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-negligible 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). This was a dramatic change that affected low-income household eligibility for welfare benefits. The reform's key features included lifetime time limits on receipt of welfare payments, work requirements, and financial incentives to work. Within these many reforms, lifetime time limits are considered to be one of the greatest departures from the pre-reform policy (Grogger (2002, 2004); Swann (2005)). Prior to the reform, parents were guaranteed welfare-payment eligibility that lasted until the youngest child reached age 18; after the reform the number of years parents were eligible for welfare payments during their lifetime was restricted by up to five years.

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 (2009a); 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 when the time limit was introduced 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 below a certain age. For example, individuals with a youngest child aged 13 or above had five or less years of eligibility remaining and would not be affected by the introduction of a five-year time limit. 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 when the time limit was introduced.[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 labor market outcomes for disadvantaged males.[4] Thus, the analysis overcomes a frequent challenge with empirical research evaluating the effects of welfare reform, which is potentially confounding alternative policies, for example, EITC expansions, or economic trends 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 the age of one's 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 who have a high school diploma or less. I expand the sample to include males who were observed either residing in a household with government transfers (AFDC/TANF or SNAP) or on Medicaid prior to time-limit introduction in their state. The longitudinal nature of the PSID allows

[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.

me to observe individuals both before and after 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 a lack of differential trends among the treated observations prior to time-limit introductions.[5]

The results confirm that the focus on females when assessing welfare reform may have underestimated its overall effects. Male employment, hours worked, and labor income increased in response to time limits, and these effects represent 4-10 percent of pre-treatment means. While these effects are sizable, I show that they are 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 their increase in 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. When observing these patterns, Grogger (2003) and Mazzolari and Ragusa (2012) propose that other household members, such as a male partner, are compensating for the female household member's lost income. Edin and Lein (1997) note that sharing housekeeping expenses with other family members or boyfriends is common in welfare-receiving families. Bitler et al. (2006) present evidence that suggests that welfare reform increased the probability that children would live with other family members to compensate for their mother's lost income. In this paper, I investigate the proposals in Grogger (2003) and Mazzolari and Ragusa (2012) regarding time limit effects

[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.

on male partners and add to the broader 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.

My 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, when assessing policy measures or shocks, it important to capture intra-household interactions.

Evaluations of labor market policies that target disadvantaged populations have mostly focused on either the younger workers, or when examining older age groups, on females. Research on the EITC found very small or null effects on men (Agan and Makowsky (2018); Eissa and Hoynes (2004); Hoffman and Seidman (2003)), but recent, first-time evaluations of minimum wage effects on disadvantaged fathers found 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.[6] 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 developing policy to reverse these downward trends.

My findings that males respond to their female partner's loss of welfare benefits emphasize the importance of taking into account all members of the household when evaluating policy, even those members that are not directly targeted by the policy. Male partners appear to be

[6]From a historical perspective, male labor force participation rates were hardly changing until the end of the 20^{th} century (see Goldin (2006), Figure 1). This, along with the sharp contrast to dramatic increases in female labor force participation rates during the same period, may have contributed to a notion of male labor supply elasticities that are close to zero.

mitigating the financial effects of decreased welfare payments, despite not necessarily being the household member that experiences the direct income loss. Chiappori and Mazzocco (2017) provide an overview of household decision models that allow separate actions for each household member. There are numerous studies that find a spousal reaction to their partner's direct impact from a labor market intervention, policy shock, or income shock, including 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 finds 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.[7] I diverge 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.[8]

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 is a description of the the data, and Section 5

[7]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 (see Section 2 for further details concerning this). As such, the change in the female partner's welfare payment eligibility is more of an income shock rather than indirectly affecting the male's welfare payment eligibility.

[8]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.

the empirical strategy. Results are discussed in Section 6, and in Section 7 I present 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 that wished to change their welfare program. Lifetime 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 in 1992 while the last state to adopt time limits was Vermont in 2014. By 2014, all states had initiated time limits.

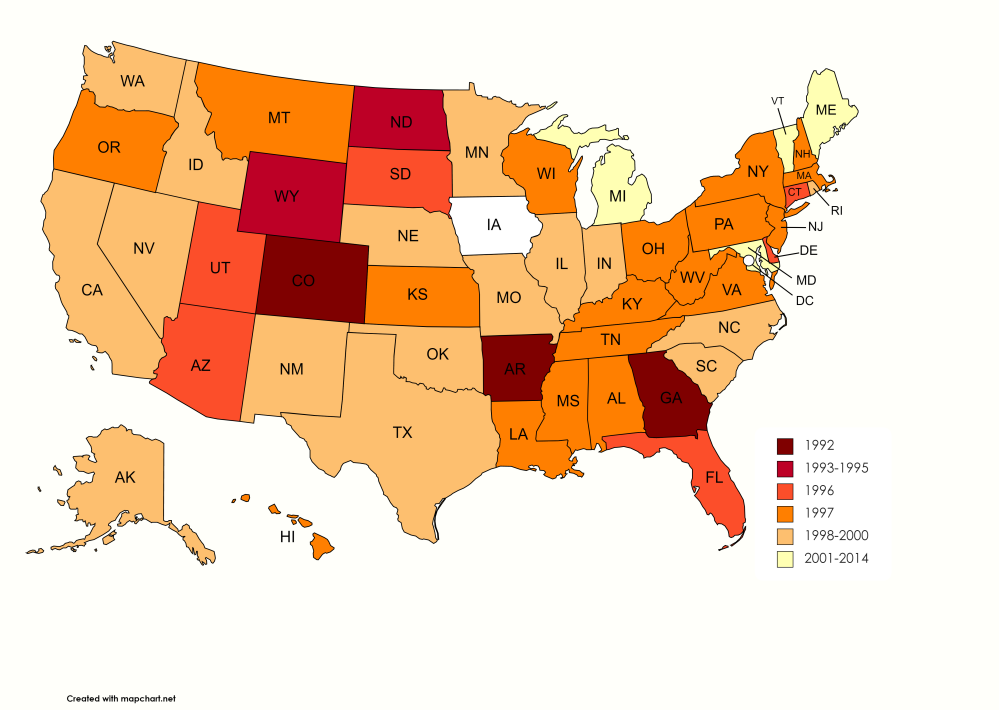
Figure 1A and Figure 1B map the year each state's time limits were introduced or their extent upon introduction (in months).[9] I exclude Iowa and Washington, D.C., as time limits were implemented on an individual basis in the former (Chan (2013)) and repealed four years after introduction in the latter. 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.[10]

[9]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.

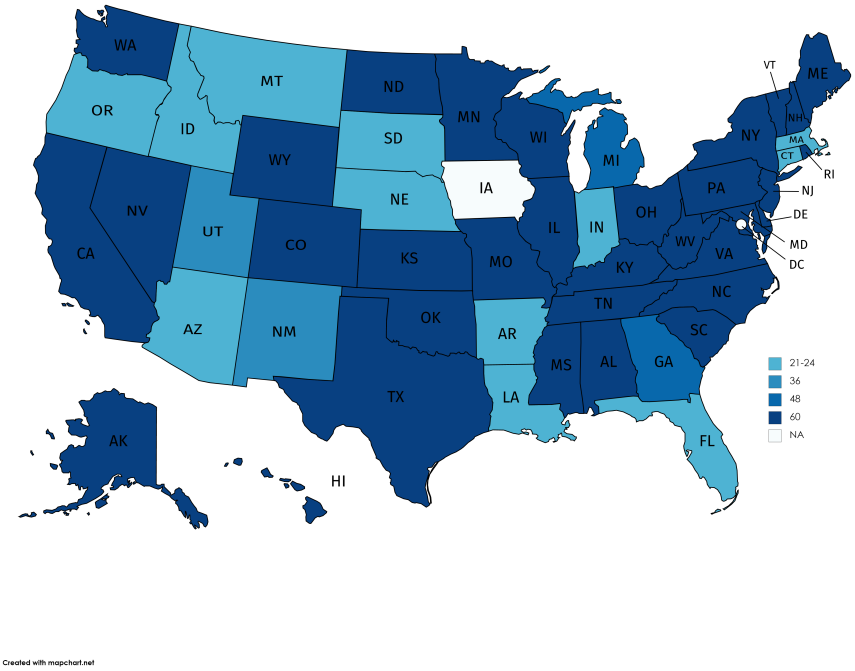
[10]A review of the vast literature on welfare time limits can reveal several discrepancies across studies in their documentation of time limit timings. I constructed my own database of time-limit introductions using the Urban Institute WRD, and undertook several rounds of communication with the Center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

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 introduced to support 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 more than just those directly receiving or eligible for payment. In fact, in Moffitt et al. (2020), over 50 percent of mothers potentially eligible for welfare payments in their sample from the Survey of Income and Program Participation (SIPP) resided with a male partner or spouse. Furthermore, over 11 percent of these males were not the biological fathers of any of the children in the household. Overall, these figures demonstrate that male adults - not necessarily biological fathers - reside in many households that are potentially eligible for welfare payments.

It is important to note that in many states male partner income is not included when 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.[11]

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.

[11]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 in detail their unsuccessful efforts to obtain the rules for other years, which would be necessary for a precise

Besides determining eligibility at the individual level, welfare payments in coupled households are also often distributed at the individual rather than at the household level. According to Fremstad (2016), as of 2013, in all but four states, if a parent is living with an unmarried partner who is not the father or mother of any of the children in the unit, the partner is not eligible for TANF assistance. Hahn et al. (2016) define two-parent families as units with two adults who are married and/or share a biological or adoptive child. They examine a sub-sample of these families which have at least one adult TANF cash assistance recipient. They find that among these households, only 2/3 have both parents receiving TANF payments. The remaining 1/3 have only one adult receiving TANF. This is despite their sub-sample excluding couples who are not married *and* do not have a joint biological/adopted child, for whom it is even less likely that the male partner would be eligible for welfare payments. These statistics demonstrate that the share of males who reside with a female partner who is receiving or potentially eligible for welfare payments, and are not directly receiving or eligible for these payments themselves, can be sizable. In the sample of disadvantaged males used in this study, the male receives welfare payments himself in fewer than 40% of the observations in which his female partner receives welfare payments.[12, 13]

3 Why Welfare Time Limits Can Affect Males

When welfare payment time limits are introduced, household welfare income should weakly decline. If one assumes an efficient household framework, based on Chiappori (1992), household labor supply should weakly increase (assuming leisure is a normal good). However, within Chiappori (1992)'s non-unitary framework for household labor supply, how the in-

analysis of their interaction with time limits.

[12]Welfare payment reporting in the database for this study is at the annual level while the reporting in the PSID is for each month (although the data is annual or bi-annual). Thus, the actual overlap between the male and his female partner's welfare payment receipt may even be less.

[13]Appendix Figure A1 presents a breakdown of the sample used in this study along with statistics on welfare payment receipt for different subgroups of the sample. One of these statistics is the share of males who are observed receiving welfare payments, conditional on their female partner receiving welfare payments that year.

crease in labor supply is split between the household members can vary. Male partners' labor supply response to the introduction of time limits can depend on their outside options, wage offers, changes in intra-household bargaining power, or shadow price of time.[14] Nevertheless, under some basic circumstances, household labor supply models would predict an increase in male labor supply. This would receive further validity when accounting for intra-household specialization (Becker (1973)) and males' likelihood of having an advantage in the production of market goods, as opposed to household goods, in comparison to their female partners.

The efficient household framework demonstrates that in order to increase labor supply it is not necessary for the male partner to be a direct recipient of welfare payments prior to the introduction of time limits.[15] One can further extend the framework and consider an *expected* decline in household welfare payments rather than an immediate decline. This would be relevant for households that are not receiving welfare payments when time limits are introduced but may expect to rely on them in the foreseeable future. It may also be relevant for households that continue to receive welfare payments despite the introduction of time limits but understand that the time limit may be binding in the foreseeable future. The same factors that can increase male labor supply within the efficient household framework described above will apply under these circumstances as well.[16]

[14]Importantly, effects on intra-household bargaining power can come into play through changes in each partner's outside options. Decreases in 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)). Welfare reform can also increase female bargaining power 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 economic independence due to increased labor force participation (Grogger and Karoly (2009b); Bitler et al. (2004)).

[15]Indeed, this is the case for many males in this study who are with a female partner receiving welfare payments, as discussed in Section 2.

[16]A different perspective accounts for the possibility that positive effects on female labor supply in response to time limits can have equilibrium implications for males, who may face greater competition in the unskilled labor market. Groves (2016) shows that such increased competition due to welfare reform more broadly 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 when time-limits were introduced in each U.S. state, and the extent of the time limits. To this end, I utilized the Welfare Rules Database (WRD) from the Urban Institute and other publicly available online sources.[17]

4.2 Panel Study of Income Dynamics Data

The PSID has been tracking individuals longitudinally in thousands of families throughout the U.S. since 1968. The following labor market outcome variables were constructed from the PSID 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 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 all individuals are always covered.[18]

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 range is 1980 to 2016.[19] I limit

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

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

[19]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 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 very similar to the baseline results.
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 bi-annual rather than annual waves.

the sample to disadvantaged males who are more likely to be affected by changes in welfare policies, either directly or via the partners they reside with. This entails examining individuals who fulfill at least one of the following: having a high school education or less, received directly 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.[20]

I limit the sample to individuals aged between 18 and 60. The treatment and control groups are defined based on the age of the youngest child upon time-limit introduction.[21] To make the treatment and control samples relatively comparable, I further limit the sample to those aged 19-55 upon time-limit introduction in their state - the age when males generally reside with minor children. Under these limitations, the sample covers 4,418-5,726 males over 39,739-49,527 observations. Additional sample criteria primarily related to the handling of individuals who move states during the sample period are outlined in Appendix Section .1.

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 extent of the time limit and the age of the youngest child residing with them (not necessarily their biological child) upon introduction.[22] 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 implementation, 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

[20]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.

[21]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.

[22]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.

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. When individuals move to a different state in the data, and this coincides with more than one time-limit introduction, they are assigned a treatment year based on the first state they resided in that adopted time limits.

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 payments for up to 18 years during their lifetime. In Georgia, a four-year time limit was introduced in 1992. Our individual's youngest child would be two years old, so the maximum number of years they can receive welfare payments over their life-cycle decreases to six years. This includes the 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 is seven 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. In California, the highest number of years this individual can receive welfare payments changes from 18 to 14 years in 1999 with the introduction of a five-year time limit.

