Welfare Time Limits and Participation

Gabrielle Pepin∗

Abstract

In 1996, the Personal Responsibility and Work Opportunity Act (PRWORA) established the Temporary Assistance for Needy Families (TANF) program within the United States. TANF, a cash welfare program for low-income families, introduced lifetime time limits for federal welfare dollars. States, however, reserve the right to determine whether and to what extent their TANF recipients are subject to time limits. In recent years, several states imposed TANF time limits for the first time or made existing time limits more stringent. In this paper, I estimate the effects of introducing these time limit policies on welfare use. I use time-varying state-level variation in time limit policies to estimate synthetic control and weighted difference-in-differences models of welfare participation. Using administrative and survey data, I find that the introduction of stricter time limit policies can decrease the state’s monthly number of adult TANF recipients by up to 48% and adults’ self-reported annual welfare use by up to 24%.

∗Economics PhD student at Michigan State University. I thank Stacy Dickert-Conlin, Steven Haider, Scott Imberman, Ben Zou, and seminar participants at Michigan State University for helpful comments and suggestions. All errors are my own.
1 Introduction

In 1996, the Personal Responsibility and Work Opportunity Act (PRWORA) established the Temporary Assistance for Needy Families (TANF) program within the United States. TANF, a cash welfare program for low-income families, replaced the Aid to Families with Dependent Children (AFDC) program and triggered a number of changes in US cash assistance administration. Notably, TANF introduced lifetime time limits for federal welfare dollars. Under AFDC, single mothers were eligible to receive cash assistance indefinitely if they continued to meet eligibility requirements, and between 1968 and 1988, 14% of welfare spells lasted 10 years or more (Bane and Ellwood, 1994). In contrast, federal TANF dollars cannot be allocated to families that already have received 60 months of assistance under PRWORA. States, however, reserve the right to determine whether and to what extent their TANF recipients are subject to time limits. States may provide benefits to entire families or to children in families whose federal lifetime time limits have expired using their own funds; alternatively, states may limit TANF benefits to shorter time periods. Early work on time limits suggests that states’ introduction of heterogeneous time limit policies may reduce welfare use by up to 16%, even before such time limits expire (Grogger and Michalopoulos, 2003).

States’ time limit policies have varied considerably since TANF’s implementation. As of July 2009, 34 states had 60-month time limits, 8 states had shorter time limits, and 8 states did not have time limits (Urban Institute, 2018).

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1 States can use federal dollars to provide extended benefits for up to 20% of their average monthly TANF caseload during times of hardship (Pub. L. 104-193, 1996).
2 States need not impose time limits on “child-only” TANF cases with nonparental caregivers. These types of cases typically involve children living with grandparents. In 2016, nonparental child-only cases constituted 51.3% of the TANF caseload (US Department of Health and Human Services, Office of Family Assistance, 2018).
3 Grogger and Michalopoulos (2003) estimate the effects of Florida’s Family Transition Program (FTP), a welfare experiment that involved time limits. 17% of the treated individuals (who were subject to time limits) reached their time limits during the study period. Of these individuals, 96% were removed from the welfare caseload although some did receive brief benefit extensions (Bloom et al., 2000). This suggests that Grogger and Michalopoulos estimate the effects of time limits that bind.
4 These are lifetime time limits in standard state programs that resulted in loss of benefits for the entire family. California, Oregon, and Rhode Island’s time limit policies applied to the adults in the assistance...
period, during which several states imposed TANF time limits for the first time or made existing time limits more stringent, most states’ time limits had not changed since 2000. Table 1 shows that between 2009 and 2015, Arizona and Kansas tightened existing time limits, Maine implemented a time limit policy for the first time, and Michigan began to enforce the 60-month federal time limit. These reductions in program generosity may have been particularly harmful to low-income families that experienced income losses during the Great Recession. Semenga et al. (2017) use data from the March Current Population Survey (CPS) to show that the poverty rate remained near heightened Great Recession era levels until 2015. Hence, the combination of a slow economic recovery and decreased TANF generosity during this time period may have proven detrimental to the wellbeing of low-income families.

In this paper, I estimate the effects of stricter time limit policies on reported welfare use and TANF receipt among adults in the wake of the Great Recession. These time limit policies differed from TANF implementation during the 1990s since they were retroactive: families’ months on welfare under the old policies counted toward the (new) time limit. Families that already had reached the (new) time limits at the time of policy implementation were removed from the TANF caseload shortly thereafter.

