Men Too: The Effects of Welfare Payment Time Limits on Male Labor Market Outcomes

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Abstract

Despite the extensive literature on the economic and social effects of the U.S. welfare reform, no study to date has evaluated its effects on male adults, even though males comprise a non-negligent fraction of adult welfare-payment recipients or reside as partners in many households with a female adult recipient. I address this gap and find that welfare-payment time limits substantially improved male labor-market outcomes. Moreover, I show that decreases in welfare benefit payments cannot account for males' entire increase in employment, thereby suggesting that males responded to their female partner's loss of welfare benefits and not just their own.

Keywords: Welfare reform, male labor supply, time limits, intrahousehold dynamics

JEL Classification: D10, H53, I38, J21

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1 Introduction

The Personal Responsibility and Work Opportunities Reconciliation Act of 1996 (PRWORA) transitioned the U.S. welfare system from the Aid to Families with Dependent Children (AFDC) to the more restrictive Temporary Assistance for Needy Families (TANF), thereby triggering dramatic changes that affected low-income household eligibility for welfare benefits. The reform's key features included lifetime time limits on welfare payment receipt, work requirements, and financial incentives to work. Among these numerous reform components, lifetime time limits are considered to be one of the greatest departures from the pre-reform policy (Grogger (2002, 2004); Swann (2005)). Time limits restricted the number of years parents were eligible for welfare payments during their lifetime by up to five years, which is in contrast to the pre-reform guaranteed welfare-payment eligibility that lasted until the youngest child reached age 18.

The introduction of time limits prompted investigations of its effects on welfare take-up, employment, and earnings.[1] These studies were part of broader research efforts to evaluate the effects of TANF and its many other policy components on welfare take-up and labor market outcomes.[2] Despite this abundant research, no studies have evaluated welfare reform effects on males residing in low-income households. The focus of the welfare reform literature has exclusively been on female adults and more specifically on mothers with low educational attainment or single mothers, although males comprised 9-15% of TANF adult recipients during 2001-2018 (DHHS (2018)) and many females who are eligible for welfare payments are married or cohabiting with males. Even if these male adults are not directly eligible for welfare benefits, they are likely to be affected by changes in eligibility due to residing with

[1] See Chan (2013, 2017, 2018); Fang and Keane (2004); Fang and Silverman (2009); Farrell et al. (2008); Fitzgerald and Ribar (2004); Grogger (2003, 2004); Keane and Wolpin (2010); Low et al. (2020); Mazzolari (2007); Mazzolari and Ragusa (2012); Pepin (2022); Swann (2005) for negative welfare take-up effects of time limits and Chan (2013, 2017); Fang and Keane (2004); Fang and Silverman (2009); Farrell et al. (2008); Grogger (2003); Keane and Wolpin (2010); Low et al. (2020); Mazzolari and Ragusa (2012); Pepin (2022); Swann (2005) for positive employment effects. Earnings effects are less conclusive in the literature and presented in Fang and Keane (2004); Grogger (2003); Mazzolari and Ragusa (2012).

[2] See Blank (2002); Grogger and Karoly (2009); Kaushal and Kaestner (2001); Meyer and Rosenbaum (2001); Moffitt (1999); Ziliak (2015).

an eligible partner. The present study fills this gap by examining changes in labor-market outcomes among disadvantaged adult males following the introduction of time limits.

The empirical analysis exploits variation in the timing of time-limit introductions across states. Moreover, the evaluation of welfare payment time limits provides an additional methodological advantage: their implementation should have only affected individuals with a youngest child aged below a certain threshold. For example, the introduction of a five-year time limit should not affect individuals with a youngest child aged 13 or above, as the remaining years that they were eligible for welfare payments were up to five years as is. Thus, in addition to variation across time and states, there is variation across individuals within state-year units based on the age of their youngest child upon time-limit introduction.[3] As a result, I can include in my regression specifications state-year fixed effects that control for any time-varying state-level policies that took place during my sample period and may have also affected disadvantaged male labor market outcomes.[4] Thus, the analysis overcomes a frequent challenge with empirical research evaluating the effects of welfare reform, which is the large number of potentially confounding alternative policies - e.g., EITC expansions - that took place during this period. In fact, in order for an alternative policy to jeopardize the causal interpretation of my findings, it needs to rely on the same source of variation within the state-year unit that is based on the specific threshold of one's age of youngest child upon time-limit introduction.

The sample is derived from the Panel Study of Income Dynamics (PSID). As in studies evaluating female welfare reform effects, I focus on disadvantaged males - those with a high school diploma or less, those who were residing in households with government transfers (AFDC/TANF or SNAP), or were on Medicaid prior to time-limit introduction in their state. The longitudinal nature of the PSID allows me to observe individuals both before and after

[3]The exact threshold for the age of youngest child also varies across states as states introduced various extents of time limits, ranging from 21-60 months.

[4]See Chan (2018); Grogger (2002, 2003, 2004); Grogger and Michalopoulos (2003); Low et al. (2020); Mazzolari (2007) for additional studies that evaluate the effects of time limits using similar variation within the state-year unit that is based on the individual's age of youngest child.

the reform in their state of residence and estimate its effects with regression specifications that include individual-level fixed effects. The PSID also allows me to examine the effect of time limits in the long term, which for most states is more than ten years after their implementation. Event-study results are also presented to assess the dynamics of the effect over time and establish lack of differential trends among the treated observations prior to time-limit introductions.[5]

The results confirm that focusing on females when assessing welfare reform may have led to underestimating its overall effects. Male employment, hours worked, and labor income increased in response to time limits, and these effects represent 4-13 percent of pre-treatment means. While these effects are sizable, I show that they are slightly smaller than female time-limit effects using the same sample criteria and regression specification. Furthermore, these female responses are aligned with past estimates of female time-limit effects.

The analysis also shows that the decline in males' own receipt of welfare payment was substantially smaller than the increase in their employment in response to time limits. This suggests that males' increased labor market participation is not just compensating for loss of their own income but rather their female partners' loss of income as well. This is in line with past literature showing some females experiencing declines in earnings in response to time limits accompanied by unchanged household income, thereby leading to conjectures that other household members, such as a male partner, are compensating for their female partner's lost income (Grogger (2003); Mazzolari and Ragusa (2012)). Edin and Lein (1997) note that sharing housekeeping expenses with other family members or boyfriends is common in welfare families. Bitler et al. (2006) present evidence suggesting that welfare reform increased the probability of children living with other family members to compensate for their mother's lost income. This paper advances the speculations raised in Grogger (2003) and Mazzolari and Ragusa (2012) regarding time limit effects on male partners and complements the broader

[5]The analysis also addresses concerns raised in recent econometric literature with event studies using two-way fixed-effects as in this paper. I employ a two-step estimator, as introduced in Borusyak et al. (2021) and Gardner (2021). The results of this procedure qualitatively and quantitatively resemble my main results, thus demonstrating the appropriateness of my event-study framework.

findings on welfare reform and household earning dynamics in Edin and Lein (1997) and Bitler et al. (2006) by specifically evaluating the effects of time limits on male labor market outcomes.

The results highlight two important economic insights regarding labor market interventions or economic shocks more broadly: first, prime-age males respond to labor market policies that are targeted at the disadvantaged; second, intra-household interactions can play a vital role in understanding the overall effects of various policy measures or shocks.

Evaluations of labor market policies that target disadvantaged populations have mostly focused on either the younger segments of the labor force, or when examining older age groups, on females. Research on the EITC has indeed demonstrated very small or null effects on men (Agan and Makowsky (2018); Eissa and Hoynes (2004); Hoffman and Seidman (2003), but minimum wage effects on disadvantaged fathers have recently been evaluated for the first time, with evidence of substantial responses (Godøy et al. (2021)). Welfare payment policy evaluations have similarly focused to date on disadvantaged mothers, possibly reflecting a notion that prime-age men have near-zero labor supply elasticities. This is despite evidence of large elasticities for low-educated males (Juhn et al. (1991)). The scarce attention to male labor supply responses in the context of welfare payment policies is surprising considering the decline in male labor force participation rates documented over the last few decades, with an emphasis on low-educated and prime-age males (Abraham and Kearney (2020); Blank (2009); Holzer and Offner (2006); Krueger (2017); Moffitt et al. (2012)). As this decline has been at least partially attributed to supply-side factors, a better understanding of male labor-supply drivers can assist in suggesting policy measures that can potentially reverse these downward trends.

This paper's findings on male responses to their female partner's loss of welfare benefits emphasize the importance of taking into account all members of the household when evaluating policy reforms, even if some household members may not be directly affected by them. Male partners appear to be mitigating the financial effects of decreased welfare payments, despite not necessarily being the direct household member that experiences the income loss. Chiappori and Mazzocco (2017) provide an overview of household decision models that allow separate actions for each household member. There are indeed numerous studies that find a spousal reaction to their partner's direct impact from a labor market intervention, policy shock, or income shock, including works concerning the added worker effect (Bredtmann et al. (2018); Halla et al. (2020); Lundberg (1985); Stephens (2002)), evaluations of tax liabilities that are calculated at the couple level (Eissa and Hoynes (2004); LaLumia (2008)), or changes in unemployment benefits and other wage shocks (Blundell et al. (2016); Cullen and Gruber (2000)). However, in all these studies, it is the female spouse who is responding to shocks experienced by her male partner. A recent exception is Chan et al. (Forthcoming) that find male declines in welfare take-up in Australia in response to introducing work requirements for their wives. However, the authors explain that this decline is likely due to mechanical loss of welfare payment eligibility on behalf of the husband rather than it being a response to an income shock experienced by the wife.[6] This paper diverges from many of these past studies by highlighting the male's response to his female partner's shock. By doing so, I account for both intrahousehold dynamics and elasticities in the male labor supply function and integrate them within a single analytical framework.[7]

The paper proceeds as follows. In Section 2 I provide background on the U.S. welfare reform, followed in Section 3 by a discussion of the underlying channels through which welfare reform may affect males. Section 4 describes the data and Section 5 the empirical strategy.

[6]In Chan et al. (Forthcoming) work requirements are introduced to married females in order to be eligible for welfare benefits in Australia. They find a male partner decline in welfare take-up that is identical in size to the female decline. This decline is likely due to loss of welfare benefit eligibility at the household level, as the female increase in labor market participation negatively affects her husband's eligibility for welfare benefits. Thus, the male decline in welfare take-up is not a response to an income shock but rather a response to mechanical changes in his welfare payment eligibility. This study differs from Chan et al. (Forthcoming) as most males in the sample are not receiving welfare payments simultaneously with their female partners and may not even be eligible for the welfare payments their female partner is receiving or potentially eligible for. As such, changes in the female partner's welfare payment eligibility is more of an income shock rather than indirectly affecting the male's welfare payment eligibility.

[7]Structural models can provide a solution to potentially endogenous labor market decisions by both husbands and wives. Borella et al. (Forthcoming) are among the first to do this for evaluating taxation and social security payments.

Results are discussed in Section 6 and Section 7 presents a series of robustness checks. The paper closes with concluding remarks.

2 Background

The departure from AFDC preceded the PRWORA by state-led reforms that became popular in the early 1990s. These reforms were implemented under waivers that were granted federally to states who wished to change their welfare program. Lifetime time limits on the receipt of welfare payments were among the more common waivers, ranging from 21 to 60 months. Under AFDC, eligibility for welfare payments was guaranteed until one's youngest child reached age 18. Upon implementation of PRWORA, welfare payments were federally limited to five years. States could offer lifetime welfare payments for more than five years at their own expense, or restrict time limits even further than what was federally provided (using the funds for other welfare-related measures). This resulted in variation across states in time-limit implementation in two aspects: first, in the timing of time-limit introductions, and second, in the extent of time limits. Arkansas, Colorado, and Georgia adopted time limits already in 1992 while the latest state was Vermont in 2014. By 2014, all states had initiated time limits.

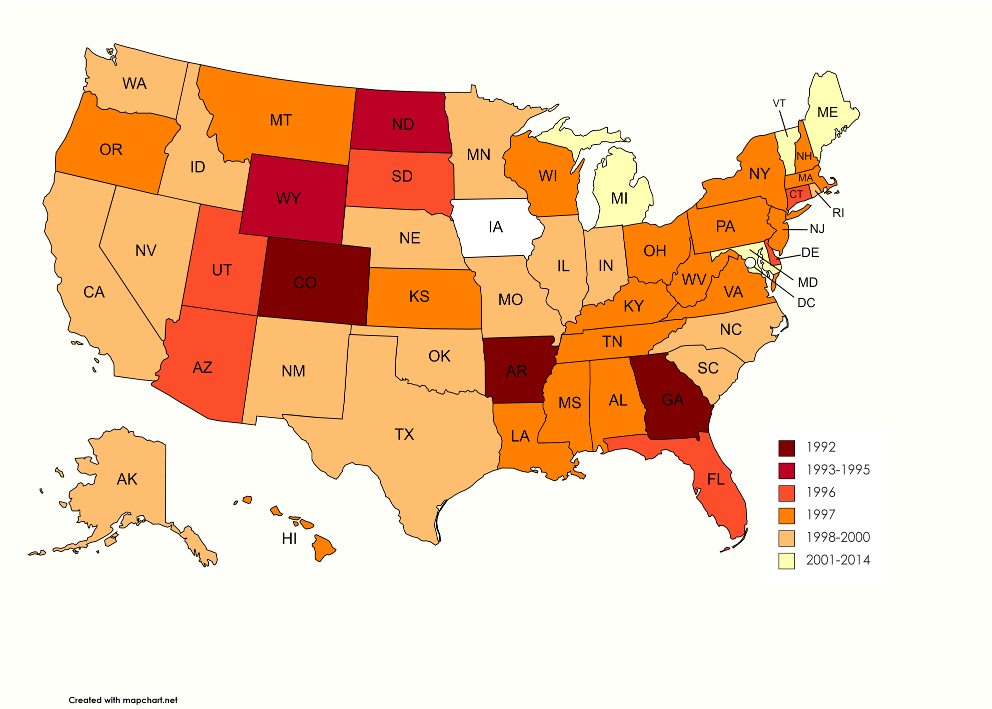
Figure 1A and Figure 1B map the year each state's time limits were introduced or their extent upon introduction (in months).[8] I exclude Iowa and Washington, D.C., as time limits were implemented on an individual basis in the former (Chan (2013)) and repealed four years after introduction in the latter. I assign time-limit implementation to states even if it applied only to adult household members and not children, as is the case in several states, such as California and Maryland.[9]

[8] My data is at the annual level, thus requiring me to determine the timing of time limit introductions at the annual level. As such, time limits introduced prior to July were assigned as introduced that year, whereas time limits introduced from July onward were assigned as introduced in the next year.