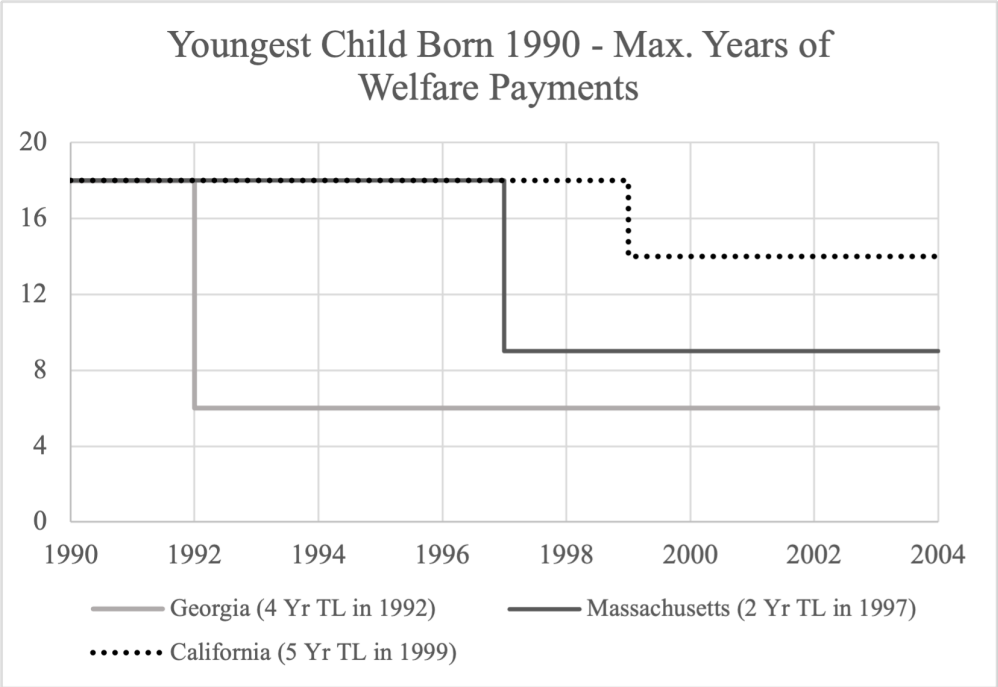
A comparison between Figure 2A and Figure 2B illustrates the within-state variation of time-limit effects across parents. In particular, a parent in California with a youngest child born in 1990 is affected by time-limit introduction in 1999, whereas a parent with a youngest child born in 1985 is not affected, as in 1999 their child is 14 years old and will age out of eligibility before reaching the five-year time limit. Time-limit lengths ranged from two to five years, so whether a parent was affected or not depended on whether their youngest child was older or younger than 13-16 years when the time limit was introduced.

4.4 Summary Statistics

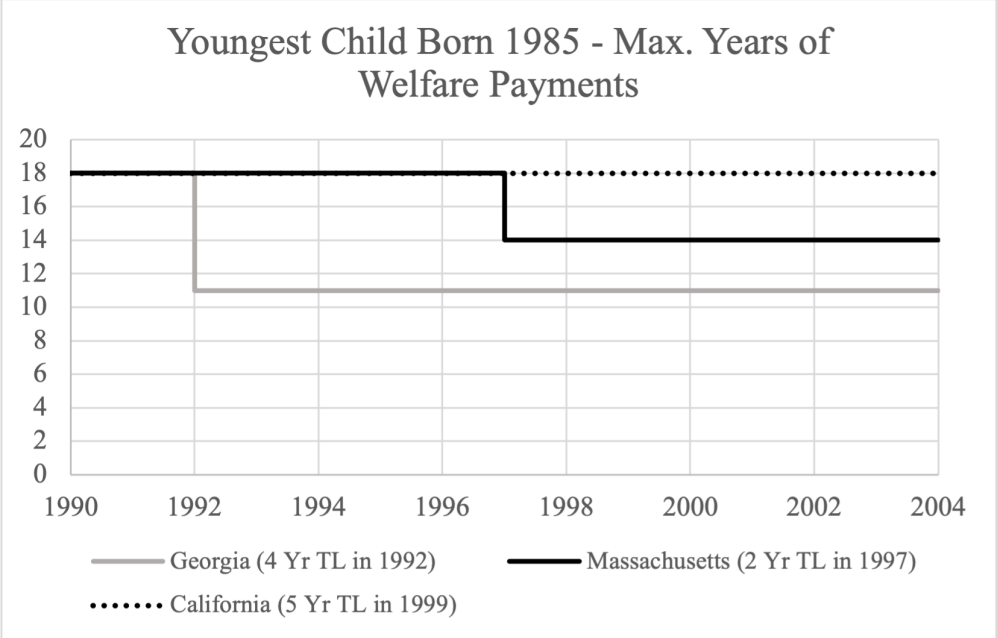
Summary statistics (means and standard deviations in parentheses) are presented in Table 1 for the entire sample, the treated sample, and the untreated sample in columns 1, 2,

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 an assumption that the youngest child is their only child. Months receiving welfare payment prior to the introduction of time limits do not count against households.

and 3, respectively. Table 1 shows substantial differences between treated and untreated males. Treated males 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 children present in the household and therefore time limits affected their future eligibility for welfare payments. Interestingly, employment rates and hours worked are greater for treated males on average, but their labor income is lower. All these substantial differences between treated and control males are accounted for in our regression analysis through the inclusion of individual fixed effects. Furthermore, the analysis verifies that these differences are not driving the results by examining parallel trends in the outcomes of interest prior to treatment.[23]

Measures of welfare payment receipt in Table 1 offer a better understanding of the sample population affected by welfare payment time limits. The last statistic among the individual-level means in Table 1 reveals that 16% of treated males in the sample received welfare payments (either themselves or their spouses) at least once prior to the introduction of time limits. The last 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. Appendix Figure A1 shows that conditional on their female partner receiving welfare payments, males are observed receiving welfare payments in fewer than 40% of the observations, thus demonstrating the gap in welfare payment receipt between the male and his female partner.

The small fraction of those reporting receipt of welfare payments can raise concern that the sample population should not be substantially affected by welfare payment time limits. However, Meyer et al. (2015) show that under-reporting of TANF/AFDC payments is sub-

[23]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.

stantial in the PSID (as in the CPS, SIPP and other frequently-used survey datasets). They estimate that the actual rate of TANF/AFDC receipt is over 2.5 times greater than what is reported in the data. This implies that in our sample, welfare receipt by either the male himself or his female partner before the introduction of time limits may have characterized over 30 percent of treated observations. This excludes some of the sample that may rely on welfare payments as a precautionary measure without actually utilizing it, thereby also potentially responding to the introduction of time limits.

Appendix Figure A1 provides a breakdown of the sample described in Table 1 based on characteristics such as residing with a child during the sample period, observed married/cohabiting during the sample period, and more. For each subgroup covered in the breakdown, the share of observations that received welfare payments prior to time limit introduction is reported. The shares are intuitively consistent. For example, more males with a child report receiving welfare payments than males without a child. Treated males also have greater reporting of welfare payment receipt (even conditional on residing with a child during the sample period) as opposed to untreated males. These patterns reinforce the qualitative validity of this variable, despite the evidence in Meyer et al. (2015) of under-reporting.

Mean annual income from labor of 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 10,314 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 I 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 little over two-thirds of the mean family income during years they received welfare payments.

5 Empirical Strategy

5.1 Male Labor Market Outcome Trends around the Introduction of Time Limits

I observe males for whom the time limits were imposed at different times. I compare the labor market outcomes between the treatment and control groups of the male sample. Figure 3 is a comparison of data trends between the treatment and control males for three labor market outcomes - employment, annual hours worked, and annual labor income - around the introduction of time limits. The data used are residuals from regressions that control only for individual fixed effects. For each sub-sample, the figure presents the means across individuals for each period in the horizontal axis, which is the year relative to the introduction of time limits.

The results suggest improvements among treated males in all three labor market measures after the introduction of time limits, in comparison to control males. The trends for employment and hours worked before the introduction of time limits are closely aligned (after accounting for individual fixed effects), while after the introduction, they diverge, with the treatment trends above the control trends.[24] For labor income, treated males appear to have had lower earnings than control males before the introduction of time limits (after accounting for individual fixed effects), but then the treatment line is mostly above the control

[24]There is evidence of a relatively sharp decline in employment and hours worked roughly 9 years following the introduction of time limits. This can be due to the recession that began in 2007 and affected a large share of the males in the sample who experienced time limit introductions in 1997-1998. It does not seem to be driven by differential life cycle trends between the treatment and control groups, as the results hold when age is accounted for (i.e. when age is controlled for along with individuals fixed effects in the regressions producing residuals - not shown).

Table 1: Summary Statistics

	All	Treated Individuals	Not Treated Individuals
Number of Observations	49,527	27,326	22,201
Number of Individuals	5,726	3,016	2,710
	Individual-Level Means		
Treatment Intensity (Yrs of	4.13	7.85	0.00
Welfare Benefits Lost)	(4.98)	(4.23)	-
Age of Youngest Child Upon	9.38	5.82	18.69
Time Limit Introduction	(7.15)	(4.16)	(4.41)
High School Degree or Less	0.90	0.89	0.91
	(0.30)	(0.31)	(0.28)
White	0.50	0.48	0.53
	(0.50)	(0.50)	(0.50)
Black	0.41	0.42	0.41
	(0.49)	(0.49)	(0.49)
Received Welfare Payments	0.12	0.16	0.07
Pre-TL (Male or Female Partner)	(0.33)	(0.37)	(0.26)
	Means Across All Obs.	Pre-Treatment Means Across All Obs.	
Age	34.58	29.95	33.12
	(9.88)	(7.30)	(8.72)
Age of Youngest Child	9.10	4.19	11.21
(Contemporaneous)	(8.38)	(4.04)	(6.72)
Married / Cohabiting	0.77	0.81	0.71
	(0.42)	(0.39)	(0.45)
Number of Children under 18	1.21	1.59	0.98
in Household	(1.27)	(1.29)	(1.17)
Employed	0.78	0.80	0.77
	(0.41)	(0.40)	(0.42)
Annual Hours Worked	1924.80	1969.61	1905.02
	(906.77)	(846.03)	(896.73)
Income from Labor (2010 $)	31822.81	30151.13	31071.87
	(25850.13)	(23796.28)	(23945.30)
Welfare Receipt - Male or	0.027	0.047	0.018
Female Partner	(0.16)	(0.21)	(0.13)
Welfare Receipt - Male	0.015	0.025	0.012
	(0.12)	(0.15)	(0.11)
Welfare Receipt - Female Partner	0.020	0.038	0.014
	(0.14)	(0.19)	(0.12)

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 parentheses are standard deviations for the means presented. 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.

line in the post-introduction period.

Figure 3 presents labor market outcome trends over time after accounting for individual fixed effects. The inclusion of individual fixed effects is important, considering the longitudinal nature of the data and the data construction method. Males are in the sample if they were aged 19-55 when time limits were introduced in their state of residence, and they are tracked up to 15 years before and after the introduction of time limits (see Section 4). Because I do not necessarily observe male outcomes during all survey years, this could result in compositional differences between treatment and control males around the year the time limit is introduced. Appendix Table A1 demonstrates the importance of accounting for these compositional differences through inclusion of individual fixed effects in the regression specification.

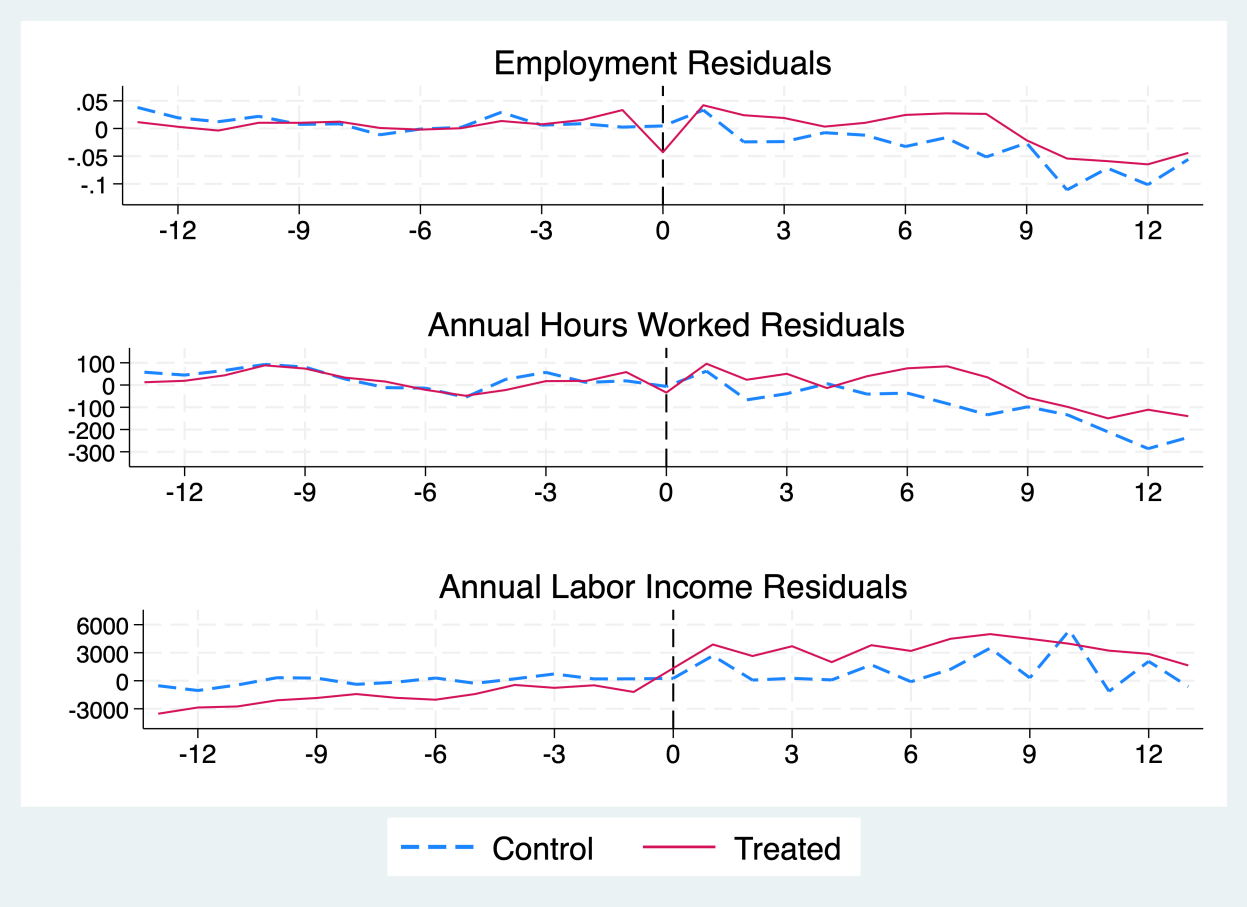
5.2 Specification

I analyse 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 number 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, there are three sources of variation. The staggered introduction of time limits creates variation across states and time. Variation is also generated within states based on the age of one's youngest child when time limits were introduced in their state of residence.[25]

There are two main challenges to evaluating labor market outcomes among the disadvantaged during the period beginning in the 1990s. First, many policies besides welfare payment time limits were implemented or expanded during these years and could have affected labor

[25]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.

