EVENT STUDIES OF FEDERAL EITC REFORMS
ALL SINGLE WOMEN, ANNUAL EMPLOYMENT
CONTROLS FOR RICH DEMOGRAPHICS AND UNEMPLOYMENT

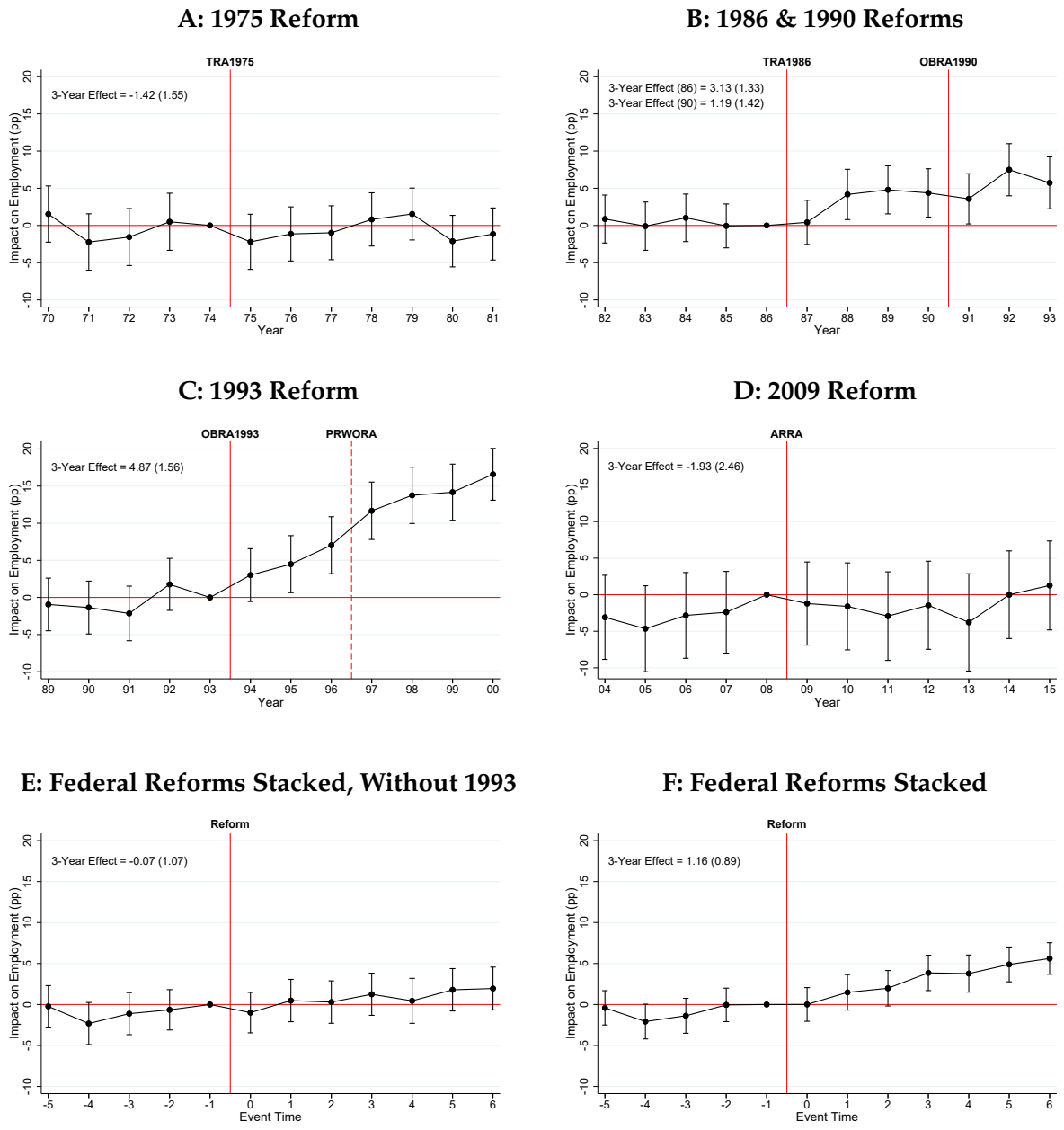


Notes: This figure is constructed exactly as Figure 5 in the main text, except for the extensive margin measure. The figure is based on specification (2) with controls for rich demographics and unemployment, the sample of all single women, and using annual employment as the extensive margin measure.

FIGURE A.37: DID EVENT STUDIES OF FEDERAL EITC REFORMS

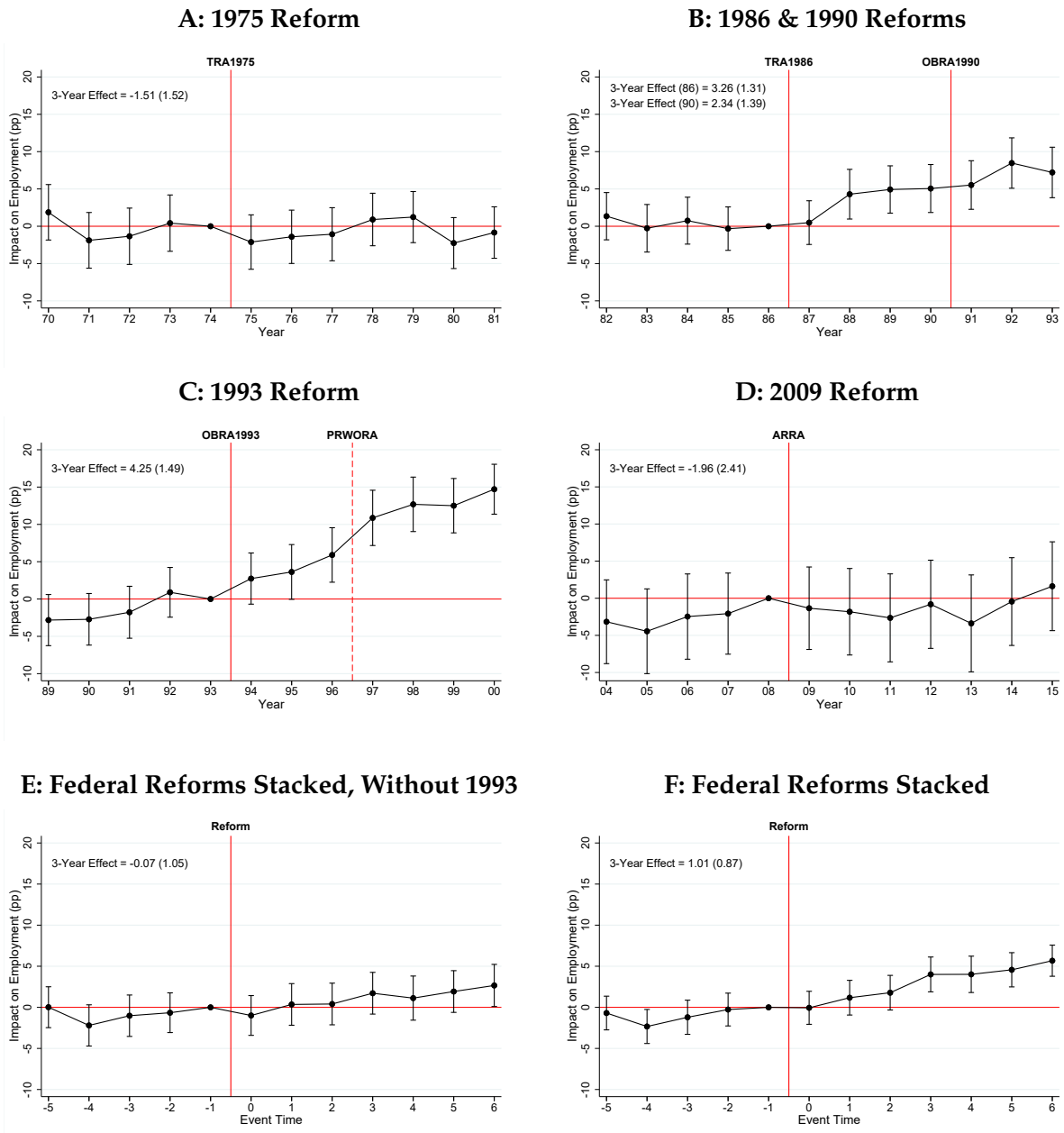
LOW-EDUCATED SINGLE WOMEN, ANNUAL EMPLOYMENT

RAW DATA



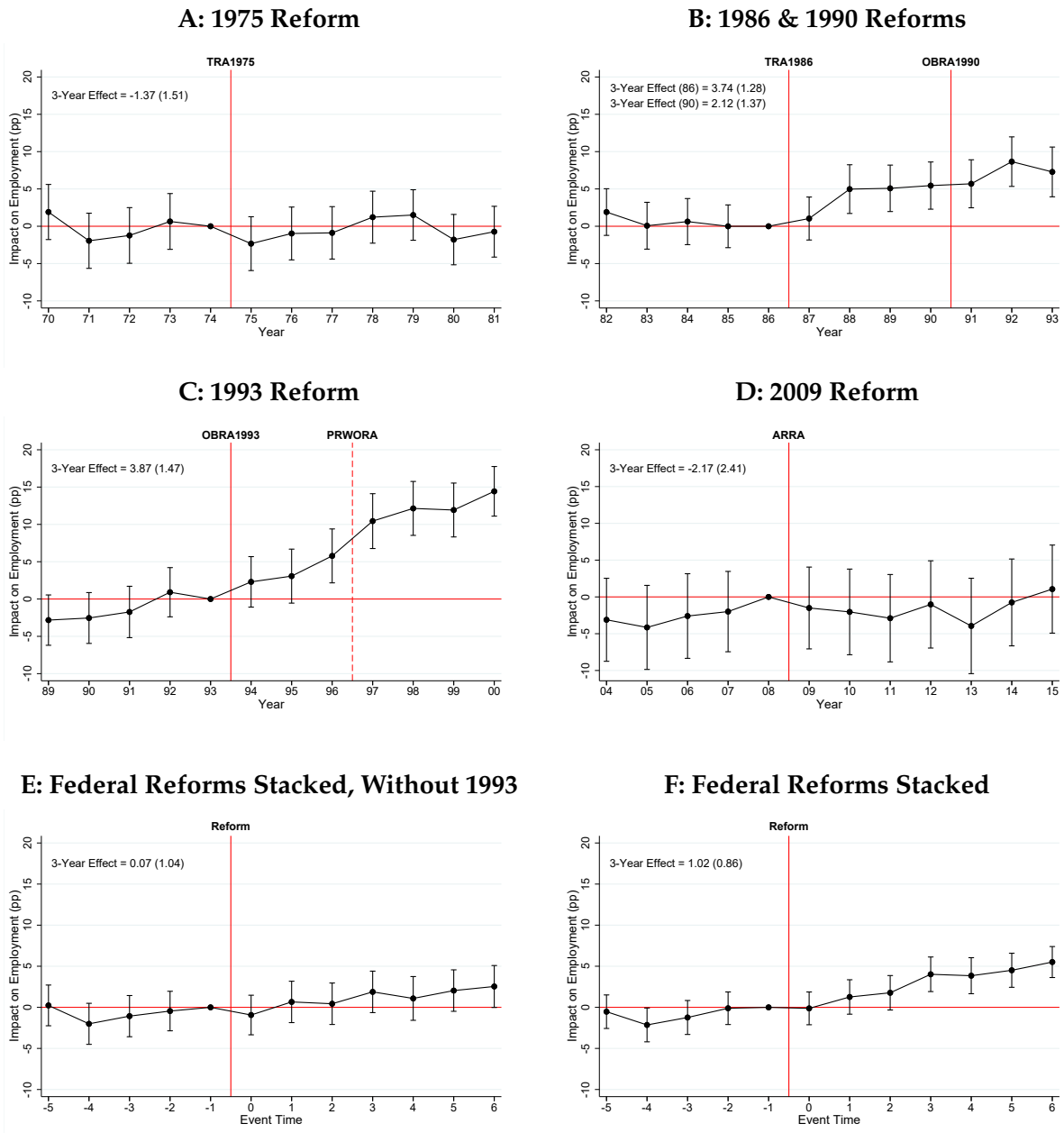
Notes: This figure is constructed exactly as Figure 5 in the main text, except for the specification of control variables, estimation sample, and the extensive margin measure. The figure is based on specification (2) without any controls, a sample of low-educated single women (those with a high school degree or less), and using annual employment as the extensive margin measure.

FIGURE A.38: DID EVENT STUDIES OF FEDERAL EITC REFORMS
LOW-EDUCATED SINGLE WOMEN, ANNUAL EMPLOYMENT
CONTROLS FOR BASIC DEMOGRAPHICS



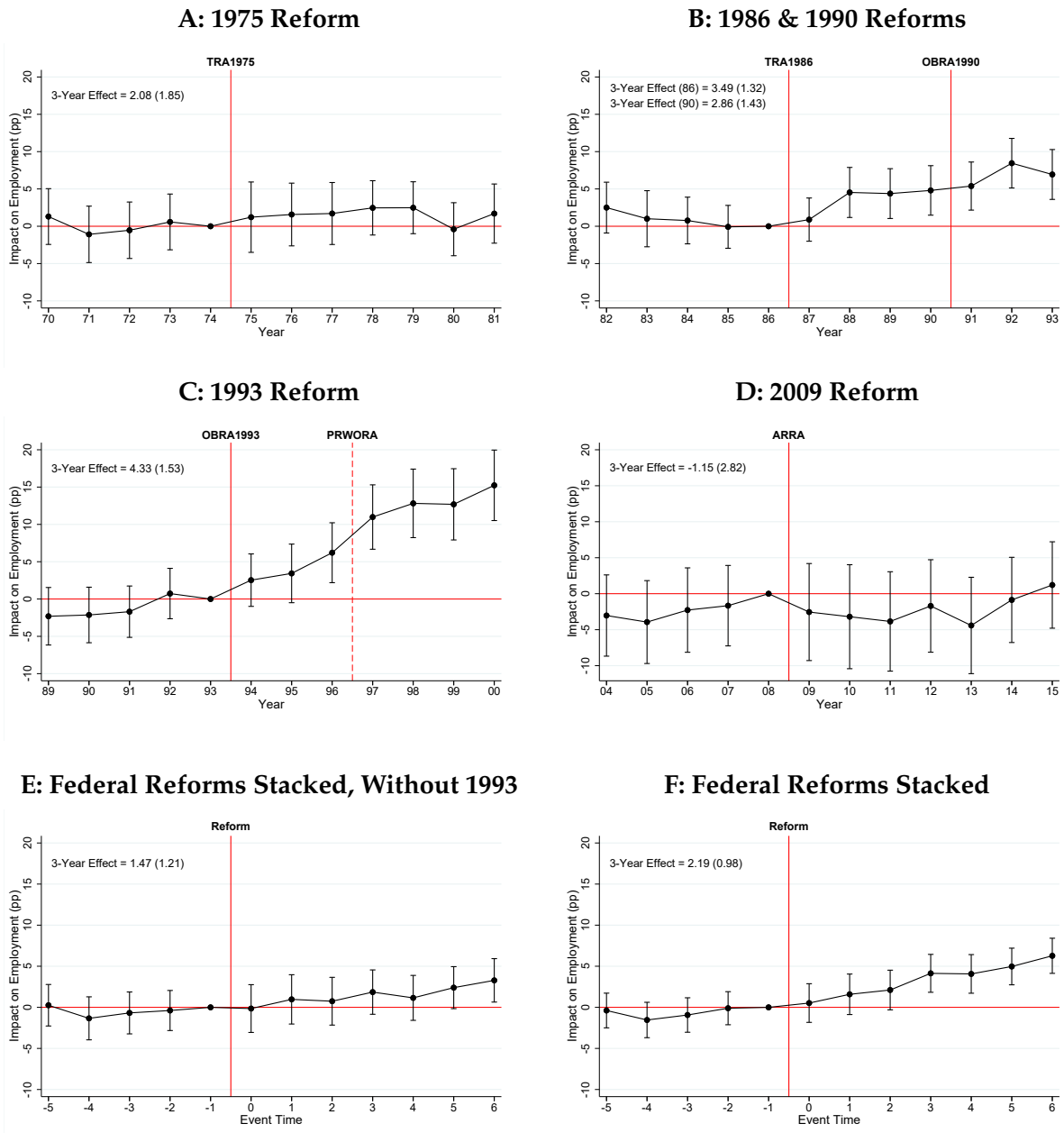
Notes: This figure is constructed exactly as Figure 5 in the main text, except for the specification of control variables, estimation sample, and the extensive margin measure. The figure is based on specification (2) with controls for basic demographics only, a sample of low-educated single women (those with a high school degree or less), and using annual employment as the extensive margin measure.

FIGURE A.39: DID EVENT STUDIES OF FEDERAL EITC REFORMS
LOW-EDUCATED SINGLE WOMEN, ANNUAL EMPLOYMENT
CONTROLS FOR RICH DEMOGRAPHICS



Notes: This figure is constructed exactly as Figure 5 in the main text, except for the specification of control variables, estimation sample, and the extensive margin measure. The figure is based on specification (2) with controls for rich demographics only, a sample of low-educated single women (those with a high school degree or less), and using annual employment as the extensive margin measure.

FIGURE A.40: DID EVENT STUDIES OF FEDERAL EITC REFORMS
LOW-EDUCATED SINGLE WOMEN, ANNUAL EMPLOYMENT
CONTROLS FOR RICH DEMOGRAPHICS AND UNEMPLOYMENT

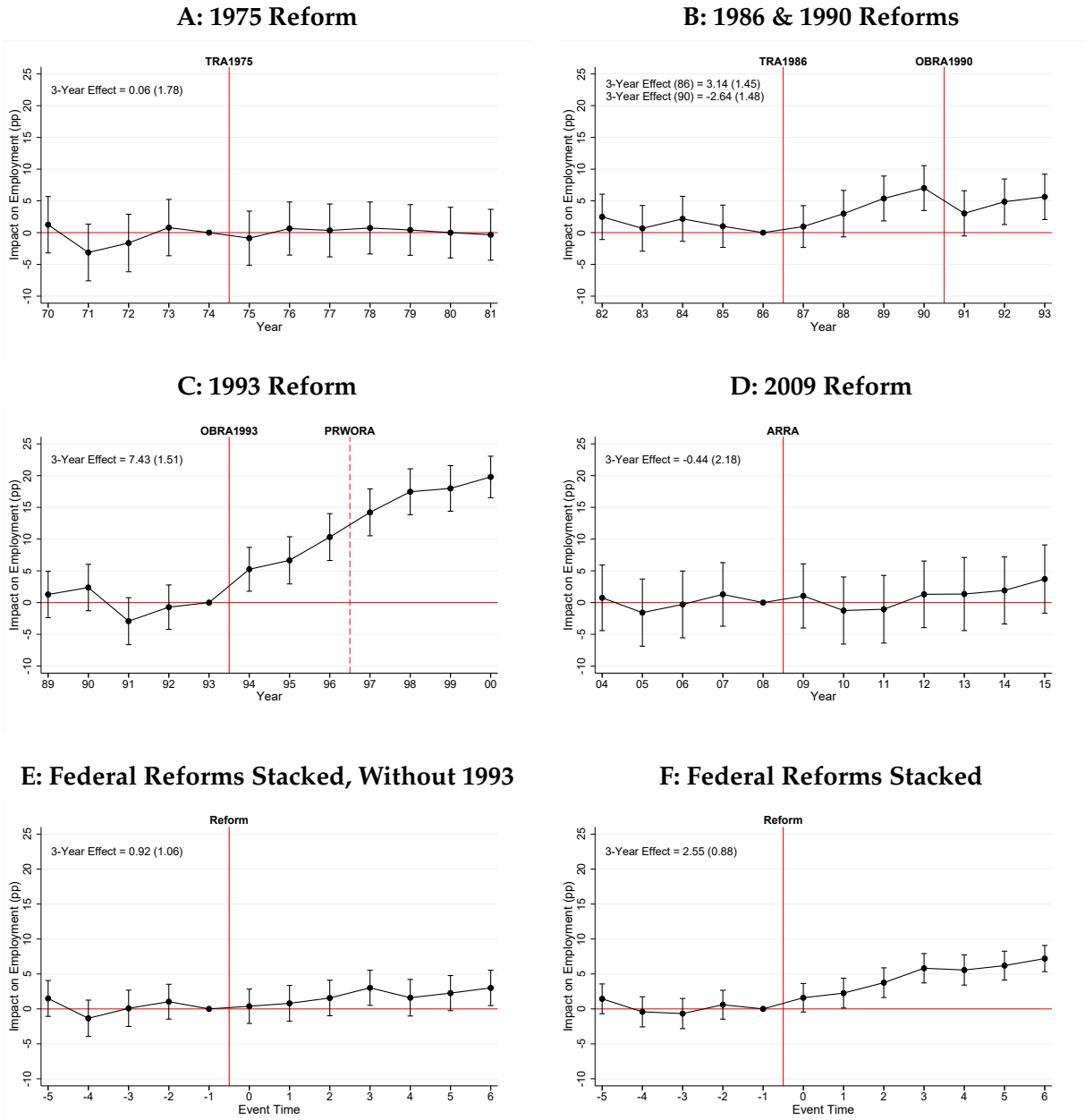


Notes: This figure is constructed exactly as Figure 5 in the main text, except for the estimation sample, and the extensive margin measure. The figure is based on specification (2) with controls for rich demographics and unemployment, a sample of low-educated single women (those with a high school degree or less), and using annual employment as the extensive margin measure.

FIGURE A.41: DID EVENT STUDIES OF FEDERAL EITC REFORMS

SINGLE WOMEN IN BOTTOM HALF OF PREDICTED EARNINGS, ANNUAL EMPLOYMENT

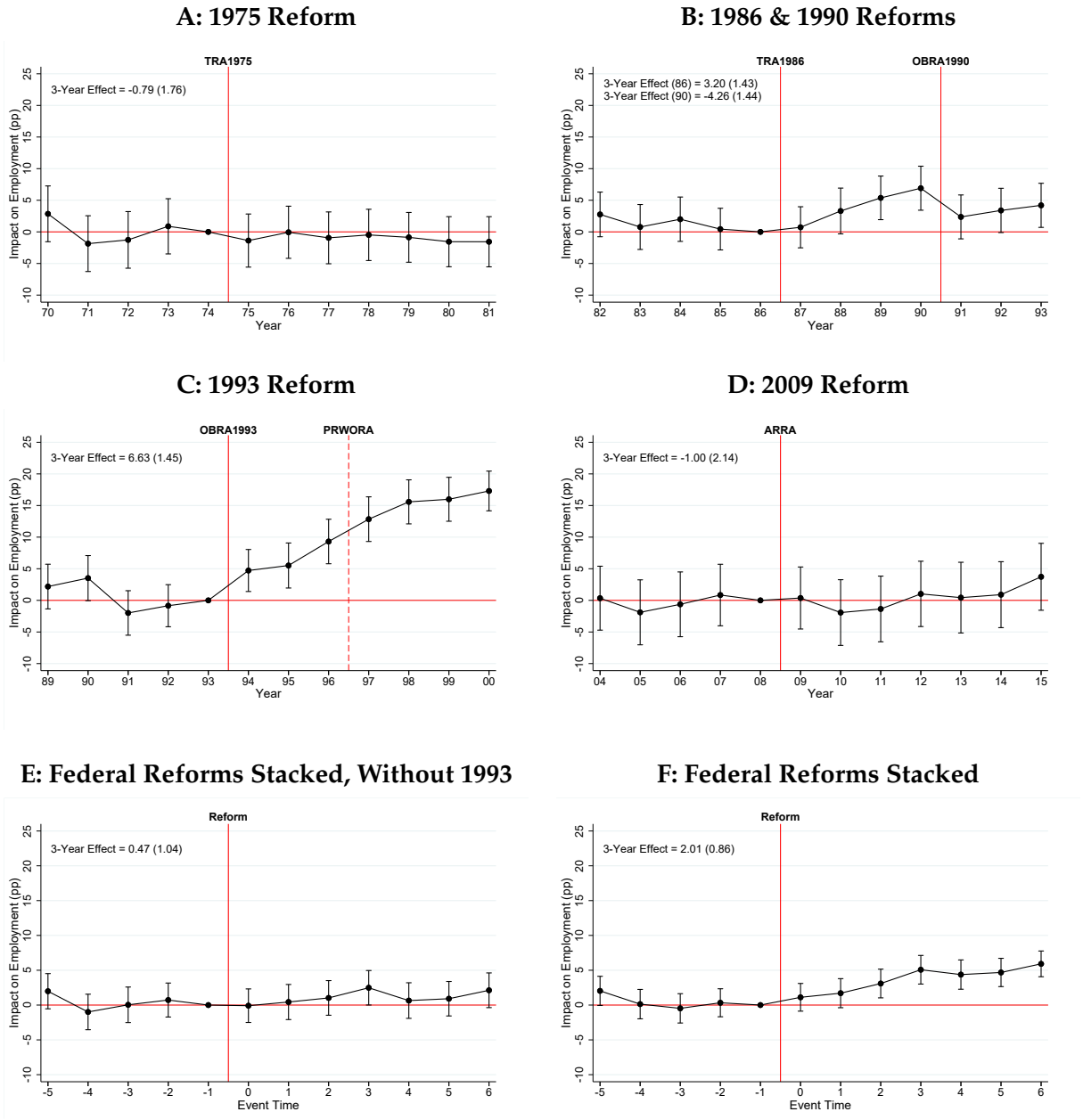
RAW DATA



Notes: This figure is constructed exactly as Figure 5 in the main text, except for the specification of control variables, estimation sample, and the extensive margin measure. The figure is based on specification (2) without any controls, a sample of single women with low predicted earnings, and using annual employment as the extensive margin measure. The low-earnings sample includes single women in the bottom half of the within-year distribution of predicted earnings, estimated using equation (1).

FIGURE A.42: DID EVENT STUDIES OF FEDERAL EITC REFORMS

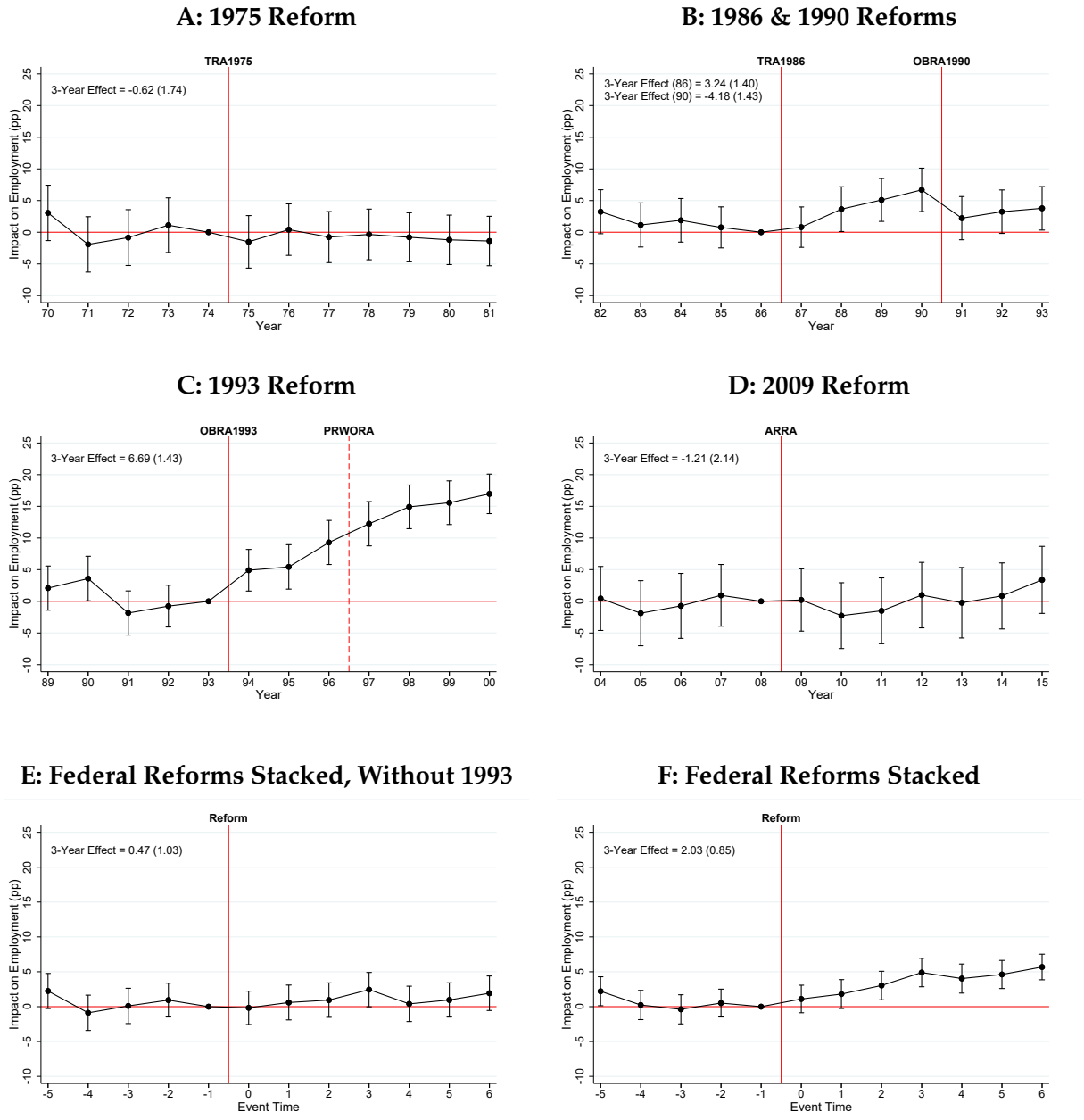
SINGLE WOMEN IN BOTTOM HALF OF PREDICTED EARNINGS, ANNUAL EMPLOYMENT
CONTROLS FOR BASIC DEMOGRAPHICS



Notes: This figure is constructed exactly as Figure 5 in the main text, except for the specification of control variables, estimation sample, and the extensive margin measure. The figure is based on specification (2) with controls for basic demographics only, a sample of single women with low predicted earnings, and using annual employment as the extensive margin measure. The low-earnings sample includes single women in the bottom half of the within-year distribution of predicted earnings, estimated using equation (1).

FIGURE A.43: DID EVENT STUDIES OF FEDERAL EITC REFORMS

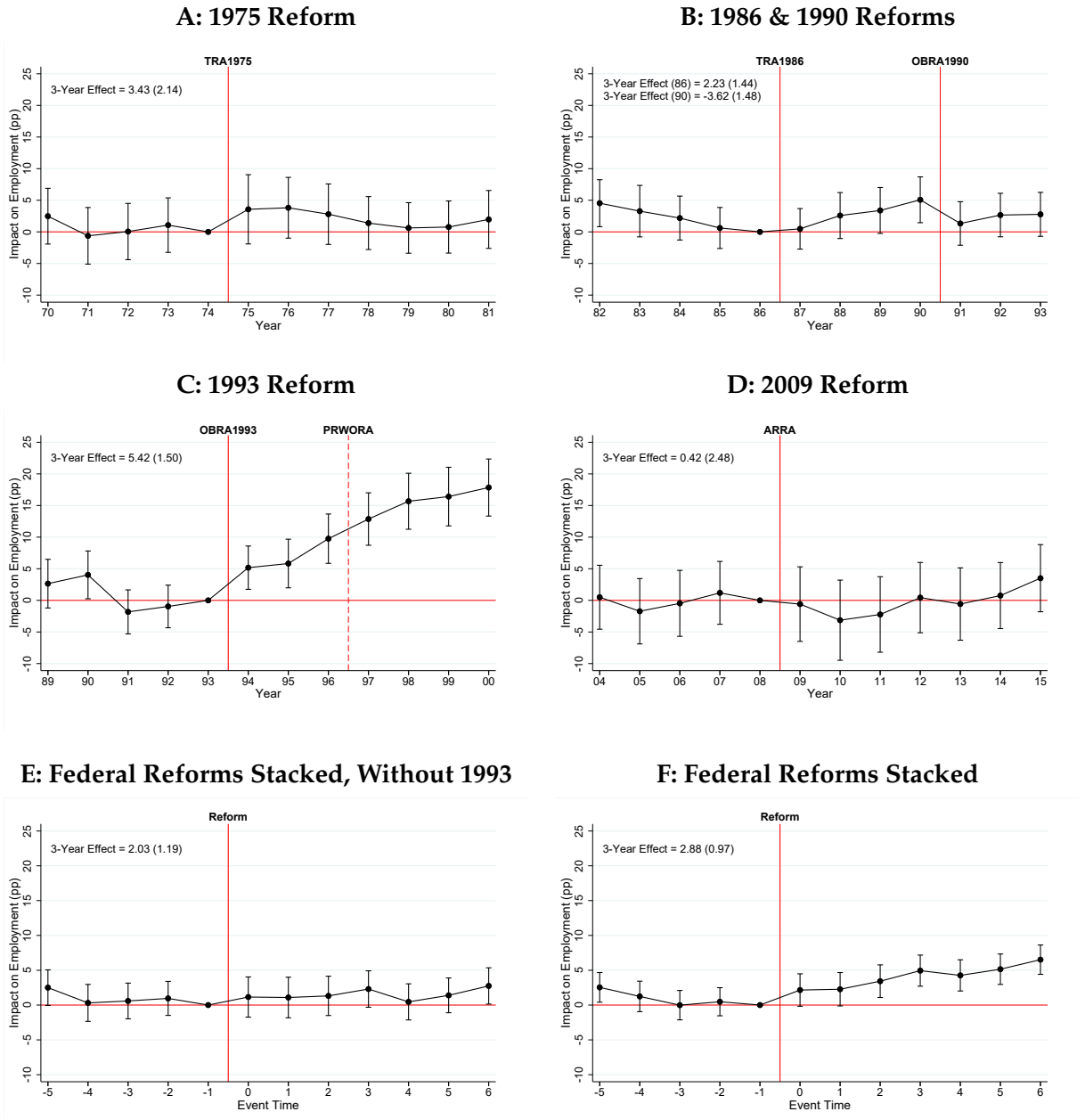
SINGLE WOMEN IN BOTTOM HALF OF PREDICTED EARNINGS, ANNUAL EMPLOYMENT
CONTROLS FOR RICH DEMOGRAPHICS



Notes: This figure is constructed exactly as Figure 5 in the main text, except for the specification of control variables, estimation sample, and the extensive margin measure. The figure is based on specification (2) with controls for rich demographics only, a sample of single women with low predicted earnings, and using annual employment as the extensive margin measure. The low-earnings sample includes single women in the bottom half of the within-year distribution of predicted earnings, estimated using equation (1).

FIGURE A.44: DID EVENT STUDIES OF FEDERAL EITC REFORMS

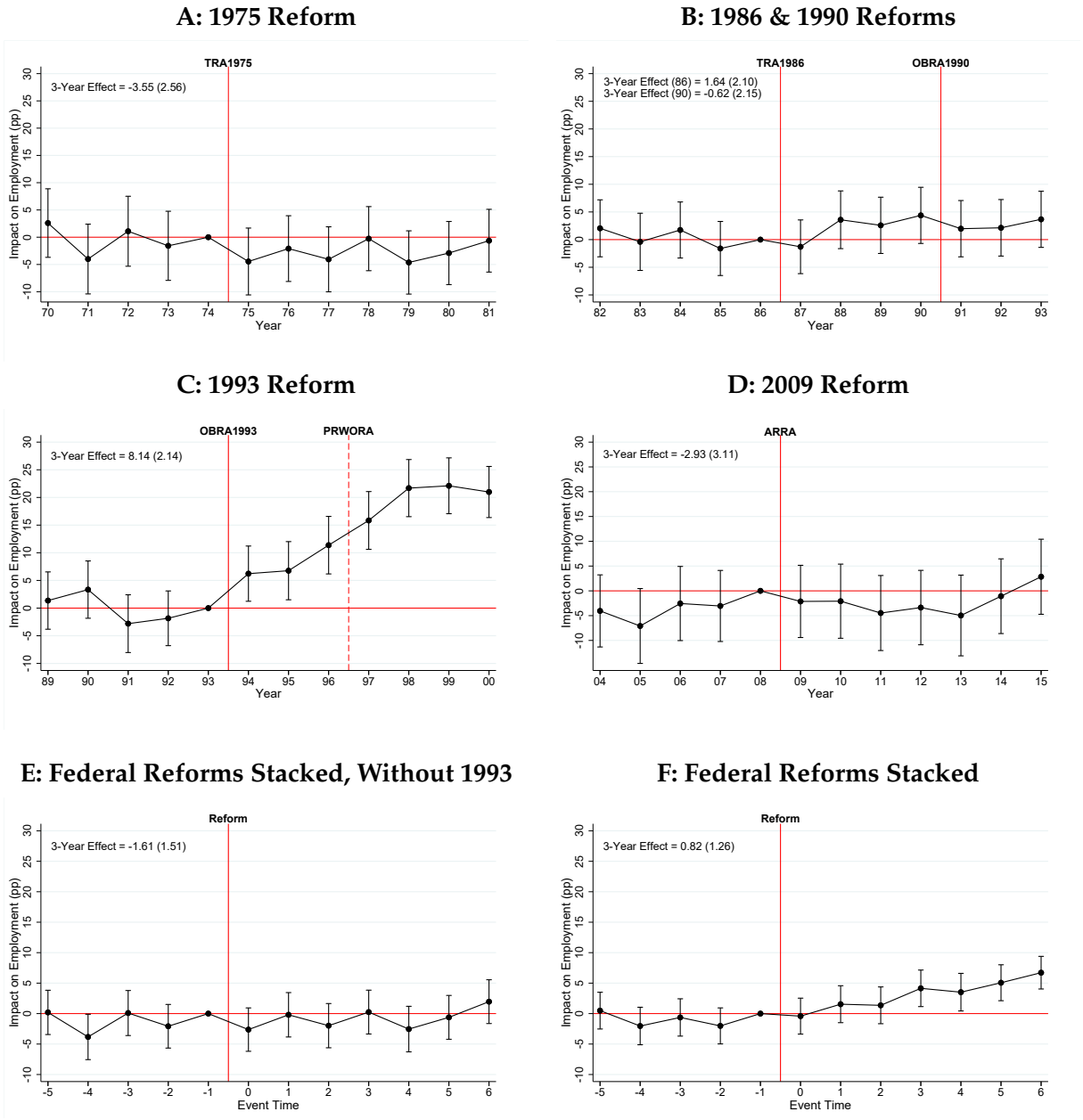
SINGLE WOMEN IN BOTTOM HALF OF PREDICTED EARNINGS, ANNUAL EMPLOYMENT
CONTROLS FOR RICH DEMOGRAPHICS AND UNEMPLOYMENT



Notes: This figure is constructed exactly as Figure 5 in the main text, except for the estimation sample and the extensive margin measure. The figure is based on specification (2) with controls for rich demographic and unemployment, a sample of single women with low predicted earnings, and using annual employment as the extensive margin measure. The low-earnings sample includes single women in the bottom half of the within-year distribution of predicted earnings, estimated using equation (1).

FIGURE A.45: DID EVENT STUDIES OF FEDERAL EITC REFORMS

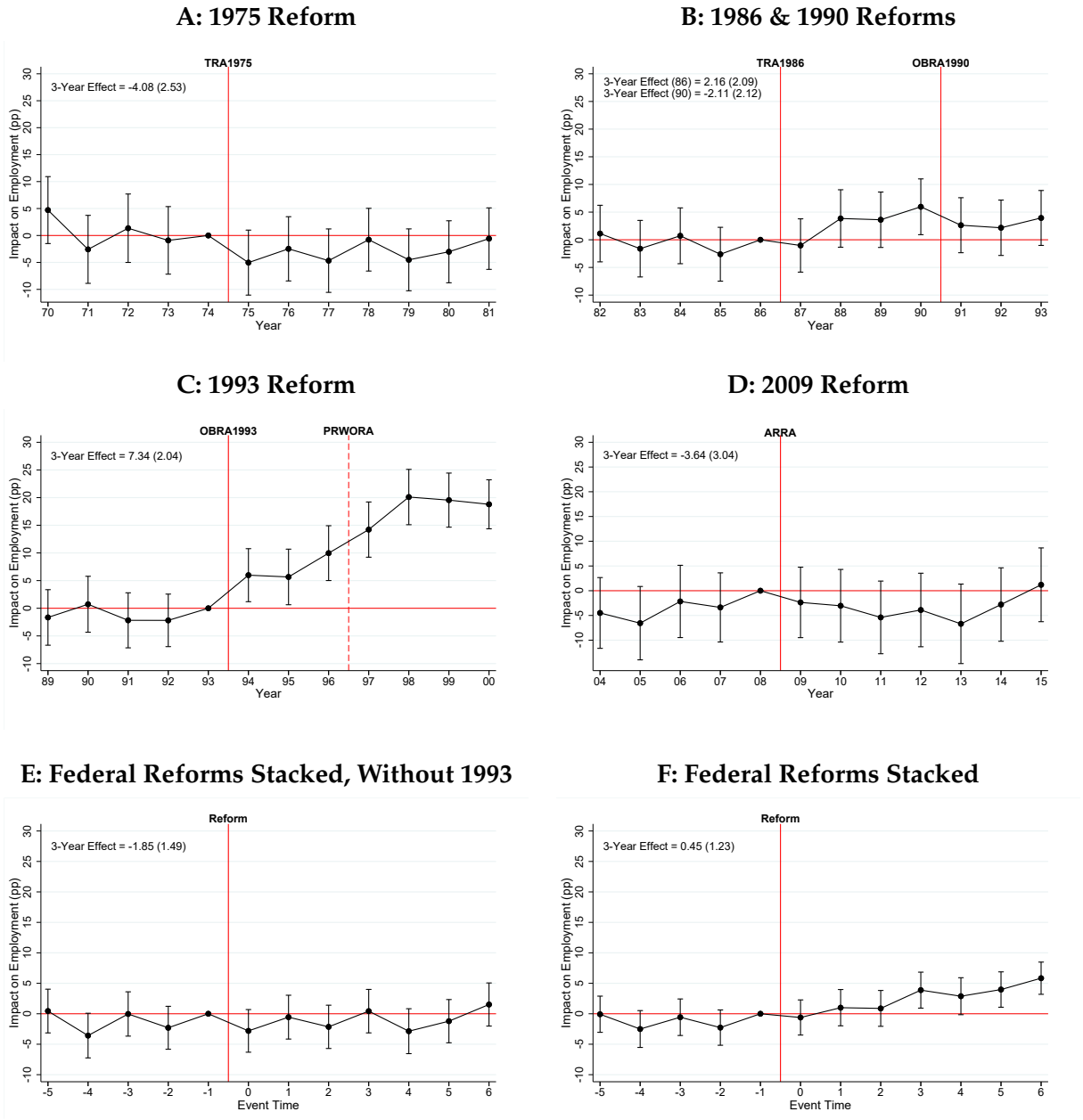
SINGLE WOMEN IN BOTTOM QUARTILE OF PREDICTED EARNINGS, ANNUAL EMPLOYMENT
RAW DATA



Notes: This figure is constructed exactly as Figure 5 in the main text, except for the specification of control variables, estimation sample and the extensive margin measure. The figure is based on specification (2) without any controls, a sample of single women with low predicted earnings, and using annual employment as the extensive margin measure. The low-earnings sample includes single women in the bottom quartile of the within-year distribution of predicted earnings, estimated using equation (1).