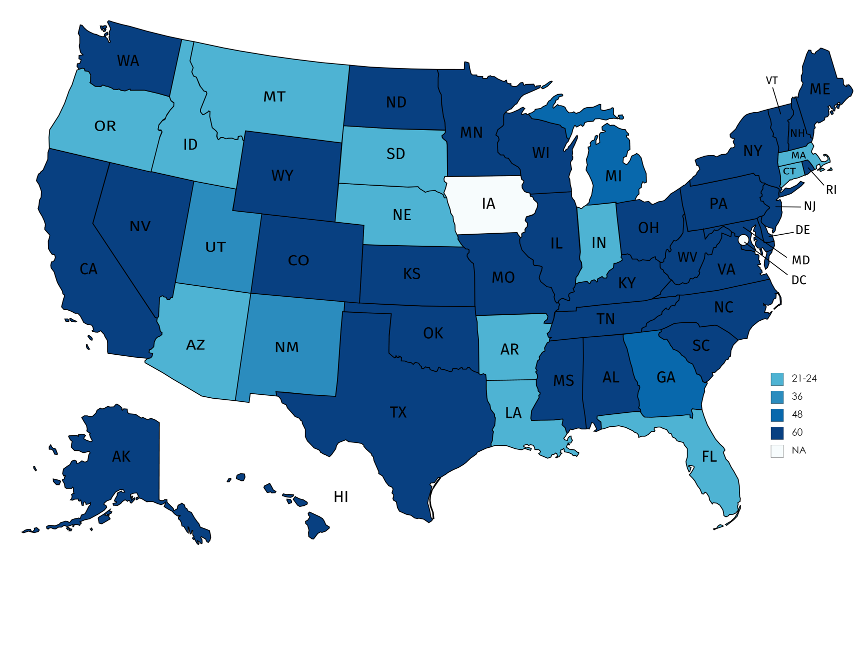
[9] A review of the vast literature on welfare time limits can reveal several discrepancies across studies in their documentation of time limit timings. I thus independently constructed my database of time-limit introductions using the Urban Institute WRD, in addition to several rounds of communication with the Center on Budget and Policy Priorities. Pepin (2022) is a recent study evaluating time-limit introductions and

Figure 1: Time Limits by State

(A) Time-Limit Introduction Year



(B) Time-Limit Extent (in Months) Upon Introduction



Notes: The top and bottom figures above present the year of time-limit introduction and the number of months individuals were eligible for welfare payments during their lifetime upon the implementation of time limits, respectively. Iowa and Washington, D.C. are excluded from the sample. See Data Section for additional details.

Although the AFDC was historically initiated for supporting single mothers, in 1961 Congress expanded the program to include households with both natural parents but with the provision that the primary parent was unemployed (Moffitt (2003)). Annual statistics on characteristics of TANF cash assistance recipients collected by the Department of Health and Human Services (DHHS (2018)) indicate that since the mid-2000s, the fraction of males among adult TANF adult recipients has been 9-15 percent. In addition, during 2001-2018, 10-14 percent of TANF adult recipients were married. Assuming a large fraction of these recipients are females and that marriage does not include cohabitation, the pool of males residing in households affected by welfare eligibility rules is beyond just those directly receiving or eligible for payment. In fact, in Moffitt et al. (2020), over 50 percent of mothers eligible for welfare payments in their sample from the Survey of Income and Program Participation resided with a male partner or spouse. Furthermore, 30 percent of these males were not the biological fathers of the children in the household. Overall, these figures demonstrate a clear presence of male adults - not necessarily biological fathers - in many households potentially eligible for welfare payments.

It is important to note that in many states male partner income is not included in calculating the income threshold for the female's welfare payment eligibility. According to Moffitt et al. (2009), in 2006, 18 states did not include male partner income in household income calculations if the male was not the biological father of all children in the household. If the male was not the biological father of any of the children, and the couple was not married, in 2006, his income was not included in total household income calculations in any state. In 1993, for married couples with a male who is not the biological father of any of the children, only seven states included male income in calculations for welfare payment eligibility, and this increased to 20 in 2006.[10]

expansions in the 2010's (and discussing also introductions prior to this period). All time-limit introductions in her independently-constructed database are identical to the documentation in my database.

[10]Moffitt et al. (2009) present interesting variation across states in welfare eligibility rules based on the male partner's status within the household that can arguably contribute to the analysis in this study. Unfortunately, these rules are only available for 2006 and in some cases also for 1993. The authors explain quite elaborately about their unsuccessful efforts to attain the rules for other years, which would be necessary

3 Why Welfare Time Limits Can Affect Males

The implementation of time limits can affect male labor-market outcomes through several potential channels. First, males may increase their labor force participation in response to a decline in their own income through loss of their welfare benefits. Second, males may change their labor force participation patterns in response to changes in their female partner's income due to welfare reform. Here, it is not clear whether their partner is experiencing an income decline due to welfare benefits lost, an income increase due to greater labor force participation replacing welfare payments, or neither as increases in partner labor-market income may offset lost welfare payments.

It may be that a disadvantaged couple's income is unchanged with time-limit introduction because the male or female may have not received welfare payments prior to the reform or they may continue to receive payments even after the reform. However, males in such households may still respond to time limits through greater labor force participation as a precautionary measure if they are concerned that their or their partner's future eligibility for welfare payment will decline. Indeed, many studies have found female responses to time limits before hitting the actual limit (Grogger (2002, 2003); Grogger and Michalopoulos (2003); Mazzolari and Ragusa (2012); Low et al. (2020)).

Welfare reform can also affect marriage or co-habitation patterns. However, the direction of this effect is unclear, since multiple predictions on male labor market effects are generated. The possibility that welfare reform decreases divorce rates due to marriage becoming more attractive given spousal income as an alternative insurance measure to the safety net provided by welfare benefits (Acs and Nelson (2004); Bitler et al. (2004); Low et al. (2020)) can lead to greater intra-household bargaining power for the male partner (Low et al. (2020)), who may respond by reducing labor force participation. Welfare reform can also increase divorce rates due to the male partner's assets or income decreasing the likelihood of being eligible for welfare payments (Moffitt et al. (2020); Ziliak (2015)) or females' greater eco-

for a precise analysis of their interaction with time limits.

nomic independence due to increased labor force participation (Bitler et al. (2004)). In such situations males may wish to increase their attractiveness in the marriage market or in an existing relationship, thereby improving their labor-market outcomes.

Lastly, if welfare reform affects labor-market patterns among disadvantaged mothers, this can have equilibrium implications for males, who may face greater competition in the unskilled labor market due to greater labor supply. Groves (2016) shows that this increased competition decreased labor force participation among low-skilled young single males. However, the results in the forthcoming analysis cannot be affected by this potential mechanism as the males in the sample are older and over 75 percent of them reside with a female partner (see Summary Statistics in Table 1).

4 Data

4.1 Time Limit Introductions

The empirical analysis relies on documentation of time-limit introduction years in each U.S. state and their extent. To this end, I utilized the Welfare Rules Database (WRD) available from the Urban Institute and other publicly available online sources.[11]

4.2 Panel Study of Income Dynamics Data

I use the PSID database for individual outcomes, which since 1968 has been longitudinally tracking individuals in thousands of families throughout the U.S. The following labor market outcome variables were constructed from the database: whether the individual is employed (indicator), annual hours worked, and annual labor income (in 2010 USD). In addition, I utilize questions about welfare payment receipt to construct three variables: whether the household head or their partner received welfare payments over the course of a year, whether

[11] The Center on Budget and Policy Priorities provides noteworthy reports that were also utilized for constructing the database.

the male in the sample received welfare payments, and whether the male's female partner (if he has one) received welfare payments. The PSID inquires specifically about each household member's receipt of welfare payments. All these variables are available for all PSID waves, although not always are all individuals covered.[12]

The time span for observations from each state is up to 15 years before or after the introduction of time limits, with the limitation that data is between 1980 and 2016.[13] I limit the sample to disadvantaged males who are more likely to be affected by changes in welfare policies - either directly themselves or through the partners they reside with. This entails examining individuals who fulfill one of the following: having a high school education or less, received themselves or resided with a partner receiving government transfers (AFDC/TANF or SNAP) prior to treatment in their state, or were on Medicaid prior to treatment in their state.[14]

I limit the sample to individuals aged between 18-60. The treatment and control groups are defined based on the age of the youngest child upon time-limit introduction.[15] Thus, in order for the treatment and control samples to be relatively comparable, I further limit the sample to those aged between 19-55 upon time-limit introduction in their state, the age when males generally reside with minor children. I also drop from the sample individuals who moved a state up to five years after the introduction of time limits in their original state

[12]Beginning in 1997, PSID waves are every two years rather than every year.

[13]PSID inquires about hours worked, labor income, and welfare payment receipt "last year". Due to this, I assign to each observation in the dataset the year preceding their survey year. This entails that the employment variable - which is contemporaneous for the survey timing - reflects a delay of the time limit variable by one year. I maintain this consistency in constructing the employment variable in order to have a consistent sample across all dependent variables evaluated, as the sample criteria are such that the year an observation is matched to determines whether or not it is in the sample. Regression results for employment when the dataset assigns the survey year to each observation (not shown) are nearly identical (coefficient estimate is 0.0366, as opposed to 0.0373 in Table 2, with a p-value of 0.049).
I note that the wording "last year" when inquiring about hours worked, labor income and welfare payment receipt is maintained even when PSID transitioned in 1997 to waves that are every two years rather than every year.

[14]In Maine and Vermont, time limits were introduced after 2010, when Medicaid coverage expanded substantially due to the Affordable Care Act. In order not to bias the sample such that the latest states to introduce time limits have a different sample of disadvantaged households, for these states, the criteria is for individuals on Medicaid prior to 2010.

[15]The age of one's youngest child is the minimal age of either one's own youngest child or the youngest child residing with the individual upon time-limit introduction.

to ensure capturing a more precise treatment effect.[16] Lastly, due to some extreme values observed for labor income, the sample for hours worked and labor income as dependent variables drops observations with a positive number of hours worked and an hourly wage calculated that is less than 5 2010 USD unless it exceeds 0.9 of the minimum wage set for their state and year,[17] or observations with a calculated hourly wage that is more than two standard deviations above the mean hourly wage observed for this individual during the sample period. Under these limitations, the sample covers 4,278-5,822 males over 33,341-46,885 observations.

4.3 Treatment Variable Assignment

The main explanatory variable is an indicator for whether an adult's eligibility for welfare payments was affected upon the introduction of time limits in their state of residence. This is based on the length of the time limit and the age of the youngest child residing with them (not necessarily their biological child) upon introduction.[18] For example, an individual whose youngest child was 14 when their state implemented a 60-month time limit would not be affected because both before and after they were eligible for welfare payments until their youngest child reached age 18. However, an individual in the same state whose youngest child that same year is only 12 years old would be affected. Furthermore, if the same individual with a youngest child aged 14 were residing in a state that implemented a 24-month time limit, they would be considered treated.

Figure 2 illustrates the variation across time and states for the effect time limits had on adult eligibility for welfare payments. According to Figure 2A, an individual with a youngest child born in 1990, prior to any time-limit introductions, was eligible for welfare

[16] This results in dropping 123 individuals, or roughly 2 percent of individuals originally covered based on the sample criteria.

[17] Minimum wage data is from Meer and West (2016). Based on this, the lowest real minimum wage during the sample period is 4.6 2010 USD.

[18] Results will also be presented when the treatment variable is an intensity-of-treatment variable that measures the number of years of potential welfare benefit years lost upon time-limit introduction. See Section 6.3.

payments for up to 18 years during his lifetime. In Georgia in 1992, upon a four-year time-limit introduction, this individual's youngest child would be two years old, and therefore, the maximum number of years they can receive welfare payments over their life-cycle decreases to six years. This accounts for two years since the child was born plus the four-year time limit. If this individual resides in Massachusetts, which introduced a two-year time limit in 1997, then by the time their youngest child reaches 7 years old, and with the additional two-year time limit, the maximum number of years they can receive welfare payments drops from 18 to nine years. Similarly, in California, this individual's maximal number of years for receiving welfare payments changes from 18 to 14 years in 1999, with the introduction of a five-year time limit.

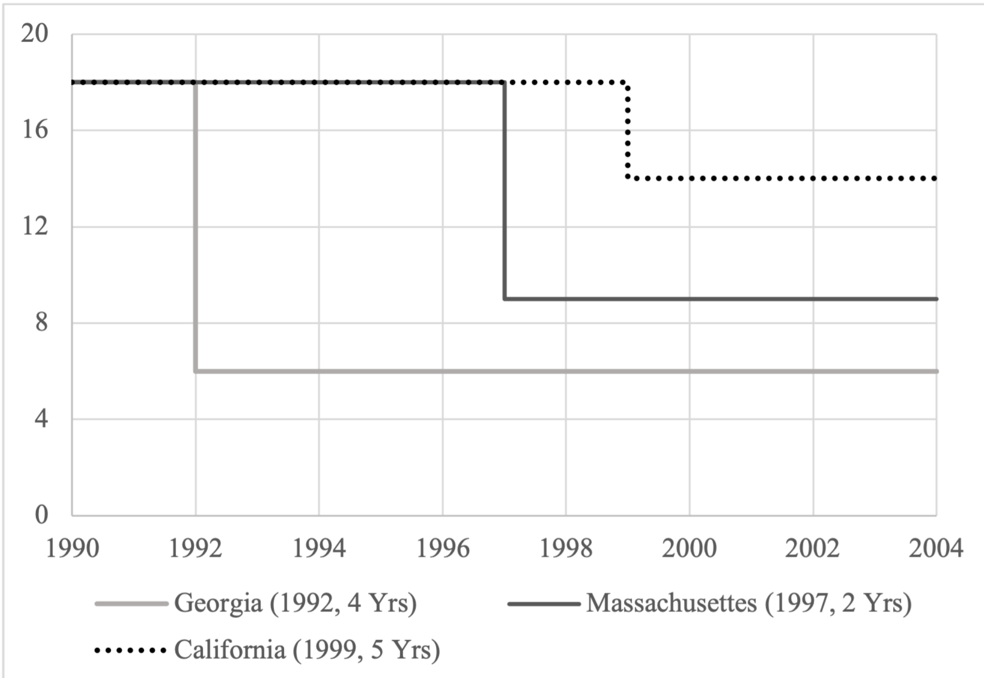
A comparison between Figure 2A and Figure 2B illustrates the within-state variation of time-limit effects across parents. In particular, a parent in California with a youngest child born in 1990 is affected by time-limit introduction in 1999, whereas a parent with a youngest child born in 1985 is not affected, as by 1999 their youngest child is 14 years old and a five-year time limit still leaves them eligible for welfare payments for up to 18 years. The fact that time-limit lengths ranged from two to five years entails that whether a parent is affected or not depends on whether their youngest child is older or younger than 13-16 years upon time-limit introduction.