Figure 3: Male Labor Market Outcome Trends - Years around the Introduction of Time Limits



Notes: The figure presents residual data trends, from regressions that control only for individual fixed effects, for the three main outcomes of interest: employment (indicator), annual hours worked, and annual labor income (2010 USD). The horizontal axis is the year relative to the introduction of time limits. For each of these "relative years", the mean is taken across all males in the sample either in the control or treatment group. Treatment is defined as experiencing a change in years of eligibility for welfare payments when time limits were introduced in one's state of residence (see Section 4).

market outcomes. The prime candidates are the Earned Income Tax Credit (EITC) and other components of welfare reform. Second, these years were characterized by substantial economic expansion, which research has shown can confound estimates during this period (Kleven (2019)). 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 or shocks are controlled for in the regression specification. Thus, in order for other state-level policies or shocks to bias the results, they need to be correlated with time-limit introductions *and* their effect has to depend on the age of the youngest child in the household when time limits were introduced. 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 policy measures introduced during this period.[26]

EITC rates are greater as one has more children under 18 in the household. This implies that even within state-year units there is variation concerning EITC intensity. This can raise concern if this variation in EITC intensity is correlated with the age of one's youngest child when time limits were introduced, which is our source of variation within state-year units. For this reason, the main regression specification is more conservative than a specification that solely controls for state-year fixed effects and controls for state-year-number of children

[26]For 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. Michelmore and Pilkauskas (2021) show that EITC had greater effects on mothers with younger children. However, the threshold is at roughly 6 years old and the underlying mechanisms discussed for this differential response are more relevant for mothers than they are for fathers or male partners.

fixed effects.[27, 28, 29]

The regression specification also exploits the longitudinal nature of the dataset by including individual fixed effects. As discussed in Section 5.1 above and demonstrated in Appendix Table A1, this component of the regression specification is crucial, as it allows me to control for potential compositional differences between the treatment and control groups over time. Lastly, the regression specification distinguishes between two types of states associated with the individual - his state of residence when time limits are introduced (indexed by s), and his contemporaneous state of residence (indexed by r).[30] I note that an individual's state of residence during time-limit introduction (s) is at the individual level (indexed by i) and is constant over time. Nevertheless, the s index is useful for fully understanding the variables defined in the regression specification.

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

$$\begin{aligned} y_{isrtn} &= \alpha_0 + \alpha_1 Treatment * After_{ist} + \alpha_2 After_{st} \\ &\quad + \alpha_3 PostTimeLimit_{it} + \gamma_i + \theta_{rtn} + \epsilon_{irtn} \end{aligned} \tag{1}$$

[27] In the remainder of the paper, whenever stating "number of children fixed effects", this refers to the number of children under age 18. Furthermore, when the number of children is 5 or more, this is grouped as one for the purpose of constructing the fixed effects.

[28] The inclusion of state-year-number of children fixed effects can raise concern for a substantial decline in the variation in the dependent or explanatory variables. This was ruled out by examining the R^2 in regressions for the sample of males with either the employment status or the main explanatory variable as the dependent variables while controlling for the various fixed effects. The R^2's were 0.52 or 0.86 for the dependent and explanatory variables, respectively. While this may indicate a low level of variation for the explanatory variable with these fixed effects, the changes in the R^2 when transitioning from regressions with state-year to state-year-number of children fixed effects were not large (0.04-0.05 lower with only state-year fixed effects rather than state-year-number of children fixed effects).

[29] While the literature has not found fertility responses due to welfare time limits (Low et al. (2020)), there is evidence that welfare reform more broadly, although not specifically time limits, affected marriage and cohabitation patterns (Moffitt et al. (2020); Bitler et al. (2004, 2006)). As such, the inclusion of one's number of children (not necessarily biological children) as a fixed effect in the regression specification can raise concern for endogeneity. With this in mind, I note that results are robust to having only state-year, rather than state-year-number of children fixed effects in the regression specification (see Table 2). Furthermore, Moffitt et al. (2020) find that strict time limits (< 60 months) did not affect family structure. The effects they find are driven by other welfare reform components.

[30] These two differ for less than 2% of individuals in the sample - those who moved a state during the sample period - and for less than 2% of all observations, as this distinction is only relevant for a subset of each of these individuals' observations (i.e., after they move).

where y_{isrtn} is the outcome of interest for individual i residing in state s when time limits were introduced and observed in state r with n children under 18 in the household in year t. The regression controls for individual and state-year-number of children fixed effects with γ_i and θ_{rtn}, respectively. The regression controls for trends after the introduction of time limits through the inclusion of $After_{st}$. This variable is not collinear with θ_{rtn} because for the latter the state is one's contemporaneous state of residency, whereas for the former it is the state of residence when experiencing the introduction of time limits. 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 limit, and this is through the inclusion of the variable $PostTimeLimit_{it}$.[31, 32] 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_{isrtn} in response to a time-limit introduction 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

[31]For example, Mississippi introduced a 60-month time limit in 1996. Thus, all males in Mississippi with a youngest child aged less than 13 in 1996 are treated and their $Treatment * Afterist$ indicator is equal to 1 beginning in 1996 and onwards in the sample. In Texas, on the other hand, a 60-month time limit was introduced in 2000, four years after the time-limit introduction in Mississippi. To account for trends among males with the same age of children as the treated males in Mississippi, the $PostTImeLimit_{it}$ variable is equal to one for all males residing in Texas (and every other state) in 1996 with a youngest child under 13. This is despite these males not yet being affected by the introduction of time limits in their state of residence, Texas.

Note that this implies that $PostTimeLimit_{it}$ is equal one for nearly all observations with a value of one for $Treatment * After_{ist}$ (1% of observations with a value of one for $Treatment * After_{ist}$ do not get a value of one for $PostTimeLimit_{it}$, and these all belong to individuals who moved after they experienced an introduction to time limits). However, it also equals one for many observations that have a value of zero for $Treatment * After_{ist}$ (28.5% of observations with a zero value for $Treatment * After_{ist}$ have a value of one for $PostTimeLimit_{it}$). These are split roughly half between observations that have a value of one and zero for the treatment indicator (at the individual level).

[32]The inclusion of $PostTimeLimit_{it}$ did not affect the results substantially (not presented).

based on one's age of youngest child upon time-limit introduction, I avoid issues arising from endogenously depleted stocks of welfare eligibility years, as discussed in Chan (2018) and Mazzolari (2007).

Some of the results presented are from an event-study analysis that uses the following variation of equation 1:

$$\begin{aligned} y_{isrtn} &= \beta_0 + \sum_{j=-4, j\neq 0}^{4} \beta_1^j Treatment * After_{ist}^j + \sum_{j=-4, j\neq 0}^{4} \beta_2^j After_{st}^j \\ &\quad + \beta_3 PostTimeLimit_{it} + \gamma_i + \theta_{rtn} + \epsilon_{irtn} \end{aligned} \tag{2}$$

Equation 2 includes pre- and post-treatment indicator variables for three-year periods. This produces eight coefficients of interest - β_1^j: 4 coefficients for before the time limit is introduced - 12+ years; 9-11 years; 6-8 years, and 3-5 years pre-treatment, and 4 coefficients for after the time limit is introduced - 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_{isrtn} in response to a reduction in the number of years of welfare payment eligibility in the relevant period relative to the excluded period. The regression controls for post-time limit trends for individuals (regardless of whether they are treated or not) through the inclusion of indicator variables for each post-treatment period with the β_2 estimates.[33] Null-valued pre-treatment β_1 coefficient estimates in the event-study analysis can confirm the validity of the identification strategy by establishing a lack of pre-trends in the outcomes of interest among treated relative to control individuals.

With its three sources of variation across states, years, and within states based on age of youngest child upon time-limit introduction, equations 1 and 2 can resemble a triple differences specification. The inclusion of the variable $PostTimeLimit_{it}$ further ensures that all two-way interaction terms are accounted for.[34] However, equation 1 is not a standard

[33]Once again the $After_{st}^j$ indicators are not collinear with the state-year-number of children fixed effects, as already explained for equation 1, and this is due to the distinction between one's contemporaneous state of residence and one's state of residence upon time-limit introduction.

[34]The $Treatment_i$ indicator is like a two-way interaction term between being observed in a state that

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.[35]

5.3 Identification

The state-year fixed effects in the regression specification should alleviate concern that results are driven by other policy measures. Nonetheless, 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 JOBS (Job Opportunities and Basic Skills) exemptions. Work-related waivers, including work requirements, were the other major welfare reform component, besides time limits. However, their timing did not vary substantially across states (Moffitt et al. (2020)), so I do not evaluate correlations for them.

Figure 4A 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 JOBS exemptions at the state level.[36] The correlations are in fact negative, indicating that if there was any bias, it would actually be downwards, as state-level EITC supplements/JOBS exemptions were implemented earlier (later) in states that introduced time limits later (ear-

introduced time limits (although all states eventually introduced time limits during the sample period) and having a youngest child when time limits were introduced that is below the age threshold for being affected by the time limit. This treatment indicator is at the individual level and is therefore collinear with the individual fixed effects in equation 1. The $PostTimeLimit_{it}$ variable could represent the two-way interaction term between the age-of-youngest-child threshold and the various post indicators.

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

[36] State-level EITC introductions are from Leigh (2010) and supplemented for more recent years with the data used in Michelmore and Pilkauskas (2021). JOBS exemptions 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.

lier). In addition, Section 7.2 presents results of regressions with separate state and year fixed effects that allow me to control for the state-level EITC rate. The results are consistent with the main findings and do not vary between specifications with or without the EITC rate.

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 4B 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.[37] The results exhibit no such correlation.

Appendix Figure A2 and Appendix Figure A3 present the same analysis as Figure 4, with the former for time-limit lengths upon introduction, rather than time-limit introduction years and the latter difference between 1980 characteristics and those of Figure 4B on the vertical axis. The results exhibit no correlations. For Appendix Figure A2, this confirms the

[37]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 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).

exogeneity of the within-state variation in the empirical strategy.

6 Results

6.1 Event Study Results for Male Labor Market Outcomes

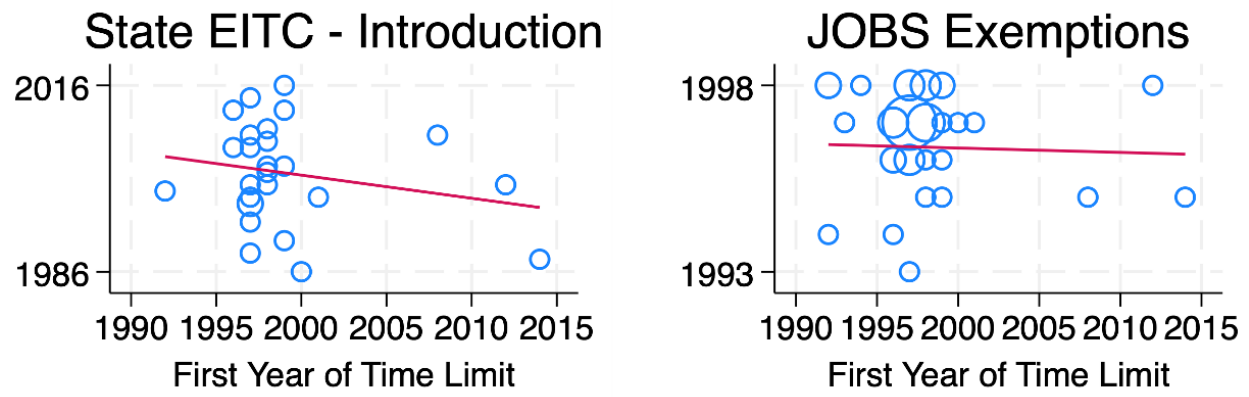
I first present event-study results from equation 2 for male labor-market outcomes - employment, annual hours worked, and annual labor income. Figure 5 plots the eight coefficient estimates of interest for each dependent variable, along with their 95 percent confidence intervals. The findings clearly 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 2-3.6 percentage points for employment, 112-166 annual hours, and 2,467-3,454 2010 USD for annual labor income. These represent increases that are 2.4-4.6, 5.8-8.6, and 8.1-11.4 percent of pre-treatment means of employment, hours worked, and labor income, respectively. While the effects observed in Figure 5 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 existing literature.

Figure 5 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.