FIGURE A.46: DID EVENT STUDIES OF FEDERAL EITC REFORMS

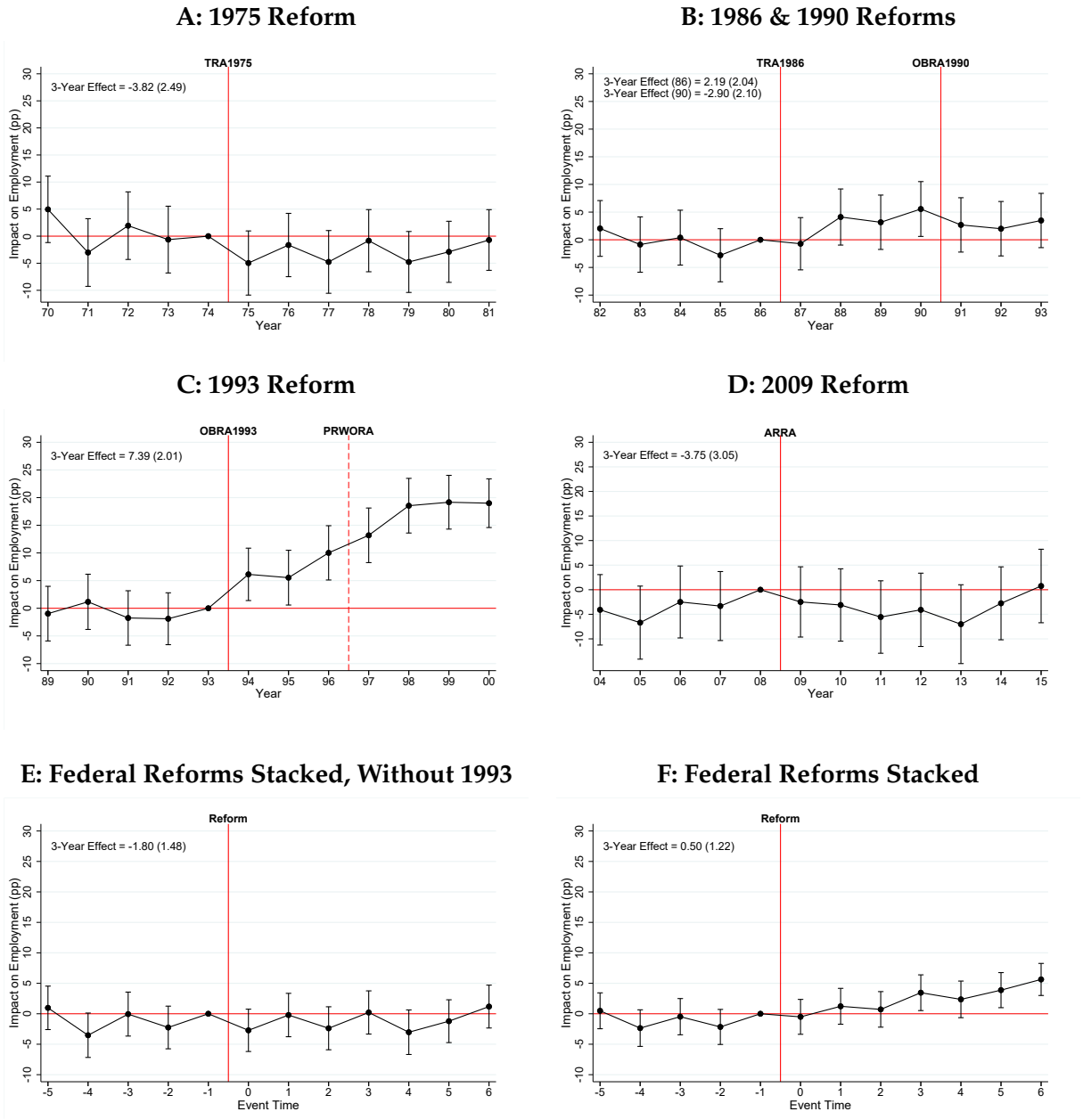
SINGLE WOMEN IN BOTTOM QUARTILE OF PREDICTED EARNINGS, ANNUAL EMPLOYMENT
CONTROLS FOR BASIC DEMOGRAPHICS



Notes: This figure is constructed exactly as Figure 5 in the main text, except for the specification of control variables, estimation sample, and the extensive margin measure. The figure is based on specification (2) with controls for basic demographics only, a sample of single women with low predicted earnings, and using annual employment as the extensive margin measure. The low-earnings sample includes single women in the bottom quartile of the within-year distribution of predicted earnings, estimated using equation (1).

FIGURE A.47: DID EVENT STUDIES OF FEDERAL EITC REFORMS

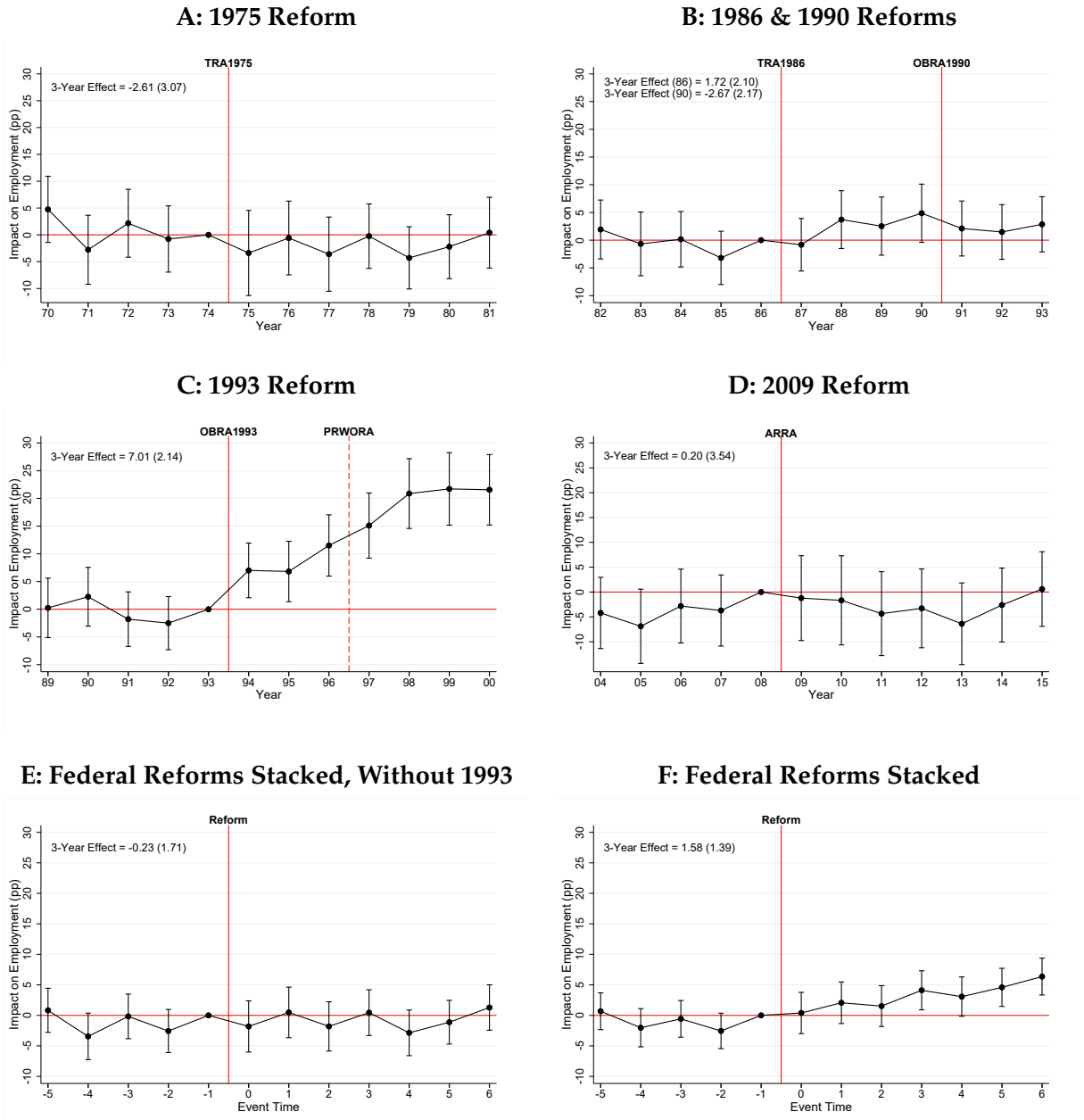
SINGLE WOMEN IN BOTTOM QUARTILE OF PREDICTED EARNINGS, ANNUAL EMPLOYMENT
CONTROLS FOR RICH DEMOGRAPHICS



Notes: This figure is constructed exactly as Figure 5 in the main text, except for the specification of control variables, estimation sample, and the extensive margin measure. The figure is based on specification (2) with controls for rich demographics only, a sample of single women with low predicted earnings, and using annual employment as the extensive margin measure. The low-earnings sample includes single women in the bottom quartile of the within-year distribution of predicted earnings, estimated using equation (1).

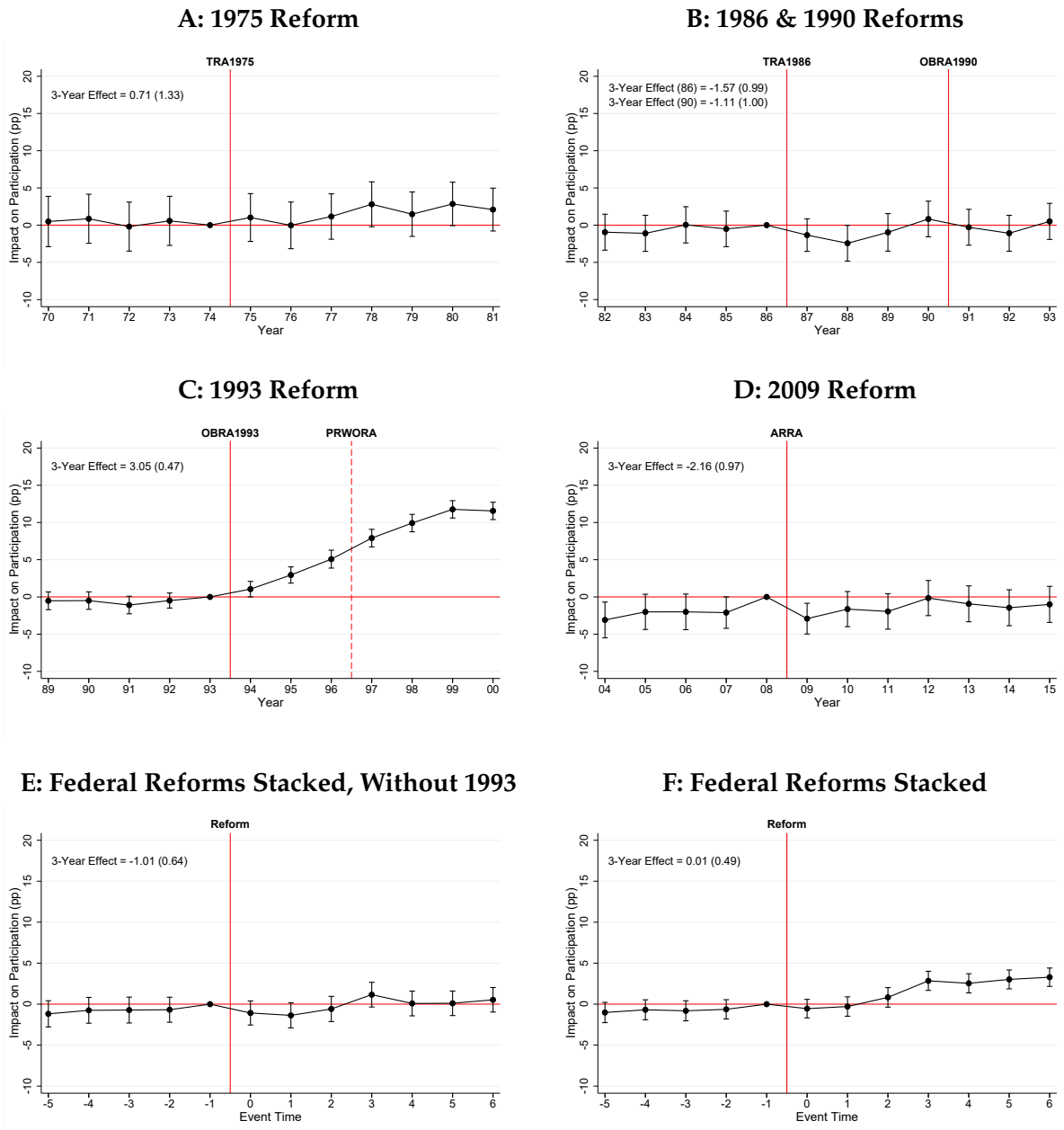
FIGURE A.48: DID EVENT STUDIES OF FEDERAL EITC REFORMS

SINGLE WOMEN IN BOTTOM QUARTILE OF PREDICTED EARNINGS, ANNUAL EMPLOYMENT
CONTROLS FOR RICH DEMOGRAPHICS AND UNEMPLOYMENT



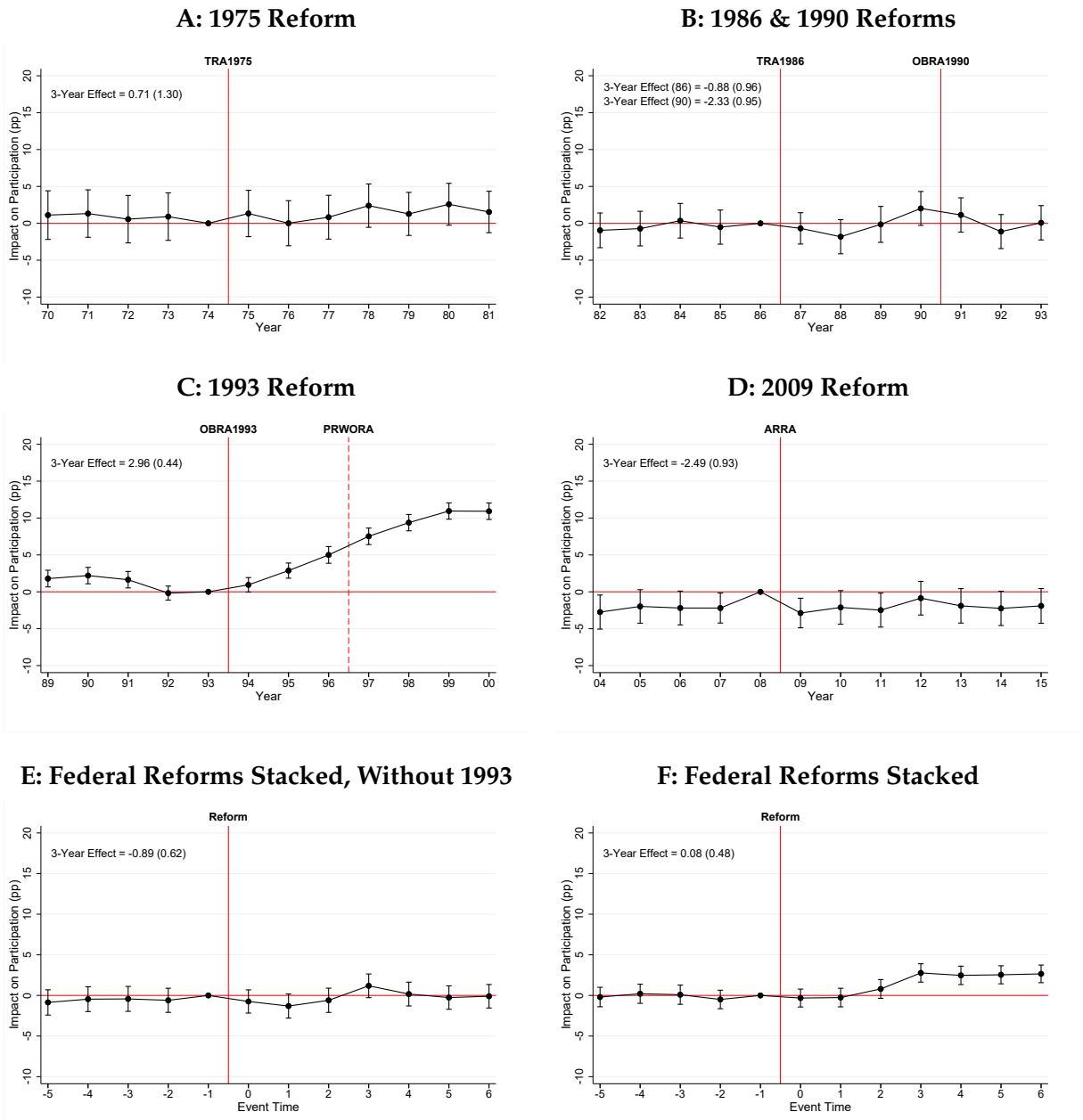
Notes: This figure is constructed exactly as Figure 5 in the main text, except for the estimation sample, and the extensive margin measure. The figure is based on specification (2) with controls for rich demographics and unemployment, a sample of single women with low predicted earnings, and using annual employment as the extensive margin measure. The low-earnings sample includes single women in the bottom quartile of the within-year distribution of predicted earnings, estimated using equation (1).

FIGURE A.49: DID EVENT STUDIES OF FEDERAL EITC REFORMS
ALL SINGLE WOMEN, WEEKLY PARTICIPATION
RAW DATA



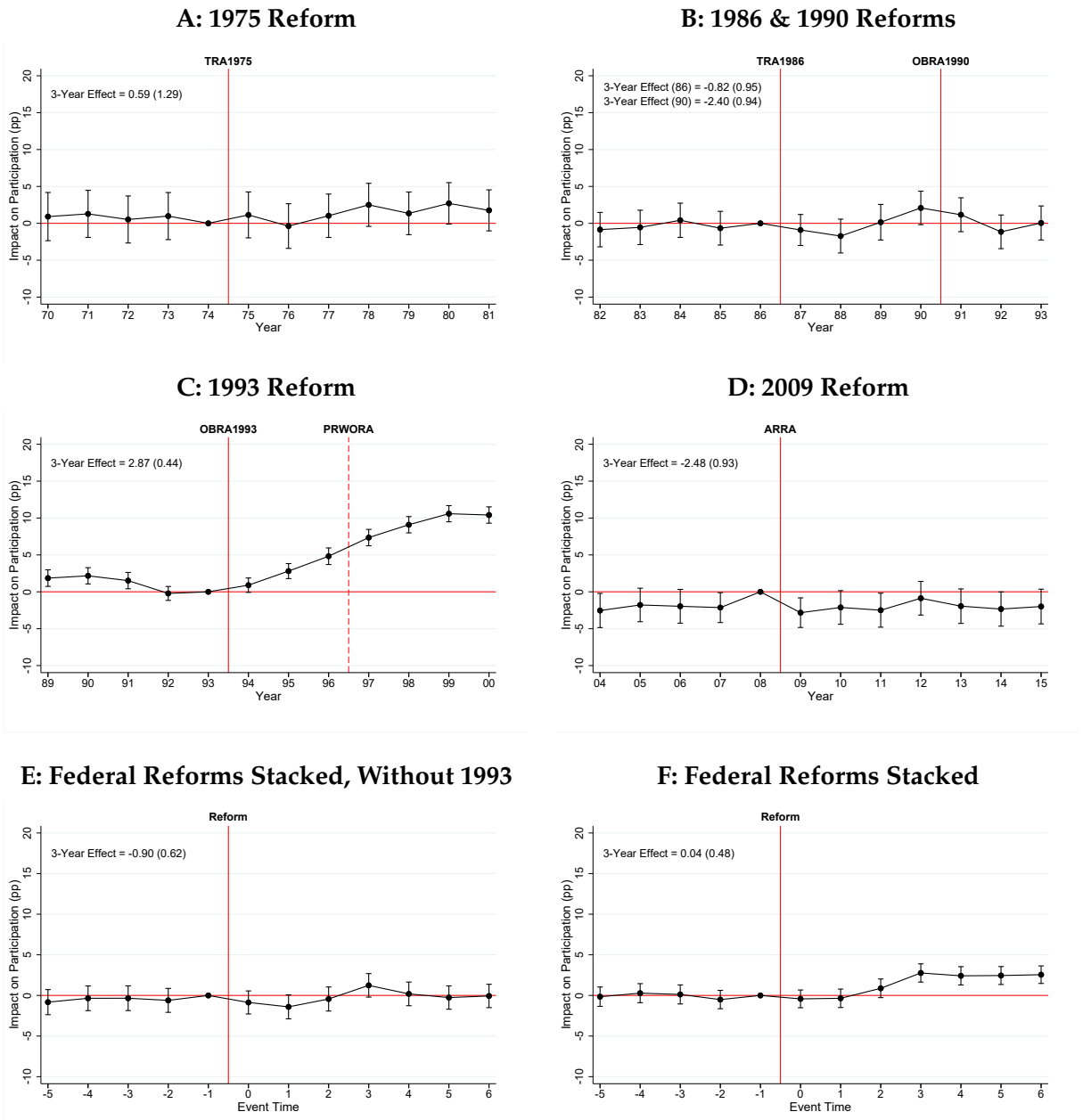
Notes: This figure is constructed exactly as Figure 5 in the main text, except for the specification of control variables and the extensive margin measure. The figure is based on specification (2) without any controls, the sample of all single women, and using weekly participation as the extensive margin measure.

FIGURE A.50: DID EVENT STUDIES OF FEDERAL EITC REFORMS
ALL SINGLE WOMEN, WEEKLY PARTICIPATION
CONTROLS FOR BASIC DEMOGRAPHICS



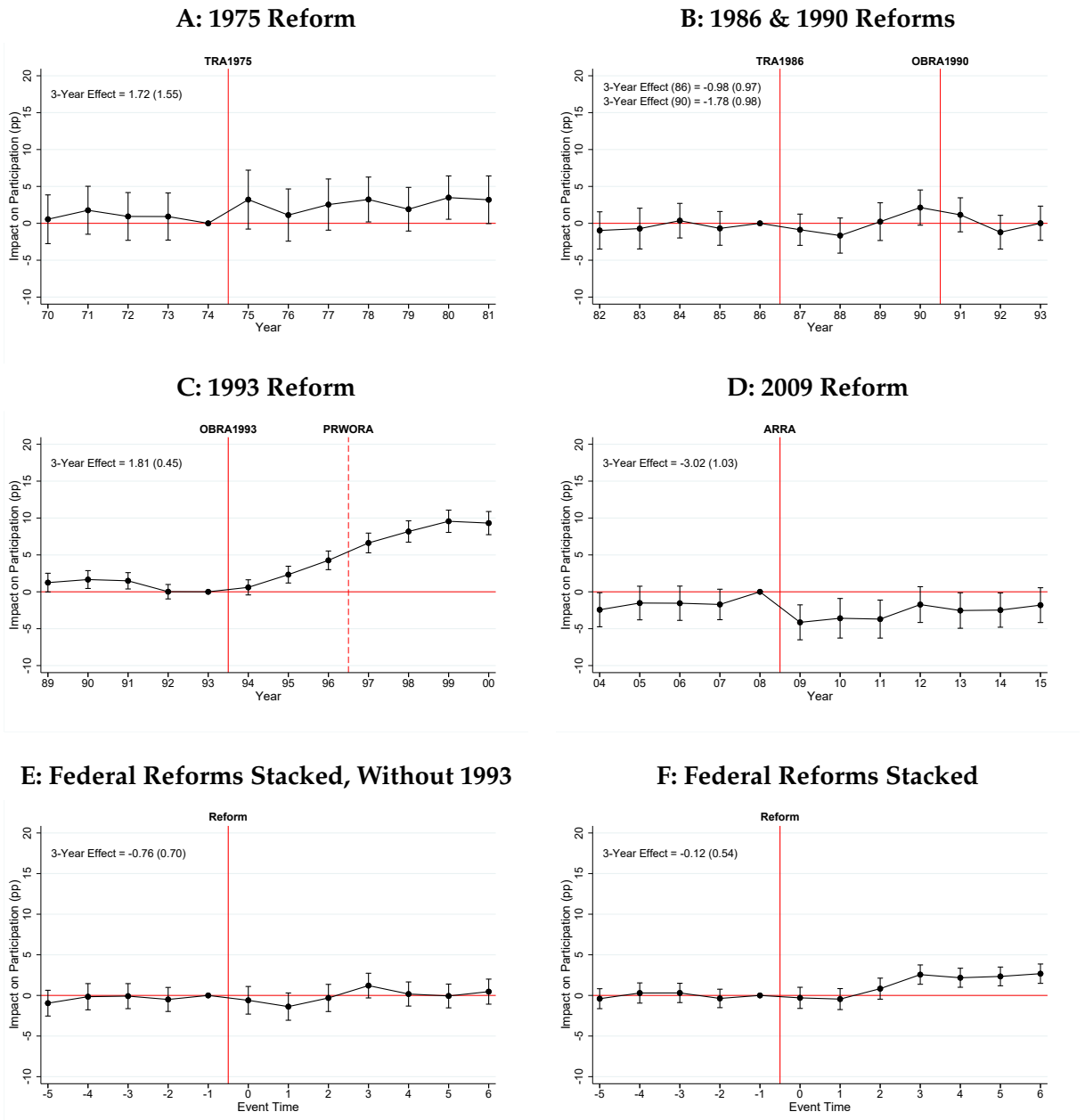
Notes: This figure is constructed exactly as Figure 5 in the main text, except for the specification of control variables and the extensive margin measure. The figure is based on specification (2) with controls for basic demographics only, the sample of all single women, and using weekly participation as the extensive margin measure.

FIGURE A.51: DID EVENT STUDIES OF FEDERAL EITC REFORMS
ALL SINGLE WOMEN, WEEKLY PARTICIPATION
CONTROLS FOR RICH DEMOGRAPHICS



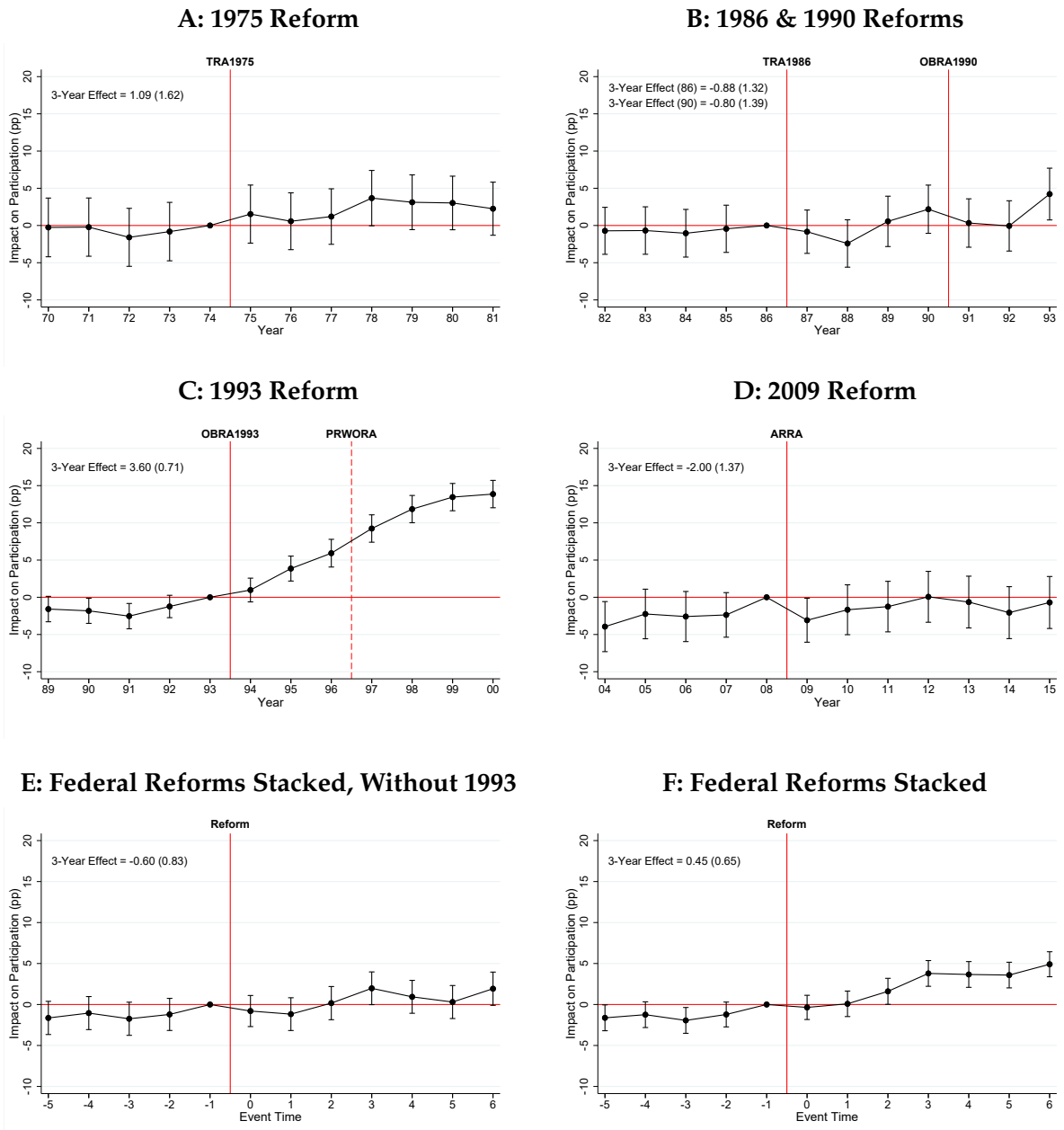
Notes: This figure is constructed exactly as Figure 5 in the main text, except for the specification of control variables and the extensive margin measure. The figure is based on specification (2) with controls for rich demographics only, the sample of all single women, and using weekly participation as the extensive margin measure.

FIGURE A.52: DID EVENT STUDIES OF FEDERAL EITC REFORMS
ALL SINGLE WOMEN, WEEKLY PARTICIPATION
CONTROLS FOR RICH DEMOGRAPHICS AND UNEMPLOYMENT



Notes: This figure is constructed exactly as Figure 5 in the main text, except for the extensive margin measure. The figure is based on specification (2) with controls for rich demographics and unemployment, the sample of all single women, and using weekly participation as the extensive margin measure.

FIGURE A.53: DID EVENT STUDIES OF FEDERAL EITC REFORMS
LOW-EDUCATED SINGLE WOMEN, WEEKLY PARTICIPATION
RAW DATA

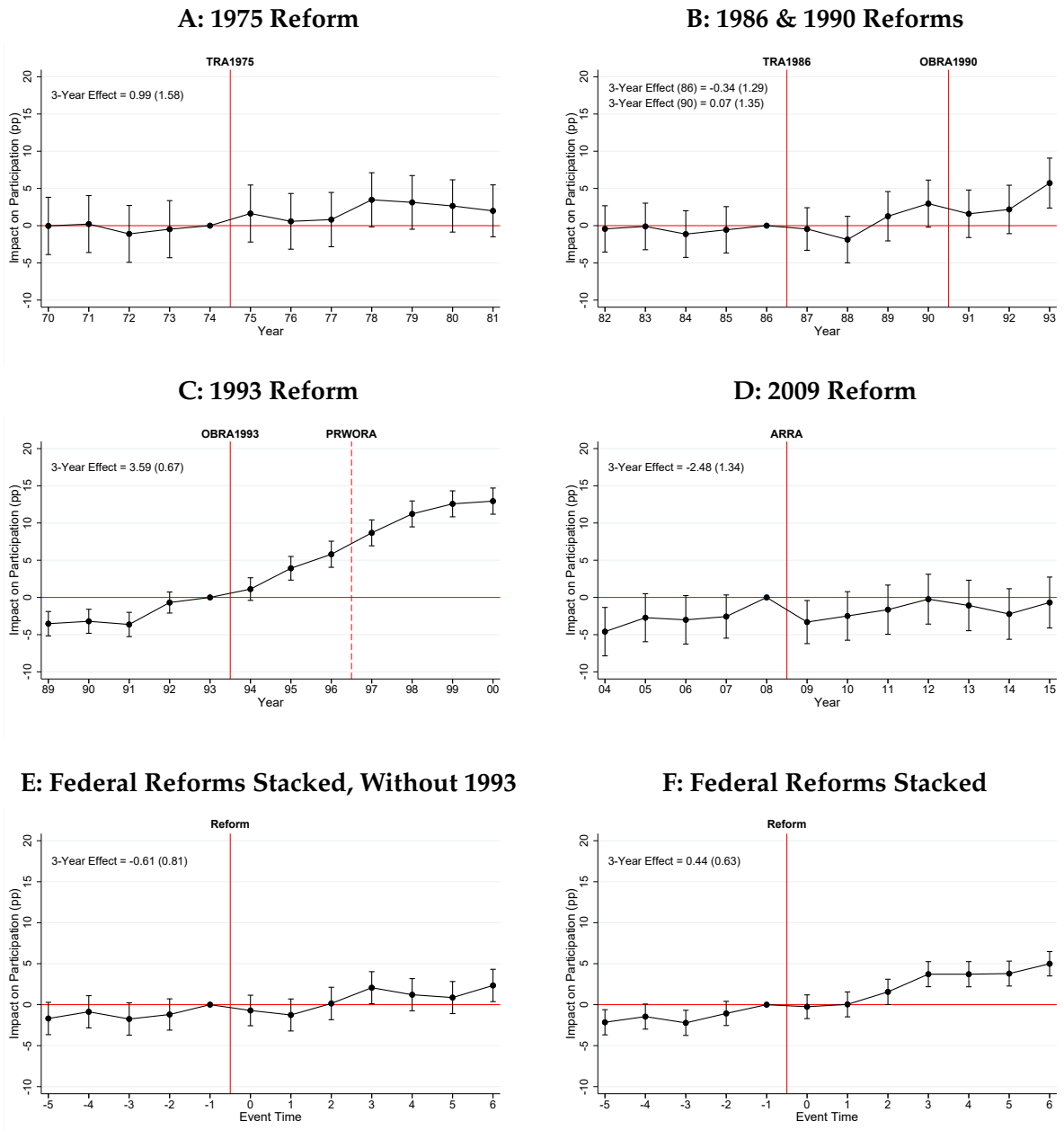


Notes: This figure is constructed exactly as Figure 5 in the main text, except for the specification of control variables, estimation sample, and the extensive margin measure. The figure is based on specification (2) without any controls, the sample of low-educated single women (those with a high school degree or less), and using weekly participation as the extensive margin measure.

FIGURE A.54: DID EVENT STUDIES OF FEDERAL EITC REFORMS

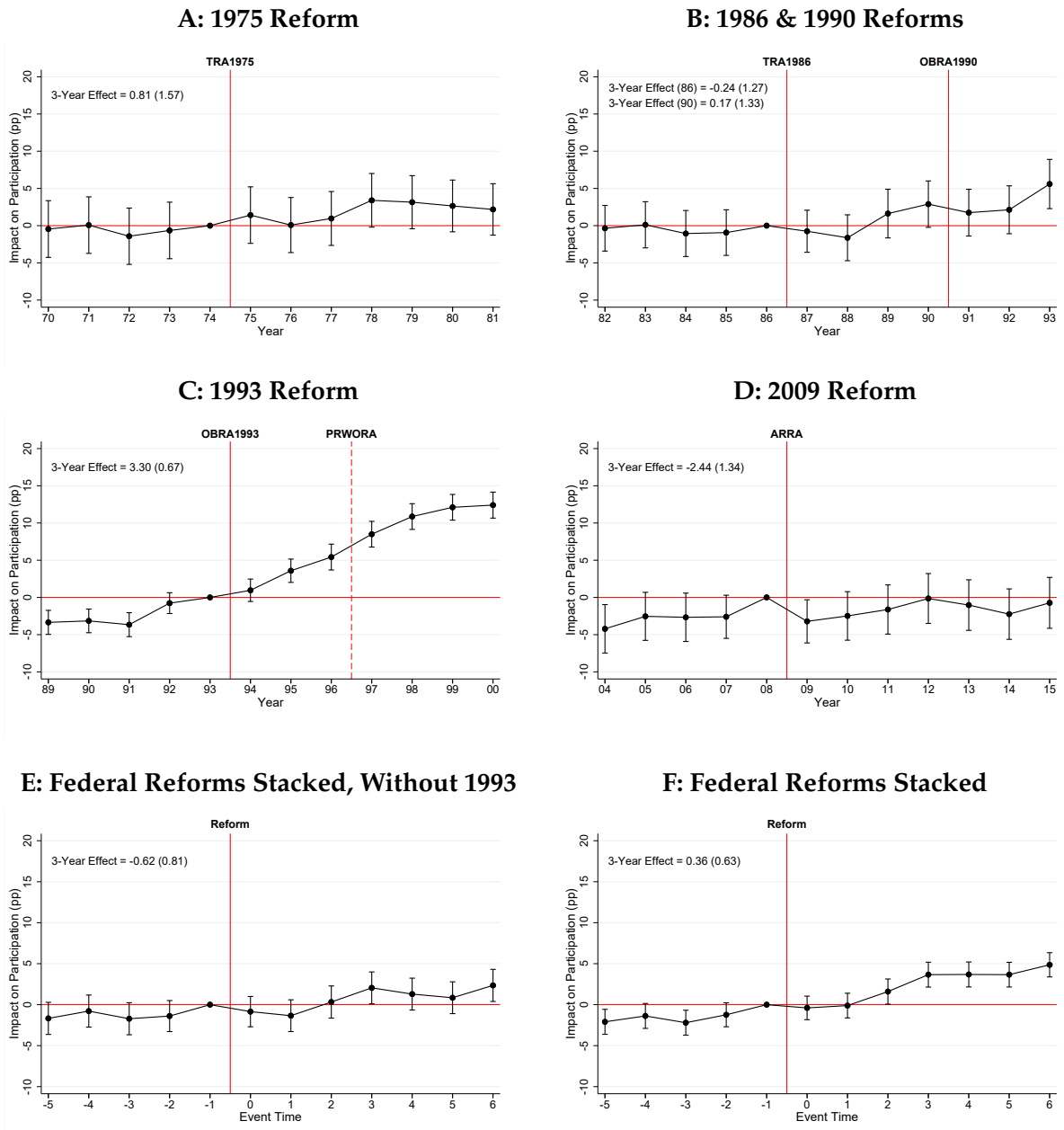
LOW-EDUCATED SINGLE WOMEN, WEEKLY PARTICIPATION

CONTROLS FOR BASIC DEMOGRAPHICS



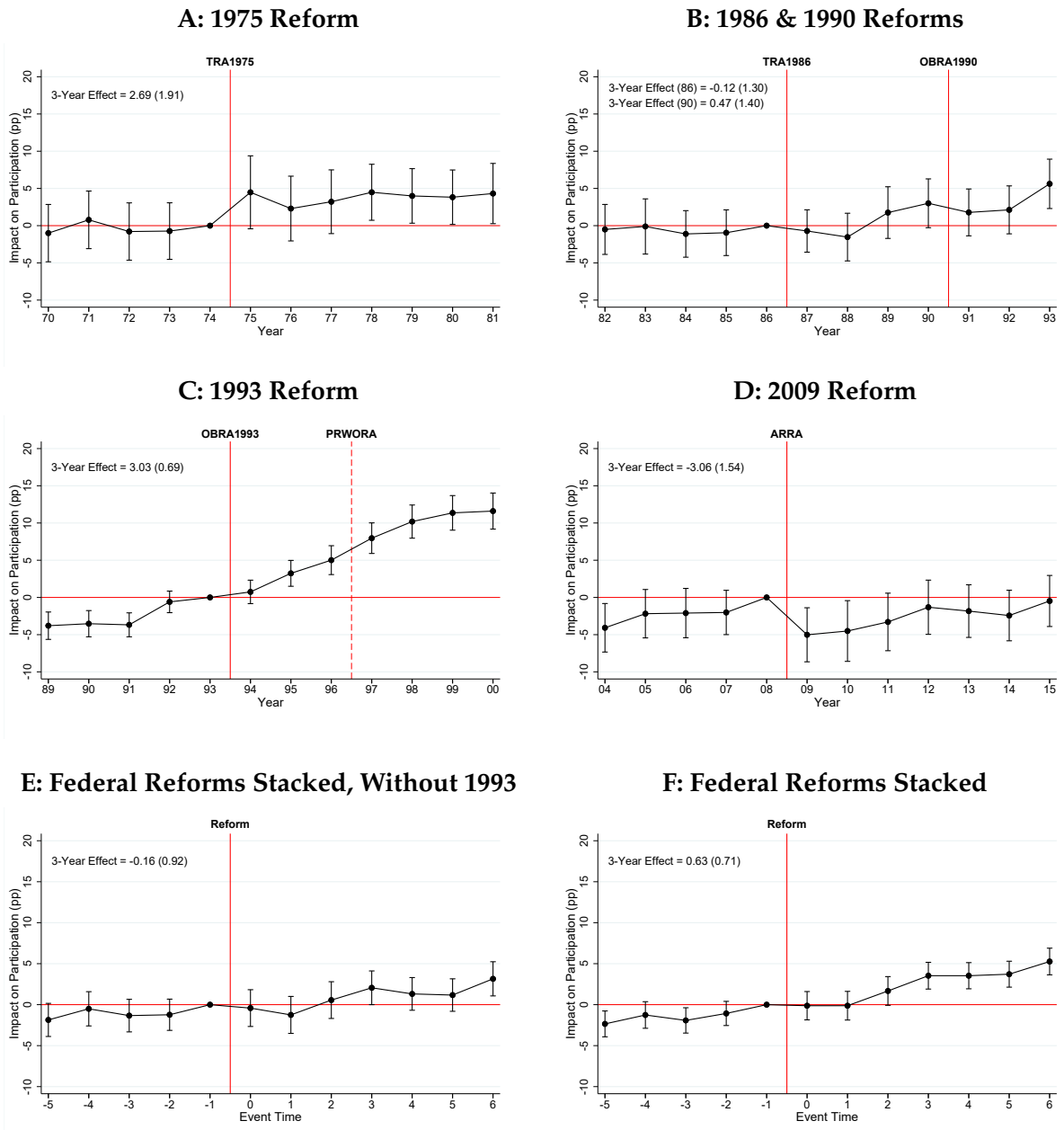
Notes: This figure is constructed exactly as Figure 5 in the main text, except for the specification of control variables, estimation sample, and the extensive margin measure. The figure is based on specification (2) with controls for basic demographics only, the sample of low-educated single women (those with a high school degree or less), and using weekly participation as the extensive margin measure.