4.4 Summary Statistics

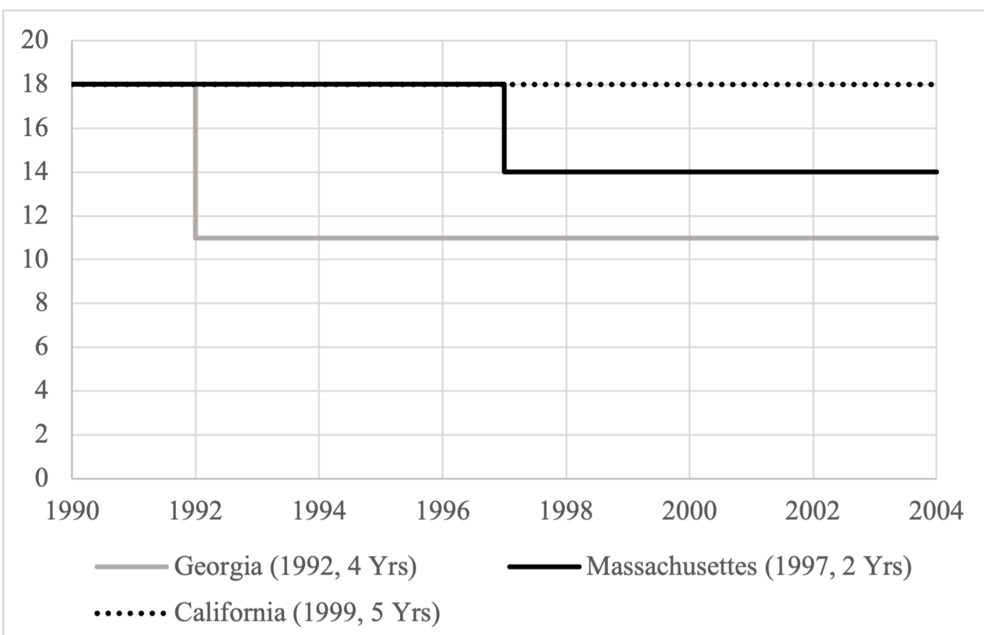
Summary statistics (means and standard deviations in parentheses) are presented in Table 1 for the entire sample, and then at the individual level for those experiencing a change in potential eligibility for welfare payments upon time-limit introduction and untreated males from the latest year up to five years prior to time-limit introduction. Table 1 demonstrates substantial differences between treated and untreated males. Treated males prior to treatment are younger, with younger children, and are more likely to be married/cohabiting. These demographic differences are mechanical, as treatment is primarily for those with younger

Figure 2: Changes in Maximum Years of Welfare Payment Eligibility upon Time Limit Introduction

(A) Youngest Child Born in 1990



(B) Youngest Child Born in 1985



Notes: The two figures above illustrate how adults with a youngest child born in 1990 or 1985 in three different states (Georgia, Massachusetts, and California) were affected by the introduction of time limits in terms of the maximum number of years they can be eligible for welfare payments during their lifetime. The maximum number of years for benefits are calculated based on assuming that the youngest child is their only child.

children present in the household and therefore time limits affected their future eligibility for welfare payments. Interestingly, labor-market outcomes are better for treated males on average, which may be due to the larger responsibility these individuals face by having younger children or possibly due to cohort differences. All these substantial differences between treated and control males will be accounted for in our regression analysis through individual fixed effects. Furthermore, the analysis will verify that these differences are not driving the results by examining parallel trends in the outcomes of interest prior to treatment.[19]

The bottom rows of Table 1 present means of welfare payment receipt. 2.7 percent of observations resided in a household where they or their female partner received welfare payments, with a larger percent among treated individuals. The female partner was more likely to receive welfare payments than the male, but the difference is only by a few tenths of a percentage point. Mean annual income from labor at slightly more than 30,000 2010 USD is typical for this population of disadvantaged males. While this figure is large for individuals receiving welfare payments, it is important to emphasize that only a small fraction are receiving welfare payments. Furthermore, because these characteristics are at the annual level, one could earn labor income during the year while receiving welfare payments during a different part of the year. For those in the sample who report direct receipt of welfare payments (i.e., not through their female partner) during the year, mean annual income is 10,075 2010 USD, which is consistent with annual income levels for households receiving welfare payments. For males residing in households receiving welfare payments either directly or through their partner (or both), mean annual income is 12,933 2010 USD. According to Morris et al. (2007), the average family income for those receiving welfare payments in the early 1990s was $11,854 (nominal). If we convert this amount from 1992 USD, it is equivalent to 18,423 2010 USD, implying that in the PSID sample, male annual labor income covered a

[19]Note that the sample of treated individuals also includes individuals who are only observed prior to the introduction of time limits. They are included in the regression analysis to better estimate fixed effects and trends during the sample period. Nevertheless, the results are not sensitive to the exclusion of these individuals from the regression analysis (not shown). Furthermore, in Section 7.3 in the Robustness Checks attrition from the sample is addressed.

little over two-thirds of the mean family income during years they received welfare payments.

5 Empirical Strategy

5.1 Specification

The analysis evaluates how disadvantaged male employment, hours worked, and income responded to decreased welfare payment eligibility with the introduction of time limits. The indicator variable $Treatment_i$ is defined at the individual level and takes the value one if time-limit introduction changed the amount of years a parent was eligible for welfare payments. It is determined based on the state of residence and age of youngest child upon time-limit introduction. When this variable is interacted with the indicator $After_{st}$ for being observed after the introduction of time limits, its variation relies on three sources. The staggered introduction of time limits creates variation across states and time. Variation is also generated within states based on one's age of youngest child when time limits were introduced in their state of residence.[20]

One of the main challenges when evaluating labor market outcomes among the disadvantaged during the period beginning in the 1990s is that many policies, besides welfare payment time limits, that could have affected labor market outcomes were implemented or expanded during these years. The prime candidates for this are the Earned Income Tax Credit (EITC) and other components of welfare reform. Variation within state-year units based on one's youngest child when time limits were introduced allows me to include in the regression specification state-year fixed effects, implying that all time-varying state-level policies are controlled for in the regression specification. Thus, in order for other state-level policies to bias the results, alternative policy measure introductions or expansions need to be correlated with time-limit introductions and their effect has to depend on the age of the

[20] There is also variation in the threshold for the age of one's youngest child across states due to varying time limit extents that were introduced.

Table 1: Summary Statistics

	All	Treated Individuals	Not Treated Individuals
Number of Observations	46,885	14,409	20,250
Number of Individuals	5,822	1,968	3,340
Treatment Intensity (Yrs of	3.43	8.26	0.00
Welfare Benefits Lost)	(4.56)	(3.96)	-
Age	34.95	34.67	38.08
	(9.62)	(6.79)	(8.89)
Age of Youngest Child Upon	9.72	5.89	18.19
Time Limit Introduction	(6.68)	(3.82)	(4.14)
Age of Youngest Child	8.79	5.85	13.90
(Contemporaneous)	(7.66)	(5.01)	(7.27)
Married / Co-Habiting	0.76	0.84	0.65
	(0.43)	(0.37)	(0.48)
Number of Children under 18	1.23	1.80	0.49
in Household (Contemporaneous)	(1.28)	(1.18)	(0.87)
Years of Schooling	11.82	11.98	11.64
	(1.72)	(1.54)	(1.81)
High School Degree or Less	0.88	0.86	0.90
	(0.33)	(0.35)	(0.30)
White	0.53	0.52	0.57
	(0.50)	(0.50)	(0.50)
Black	0.42	0.45	0.36
	(0.49)	(0.50)	(0.48)
Employed	0.84	0.85	0.77
	(0.36)	(0.35)	(0.42)
Annual Hours Worked	1,911.49	1,962.28	1,785.16
	(891.80)	(877.17)	(966.11)
Income from Labor (2010 $)	32,581.39	32,783.55	30,527.27
	(24,410.56)	(24,307.54)	(24,178.50)
Welfare Receipt - HH Head or	0.027	0.053	0.012
Female Partner	(0.162)	(0.225)	(0.108)
Welfare Receipt - HH Head	0.015	0.027	0.008
	(0.123)	(0.162)	(0.090)
Welfare Receipt - Female Partner	0.020	0.029	0.004
	(0.141)	(0.169)	(0.061)

Notes: The sample is males with a high school education or less as of age 25, or resided in a household receiving welfare payments or government transfers prior to treatment ages 18-60 who were 19-55 when time limits were introduced in their state and are observed up to +/-15 years relative to time-limit introduction during 1980-2016. The sample represents individuals reporting their employment status during the sample period (the largest sample in the regression analysis). Numbers in parenthesis are standard deviations for the means presented. Treated and untreated means in the second and third columns for males are at the individual level, taking the latest non-empty observation for that person up to five years prior to treatment, with the exception of the intensity of treatment variable that is the mean of all treated persons' observations post-reform. Sample sizes are substantially smaller than the reported number of individuals in the first column, as not all individuals were in the sample or reported the variables of interest up to five years prior to treatment. For age of youngest child means, the samples are only for those who have children. Annual hours worked and income from labor means are for a sub-sample as described in the Data section.

youngest child in the household. Moreover, the threshold for treatment status needs to be ages 13-16 (depending on the time-limit extent) for one's youngest child upon time-limit introduction, which is not the threshold for other potential policy measures introduced during this period.[21]

In order to further address concerns that any effects observed are driven by the EITC, the main regression specification is even more conservative and rather than including state-year fixed effects, it includes state-year-number of children fixed effects, as EITC's intensity varies based on the number of children under age 18 present in the household. This alleviates concern that within state-year variation that is based on one's age of youngest child upon time limit introduction is correlated with the number of children under 18 one contemporaneously resides with in the household. Regression results will be presented for specifications with only state-year fixed effects and with state-year-number of children fixed effects (see Table 2).[22]

This results in the following form of a difference-in-differences (DID) specification:

$$
\begin{aligned}
y_{istn} &= \alpha_0 + \alpha_1 Treatment_i * After_{st} + \alpha_2 PostTimeLimit_{it} \\
&\quad + \gamma_i + \theta_{stn} + \epsilon_{ist}
\end{aligned}
\tag{1}
$$

where y_{istn} is the outcome of interest for individual i in state s and year t with n children under 18 in the household. The regression controls for individual and state-year-number of children fixed effects with γ_i and θ_{stn}, respectively. The regression controls for post-treatment trends among individuals that could have been affected by the introduction of time limits in any of the states that introduced time limits (based on the age of their youngest child) regardless of whether that individual resided in the actual state that introduced the time

[21] 21For work requirement expansions, the other major policy component of welfare reform, maternal exemptions were for having a newborn less than 3-12 months old (depending on the state), so these expansions applied nearly to all mothers and did not have a threshold of having a youngest child less than ages 13-16. EITC also applied to parents of children of all ages through age 18 or even under 24 if enrolled as a full-time student.

[22] In the remainder of the paper, whenever stating "number of children fixed effects", this refers to the number of children under age 18, unless specifically indicated otherwise.

limit, and this is through the inclusion of the variable $PostTimeLimit_{it}$.[23] Equation 1 does not include constant individual-level variables (e.g., completed years of schooling, race, etc.) as they are collinear with individual fixed effects. In addition, age is collinear with year and individual fixed effects. Standard errors are clustered at the state level.

The coefficient of interest in equation 1, α_1, measures the mean change in y_{istn} in response to experiencing a time limit that changed an individual's welfare payment eligibility during their parenting years. Because the explanatory variable measures treatment status upon time-limit introduction, the analysis provides the estimated effect of experiencing an ongoing shock to welfare payment eligibility. By defining my treatment status variable of interest based on one's age of youngest child upon time limit introduction, I avoid concerns that can arise from endogenously depleted stocks of welfare eligibility years, as discussed in Chan (2018) and Mazzolari (2007).

With its three sources of variation across states, years, and within states based on age of youngest child upon time-limit introduction, equation 1 resembles a triple differences specification. The inclusion of the variable $PostTimeLimit_{it}$ further ensures that all two-way interaction terms are accounted for. However, equation 1 is not a standard triple differences model, and this is because the third source of variation is dependent on the other two sources. Specifically, within-state assignment of treatment status is based on one's age of youngest child and the extent of time limits upon their introduction, which is state-specific. Thus, in contrast to standard triple differences models, there is an inter-dependence between the within-state variation and the across-state variation.[24]

[23] For example, in 1998 a 24-month time limit was introduced in Idaho and Nebraska. Due to this, the variable $PostTimeLimit_{it}$ receives a value of 1 for all individuals who have a child 16 years old or less in 1998 for all years in the sample from 1998 and onward, regardless of their state of residence in 1998. This controls for post-time-limit-introduction trends among those who could have been affected by this time limit, regardless whether their state of residence in 1998 determined that they were actually affected by it. This indicator is also equal to 1 for all individuals who could have been affected by a 60-month time limit that was introduced in 1998 (seven states introduced such a time limit) as those affected by this time limit with a youngest child younger than 13 are a subset of those effected by the 24-month time limit with a youngest child younger than 16. Similarly, $PostTimeLimit_{it}$ receives a value of 1 for observations of individuals who could have been affected by any time limit introduction in the sample after its introduction, regardless of their state of residence.

[24] As an example, an individual who is treated in one state due to having a youngest child aged 14 when a

Many of the results presented are from an event-study analysis using the following variation of equation 1:

$$\begin{aligned} y_{istn} &= \beta_0 + \sum_{j=-4, j\neq 0}^{4} \beta_1^j Treatment * After_{ist}^j + \beta_2 PostTimeLimit_{it} \\ &+\gamma_i + \theta_{stn} + \epsilon_{ist} \end{aligned} \tag{2}$$

Equation 2 includes pre- and post-treatment indicator variables for three-year periods. This produces eight coefficients of interest - β_1^j: 4 for prior to time-limit introduction - 12+ years, 9-11 years, 6-8 years, and 3-5 years pre-treatment; and 4 for after time-limit introduction - 1-3 years, 4-6 years, 7-9 years, and 10+ years post-treatment. The excluded period is 0-2 years pre-treatment. Each β_1^j measures the mean change in y_{istn} in response to a shortening in the number of years of welfare eligibility in the relevant period relative to the excluded period. The pre-treatment coefficient estimates in the event-study analysis can confirm the identification strategy's validity through establishing a lack of pre-trends in the outcomes of interest among treated individuals.

5.2 Identification

Although the state-year fixed effects in the regression specification should alleviate concern that results are being driven by other potential policy measures, I also test for a correlation between the timing of time-limit introductions and the timing of two additional large policies that may have affected disadvantaged households' labor market outcomes beginning in the 1990s: state-level EITC supplements that were added to the federal EITC, and work requirements, the other major policy component of welfare reform. Figure 3A demonstrates very weak correlations (-0.21 to -0.11 correlation coefficients; p-values are 0.30-0.45) between time-limit introductions and EITC supplements or work requirement expansions at the state

60-month time limit is introduced may not be treated in another that introduced a 24-month time limit in the same year. This is not custom of standard triple differences models.

level.[25] The correlations are in fact negative, indicating that if there was any bias, it would actually be downwards, as state-level EITC supplements/work requirements were introduced earlier (later) in states that introduced time limits later (earlier). For EITC, Figure 3A includes a subset of states as only roughly half of states introduced EITC by 2016. Appendix Figure A1 demonstrates that there is large variation in the years time limits were introduced for the remaining states. In addition, Section 7.2 presents results of regressions with separate state and year fixed effects that allow for controlling for the state-level EITC. The results are consistent with the main findings and do not vary between specifications with or without the EITC rate. Lastly, Appendix Section .5 presents regression results from a robustness check with the sample limitation that observations are from states while there was no state-level EITC in place. Here too results are consistent with the main findings.