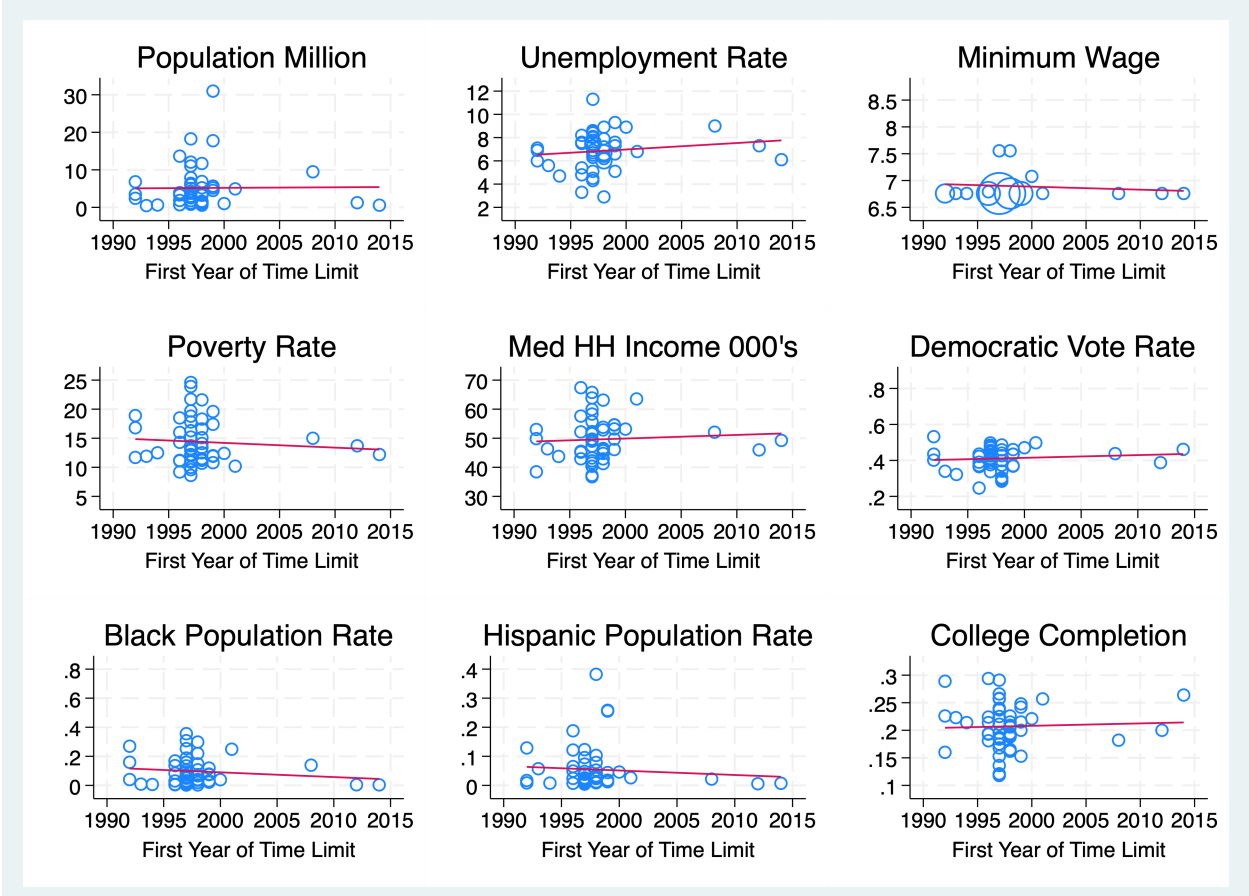
To increase the transparency of the estimates of equation 2, I show results from an event study model that is simpler and has only a single period of time-limit introductions: 1997-1998, i.e., the sample is limited to the 28 states that introduced time limits in 1997-1998. This analysis compares male labor market outcomes before and after the introduction of time limits in a single period and between treatment and control groups, thereby omitting the staggered component of the regression framework in our baseline specification and resulting

Figure 4: 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 exempt (JOBS) and the years time limits were introduced. The fitted linear regression line is indicated in red. Only 26 states adopted EITC supplements (excluding Iowa and Washington, D.C. that are not in the sample). 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.

in a classic two-by-two DID regression model. Results are presented in Appendix Figure A5 and are consistent with the results in Figure 5.

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. I show the single coefficient estimate from equation 1 so that the magnitude of the effects can be compared. 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.[38] Labor-market outcomes are identical to those already analyzed in Figure 5 (employment, hours worked and labor income).

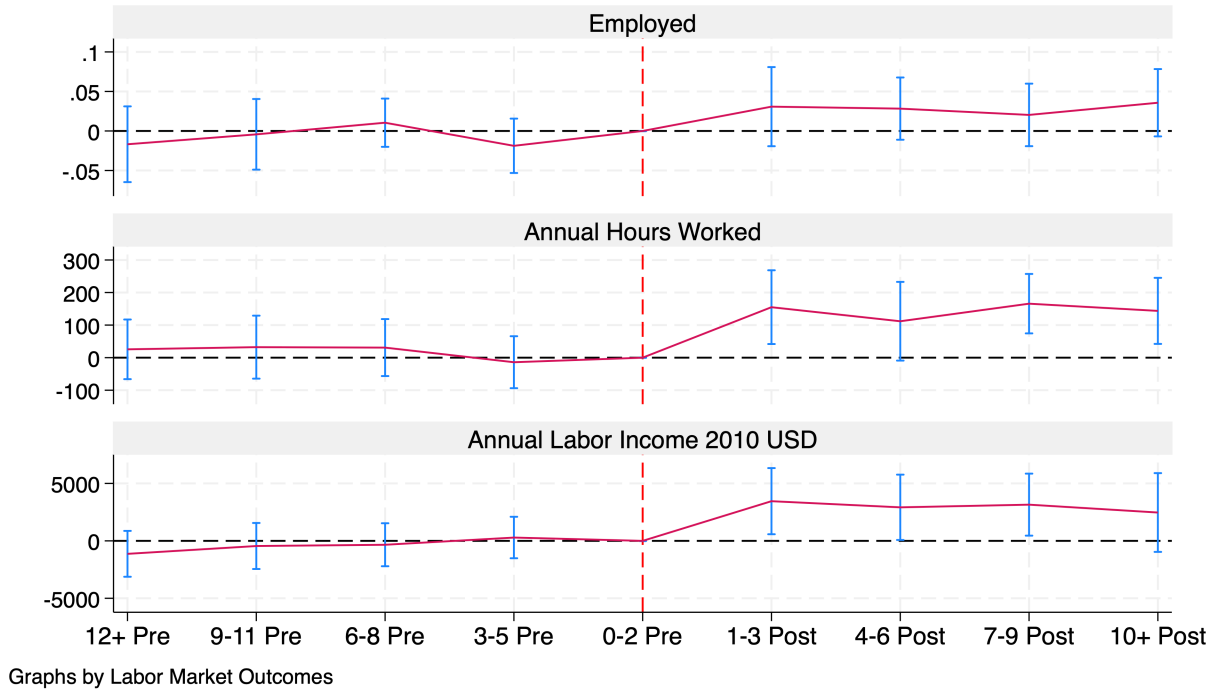
The first column of each dependent variable presents results with only state-year fixed effects, whereas the second column converts these to state-year-number of children fixed effects. As can be seen, most of the coefficient estimates do not vary dramatically with the change in fixed effects, although the magnitude of the effect for annual labor income declines.

The top panel of Table 2 shows that welfare-payment receipt by either the male or his female partner declined by 2.59 percentage points in response to the implementation of time limits. The last four columns in the top panel reveal that a larger share of this decline is attributed to the female partner's loss of welfare benefits rather than to the male loss. An event-study analysis in Appendix Figure A4 confirms the declines in welfare payment receipt observed in Table 2.

The magnitude of the increase in employment is much larger than the reduction in men's welfare payments receipts. This implies that men could also be increasing their employment in response to their partner's loss of welfare payments. Still, the increase in employment is somewhat larger than the decrease in overall welfare payments (to either the male or his female partner). This further suggests that males may be responding to something

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

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



Notes: The figure shows event study coefficient estimates from equation 2. The values on the horizontal axis are the number of years relative to the introduction of time limits. The vertical lines represent 95 percent confidence intervals. Standard errors are clustered at the state level. Sample sizes range from 39,739 to 49,527 observations covering 4,419 to 5,726 males. Pre-treatment means are 0.785, 1,930, and 30,415 for employed, annual hours worked, and annual labor income, respectively.

beyond contemporaneously lost income. It is possible that some of the employment response is related to increased uncertainty about future income, or that time limits also induced changes in marriage/cohabitation patterns that changed men's labor market incentives. It is not in the scope of this paper to distinguish which of these mechanisms may be at work here. Nevertheless, mechanisms such as these 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 bottom panel of Table 2 can at first glance suggest that the hourly wage rate for the increased hours worked is set at over 23 2010 USD.[39] This is relatively 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, columns (1)-(2) in Table 3 suggest increased hourly wages among treated males following the introduction of time limits. It turns out that a mean increase of 1.05 2010 USD in the hourly wage rate is sufficient to fully cover the excess annual labor income increase in Table 2, beyond the increase due to greater annual hours worked.[40]

The underlying mechanisms that can be driving increases in hourly wages are addressed in the bottom panel of Table 3 by examining job tenure and union coverage following time-limit introduction. The results suggest an increase in these measures. Increases in job tenure have been shown to increase wage rates for low-skilled workers (Dustmann and Meghir (2005); Caplin et al. (2022)). Greater union coverage indicates that the increased hourly wages could be due to higher quality jobs. The last column in the bottom panel of Table 3 tests whether self-employment increased following the introduction of time limits. Changes in self-employment would not necessarily be expected to be a first-order mechanism for wage increases in response to time-limit introduction. In fact, an increase in self-employment in

[39]This is 2,929 2010 USD divided by 122.5 hours.

[40]If I multiply the change in annual hours from Table 2 by 7.25 2010 USD, which was the federal minimum wage in 2010, I get an increase in annual income of 888 2010 USD. This leaves 2,041 2010 USD that are not accounted for from the 2,929 2010 USD increase observed in Table 2. Dividing this by 1,930, the pre-treatment mean number of annual hours worked in the sample, results in an hourly wage increase of 1.05 2010 USD that is able to cover the remaining increase in annual labor income.

response to time limits could be counter-intuitive, as a decline in the social safety net can entail a reduction in the risk-taking which is associated with self-employment. Accordingly, Table 3 rules out changes in self-employment in response to time-limit introduction.

Table 3 also 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 5 and Table 2 for the employment outcome. Thus, in addition to estimating the change in hourly wages following time limits in columns (1)-(2) of the top panel, columns (3) and (4) in the same panel 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 fewer hours on average to join the labor market after time-limit implementation. Despite this downward bias, columns (3) and (4) in the top panel of Table 3 suggest an increase in annual hours and labor income - the coefficient estimates are in the right direction and of an expected magnitude after considering the potential for a downward bias. This supports the notion that hours and income increases were not just driven by individuals joining the labor market at the extensive margin. These labor income results could lend further support to evidence of increases in hourly wage rates.

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)).[41] This analysis further exploits the variation generated within state-year units and across treated indi-

[41] 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 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 children affected by reform)	-0.0261***	-0.0259***	-0.00733*	-0.00990**	-0.0269***	-0.0258***
	(0.00536)	(0.00593)	(0.00371)	(0.00429)	(0.00466)	(0.00529)
Observations	42,567	41,448	42,563	41,443	41,510	40,466
Number of Individuals	4,744	4,684	4,744	4,684	4,744	4,682
R-squared	0.399	0.484	0.328	0.434	0.375	0.468
Pre-Treatment Mean Dep. Var.	0.0345	0.0340	0.0199	0.0194	0.0278	0.0273
	Labor Market Outcomes					
	Employed		Annual Hours Worked		Ann. Labor Inc. (2010 USD)	
Post*Treatment (with children affected by reform)	0.0335***	0.0321**	113.3***	122.5***	4,421***	2,929**
	(0.0117)	(0.0135)	(33.23)	(36.99)	(1,301)	(1,379)
Observations	50,525	49,527	40,861	39,744	40,857	39,739
Number of Individuals	5,762	5,726	4,458	4,418	4,459	4,419
R-squared	0.465	0.519	0.544	0.578	0.625	0.662
Pre-Treatment Mean Dep. Var.	0.786	0.785	1,929	1,930	30,371	30,415
Individual Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
State-Year Fixed Effects	Yes	X	Yes	X	Yes	X
State-Year-Num Children 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 fixed effects 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. Numbers in parentheses are standard errors clustered at the state level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 3: Additional Labor Market Outcomes following Time-Limit Introduction - Intensive Margin, Hourly Wages, and Mechanisms for Hourly Wage Changes

	Hourly Wage	Hourly Wage Winsorized	Annual Hours Conditional on Employment	Annual Lab. Inc. Conditional on Employment
Post*Treatment (with child affected by reform)	1.465	1.057**	47.44*	1,516
	(1.435)	(0.524)	(28.01)	(1,610)
Observations	39,693	39,278	32,880	32,883
Number of Individuals	4,416	4,405	3,960	3,960
R-squared	0.324	0.628	0.494	0.660
Pre-Treatment Mean Dep. Var.	15.11	14.36	2,136	34,339

	Tenure w/ Employer $\geq$ 1 Year	Tenure w/ Employer $\geq$ 3 Years	Job w/ Union Union Coverage	Self-Employed
Post*Treatment (with child affected by reform)	0.0311*	0.0407**	0.0309**	0.00397
	(0.0182)	(0.0202)	(0.0124)	(0.0100)
Observations	49,243	49,243	49,527	49,527
Number of Individuals	5,711	5,711	5,726	5,726
R-squared	0.509	0.521	0.644	0.575
Pre-Treatment Mean Dep. Var.	0.600	0.469	0.183	0.0901
Individual Fixed Effects	Yes	Yes	Yes	Yes
State-Year-Num Child F.E.	Yes	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). In column (2) in the top panel, the sample excludes the top 1% of hourly wage values. In columns (3) and (4) in the top panel, observations have non-zero employment. Hourly wage is calculated as the reported annual labor income (in 2010 USD) divided by the reported number of annual hours. Numbers in parentheses are standard errors clustered at the state level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

viduals 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 9, which is the mean number of years of welfare-payment receipt lost among the treated observations after time-limit introduction. These calculated effects are 46-77 percent of those estimated 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 9 years of potential benefits lost, the median is 9.6. 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.

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 does two things: it confirms that my sample criteria and regression specification are aligned with past studies on the effects of time limits, and it enables me to compare female welfare take-up effects to their employment effects, as I do for males in Table 2.[42] I run the regression specified in equation 1 on the sample of females,

[42]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.

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)	-0.00129 (0.000821)	-1.91e-05 (0.000655)	-0.00199*** (0.000639)
Observations	41,448	41,443	40,466
Number of Individuals	4,684	4,684	4,682
R-squared	0.483	0.434	0.468
Pre-Treatment Mean Dependent Variable	0.0340	0.0194	0.0273
	Labor Market Outcomes		
	Employed	Annual Hours Worked	Annual Labor Income (2010 USD)
Post*Treatment Intensity (number of potential benefit years lost)	0.00275*** (0.000923)	6.216** (2.418)	214.6 (144.8)
Observations	49,527	39,744	39,739
Number of Individuals	5,726	4,418	4,419
R-squared	0.519	0.578	0.662
Pre-Treatment Mean Dependent Variable	0.785	1,930	30,415
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 parentheses are standard errors clustered at the state level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

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 2.[43] 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 Appendix Figure A6. The event study results further suggest that the effects observed in Table 5 are long term (more than 3 years after time-limit introduction) rather than short term.

The declines in female welfare payment receipt in Table 5 are 4.4-6.4 percentage points, representing 37-55 percent of the pre-treatment mean.[44] These estimated effects are not dramatically different from past estimated female 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. 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 magnitude 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

(2008); Grogger (2003); Keane and Wolpin (2010); Low et al. (2020); Mazzolari and Ragusa (2012); Pepin (2022); Swann (2005)).

[43]Appendix Table A2 provides Summary Statistics for the female sample.

[44]To calculate the mean effect for the intensity of treatment coefficient estimates, I multiply them by 8.76, which is the mean number of years of potential benefits lost upon time-limit introduction among treated female observations after time-limit introduction.

evaluate the effect of time limits only several years after their introduction, such that the declines found in welfare take-up are 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 anticipated that the actual effect of time limits over longer time horizons, as in this study, would be greater.