Figure A.55: DiD Event Studies of Federal EITC Reforms
Low-Educated Single Women, Weekly Participation
Controls for Rich Demographics



Notes: This figure is constructed exactly as Figure 5 in the main text, except for the specification of control variables, estimation sample, and the extensive margin measure. The figure is based on specification (2) with controls for rich demographics only, the sample of low-educated single women (those with a high school degree or less), and using weekly participation as the extensive margin measure.

FIGURE A.56: DID EVENT STUDIES OF FEDERAL EITC REFORMS
LOW-EDUCATED SINGLE WOMEN, WEEKLY PARTICIPATION
CONTROLS FOR RICH DEMOGRAPHICS AND UNEMPLOYMENT

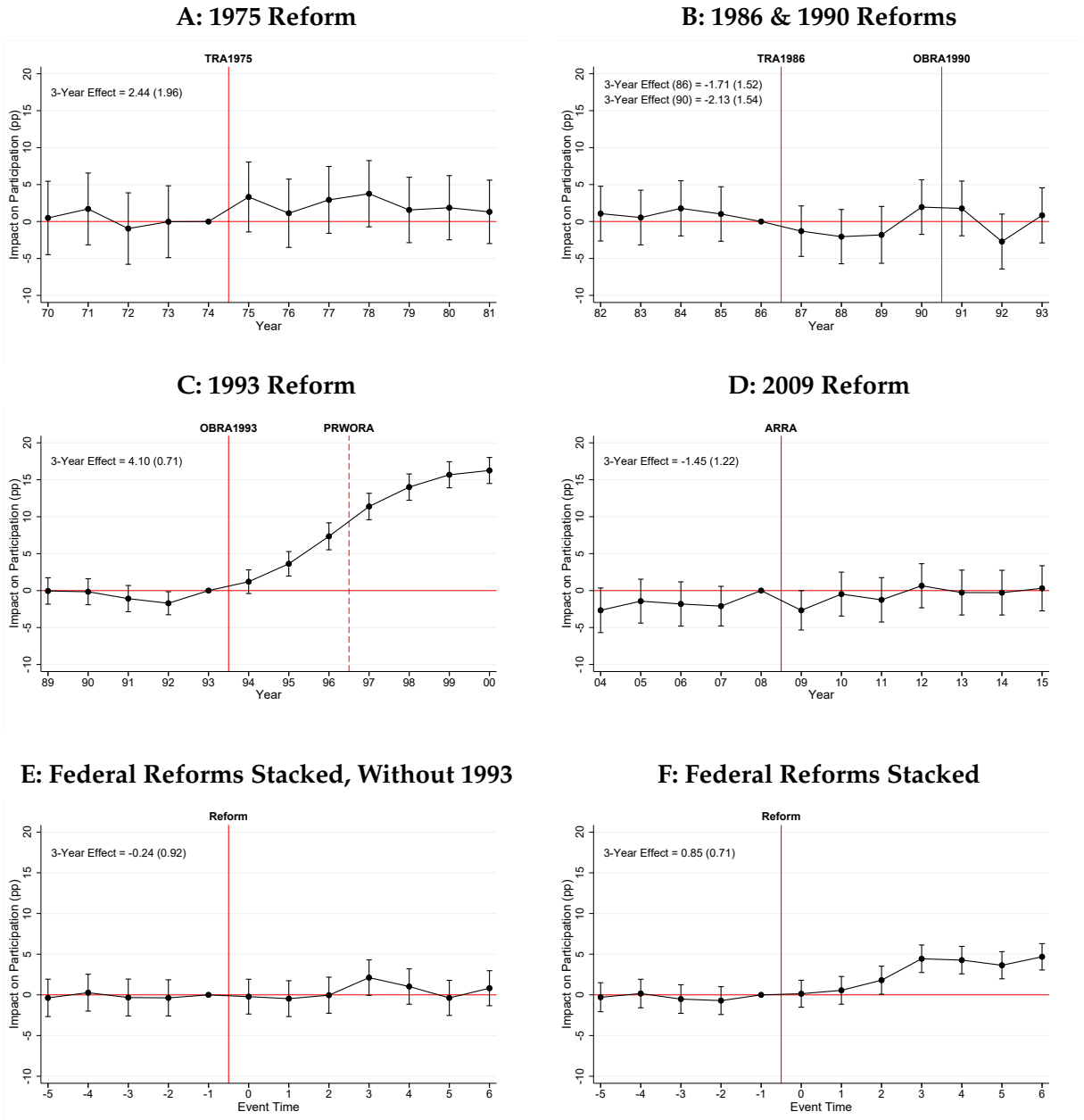


Notes: This figure is constructed exactly as Figure 5 in the main text, except for the estimation sample and the extensive margin measure. The figure is based on specification (2) with controls for rich demographics and unemployment, the sample of low-educated single women (those with a high school degree or less), and using weekly participation as the extensive margin measure.

FIGURE A.57: DID EVENT STUDIES OF FEDERAL EITC REFORMS

SINGLE WOMEN IN BOTTOM HALF OF PREDICTED EARNINGS, WEEKLY PARTICIPATION

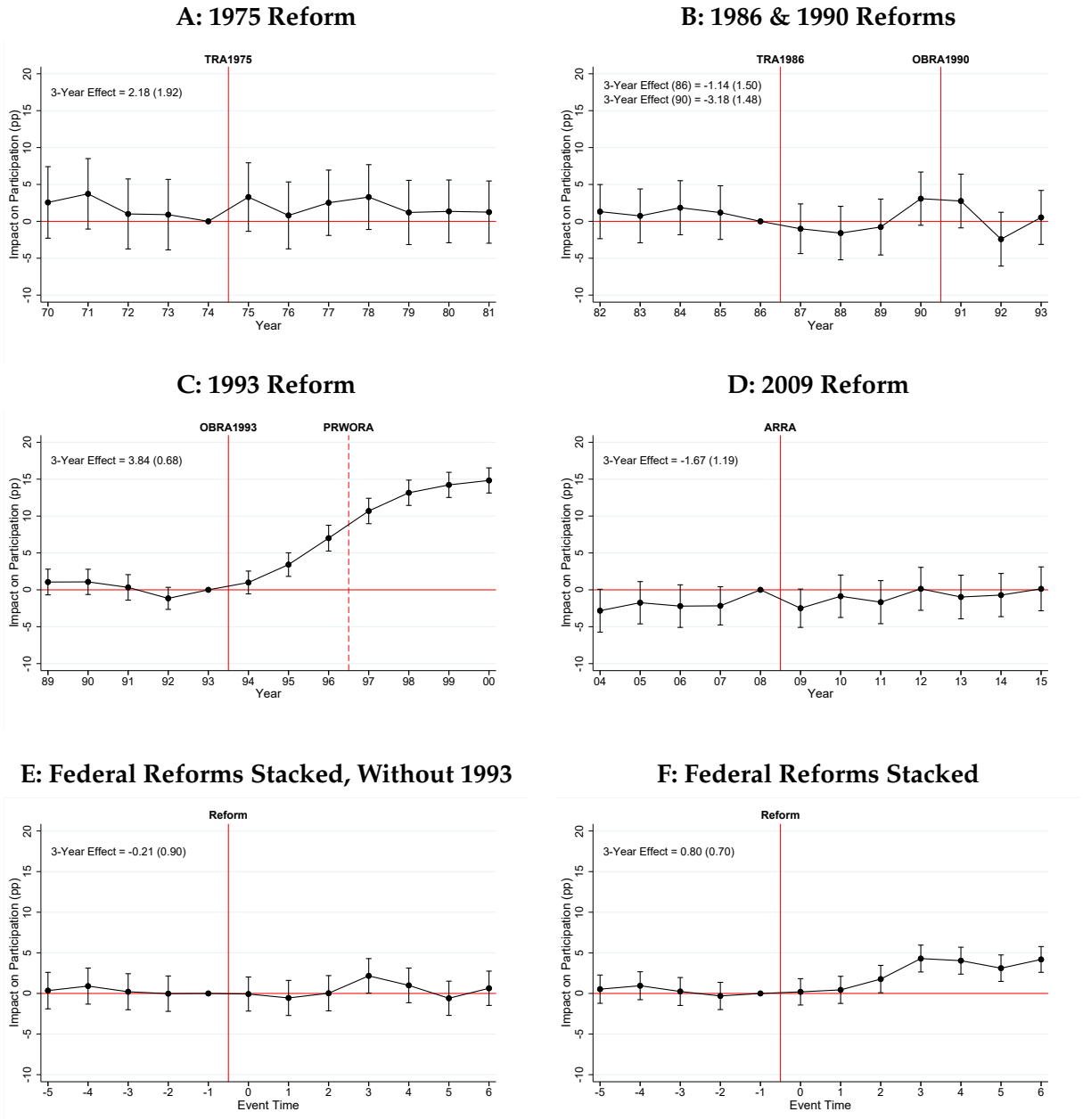
RAW DATA



Notes: This figure is constructed exactly as Figure 5 in the main text, except for the specification of control variables, estimation sample, and the extensive margin measure. The figure is based on specification (2) without any controls, a sample of single women with low predicted earnings, and using weekly participation as the extensive margin measure. The low-earnings sample includes single women in the bottom half of the within-year distribution of predicted earnings, estimated using equation (1).

FIGURE A.58: DID EVENT STUDIES OF FEDERAL EITC REFORMS

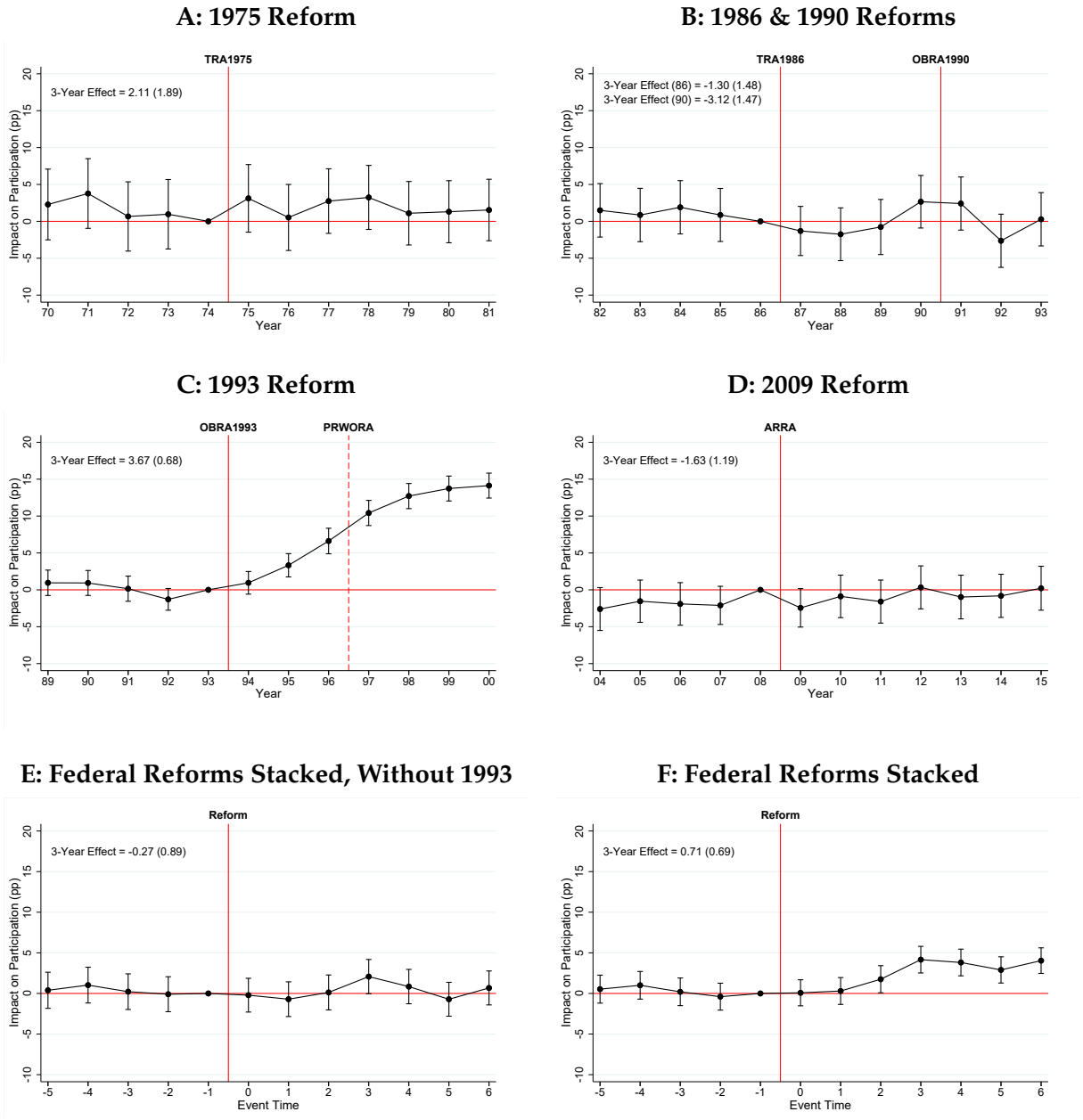
SINGLE WOMEN IN BOTTOM HALF OF PREDICTED EARNINGS, WEEKLY PARTICIPATION
CONTROLS FOR BASIC DEMOGRAPHICS



Notes: This figure is constructed exactly as Figure 5 in the main text, except for the specification of control variables, estimation sample, and the extensive margin measure. The figure is based on specification (2) with controls for basic demographics only, a sample of single women with low predicted earnings, and using weekly participation as the extensive margin measure. The low-earnings sample includes single women in the bottom half of the within-year distribution of predicted earnings, estimated using equation (1).

FIGURE A.59: DID EVENT STUDIES OF FEDERAL EITC REFORMS

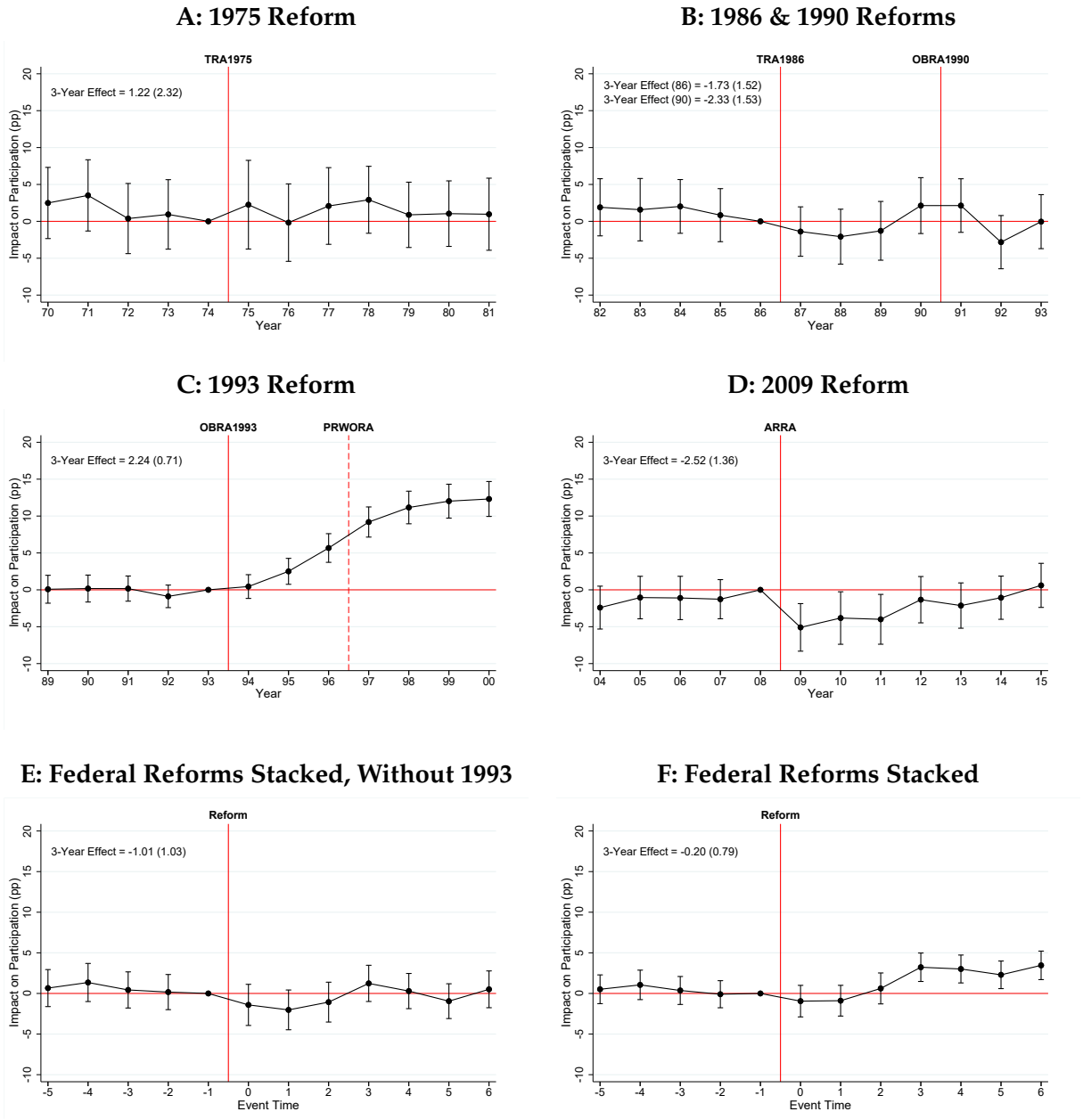
SINGLE WOMEN IN BOTTOM HALF OF PREDICTED EARNINGS, WEEKLY PARTICIPATION
CONTROLS FOR RICH DEMOGRAPHICS



Notes: This figure is constructed exactly as Figure 5 in the main text, except for the specification of control variables, estimation sample, and the extensive margin measure. The figure is based on specification (2) with controls for rich demographics only, a sample of single women with low predicted earnings, and using weekly participation as the extensive margin measure. The low-earnings sample includes single women in the bottom half of the within-year distribution of predicted earnings, estimated using equation (1).

FIGURE A.60: DID EVENT STUDIES OF FEDERAL EITC REFORMS

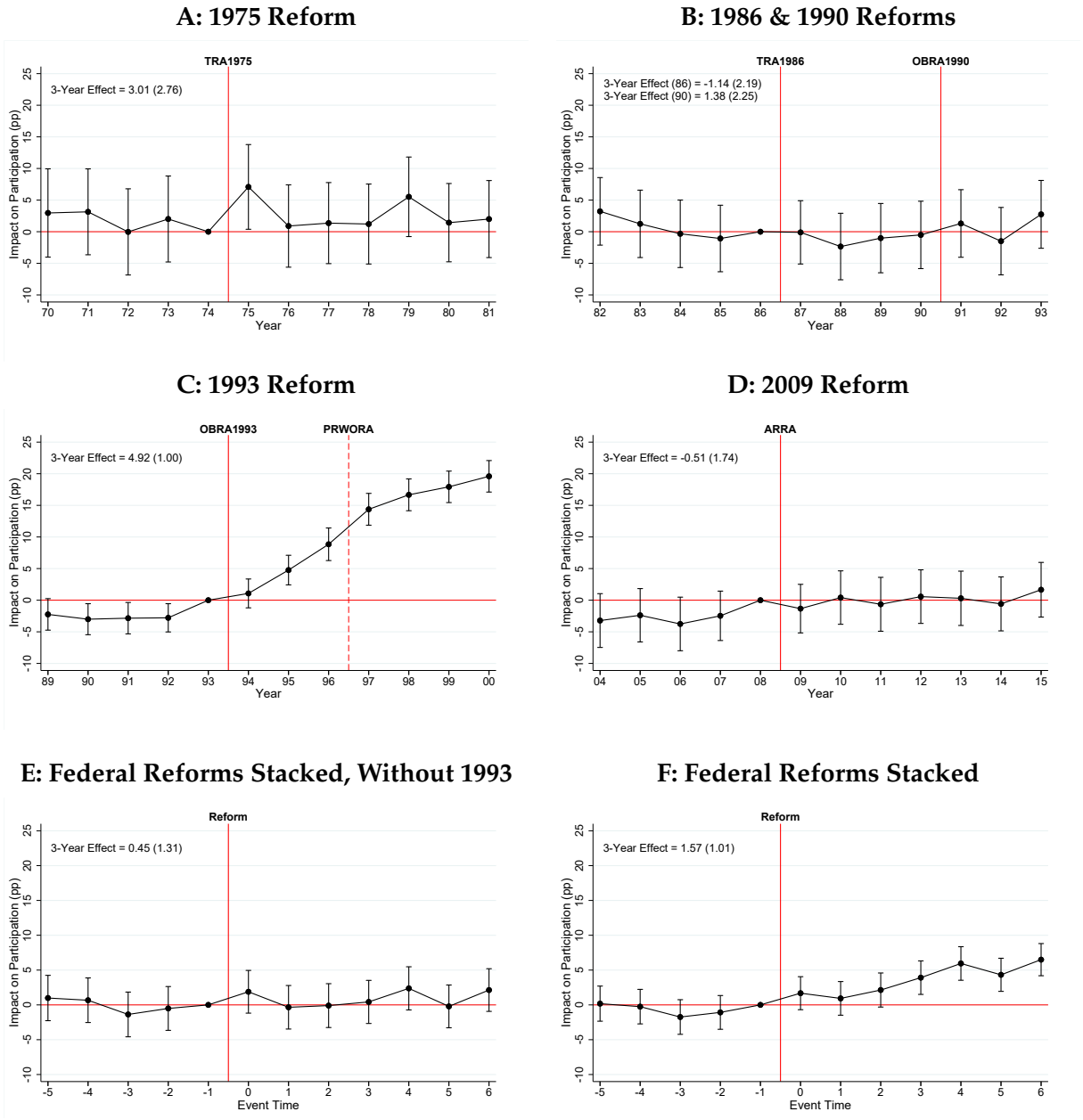
SINGLE WOMEN IN BOTTOM HALF OF PREDICTED EARNINGS, WEEKLY PARTICIPATION
CONTROLS FOR RICH DEMOGRAPHICS AND UNEMPLOYMENT



Notes: This figure is constructed exactly as Figure 5 in the main text, except for estimation sample and the extensive margin measure. The figure is based on specification (2) with controls for rich demographics and unemployment, a sample of single women with low predicted earnings, and using weekly participation as the extensive margin measure. The low-earnings sample includes single women in the bottom half of the within-year distribution of predicted earnings, estimated using equation (1).

FIGURE A.61: DID EVENT STUDIES OF FEDERAL EITC REFORMS

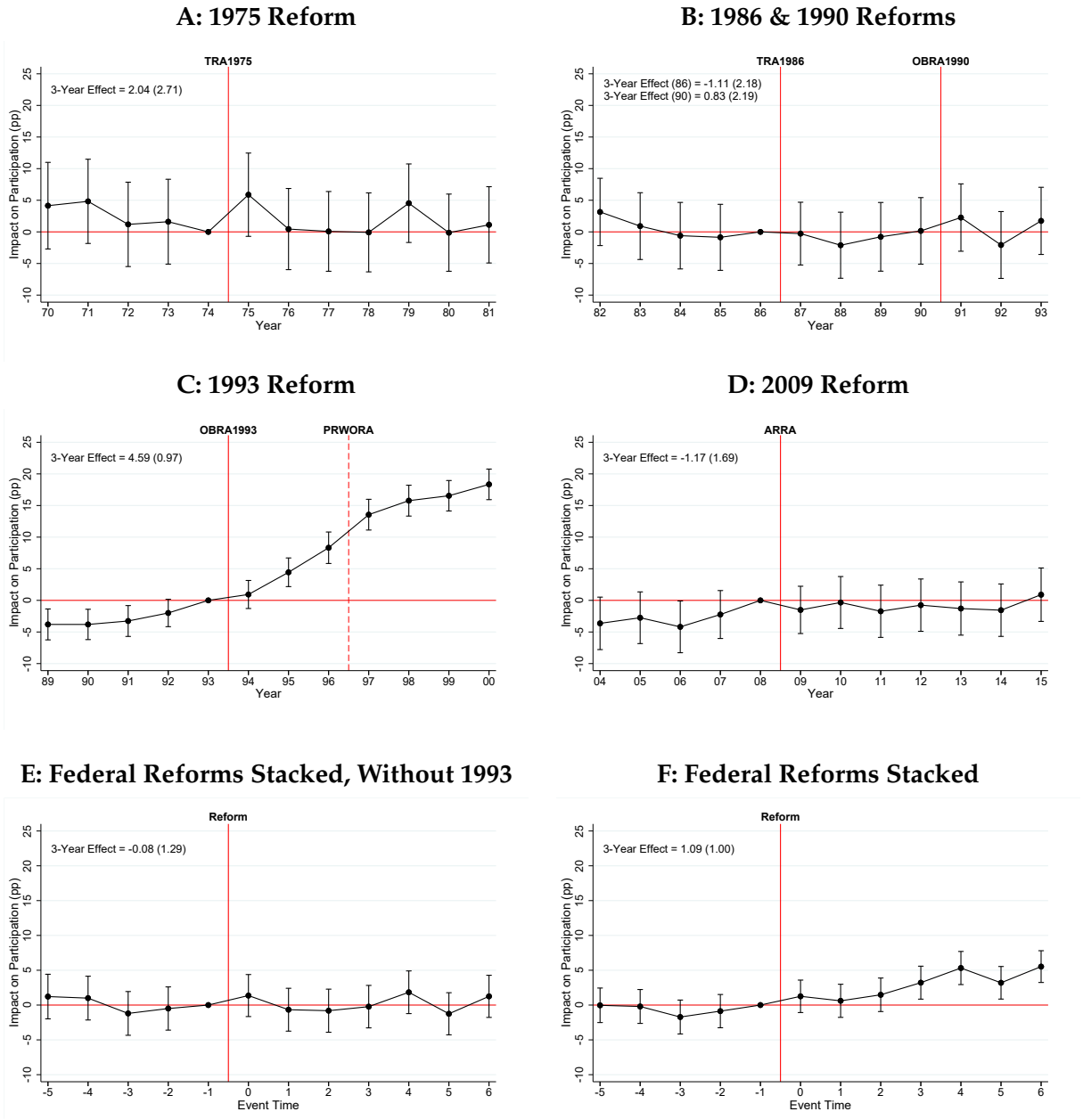
SINGLE WOMEN IN BOTTOM QUARTILE OF PREDICTED EARNINGS, WEEKLY PARTICIPATION
RAW DATA



Notes: This figure is constructed exactly as Figure 5 in the main text, except for the specification of control variables, estimation sample, and the extensive margin measure. The figure is based on specification (2) without any controls, a sample of single women with low predicted earnings, and using weekly participation as the extensive margin measure. The low-earnings sample includes single women in the bottom quartile of the within-year distribution of predicted earnings, estimated using equation (1).

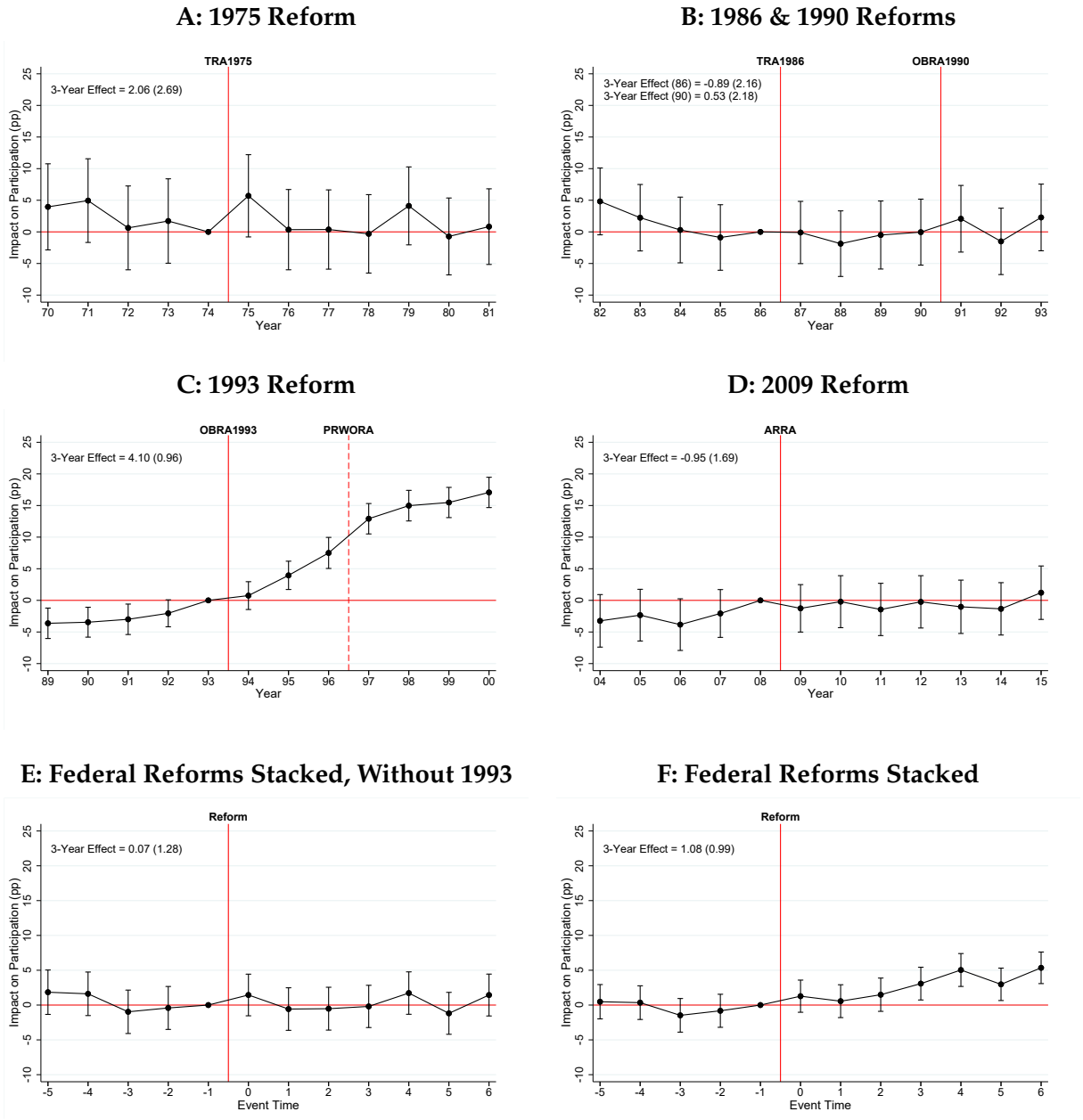
FIGURE A.62: DID EVENT STUDIES OF FEDERAL EITC REFORMS

SINGLE WOMEN IN BOTTOM QUARTILE OF PREDICTED EARNINGS, WEEKLY PARTICIPATION
CONTROLS FOR BASIC DEMOGRAPHICS



Notes: This figure is constructed exactly as Figure 5 in the main text, except for the specification of control variables, estimation sample, and the extensive margin measure. The figure is based on specification (2) with controls for basic demographics only, a sample of single women with low predicted earnings, and using weekly participation as the extensive margin measure. The low-earnings sample includes single women in the bottom quartile of the within-year distribution of predicted earnings, estimated using equation (1).

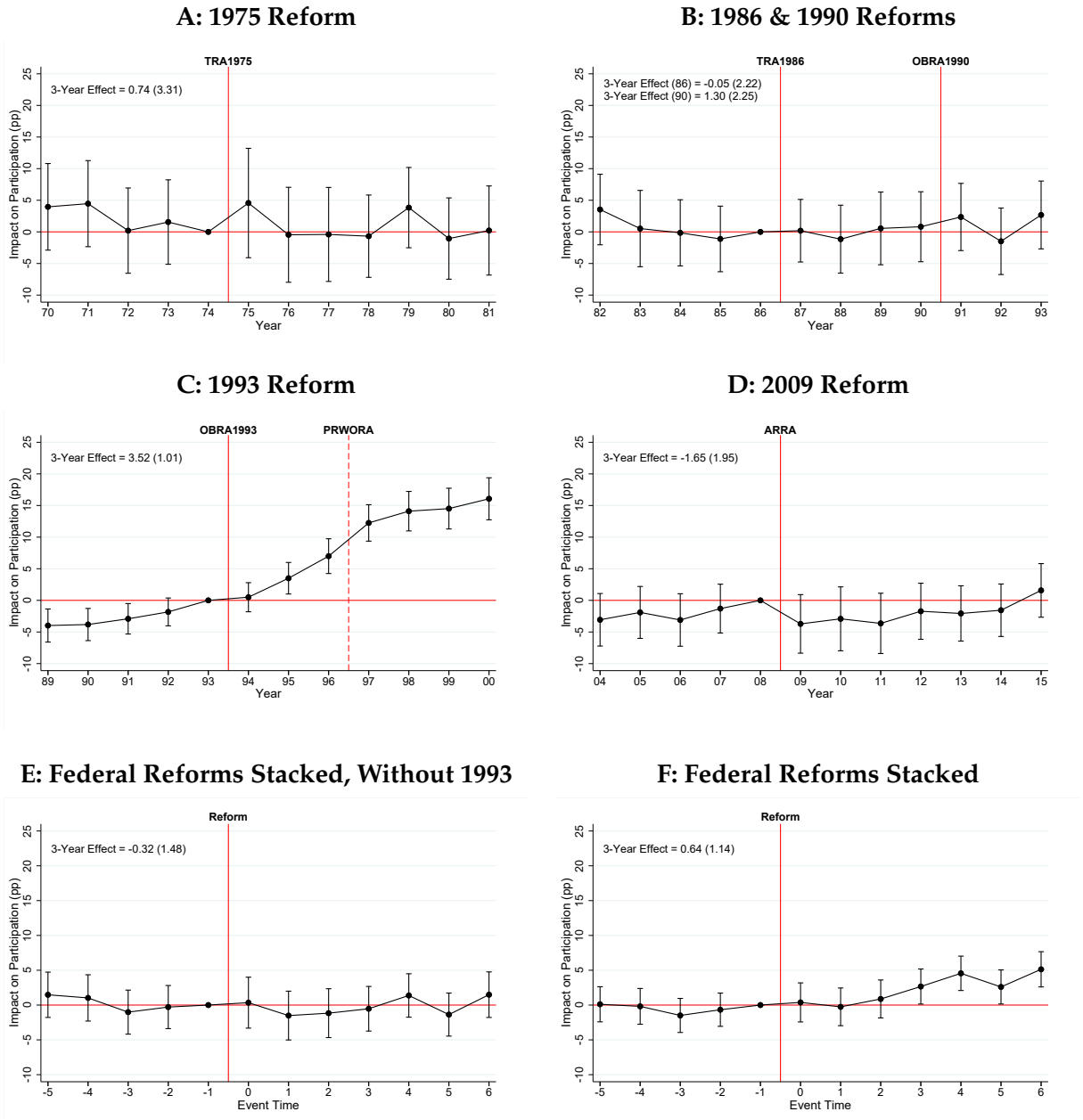
FIGURE A.63: DID EVENT STUDIES OF FEDERAL EITC REFORMS
SINGLE WOMEN IN BOTTOM QUARTILE OF PREDICTED EARNINGS, WEEKLY PARTICIPATION
CONTROLS FOR RICH DEMOGRAPHICS



Notes: This figure is constructed exactly as Figure 5 in the main text, except for the specification of control variables, estimation sample, and the extensive margin measure. The figure is based on specification (2) with controls for rich demographics only, a sample of single women with low predicted earnings, and using weekly participation as the extensive margin measure. The low-earnings sample includes single women in the bottom quartile of the within-year distribution of predicted earnings, estimated using equation (1).

FIGURE A.64: DID EVENT STUDIES OF FEDERAL EITC REFORMS

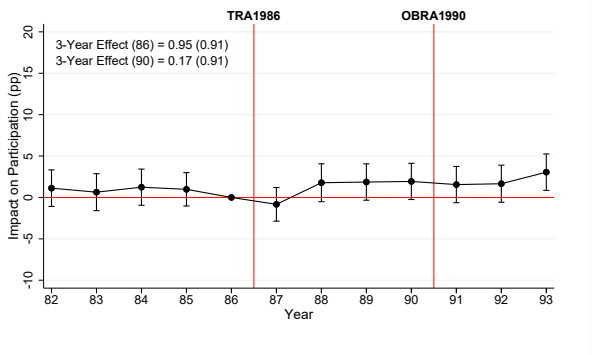
SINGLE WOMEN IN BOTTOM QUARTILE OF PREDICTED EARNINGS, WEEKLY PARTICIPATION
CONTROLS FOR RICH DEMOGRAPHICS AND UNEMPLOYMENT



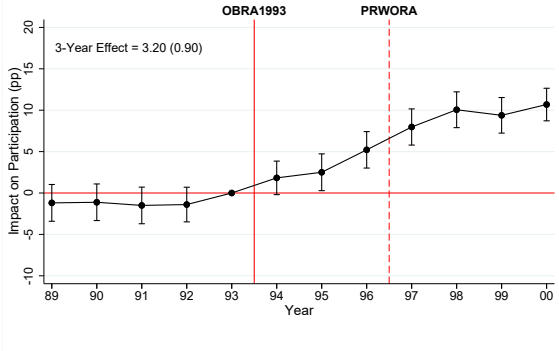
Notes: This figure is constructed exactly as Figure 5 in the main text, except for the estimation sample, and the extensive margin measure. The figure is based on specification (2) with controls for rich demographics and unemployment, a sample of single women with low predicted earnings, and using weekly participation as the extensive margin measure. The low-earnings sample includes single women in the bottom quartile of the within-year distribution of predicted earnings, estimated using equation (1).

FIGURE A.65: DID EVENT STUDIES OF FEDERAL EITC REFORMS
ALL SINGLE WOMEN, ANNUAL PARTICIPATION
RAW DATA

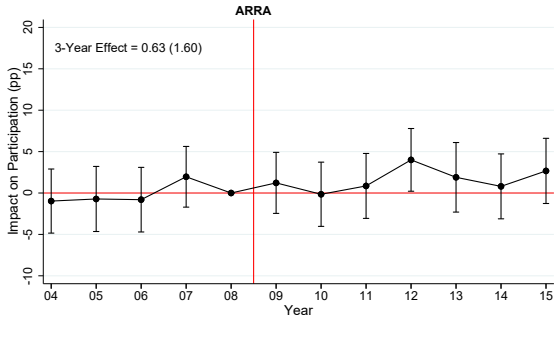
A: 1986 & 1990 Reforms



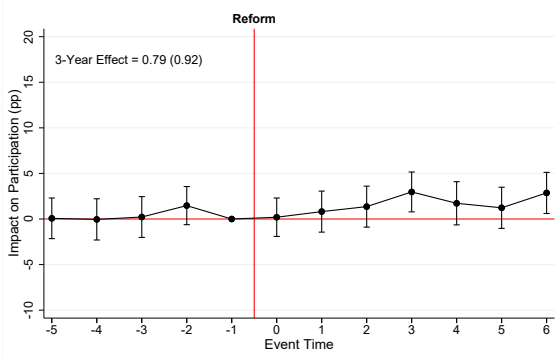
B: 1993 Reform



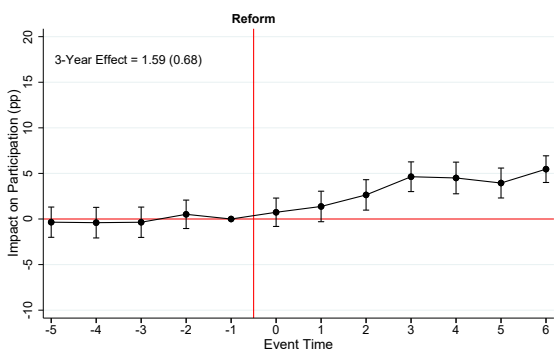
C: 2009 Reform



D: Federal Reforms Stacked, Without 1993



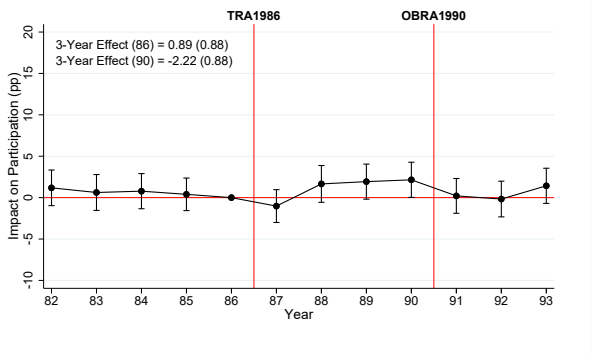
E: Federal Reforms Stacked



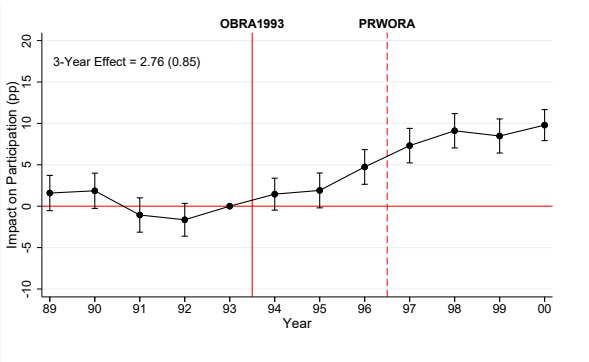
Notes: This figure is constructed exactly as Figure 5 in the main text, except for the specification of control variables and the extensive margin measure. The figure is based on specification (2) without any controls, the sample of all single women, and using annual participation as the extensive margin measure.