The identifying assumption is that an individual's determinants of whether time-limit introduction affected welfare payment eligibility (state of residence and age of youngest child at the time of introduction) are predetermined prior to the reform. Therefore, the main variable of interest concerning an individual's treatment would be exogenous. At the individual level there are inherent differences between treated and untreated individuals that are mechanical due to treatment status relying greatly on the age of the youngest child at the time of time-limit introductions (see Table 1). However, treatment status also relies heavily on the exact timing of time-limit introductions across states. I test for the exogeneity of this timing by examining the correlation between time-limit introduction timings and state-level characteristics as of 1990-1992, before the first introduction. Figure 3B graphs such correlations for nine state characteristics: population, unemployment rates, minimum wage, poverty rate, median household income, percent democratic vote in the national elections, percent black, percent Hispanic, and percent with college completion.[26] The results exhibit

[25]State-level EITC introductions are from Leigh (2010) and supplemented for more recent years with the data used in Kleven (2019). JOBS expansions and waivers timings are from the DHHS - https://web.archive.org/web/20170201085413/https://aspe.hhs.gov/pdf-report/state-implementation-major-changes-welfare-policies-1992-1998.

[26]Unemployment figures (1992) are obtained from the Bureau of Labor Statistics. Population, poverty, and median household income estimates (1992) are obtained from the US Census Bureau State and

no such correlation.

Appendix Figure A2 presents the same analysis as Figure 3 for time-limit lengths upon introduction, rather than time-limit introduction years. The results exhibiting no correlation provide further validity to the exogeneity of the within-state variation in the empirical strategy.

6 Results

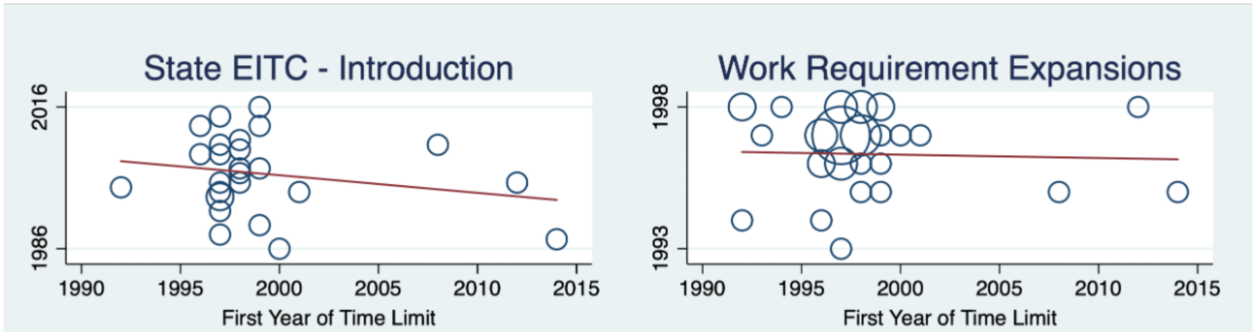
6.1 Event Study Results for Male Labor Market Outcomes

I first present event-study results from equation 2 for male labor-market outcomes - employment, annual hours worked, and annual labor income. Figure 4 plots the eight coefficient estimates of interest for each dependent variable, along with their 95 percent confidence intervals. The findings clearly demonstrate increases in all three measures evaluated in response to time limits. All the effects are persistent in the long-term and the effect on employment even slightly increases (although the estimates are indistinguishable statistically). The magnitudes of the effects are 2.5-5.5 percentage points for employment, 149-191 annual hours, and 2,600-4,100 2010 USD for annual labor income. These represent increases that are 2.9-6.5, 7.7-9.9, and 10.7-12.9 percent of pre-treatment means of employment, hours worked, and labor income, respectively. While the effects observed in Figure 4 are sizable, I note that they are smaller as a percent of pre-treatment means than the female effects presented in Section 6.4, which align with the magnitude of time-limit effects on female labor market outcomes in past literature.

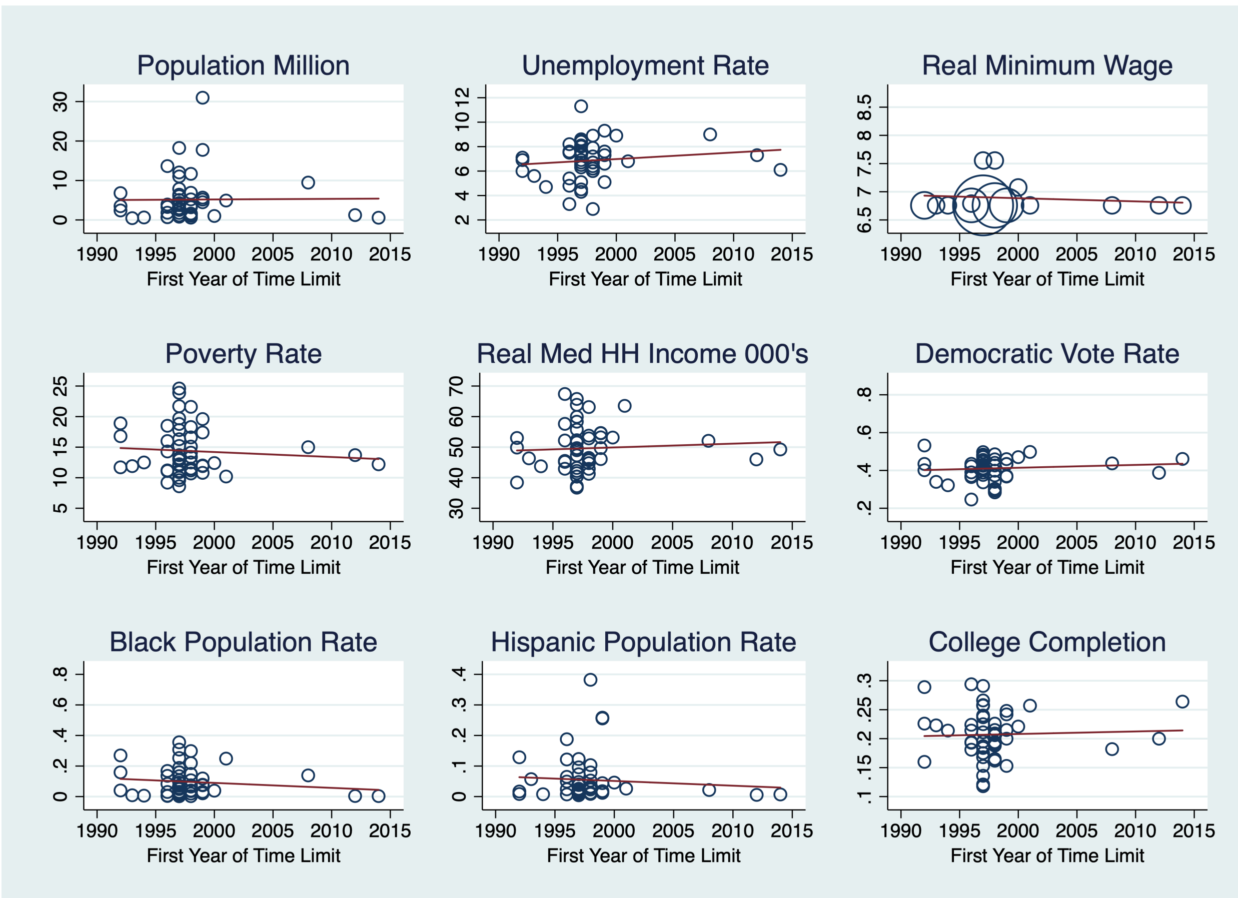
County Intercensal Tables and SAIPE Datasets. Minimum wages as of 1992 at 2011 US dollars are obtained from data from Meer and West (2016). The share voting for the democratic party in the 1992 national elections is from the Federal Election Commission (https://www.fec.gov/resources/cms-content/documents/federalelections92.pdf). Percent Black or Hispanic as of 1990 is from The U.S. Census Bureau - Statistical Abstract of the United States: 1992, section 1 - population, Table 26 (https://www2.census.gov/library/publications/1992/compendia/statab/112ed/1992-02.pdf). Percent with four or more years of college is from the National Center for Education Statistics Digest of Education Statistics for 1992, Table 12 (https://nces.ed.gov/pubs92/92097.pdf).

Figure 3: Year of Time-Limit Introduction and State-Level Policies/Characteristics

(A) State-Level Policies and Year of Time-Limit Introduction



(B) State-Level Characteristics (1990-1992) and the Year of Time-Limit Introduction



Notes: Larger circles indicate more states.

The top panel plots for each state the year the stated policy was introduced (state-level EITC) or substantially expanded (work requirements) and the years time limits were introduced. The fitted linear regression line is indicated in red. Work requirements are based on JOBS (Jobs Opportunities and Basic Skills Training Program) waivers or expansions. For work requirements, all states are covered. Only 26 states (excluding Iowa and Washington, D.C. that are not in the sample) adopted EITC supplements. States that did not introduce EITC are not plotted.

The bottom panel plots for each state the value of the stated state-level characteristics as of 1990-1992 and the years time limits were introduced. The fitted linear regression line is indicated in red.

Figure 4 presents no evidence of pre-trends in the dependent variables prior to time-limit introduction. This strengthens the interpretation of the estimates as causal and refutes concerns for differential trends between the treated and control groups prior to the reform.

6.2 Male Labor-Market Outcomes vs. Welfare-Payment Receipt

Table 2 presents results for welfare-payment receipt and labor-market outcomes in the top and bottom panels, respectively. The presentation of the single coefficient estimate from equation 1 facilitates a comparison of the magnitude of the effects. Welfare payment during the year is evaluated using three measures: whether the male or his female partner received welfare payments; whether the male himself received them; and whether the male's female partner received them.[27] Labor-market outcomes are identical to those already analyzed in Figure 4 (employment, hours worked and labor income).

The first column of each dependent variable presents results with only state-year fixed effects, whereas the second column converts these to state-year-number of children fixed effects. As can be seen, the coefficient estimates do not vary dramatically with the change in fixed effects, with the exception of the effect on male welfare payment receipt, which increases by roughly 60 percent.

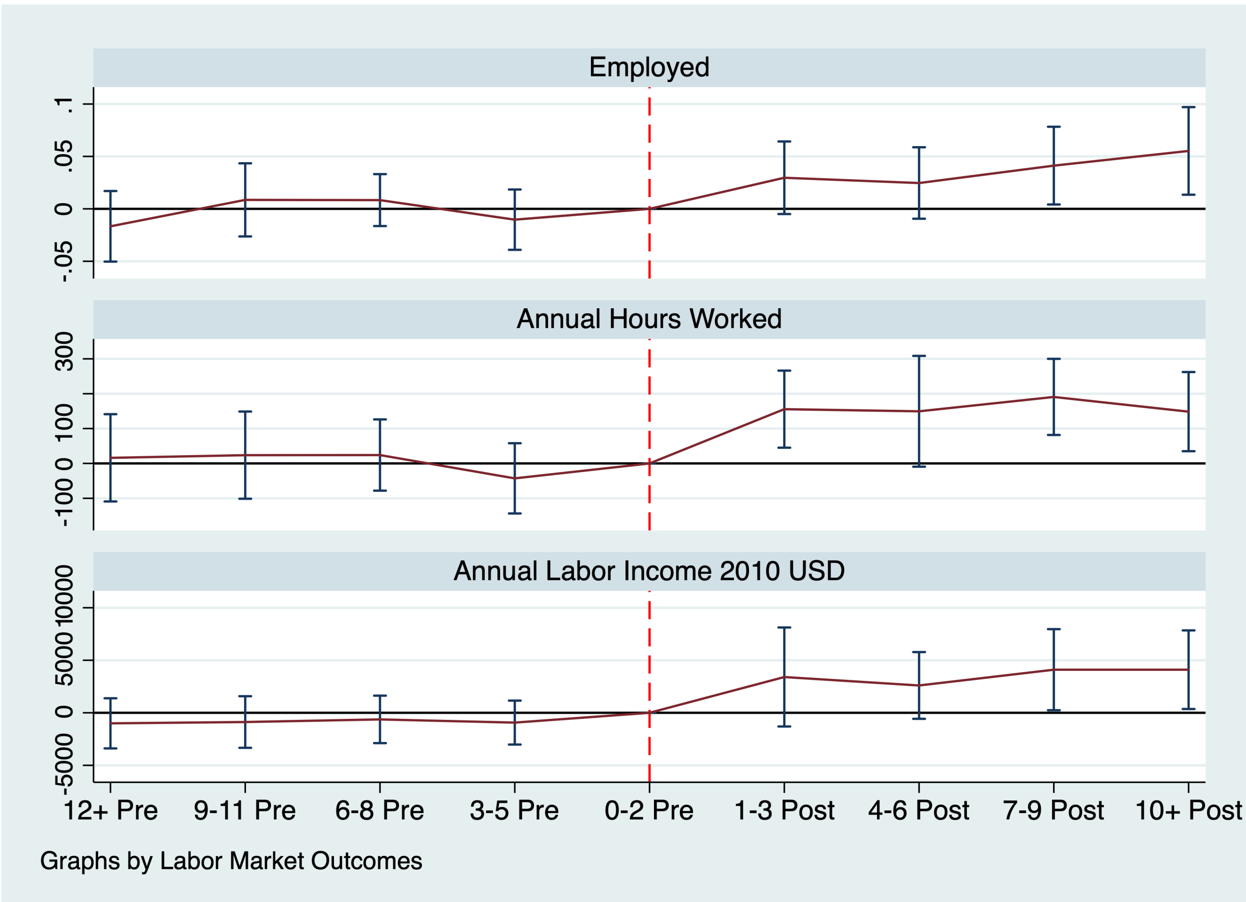
The first two columns in the top panel of Table 2 show that welfare-payment receipt by either the male or their female partner declined by 2.6 percentage points in response to the implementation of time limits. The next four columns reveal that a larger share of this decline is attributed to the female partner's loss of welfare benefits rather than to his own losses.[28] An event-study analysis in Appendix Figure A3 confirms the declines in welfare payment receipt observed in Table 2, although for males the results are slightly noisier.

The magnitude of the increase in employment is much larger than the reduction in men's welfare payments receipt. This implies that men could also be increasing their employment

[27]The PSID documents specifically which household member receives welfare payments.

[28]It may be that more of the males receiving welfare payment are extreme cases who are eligible for time-limit exemptions. According to Ziliak (2015), in 2000, 20 percent of TANF caseloads were exempt from time limits.