Employment increases in Table 5 are 7.15 and 3.6 percentage points for the indicator variable and intensity-of-treatment specifications, respectively. These represent 12 and 6 percent of the pre-treatment mean. Past studies evaluating female employment responses to time limits have shown some variation in the magnitude of the effects. 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 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. This can be explained by the event study results in Appendix Figure A6 which show that labor income effects are only observed more than seven years after the introduction of time limits. Thus, positive female labor income effects are 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.

It is worth noting in the context of this study that while the male results in Table 2 show increased employment that cannot be driven solely by a decline in males' own welfare payment receipt, for females this is not necessarily the case. The female increase in employment in Table 5 for the indicator variable is very close to the decline in welfare receipt and for the intensity of treatment analysis it is larger. Grogger (2003) and Mazzolari and Ragusa (2012) also observe gaps between female employment and welfare receipt, while finding that household income did not decline in response to time limits. Both studies raise 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

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

	Welfare Payment Receipt					
	Male or Female		Female		Male Partner	
Post*Treatment (w/ child affected by reform)	-0.0646***		-0.0641***		-0.0194**	
	(0.0149)		(0.0144)		(0.00920)	
Post*Treat Intense (# of potential benefit yrs lost)		-0.00495***		-0.00500***		-0.00230***
		(0.00122)		(0.00115)		(0.000678)
Observations	48,209	48,209	48,209	48,209	48,209	48,209
Number of Individuals	4,578	4,578	4,578	4,578	4,578	4,578
Pre-Trt Mean Dep. Var.	0.119	0.119	0.116	0.116	0.0565	0.0565
	Labor Market Outcomes					
	Employed		Annual Hours Worked		Ann. Labor Inc. (2010 USD)	
Post*Treatment (w/ child affected by reform)	0.0715***		187.3***		1,324	
	(0.0189)		(40.18)		(1,081)	
Post*Treat Intense (# of potential benefit yrs lost)		0.00412**		9.063**		-77.37
		(0.00157)		(3.536)		(75.90)
Observations	57,444	57,444	47,397	47,397	43,118	43,118
Number of Individuals	5,616	5,616	4,489	4,489	4,367	4,367
Pre-Trt Mean Dep. Var.	0.598	0.598	1,166	1,166	13,385	13,385
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 parentheses are standard errors clustered at the state level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

due to biased unit effects and time trends.

To respond 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:

$$y_{irtn} = \beta_0 + \gamma_i + \theta_{rtn} + \epsilon_{irt} \tag{3}$$

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

$$\begin{aligned} y_{isrtn} = \beta_0 &+ \sum_{j=-4, j\neq 0}^{4} \beta_1^j Treatment * After_{ist}^j + \sum_{j=-4, j\neq 0}^{4} \beta_2^j After_{st}^j \\ &+ \beta_3 PostTimeLimit_{it} + \hat{\gamma}_i + \hat{\theta_{rtn}} + \epsilon_{irtn} \end{aligned} \tag{4}$$

where all the variables are as in equation 2, and parameters $\hat{\gamma}_i$, $\hat{\theta_{rtn}}$ are as estimated in equation 3.[45] 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:[46] 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

[45] 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.

[46] 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.

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.[47] The results are remarkably consistent with the main results presented in Figure 5, 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.

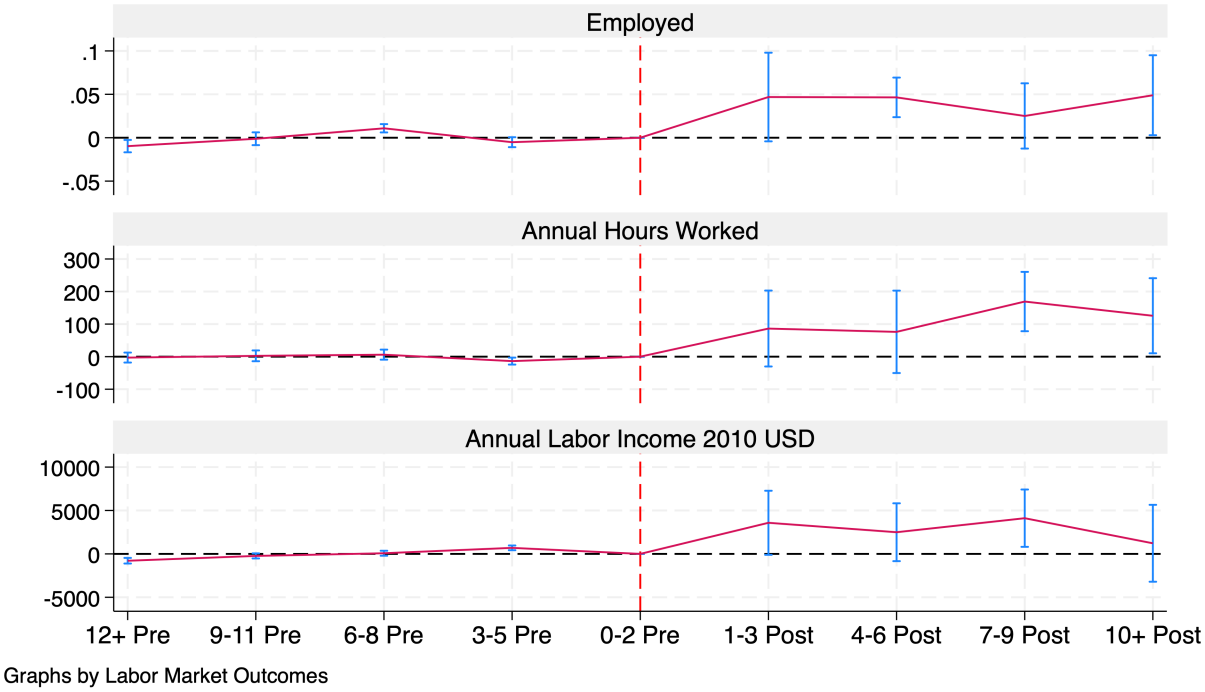
6.6 Welfare Implications

Examining overall welfare implications of welfare payment time limits is challenging, primarily because time limits have been shown to have additional effects, besides those on welfare take-up or labor market outcomes, and it is difficult to fully account for all of this in an analysis of welfare implications.[48] Nevertheless, given the results presented in this study, I attempt to evaluate some welfare implications due to time-limit introduction from two perspectives. First, I position the estimated increase in male employment within the context of recent declines in unskilled prime-age males' labor force participation rates (Abraham and Kearney (2020); Blank (2009); Holzer and Offner (2006); Krueger (2017); Moffitt et al. (2012)). Second, I calculate the changes to the Marginal Value of Public Funds (MVPF)

[47] 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.

[48] Adverse effects of time limits have been documented for health-related measures, such as birth outcomes (Leonard and Mas (2008)) or mortality (Lichtman-Sadot and Slusky (2024)). Furthermore, there is evidence that welfare reform more broadly affected other measures of well-being (both positively and negatively). The results of this literature are mixed. Some positive effects include evidence of decreased female and youth crime rates (Corman et al. (2014, 2017)), reduced food insecurity and welfare dependency and greater educational attainment among children exposed to welfare reform (Corman et al. (2014); Hartley et al. (2022); Vaughn (2018); Corman et al. (2022)). Adverse effects include reductions in maternal educational attainment (Corman et al. (2014)), decreased parent-child engagement and parental time devoted to child health care (Baltagi and Yen (2016); Reichman et al. (2020)), increased adolescent delinquency and risky behavior (Dave et al. (2021)), and reduced health insurance coverage and utilization (Bitler et al. (2005); Kaestner and Kaushal (2003)), which may have also driven worse prenatal care and birth outcomes for infants of mothers exposed to the reform (Kaestner and Chan Lee (2005)).

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 39,739 to 49,527 observations covering 4,418 to 5,726 males.

that result from removing time limits on welfare payments. The MVPF of removing welfare payment time limits has already been calculated in Hendren and Sprung-Keyser (2020), while only considering single mothers as those affected by the policy.

Abraham and Kearney (2020) provide detailed statistics on employment-to-population ratios for the period 1999-2018 by sex, age group, and education level. Taking these statistics, my own analysis suggests that the employment-to-population ratio among males aged 25-54 with a high school education or less declined by 4.1 percentage points during 1999-2018.[49] This calculation represents all males within this age group and education level, rather than males who had a minor present in their household when time limits were introduced in their state of residence. Nevertheless, for the purpose of this analysis, I make the (very) rough assumption that this represents an approximate decline in employment for this male population as well. As such, the 3.2 increase in employment in response to time-limit introduction (Table 2) suggests that absent time limits, the decline in the employment-to-population ratio during 1999-2018 among low-skilled prime-age males who reside with minors in their household may have been 75% greater.

The Hendren and Sprung-Keyser (2020) calculation for the MVPF of removing time limits on welfare payments does not account for additional effects of time limits, beyond single mothers' changes in welfare take-up. Using findings from Grogger (2003) and Pavetti (1995), they calculate a MVPF of 0.81 for the removal of time limits - for every dollar spent on welfare payments due to the removal of time limits, the return is 81 cents.

Accounting for the positive male labor market effects due to the introduction of time limits would imply a higher cost for removing time limits, thereby decreasing the MVPF value that is found in Hendren and Sprung-Keyser (2020). I present the calculations for this in Appendix Sub-Section .6. It is worth noting that while the new MVPF is lower than that of Hendren and Sprung-Keyser (2020), the difference is not very dramatic. This is due

[49]This calculation takes the changes in employment-to-population ratios presented in Table 1B in Abraham and Kearney (2020) for each relevant age group and education level and weighs them by the relevant population share. The relevant population share is calculated as the mean between 1999 and 2018.

to two factors. First, the Hendren and Sprung-Keyser (2020) MVPF was already relatively low with a value lower than one, thus demonstrating the potential for economic inefficiencies from the removal of time limits. Second, the change in tax liabilities due to increased male labor market outcomes is small in comparison to welfare payments, and as such, integrating that into the calculation does not affect the MVPF to a very large extent. Accordingly, my calculations suggest that a MVPF for the removal of time limits that accounts for the male response to this policy would be 0.73 rather than 0.81.

7 Robustness Checks

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

Table 6 presents results for different samples of the data to address potential challenges to 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, I do not expect time limits to have an effect. 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. One would not expect these males to be affected by time limits and 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 compara-

ble 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 youngest child aged 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. However, this is at the cost of reducing substantially the sample size. The results confirm improvements in labor-market outcomes. The labor income effect is noisier than the corresponding estimate in Table 2 with a p-value of 0.28.

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, although the labor income effect is only marginally statistically significant with a p-value of 0.11.

7.2 Controlling for State EITC Rate and Economic Conditions in the Regression Analysis

Two of the most substantial identification threats when evaluating the effect of policies enacted in the 1990s on labor market outcomes are the EITC, which was significantly expanded during this period, and the economic upturn of this period, which research has shown can confound estimates (Kleven (2019)). The inclusion of state-year fixed effects in the regression specifications should alleviate these identification concerns to a large extent. Furthermore, as discussed in Section 5.3, 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

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)	-0.00980	34.25	13,960
	(0.0135)	(34.01)	(14,529)
Observations	25,304	22,500	22,500
Number of Individuals	2,209	1,969	1,969
R-squared	0.453	0.492	0.510
Pre-Treatment Mean Dependent Variable	0.903	2,247	63,841
	Fathers to Teenagers upon Time-Limit Introduction		
Post*Treatment (with children affected by reform)	0.0443	142.6**	2,826
	(0.0384)	(56.90)	(2,575)
Observations	10,272	9,360	9,361
Number of Individuals	1,090	961	962
R-squared	0.580	0.618	0.715
Pre-Treatment Mean Dependent Variable	0.816	1,941	33,129
	Years Through 2006		
Post*Treatment (with children affected by reform)	0.0281*	106.3**	2,285
	(0.0145)	(41.51)	(1,421)
Observations	43,252	34,320	34,315
Number of Individuals	5,277	4,014	4,015
R-squared	0.525	0.585	0.673
Pre-Treatment Mean Dependent Variable	0.788	1,937	30,417
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 parentheses are standard errors clustered at the state level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

the inclusion of state-year-number of children under 18 fixed effects in the regression specification.

To further establish that the EITC or various economic conditions are not driving the observed responses to time limits, in Table 7, I show regression results that control for the state-level EITC rate or local economic measures, such as unemployment or GDP per capita.[50] The unemployment rate and GDP per capita are intended to proxy for local time-varying economic conditions. Local unemployment rates have already been used for this purpose in past studies (Kleven (2019)). The regression specification is the same as that presented in equation 1, with the important exception that the state-year-number of children under 18 fixed effects are replaced with separate year and state-number of children under 18 fixed effects. With the state and year fixed effects separated, the state-level EITC rate and economic measures can be controlled for in the regression specification. For each dependent variable, Table 7 presents results with only the EITC rate in the first column, followed by the additional inclusion of the unemployment rate and state real GDP per capita in the second column.

As can be seen, the results in each of the columns are maintained and demonstrate an improvement in labor market outcomes in response to the introduction of time limits.[51] Thus, the results are consistent with the notion that neither EITC nor economic conditions are driving the results.

Table 7 presents the expected coefficient signs on the measures of economic activity - they improve labor market outcomes. However, interestingly, the state-level EITC rate does not affect male labor market outcomes with precisely estimated null coefficient estimates. This is consistent with past studies that have found either very small or non-significant effects of the EITC on men (Agan and Makowsky (2018); Eissa and Hoynes (2004); Hoffman and

[50]State EITC rates were obtained from Leigh (2010) and supplemented for more recent years with the data used in Michelmore and Pilkauskas (2021). Unemployment rates are available from the Bureau of Labor Statistics. State GDP per capita is available from the Bureau of Economic Analysis.

[51]One can compare the coefficients in Table 7 to the corresponding coefficients in regression specifications that do not control for the EITC and/or economic conditions by referring to the last column in Appendix Table A1. The coefficient estimates are highly similar.

Seidman (2003)).

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 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 outcomes in response to time-limit introduction.[52, 53]

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

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

[53] 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 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.