When TANF recipients reach their time limits and are removed from the welfare caseload, welfare use decreases mechanically. It is not entirely clear, however, that this occurs in practice. Many states offer hardship extensions or exempt certain individuals from their time limit policies. For example, in each of the treatment states, individuals who are ill or incapacitated, caring for someone who is ill or incapacitated, or victims of domestic violence unit only. Indiana implemented a 24-month time limit for adults but a 60-month time limit for the entire assistance unit.

California shortened its time limit from 60 to 48 months in 2011, but children of adults who reach California’s time limit remain eligible for TANF benefits (Urban Institute, Welfare Rules Database, 2018). Due to data limitations described in Section 5, I am unable to estimate the effect of this policy change in California, so I drop the state from the sample. While Washington formally introduced stricter TANF time limit policies in 2011, it continued to allow individuals who complied with TANF policies to remain on the caseload indefinitely.

I focus on welfare participation among adults since nonparental child-only cases are exempt from time limit policies. I also limit my analysis to state TANF programs as Tribal TANF programs, cash assistance programs for eligible Indian tribes, are subject to different regulations.
can be granted extended time on TANF. If time limit policies are enforced, however, then stricter time limits lead to decreases in welfare use as individuals reach their time limits more quickly. Moreover, welfare use may decrease further through behavioral effects. For example, stricter time limit policies may signal less generous welfare policies more generally, which could lead to ‘chilling effects’ (Watson, 2014): individuals leave or do not take up TANF due to decreased program generosity or increased welfare stigma. Finally, some TANF-eligibles may forgo welfare benefits to ‘bank’ their months on welfare. If effects of stricter time limit policies on welfare participation are economically and statistically significant, then this work takes the first step in understanding the effects of ending entitlements on economic wellbeing.

I use two datasets to estimate the effects of state time limit policies on adult welfare participation: monthly state-level administrative data and yearly individual-level survey data from the American Community Survey (ACS). The two datasets complement each other well as each has its own advantages. The administrative data are not subject to under-reporting bias (Meyer and Wu, 2018; Meyer et al., 2009), which eliminates the possibility of non-classical measurement error in reported welfare receipt. The ACS data allow me to estimate the effects of stricter time limit policies conditional on individual demographics. I estimate synthetic control and weighted difference-in-differences specifications of welfare participation, and similar estimates across datasets and empirical specifications help to mitigate concerns about potential data issues and identification assumptions.

I find that state time limit policies can decrease the state’s monthly number of adult TANF recipients by up to 48% and adults’ self-reported annual welfare use by up to 24%. Consistency in signs on coefficients across states, datasets, and empirical specifications is reassuring. The results in this paper are relevant for policymakers as they consider how TANF policy changes may affect state budgets and individual wellbeing: while stricter TANF time limits may be effective cost-saving measures, they also may encumber a low-income population with low levels of labor force attachment.

\footnote{Data from samples of the TANF caseload show that the proportion of adult TANF recipients who receive disability benefits has remained relatively stable in each treatment state throughout the sample period (US Department of Health and Human Services, Office of Family Assistance, 2018).}
2 Background

TANF is a means-tested transfer program: income, assets, and family size determine households’ eligibility. In 1996, the TANF program replaced its predecessor, the AFDC program. In crafting the TANF program, policymakers sought to “end the dependence of needy parents on government benefits” (US Department of Health and Human Services, Office of Family Assistance, 2018), and cash assistance caseloads generally have been declining since TANF’s implementation. In fact, between 1996 and 2015, the number of adult TANF recipients decreased from nearly 4 million to fewer than 800,000 (US Department of Health and Human Services, Office of Family Assistance, 2018). Still, TANF’s target population includes those most likely to fall into poverty. Table 2 displays state TANF program parameters as of July 2009, which largely had been established by 2000 as state TANF program characteristics changed little between 2000 and 2009. Table 2 shows that eligibility requirements vary markedly across states but that many states limit monthly income to less than $1,000 for initial TANF eligibility.