Figure 4: Event-Study Analysis - Male Labor-Market Outcomes in Response to Time-Limit Introductions



Notes: The figure presents event study coefficient estimates from equation 2. The vertical lines represent 95 percent confidence intervals. Standard errors are clustered at the state level. Sample sizes range from 33,341 to 46,885 observations covering 4,278 to 5,822 males. Pre-treatment means are 0.845, 1,931, and 31,754 for employed, annual hours worked, and annual labor income, respectively.

in response to their partner's loss of welfare payments. Still, the increase in employment is somewhat larger that the decrease in welfare payments. This further suggests that men may be responding to something beyond contemporaneously lost income. It is possible that some of the employment response is related to increased uncertainty of future income, or that time limits also induced changes in marriage/cohabitation patterns that changed men's labor market incentives. The scope of this paper cannot distinguish which of these mechanisms may be at work here. Nevertheless, these possibilities demonstrate the role of forward-looking behavior in the male response to time limits.

The point estimates for labor income and hours worked in the second and third columns of the bottom panel of Table 2 can at first glance suggest that the hourly wage rate for the increased hours worked is set at over 27 2010 USD.[29] This is very large for this disadvantaged population. However, an increase in annual labor income can be attributed to an increase in the hourly wage rate as well, and not just an increase in hours worked. Accordingly, column (1) in Table 3 presents results for the change in hourly wages among treated males following the introduction of time limits, and the results suggest an increase of 1.03 2010 USD with a p-value of 0.125. It turns out that a mean increase of 1.58 2010 USD in the hourly wage rate is sufficient to fully cover the annual labor income increase of 4,133 2010 USD presented in Table 2.[30] Although 1.58 exceeds the estimated hourly wage increase in Table 3, it is within one standard deviation from the estimated effect. Thus, the relatively large increase in annual labor income in Table 2 is reasonable when considering increases in the hourly wage rates.

Table 3 more broadly assesses how much of the increased hours worked and labor income can be attributed to the intensive margin, rather than just the extensive margin, which is well-documented in Figure 4 and Table 2 for the employment outcome. Thus, in addition

[29] This is 4,133 2010 USD divided by 150 hours.

[30] If we multiply the change in annual hours from Table 2 by 7.25 2010 USD, which was the federal minimum wage in 2010, we get an increase in annual income of 1090 2010 USD. This leaves 3,043 2010 USD that are not accounted for from the 4,133 2010 USD increase observed in Table 2. Dividing this by 1,931, the pre-treatment mean number of annual hours worked in the sample, results in an hourly wage increase of 1.58 2010 USD that is able to cover the remaining increase in annual labor income.

Table 2: Male Welfare Receipt and Labor-Market Outcomes following Time-Limit Introduction

	Welfare Payment Receipt					
	Male or Female Partner		Male		Female Partner	
Post*Treatment (with child affected by reform)	-0.0231*** (0.00505)	-0.0261*** (0.00589)	-0.00665* (0.00332)	-0.0108** (0.00406)	-0.0242*** (0.00452)	-0.0256*** (0.00569)
Observations	39,877	38,926	39,874	38,924	38,885	37,992
Number of Individuals	4,743	4,693	4,743	4,693	4,743	4,690
R-squared	0.412	0.495	0.341	0.444	0.383	0.470
Pre-Treat Mean Dep. Var.	0.0338	0.0332	0.0197	0.0192	0.0265	0.0260
	Labor Market Outcomes					
	Employed		Annual Hours Worked		Ann. Labor Inc. (2010 USD)	
Post*Treatment (with child affected by reform)	0.0331** (0.0136)	0.0373*** (0.0137)	122.9*** (34.95)	150.3*** (37.37)	4,867*** (1,352)	4,133*** (1,451)
Observations	47,747	46,885	34,346	33,341	34,348	33,347
Number of Individuals	5,859	5,822	4,325	4,278	4,326	4,280
R-squared	0.512	0.583	0.587	0.621	0.693	0.728
Pre-Treat Mean Dep. Var.	0.847	0.845	1,930	1,931	31,736	31,755
Individual Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
State-Year Fixed Effects	Yes	X	Yes	X	Yes	X
State-Year-Num Child FE	X	Yes	X	Yes	X	Yes

Notes: The table presents the coefficient estimate α_1 from equation 1, with either state-year or state-year-number of children in the first and second column of each dependent variable, respectively. The sample is males as specified in the Data section. The number of observations varies within dependent variable columns due to the exclusion of singletons. The number of variables is smaller for hours worked and labor income, as opposed to employment, due to excluding observations with extreme values of a calculated hourly wage rate for the hours worked and labor income sample (see Section 4). Numbers in parenthesis are standard errors clustered at the state level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

to estimating the change in hourly wages following time limits in column (1), columns (2) and (3) examine changes in annual hours and labor income conditional on being employed. These regressions should be interpreted with caution, as conditioning the sample on those employed should lead to a downward bias of the estimates. One would expect lower-earning individuals who may also work less hours on average to join the labor market after time-limit implementation. Despite this downward bias, column (3) of Table 3 suggests an increase in annual labor income, which supports the notion that increases in annual labor income are not just driven by individuals joining the labor market at the extensive margin but also experiencing an increase in their hourly wage rate, as demonstrated by column (1) of Table 3.

Table 3: Changes in Labor Market Outcomes at the Intensive Margin following Time-Limit Introduction

	Hourly Wage	Annual Hours Conditional on Employment	Annual Labor Income Conditional on Employment
Post*Treatment (with children affected by reform)	1.030	90.02***	3,225*
	(0.657)	(27.76)	(1,660)
Observations	33,296	29,936	29,936
Number of Individuals	4,275	3,994	3,994
R-squared	0.599	0.527	0.732
Pre-Treatment Mean Dependent Variable	15.45	2,099	34,518
Individual Fixed Effects	Yes	Yes	Yes
State-Year-Num Children Fixed Effects	Yes	Yes	Yes

Notes: The table presents the coefficient estimate α_1 from equation 1. The sample is males as specified in the Data section in column (1) and in columns (2) and (3) with the added condition that employment in non-zero. Hourly wage is calculated as the reported annual labor income (in 2010 USD) divided by the reported number of annual hours. Numbers in parenthesis are standard errors clustered at the state level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

6.3 Intensity-of-Treatment Analysis

It is possible to replace the $Treatment$ indicator variable in equation 1 with an intensity-of-treatment variable measuring the number of years of welfare payment eligibility lost upon time-limit introduction (similar to the analysis in Grogger (2002, 2003, 2004)).[31] This analysis further exploits the variation generated within state-year units and across treated individuals based on the age of their youngest child when time limits were introduced, thereby strengthening to an even greater extent the identification assumption that results are indeed driven by variation that is unique to the introduction of time limits rather than other policy measures or confounding events taking place during this period.

Results from this variation of equation 1 are presented in Table 4. All point estimates are qualitatively consistent with those presented in Table 2 and statistically significant, with the exception of the estimate for the effect on male welfare payment receipt, which is not statistically significant. The overall mean effects are obtained by multiplying the point estimates by 8.3, which is the mean number of years of welfare-payment receipt lost among the treated (see Table 1). While for female welfare payment receipt and employment, the calculated effects are 81-86 percent of those calculated for the same dependent variables in Table 2, for male or female welfare payment receipt, annual hours worked, and annual labor income, the estimates are 46-61 percent of the corresponding estimates in Table 2. One possible explanation for these gaps is that the distribution of the treatment intensity measure is skewed to the right - while the mean is 8.3 years of potential benefits lost, the median is 9. In addition, some of the effects may be nonlinear, which could also generate gaps between the two estimations. Overall, despite the gaps in the magnitude of the effect between Table 2 and Table 3, the results in Table 3 further validate the effects that time limits had on male labor market outcomes and their female partner's welfare payment receipt.

[31] In contrast to my analysis, Grogger (2002, 2003, 2004) use the contemporaneous age of youngest child rather than the age of youngest child upon time-limit introduction. Note that the data in these studies is from the March CPS or SIPP and neither of these datasets follows individuals over such long time-spans as in the PSID.

Table 4: Intensity of Treatment Results - Welfare Receipt and Male Labor Market Outcomes

	Welfare Payment Receipt		
	Male or Female Partner	Male	Female Partner
Post*Treatment Intensity (number of potential benefit years lost)	-0.00191**	-0.000424	-0.00253***
	(0.000928)	(0.000715)	(0.000770)
Observations	38,926	38,924	37,992
Number of Individuals	4,693	4,693	4,690
R-squared	0.494	0.444	0.470
Pre-Treatment Mean Dependent Variable	0.0332	0.0192	0.0260
	Labor Market Outcomes		
	Employed	Annual Hours Worked	Annual Labor Income (2010 USD)
Post*Treatment Intensity (number of potential benefit years lost)	0.00387***	8.307**	255.9*
	(0.00129)	(3.218)	(130.8)
Observations	46,885	33,341	33,347
Number of Individuals	5,822	4,278	4,280
R-squared	0.583	0.621	0.728
Pre-Treatment Mean Dependent Variable	0.845	1931	31,755
Individual Fixed Effects	Yes	Yes	Yes
State-Year-Num Children Fixed Effects	Yes	Yes	Yes

Notes: The table presents the coefficient estimate α_1 from equation 1, only the coefficient estimate α_1 is for the intensity of treatment (number of years of potential benefit years lost) rather than an indicator for being treated. The sample is males as specified in the Data section. Numbers in parenthesis are standard errors clustered at the state level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

6.4 The Effects of Welfare Payment Time Limits on Females

I compare the male results to those of females using the same regression specification and sample criteria in the PSID data. This serves as a check that my sample criteria and regression specification are aligned with past studies on the effects of time limits and also in order to compare female welfare take-up effects to their employment effects, as I do for males in Table 2.[32] I run the regression specified in equation 1 on the sample of females, as specified in Section 4 for males, in addition to a variation of equation 1 that converts the indicator variable of interest to an intensity of treatment variable of interest, as in Table 3.[33] Table 5 presents results for female welfare receipt and labor market outcomes in the first and second panel, respectively. For each dependent variable, the first column presents the coefficient estimate on the indicator $Treatment*After$, while the second column presents the coefficient estimate from the intensity-of-treatment variation of this variable. Event study results for labor market outcomes that support the findings in Table 5 and, in addition, rule out pre-treatment trends among treated females are presented in Figure 5.

The indicator variable results for female welfare payment in Table 5 show decreases that are 36-47 percent of pre-treatment means. To calculate the overall mean effect based on the intensity of treatment coefficient estimates, I multiply them by 8.15, which is the mean number of years of potential benefits lost upon time-limit introduction among treated females (see Appendix Table A1). These effects are smaller in magnitude and represent roughly 30 percent of pre-treatment means.[34]

[32]The effects of time limits on females have been explored in numerous studies that have primarily shown negative effects on welfare take-up (Chan (2013, 2017, 2018); Fang and Keane (2004); Fang and Silverman (2009); Farrell et al. (2008); Fitzgerald and Ribar (2004); Grogger (2003, 2004); Keane and Wolpin (2010); Low et al. (2020); Mazzolari (2007); Mazzolari and Ragusa (2012); Pepin (2022); Swann (2005)), and positive effects on employment (Chan (2013, 2017); Fang and Keane (2004); Fang and Silverman (2009); Farrell et al. (2008); Grogger (2003); Keane and Wolpin (2010); Low et al. (2020); Mazzolari and Ragusa (2012); Pepin (2022); Swann (2005)).

[33]Appendix Table A1 provides Summary Statistics for the female sample.

[34]The difference between the estimated effects in the indicator variable versus intensity of treatment specification are again worth noting, as is already discussed in Section 6.3. As in the male sample, the female sample intensity of treatment measures are also skewed to the right with a median of 8.75, as opposed to a mean of 8.15. In addition, the smaller effects may again be due to more of an effect at the extensive margin of time limits, rather than the intensive margin.

The estimated effects on female welfare payment receipt in Table 5 are slightly larger, although not dramatically different, from past estimated effects of time limits. Low et al. (2020) find that welfare receipt decreased by 3.8 or 2.2 percentage points from pre-treatment means of 9.8 or 7.7 percent using SIPP or CPS data, respectively. These reductions represent 29-39 percent of pre-treatment means. Pepin (2022) finds that time limits reduced TANF participation by 26 percent. However, Pepin (2022) mainly evaluates decreases in time-limit extent rather than the introduction of time limits, which could result in smaller magnitudes of the effect, and indeed when she evaluates time-limit introductions separately in two states, the effects are larger. Structural models, as in Chan (2013) or Swann (2005) predict larger orders of magnitudes for declines in female welfare payment receipt following time limits that can be as high as 60 percent of pre-treatment means. Finally, earlier studies find smaller effects (Grogger (2002, 2003, 2004); Grogger and Michalopoulos (2003); Mazzolari (2007)) that are mostly under 30 percent of pre-treatment means. However, many of these studies evaluate the effect of time limits only several years after their introduction, such that the decline found in welfare take-up is capturing for the most part the behavioral effect of banking time limit benefits, rather than the actual effect of hitting the limit. Thus, many of these studies expect the actual effect of time limits over longer time horizons, as in this study, to be greater.

Employment increases in Table 5 are 4.3 and 2.7 percentage point for the indicator variable and intensity-of-treatment specifications, respectively. These represent 6.3 and 4 percent of the corresponding pre-treatment means. Past studies evaluating female employment responses to time limits have shown some variation in the magnitude of the effects with increases ranging from 1.1 to 5 percentage points. Grogger (2003)'s estimated effects of time limits on employment depend on the mother's age of youngest child and range from 1 to 3.4 percentage points, but as already mentioned with respect to welfare take-up effects, the period is only several years after the introduction of time limits, thereby potentially not capturing the effect of reaching the time limit. In fact, Mazzolari and Ragusa (2012) estimate that women who hit their time limits increased employment by as much as 21 percentage points. Low et al. (2020) find an employment effect of time limits that is 5 percentage points from a pre-treatment mean of 63 percent, but this is only for unmarried women, whereas the sample in this study includes both married, single and cohabiting women. In Pepin (2022)'s more recent findings from 2008 onward, time limit effects are actually heterogeneous, even qualitatively, depending on the state evaluated.

To the best of my knowledge, past studies have not evaluated the effects of time limits on female hours worked. Effects on earnings have been sparse with Grogger (2003) finding no effect but Mazzolari and Ragusa (2012) arguing that while women in families that reach the time limit do not increase their earnings, women in families that bank their benefits do increase earnings. The results in Table 5 are not conclusive. While the indicator variable specification suggests a large increase in earnings that is over 10 percent of pre-treatment means, the intensity of treatment estimate is very small and noisy. In addition, the event study analysis in Figure 5 shows that increases in female labor income only occur more than 7 years after the introduction of time limits. This suggests that the positive effect on labor income in Table 5 can still be consistent with Grogger (2003)'s null effects that were only estimated using data through 1999.