FIGURE A.66: DID EVENT STUDIES OF FEDERAL EITC REFORMS
ALL SINGLE WOMEN, ANNUAL PARTICIPATION
CONTROLS FOR BASIC DEMOGRAPHICS

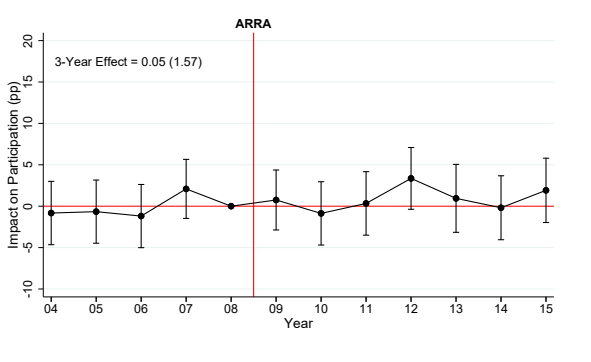
A: 1986 & 1990 Reforms



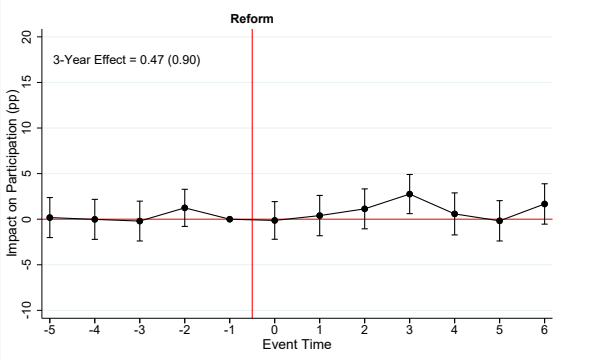
B: 1993 Reform



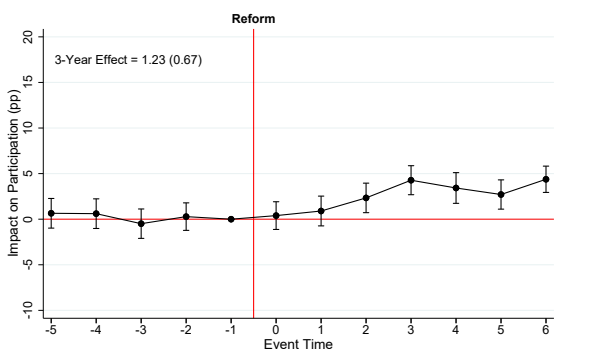
C: 2009 Reform



D: Federal Reforms Stacked, Without 1993



E: Federal Reforms Stacked



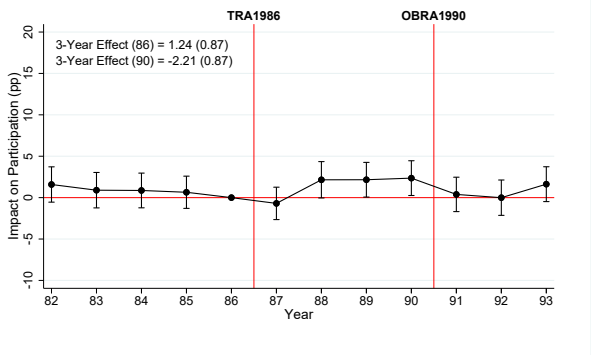
Notes: This figure is constructed exactly as Figure 5 in the main text, except for the specification of control variables and the extensive margin measure. The figure is based on specification (2) with controls for basic demographics only, the sample of all single women, and using annual participation as the extensive margin measure.

FIGURE A.67: DID EVENT STUDIES OF FEDERAL EITC REFORMS

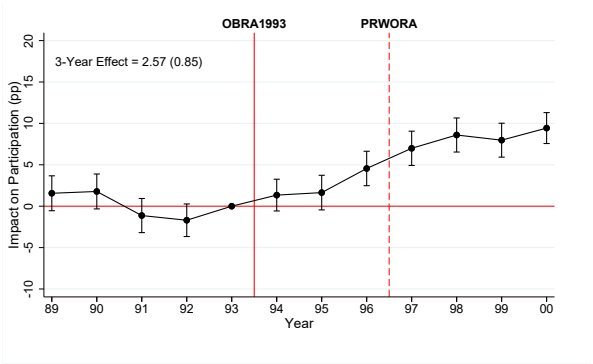
ALL SINGLE WOMEN, ANNUAL PARTICIPATION

CONTROLS FOR RICH DEMOGRAPHICS

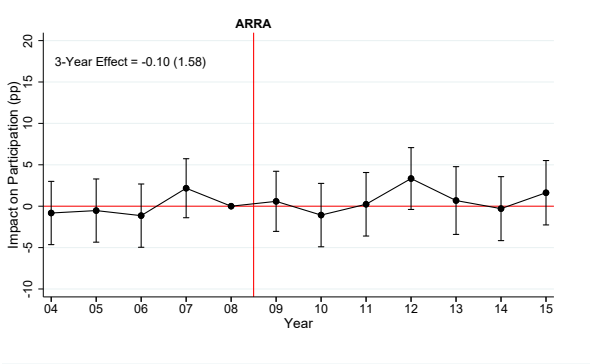
A: 1986 & 1990 Reforms



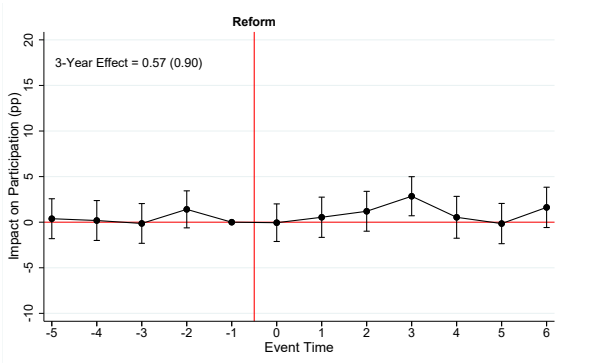
B: 1993 Reform



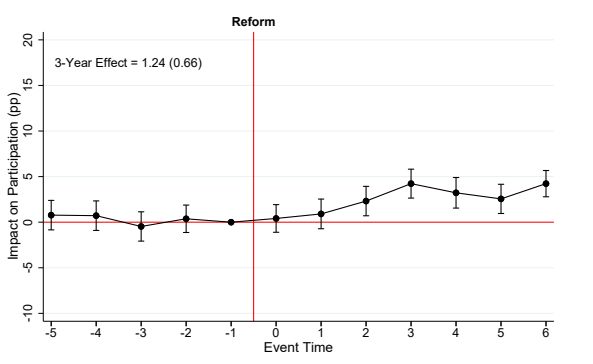
C: 2009 Reform



D: Federal Reforms Stacked, Without 1993



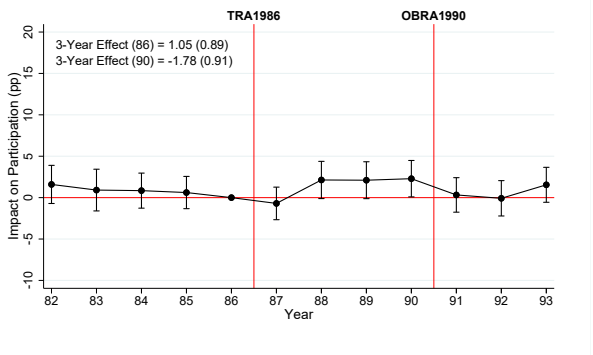
E: Federal Reforms Stacked



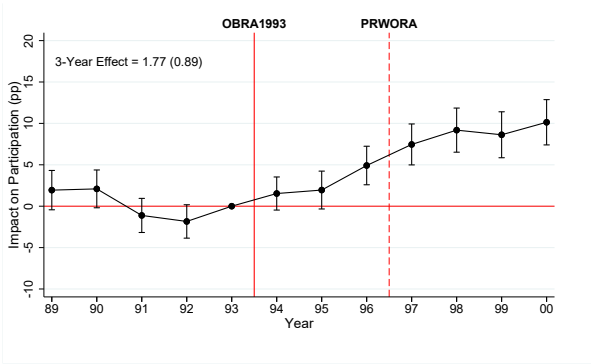
Notes: This figure is constructed exactly as Figure 5 in the main text, except for the specification of control variables and the extensive margin measure. The figure is based on specification (2) with controls for rich demographics only, the sample of all single women, and using annual participation as the extensive margin measure.

FIGURE A.68: DID EVENT STUDIES OF FEDERAL EITC REFORMS
ALL SINGLE WOMEN, ANNUAL PARTICIPATION
CONTROLS FOR RICH DEMOGRAPHICS AND UNEMPLOYMENT

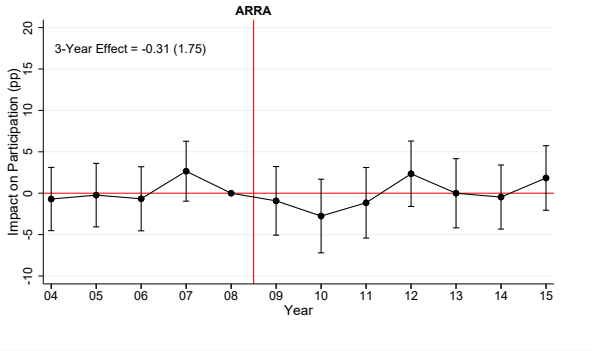
A: 1986 & 1990 Reforms



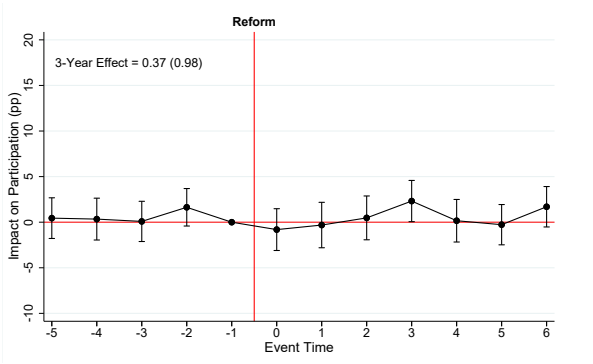
B: 1993 Reform



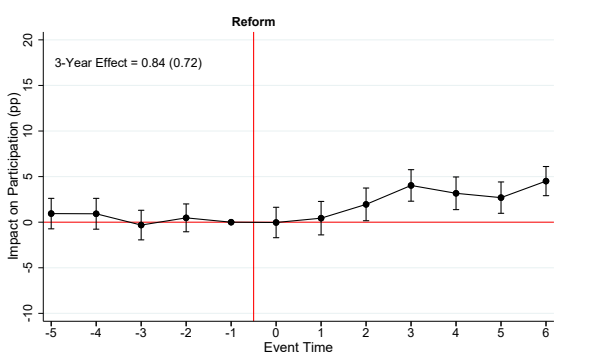
C: 2009 Reform



D: Federal Reforms Stacked, Without 1993



E: Federal Reforms Stacked



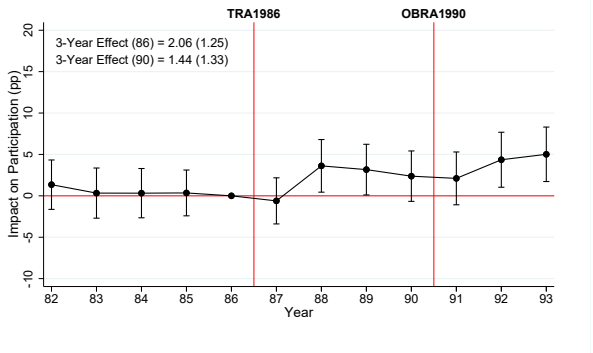
Notes: This figure is constructed exactly as Figure 5 in the main text, except for the extensive margin measure. The figure is based on specification (2) with controls for rich demographics and unemployment, the sample of all single women, and using annual participation as the extensive margin measure.

FIGURE A.69: DID EVENT STUDIES OF FEDERAL EITC REFORMS

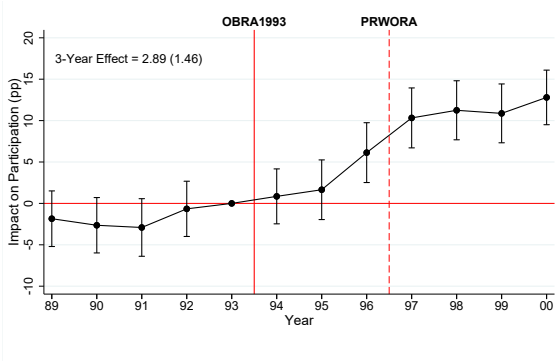
LOW-EDUCATED SINGLE WOMEN, ANNUAL PARTICIPATION

RAW DATA

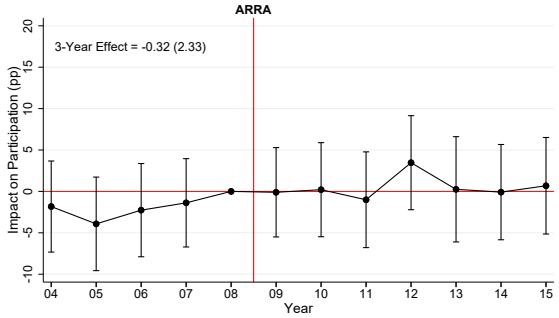
A: 1986 & 1990 Reforms



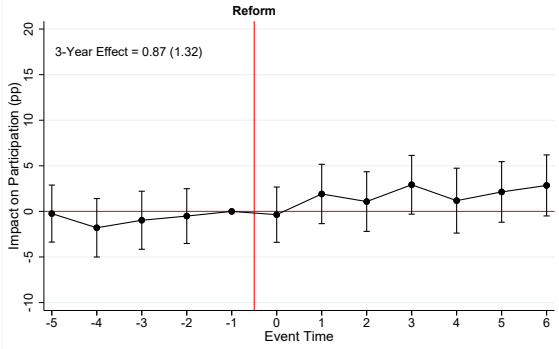
B: 1993 Reform



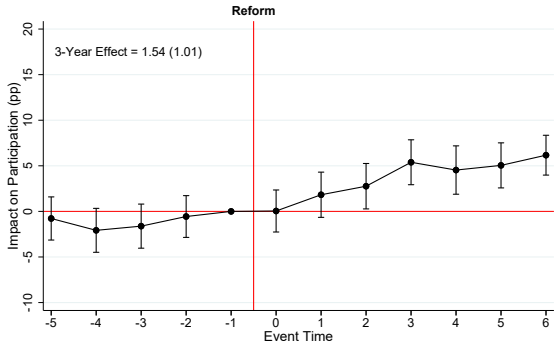
C: 2009 Reform



D: Federal Reforms Stacked, Without 1993



E: Federal Reforms Stacked



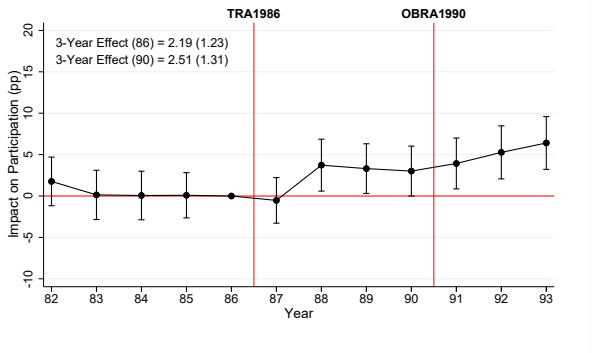
Notes: This figure is constructed exactly as Figure 5 in the main text, except for the specification of control variables, estimation sample, and the extensive margin measure. The figure is based on specification (2) without any controls, the sample of low-educated single women (those with a high school degree or less), and using annual participation as the extensive margin measure.

FIGURE A.70: DID EVENT STUDIES OF FEDERAL EITC REFORMS

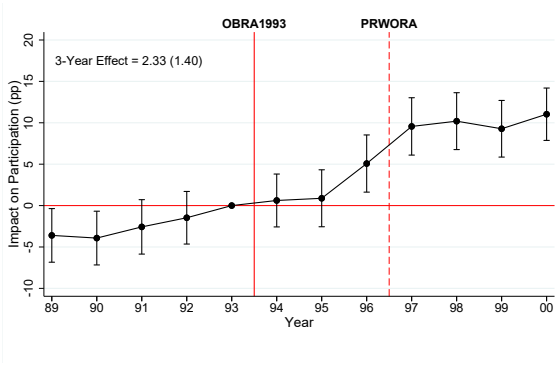
LOW-EDUCATED SINGLE WOMEN, ANNUAL PARTICIPATION

CONTROLS FOR BASIC DEMOGRAPHICS

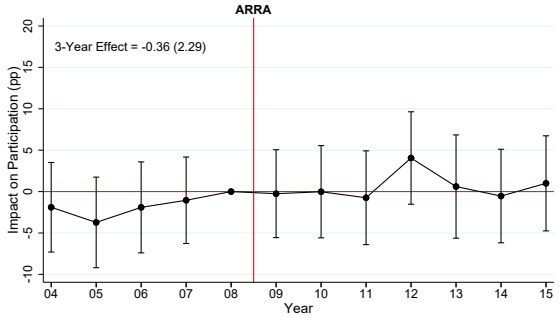
A: 1986 & 1990 Reforms



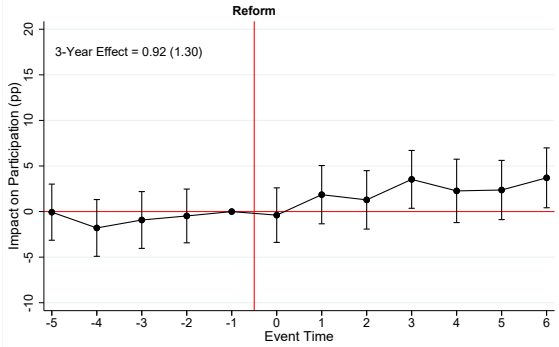
B: 1993 Reform



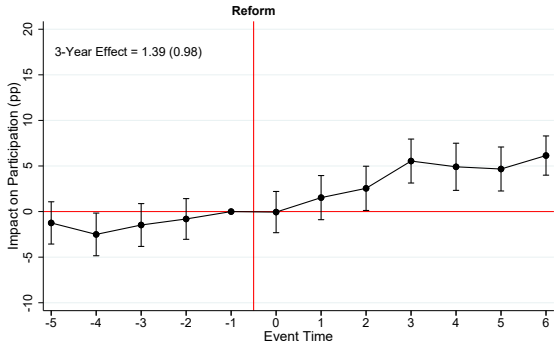
C: 2009 Reform



D: Federal Reforms Stacked, Without 1993



E: Federal Reforms Stacked



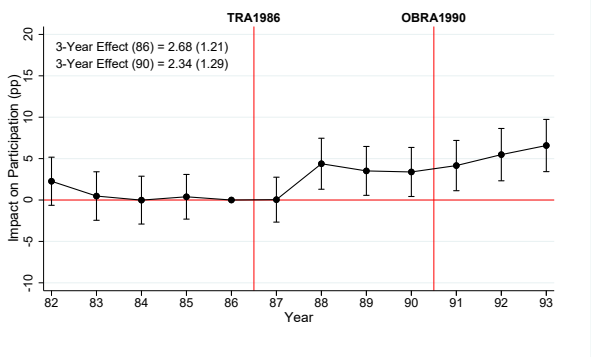
Notes: This figure is constructed exactly as Figure 5 in the main text, except for the specification of control variables, estimation sample, and the extensive margin measure. The figure is based on specification (2) with controls for basic demographics only, the sample of low-educated single women (those with a high school degree or less), and using annual participation as the extensive margin measure.

FIGURE A.71: DID EVENT STUDIES OF FEDERAL EITC REFORMS

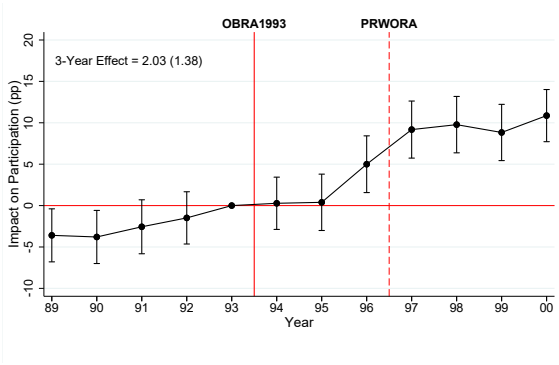
LOW-EDUCATED SINGLE WOMEN, ANNUAL PARTICIPATION

CONTROLS FOR RICH DEMOGRAPHICS

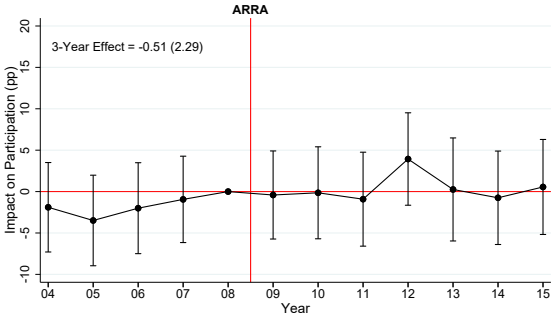
A: 1986 & 1990 Reforms



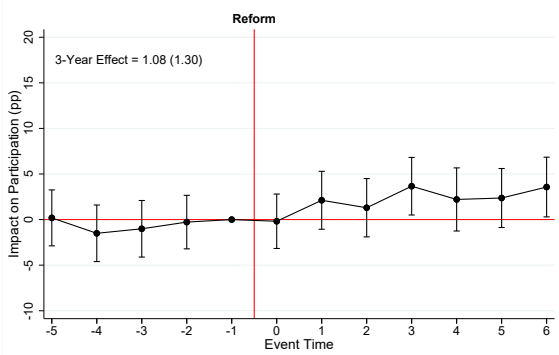
B: 1993 Reform



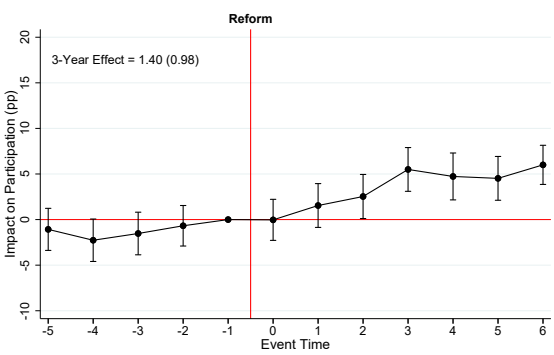
C: 2009 Reform



D: Federal Reforms Stacked, Without 1993



E: Federal Reforms Stacked

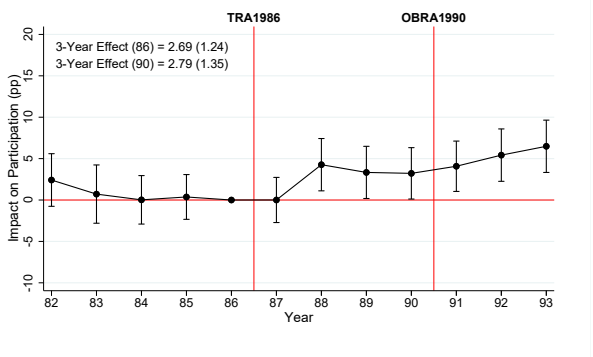


Notes: This figure is constructed exactly as Figure 5 in the main text, except for the specification of control variables, estimation sample, and the extensive margin measure. The figure is based on specification (2) with controls for rich demographics only, the sample of low-educated single women (those with a high school degree or less), and using annual participation as the extensive margin measure.

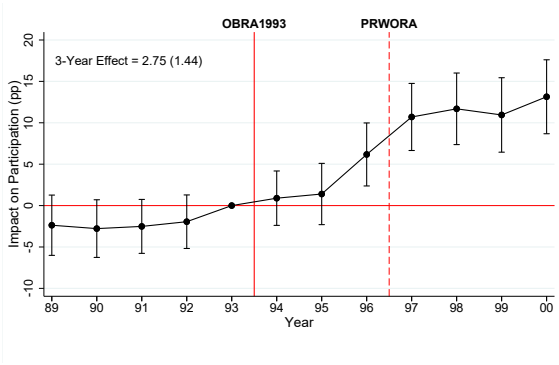
FIGURE A.72: DID EVENT STUDIES OF FEDERAL EITC REFORMS

LOW-EDUCATED SINGLE WOMEN, ANNUAL PARTICIPATION
CONTROLS FOR RICH DEMOGRAPHICS AND UNEMPLOYMENT

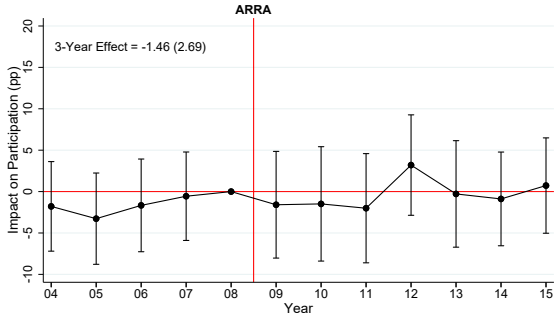
A: 1986 & 1990 Reforms



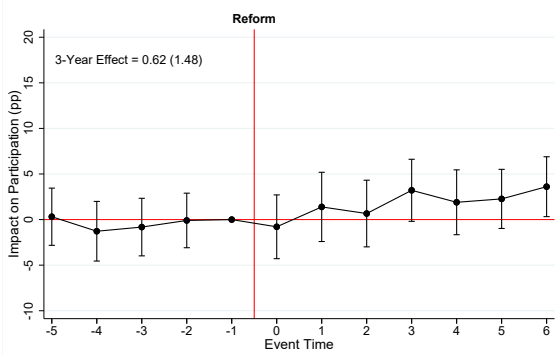
B: 1993 Reform



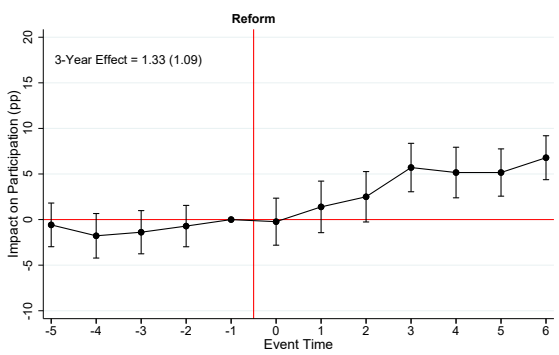
C: 2009 Reform



D: Federal Reforms Stacked, Without 1993



E: Federal Reforms Stacked



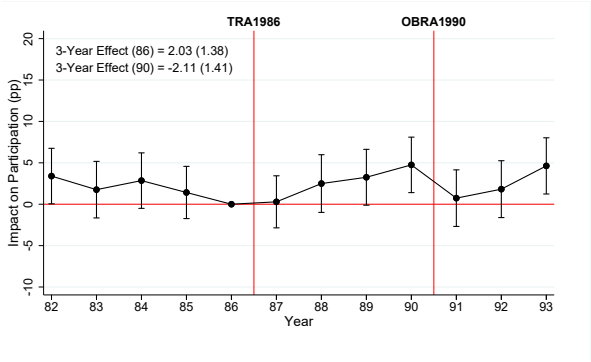
Notes: This figure is constructed exactly as Figure 5 in the main text, except for the estimation sample and the extensive margin measure. The figure is based on specification (2) with controls for rich demographics and unemployment, the sample of low-educated single women (those with a high school degree or less), and using annual participation as the extensive margin measure.

FIGURE A.73: DID EVENT STUDIES OF FEDERAL EITC REFORMS

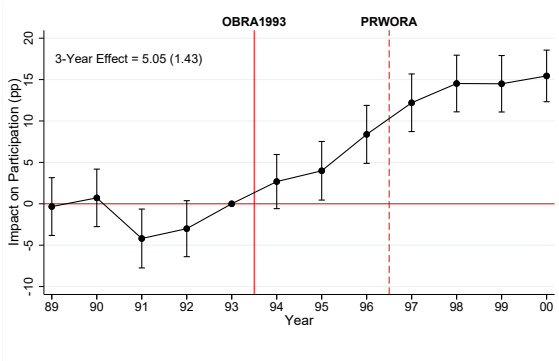
SINGLE WOMEN IN BOTTOM HALF OF PREDICTED EARNINGS, ANNUAL PARTICIPATION

RAW DATA

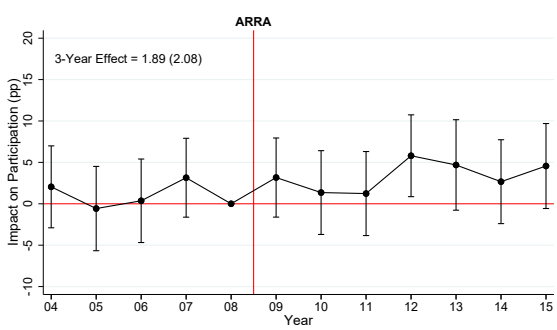
A: 1986 & 1990 Reforms



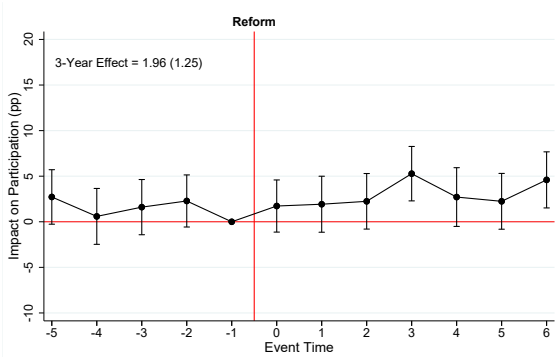
B: 1993 Reform



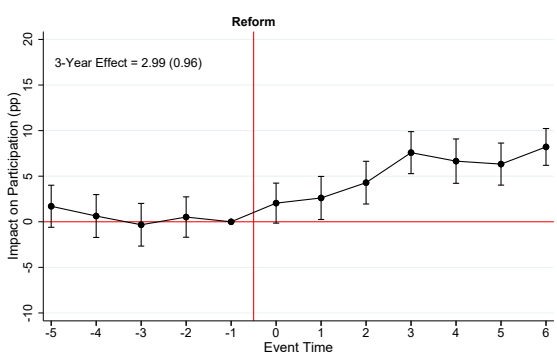
C: 2009 Reform



D: Federal Reforms Stacked, Without 1993



E: Federal Reforms Stacked

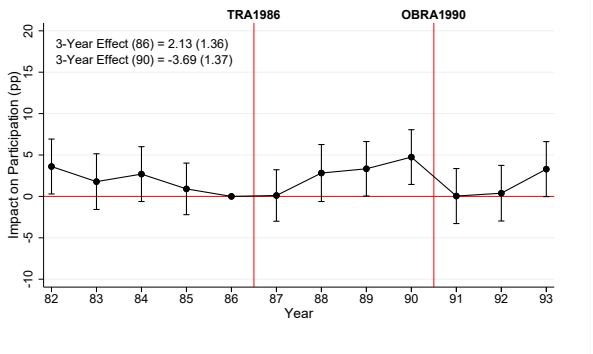


Notes: This figure is constructed exactly as Figure 5 in the main text, except for the specification of control variables, estimation sample, and the extensive margin measure. The figure is based on specification (2) without any controls, a sample of single women with low predicted earnings, and using annual participation as the extensive margin measure. The low-earnings sample includes single women in the bottom half of the within-year distribution of predicted earnings, estimated using equation (1).

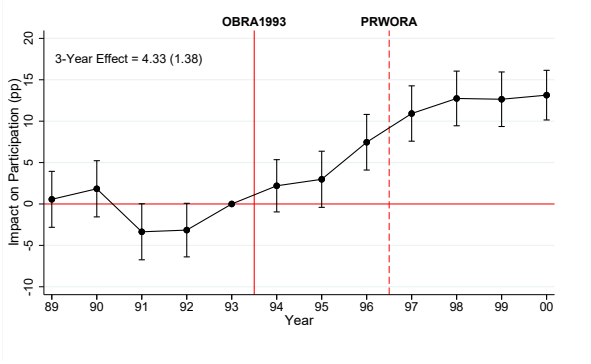
FIGURE A.74: DID EVENT STUDIES OF FEDERAL EITC REFORMS

SINGLE WOMEN IN BOTTOM HALF OF PREDICTED EARNINGS, ANNUAL PARTICIPATION
CONTROLS FOR BASIC DEMOGRAPHICS

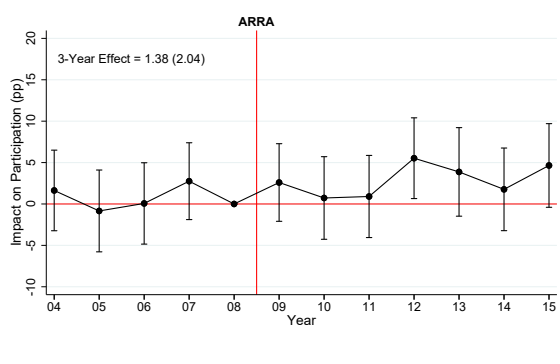
A: 1986 & 1990 Reforms



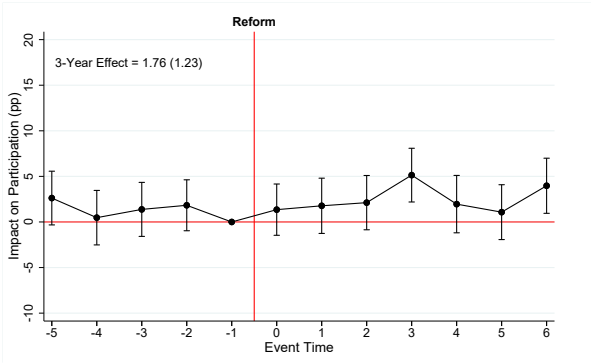
B: 1993 Reform



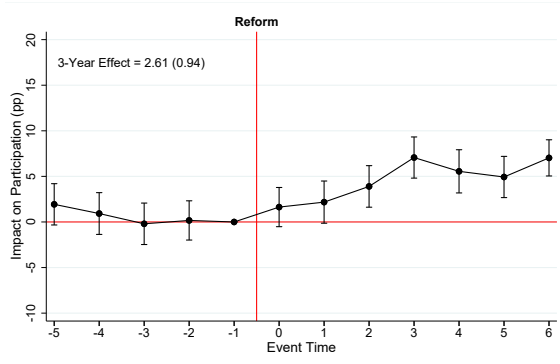
C: 2009 Reform



D: Federal Reforms Stacked, Without 1993



E: Federal Reforms Stacked

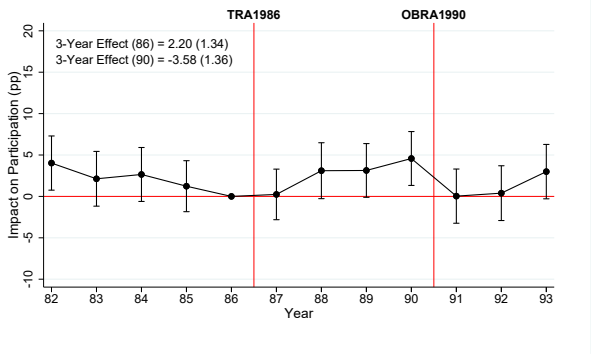


Notes: This figure is constructed exactly as Figure 5 in the main text, except for the specification of control variables, estimation sample, and the extensive margin measure. The figure is based on specification (5) with controls for basic demographics only, a sample of single women with low predicted earnings, and using annual participation as the extensive margin measure. The low-earnings sample includes single women in the bottom half of the within-year distribution of predicted earnings, estimated using equation (1).

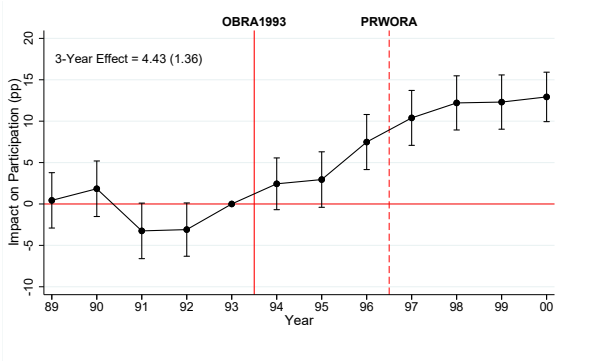
FIGURE A.75: DID EVENT STUDIES OF FEDERAL EITC REFORMS

SINGLE WOMEN IN BOTTOM HALF OF PREDICTED EARNINGS, ANNUAL PARTICIPATION
CONTROLS FOR RICH DEMOGRAPHICS

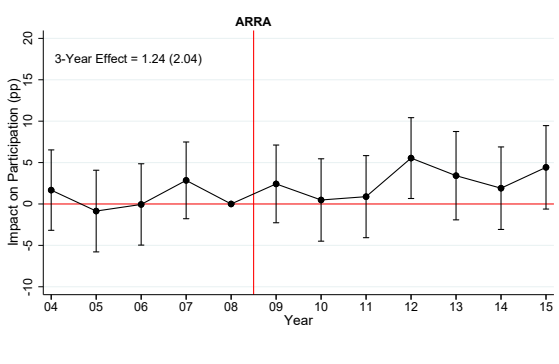
A: 1986 & 1990 Reforms



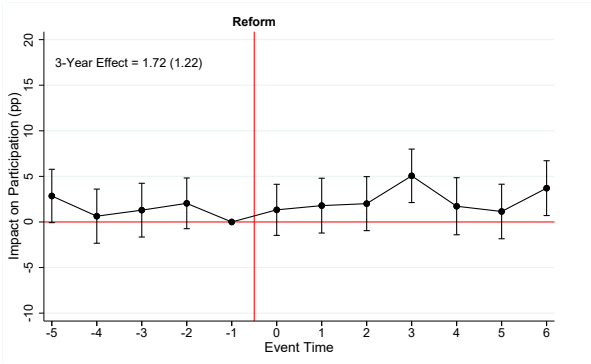
B: 1993 Reform



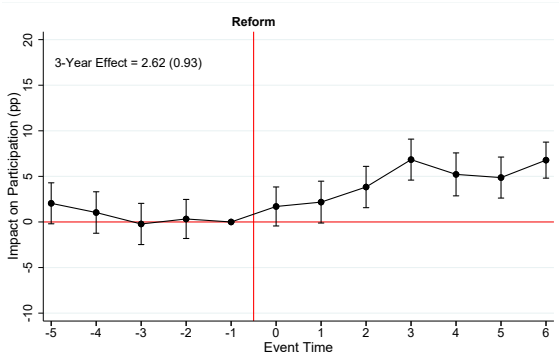
C: 2009 Reform



D: Federal Reforms Stacked, Without 1993



E: Federal Reforms Stacked

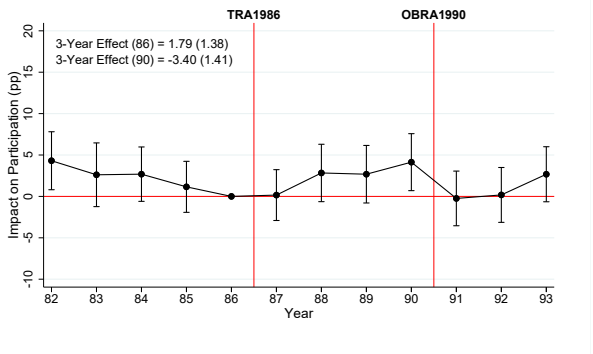


Notes: This figure is constructed exactly as Figure 5 in the main text, except for the specification of control variables, estimation sample, and the extensive margin measure. The figure is based on specification (2) with controls for rich demographics only, a sample of single women with low predicted earnings, and using annual participation as the extensive margin measure. The low-earnings sample includes single women in the bottom half of the within-year distribution of predicted earnings, estimated using equation (1).