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

	Employed		Annual Hours Worked		Annual Labor Income (2010 USD)	
Post*Treatment (w/ child affected by reform)	0.0403***	0.0399***	115.9***	113.3***	4,165***	4,137***
	(0.0109)	(0.0108)	(37.47)	(36.22)	(1,295)	(1,293)
State EITC Rate	0.000888	0.000257	0.653	-0.206	16.23	-53.17
	(0.000858)	(0.000720)	(1.808)	(1.868)	(58.54)	(55.52)
State Unemployment Rate		-0.00936***		-29.43***		-483.2***
		(0.00280)		(6.945)		(157.6)
State Real GDP per capita (000's)		0.00358***		5.592		395.2***
		(0.00127)		(3.983)		(126.4)
Observations	50,572	50,572	40,926	40,926	40,922	40,922
Number of Individuals	5,762	5,762	4,459	4,459	4,460	4,460
R-squared	0.467	0.468	0.533	0.534	0.618	0.619
Pre-Treat Mean Dep. Var.	0.786	0.786	1,929	1,929	30,400	30,400
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

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 the state-level EITC rate or measures of economic conditions are controlled for, as indicated for each column. Numbers in parentheses are standard errors clustered at the state level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

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 who experienced the introduction of time limits before 2002.[54]

I select a range of times or years that individuals appear in the data, such that a 4-5-unit range maximizes the number of males. This results in confining the sample to males appearing either 12-15 years or 6-11 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 6-11 times 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 substantial reductions in statistical significance.

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 research 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 show that male partners alleviated some of the negative shock to household income due to decreased welfare benefits by increasing their labor-market outcomes. This is despite them 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 only a partial understanding of the overall economic and social implications of welfare reform. However, the implications of this paper on policy evaluations go beyond welfare reform, by highlighting the value of evaluating policy measures or shocks experienced within the

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

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)	0.0236	82.25	565.7
	(0.0208)	(54.32)	(1,668)
Observations	17,271	13,762	13,762
Number of Individuals	1,195	994	994
R-squared	0.504	0.550	0.625
Pre-Treatment Mean Dependent Variable	0.809	1,997	30,759
	Observed 6-11 Times Post-Treatment		
Post*Treatment (with children affected by reform)	0.0527	214.5*	2,564
	(0.0357)	(114.7)	(1,956)
Observations	12,156	10,189	10,188
Number of Individuals	1,571	1,344	1,344
R-squared	0.601	0.663	0.737
Pre-Treatment Mean Dependent Variable	0.741	1,857	27,226
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 parentheses are standard errors clustered at the state level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

household for all household members. My findings also identify 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.

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Appendices

.1 Sample Criteria

Section 4 outlines the sample criteria for the analysis. In what follows I provide further details on sample restrictions in this section. The main challenge when constructing the sample was assigning the proper state of residence for time-limit introductions to individuals. While for the majority of PSID respondents, this implies simply observing an individual's state of residence when time limits are introduced (or the year after time limit introduction when the PSID surveys individuals only every two years), for individuals who move states exactly during the years of time-limit introductions or disappear from the sample for some of the sample period, some judgement calls had to be made. The following outlines these judgement calls and provides details on the conditions for excluding observations from the sample:

1. Observations documented as residing in a U.S. territory or foreign country were dropped. This resulted in less than 0.5% of observations being dropped (before applying any conditions, except for the year-span condition of 1980-2016).

2. Individuals who are documented as residing in an "unknown" state for more than 8% of their observations were dropped. This resulted in dropping slightly more than 11% of observations (before applying any conditions). However, over 99% of those observations dropped had most of their other variables missing as well. This includes employment status and labor income (missing for more than 99% of those observations dropped under this condition).

3. When individuals were observed in two states in the year time limits were introduced (due to moving states), the earlier time-limit introduction was determined to be the relevant one.

4. Individuals who had a break exceeding five years in the PSID, *and* moved states during that break, *and* this break occurred during time-limit introduction in both the first

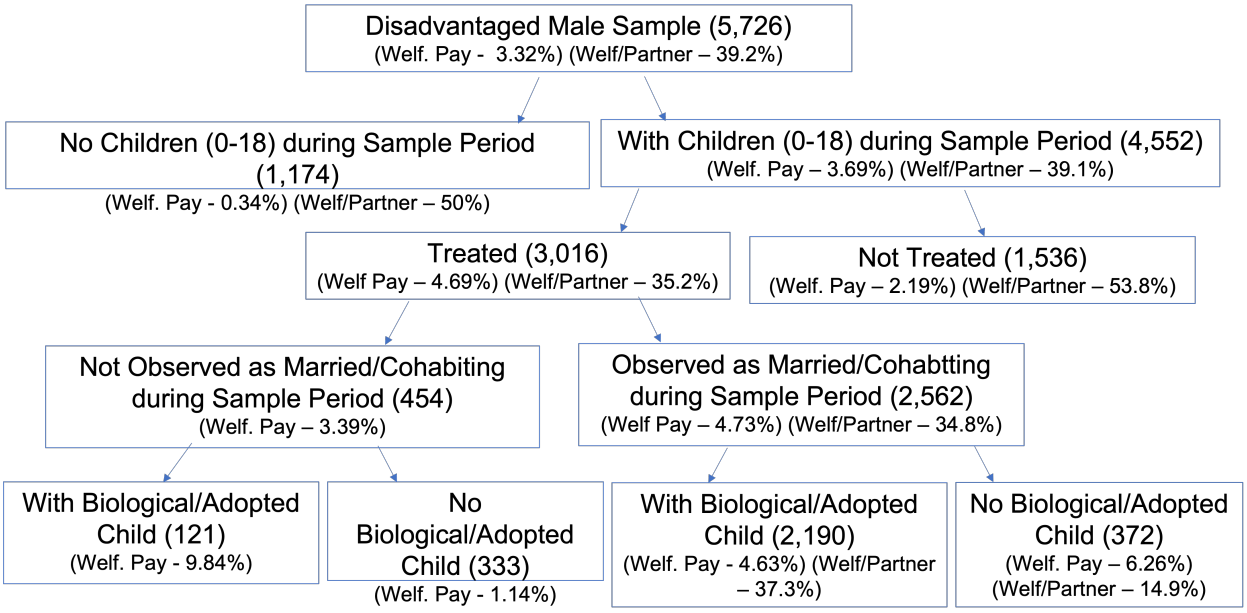
and second states they were observed residing in, *and* this break occurred between the ages of 19 and 55, which is the criterion for assigning time-limit introduction, were dropped. For individuals meeting all four of the above mentioned conditions, it was not possible to assign a state of residence when time limits were introduced. This resulted in dropping 9 individuals covering 91 observations (either males or females and no socioeconomic conditions applied on the sample yet).

5. Individuals whose earliest observation in the PSID appeared more than 5 years after their state of residence introduced a time limit were dropped, as it was difficult to assign that state of residence to them during time-limit introduction. This resulted in dropping 3 individuals covering 20 observations (either males or females and no socioeconomic conditions applied on the sample yet).

6. Individuals who were assigned to Washington, D.C. or Iowa when time limits were introduced were dropped. Recall that these two states are excluded from the analysis because time limits were canceled after introduction in the former and were differentially applied across individuals in the latter. This resulted in excluding slightly over 2% of the observations (without applying any additional criteria besides the year span of 1980-2016).

7. Seven observations belonged to individuals identified as low socioeconomic or with a high school education or less (the sample criteria - see Section 4 for details), but were observed with over 500,000 2010 USD in annual labor income. Browsing at these seven individuals' labor income patterns during the sample period seemed to indicate a coding error in the labor income variable. As a result, these 7 observations had annual labor income replaced to missing.

Appendix Figure A1: Disadvantage Male Sample Breakdown and Welfare Payment Statistics



Notes: The figure presents the breakdown for the disadvantage male sample used in this study (from the PSID). In each cell, the number in parenthesis is the number of individuals in the sample for that cell. The percent after "Welf. Pay" represents means for that cell (across all observations, so not at the individual level) of welfare payment receipt either for the male himself or his female partner prior to the introduction of time limits. The percent after "Welf/Partner" is the share of males observed receiving welfare payments themselves, conditional on their female partners receiving welfare payments (across all observations and not limited to the period before time limit introduction) for that cell. Males who are not observed married/cohabiting during the sample period also includes males who had missing values for the "married" variable in the PSID throughout the sample period.

.2 Additional Scatter Plots for Time-Limits and State-Level Characteristics/Policies

Appendix Figure A2 Appendix Figure A3 present similar correlation plots to those presented in Figure 4. In Appendix Figure A2 rather than having the year time limits were introduced on the horizontal axis, there are state time-limit lengths (in months) upon introduction. In Appendix Figure A3 the vertical axis is the difference in state-level characteristics between 1980 and 1990-1992. The results further strengthen the lack of correlation between state-level characteristics prior to time-limit introduction or state-level policy expansions.

.3 2X2 Difference-in-Differences Regression

To increase transparency of the estimates of equation 2, I show results from an event study model that is simpler and has only a single period of time-limit introductions: 1997-1998, i.e., the sample is limited to the 28 states that introduced time limits in 1997-1998. This analysis compares male labor market outcomes before and after the introduction of time limits in a single period and between treatment and control groups, thereby omitting the staggered component of the regression framework in our baseline specification and resulting in a classic two-by-two DID regression model.[55] This results in the following regression specification:

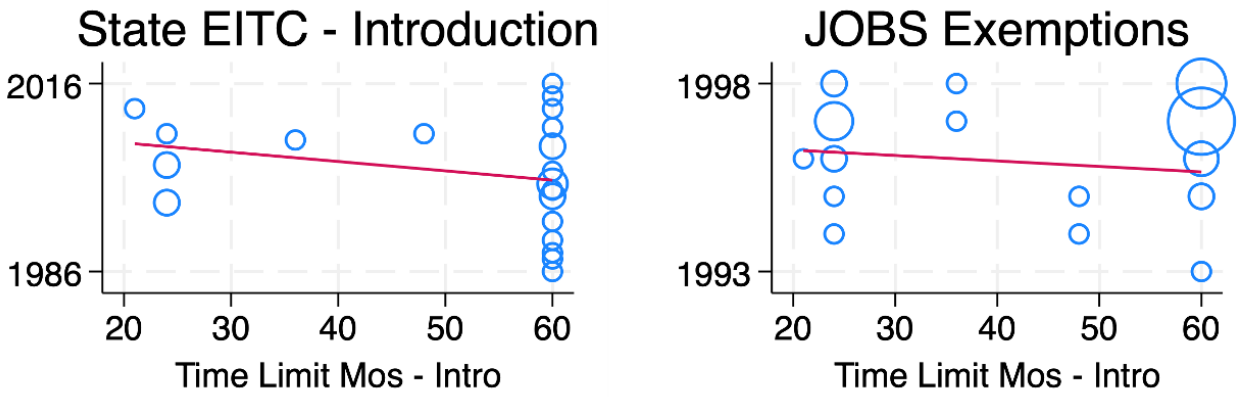
$$y_{isrtn} = \pi_0 + \sum_{j=-4, j\neq 0}^{4} \pi_1^j Treatment * After_{ist}^j + \tau_i + \mu_r + \eta_t + \psi_n + \epsilon_{ist} \qquad \text{(A1)}$$

Equation A1 is similar to equation 2 with a few exceptions. First, the fixed effects are separate for state, year, and number of children. While with state-year fixed effects the estimates were very much in line with the baseline results, the standard errors were

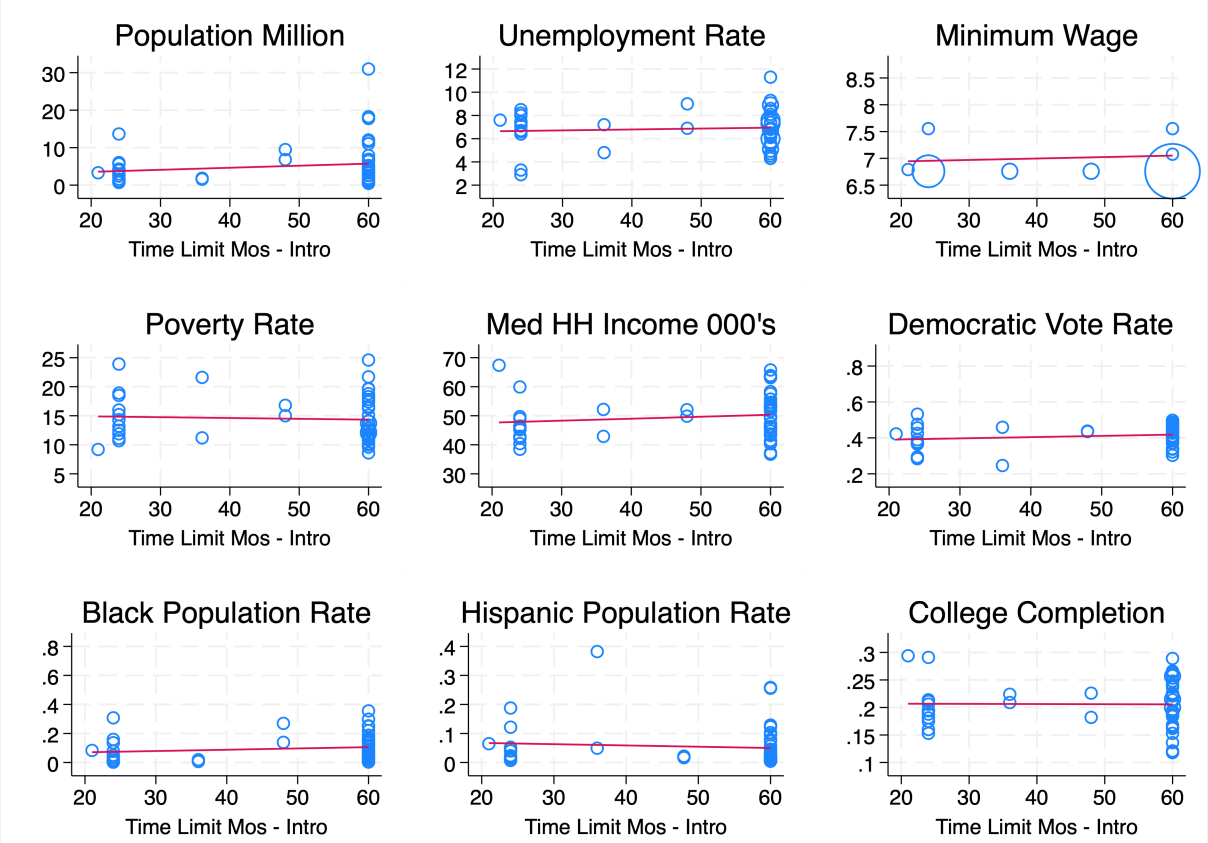
[55]The PSID became bi-annual in 1997, so the subsequent survey year was 1999. Recall that in each survey year, the dependent variable is defined for the previous year (see Section 4 for data construction details). Thus, defining the before and after periods as pre-1997 and 1998 onwards, respectively, does not entail omitting any survey years, despite the introduction of time limits spanning over two years in this sample.