Differences in TANF program rules motivate my decision to study each treatment state separately. TANF program generosity at baseline necessarily affects the size of time limit effects on welfare participation. Maine and Michigan both had relatively generous TANF policies before their time limit policy changes, but Arizona and Kansas did not. As shown in Table 2, Maine allowed individuals to remain on TANF indefinitely; Michigan effectively did the same through time limit exemptions and extensions. Arizona and Kansas had 60-month time limits. Likewise, the monthly income thresholds for the initial eligibility of three-person families were $1,023 and $815 in Maine and Michigan, respectively, but less than $600 in both Arizona and Kansas. Finally, Kansas, Maine, and Michigan’s maximum monthly benefits for three-person families were $400 and $500, but Arizona’s was only $278. Therefore, larger caseload declines in Michigan and Maine relative to those in Arizona and Kansas would suggest that stricter TANF policies may exhibit decreasing marginal effects on caseloads.

Demographic differences in TANF caseloads offer another channel through which effect sizes may differ across states. Figure 1 displays characteristics of adult TANF recipients from fiscal year 2007 through fiscal year 2016 (US Department of Health and Human Services,
Office of Family Assistance, 2018). Figure 1 shows that throughout the sample period, Michigan had a high proportion of female TANF recipients, and Maine and Michigan’s proportions of single TANF recipients are higher than those of the other treatment states and the total US adult TANF caseload.

Figure 1 also displays the dire economic circumstances of the TANF population: only about a quarter of adult TANF recipients are employed, and a slightly larger proportion have any non-TANF income. This suggests that TANF benefits could decrease the number of children and adults living in poverty. Bitler and Hoynes (2016), however, provide evidence that TANF has become less effective at reducing instances of extreme poverty over time. Bitler and Hoynes use data from the March CPS to calculate poverty measures that account for taxes and in-kind transfer income. They find that the percent of non-elderly individuals below 50% of the federal poverty line in 1982 increases by nearly 3 percentage points in the absence of welfare income. That same measure increases by less than 0.5 percentage points in the absence of welfare income in 2010. Thus, Bitler and Hoynes’ work suggests that TANF has become less effective at minimizing the extent of extreme poverty within the United States over time.

Bitler and Hoynes’ results may seem somewhat surprising, given that states must maintain specified levels of spending on services for needy families with children each year to receive TANF block grant funds from the federal government. Since 1996, the federal TANF block grant has been set at $16.5 billion per year, and AFDC spending in 1994 determines the size of states’ block grants. State spending funds about half of the TANF program, totaling $15.2 billion in 2015 (US Department of Health and Human Services, Office of Family Assistance, 2018). In addition to spending requirements, the federal government re-

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8The US Department of Health and Human Services, Office of Family Assistance samples the TANF caseload each year to estimate caseload characteristics. Samples usually include less than 1% of the TANF caseload but have included over 20% of the caseload in some years.

9The spikes in proportions of individuals who are employed and who have non-TANF income in Arizona during fiscal year 2012 are likely due to reporting errors.

10In calculating their poverty measures, Bitler and Hoynes (2016) do not account for behavioral effects of welfare availability that may affect poverty.

11These spending amounts are in nominal dollars. Hence, the real value of the federal TANF block grant has decreased over time due to inflation.

12State spending is not restricted to cash assistance. It also can finance childcare assistance programs;
quires that a proportion of states’ TANF recipients participate in work-related activities, such as employment, job training programs, and vocational training. Save for the spending and work requirements, however, states exercise a great deal of autonomy in determining TANF eligibility requirements, benefit amounts, sanctions for noncompliance with welfare-to-work laws, and whether to exempt certain individuals from time limits (Pub. L. 104-193, 1996).

Much of the existing literature on welfare time limits estimates effects of PRWORA and relies on variation in states’ timing of TANF implementation for identification. Individuals’ welfare time clocks began when their states implemented TANF, so households whose youngest children would turn 18 before the time limit could expire remained unaffected by the new time limits. Using age of youngest child as a source of variation, Grogger (2003) finds that the introduction of time limits reduces welfare use by 6.6 percentage points.