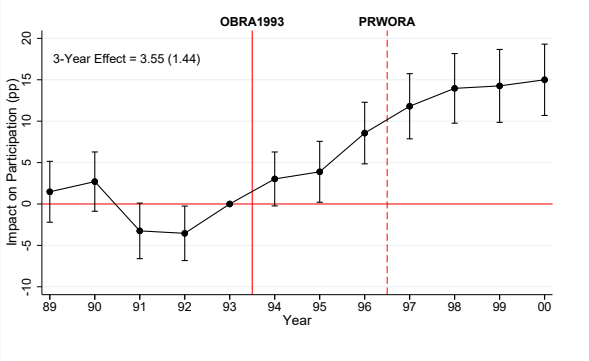
Figure A.76: DiD Event Studies of Federal EITC Reforms

Single Women in Bottom Half of Predicted Earnings, Annual Participation
Controls for Rich Demographics and Unemployment

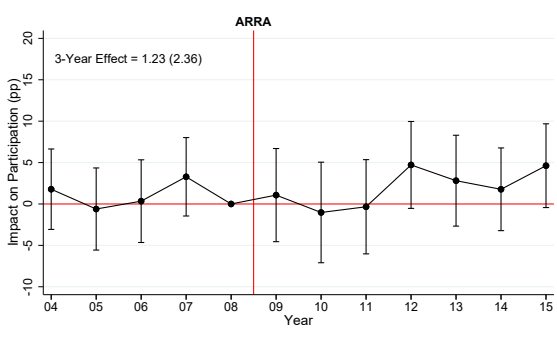
A: 1986 & 1990 Reforms



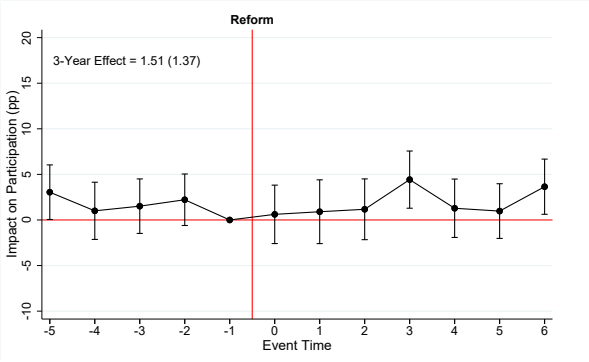
B: 1993 Reform



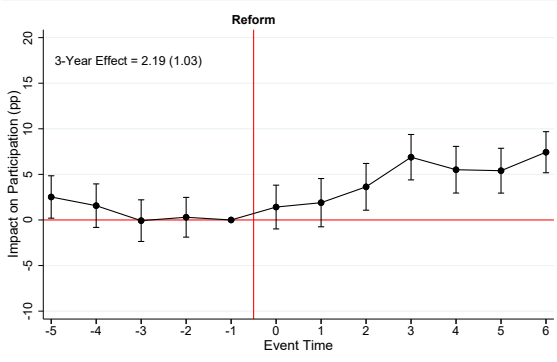
C: 2009 Reform



E: Federal Reforms Stacked, Without 1993



F: Federal Reforms Stacked

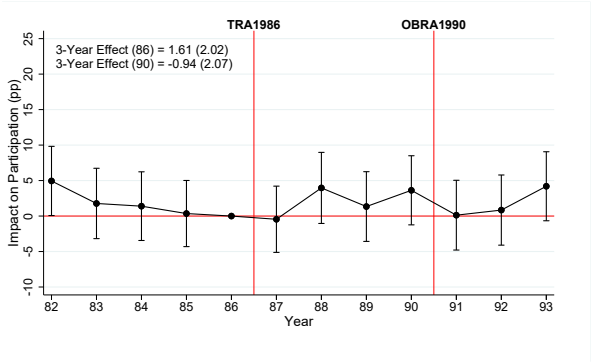


Notes: This figure is constructed exactly as Figure 5 in the main text, except for the estimation sample, and the extensive margin measure. The figure is based on specification (2) with controls for rich demographics and unemployment, a sample of single women with low predicted earnings, and using annual participation as the extensive margin measure. The low-earnings sample includes single women in the bottom half of the within-year distribution of predicted earnings, estimated using equation (1).

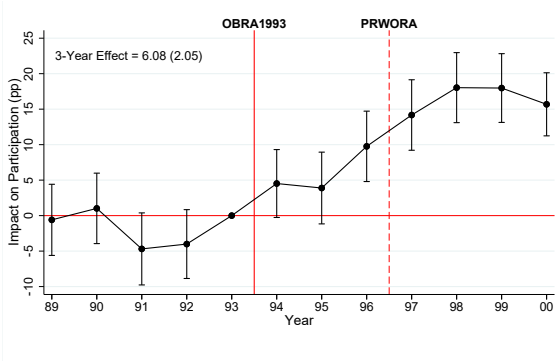
FIGURE A.77: DID EVENT STUDIES OF FEDERAL EITC REFORMS

SINGLE WOMEN IN BOTTOM QUARTILE OF PREDICTED EARNINGS, ANNUAL PARTICIPATION
RAW DATA

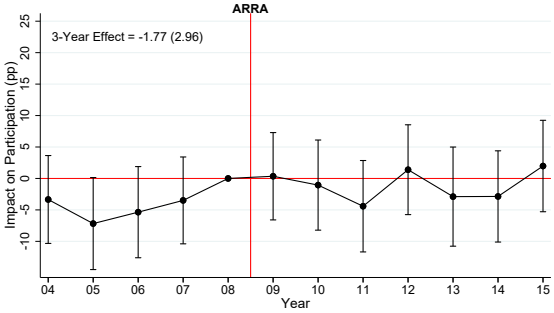
A: 1986 & 1990 Reforms



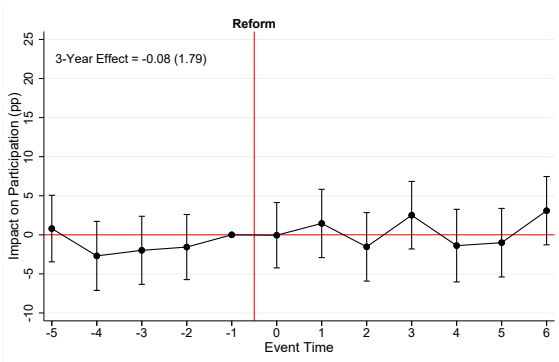
B: 1993 Reform



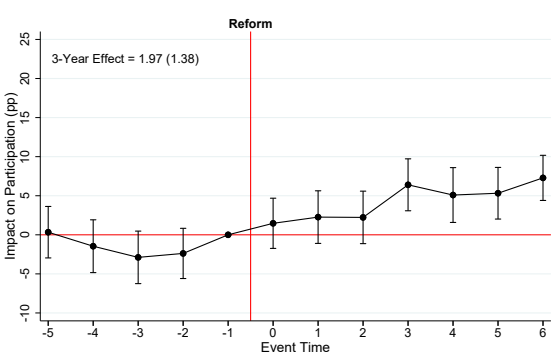
C: 2009 Reform



D: Federal Reforms Stacked, Without 1993



E: Federal Reforms Stacked

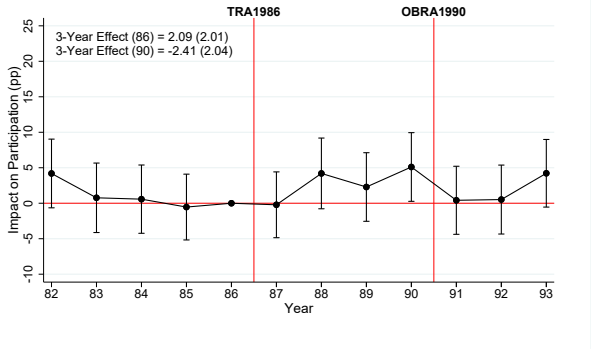


Notes: This figure is constructed exactly as Figure 5 in the main text, except for the specification of control variables, estimation sample, and the extensive margin measure. The figure is based on specification (2) without any controls, a sample of single women with low predicted earnings, and using annual participation as the extensive margin measure. The low-earnings sample includes single women in the bottom quartile of the within-year distribution of predicted earnings, estimated using equation (1).

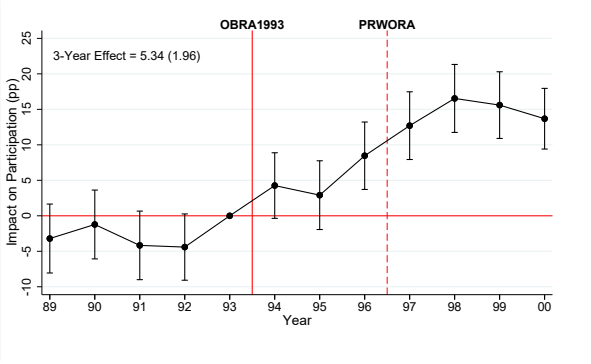
FIGURE A.78: DID EVENT STUDIES OF FEDERAL EITC REFORMS

SINGLE WOMEN IN BOTTOM QUARTILE OF PREDICTED EARNINGS, ANNUAL PARTICIPATION
CONTROLS FOR BASIC DEMOGRAPHICS

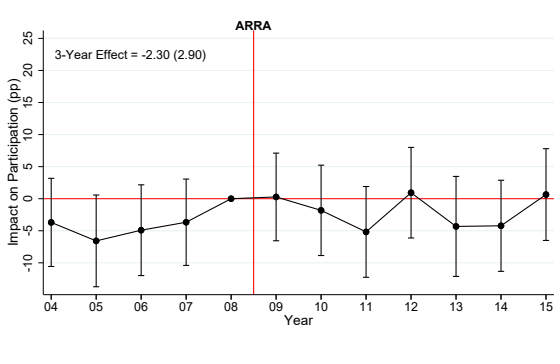
A: 1986 & 1990 Reforms



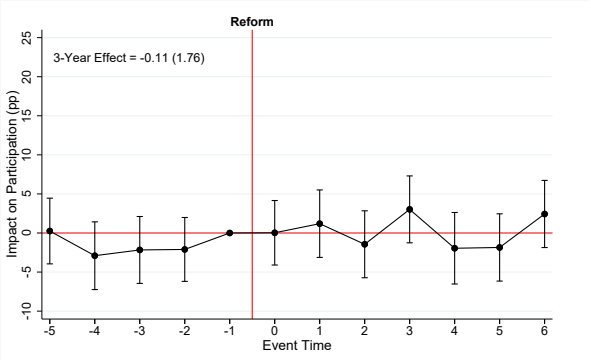
B: 1993 Reform



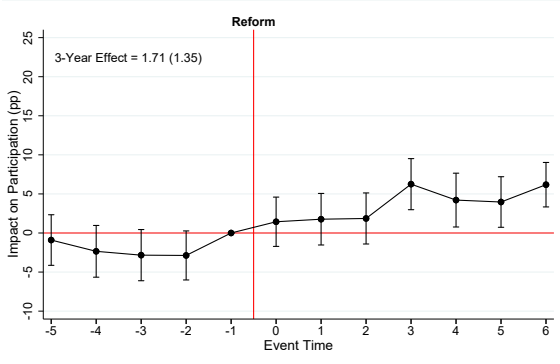
C: 2009 Reform



D: Federal Reforms Stacked, Without 1993



E: Federal Reforms Stacked

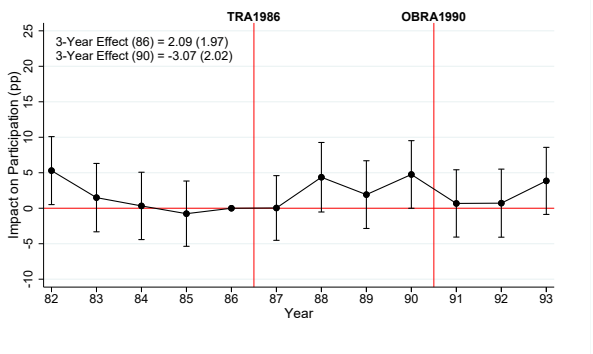


Notes: This figure is constructed exactly as Figure 5 in the main text, except for the specification of control variables, estimation sample, and the extensive margin measure. The figure is based on specification (2) with controls for basic demographics only, a sample of single women with low predicted earnings, and using annual participation as the extensive margin measure. The low-earnings sample includes single women in the bottom quartile of the within-year distribution of predicted earnings, estimated using equation (1).

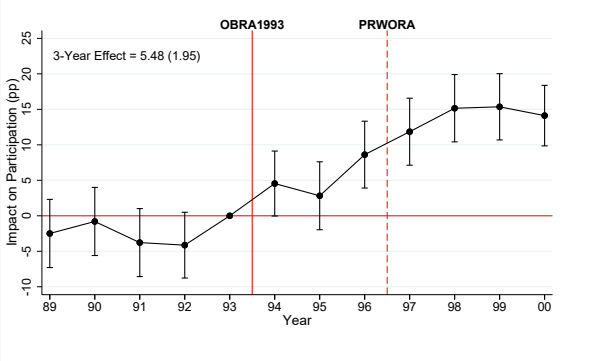
FIGURE A.79: DID EVENT STUDIES OF FEDERAL EITC REFORMS

SINGLE WOMEN IN BOTTOM QUARTILE OF PREDICTED EARNINGS, ANNUAL PARTICIPATION
CONTROLS FOR RICH DEMOGRAPHICS

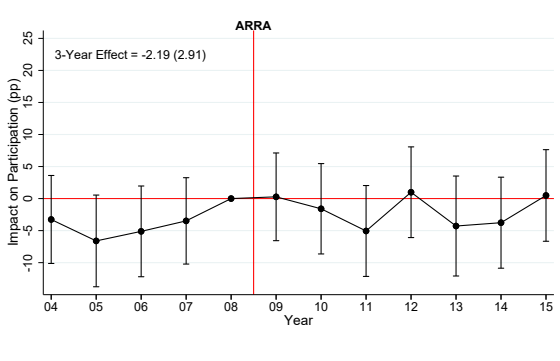
A: 1986 & 1990 Reforms



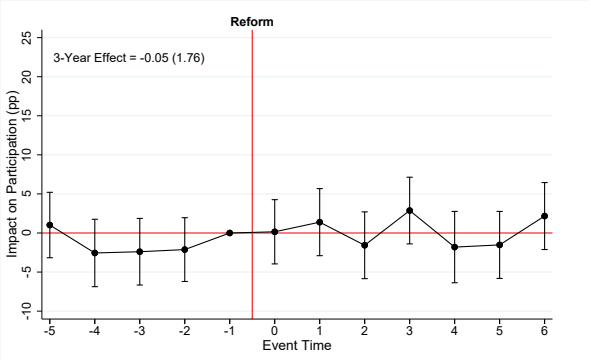
B: 1993 Reform



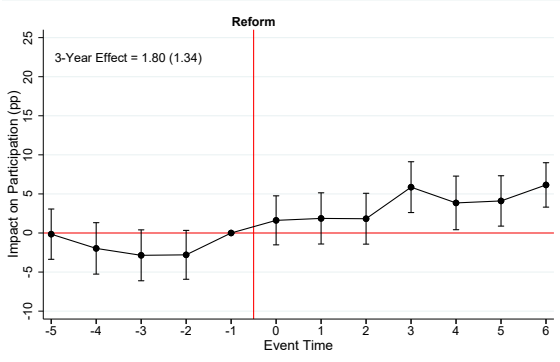
C: 2009 Reform



D: Federal Reforms Stacked, Without 1993



E: Federal Reforms Stacked

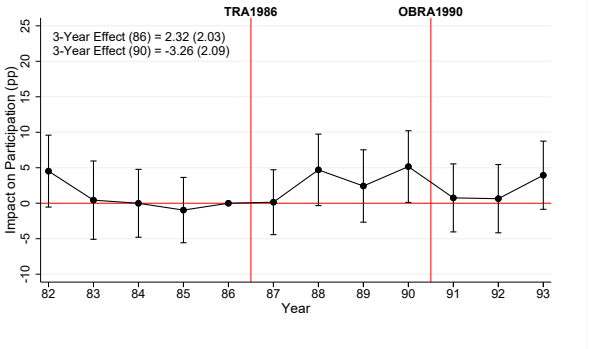


Notes: This figure is constructed exactly as Figure 5 in the main text, except for the specification of control variables, estimation sample and the extensive margin measure. The figure is based on specification (2) with controls for rich demographics only, a sample of single women with low predicted earnings, and using annual participation as the extensive margin measure. The low-earnings sample includes single women in the bottom quartile of the within-year distribution of predicted earnings, estimated using equation (1).

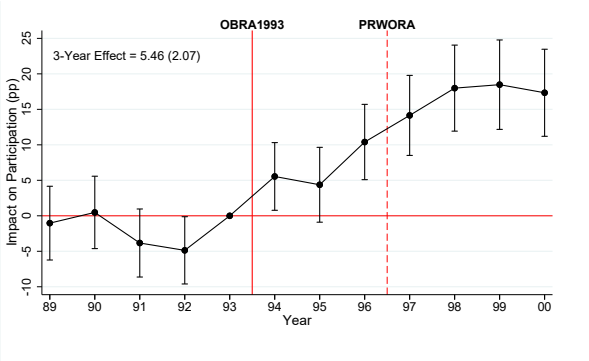
FIGURE A.80: DID EVENT STUDIES OF FEDERAL EITC REFORMS

SINGLE WOMEN IN BOTTOM QUARTILE OF PREDICTED EARNINGS, ANNUAL PARTICIPATION
CONTROLS FOR RICH DEMOGRAPHICS AND UNEMPLOYMENT

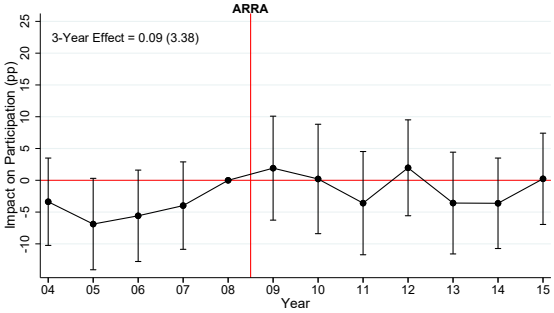
A: 1986 & 1990 Reforms



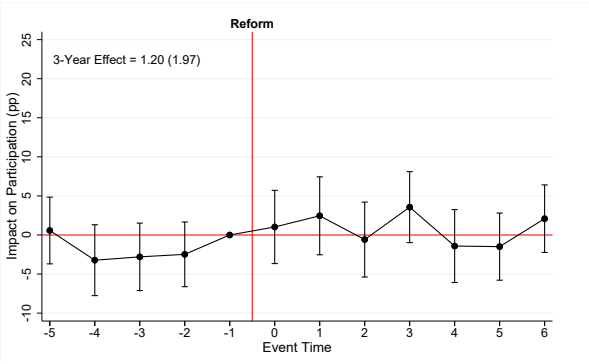
B: 1993 Reform



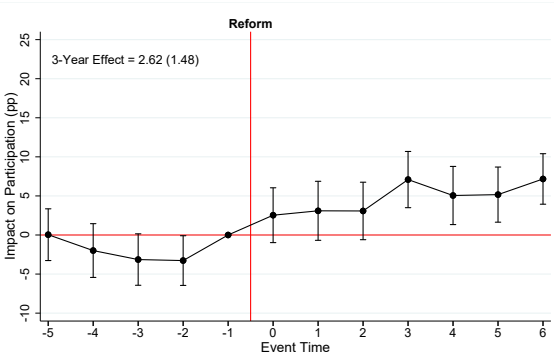
C: 2009 Reform



E: Federal Reforms Stacked, Without 1993



F: Federal Reforms Stacked



Notes: This figure is constructed exactly as Figure 5 in the main text, except for the estimation sample, and the extensive margin measure. The figure is based on specification (2) with controls for rich demographics and unemployment, a sample of single women with low predicted earnings, and using annual participation as the extensive margin measure. The low-earnings sample includes single women in the bottom quartile of the within-year distribution of predicted earnings, estimated using equation (1).

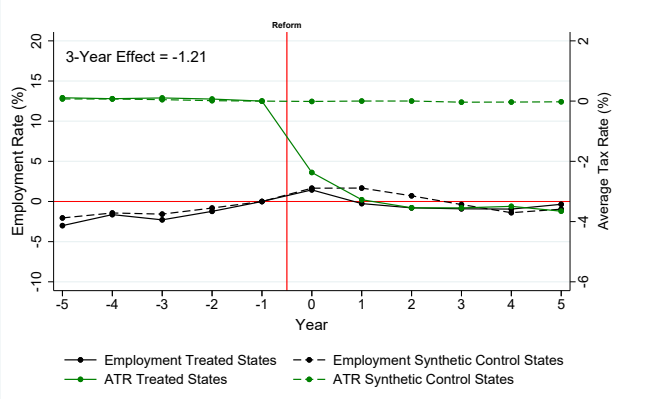
FIGURE A.81: SYNTHETIC CONTROL ANALYSIS OF STATE EITC REFORMS

WEEKLY EMPLOYMENT FOR ALL SINGLE WOMEN

All Reforms Stacked

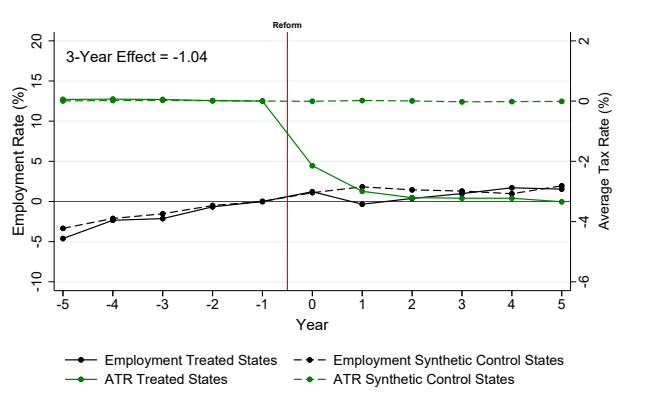
A: Difference-in-Differences

Treated vs Control States (With Kids)



B: Triple-Differences

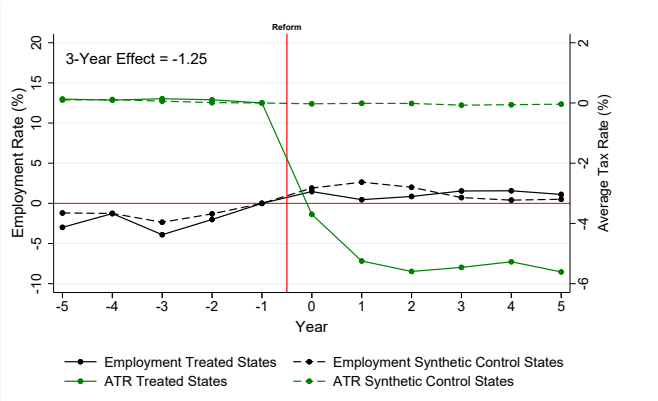
Treated vs Control States (With vs Without Kids)



Ten Largest Reforms Stacked

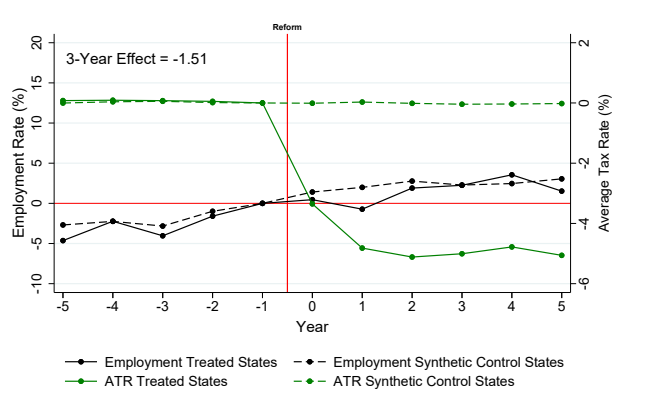
C: Difference-in-Differences

Treated vs Control States (With Kids)



D: Triple-Differences

Treated vs Control States (With vs Without Kids)

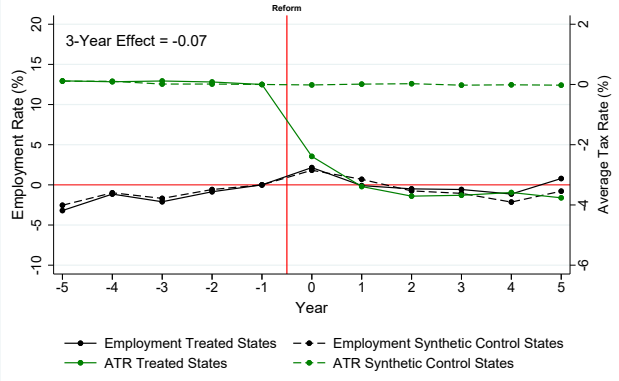


Notes: This figure is exactly the same as Figure 11 in the main text.

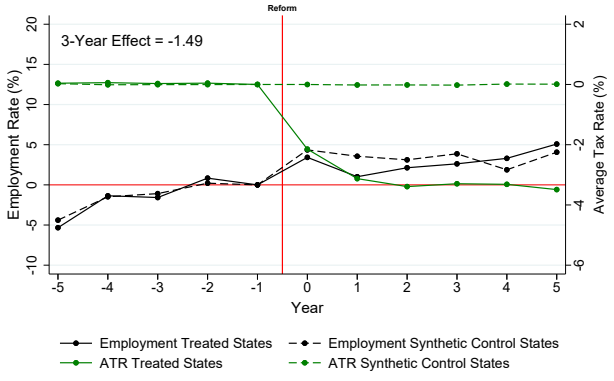
FIGURE A.82: SYNTHETIC CONTROL ANALYSIS OF STATE EITC REFORMS
WEEKLY EMPLOYMENT FOR LOW-EDUCATED SINGLE WOMEN

All Reforms Stacked

A: Difference-in-Differences
Treated vs Control States (With Kids)

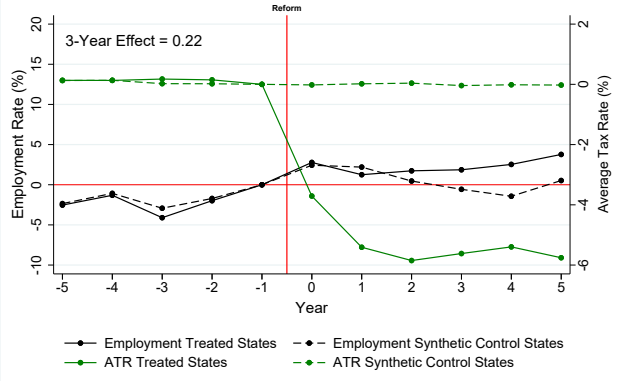


B: Triple-Differences
Treated vs Control States (With vs Without Kids)

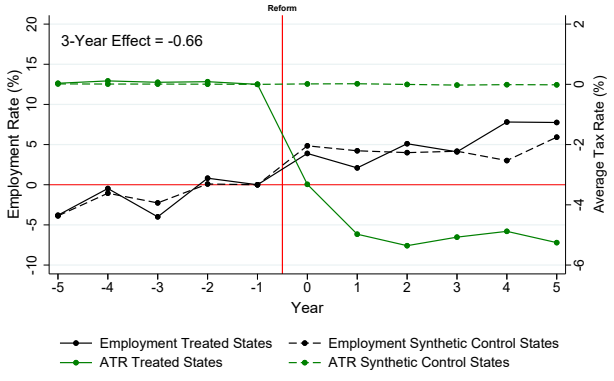


Ten Largest Reforms Stacked

C: Difference-in-Differences
Treated vs Control States (With Kids)



D: Triple-Differences
Treated vs Control States (With vs Without Kids)

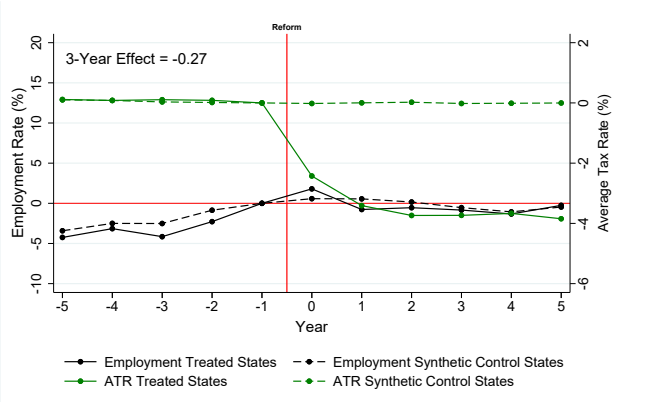


Notes: This figure is constructed exactly as Figure 11 in the main text, except for the estimation sample. In this figure, the sample is low-educated single women (those with a high school degree or less) and the extensive margin measure is weekly employment.

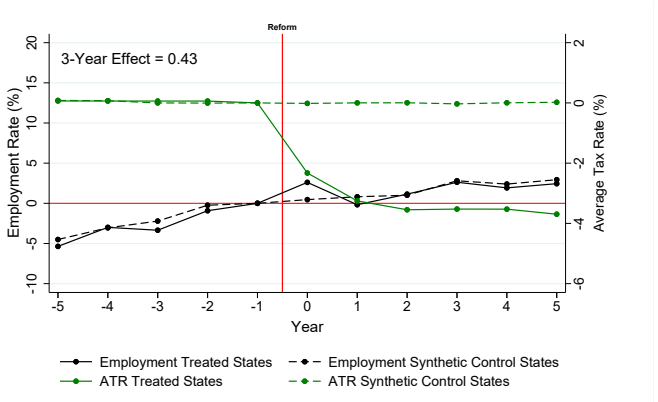
FIGURE A.83: SYNTHETIC CONTROL ANALYSIS OF STATE EITC REFORMS
WEEKLY EMPLOYMENT FOR SINGLE WOMEN IN BOTTOM HALF OF PREDICTED EARNINGS

All Reforms Stacked

A: Difference-in-Differences
Treated vs Control States (With Kids)

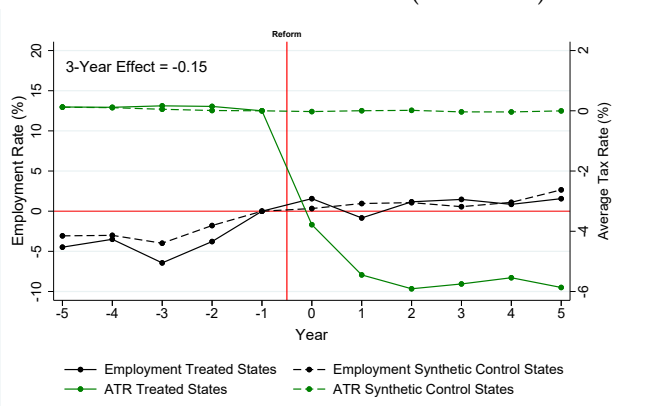


B: Triple-Differences
Treated vs Control States (With vs Without Kids)

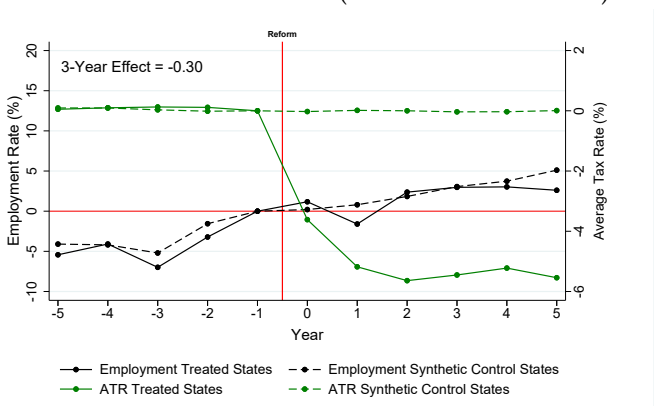


Ten Largest Reforms Stacked

C: Difference-in-Differences
Treated vs Control States (With Kids)



D: Triple-Differences
Treated vs Control States (With vs Without Kids)

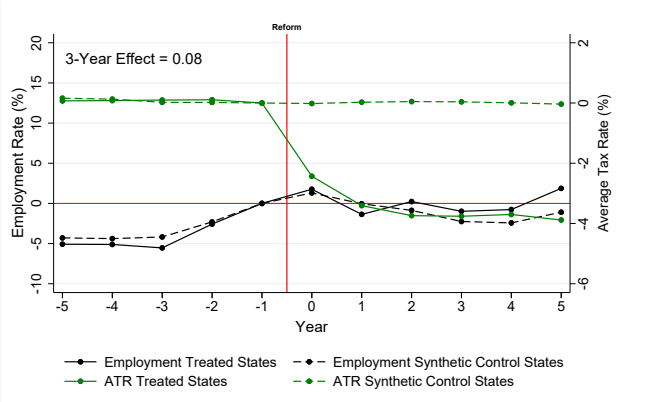


Notes: This figure is constructed exactly as Figure 11 in the main text, except for the estimation sample. In this figure, the sample is single women with low predicted earnings and the extensive margin measure is weekly employment. The low-earnings sample includes single women in the bottom half of the within-year and within-state distribution of predicted earnings, estimated using equation (1).

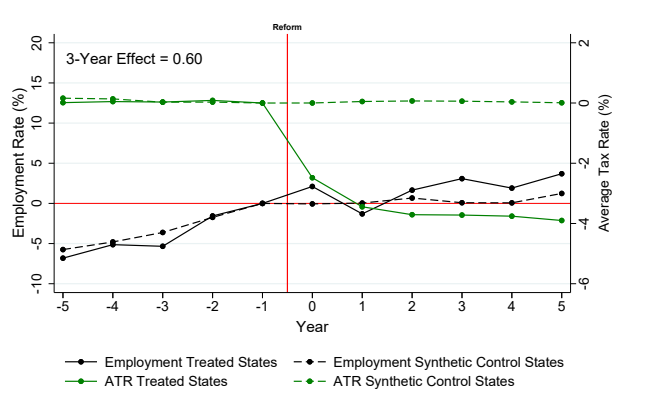
FIGURE A.84: SYNTHETIC CONTROL ANALYSIS OF STATE EITC REFORMS
WEEKLY EMPLOYMENT FOR SINGLE WOMEN IN BOTTOM QUARTILE OF PREDICTED EARNINGS

All Reforms Stacked

A: Difference-in-Differences
Treated vs Control States (With Kids)

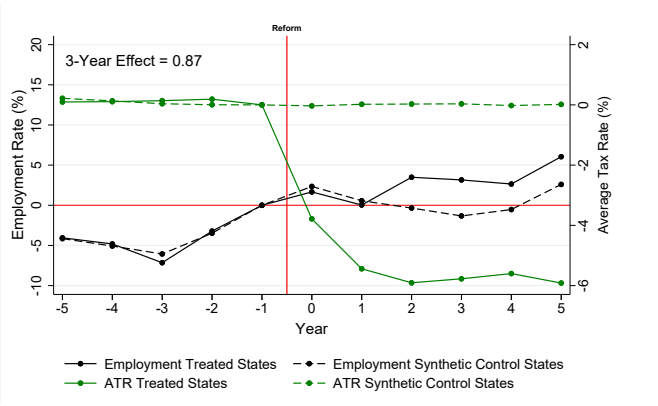


B: Triple-Differences
Treated vs Control States (With vs Without Kids)

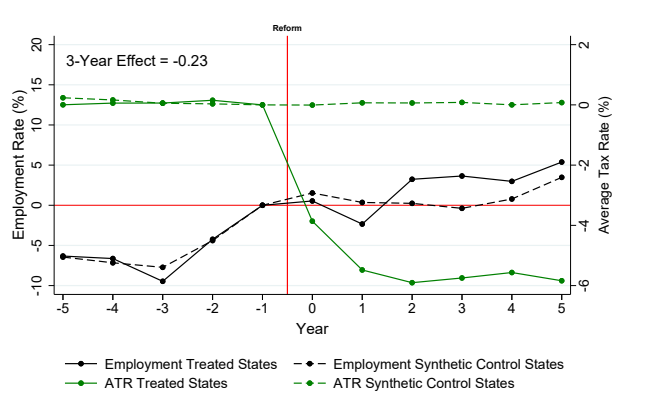


Ten Largest Reforms Stacked

C: Difference-in-Differences
Treated vs Control States (With Kids)



D: Triple-Differences
Treated vs Control States (With vs Without Kids)

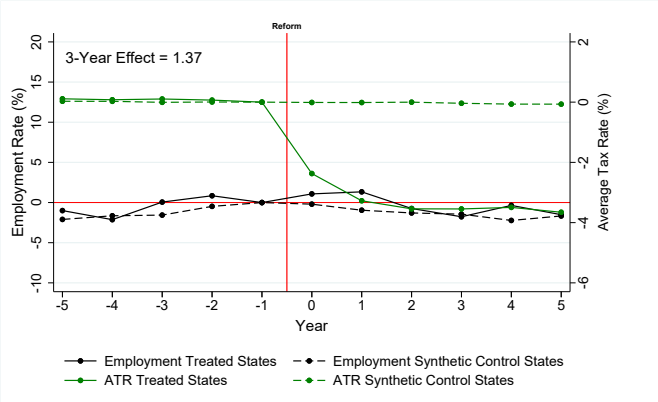


Notes: This figure is constructed exactly as Figure 11 in the main text, except for the estimation sample. In this figure, the sample is single women with low predicted earnings and the extensive margin measure is weekly employment. The low-earnings sample includes single women in the bottom quartile of the within-year and within-state distribution of predicted earnings, estimated using equation (1).

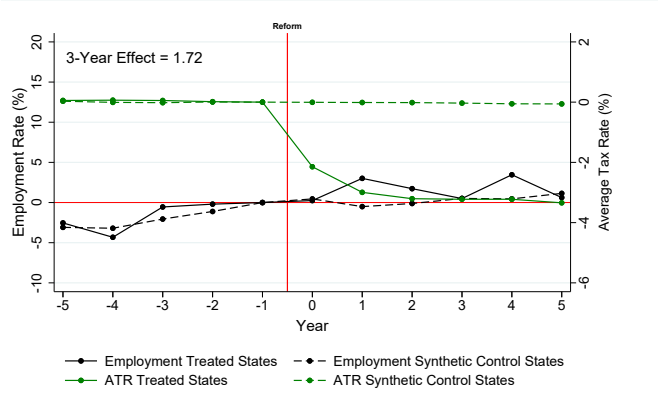
FIGURE A.85: SYNTHETIC CONTROL ANALYSIS OF STATE EITC REFORMS
ANNUAL EMPLOYMENT FOR ALL SINGLE WOMEN

All Reforms Stacked

A: Difference-in-Differences
Treated vs Control States (With Kids)

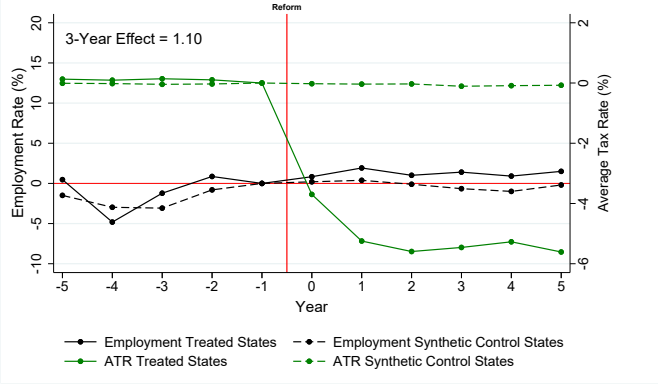


B: Triple-Differences
Treated vs Control States (With vs Without Kids)

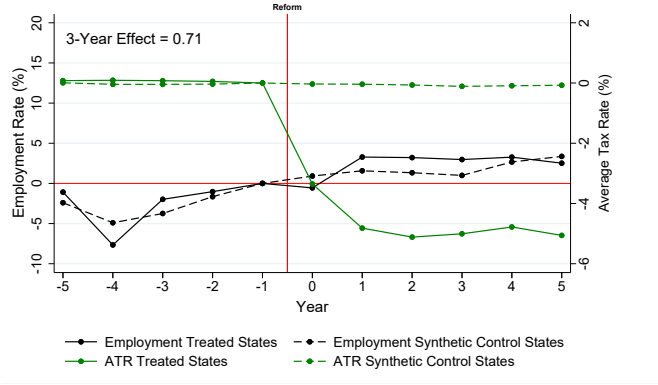


Ten Largest Reforms Stacked

C: Difference-in-Differences
Treated vs Control States (With Kids)



D: Triple-Differences
Treated vs Control States (With vs Without Kids)

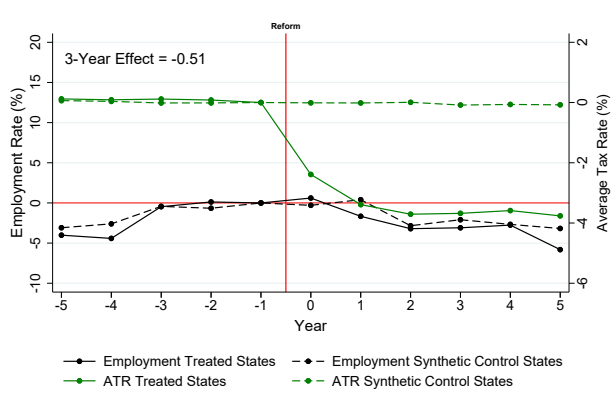


Notes: This figure is constructed exactly as Figure 11 in the main text, except for the extensive margin measure. In this figure, the sample is all single women and the extensive margin measure is annual employment.