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

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

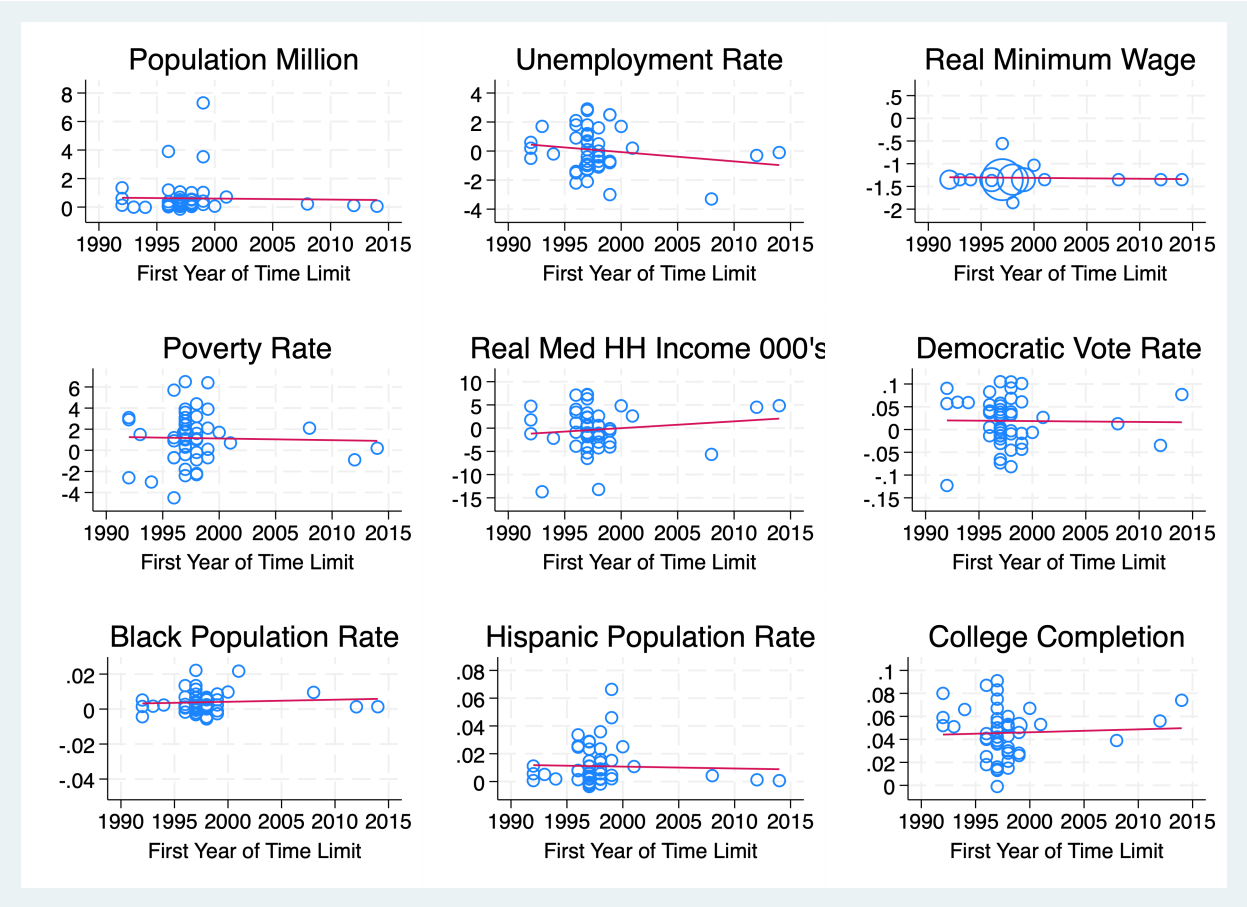


(B) Time-Limit Extent and 1990-1992 State-Level Characteristics



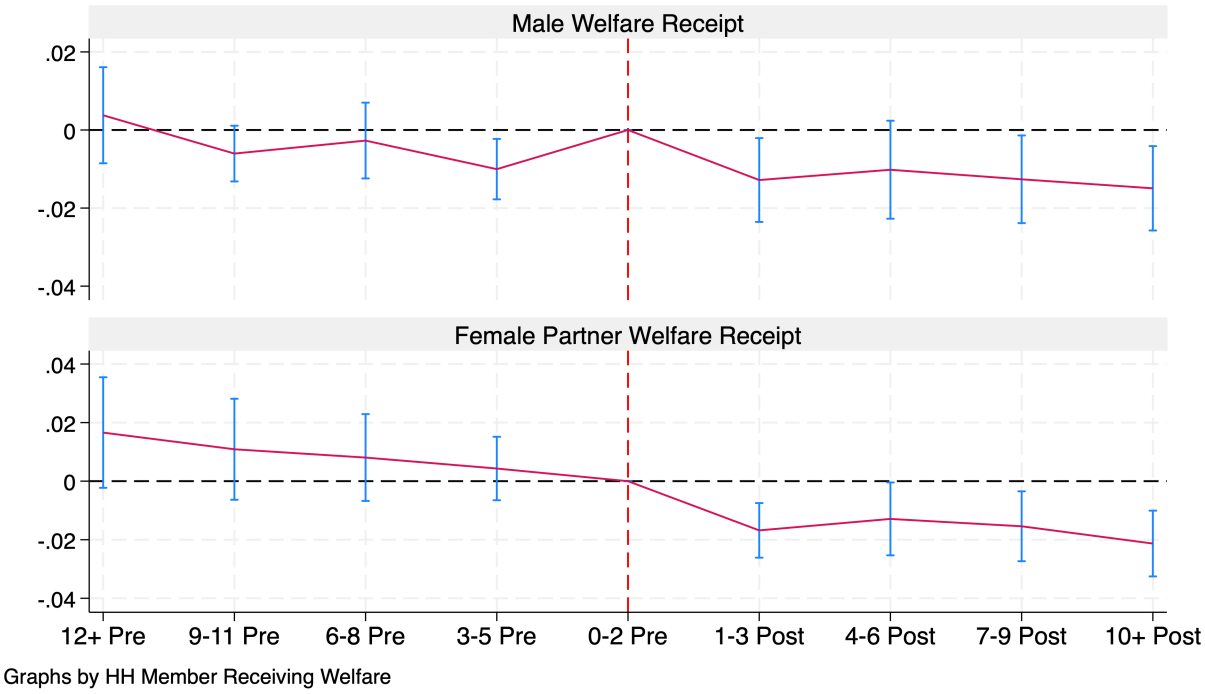
Notes: For data sources, see Section 5.3.
The top panel plots for each state the year the stated policy was introduced (state-level EITC) or exempt (JOBS) and the number of months for time limits upon their introduction.
The bottom panel plots for each state the value of the stated state-level characteristics 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 states.

Appendix Figure A3: First Time Limit and Differences over Time in State-Level Characteristics (1980 vs. 1990-1992)



Notes: The figure plots for each state the difference of the stated state-level characteristics between 1980 and 1990-1992 and the year time limits were introduced. The sources for the data are the same as those for 1990-1992 (see footnote 37).

Appendix Figure A4: 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 40,446 to 41,443 observations covering 4,682-4,684 males. Pre-treatment means are 0.0194 and 0.0273 for male and female partner receipt, respectively.

much larger. Second, standard errors in equation A1 are clustered at the level of the age of youngest child when time limits were introduced rather than at the state level. This is because there is no longer variation at the state level (all time limits were introduced during the same period across states in the sample); rather, the main source of variation is based on one's age of youngest child when time limits were introduced.[56] Lastly, the variable $PostTimeLimit_{it}$ is excluded from equation A1, as now all the males in the sample with similarly-aged youngest children are either in the post-treatment period or not from 1998 onwards. Similarly, equation Appendix Figure A5 does not need to include the $After_{st}$ variable, as in equation 2 - even if an individual moves, his post-time-limit period is the same in the sample, as everyone experienced time-limit introduction in the same period.

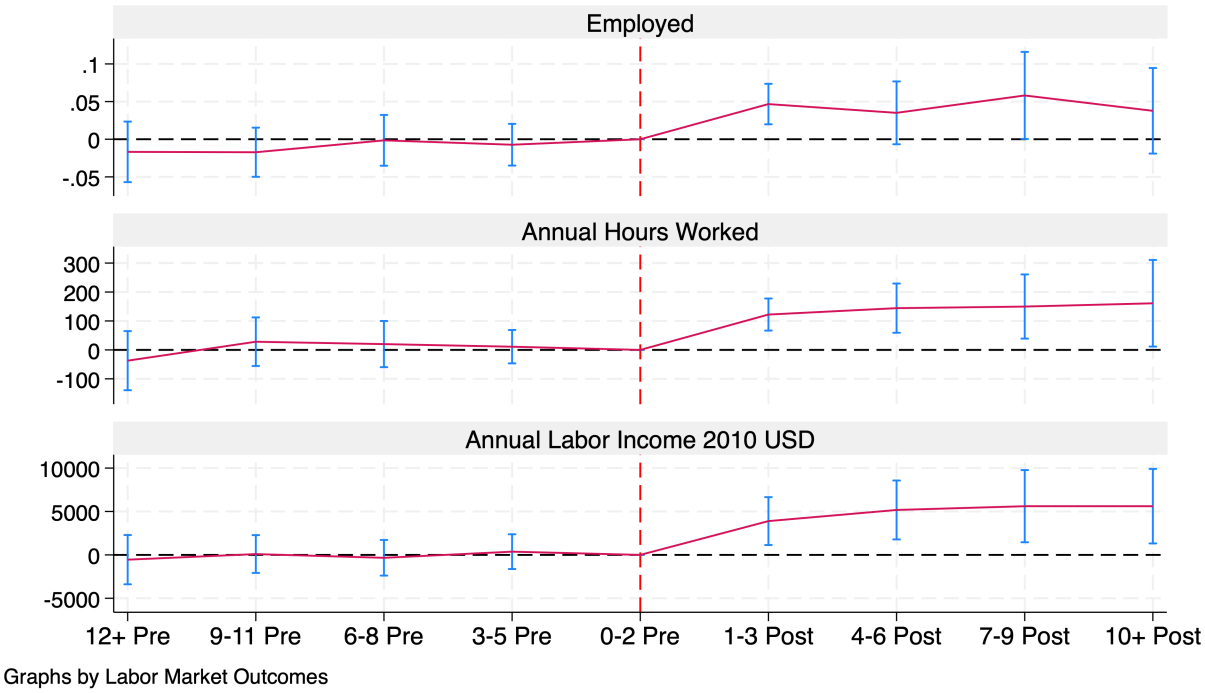
The results are presented in Appendix Figure A5 and are highly consistent with the baseline results presented in Figure 5. Despite the more simplistic approach to the regression framework in Appendix Figure A5, the results in Figure 5 are considered more credible, as the variation in the timing of time-limit introduction further supports the notion that the responses observed are indeed driven by these reforms as opposed to other shocks or policies. The results in Appendix Figure A5 can be more easily challenged by the argument of a confounding factor that occurred around 1997-1998 and differentially affected parents of younger children as opposed to older children (or no children).

.4 Disentangling Fixed Effects

Appendix Table A1 presents variations of equation 1 with a different set of fixed effects. The results exhibit the important role of the individual fixed effects in the regression specification, which is consistent with the patterns in the data observed in Figure 3. See section 5.1 for a discussion as to the possible reasons behind the importance of the individual fixed effects.

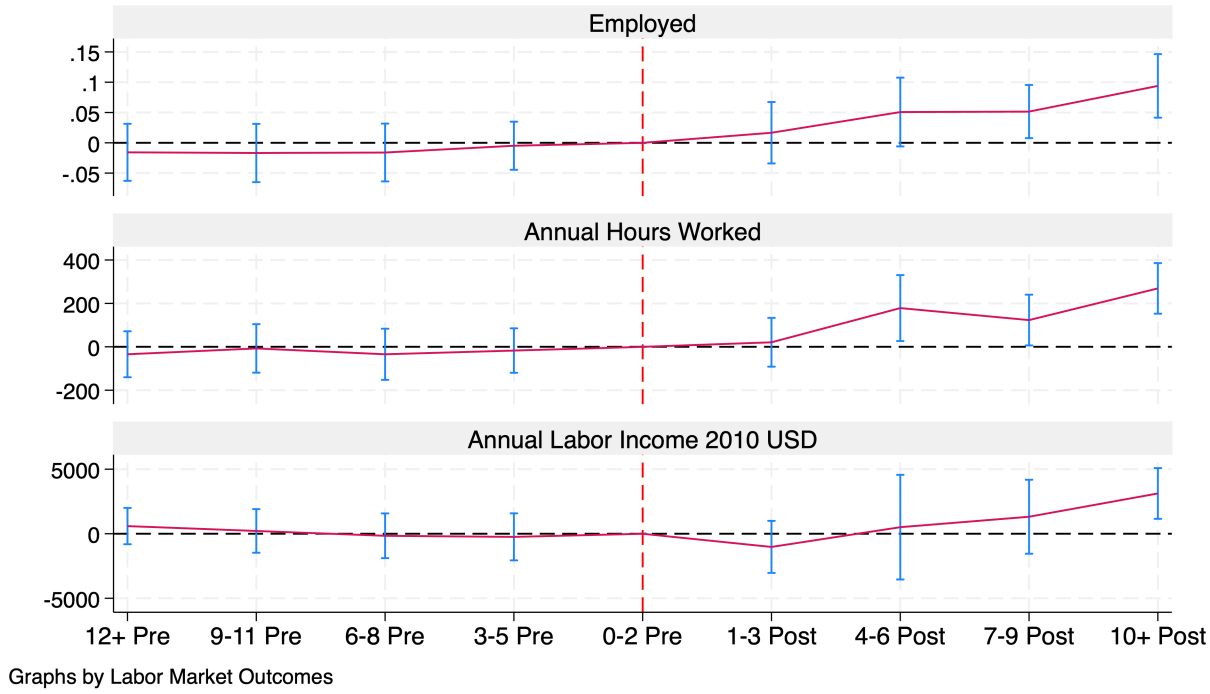
[56]This is not entirely precise, as there are still differences across states in the age of youngest child threshold for treatment status assignment. This is due to differences in the intensity of the time limit that was introduced.

Appendix Figure A5: Two-by-Two Event-Study Analysis - Male Labor-Market Outcomes in Response to Time-Limit Introductions



Notes: The figure presents event study coefficient estimates from equation A1 with the sample limited to individuals experiencing the introduction of time limits in 1997-1998 (28 states). The values on the horizontal axis are the number of years relative to the introduction of time limits. The vertical lines represent 95 percent confidence intervals. Standard errors are clustered at the level of the age of youngest child when time limits were introduced. Sample sizes range from 24,537 to 30,077 observations covering 2,479 to 3,169 males. Pre-treatment means are 0.80, 1,946, and 29,993 for employed, annual hours worked, and annual labor income, respectively.