More recently, researchers estimate the responsiveness of TANF caseloads and spending to the Great Recession. Bitler and Hoynes (2016) find that neither TANF caseloads nor total state and federal TANF spending increased during the Great Recession, which may have led to the increased cyclicality of extreme poverty during this time period relative to that of past recessions. This was in spite of increased federal TANF funding through the American Recovery and Reinvestment Act of 2009 (ARRA), a comprehensive stimulus package that allocated an estimated $787 billion of federal spending toward a number of areas, including education, health care, infrastructure, and social safety net programs. Five billion dollars of these funds went toward emergency TANF spending during fiscal years 2009 and 2010 (Pub. L. 111-5, 2009). When the ARRA appropriations ended in 2011, some state governments, particularly those facing severe budget shortfalls, found themselves struggling to fund TANF caseloads, despite the fact that these caseloads did not respond to the Great Recession.

In the context of these budget shortfalls, several states imposed lifetime TANF time limits for the first time or made existing time limits more stringent between 2010 and 2012. As shown in Table 1, Arizona and Kansas gradually shortened their time limits from 60 months to 12 and 24 months, respectively. Maine adopted the federal 60-month time limit. In my empirical specifications, I consider a state’s treatment period to begin when it first established or shortened its time limit. Although effects of individual time limit policy changes from 60 to 36 months,
limit for the first time in 2012. In 2011, Michigan eliminated the federal hardship exemption from the 60-month federal time limit. This stipulation allows states to exempt up to 20% of their TANF caseloads from the federal time limit if the state can demonstrate economic difficulties. Thus, at the time of the policy change, families in Michigan could have been receiving TANF benefits for up to 15 years. In practice, the Michigan Department of Human Services projected that the elimination of the federal hardship exemption would immediately close nearly 11,000 TANF cases (Carley, 2011).  

Descriptive evidence suggests that states reduced TANF generosity in response to economic difficulties. All of the states that imposed stricter time limit policies were experiencing budget shortfalls when they first did so (Oliff et al., 2012). And in all cases, these cuts to TANF spending coincided with similar cuts to education, programs for the elderly and disabled, and government workforces (Johnson et al., 2011). For example, Brewer and Young (2010) cite economic difficulties in Arizona’s TANF Caseload Reduction Report for fiscal year 2010. The authors explain that the Arizona Department of Economic Security’s budget was cut by over 31% between the beginning of fiscal year 2009 and the end of fiscal year 2010 and claim that deeper cuts would have been necessary in the absence of the ARRA appropriations. Likewise, Michigan forecasted $74,852,364 in cost-savings due to its time limit case closures during fiscal year 2012 (Carley, 2011).  

In this paper, I estimate the effects of these recent time limit policies on adult welfare participation. This differs from earlier work on time limits for several reasons. In contrast to previous studies, I estimate the effects of time limit policies that were retroactive and independent of other major welfare reforms. To the best of my knowledge, no one has 36 to 24 months, and 24 to 12 months in Arizona and from 60 to 48 months, 48 to 36 months, and 36 to 24 months in Kansas likely become confounded with one another, I can estimate the effects of these states’ stricter TANF time limit policies more generally.

While Michigan formally established a 48-month time limit in 2007, Michigan’s legislature did not implement this policy until 2011 and still allows time limit extensions for several groups of individuals, including individuals who have disabilities, care for disabled family members, or have young children. The Michigan Department of Human Services projected that the implementation of the 48-month state time limit would immediately close fewer than 100 TANF cases (Carley, 2011).  

Kansas implemented some other TANF reforms during the sample period. Changes in application requirements are the only such changes that the Kansas Department for Children and Families estimates to have had a large effect on TANF caseloads. See Kansas’s TANF Caseload Reduction Report (2018) for
studied these recent time limit policies yet. Further, there are substantial macroeconomic
differences between the 1990s and 2010s. The existing literature suggests that the economy
likely accounted for a large proportion of the decrease in AFDC/TANF caseloads during
the 1990s (Klerman and Haider, 2004). Since the unemployment rate was 5.4% in 1996
and 8.9% in 2011 (US Department of Labor, Bureau of Labor Statistics, 2018), differences
between results can provide suggestive evidence about whether effect sizes differ between
recessions and expansions.