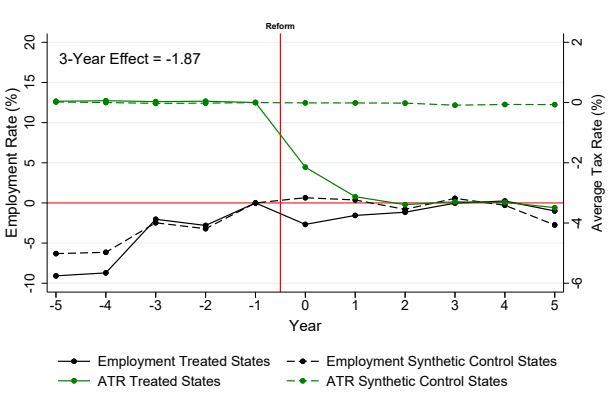
FIGURE A.86: SYNTHETIC CONTROL ANALYSIS OF STATE EITC REFORMS
ANNUAL EMPLOYMENT FOR LOW-EDUCATED SINGLE WOMEN

All Reforms Stacked

A: Difference-in-Differences
Treated vs Control States (With Kids)

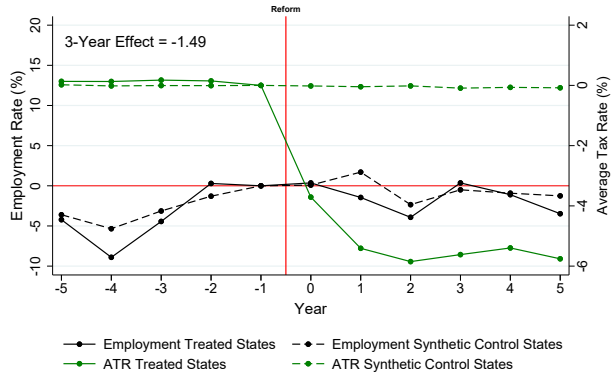


B: Triple-Differences
Treated vs Control States (With vs Without Kids)

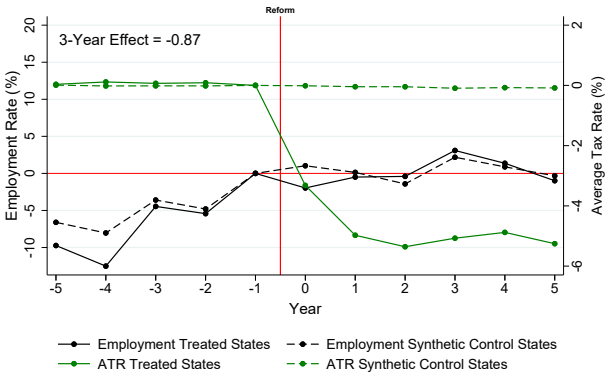


Ten Largest Reforms Stacked

C: Difference-in-Differences
Treated vs Control States (With Kids)



D: Triple-Differences
Treated vs Control States (With vs Without Kids)

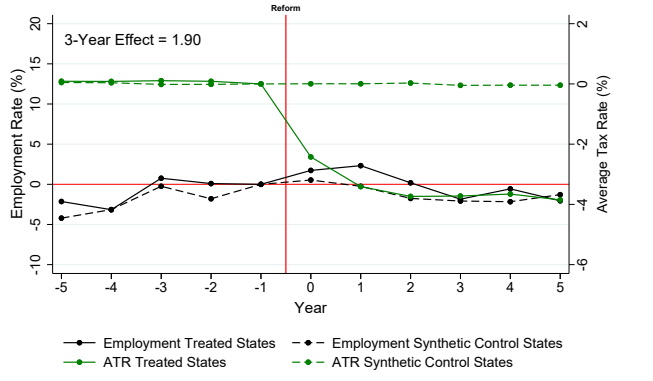


Notes: This figure is constructed exactly as Figure 11 in the main text, except for the estimation sample and the extensive margin measure. In this figure, the sample is low-educated single women (those with a high school degree or less) and the extensive margin measure is annual employment.

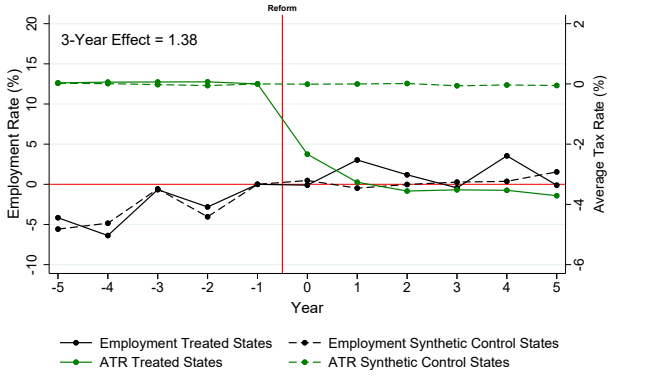
FIGURE A.87: SYNTHETIC CONTROL ANALYSIS OF STATE EITC REFORMS
ANNUAL EMPLOYMENT FOR SINGLE WOMEN IN BOTTOM HALF OF PREDICTED EARNINGS

All Reforms Stacked

A: Difference-in-Differences
Treated vs Control States (With Kids)

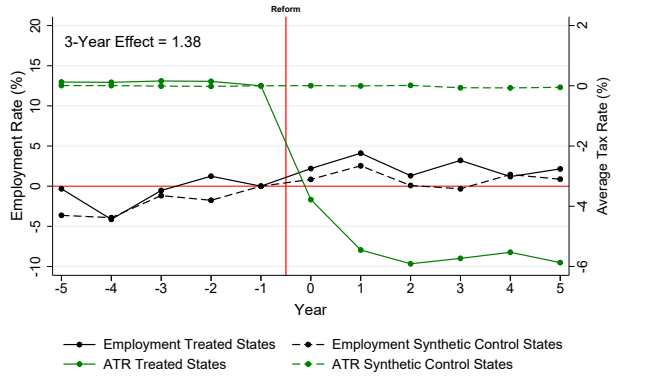


B: Triple-Differences
Treated vs Control States (With vs Without Kids)

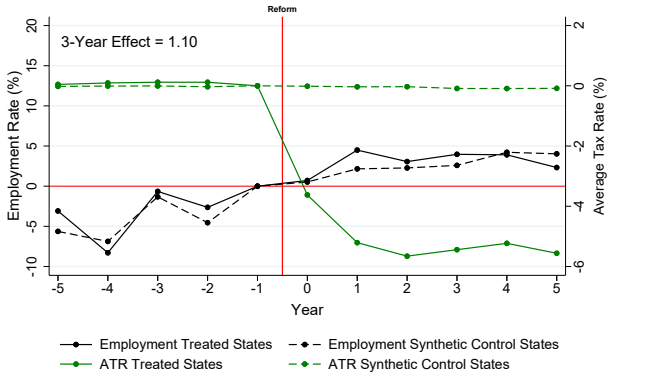


Ten Largest Reforms Stacked

C: Difference-in-Differences
Treated vs Control States (With Kids)



D: Triple-Differences
Treated vs Control States (With vs Without Kids)

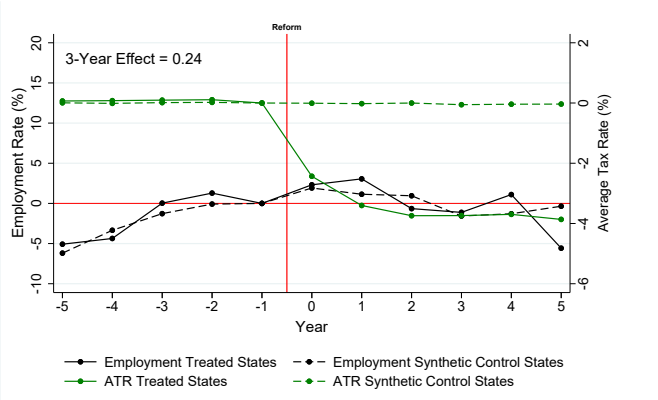


Notes: This figure is constructed exactly as Figure 11 in the main text, except for the estimation sample and the extensive margin measure. In this figure, the sample is single women with low predicted earnings and the extensive margin measure is annual employment. The low-earnings sample includes single women in the bottom half of the within-year and within-state distribution of predicted earnings, estimated using equation (1).

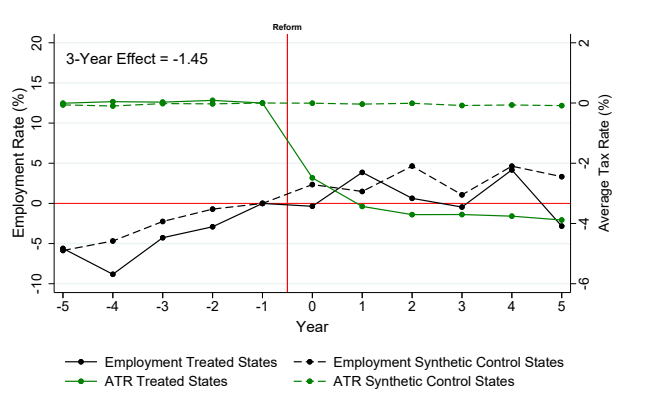
FIGURE A.88: SYNTHETIC CONTROL ANALYSIS OF STATE EITC REFORMS
ANNUAL EMPLOYMENT FOR SINGLE WOMEN IN BOTTOM QUARTILE OF PREDICTED EARNINGS

All Reforms Stacked

A: Difference-in-Differences
Treated vs Control States (With Kids)

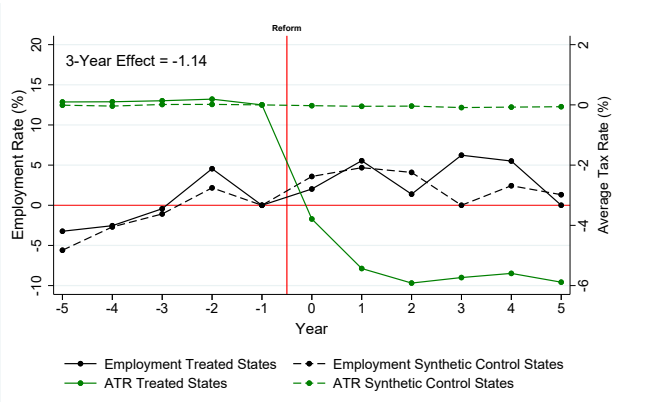


B: Triple-Differences
Treated vs Control States (With vs Without Kids)

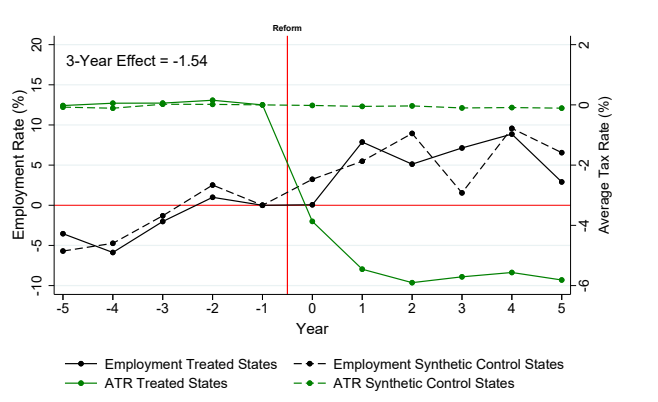


Ten Largest Reforms Stacked

C: Difference-in-Differences
Treated vs Control States (With Kids)



D: Triple-Differences
Treated vs Control States (With vs Without Kids)

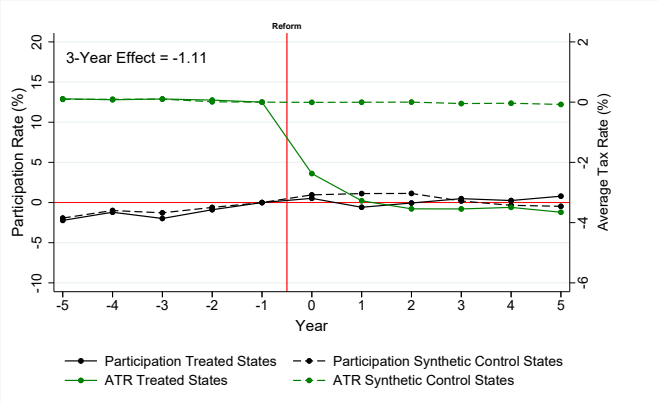


Notes: This figure is constructed exactly as Figure 11 in the main text, except for the estimation sample and the extensive margin measure. In this figure, the sample is single women with low predicted earnings and the extensive margin measure is annual employment. The low-earnings sample includes single women in the bottom quartile of the within-year and within-state distribution of predicted earnings, estimated using equation (1).

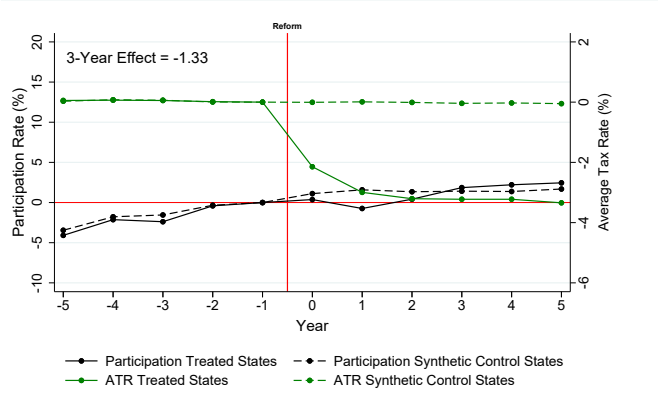
FIGURE A.89: SYNTHETIC CONTROL ANALYSIS OF STATE EITC REFORMS
WEEKLY PARTICIPATION FOR ALL SINGLE WOMEN

All Reforms Stacked

A: Difference-in-Differences
Treated vs Control States (With Kids)

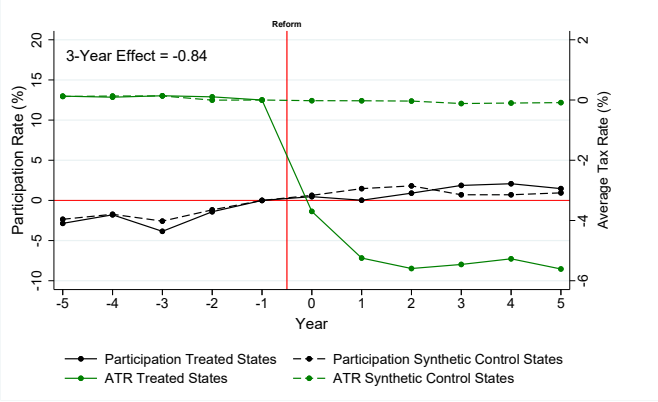


B: Triple-Differences
Treated vs Control States (With vs Without Kids)

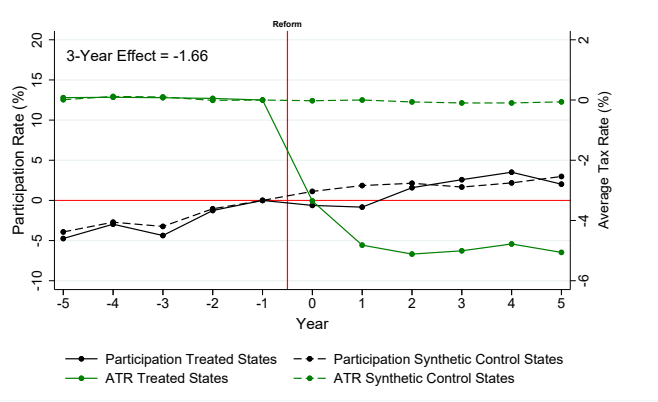


Ten Largest Reforms Stacked

C: Difference-in-Differences
Treated vs Control States (With Kids)



D: Triple-Differences
Treated vs Control States (With vs Without Kids)

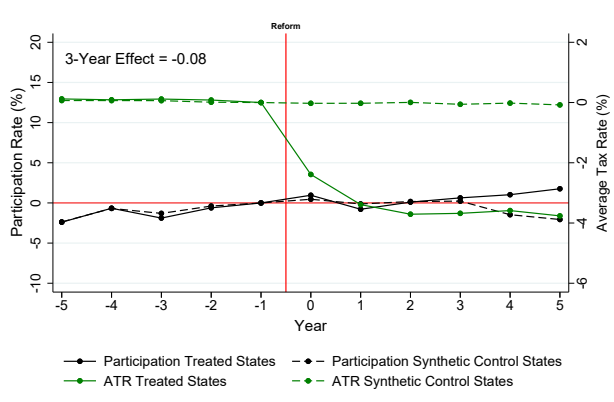


Notes: This figure is constructed exactly as Figure 11 in the main text, except for the extensive margin measure. In this figure, the sample is all single women and the extensive margin measure is weekly participation.

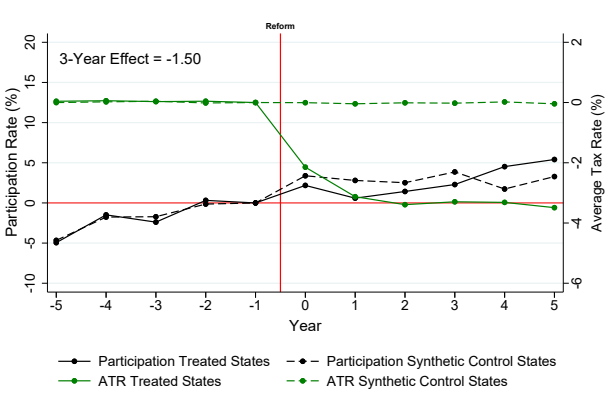
FIGURE A.90: SYNTHETIC CONTROL ANALYSIS OF STATE EITC REFORMS
WEEKLY PARTICIPATION FOR LOW-EDUCATED SINGLE WOMEN

All Reforms Stacked

A: Difference-in-Differences
Treated vs Control States (With Kids)

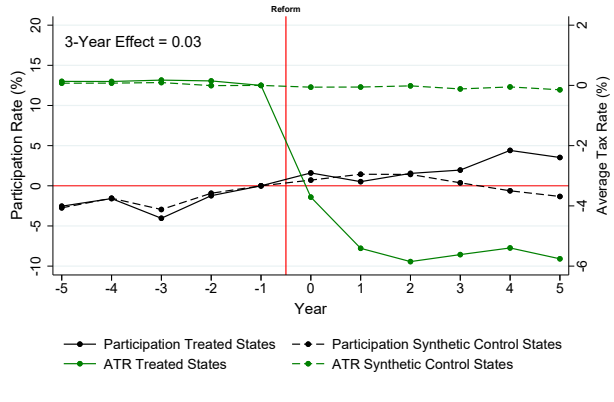


B: Triple-Differences
Treated vs Control States (With vs Without Kids)

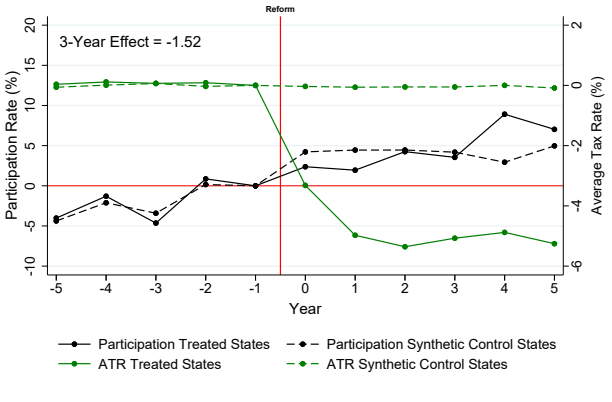


Ten Largest Reforms Stacked

C: Difference-in-Differences
Treated vs Control States (With Kids)



D: Triple-Differences
Treated vs Control States (With vs Without Kids)

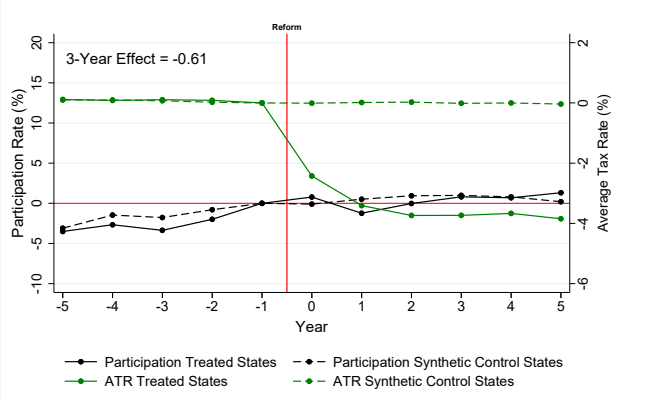


Notes: This figure is constructed exactly as Figure 11 in the main text, except for the estimation sample and the extensive margin measure. In this figure, the sample is low-educated single women (those with a high school degree or less) and the extensive margin measure is weekly participation.

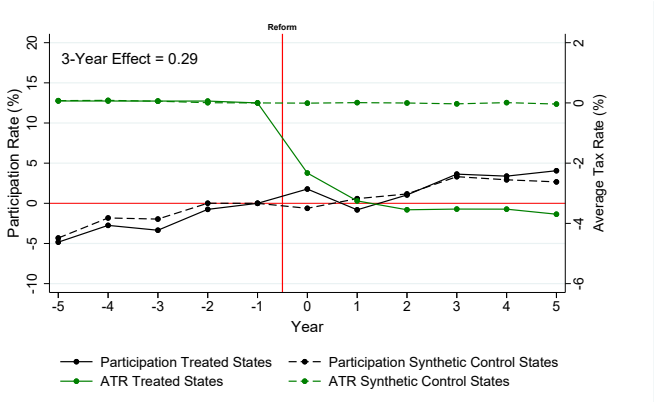
FIGURE A.91: SYNTHETIC CONTROL ANALYSIS OF STATE EITC REFORMS
WEEKLY PARTICIPATION FOR SINGLE WOMEN IN BOTTOM HALF OF PREDICTED EARNINGS

All Reforms Stacked

A: Difference-in-Differences
Treated vs Control States (With Kids)

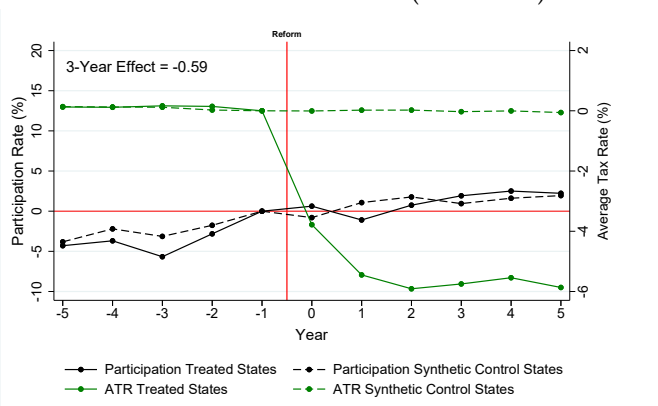


B: Triple-Differences
Treated vs Control States (With vs Without Kids)

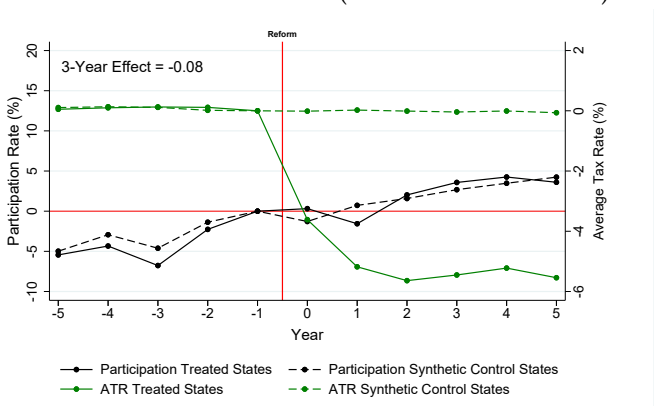


Ten Largest Reforms Stacked

C: Difference-in-Differences
Treated vs Control States (With Kids)



D: Triple-Differences
Treated vs Control States (With vs Without Kids)

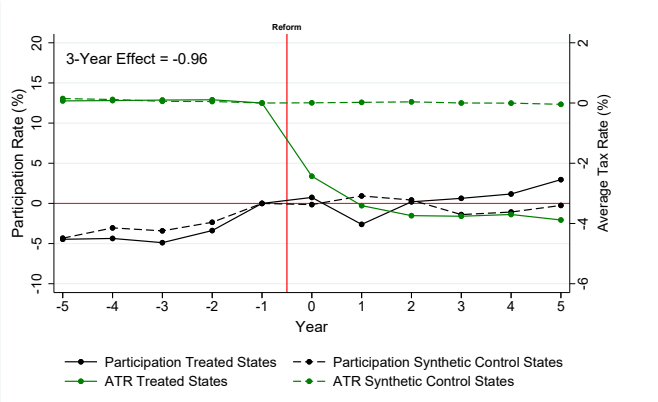


Notes: This figure is constructed exactly as Figure 11 in the main text, except for the estimation sample and the extensive margin measure. In this figure, the sample is single women with low predicted earnings and the extensive margin measure is weekly participation. The low-earnings sample includes single women in the bottom half of the within-year and within-state distribution of predicted earnings, estimated using equation (1).

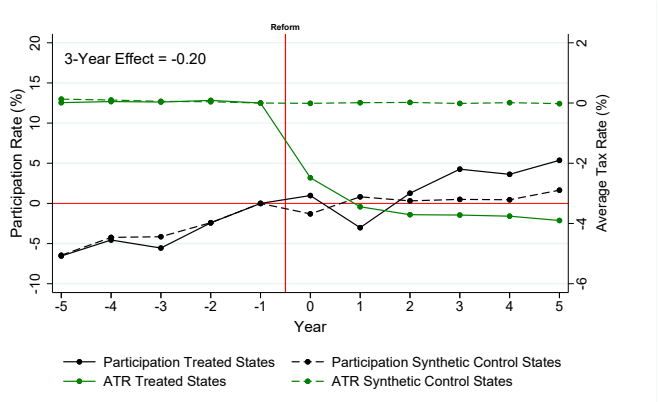
FIGURE A.92: SYNTHETIC CONTROL ANALYSIS OF STATE EITC REFORMS
WEEKLY PARTICIPATION FOR SINGLE WOMEN IN BOTTOM QUARTILE OF PREDICTED EARNINGS

All Reforms Stacked

A: Difference-in-Differences
Treated vs Control States (With Kids)

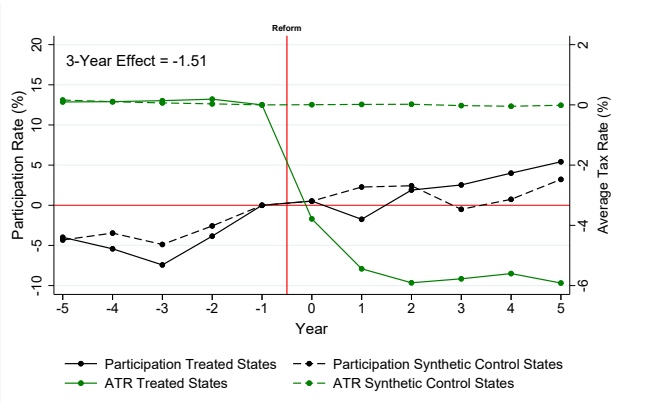


B: Triple-Differences
Treated vs Control States (With vs Without Kids)

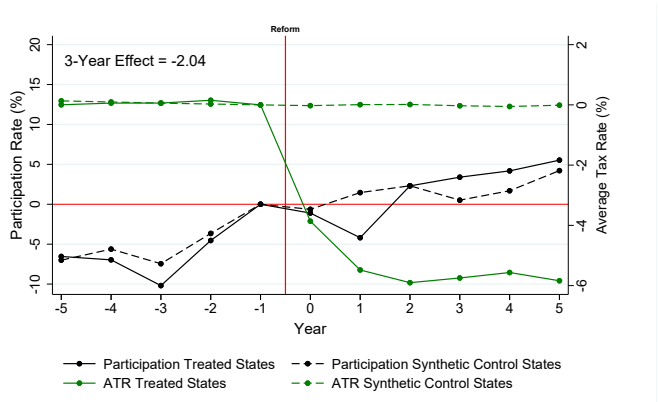


Ten Largest Reforms Stacked

C: Difference-in-Differences
Treated vs Control States (With Kids)



D: Triple-Differences
Treated vs Control States (With vs Without Kids)

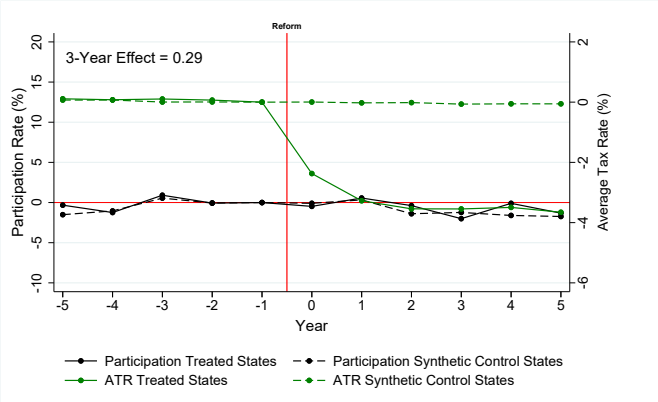


Notes: This figure is constructed exactly as Figure 11 in the main text, except for the estimation sample and the extensive margin measure. In this figure, the sample is single women with low predicted earnings and the extensive margin measure is weekly participation. The low-earnings sample includes single women in the bottom quartile of the within-year and within-state distribution of predicted earnings, estimated using equation (1).

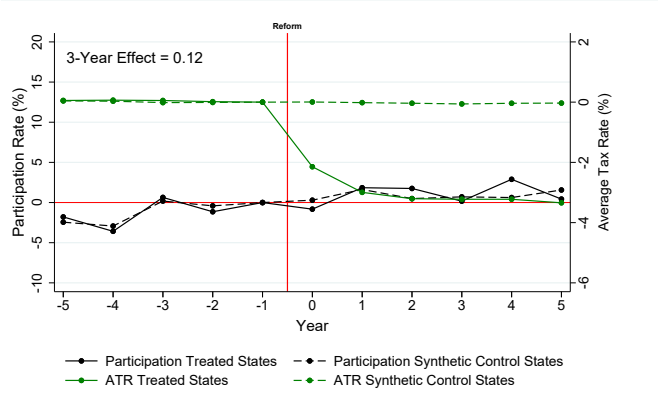
FIGURE A.93: SYNTHETIC CONTROL ANALYSIS OF STATE EITC REFORMS
ANNUAL PARTICIPATION FOR ALL SINGLE WOMEN

All Reforms Stacked

A: Difference-in-Differences
Treated vs Control States (With Kids)

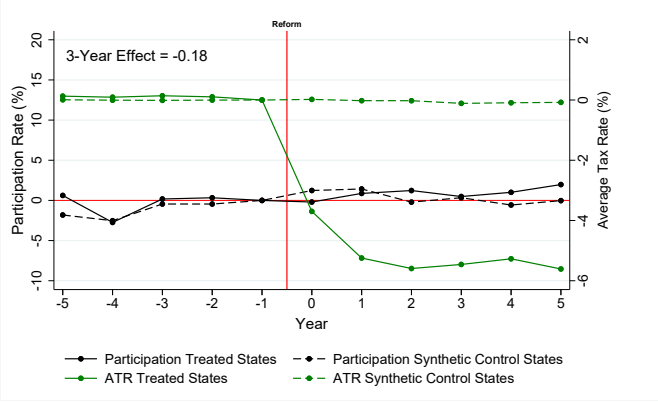


B: Triple-Differences
Treated vs Control States (With vs Without Kids)

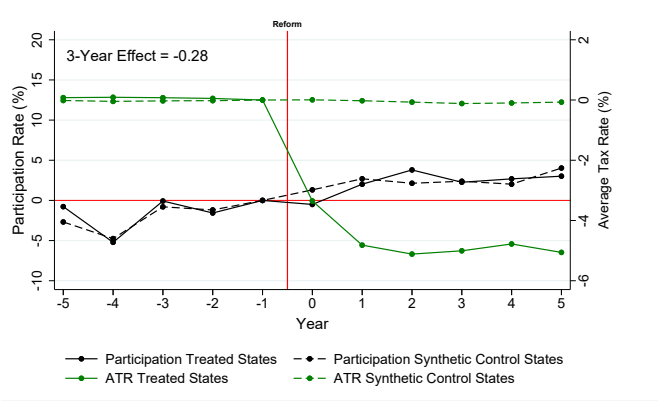


Ten Largest Reforms Stacked

C: Difference-in-Differences
Treated vs Control States (With Kids)



D: Triple-Differences
Treated vs Control States (With vs Without Kids)

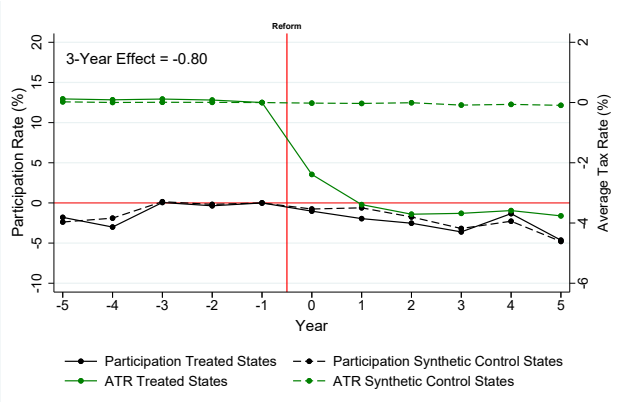


Notes: This figure is constructed exactly as Figure 11 in the main text, except for the extensive margin measure. In this figure, the sample is all single women and the extensive margin measure is annual participation.

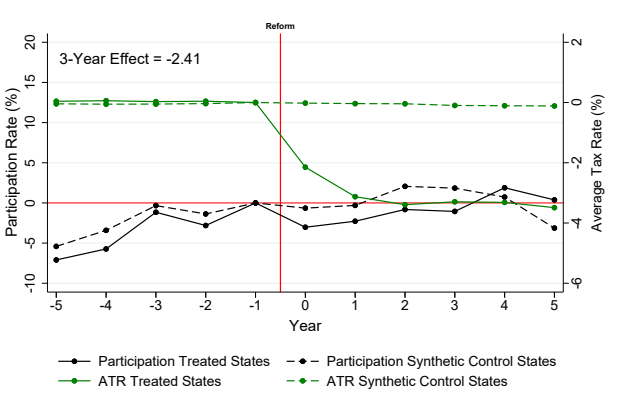
FIGURE A.94: SYNTHETIC CONTROL ANALYSIS OF STATE EITC REFORMS
ANNUAL PARTICIPATION FOR LOW-EDUCATED SINGLE WOMEN

All Reforms Stacked

A: Difference-in-Differences
Treated vs Control States (With Kids)

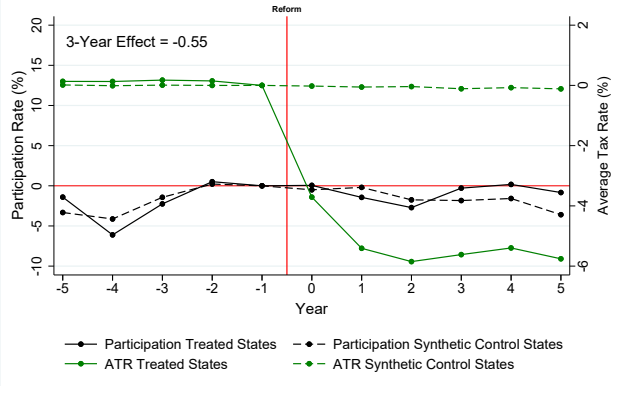


B: Triple-Differences
Treated vs Control States (With vs Without Kids)

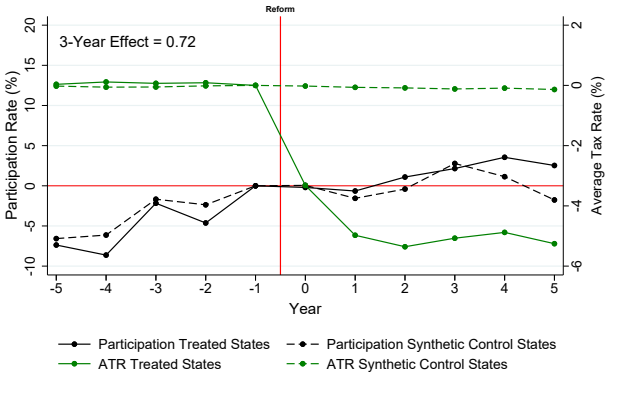


Ten Largest Reforms Stacked

C: Difference-in-Differences
Treated vs Control States (With Kids)



D: Triple-Differences
Treated vs Control States (With vs Without Kids)



Notes: This figure is constructed exactly as Figure 11 in the main text, except for the estimation sample and the extensive margin measure. In this figure, the sample is low-educated single women (those with a high school degree or less) and the extensive margin measure is annual participation.

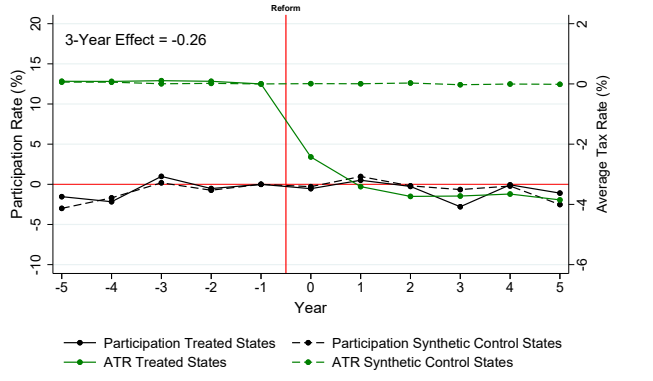
FIGURE A.95: SYNTHETIC CONTROL ANALYSIS OF STATE EITC REFORMS

ANNUAL PARTICIPATION FOR SINGLE WOMEN IN BOTTOM HALF OF PREDICTED EARNINGS

All Reforms Stacked

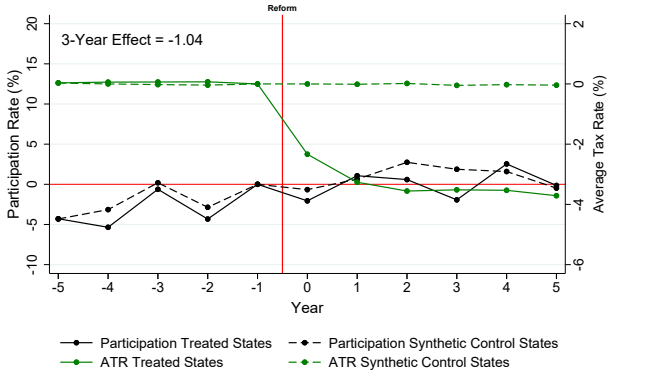
A: Difference-in-Differences

Treated vs Control States (With Kids)



B: Triple-Differences

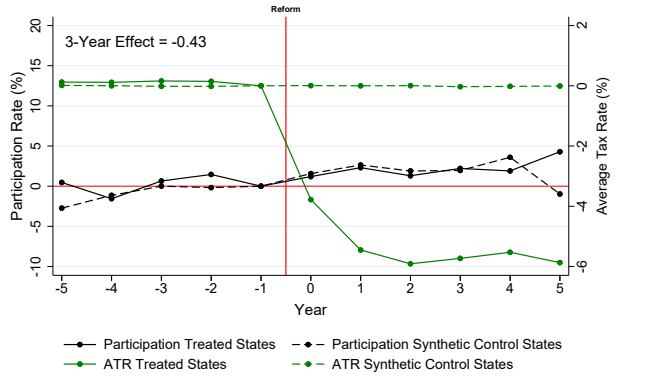
Treated vs Control States (With vs Without Kids)



Ten Largest Reforms Stacked

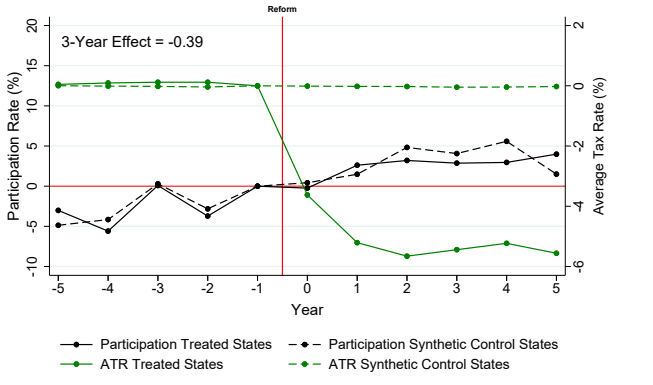
C: Difference-in-Differences

Treated vs Control States (With Kids)



D: Triple-Differences

Treated vs Control States (With vs Without Kids)

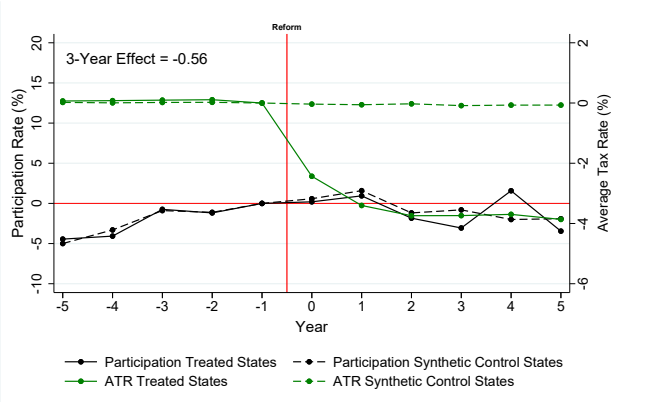


Notes: This figure is constructed exactly as Figure 11 in the main text, except for the estimation sample and the extensive margin measure. In this figure, the sample is single women with low predicted earnings and the extensive margin measure is annual participation. The low-earnings sample includes single women in the bottom half of the within-year and within-state distribution of predicted earnings, estimated using equation (1).