Overall, the results in Table 5 do not diverge from past studies that evaluated the effect of time limits on female welfare take-up and labor market outcomes. In addition, for labor market outcomes, the percent increases from the pre-treatment means are slightly larger than the corresponding percent increases for males presented in Table 2. This further strengthens the male results and demonstrates that their magnitudes are plausible in the context of past literature.

What is additionally worth noting in the context of this study is that in contrast to the male results in Table 2 that show increased employment that cannot be driven solely by a decline in males' own welfare payment receipt, this is not the case for females. In fact, the female increase in employment in Table 5 is less than the decline in welfare payment receipt. Grogger (2003) and Mazzolari and Ragusa (2012) also observe these gaps, while finding that household income did not decline in response to time limits, thereby raising the possibility of a partner labor income increase in response to time limits. This paper takes these speculations one step further by directly evaluating male labor market responses to time limits.

6.5 Event-Study Analysis using a Two-Step Estimation Procedure

Recent econometric literature has raised concerns regarding staggered adoption in difference-in-differences designs with two-way fixed effects (as in equations 1 and 2). Borusyak et al. (2021), De Chaisemartin and d'Haultfoeuille (2020), and Goodman-Bacon (2021) have established that in the presence of heterogeneous effects, a conventional event-study design, namely ordinary least squares (OLS) with two-way fixed effects and some lags and leads of treatment, can produce unreliable estimates that may even have the wrong sign. This problem arises due to assuming that within the conventional event-study design the treatment effect depends only on the time elapsed since treatment initialization. However, if treatment effects vary across units or periods as well, then the estimates from this model may be biased due to biased unit effects and time trends.

In response to these concerns, I provide the results of my event-study analysis using the two-step estimator proposed in Borusyak et al. (2021) and Gardner (2021) and implemented in Thakral and Tô (2020). The first stage of this approach estimates all coefficients, except for the event-study coefficients, on all non-treated data. Specifically, the first stage estimates regressions of the following form:

$$y_{istn} = \beta_0 + \gamma_i + \theta_{stn} + \epsilon_{ist} \tag{3}$$

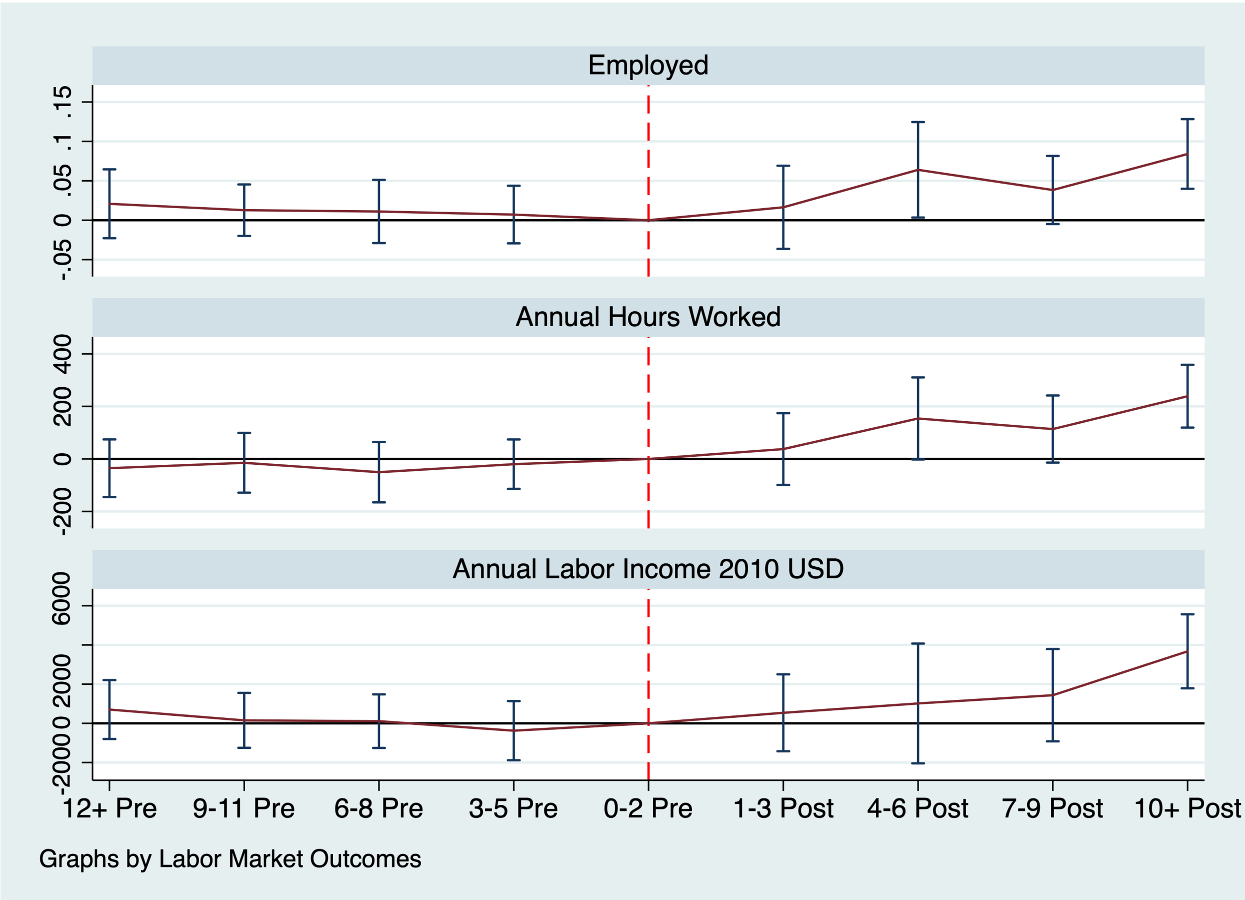
where all the variables are as in equation 2 but the sample is restricted to include only observations with a value of zero for the interaction $Treatment * After$ specified in equation

Table 5: Female Welfare Receipt and Labor-Market Outcomes following Time-Limit Introduction

	Welfare Payment Receipt					
	Male or Female		Female		Male Partner	
Post*Treat (w/ child affected by reform)	-0.0528***		-0.0535***		-0.0198*	
	(0.0128)		(0.0128)		(0.0100)	
Post*Treat Intense (# of benefit yrs lost)		-0.00407***		-0.00408***		-0.00195*
		(0.00149)		(0.00146)		(0.00104)
Observations	45,963	45,963	45,963	45,963	45,963	45,963
Number of Individuals	4,773	4,773	4,773	4,773	4,773	4,773
Pre-Trt Mean Dep. Var.	0.116	0.116	0.112	0.112	0.0544	0.0544
	Labor Market Outcomes					
	Employed		Annual Hours Worked		Ann. Labor Inc. (2010 USD)	
Post*Treat (w/ child affected by reform)	0.0431**		180.2***		1,876**	
	(0.0202)		(38.17)		(803.9)	
Post*Treat Intense (# of benefit yrs lost)		0.00338*		10.32***		42.56
		(0.00180)		(3.704)		(70.52)
Observations	54,755	54,755	39,602	39,602	35,764	35,764
Number of Individuals	5,894	5,894	4,519	4,519	4,400	4,400
Pre-Trt Mean Dep. Var.	0.686	0.686	1,151	1,151	14,268	14,268
Individual Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
State-Year-Num Child FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The table presents the coefficient estimate α_1 from equation 1. In the second column of each dependent variable, the coefficient estimate α_1 is for the intensity of treatment (number of years of potential benefit years lost) rather than an indicator for being treated. The sample is females as specified for the male sample in the Data section. Numbers in parenthesis are standard errors clustered at the state level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Figure 5: Event-Study Analysis - Female Labor-Market Outcomes in Response to Time-Limit Introductions



Notes: The figure presents event study coefficient estimates from equation 2. The vertical lines represent 95 percent confidence intervals. Standard errors are clustered at the state level. Sample sizes range from 35,764 to 54,755 observations covering 4,400 to 5,894 females. Pre-treatment means are 0.69, 1,151, and 14,268 for employed, annual hours worked, and annual labor income, respectively.

1. In the second step, I run regressions of the following form:

$$
\begin{aligned}
y_{istn} \quad = \quad & \beta_0 + \sum_{j=-4, j\neq 0}^{4} \beta_1^j Treatment * After_{ist}^j + \beta_2 PostTimeLimit_{it} \\
& + \hat{\gamma}_i + \hat{\theta_{stn}} \epsilon_{ist}
\end{aligned} \tag{4}
$$

where all the variables are as in equation 2, and parameters $\hat{\gamma}_i$, $\hat{\theta_{stn}}$ are as estimated in equation 3.[35] Equation 4 will yield unbiased β_1^j estimates without concern that unit and time fixed effects include the dynamic causal effects that equation 2 attempts to capture.

I use a block-bootstrap procedure to compute the standard errors adjusted for clustering at the state level, as described by the following procedure:[36] First, I randomly draw with replacement a set of states equal to the number of states in the original regression. Next, I rerun the two-step estimator. If a state is drawn more than once, its state-year combinations receive new fixed effects each time it is drawn. Individuals in states that are drawn more than once also receive new individual fixed effects for each time their state is drawn. I repeat the random draw and two-step estimation procedure two hundred times. The standard errors reported for all β_1^j estimates from equation 4 are the standard deviations across these 200 estimates. The results presented in Figure 6 clearly demonstrate an increase in all male labor-market outcomes in response to time limits, in addition to a lack of pre-trends prior to treatment.[37] The results are remarkably consistent with the main results presented in Figure 4, thereby supporting the idea that the estimates in equation 2 do not suffer from bias due to dynamic effects that are heterogeneous as a function of time or unit of observation.

[35] Note that all state-year combinations include untreated individuals even if they are observed after the introduction of time limits. This is due to the fact that some individuals within these states were not affected by time-limit introduction in terms of welfare payment eligibility given the age of their youngest child.

[36] Gardner (2021) suggests using the Stata gmm command, while Borusyak et al. (2021) have created the did_imputation Stata command for running this two-step approach and to obtain event study estimates (and standard errors). However, due to the large number of fixed effects in my regression specifications, these commands could not be implemented to obtain my estimates.

[37] Standard errors for pre-treatment estimates decrease substantially in the two-step procedure due to the exclusion of the fixed effects from the dependent variables for these observations.

Figure 6: The Effect of Time Limits - Two-Step Estimation Procedure

Notes: The figure presents event study coefficient estimates from equation 4. The vertical lines represent 95 percent confidence intervals. Sample sizes range from 29,502 to 42,633 observations covering 3,852 to 5,400 males.

7 Robustness Checks

7.1 Different Samples: High-Education Males, Fathers to Teenage Children, and Time-Limit Effects Through 2006

Table 6 presents results for different samples of the data to address potential concerns regarding the validity of the results. The top panel is a placebo analysis that limits the sample to males with at least 15 years of schooling. For these high-income males, we do not expect an effect of time limits. The middle panel limits the baseline sample of disadvantaged males to those who resided with a teenage child when time limits were introduced. In this sample, the treatment and control groups are more comparable as opposed to the baseline sample, given that they all have children within the same age range. The bottom panel restricts the baseline sample years until 2006, rather than 2016. This addresses concerns that the economy changed substantially following the great recession.

In the top panel of Table 6, I limit the sample to high-education males who were not observed residing in a household receiving government transfers and were not on Medicaid prior to time-limit introduction in their state, and with a mean annual income during the years they are observed exceeding 30,000 2010 USD. One would not expect these males to be affected by time limits. Indeed the results confirm this For annual labor income, the coefficient estimate is large but also very noisy.

In the middle panel of Table 6, I examine a sample of males who are much more comparable to each other across the treatment and control groups: those whose youngest child in the household at the time of time-limit introduction was 11-18 years old. The baseline sample includes males with much younger children and even without a child present in the household. Given that the threshold for being affected by time limits is based on having a child ages 13-16 in the household upon time limit introduction, a sample of males residing with teenage children upon time-limit introduction includes both treated and control males with greater comparability between them in contrast to the baseline sample, although at the cost of reducing substantially the sample size. The results confirm an increase in labor-market outcomes. The employment estimate is noisier with a much larger standard error, but I note that the pre-treatment mean in employment rate for this sample is larger than the mean in the baseline sample, so it is more difficult to increase employment rates from this starting point. The magnitude of the annual hours effect is larger than the effect in Table 2 and the labor income effect is of similar magnitude but less precise with a p-value of 0.169.

The economy and labor market have changed substantially in the past two decades in terms of opportunities for disadvantaged and low-educated males, particularly after the great recession (Hershbein and Kahn (2018); Blair and Deming (2020)). Thus, I perform the same analysis as in Table 2, only the sample is limited to states that introduced time limits through 2002 (three states are excluded) and the data is through 2006. The results are presented in the bottom panel of Table 6 and remain similar to those presented in Table 2.

7.2 Controlling for State EITC Rate in the Regression Analysis

The EITC was a major policy that affected disadvantaged households' labor supply decisions during the same period that time limits were introduced. As changes in the EITC were either at the federal or state level, my regression specification addresses concerns that the EITC may be driving the results through the inclusion of state-year fixed effects in the regression analysis. Furthermore, as discussed in Section 5.2, I address the fact that EITC intensity could have varied within state-year units based on the number of children that are under 18 in the household through the inclusion of state-year-number of children under 18 fixed effects in the regression specification.

Despite these various measures, in Table 7, I show that the results in Table 2 are not sensitive to controlling for the state-level EITC rate. The regression specification is the same as what is presented in equation 1, with the important exception that the state-year-number of children under 18 fixed effects are replaced with separate state and year fixed effects, as well as separate fixed effects for the number of children under 18 the individual has. With

Table 6: High Education Males, Fathers to Teenage Children, and Time-Limit Effects Through 2006

	Employed	Annual Hours Worked	Annual Labor Income (2010 USD)
	High Education - ≥15 Years of Schooling		
Post*Treatment (with children affected by reform)	-0.00465	45.93	13,542
	(0.0128)	(39.34)	(11,922)
Observations	16,598	14,513	14,513
Number of Individuals	1,392	1,336	1,336
R-squared	0.553	0.514	0.575
Pre-Treatment Mean Dependent Variable	0.958	2,292	68,828
	Fathers to Teenagers upon Time-Limit Introduction		
Post*Treatment (with children affected by reform)	0.0441*	223.7***	4,540
	(0.0233)	(71.53)	(3,232)
Observations	8,310	6,853	6,853
Number of Individuals	862	734	734
R-squared	0.653	0.642	0.775
Pre-Treatment Mean Dependent Variable	0.902	2,014	35,872
	Years Through 2006		
Post*Treatment (with children affected by reform)	0.0278**	138.4***	3,332**
	(0.0110)	(37.72)	(1,485)
Observations	41,705	29,369	29,375
Number of Individuals	5,418	3,944	3,946
R-squared	0.593	0.630	0.744
Pre-Treatment Mean Dependent Variable	0.848	1,937	31,751
Individual Fixed Effects	Yes	Yes	Yes
State-Year-Num Children Fixed Effects	Yes	Yes	Yes

Notes: The table presents the coefficient estimate α_1 from equation 1. Sample restrictions are discussed in Section 7.1. For the top panel (high-education sample), the minimal age at time-limit introduction is 23 rather than 19, and for the middle panel (fathers to teenagers), the minimal age at time-limit introduction is 25. Numbers in parenthesis are standard errors clustered at the state level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

separate state and year fixed effects, the state-level EITC rate can be controlled for in the regression specification. For each dependent variable, Table 7 presents results both without and with the EITC rate in the first and second columns, respectively. As can be seen, the results remain the same across the two columns, which is evidence that the EITC cannot be driving our results.