3 Conceptual Framework

In theory, stricter TANF time limit policies may decrease welfare use through both me-
chanical and behavioral effects. First, if states enforce time limit policies, then welfare use
decreases mechanically as states remove non-exempt ineligible TANF recipients from their
welfare caseloads. This is depicted in Figure 2, which graphs data on years of TANF receipt
from an annual sample of TANF heads-of-household and their spouses (US Department of
Health and Human Services, Office of Family Assistance, 2018). The graphs in Figure 2
show the proportion of adult TANF recipients who have received TANF benefits for fewer
months than the treatment states’ (new) TANF time limits allow. I compare each treat-
ment state’s trajectory to that of a control state that had the same TANF time limit and
other similar program policies at baseline. While the control states’ trajectories decrease or
remain relatively flat, the proportion of TANF recipients whose time on welfare falls within
the new time limit increases in each treatment state, which suggests that mechanical effects
are at work.

Second, stricter time limit policies may lead to changes in TANF participation among
eligibles. Specifically, stricter time limit policies may signal stricter TANF policies more
generally and induce eligibles not to participate. For instance, individuals may leave TANF
or postpone participation to bank their months on welfare. This may occur if agents are
forward-looking and seek to smooth lifetime consumption or are risk-averse and want to
details. Some states also implemented drug testing for TANF benefits, usually suspicion-based, during the
sample period. Still, very few TANF applicants and recipients—“in many cases, less than 1 percent”—tested
positive for illicit substances (Hall, 2016).
insure against negative income shocks. Chilling effects of decreased TANF enrollment offer another channel through which TANF take-up may decrease if the “icy” policy climate deters TANF-eligibles from taking up benefits (Watson, 2014). Any such chilling effects will cause even larger decreases in TANF use in the long run as smaller groups of recipients that take up welfare under the new policy gradually replace larger groups that took up welfare under the previous policy (Haider and Klerman, 2005). Aizer and Currie (2004) provide evidence that institutions may affect benefit take-up. They study take-up of public maternity care in California, and results suggest that hospital characteristics largely determine program participation. This notion is relevant for take-up within the TANF program as enrollment costs are non-trivial. After filing an application for benefits, the TANF applicant generally must interview with a caseworker. During this interview, the applicant may have to present a number of income verification documents, such as pay stubs, bank statements, rental agreements, utility bills, and child support court orders. These enrollment costs, combined with stricter time limit policies, may generate the sort of icy policy environment that would deter many eligibles from taking up benefits.

Additionally, Fang and Silverman (2009) show that time limits can decrease lifetime utility among young women with low levels of education in high welfare benefits states due to decreased welfare participation. The authors’ model suggests that these women exhibit an inability to commit to the stigma effects of welfare take-up; therefore, increases in welfare stigma could lead to further decreases in their lifetime utility. Low, Meghir, Pistaferri, and Voena (2018) develop a life-cycle model of welfare use, labor supply, and marriage market decisions. Consistent with Fang and Silverman, these authors find that time limits are associated with decreased welfare participation because they cause women to delay welfare take-up until their children are older. Taken as a whole, this research suggests that time limits decrease welfare use; that decreased welfare use is due, at least in part, to behavioral effects; and that such changes may decrease lifetime utility.
4 Previous Literature

Randomized welfare experiments that took place shortly before PRWORA provide some of the most credible studies of the effects of welfare reform. Florida’s Family Transition Program (FTP), one such experiment that involved time limits, took place in 1994 in Escambia County, Florida. While the control participants in FTP remained in the existing AFDC program, treatment participants entered a new welfare program with 24-month time limits, financial work incentives, and more extensive welfare-to-work services. Grogger and Michalopoulos (2003) exploit variation in ages of households’ youngest children to separate time limit effects from effects of the other treatments. A family expects to lose benefits once its youngest child turns 18, so time limits do not affect households whose youngest children will turn 18 before the time limit expires. Under the assumptions that effects of policy changes are additive and that effects of financial incentives and work services are age invariant, the authors find that time limits can reduce welfare use by up 16%, even before they expire. Still, the age invariance assumption seems unlikely to hold, given the well-documented life-cycle effects of earnings and consumption (Meghir and Pistaferri, 2011).

Similarly, Grogger (2002, 2003, 2004) uses variation in states’ timing of TANF implementation and ages of households’ youngest children to model the effects of time limits on welfare use, income, and labor market outcomes. Using a sample of women heads-of-household from the March CPS, Grogger (2003) finds that on average, families whose youngest children are three years old reduce their welfare use by 6.6 percentage points relative to families whose youngest children exceed the age threshold at which time limits do not bind.