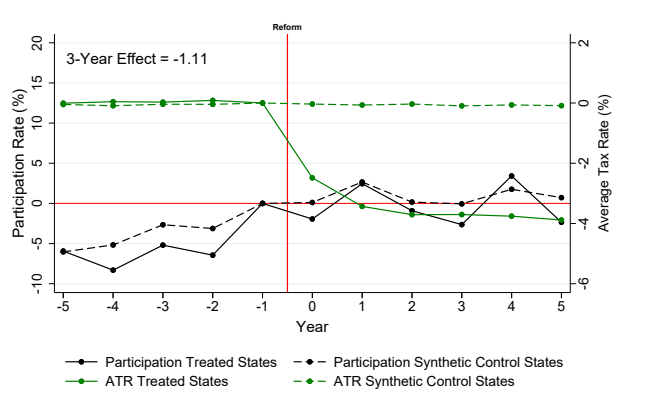
FIGURE A.96: SYNTHETIC CONTROL ANALYSIS OF STATE EITC REFORMS
ANNUAL PARTICIPATION FOR SINGLE WOMEN IN BOTTOM QUARTILE OF PREDICTED EARNINGS

All Reforms Stacked

A: Difference-in-Differences
Treated vs Control States (With Kids)

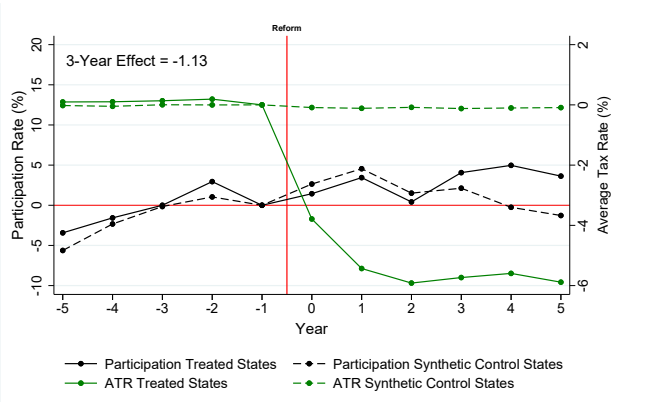


B: Triple-Differences
Treated vs Control States (With vs Without Kids)

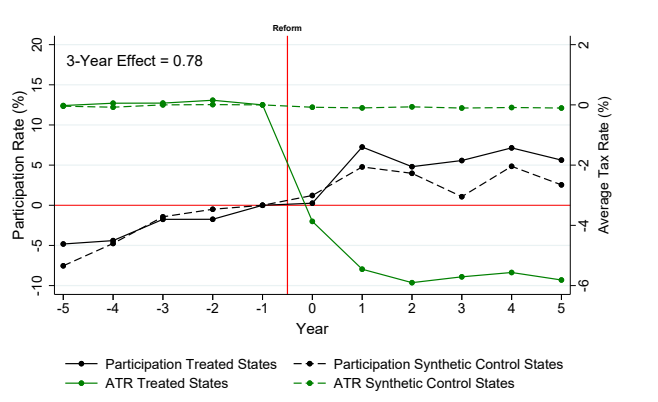


Ten Largest Reforms Stacked

C: Difference-in-Differences
Treated vs Control States (With Kids)



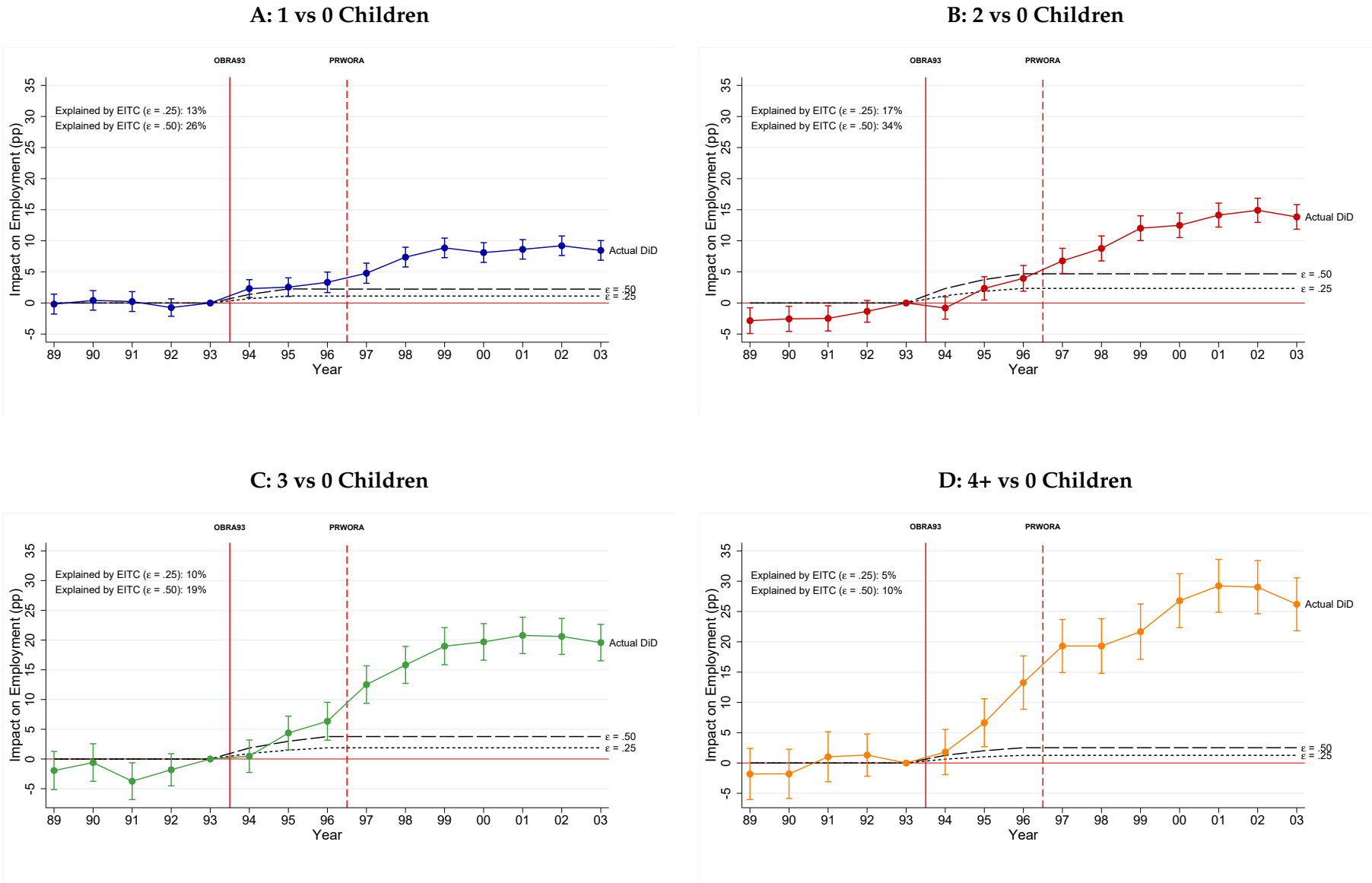
D: Triple-Differences
Treated vs Control States (With vs Without Kids)



Notes: This figure is constructed exactly as Figure 11 in the main text, except for the estimation sample and the extensive margin measure. In this figure, the sample is single women with low predicted earnings and the extensive margin measure is annual participation. The low-earnings sample includes single women in the bottom quartile of the within-year and within-state distribution of predicted earnings, estimated using equation (1).

Figure A.97: Estimated vs Simulated Responses to the 1993 Reform

All Single Women, Weekly Employment

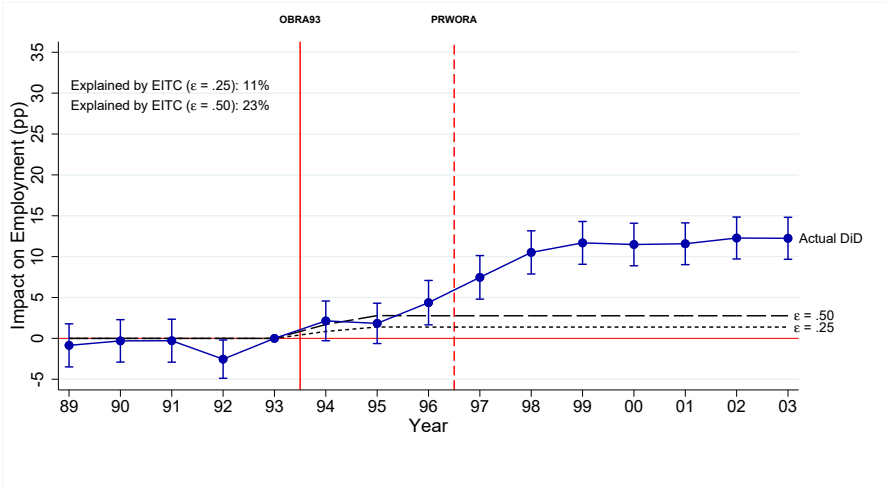


Notes: This figure shows actual and simulated DiD event studies of the 1993 reform, by number of EITC-eligible children. The actual DiD series plot estimates $\hat{\gamma}_t^n$ based on an extension of specification (2) with separate dummies for each number of children n (and without any controls). The simulated DiD series plot ΔP_t calculated from equation (4), assuming an elasticity of 0.25 (short dashes) or 0.5 (long dashes). Each panel also shows the fraction of the employment increase over the post-reform period that can be explained by the 1993 tax reform (including but not limited to the EITC expansion) under the two elasticity scenarios. The extensive margin outcome is weekly employment, and the sample includes all 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.

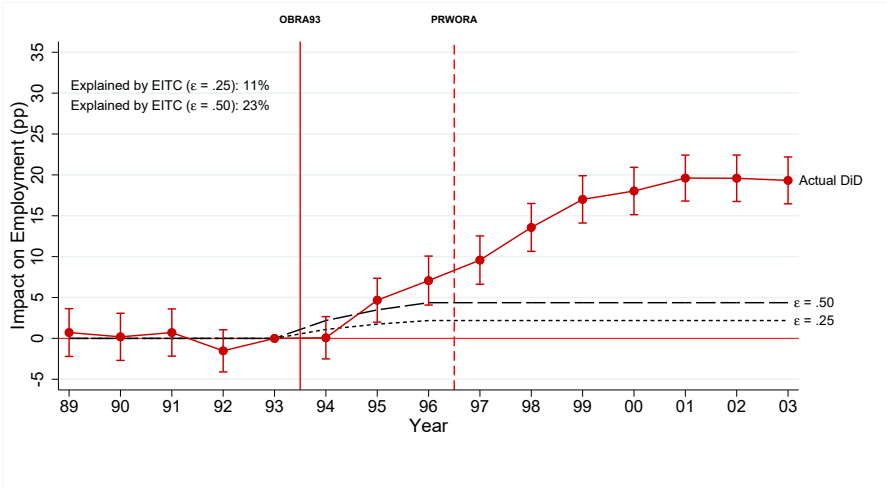
Figure A.98: Estimated vs Simulated Responses to the 1993 Reform

Single Women in Bottom Half of Predicted Earnings, Weekly Employment

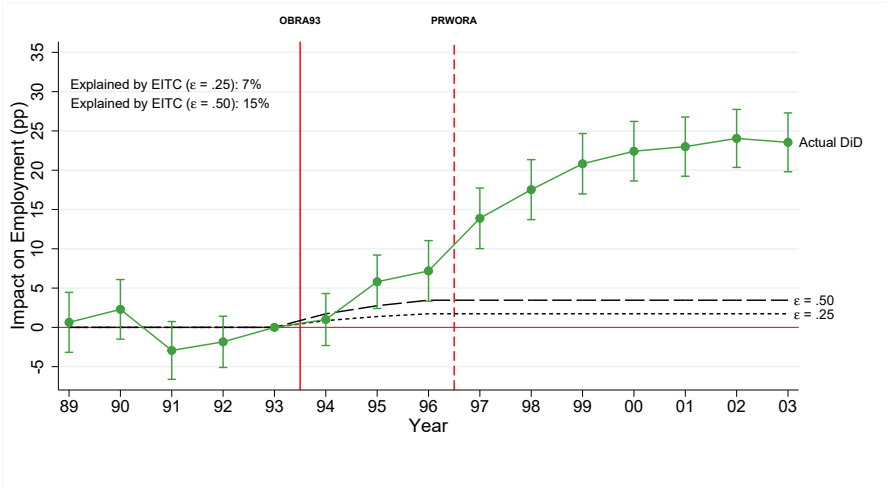
A: 1 vs 0 Children



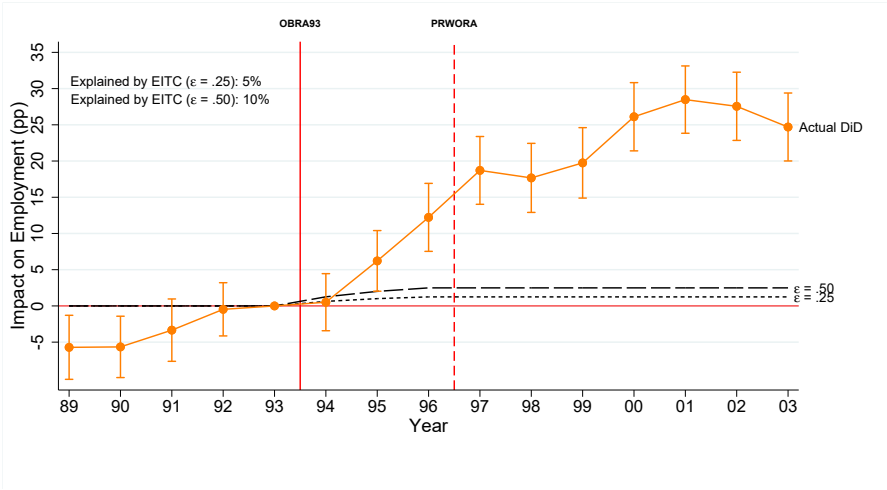
B: 2 vs 0 Children



C: 3 vs 0 Children

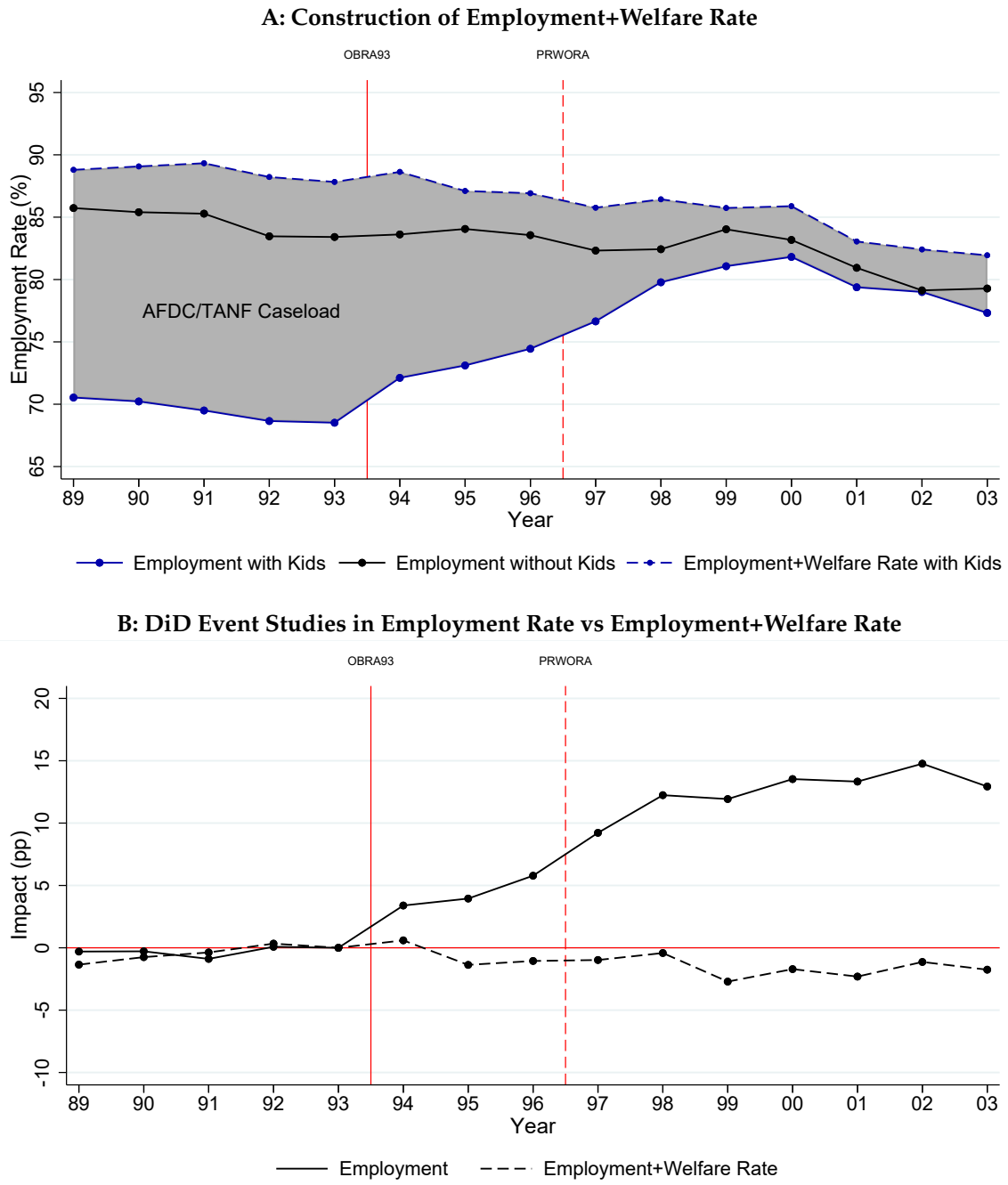


D: 4+ vs 0 Children



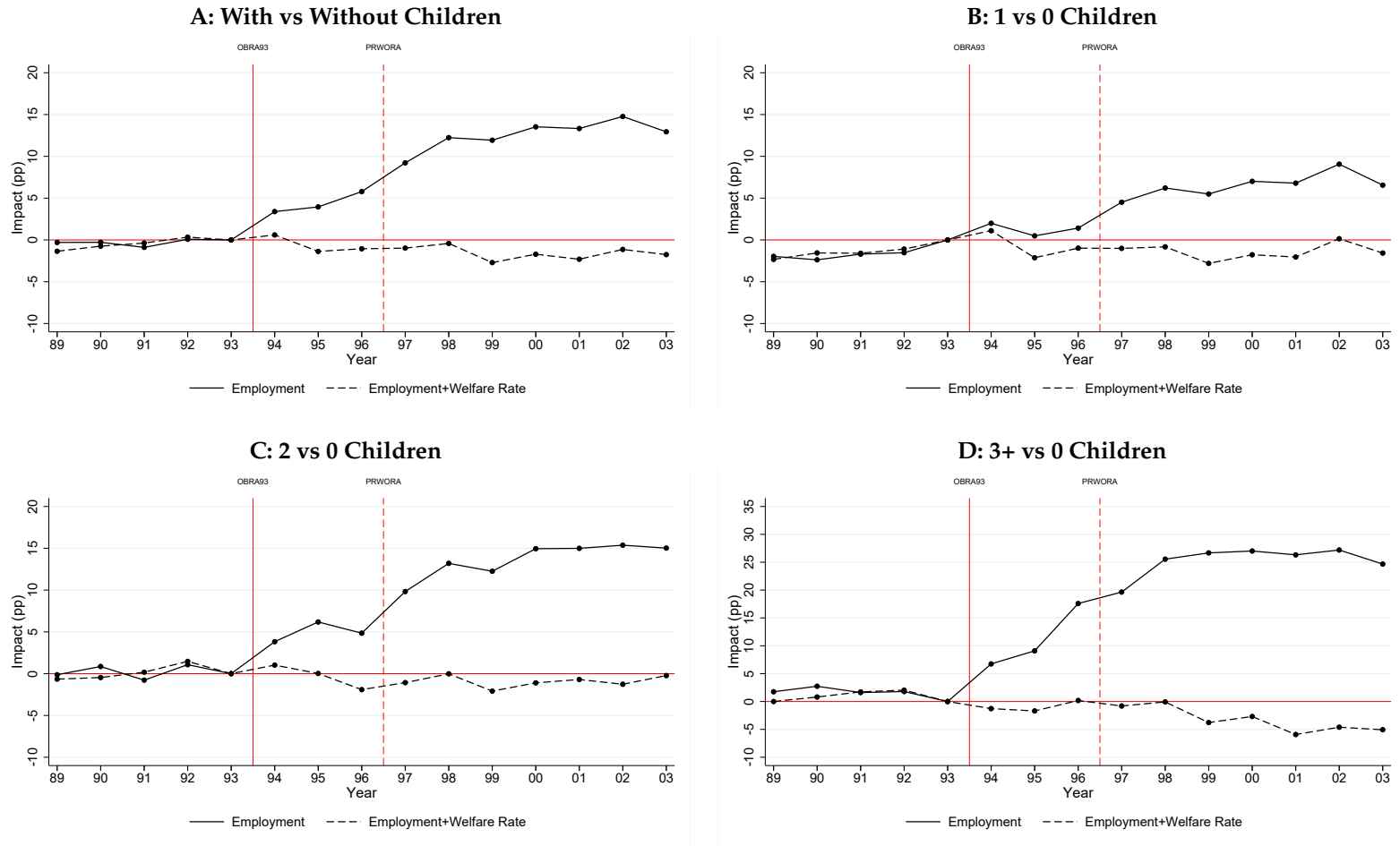
Notes: This figure shows actual and simulated DiD event studies of the 1993 reform, by number of EITC-eligible children. The actual DiD series plot estimates $\hat{\gamma}_t^n$ based on an extension of specification (2) with separate dummies for each number of children n (and without any controls). The simulated DiD series plot ΔP_t calculated from equation (4), assuming an elasticity of 0.25 (short dashes) or 0.5 (long dashes). Each panel also shows the fraction of the employment increase over the post-reform period that can be explained by the 1993 tax reform (including but not limited to the EITC expansion) under the two elasticity scenarios. The extensive margin outcome is weekly employment, and the sample includes single women aged 20-50 with low predicted earnings using the March and monthly CPS files combined. The low-earnings sample includes single women in the bottom half of the within-year distribution of predicted earnings, estimated using equation (1). The 95% confidence intervals are based on robust standard errors clustered at the individual level.

FIGURE A.99: EMPLOYMENT+WELFARE RATE FOR SINGLE MOTHERS



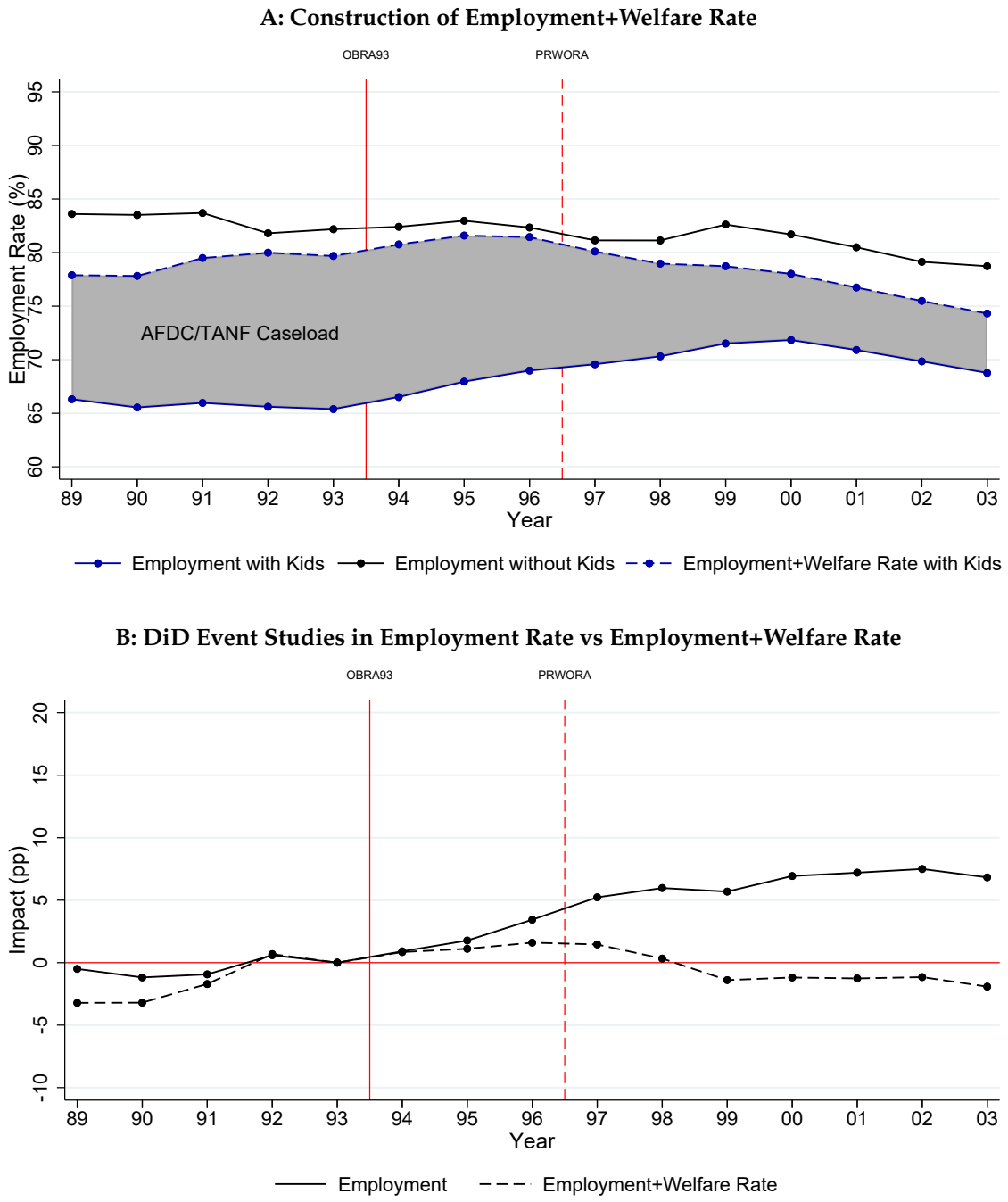
Notes: This figure shows the construction of the "employment+welfare rate" (Panel A) and DiD event studies of the 1993 reform for the employment rate and the employment+welfare rate (Panel B). The employment+welfare rate is defined as the fraction of people who are employed and/or receiving AFDC/TANF benefits. Single women without children are ineligible for AFDC/TANF, and hence there is no distinction between the employment and the employment+welfare rate for this group. Outcome variables are measured at the annual level. The sample includes single women aged 20-50 using March CPS files alone. Further details of the analysis are provided in appendix section F.

FIGURE A.100: EMPLOYMENT+WELFARE RATE FOR SINGLE MOTHERS BY NUMBER OF CHILDREN



Notes: This figure shows DiD event studies of the 1993 reform by number of children for the employment rate (solid) and the employment+welfare rate (dashed). The employment+welfare rate is defined as the fraction of people who are employed and/or receiving AFDC/TANF benefits. Single women without children are ineligible for AFDC/TANF, and hence there is no distinction between the employment and the employment+welfare rate for this group. Outcome variables are measured at the annual level. The sample includes single women aged 20-50 using March CPS files alone. Further details of the analysis are provided in appendix section F.

FIGURE A.101: EMPLOYMENT+WELFARE RATE FOR ALL MOTHERS
USING ADMINISTRATIVE DATA

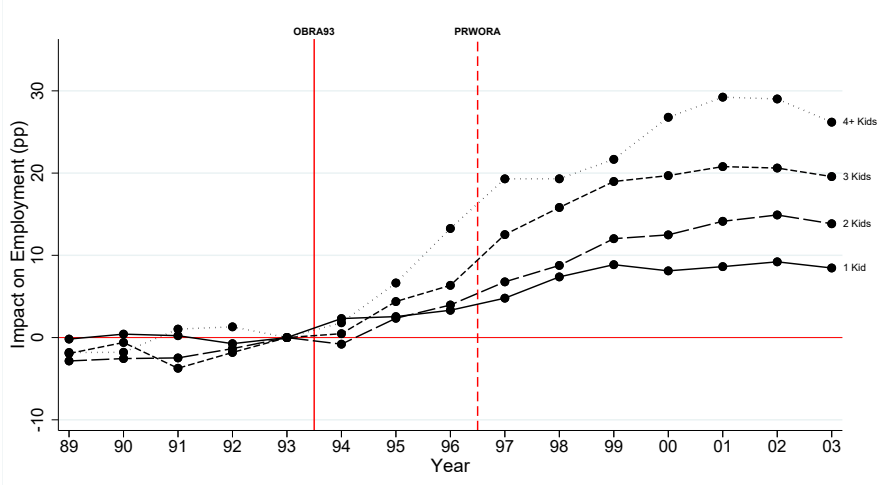


Notes: This figure replicates the analysis from Figure A.99, but using administrative data (instead of CPS data) to measure welfare caseloads and for the population of all women (instead of single women). To calculate welfare participation rates based on the publicly available administrative data, the total number of families receiving AFDC/TANF are divided by the total number of mothers aged 20-50 in the March CPS files. The full details of the analysis are provided in appendix section F.

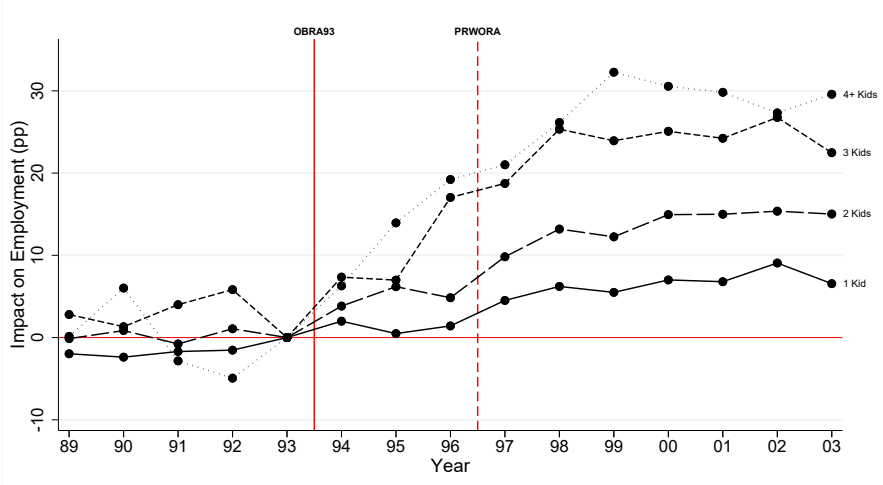
Figure A.102: Fanning Out by Number of Children

All Single Women, Raw Data

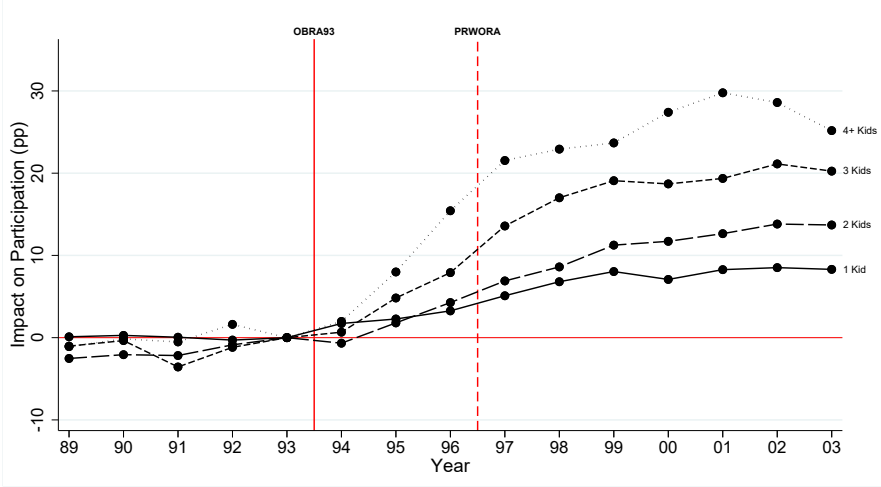
A: Weekly Employment



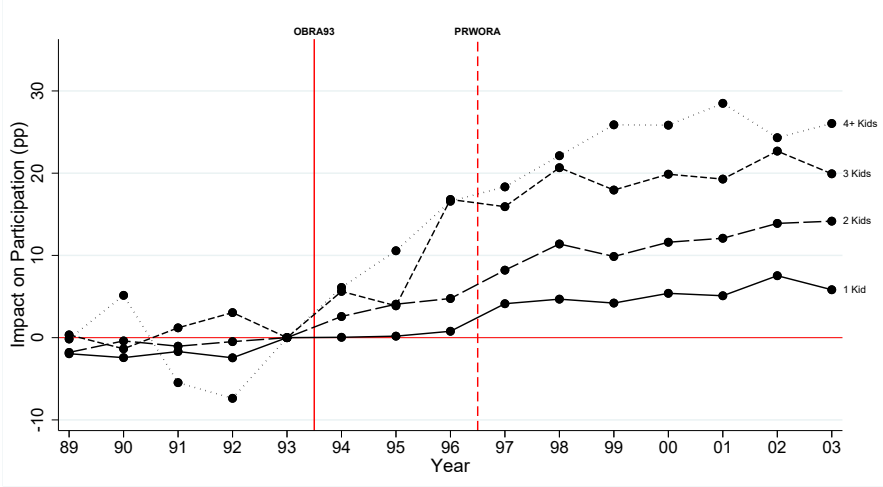
B: Annual Employment



C: Weekly Participation



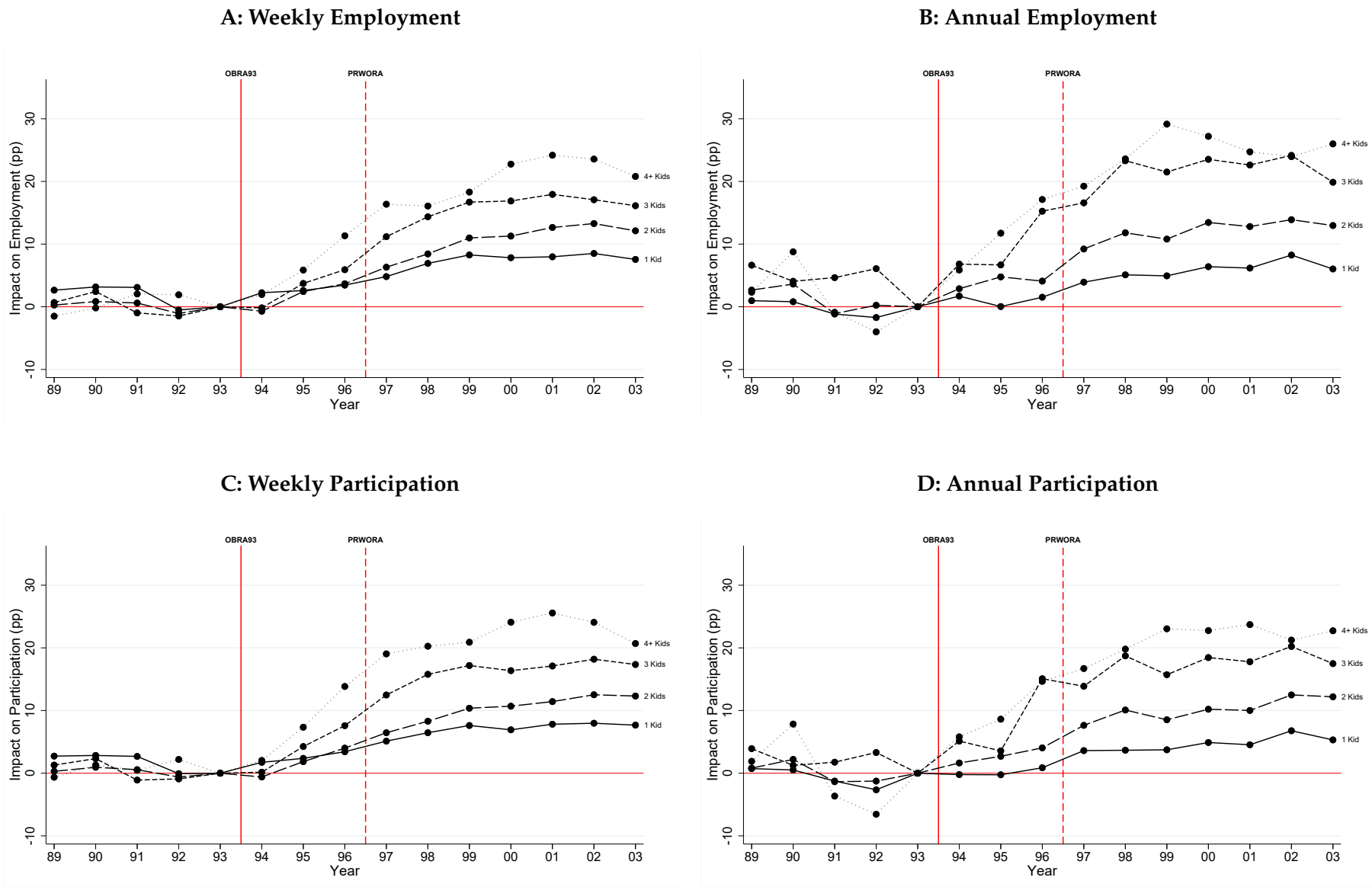
D: Annual Participation



Notes: This figure is constructed exactly as Panel A of Figure 14 in the main text, but shows results for all four extensive margin measures.

Figure A.103: Fanning Out by Number of Children

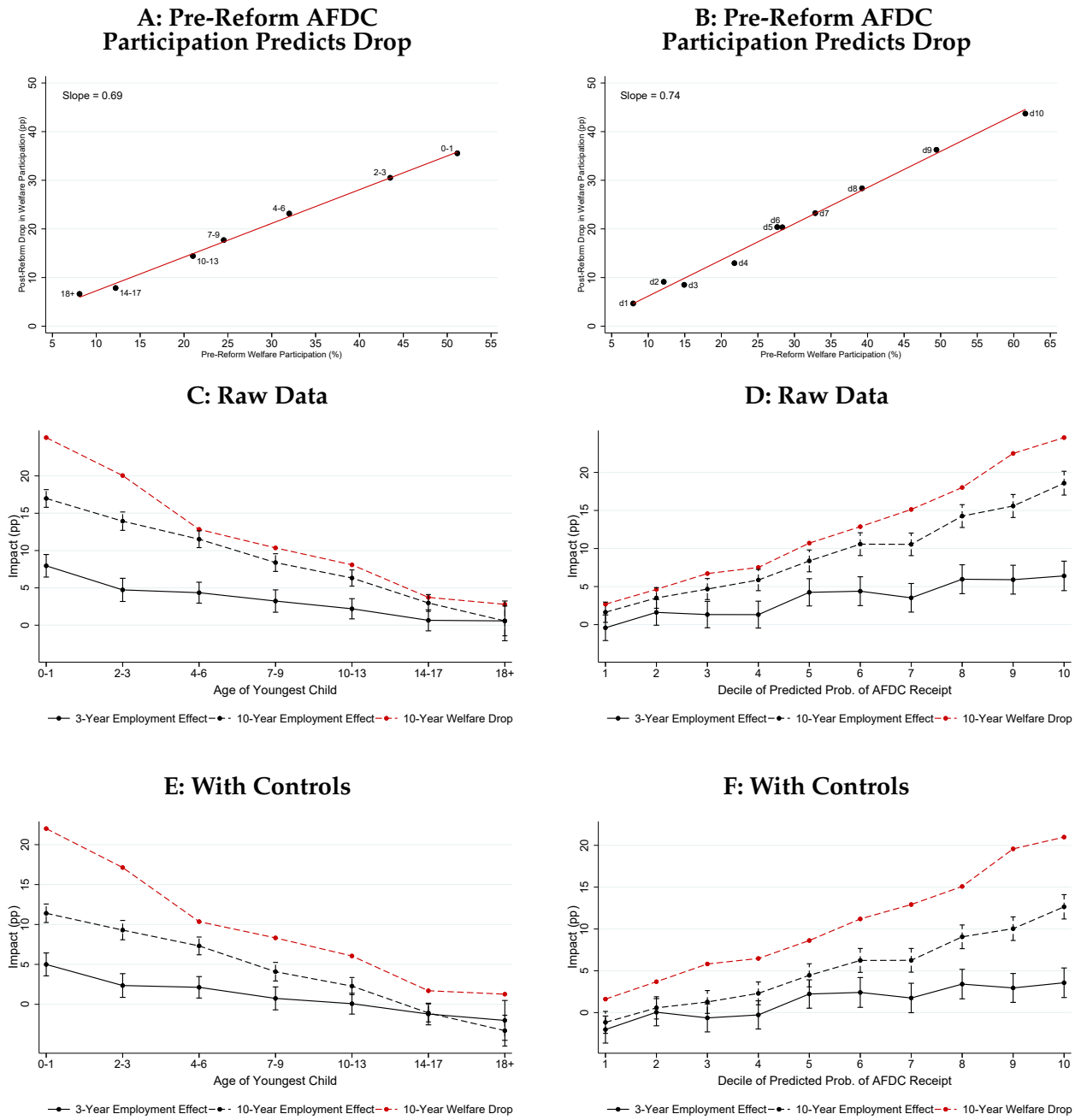
All Single Women, With Demographic Controls



Notes: This figure is constructed exactly as Panel B of Figure 14 in the main text, but shows results for all four extensive margin measures.