Appendix Figure A6: Event-Study Analysis - Female Labor-Market Outcomes in Response to Time-Limit Introductions



Notes: The figure presents event study coefficient estimates from equation 2 for the female sample. The values on the horizontal axis are the number of years relative to the introduction of time limits. The vertical lines represent 95 percent confidence intervals. Standard errors are clustered at the state level. Sample sizes range from 43,118 to 57,444 observations covering 4,367 to 5,616 females. Pre-treatment means are 0.60 1,166, and 13,384 for employed, annual hours worked, and annual labor income, respectively.

.5 Female Summary Statistics

Appendix Table A2 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 of the male sample, at 59 percent as opposed to 77 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 partners.

.6 MVPF Calculation

If males experience improved labor market outcome in response to time limits, then a MVPF calculation for the removal of time limits that only accounts for females as beneficiaries or respondents would likely be biased upwards. To quantify the implications of this paper's findings on the MVFP for time limits, I begin with the MVPF calculated in Hendren and Sprung-Keyser (2020) for the removal of welfare payment time limits, which only considers females. Hendren and Sprung-Keyser (2020) use the following formula to reach their MVPF:

$$MVPF_f = \frac{\Delta_{fWR} \times MeanWP_f}{\Delta_{fWC} \times MeanWP_f} \tag{A2}$$

where Δ_{fWR} and Δ_{fWC} represent the change in females receiving welfare payment and the change in the welfare payment cost for females when removing time limits, respectively. Both are multiplied by $MeanWP_f$, the mean annual welfare payments for females, which cancel each other out in the numerator and denominator. Hendren and Sprung-Keyser (2020) use results from Pavetti (1995) and Grogger (2003) to calculate the percent share of the female welfare receipt population that is relevant for the numerator and denominator in equation A2, resulting in an MVPF of $\frac{28.1\%}{34.6\%} = 0.81$. Equation A2 captures females' behavioral responses to time-limit introductions in terms of welfare take-up by incorporating into the calculations the difference between the actual change in welfare take-up and the change that is relevant

Appendix Table A1: Male Labor Market Outcomes and Time-Limit Introduction - Disentangling Fixed Effects

	Employment				
Post*Treatment (with children affected by reform)	0.00939	0.0445***	-0.00129	0.0414***	0.0444***
	(0.0128)	(0.0124)	(0.0128)	(0.0122)	(0.0115)
Observations	51,446	50,642	51,446	50,642	50,642
Number of Individuals	6,569	5,765	6,569	5,765	5,765
R-squared	0.003	0.444	0.025	0.451	0.463
Pre-Treatment Mean Dep. Var.	0.782	0.786	0.782	0.786	0.786
	Annual Hours Worked				
Post*Treatment (with children affected by reform)	37.57	143.7***	19.41	126.7***	120.8***
	(35.45)	(38.42)	(37.95)	(38.10)	(38.05)
Observations	41,612	40,991	41,612	40,991	40,991
Number of Individuals	5,081	4,460	5,081	4,460	4,460
R-squared	0.002	0.520	0.031	0.528	0.529
Pre-Treatment Mean Dep. Var.	1,927	1,929	1,927	1,929	1,929
	Annual Labor Income (2010 USD)				
Post*Treatment (with children affected by reform)	608.1	4,274***	797.9	4,684***	4,345***
	(1,345)	(1,231)	(1,392)	(1,291)	(1,277)
Observations	41,607	40,987	41,607	40,987	40,987
Number of Individuals	5,081	4,461	5,081	4,461	4,461
R-squared	0.011	0.607	0.054	0.611	0.613
Pre-Treatment Mean Dep. Var.	30,297	30,393	30,297	30,393	30,393
Individual Fixed Effects	X	Yes	X	Yes	Yes
State Fixed Effects	X	X	Yes	Yes	Yes
Year Fixed Effects	X	X	Yes	Yes	Yes
Number of Children Fixed Effects	X	X	X	X	Yes

Notes: The table presents the coefficient estimate α_1 from equation 1 but with different fixed effects. When the omission of fixed effects requires controlling for an additional variable within the difference-in-differences framework, this variable is included as well. The sample is described in the Data section. Numbers in parenthesis are standard errors clustered at the state level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Appendix Table A2: Summary Statistics - Females

	All	Treated Individuals	Not Treated Individuals
Number of Observations	57,444	33,781	23,663
Number of Individuals	5,616	3,152	2,464
	Individual-Level Means		
Treatment Intensity (Yrs of	4.35	7.75	0.00
Welfare Benefits Lost)	(5.01)	(4.29)	-
Age of Youngest Child Upon	10.58	5.90	20.07
Time Limit Introduction	(8.05)	(4.20)	(5.07)
High School Degree or Less	0.82	0.78	0.86
	(0.39)	(0.42)	(0.34)
White	0.47	0.44	0.51
	(0.50)	(0.50)	(0.50)
Black	0.45	0.46	0.43
	(0.50)	(0.50)	(0.50)
Received Welfare Payments	0.27	0.35	0.17
Pre-Treatment (Female or Male Partner)	(0.44)	(0.48)	(0.38)
	Means Across All Obs.	Pre-Treatment Means Across All Obs.	
Age	35.06	28.92	35.11
	(9.94)	(7.13)	(8.09)
Age of Youngest Child	10.34	4.28	12.76
	(9.47)	(4.25)	(7.58)
Married / Cohabiting	0.59	0.61	0.63
	(0.49)	(0.49)	(0.48)
Number of Children under 18	1.47	1.96	1.22
in Household	(1.30)	(1.26)	(1.19)
Employed	0.61	0.55	0.65
	(0.49)	(0.50)	(0.48)
Annual Hours Worked	1216.76	1084.03	1259.76
	(949.27)	(923.66)	(931.50)
Income from Labor (2010 $)	15379.77	11711.51	15219.11
	(17576.55)	(13531.68)	(15988.96)
Welfare Receipt - Female or	0.09	0.16	0.07
Male Partner	(0.29)	(0.37)	(0.26)
Welfare Receipt - Female	0.09	0.16	0.07
	(0.29)	(0.36)	(0.26)
Welfare Receipt - Male Partner	0.04	0.07	0.04
	(0.19)	(0.25)	(0.20)

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. 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.

only for females who would have reached their time limit.[57]

To account for the male effects I find, I construct a household-level MVPF, intended to represent households that are potential welfare payment beneficiaries or respondents with two adults, rather than just a female adult. I assume that the household-level behavioral response in terms of welfare take-up is similar to the response in the Hendren and Sprung-Keyser (2020) MVPF calculation (equation A2). While this is not ideal, it is not completely far-fetched, considering that Grogger (2003), on which the Hendren and Sprung-Keyser (2020) MVPF is based, uses a sample of female-headed households from the CPS, and this should include some households with a male partner.[58] Furthermore, there are no alternative statistics for males reaching their welfare payment time limits, as in Pavetti (1995) for females. Thus, the precise statistics for females used by Hendren and Sprung-Keyser (2020) are not available for males exclusively.

The household-level MVPF due to the removal of time limits takes the following form:

$$MVPF_{hh} = \frac{\Delta_{fWR} \times MeanWP_{hh}}{\Delta_{fWC} \times MeanWP_{hh} + Add'lTaxes} \tag{A3}$$

In equation A3 the $MeanWP_{hh}$ terms do not cancel each other out from the numerator and denominator, as is the case in equation A2. It is therefore necessary to find a measure for the mean welfare payment at the household level. According to Hahn et al. (2016), in 2013, the average national maximum TANF benefit for a family of three (including two parents) was "just over $400 per month". Based on this, the representative two-adult household-level

[57] Grogger (2003) also shows female employment responses to welfare payment time limits, but this behavioral response is not included in the Hendren and Sprung-Keyser (2020) MVPF calculation. This is probably due to this female population's extremely low earnings. Therefore, their labor market responses to time limits should not affect their tax liabilities. Notwithstanding, these labor market responses can also have intergenerational effects, as demonstrated in Hartley et al. (2022) for welfare reform more broadly, but as already explained in Section 6.6, accounting for all potential implications of welfare payment time limits is beyond the scope of this paper.

[58] According to Brower and Ruggles (2003), in 1990 U.S. Census data, 7 percent of married couples reported having a female "household head" (or female "householders", according to the official title of household heads in U.S. Census data since 1980). This is just for married couples, so if taking into consideration cohabiting couples as well, an examination of female-headed households using U.S. census data is likely to result in a small, although not necessarily negligible, fraction of females who have a male partner present in the household.

MVPF is for a family of three with one child. For these households, the WTP and the cost of welfare benefits if time limits are removed ($MeanWP_f$) is assumed to be \$4,800 annually. I note that Hahn et al. (2016) also provide the distribution of benefit levels in ranges for their sample of two-parent households receiving TANF in 2013 (Table B.1). Although this distribution is not necessarily representative of two-parent TANF recipient households at the national level, it is not confined to families of three. Calculating the weighted average of this also results in just over \$400 per month in benefits. Given this specific measure for $MeanWP_{hh}$ available for 2013, I take 2013 as a representative year for my MVPF calculation.

$Add'lTaxes$ in equation A3 represents the fiscal externality that results from changes in male labor market outcomes due to the removal of time limits. I use NBER TAXSIM (Feenberg and Coutts (1993)) to calculate the change in tax liability. The change in earnings is the estimated increase in annual labor income in response to time limit introduction presented in Section 6. Table 2 shows this estimate is 2,929 2010 USD (converted to 3,129 2013 USD). Figure 5 shows that this increase is fairly stable across the years since the introduction of time limits. Annual earnings for males without time limits are the pre-treatment mean labor income in the sample of treated males in my analysis (Table 1 - 30,151 2010 USD converted to 32,211 2013 USD). I also take mean pre-treatment age and age of youngest child among the treated males in the sample (Table 1) to input in the NBER TAXSIM interface. Although the mean number of children under 18 for the pre-treatment males in my sample is 1.59, I assign for the households in the TAXSIM analysis only one child, and this is due to the choice discussed above of using two-adult, one child households as the representative households. The female spouse characteristics (age and labor income) are the pre-treatment means of the female spouses for the treated males in my sample.[59] Lastly, I add on childcare expenses amounting to 40.82 2013 USD per week for tax deduction purposes, based on the estimates in Anderson and Levine (2000) and also utilized in Kuka and Shenhav (Forthcoming). This results in a change in tax liability for the

[59]This is not presented in the paper, but a separate analysis of this resulted in a pre-treatment mean age of 30 and mean annual labor income of 14,069 2013 USD for these female partners.

2013 tax year of 469.35 2013 USD.

An important factor in the MVPF calculation presented in equation A3 is that the ratio of households that are changing their labor market outcomes (and as a result experiencing changes in tax liability) to those that are experiencing changes in welfare payment receipt is greater than one. This was demonstrated in Section 6.2 when comparing males' increased employment to the decline in welfare payments (for both the male and his female partner) in Table 2. A highly conservative measure of this ratio would thus take the percentage change in employment estimated in Table 2 for the numerator and the percentage change in welfare payment receipt (either male or his female partner) as the denominator. This results in a multiplier of $\frac{3.21}{2.59} = 1.24$ on the actual change in tax liability resulting from male labor market responses to time limits.

However, results in Table 3 demonstrate that male labor market responses to time limits are not just at the extensive margin but also at the intensive margin. Males who are responding to time limits at the intensive margin may not be changing their welfare payment receipt status as well, but they are not included in the numerator of $\frac{3.21}{2.59} = 1.24$, which is only capturing males who respond to time limits at the extensive margin (recall that 3.21 represents the point estimate for changes in employment in response to time limits from Table 2). As such, a multiplier larger than 1.24 seems more appropriate. With this in mind, I ran the regression specification in equation 1 with a dependent variable that is an indicator for having annual labor income that is above the median in my sample of males in the year one is observed. The median for each of the years in my sample is around 30,000 2010 USD, ranging from 24,200 to 35,600. This is very similar to the pre-treatment annual labor income among the treated males in my sample, which is used in the TAXSIM analysis. Thus, this transition within the distribution of labor income may be a decent representation of the males that are experiencing the relevant changes in their tax liabilities due to the introduction of time limits. The point estimate for the effect of welfare payment time limits in this regression is 5.99 (p-value is less than 1%). If I use this estimate, the multiplier on

the changes in tax liabilities for presenting the $Add'lTaxes$ measure in equation A3 is now $\frac{5.99}{2.59} = 2.31$, rather than 1.24.

Plugging all the relevant estimates into equation A3 results in the following:

$$MVPF_{hh} = \frac{28.1 \times 4,800}{34.6 \times 4,800 + 2.31 \times 469.35} = 0.66 \tag{A4}$$

Recall that this is in comparison to the 0.81 MVPF estimate of Hendren and Sprung-Keyser (2020).[60]

The MVPF in equation A4 represents two-adult households. However, I cannot disregard the Hendren and Sprung-Keyser (2020) estimate for single female households.[61] According to Moffitt et al. (2020), roughly 50% of females who are potentially eligible for welfare payments reside with a male partner. As such, the final MVPF should be a weighted average of the household-level MVPF in equation A4 and the Hendren and Sprung-Keyser (2020) MVPF. This results in the following overall new MVFP:

$$MVPF = 0.5 \times MVPF_f + 0.5 \times MVPF_{hh} = 0.5 \times 0.81 + 0.5 \times 0.66 = 0.73 \tag{A5}$$

This MVPF calculation was based on numerous simplifying assumptions. The relatively small difference in comparison to the Hendren and Sprung-Keyser (2020) MVPF is due to two factors. First, the Hendren and Sprung-Keyser (2020) MVPF already had a low value to begin with, demonstrating potential economic inefficiencies in the elimination of time limits. Second, the large difference between the expected change in annual tax liability (469.35 2013 USD) and annual welfare payments per household (4,800 2013 USD) imply

[60] If one takes the more conservative 1.24 multiplier for the changes in tax liability, the result of equation A4 would be 0.72 rather than 0.66.

[61] I disregard from these calculations single male fathers who may also be welfare payment beneficiaries or respondents. This is due to their small fraction among all welfare payment beneficiaries (and potentially respondents) and lack of data availability on their welfare payments and share reaching time limits. Furthermore, it is worth noting that the welfare payment response to time limits among males themselves is not clear-cut. While Table 2 demonstrates such a response, an analysis based on the intensity of treatment in Table 4 shows a null effect.

that accounting for the change in tax liability in the MVPF calculation should not have a dramatic effect on its value. This large difference is likely to persist even under more precise or rigid assumptions, thus not changing substantially the difference between an improved MVPF calculations that accounts for male labor market responses to time limits and the Hendren and Sprung-Keyser (2020) MVPF.