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16 See Ziliak (2015) for a comprehensive review of the TANF literature.
17 Individuals who were particularly disadvantaged faced 36-month time limits.
18 As of 2016, 1.9% of TANF families had no child recipients (US Department of Health and Human Services, Office of Family Assistance, 2018).
19 Chan (2018) shows that Grogger and Michalopoulos’s estimates may be biased downward.
20 In other work, Grogger (2002) finds evidence of state policy endogeneity. In this work, Grogger uses data from the Survey of Income and Program Participation (SIPP) to estimate the effect of time limits on welfare use among families with children of all ages. The positive coefficient on the time limit dummy is evidence of policy endogeneity as time limits should not affect welfare use among families with older children. Still, standard errors are large.
Authors also attempt to disentangle the effects of the economy and welfare policy on welfare participation. Early estimates of relative effect sizes vary widely across functional form assumptions; see Grogger and Karoly (2005) for a review. In a policy report, the Council of Economic Advisers (1999) compares caseload declines during the pre-TANF waiver period with those throughout TANF’s early years. The unemployment rate decreased by a greater amount during the pre-TANF period, and the Council of Economic Advisers attributes a greater share of the caseload decline to economy effects during this time. They find that the economy can explain 26-36% of the caseload decline during the pre-TANF period but only 8-10% of the decline during the TANF period. Klerman and Haider (2004) develop a more formal stock-flow model of welfare participation in which the existing stock of welfare recipients is a function of previous flows onto and off of welfare. Klerman and Haider estimate their model using administrative data from California and find that about half of California’s decrease in welfare participation can be explained by decreases in the unemployment rate over the sample period.

5 Welfare Participation Data

I use administrative caseload data from the US Department of Health and Human Services, Office of Family Assistance to estimate the effects of state time limit policies on the number of adult TANF recipients. The federal government requires states to document the number of their adult TANF recipients by month, which minimizes reporting bias from individuals who may be reluctant to report TANF receipt. This aspect of the administrative data may be particularly advantageous, given empirical evidence that survey respondents underreport welfare use (Meyer and Wu, 2018; Meyer et al., 2009). I restrict my sample to October 2006 through December 2016 as some federal TANF laws differed before the Deficit Reduction Act of 2005 (DRA) took effect. \footnote{PRWORA required that each state gradually increase its percentage of TANF recipients engaged in work activities. By 2002, states needed 50% of all families to participate in work activities, but states’ work participation percentages could decrease if their TANF caseloads declined. The DRA changed the base year for this caseload reduction credit from 1995 to 2005, effective October 1, 2006 (Pub. L. 109-171, 2006).} I use adult population data from the US Census Bureau to scale the number of adult TANF recipients by each state’s adult population so that I can
compare changes in adult TANF participation across states.

Unfortunately, the administrative TANF data exhibit some problems. First, during the sample period, a number of states provided token payments to working individuals in an effort to satisfy the federal work requirements. Specifically, states can provide token payments to individuals who leave the TANF caseload for employment to boost state work participation rates to ensure federal funding. But when states introduce or eliminate these token payments, the sizes of their TANF caseloads can change substantially, making the administrative data reported by these states a poor measure of trends in traditional TANF participation. For example, after Ohio instated a $10 food benefit payment program in 2014, the state’s reported number of adult TANF recipients more than doubled (Ohio Department of Job and Family Services, 2018). This creates a discontinuity in the data that does not reflect a change in TANF participation as measured in previous literature.

Further, employees at Delaware Health and Social Services, Kentucky’s Cabinet for Health and Family Services, and Vermont’s Agency of Human Services confirmed programming glitches or reporting errors in their states’ TANF data as reported on the US Department of Health and Human Services, Office of Family Assistance website. Finally, Illinois experienced a budget shortfall in 2012 that caused some benefit payments to be delayed until 2013; Oregon and Rhode Island instituted TANF reforms in 2007 and 2008, respectively; and Missouri and changed its time limit policy in 2016 (Urban Institute, 2018).

Due to token payments, reporting errors, and state policies, I drop the following states from the administrative data sample: California, Delaware, Illinois, Kentucky, Maine, Missouri, Oregon, Rhode Island, and Vermont.