FIGURE A.104: EFFECTS OF THE 1993 EITC REFORM BY WELFARE TREATMENT INTENSITY
ALL SINGLE WOMEN, WEEKLY EMPLOYMENT

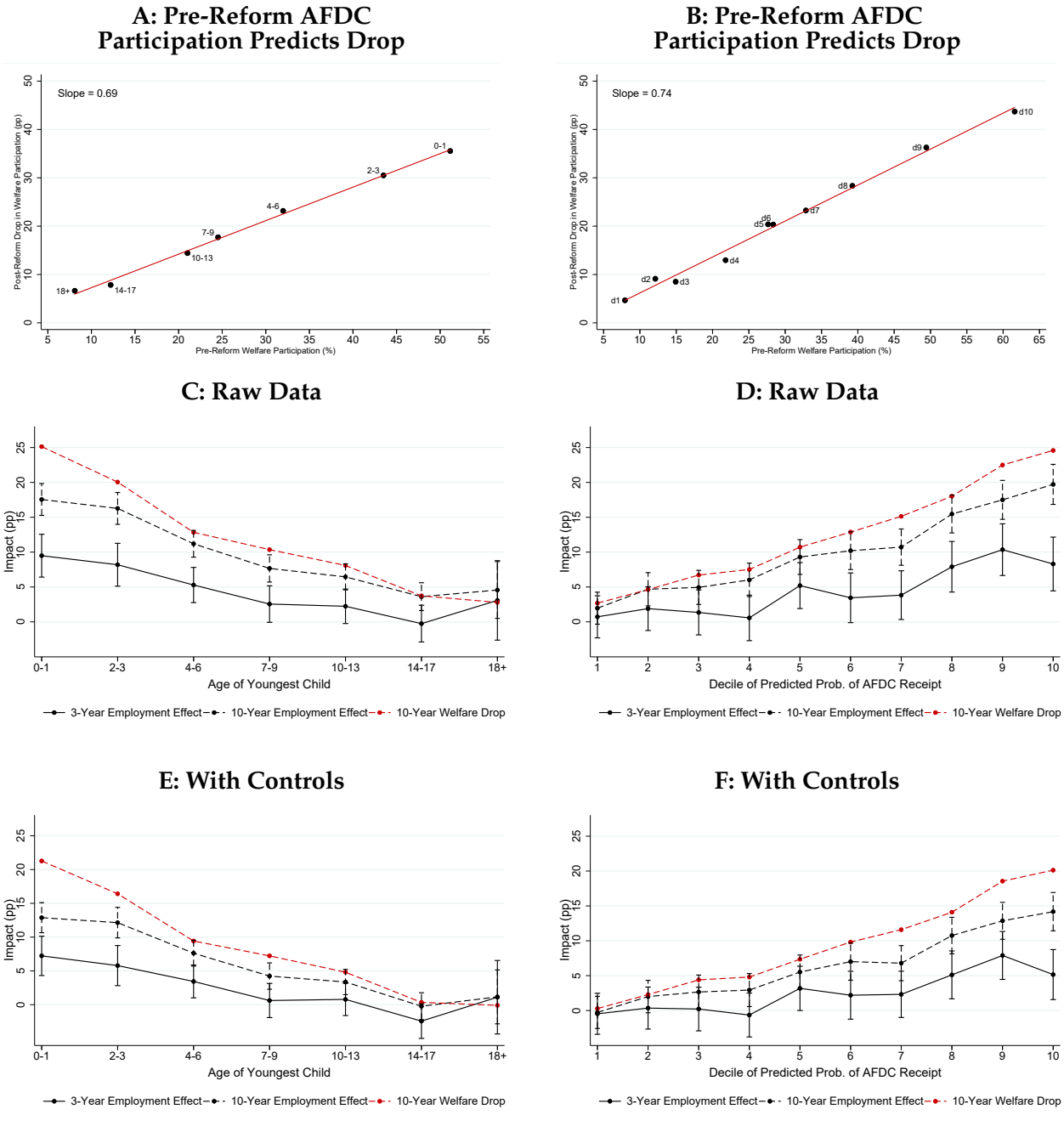
BY AGE OF YOUNGEST CHILD | BY PROBABILITY OF AFDC PARTICIPATION



Notes: This figure is exactly the same as Figure 15 in the main text.

FIGURE A.105: EFFECTS OF THE 1993 EITC REFORM BY WELFARE TREATMENT INTENSITY
ALL SINGLE WOMEN, ANNUAL EMPLOYMENT

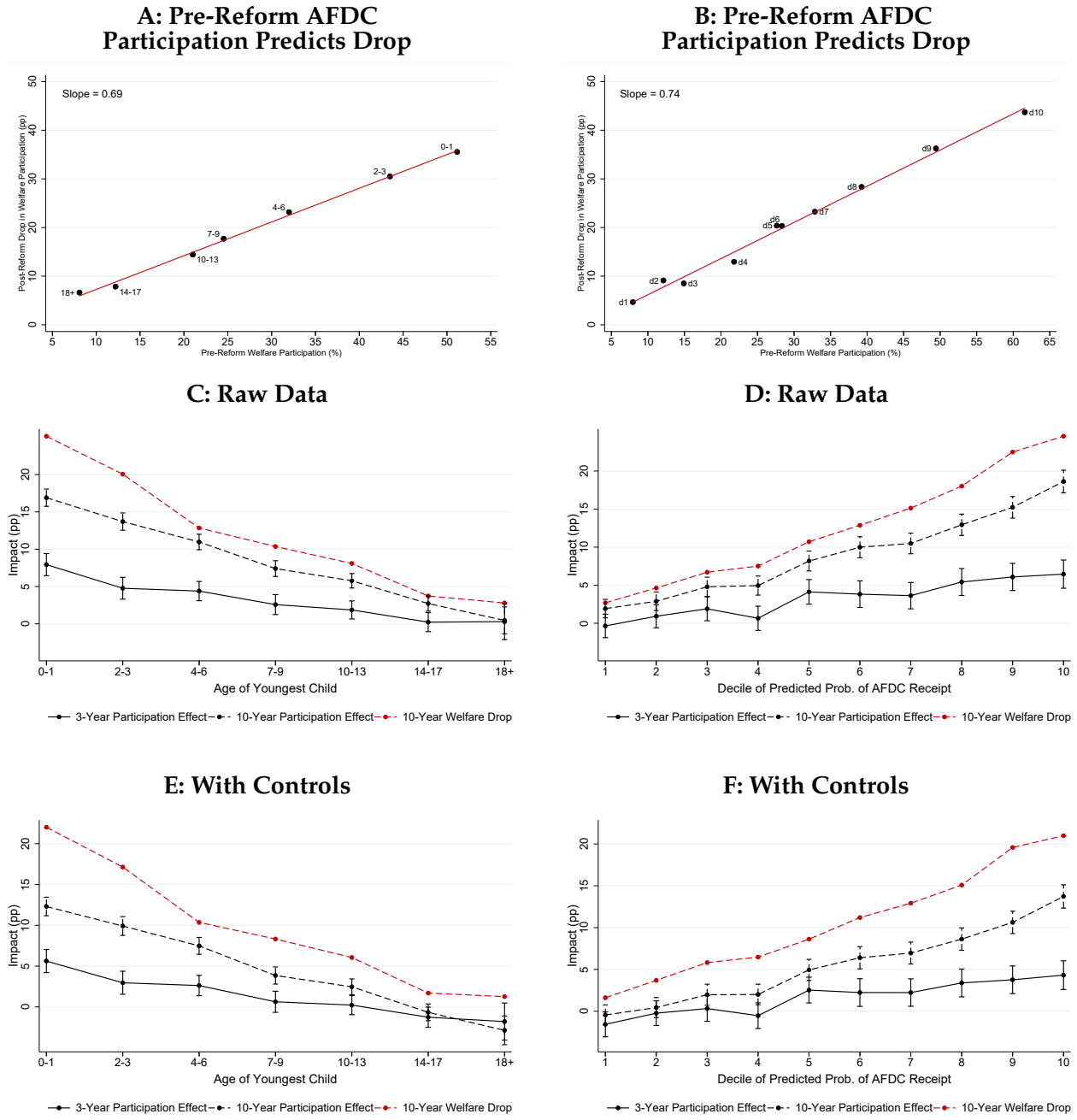
BY AGE OF YOUNGEST CHILD | BY PROBABILITY OF AFDC PARTICIPATION



Notes: This figure is constructed exactly as Figure 15 in the main text, except that it uses annual employment as the extensive margin measure.

FIGURE A.106: EFFECTS OF THE 1993 EITC REFORM BY WELFARE TREATMENT INTENSITY
ALL SINGLE WOMEN, WEEKLY PARTICIPATION

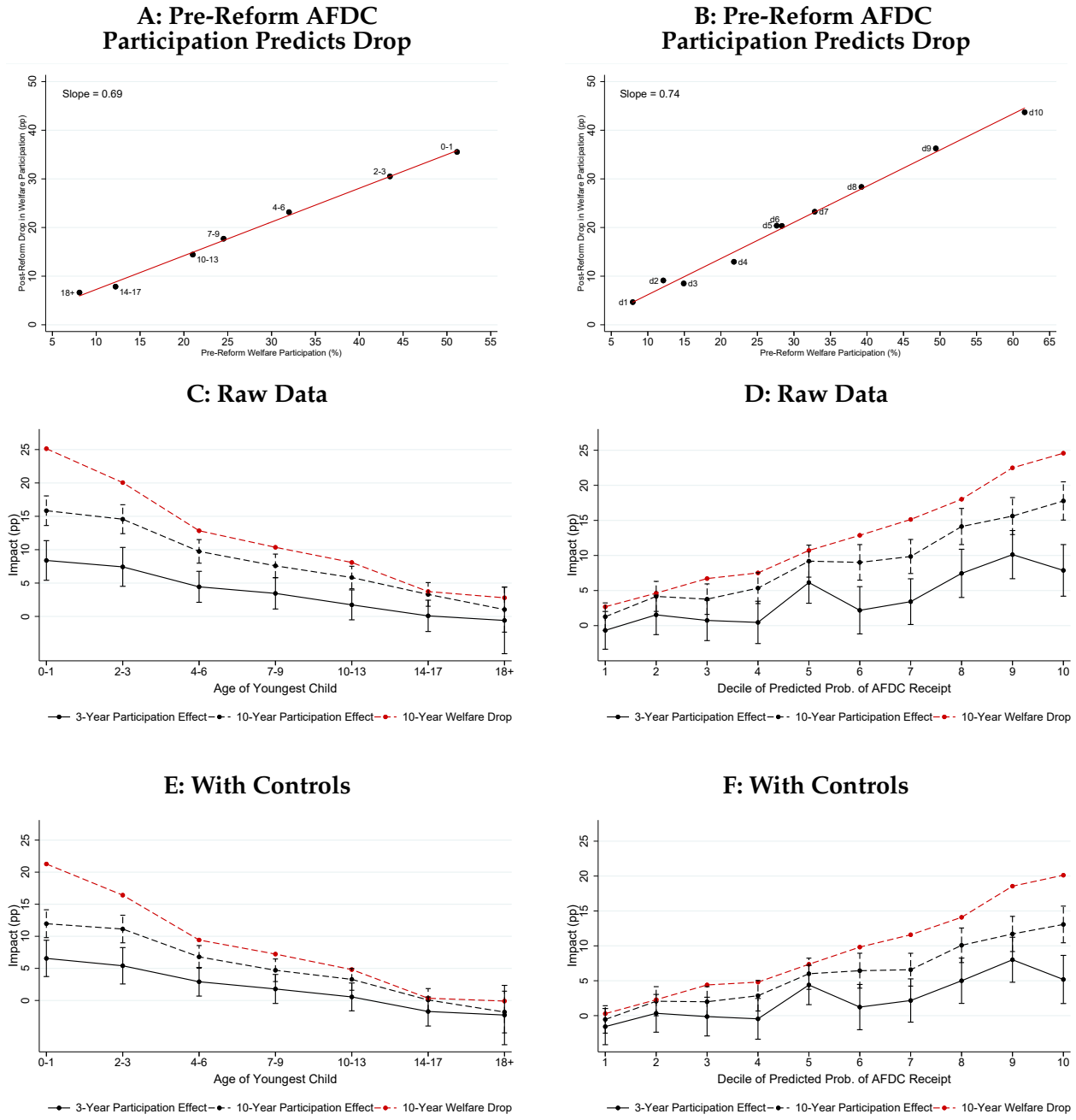
BY AGE OF YOUNGEST CHILD | BY PROBABILITY OF AFDC PARTICIPATION



Notes: This figure is constructed exactly as Figure 15 in the main text, except that it uses weekly participation as the extensive margin measure.

FIGURE A.107: EFFECTS OF THE 1993 EITC REFORM BY WELFARE TREATMENT INTENSITY
ALL SINGLE WOMEN, ANNUAL PARTICIPATION

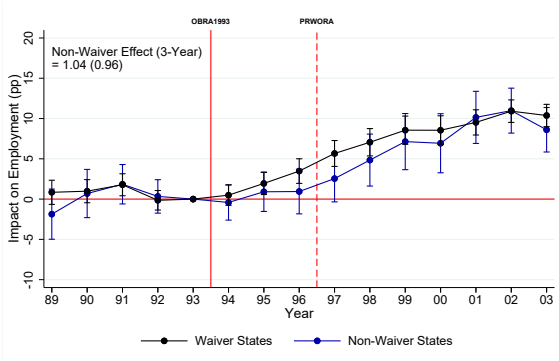
BY AGE OF YOUNGEST CHILD | BY PROBABILITY OF AFDC PARTICIPATION



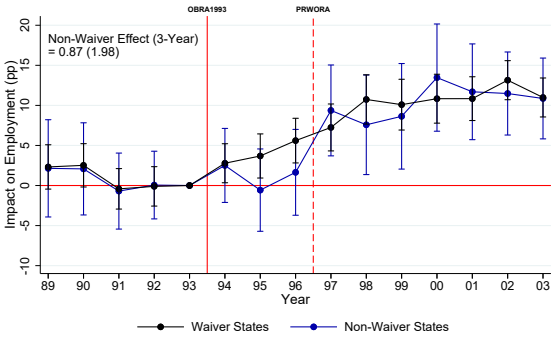
Notes: This figure is constructed exactly as Figure 15 in the main text, except that it uses annual participation as the extensive margin measure.

FIGURE A.108: HOW MUCH CAN BE EXPLAINED BY WELFARE WAIVERS?

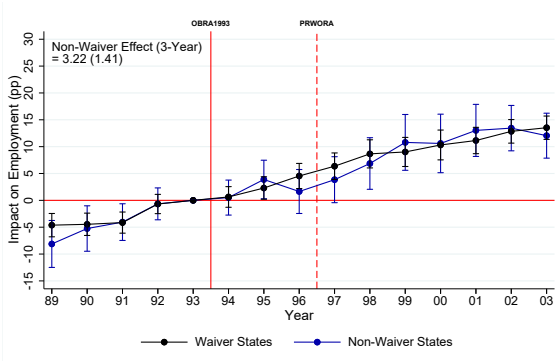
A: Weekly Employment
All Single Women



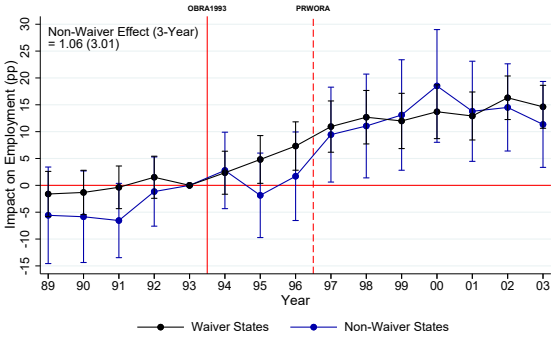
B: Annual Employment
All Single Women



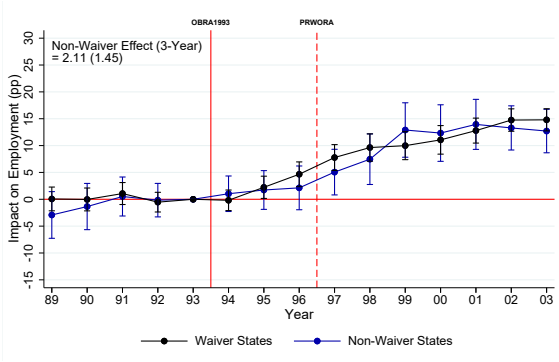
C: Weekly Employment
Low-Educated Single Women



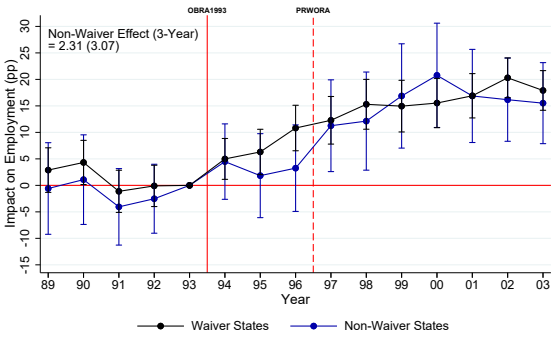
D: Annual Employment
Low-Educated Single Women



E: Weekly Employment
Single Women With Low Predicted Earnings

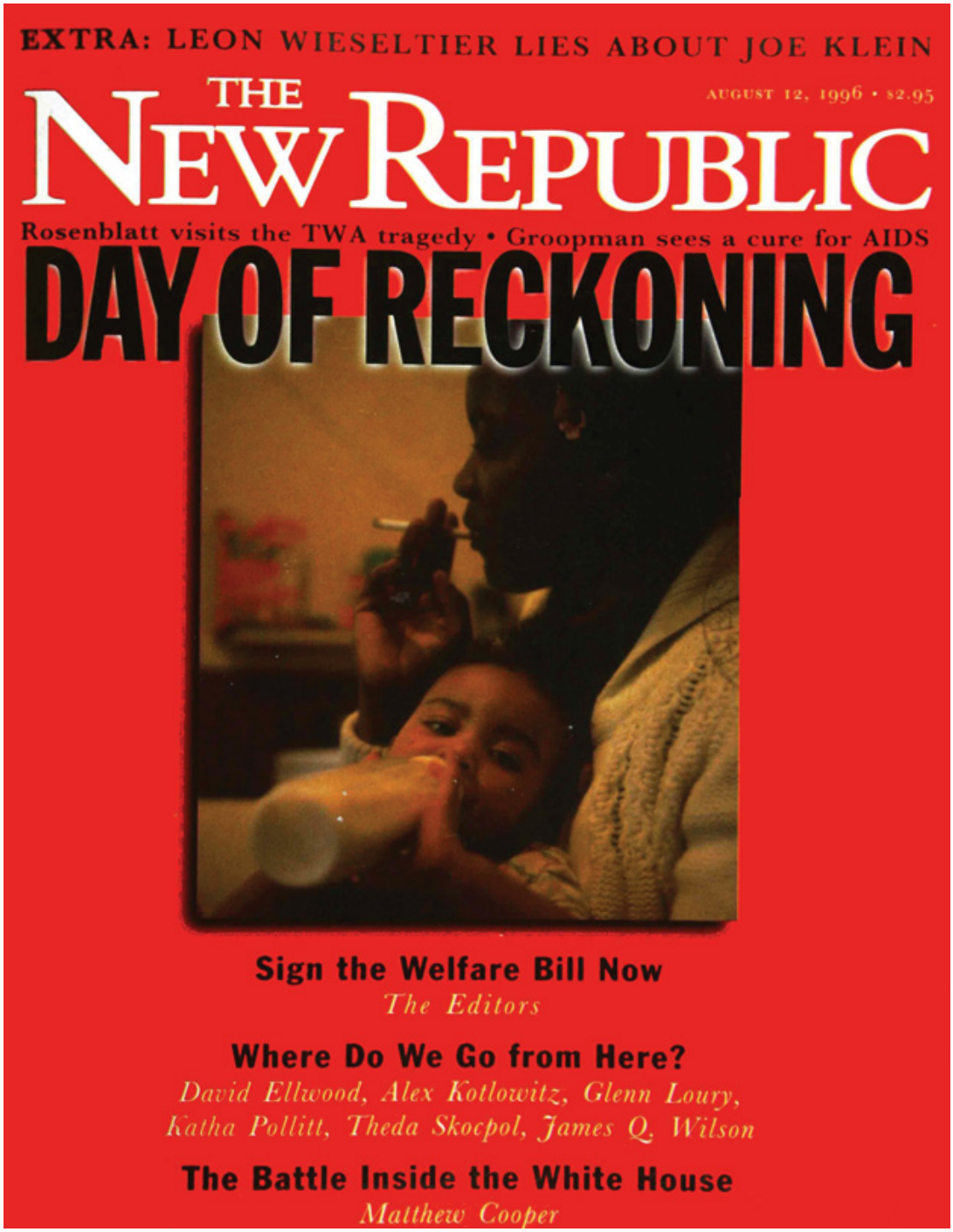


F: Annual Employment
Single Women With Low Predicted Earnings



Notes: This figure is constructed exactly as Figure 16 in the main text, but shows results for two extensive margin measures (weekly and annual employment) and three estimation samples (all single women, low-educated single women, and single women with low predicted earnings). The low-education sample includes those with a high school degree or less, while the low-earnings sample includes single women in the bottom half of the within-year distribution of predicted earnings, estimated using equation (1).

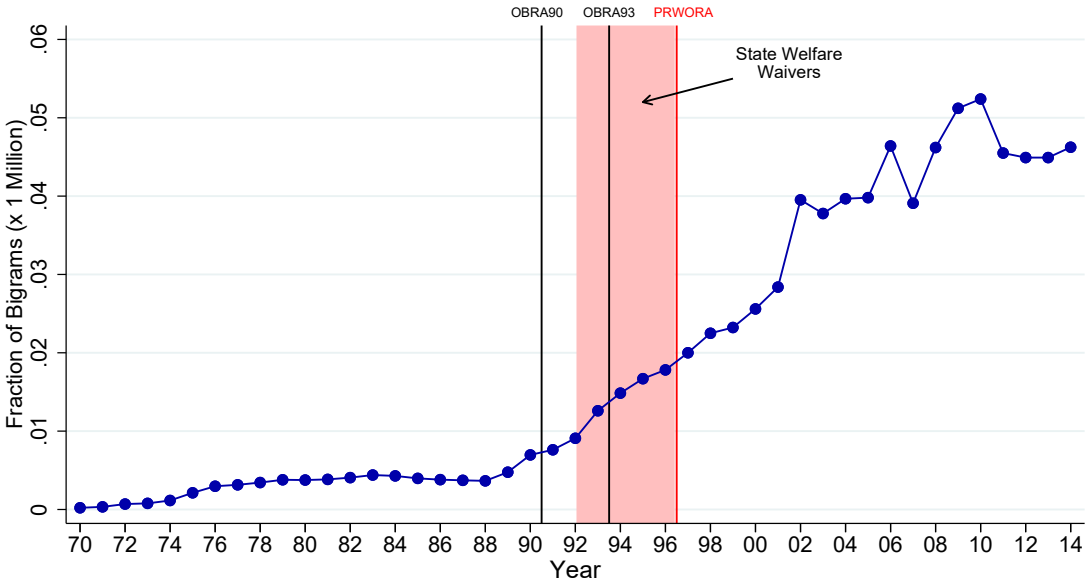
FIGURE A.109: A PICTURE IS WORTH A THOUSAND WORDS



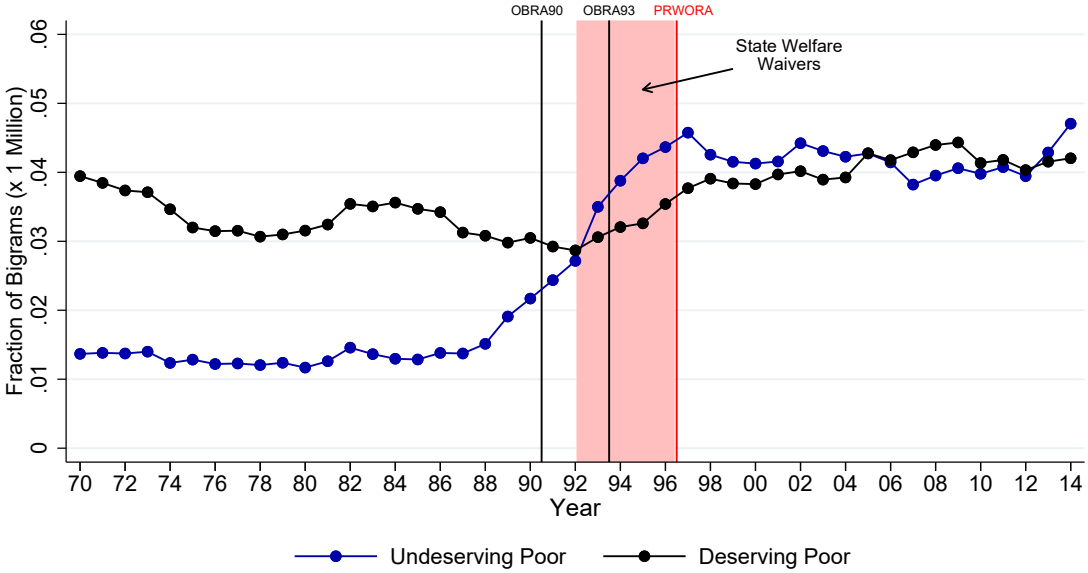
Notes: This figure shows the cover of the magazine *The New Republic* on August 12, 1996, just before PRWORA was signed into law by President Bill Clinton. It provides an example of the stereotypes and stigma surrounding welfare receipt in the 1990s.

FIGURE A.110: CHANGING SOCIAL NORMS

A: Use of "Welfare Queen" in US Publications



B: Use of "Undeserving Poor" and "Deserving Poor" in US Publications



Notes: This figure shows the usage of phrases "Welfare Queen," "Undeserving Poor," and "Deserving Poor" in books published in the US using Google Ngrams. The graphs show the five year moving average of the frequency of each phrase, which is measured as the number of occurrences of the phrase in each year, divided by the total number of bigrams in that year from the database of books digitized by Google.

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.[63] 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 1989-2019 and the ASEC files from 1968-2019.[64] 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

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

[64] 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.[65] 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

[65] 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, Polivka, and Rothgeb (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, Polivka, and Rothgeb (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. Longitudinal 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, Mok, and Sullivan 2015; Jones and Ziliak 2020). Household non-interview rates have risen from 7-9 percent in 2004 to 13-15 percent in 2017.[66]

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

[66] 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.[67] 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.[68] 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.[69] These restrictions leave me with a sample of 4,809,195 individual-month observations across survey years 1968-2019.

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.

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

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

[69] 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 EITC 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 ($lowedB$): takes on a value of one if respondent has less than a HS education ($edlevel = 1$) and zero if the respondent has HS education and above ($edlevel = 2 - 3$).

- Race ($raced$): takes on a value of one if respondent is white (IPUMS variable RACE = 100), a value of two if the respondent is black (RACE = 200), a value of 3 if the respondent is Asian/Pacific islander (RACE = 650-652), and a value of four otherwise (RACE = 300 and RACE = 700-830).

- 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.[70]

- 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.

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

B.2 Supplementary Data

Data on state welfare waivers comes from the Department of Health and Human Services (HHS).[71] I follow HHS and consider major statewide waivers in the following six categories: termination time limits, work requirement time limits, JOBS exemptions, JOBS sanctions, family caps, and earnings disregards.

Data on federal EITC parameters come from the Tax Policy Center.[72] Data on state EITC parameters come from the Tax Policy Center,[73] the National Bureau of Economic Research (NBER),[74] and various state-specific sources.

[71] Health and Human Services, Assistant Secretary for Planning and Evaluation, State Implementation of Major Changes to Welfare Policies, 1992-1998. Retrieved from https://aspe.hhs.gov/report/state-implementation-major-changes-welfare-policies-1992-1998.

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

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

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

C Tax Simulations

I use NBER's tax simulation model (TAXSIM) to calculate the average tax rate (ATR) relevant for extensive margin labor supply decisions. The average tax rate includes tax liabilities from state income taxes, federal income taxes, and payroll taxes. It is defined as

$$ATR(Y) = \frac{(T_f(Y) - T_f(0)) + (T_s(Y) - T_s(0)) + T_p(Y)}{Y}, \tag{7}$$

where Y is earnings conditional on working, $T_f(Y)$ and $T_f(0)$ ($T_s(Y)$ and $T_s(0)$) are the federal (state) income tax liabilities when working and not working, respectively, and $T_p(Y)$ is the payroll tax liability (Federal Insurance Contributions Act tax) when working.

TAXSIM requires information on income variables, dependents, and demographics.[75] 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.
- 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 either zero (when not working) or Y (when working). As will be described below, I use two different approaches to obtain Y from the CPS data.

[75] The full list of TAXSIM inputs is listed online at https://users.nber.org/~taxsim/taxsim32/

- 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 ($pwages = Y$) and when not working ($pwages = 0$). The setting of earnings when working, Y, is done differently for the elasticity approximations (see section C.1 below) and the labor supply simulations (see section D.1 below).

C.1 Calculation of ATRs for Figure 2 and Elasticities

The calculation of ATRs requires a measure of earnings conditional on working for both workers and non-workers. For each woman observed in year t with n children, I set earnings when working equal to the first kink of the federal EITC in year t for families with n children. Prior to 1975, there was no EITC. Hence, for the years 1968-1974, I set earnings equal to the first kink of the federal EITC in 1975, adjusted for inflation. Prior to 1994, there was no EITC for families without children.

For childless women during the years 1968-93, I set earnings equal to the first kink of the federal EITC for childless families in 1994, adjusted for inflation.

The resulting ATRs for single women with different numbers of children between 1968-2018 are plotted in Figure 2. In this figure, because TAXSIM only includes *state* tax laws from 1977 onwards, the calculation of ATRs prior to 1977 is based on state ATRs in 1977 within cells of number of children (0, 1, 2, 3+) and cells of education (4 categories).

The extensive margin elasticity with respect to the average net-of-tax rate is defined as

$$\varepsilon \equiv \frac{\Delta P / P}{\Delta \left(1-\tau\right) / \left(1-\tau\right)}. \tag{8}$$

The numerator of the elasticity is the percentage effect of a given tax reform on the extensive margin labor supply of single mothers. I focus on average three-year effects to avoid overlap between reforms. Specifically, ΔP is estimated as the difference-in-differences between treatment and control groups using a modified version of equation (2) in which the three year dummies just after the reform are collapsed into a single post dummy. The baseline employment or participation rate of single mothers P is calculated using data from the pre-reform year. The numerator $\Delta P / P$ is obtained by dividing the two population averages.

The denominator of the elasticity is the percentage effect of the tax reform on the average net-of-tax rate. That is, the calculation of extensive margin elasticities is based on $\tau = ATR$, using the tax rate calculations shown in Figure 2. To be consistent with the numerator, I focus on the average three-year change in the ATR, calculated as the difference-in-differences between treatment and control groups using the following specification

$$ATR_{imt} \quad = \quad \alpha \cdot Post_t + \beta \cdot Kids_i + \gamma \cdot Post_t \cdot Kids_i + \nu_{imt}, \tag{9}$$

where $Post_t$ is a dummy variable equal to 1 in the years after the reform. The regression is run on a four-year sample, the pre-reform year and three post-reform years, ensuring that the time horizon is consistent with the numerator. The reform-induced change in the average net-of-tax rate for the treatment relative to control groups, $\Delta\left(1-ATR\right)$, is given by $-\gamma$. The baseline tax rate is the ATR of single mothers in the pre-reform year. The denominator $\Delta\left(1-ATR\right) / \left(1-ATR\right)$ is obtained by dividing the two population averages.

D Benefit Simulations

To simulate the impact of the 1993 tax reform on extensive margin labor supply (as presented in section 7.1), I calculate participation tax rates that account for both taxes paid and benefits lost when entering the labor market. To this end, I combine NBER's tax simulation model (TAXSIM) with a benefit calculator that includes Aid to Families with Dependent Children (AFDC) and Food Stamps (FS). The participation tax rate (PTR) is defined as

$$PTR(Y) = ATR(Y) + takeup \times \frac{B(0) - B(Y)}{Y}, \tag{10}$$

where $ATR(Y)$ is the average tax rate defined in equation (7), $takeup$ is the average take-up rate of benefits by eligibles, while $B(0)$ and $B(Y)$ are the annual benefits from AFDC and Food Stamps when not working and working, respectively.

Annual benefits from AFDC and Food Stamps when not working ($B(0)$) are calculated as

$$B(0) = (AFDC(0) + FS(0)) \cdot 12$$

where $AFDC(0)$ and $FS(0)$ denote the maximum monthly benefits from AFDC and Food Stamps (in 1993), which vary by family size and by state.

Next, I calculate the annual benefits from AFDC and Food Stamps when working ($B(Y)$). 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

$$\begin{aligned} AFDC(1) &= \min\left(PaymentStandard - NetIncome_{AFDC}, AFDC(0)\right) \cdot \mathbf{I}_{AFDC} \\ FS(1) &= \left(FS(0) - 0.3 \cdot NetIncome_{FS}\right) \cdot \mathbf{I}_{FS}, \end{aligned}$$

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. These formulas account for the fact that $AFDC$ has a phase-out rate of 1 (each additional dollar of net income implies \$1 less of $AFDC$), while FS has a phase-out rate of 0.3 (each additional dollar of net income implies \$0.3 less of food stamps). The variable $PaymentStandard$ 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 $AFDC(0)$, but some states set $PaymentStandard$ to a level above $AFDC(0)$. The net income measures ($NetIncome_{AFDC}$ and $NetIncome_{FS}$) are defined as follows

$$\begin{aligned} NetIncome_{AFDC} &= \max\Big(GrossIncome - WorkExp - ChildCareExp \cdot n \\ &\quad - EarningsDisregard_{AFDC}, 0\Big) \\ NetIncome_{FS} &= \max\Big(GrossIncome - ShelterCosts - ChildCareExp \cdot n \\ &\quad - StandardDeduction - EarningsDisregard_{FS}, 0\Big). \end{aligned}$$

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

- Gross Income ($GrossIncome$) is calculated by dividing annual wage and salary income (Y) by the number of months worked ($MonthsWorked$).[76] The variable $MonthsWorked$ is calculated from the IPUMS variable WKSWORK1, which is the number of weeks worked. For non-workers, I impute a value for $MonthsWorked$ 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, Morales, Ruggieri, and Zidar (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, Morales, Ruggieri, and Zidar (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 ($EarningsDisregard_{AFDC}$) 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.

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

[76] In principle, Gross Income also includes unearned income (interest income, dividends, capital gains, etc.), but I set such income equal to zero in this sample of single women.

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

- 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 (Y) net of the standard deduction ($StandardDeduction$) and the earnings disregard ($EarningsDisregard_{FS}$). Following Fajgelbaum, Morales, Ruggieri, and Zidar (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 are calculated as follows

$$\begin{aligned} B(Y) &= AFDC(1) \cdot MonthsWorked + AFDC(0) \cdot (12 - MonthsWorked) \\ &\quad + FS(1) \cdot MonthsWorked + FS(0) \cdot (12 - MonthsWorked)\,, \end{aligned}$$

where $AFDC(1)$ and $FS(1)$ are functions of earnings conditional on working Y through the net and gross income measures that enter into their formulas as described above. Eligible individuals working some of the year receive benefits $AFDC(1)$ and $FS(1)$ in each month of working, and they receive the maximum benefits $AFDC(0)$ and $FS(0)$ in the remaining months.

The values of $B(0)$ and $B(Y)$ described above enter the participation tax rate formula (10). The average take-up rate $takeup$ scales welfare benefits to account for incomplete benefit take-up among eligibles. Based on the evidence presented in Blank and 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.

D.1 Calculations of PTRs used in Labor Supply Simulations

The calculation of participation tax rates requires information on earnings conditional on working, Y. Unlike the elasticity approximations described above (assuming single women enter the labor

market at the first kink of the federal EITC), here I take an exact approach that accounts for the entire distribution of earnings. Specifically, I use observed earnings for workers and predicted earnings for non-workers, where the latter is estimated based on specification (1) in the main text:

$$Y_i = \alpha_a + \beta_n + \gamma_y + \delta_e + \zeta_r + \eta_s + \nu_i.$$

Earnings are regressed on dummies for the age of the woman a (20-24, 25-29, 30-34, 35-39, 40-44, 45-50), dummies for the number of EITC eligible children n (0, 1, 2, 3, 4, 5, 6+), dummies for the age of youngest child y (0-1, 2-3, 4-6, 7-9, 10-13, 14-17, 18+), dummies for education level e (below high school, high school degree, some college, college degree), dummies for race r (white, black, Asian, and other), and dummies for state of residence s. The regression is run on a pre-reform sample (1993) of single women with positive earnings. Based on the parameters from this regression, earnings are predicted for non-workers. Those with predicted earnings below zero are dropped (131 observations).

Having obtained a measure of earnings conditional on working for both workers and non-workers, participation tax rates can be simulated using equation (10). The labor supply impact of the 1993 reform can then be simulated based on equation (4) in the main text:

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

where ΔP_t is the impact on extensive margin labor supply in year t relative to the pre-reform year 1993, ε is the extensive margin elasticity, $\Delta\left(1-\tau_t\right)$ is the reform-induced change in the net-of-tax rate on participation in year t relative to 1993, $\tau_{93} = PTR_{93}$ is the baseline participation tax rate in 1993, and P_{93} is the baseline employment rate in 1993. Because the goal is to simulate the impact of the federal tax reform act of 1993, the calculation of $\Delta\left(1-\tau_t\right)$ accounts only for *federal income tax* changes (including but not restricted to the EITC expansion), holding the tax rates from federal payroll taxes, state income taxes, and welfare benefits constant at their 1993 levels. The federal income taxes in each year t are based on 1993 earnings adjusted for inflation, thereby isolating the mechanical effect of the tax reform. Calculating $\Delta\left(1-\tau_t\right)$ as the difference-in-differences between treatment and control groups in each year t relative to the pre-reform year 1993, the implied time path of ΔP_t can be compared to the observed difference-in-differences impacts from the event studies.

E Synthetic Control Study of State Reforms

State EITC reforms are analyzed using a synthetic control approach. For each state with an EITC supplement, a synthetic control state is created from among the pool of states that never had an EITC supplement. Then I run a stacked event study comparing treatment and synthetic control states around state EITC introductions. Table A.2 lists all states with an EITC supplement and provides key details about those supplements.

E.1 Selected State Reforms

There are a total of 27 states that implemented and maintained an EITC supplement for at least 3 years.[77] I exclude six states from my analysis (Iowa, Maryland, Minnesota, Rhode Island, Vermont and Wisconsin) due to small sample sizes. These states have well below 100 single mothers per year around the time of the reform. I exclude another three states (Indiana, Ohio, and Oregon) because their EITC introduction was offset by other tax changes so that the total ATR was increased. The reform-induced change in the average tax rates can be seen from Table A.2. These tax changes are calculated by setting earnings equal to the first kink of the federal EITC for women with n children in year t, as described in Section C.

Many state supplements were very small in size, so I also select the ten largest reforms in order to make it easier to detect any positive effect. The ten largest reforms are selected as those reforms which induce a reduction in ATR of at least 3pp as shown in Table A.2. These states are Colorado, Connecticut, District of Columbia, Kansas, Massachusetts, Michigan, Nebraska, New Jersey, New Mexico, and New York.

E.2 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 six 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.

[77] A total of 30 states have instituted a supplement (see Table A.2). But the state of Washington never funded and paid out the credit, while Hawaii and South Carolina instituted their supplements only in 2018.

For each treatment state, a synthetic control state is constructed from states that never had a supplement, matching on the level of the outcome variable (employment or participation rate) in each of the five pre-reform years. Synthetic control regressions are run using the Stata command `synth`.[78]

E.3 Stacked Event Study

I create the stacked event studies using two different approaches. The first is a difference-in-differences approach where the synthetic control regressions are run on the sample of single women with children comparing different states over time. The second is a triple-differences specification which also exploits the variation between those with and without children within states.

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 studies shown in Panels A and C of Figure 11 are based on the following specification

$$P_{st} = \sum_j \alpha_j \cdot Event_{j=t} + \beta \cdot Treat_s + \sum_{j \neq -1} \gamma_j \cdot Event_{j=t} \cdot Treat_s + \nu_{st}, \tag{11}$$

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. The treatment series (solid line) corresponds to the coefficient $\hat{\alpha}_t + \hat{\gamma}_t$, while the synthetic control series (dashed line) corresponds to $\hat{\alpha}_t$.

Panels B and D of Figure 11 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

$$\begin{aligned} P_{kst} = & \sum_j \alpha_j \cdot Event_{j=t} + \beta \cdot Kids_k + \gamma \cdot Treat_s + \delta \cdot Kids_k \cdot Treat_s \\ & + \sum_{j \neq -1} \zeta_j \cdot Event_{j=t} \cdot Kids_k + \sum_{j \neq -1} \eta_j \cdot Event_{j=t} \cdot Treat_s \\ & + \sum_{j \neq -1} \theta_j \cdot Event_{j=t} \cdot Kids_k \cdot Treat_s + \nu_{kst}, \end{aligned} \tag{12}$$

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

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

series (dashed line) corresponds to $\hat{\zeta}_t$.

F Employment + Welfare Rate

The "employment+welfare rate" is defined as the fraction of people who are employed and/or on AFDC/TANF. To measure the employment+welfare rate for single women, I add the number of employed people and the number of AFDC/TANF participants (who are not also employed, to avoid double counting) using CPS March files. Panel A of Figure A.99 plots the employment rate series for single women with and without children, as well as the employment+welfare rate series for single women with children. Because single women without children are ineligible for AFDC/TANF, the employment and employment+welfare series are the same for this group. Panel B of Figure A.99 plots DiD estimates γ_t (based on equation 2 without controls) for the employment rate and the employment+welfare rate. Figure A.100 plots DiD estimates γ_t^n by number of children n (again, without controls) for the two outcomes.

A concern with this analysis is that AFDC/TANF participation may be underreported in the CPS data and that such underreporting is changing over time (see Meyer and Mittag 2019; Meyer, Mok, and Sullivan 2015). Therefore, I also use administrative data from the Department of Health and Human Services on the number of AFDC/TANF recipients in each year. The publicly available data provides information on families, not separated by marital status or gender, thereby preventing me from reproducing the analysis for single mothers using the administrative data. But given very few families are headed by single fathers (6.5% of families in the CPS data around the 1993 reform), the number of *families* receiving AFDC/TANF is approximately equal to the number of *women* with children receiving AFDC/TANF. Hence, I calculate the welfare participation rate for all women with children by dividing the total number of welfare recipients in the administrative data by the population of women with children based on the weighted CPS sample (women aged 20-50). To calculate the employment+welfare rate for women with children, the welfare participation rate is added to the employment rate (i.e., the fraction of women with children who are employed but not on AFDC/TANF, to avoid double counting). Panel A of Figure A.101 plots the employment rate series for women with and without children, as well as the employment+welfare rate series for women with children. Panel B of Figure A.101 plots DiD estimates for two outcomes in each year.