Interestingly, the state-level EITC rate does not affect male labor market outcomes with fairly precisely estimated null coefficient estimates for that variable. This is consistent with past studies that have found either very small or insignificant effects of the EITC on men (Agan and Makowsky (2018); Eissa and Hoynes (2004); Hoffman and Seidman (2003)).

In Appendix Figure A4 I show that positive labor market responses to time limits persist when limiting the sample to states with no EITC in place. The effects fade out over time, as more states that adopted their own EITC rate were excluded from the sample.

7.3 Attrition in the PSID Data

As with many other panel data sets, the PSID suffers from attrition among its respondents. Attrition can compromise the conclusions arising from the analysis if it is selective. In our case, attrition can be driven by incarceration, which can be sizable among our sample of disadvantaged males. Attrition can also result from family dissolution, as the PSID only tracks those who belong to the core baseline sample. If those incarcerated or separating from the tracked family unit have on average worse labor market outcomes and their attrition over time is correlated with the introduction of time limits over time, then the data may falsely exhibit improvements in labor market outcome in response to time-limit introduction.[38, 39]

[38] I note that a large part of this concern is addressed through individual and state-year fixed effects in the regression analysis, as variation does not only exist across time in time-limit introductions but also within individuals or within state-year units.

[39] Attrition could be particularly of concern considering past evidence that mothers who were potentially eligible for welfare payments exhibited a higher probability of being single in response to work-related welfare rules (Moffitt et al. (2020)), or that eliminating welfare eligibility due to a drug conviction increased the chances of recidivism (Yang (2017)). However, it should also be noted that in Moffitt et al. (2020), the effect of the welfare reform on family dissolution is not driven by time limits (which exhibit null coefficient estimates) but rather by other work-related welfare reform components. Furthermore, the policy referred

Table 7: Labor-Market Outcomes following Time-Limit Introduction - Controlling for EITC

	Employed		Annual Hours Worked		Annual Labor Income (2010 USD)	
Post*Treatment (w/ child affected by reform)	0.0245**	0.0245**	91.93***	91.80***	3,347***	3,345***
	(0.0114)	(0.0114)	(28.64)	(28.66)	(1,123)	(1,124)
State EITC Rate		0.00035		-0.872		-11.18
		(0.00074)		(2.45)		(66.06)
Observations	41,231	41,231	29,773	29,773	29,775	29,775
Number of Individuals	5,627	5,627	4,141	4,141	4,142	4,142
R-squared	0.564	0.564	0.601	0.601	0.700	0.700
Pre-Treat Mean Dep. Var.	0.846	0.846	1,934	1,934	32,036	32,036
Individual Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
State-Num. Children FE	Yes	Yes	Yes	Yes	Yes	Yes
State-Level Controls	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The table presents the coefficient estimate α_1 from equation 1, only state-year fixed effects are replaced with state and year fixed effects and in the second column of each dependent variable, the state EITC rate for that year is included as a control variable. Sample restrictions are as in Table 2. State-Level Controls refers to the real minimum wage, real median household income, and unemployment rate. Numbers in parenthesis are standard errors clustered at the state level. The number of observations varies from those in Table 2 due to the inclusion of the control variables, which are not available for earlier years. The results are not sensitive to this. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

To address this concern, I performed the regression analysis on two sub-samples of the males in the data, conditioning on the number of times or the range of years they appear in the data during the sample period. These males should be more comparable to each other in terms of attrition, so if the results still hold for them, then this is evidence that attrition is not driving the results. One challenge with an attrition analysis using PSID data is that the PSID transitioned from being an annual to a bi-annual survey in 1996, which is in the midst of time-limit introductions. To deal with the possibility that states introducing time limits later had greater attrition due to the bi-annual survey, I further limit this sample to states that introduced time limits from 1996 onward. In addition, the baseline sample has observations through 2016 and includes observations only up to 15 years after the introduction of time limits in their states. I therefore also limit the sample in the attrition analysis to individuals that experienced the introduction of time limits before 2002.[40]

I select a range of times or years that individuals appear in the data, such that a four-unit range maximizes the number of males. This results in confining the sample to males appearing either 12-15 years or 5-8 times after the introduction of time limits in their states. Table 8 presents the results for all three labor market dependent variables for both male samples - those appearing 12-15 years or 5-8 post time limit introduction in the top and bottom panels, respectively. The coefficient estimates are consistent with those of the baseline in Table 2, although with reduced statistical significance, and for employment when limiting the sample to those appearing 5-8 times after time limit introduction, the effect is small (1.8 percentage point increase) with a p-value of 0.3. The p-value for the employment effect of 3.16 percentage points for the sample of males appearing 12-15 years post-treatment is 0.112. For the labor income effect for males appearing 5-8 times post-treatment the p-value is 0.127. Overall, the results in Table 8 suggest that attrition is not driving the effects of

to in Yang (2017) is for released felons convicted of a drug offense after the passage of the PRWORA in 1996 and the ban on welfare receipt for these individuals is gradually cancelled in the post-PRWORA years, such that by the end of 2000, 20 states have canceled the ban. It may very well be that under these specific circumstances, attrition in my sample is not affected by this policy.

[40] Overall, eight states are excluded from the attrition analysis.

time limits in the baseline analysis.

8 Concluding Remarks

The U.S. welfare reform of the 1990s fundamentally changed cash assistance to poor families. Despite this substantial change in insurance measures for low-income households, evaluations of this reform have focused solely on females and their children, but not on males. This paper is the first to assess the effect on males from low-income households. Overall, the results show substantial increases in employment, hours worked, and income from labor.

Results also show that male spouses alleviated some of the negative shock to household income due to decreased welfare benefits by increasing their labor-market outcomes. This is despite their not being the direct household member experiencing the shock, like their female counterpart. Thus, I show that the focus of the past literature on female responses to male partner shocks is too narrow and that male responses play an important role as well.

Taken as a whole, the results imply that overlooking male responses can result in a partial understanding of the overall economic and social implications of welfare reform. However, the implications of this paper on policy evaluations can go beyond welfare reform by highlighting the value of evaluating policy measures or shocks experienced within the household more broadly in terms of all household members. The paper also raises the potential for labor market interventions to alleviate or possibly even reverse the decline in male labor force participation rates observed in recent decades, particularly among low-educated and prime-age men.

Table 8: Time Limit Effects - Conditioning on the Number of Years or Times Appearing after the Introduction of Time Limits

	Employed	Annual Hours Worked	Annual Labor Income (2010 USD)
	Observed 12-15 Years Post-Treatment		
Post*Treatment (with children affected by reform)	0.0316	142.4***	3,506**
	(0.0194)	(50.66)	(1,572)
Observations	14,624	10,067	10,067
Number of Individuals	979	772	772
R-squared	0.560	0.594	0.693
Pre-Treatment Mean Dependent Variable	0.864	1,997	31,808
	Observed 5-8 Times Post-Treatment		
Post*Treatment (with children affected by reform)	0.0188	107.7**	2,860
	(0.0181)	(50.97)	(1,825)
Observations	17,342	11,720	11,720
Number of Individuals	1,166	905	905
R-squared	0.553	0.586	0.694
Pre-Treatment Mean Dependent Variable	0.883	2,024	33,824
Individual Fixed Effects	Yes	Yes	Yes
State-Year-Num Children Fixed Effects	Yes	Yes	Yes

Notes: The table presents the coefficient estimate α_1 from equation 1. The sample is described in the Data section, but it is confined in each panel to those appearing the range of year or number of times stated after experiencing the introduction of time limits, as long as the time limit took place between 1996 and 2001 (see Section 7.3). Numbers in parenthesis are standard errors clustered at the state level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

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Appendices

.1 Time-Limit Introduction Year in States without EITC Supplements

Appendix Figure A1 presents the distribution of the year that time limits were first introduced among states that did not adopt an EITC supplement by 2016. As can be seen, there is a large degree of variation. This variation covers the time span during which all but six states in the full dataset introduced time limits.

.2 Time-Limit Extent and State-Level Characteristics/Policies

Appendix Figure A2 presents similar correlation plots to those presented in Figure 3, only rather than having the year time limits were introduced on the horizontal axis, there are state time-limit lengths (in months) upon introduction. The results further strengthen the lack of correlation between state-level characteristics prior to time-limit introduction or state-level policy expansions and an additional aspect contributing to the variation of the main explanatory in the analysis, namely the extent of time limits upon introduction.

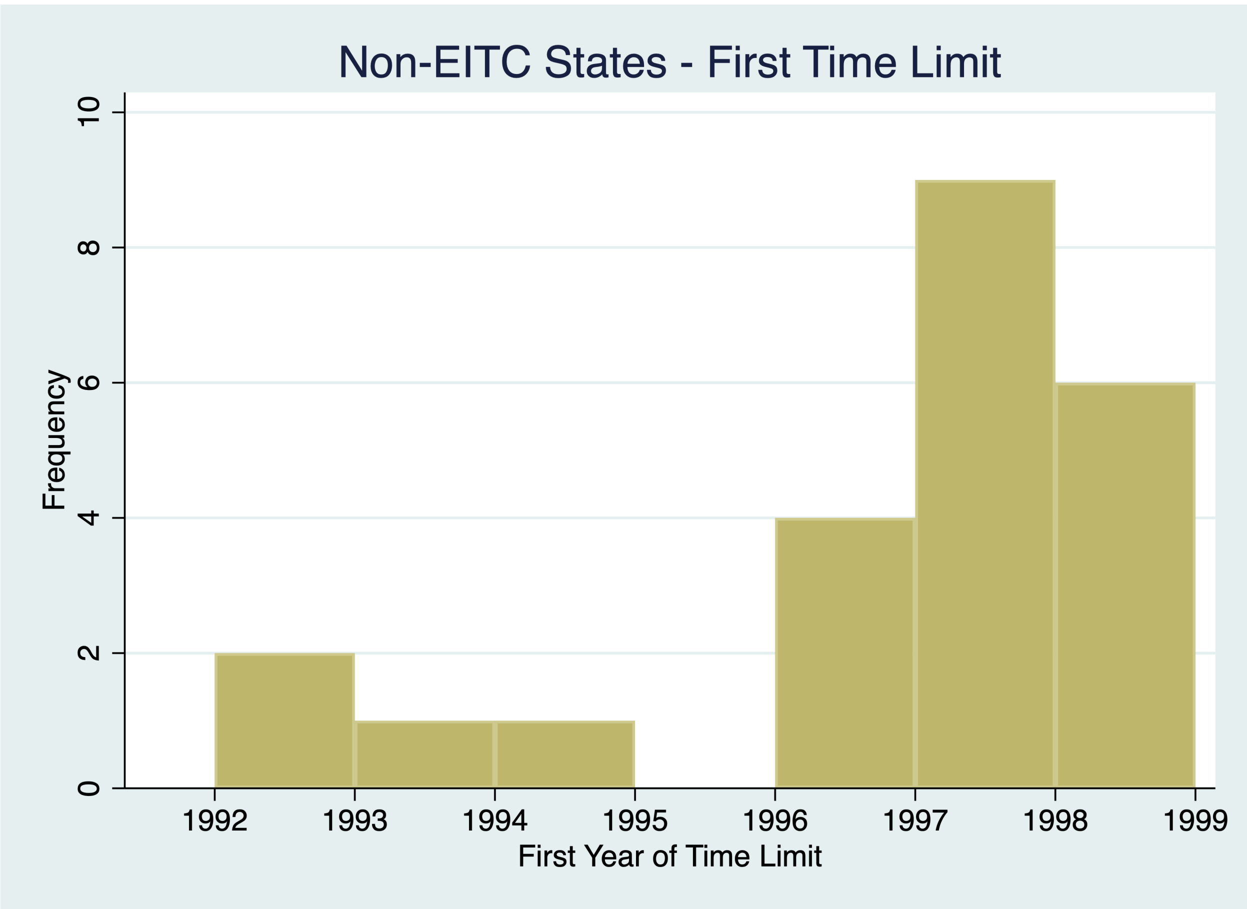
.3 Welfare Payment Event-Study Analysis

Appendix Figure A3 presents an event-study analysis for the male sample on whether the male or their female partner received welfare payments over the last year in the top and bottom panels, respectively. The results confirm that males did not experience a decline in welfare payment receipt, while their female partners did experience one.