After I drop a handful of states from the administrative data sample, one may be concerned about the ability of the remaining states to produce valid counterfactuals for the treatment states. Because of this, I conduct a complementary analysis. I estimate the effects of time limits on reported welfare use among adults using data that spans 2007-2016.

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22 I do not estimate the effects of TANF reforms in Oregon, Rhode Island, and Missouri due to the lack of pre- or post-treatment data.
23 If I drop states that begin to provide token payments during the sample period from the sample since this distorts traditional TANF participation trends. I do not, however, drop states that provided these payments over the entire sample period from the sample.
from the American Community Survey (ACS). The ACS is an annual state-representative survey of over 3.5 million households. The ACS data describe individuals' demographics, family structures, income, and program participation, among other topics. In particular, ACS asks respondents whether they received “[a]ny public assistance or welfare payments from the state or local welfare office” within the past year (US Census Bureau, 2018). While I still drop states that changed their TANF policies during the period under study from the sample (California, California, Illinois, Missouri, Oregon, and Rhode Island), survey data should not exhibit the types of programming glitches that can materialize in states’ TANF reporting systems.

There are a couple of major differences between estimates of welfare participation generated by the ACS and administrative datasets. First, annual welfare use rates from the ACS are necessarily higher than monthly welfare use rates from the administrative data. Estimates from fiscal year 2010 indicate that over 40% of adults on TANF have received benefits for one year or less (US Department of Health and Human Services, Office of Family Assistance, 2018), so differences between monthly and annual welfare use rates may be nontrivial. Second, self-reported welfare data are prone to reporting bias. Meyer, Mok, and Sullivan (2008) compare administrative and reported welfare data and find that ACS respondents underreport TANF income by about 20%.

6 Empirical Strategy

I first estimate synthetic control models of adult welfare participation separately for each treatment state using the administrative data. By studying each treatment state separately, I can identify whether heterogeneous effects exist across state policy environments, given

\footnote{California shortened its time limit for adults in the assistance unit only in 2011. Families affected by this time limit policy likely still report having received welfare income for their children.}

\footnote{While ACS respondents who only received token payments could still report welfare receipt, this does not seem to materialize in the data. Many token payments are in the form of food benefits, which respondents may not consider “welfare payments”.

One may be concerned that individuals who received Supplemental Security Income (SSI) benefits may report that they received welfare income, but ACS respondents are asked whether they received any SSI income within the past year immediately before they are asked about welfare receipt.
the differences in state TANF program generosity at baseline. And by using the synthetic control method, I allow state-specific characteristics to vary over time. This is particularly important when studying the effects of welfare time limits: the descriptive evidence documented in Section 2 suggests that treatment states changed their time limits in response to the Great Recession’s effects on their economies. If this is true, then welfare participation in treatment states likely would have increased disproportionately in the absence of their policy changes as more individuals would have qualified for TANF benefits. In this case, failing to control for time-varying state-specific characteristics leads one to underestimate the effects of stricter time limit policies on welfare participation.

I estimate the following model:

$$Y_{st} = \beta TimeLimit_{st} + \mu_s \delta_t + \theta_{st}\phi + \epsilon_{st},$$

(1)

where $Y_{st}$ is the number of adult TANF recipients per 1000 in adults in state $s$ during month $t$, a measure of the probability of receiving TANF during a given month. $TimeLimit_{st}$ is an indicator for the state policy change that is a 1 in the treatment region during the post-treatment period. $\mu_s \delta_t$ represents state-specific fixed effects with time-varying coefficients. $\theta_{st}$ is a vector of state characteristics, including the state’s unemployment rate (US Department of Labor, Bureau of Labor Statistics, 2018), inflation-adjusted minimum wage (US Department of Labor, Bureau of Labor Statistics, 2018), and inflation-adjusted maximum TANF benefit for a three-person family (Urban Institute, Welfare Rules Database, 2018). $\epsilon_{st}$ is the error term.

In Equation (1), a weighted average of the control states comprises the synthetic control unit for each treatment state\(^{27}\). Specifically, the trajectory of the synthetic control unit’s dependent variable mirrors that of the treatment state’s dependent variable during the pre-treatment period. Since the synthetic control unit is made up of control states, I can use the synthetic control unit’s dependent variable trajectory as a counterfactual for the treatment state’s dependent variable trajectory during the post-treatment period. $\beta$ captures the difference between the treatment state and synthetic control unit during each post-treatment

\(^{27}\)Control states include all US states, except Arizona, California, Delaware, Illinois, Kansas, Kentucky, Maine, Michigan, Missouri, Ohio, Oregon, and Vermont.
time period\footnote{I average lagged TANF caseload values over periods of three pre-treatment months to avoid model overfit. Hence, each post-treatment time period consists of three consecutive months.} the causal effect of time limits on adult welfare use.