.4 Female Summary Statistics

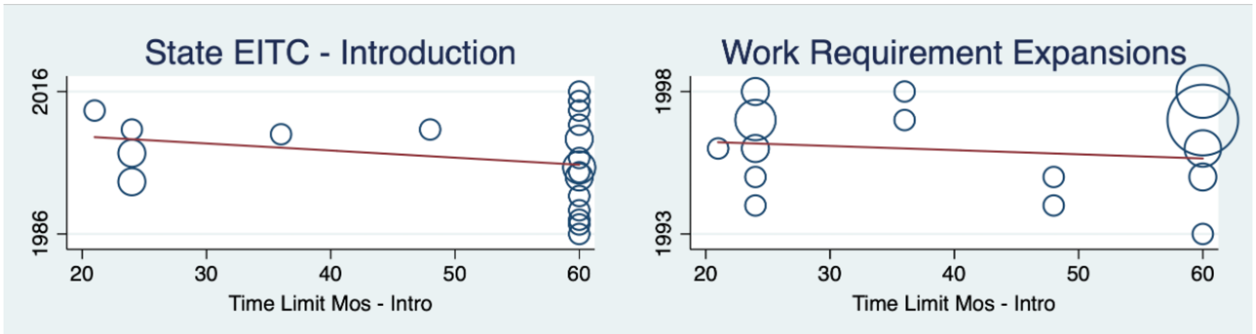
Appendix Table A1 presents female summary statistics for the female time limit effects analysis presented in Section 6.4. The female marriage/cohabitation rate is lower than that

Appendix Figure A1: Time-Limit Introduction Distribution Among States Without EITC Supplements

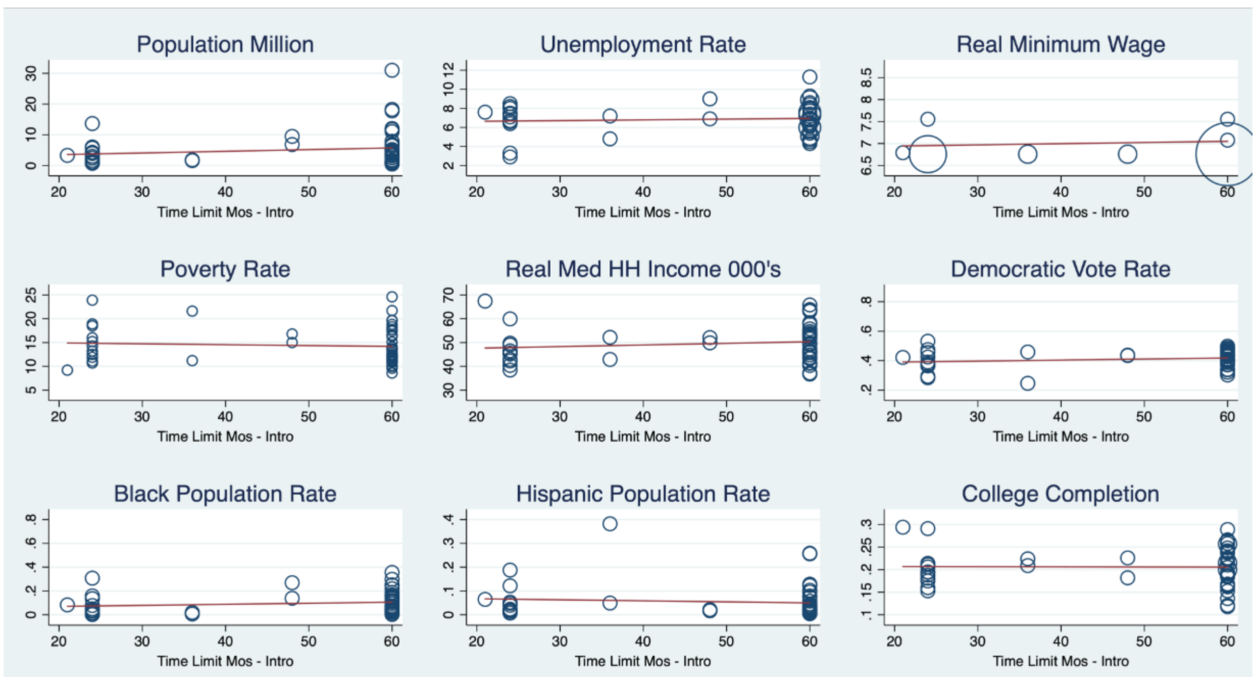


Appendix Figure A2: Time-Limit Extent and State-Level Policies/Characteristics

(A) Time-Limit Extent and State-Level Policies Introduction/Expansion

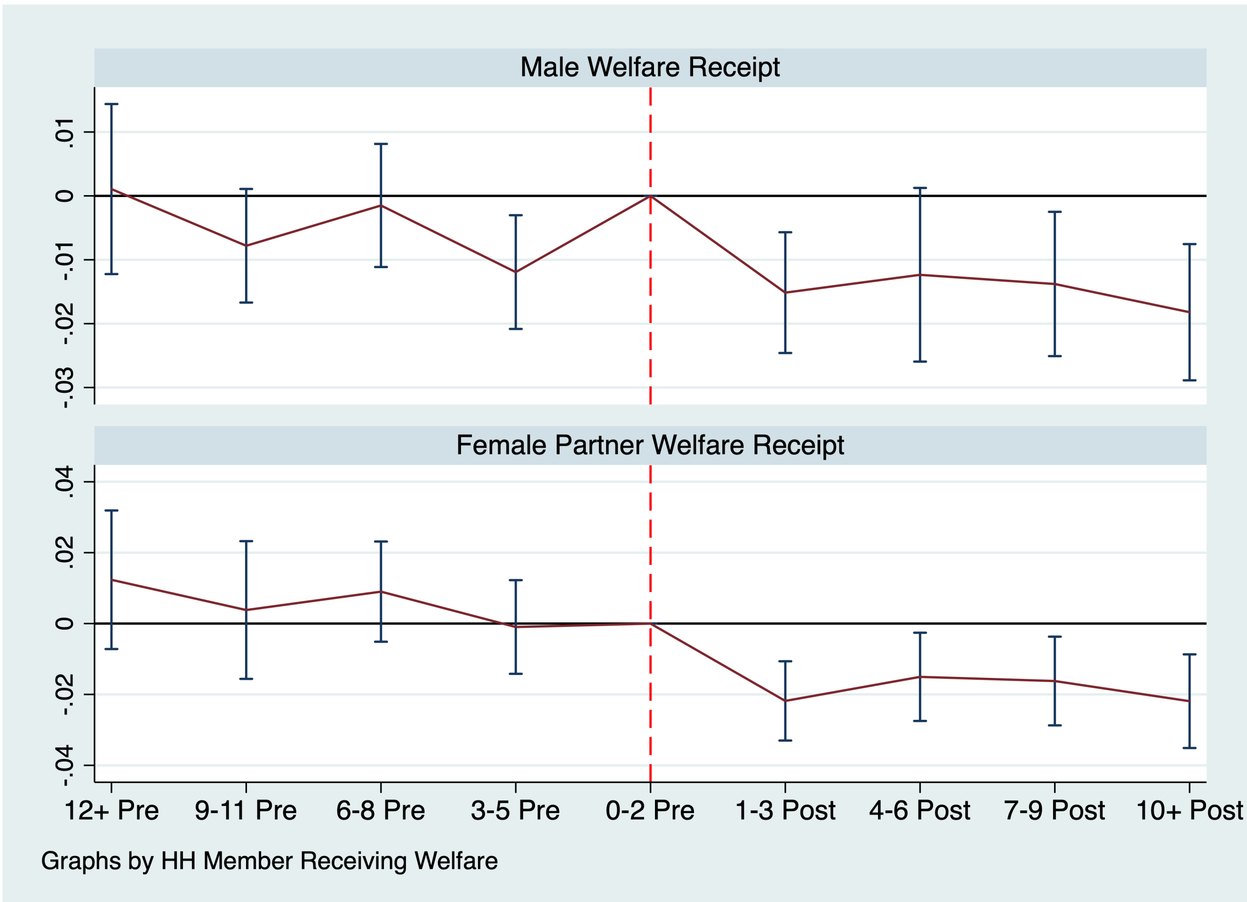


(B) Time-Limit Extent and 1990-1992 State-Level Characteristics



Notes: For data sources, see Section 5.2.
The top panel plots for each state the year the stated policy was introduced (state-level EITC) or substantially expanded (JOBS) and the number of months for time limits upon their introduction.
The bottom panel plots for each state the value of the stated state-level characteristics as of 1990-1992 and the number of months for time limits upon their introduction.
The fitted linear regression line is indicated in red in both panels. Larger circles indicate more combinations.

Appendix Figure A3: Welfare Payment Receipt



Notes: The figure presents event study coefficient estimates from equation 2. The vertical lines represent 95 percent confidence intervals. Standard errors are clustered at the state level. The sample is males as specified in the data section. Sample sizes range from 37,992 to 38,924 observations covering 4,690-4,693 males. Pre-treatment means are 0.0192 and 0.0260 for male and female partner receipt, respectively.

of the male sample, at 59 percent as opposed to 76 percent. Unlike the males, females who are treated have worse labor market outcomes than those who are untreated. Lastly, welfare receipt rates are substantially higher, even among the male partner.

.5 Zero State-Level EITC

To further address concerns that the EITC, which coincided with welfare reform and also greatly affected disadvantaged families, may be driving the results, Appendix Figure A4 presents the event-study analysis on states without a state-level EITC in place. I present results for an event-study analysis rather than the coefficient estimates from equation 1 because statistical significance of the effect may decline substantially in the long-term for this sample that excludes observations for many states from later years when a state-level EITC was already in place. Indeed, for employment and labor income, a (marginally) statistically significant effect is only observed up to 6 years after the introduction of time limits.

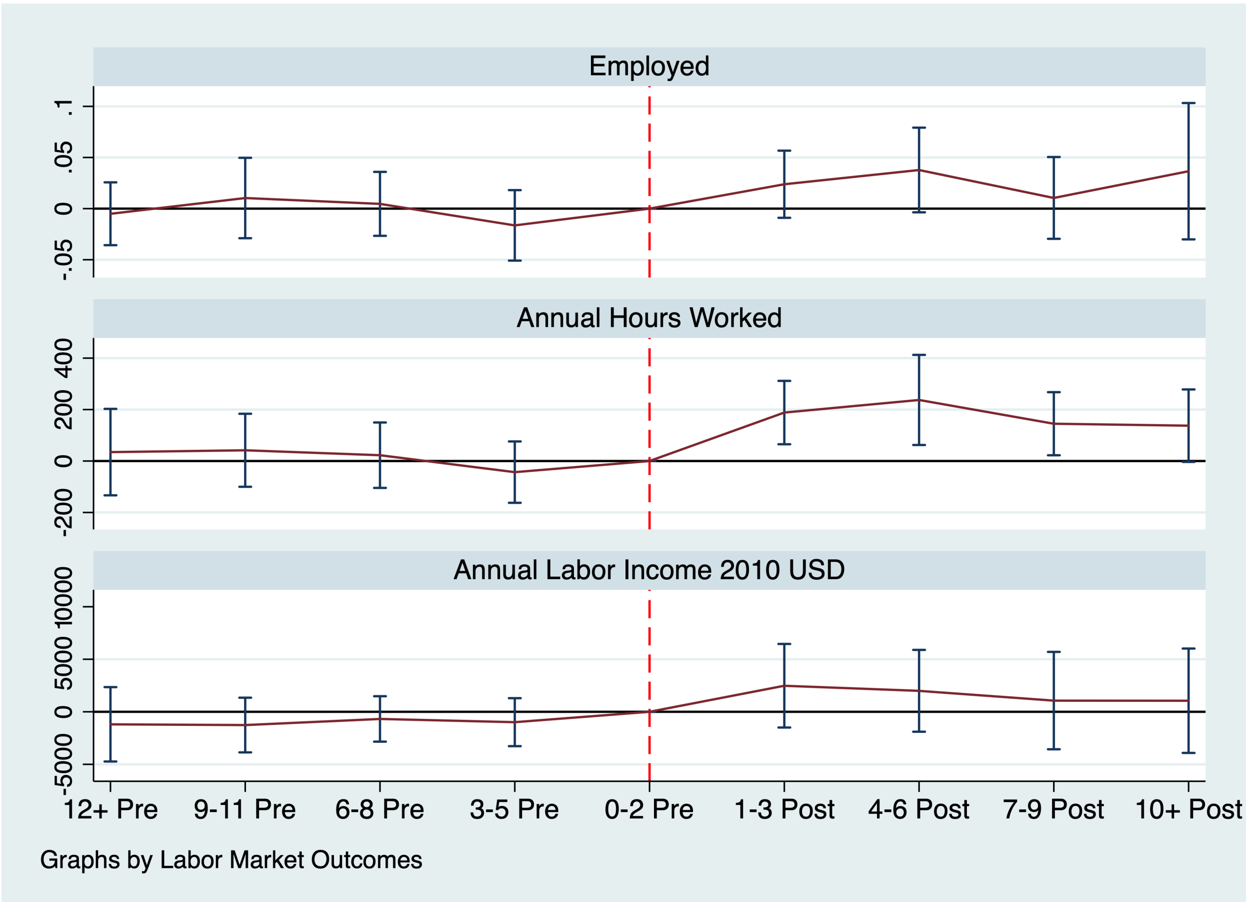
In Table 7 I also present evidence showing that the results are not sensitive to controlling for the state-level EITC rate in the regression specification. These regressions include separate state and year fixed effects, rather than state-year fixed effects, and are therefore not the main results of this study.

Appendix Table A1: Summary Statistics - Females

	All	Treated Individuals	Not Treated Individuals
Number of Observations	49,359	17,628	17,551
Number of Individuals	5,319	2,199	2,802
Treatment Intensity (Yrs of	4.07	8.15	0.00
Welfare Benefits Lost)	(4.70)	(3.97)	-
Age	33.66	31.96	35.39
	(9.41)	(7.52)	(8.95)
Age of Youngest Child Upon	9.65	5.92	18.16
Time Limit Introduction	(6.59)	(3.85)	(3.90)
Age of Youngest Child	9.41	6.20	14.06
(Contemporaneous)	(8.02)	(5.20)	(6.98)
Married / Co-Habiting	0.59	0.56	0.55
	(0.49)	(0.50)	(0.50)
Number of Children under 18	1.56	2.12	0.84
in Household (Contemporaneous)	(1.31)	(1.13)	(1.05)
Years of Schooling	12.21	12.40	11.82
	(1.84)	(1.68)	(1.89)
High School Degree or Less	0.76	0.73	0.84
	(0.43)	(0.45)	(0.36)
White	0.46	0.45	0.55
	(0.50)	(0.50)	(0.50)
Black	0.49	0.51	0.36
	(0.50)	(0.50)	(0.48)
Employed	0.70	0.70	0.68
	(0.46)	(0.46)	(0.47)
Annual Hours Worked	1,196.8	1,158.0	1,301.3
	(957.1)	(944.3)	(967.6)
Income from Labor (2010 $)	15,799.9	14,558.8	17,692.4
	(16,843.4)	(14,929.3)	(17,819.8)
Welfare Receipt - Female or	0.101	0.144	0.054
Male Partner	(0.301)	(0.351)	(0.225)
Welfare Receipt - Female	0.097	0.137	0.050
	(0.296)	(0.343)	(0.219)
Welfare Receipt - Male Partner	0.042	0.015	0.002
	(0.200)	(0.120)	(0.040)

Notes: The sample is females with a high school education or less as of age 25, or resided in a household receiving welfare payments or government transfers prior to treatment ages 18-60 who were 19-55 when time limits were introduced in their state and are observed up to +/-15 years relative to time-limit introduction during 1980-2016. The sample represents individuals reporting their employment status during the sample period (the largest sample in the regression analysis). Numbers in parenthesis are standard deviations for the means presented. Treated and untreated means in the second and third columns for males are at the individual level, taking the latest non-empty observation for that person up to five years prior to treatment, with the exception of the intensity of treatment variable that is the mean of all treated persons' observations post-reform. Sample sizes are substantially smaller than the reported number of individuals in the first column, as not all individuals were in the sample or reported the variables of interest up to five years prior to treatment. For age of youngest child means, the samples are only for those who have children. Annual hours worked and income from labor means are for a sub-sample as described in the Data section.

Appendix Figure A4: The Effect of Time Limits when there is No State-Level EITC



Notes: The figure presents event study coefficient estimates from equation 2. The vertical lines represent 95 percent confidence intervals. The sample is the same as the baseline sample with the additional restriction that states only enter the sample while they had no state-level EITC in place. Sample sizes range from 23,939-33,850 observations covering 3,570-4,910 males. Pre-treatment means are 0.85, 1,945, and 31,728 for employed, annual hours worked, and annual labor income, respectively.