I find the optimal control state weights for each counterfactual by minimizing the distance between pre-treatment dependent variable trajectories in the treatment state and synthetic control group:

\[
W = \arg\min_{w_k \in [0,1]} \| Y - \sum_{k \in K} w_k Y_k \|, \tag{2}
\]

where \( Y \) denotes the dependent variable in the treatment state in each of the pre-treatment time periods, \( Y_k \) denotes the dependent variable in control state \( k \) during that same time period, and weights \( w_k \) sum to 1.

In order to compare results across specifications, I calculate the mean of the elements of \( \hat{\beta} \), measuring the average treatment effect during the post-treatment period. Denote this measure as \( \tilde{\beta} \). While the synthetic control method does not produce standard errors on this estimate, I measure the probability that sampling variation drives treatment effects using placebo tests. Specifically, I apply the synthetic control method to each state in the control region by removing that state from the control group and acting as though it experienced the treatment. I calculate the probability of estimating an effect size of \( \hat{\beta} \) in a random permutation as

\[
\hat{\lambda} = \frac{\sum_{k \in K} 1[\hat{\beta}_k > \tilde{\beta}]}{K}, \tag{3}
\]

where \( \hat{\beta} \) denotes the estimated actual average treatment effect and \( \hat{\beta}_k \) denotes the estimated average treatment effect in control state \( k \). Hence, the relative magnitudes of the actual and placebo average treatment effects generate the \( \hat{\lambda} \) for each synthetic control estimate. When the magnitude of the actual average treatment effect is larger than that of nearly any placebo treatment during the post-treatment period, the probability that sampling variation drives treatment effects likely is small.

Still, placebo estimates with poor pre-treatment fit may not provide sufficient information to evaluate the relative magnitudes of actual and placebo treatment effects. The pre-treatment root mean squared prediction error (RMSPE) enables one to measure the
goodness of fit of the synthetic control to the actual data during the pre-treatment period. In light of this, I display figures of the distribution of

$$\hat{\alpha}_s = \frac{PostRMSPE_s}{PreRMSPE_s},$$

(4)

where $PostRMSPE_s$ denotes the RMSPE of synthetic state $s$ during the post-treatment period, and $PreRMSPE_s$ denotes the RMSPE of synthetic state $s$ during the pre-treatment period. If the actual treatment states exhibits good pre-treatment fit (low $PreRMSPE$) and a large estimated effect size (high $PostRMSPE$), then the treatment state’s $\hat{\alpha}$ will lie in the right tail of the distribution.

Next, I supplement the synthetic control models with weighted difference-in-differences models (Dickert-Conlin et al., forthcoming; Severini, 2014; Spreen, 2018) using the ACS data. I first aggregate the data to the state level using weights that account for the representatbility of each sample household within the US population. Then, I estimate Equation (1), controlling for age, race, number of children, educational attainment, and the inflation-adusted maximum Earned Income Tax Credit (EITC) benefit by family size at the state level (National Bureau of Economic Research, 2017). This yields $W$, the set of optimal weights generated by the synthetic control method. I then estimate the following equation that exploits the micro-level information contained in the ACS data, using $W$ as a weighting matrix:

$$Y_{ist} = \beta TimeLimit_{st} + \mu_s + \delta_t + \theta_{ist} + X_{ist}\Gamma + \epsilon_{ist},$$

(5)

where $Y_{ist}$ is an indicator for reported welfare receipt for individual $i$ in state $s$ during year $t$, and $X_{ist}$ is a vector of individual characteristics. The remaining parameters are similar to those listed earlier.

In these specifications, I maintain the difference-in-differences assumption that in the absence of treatment, the difference between the treatment and control states would have remained constant. When I estimate the weighted difference-in-differences models, however, I minimize the distance between pre-treatment dependent variable trajectories in the

---

29 Results from standard difference-in-differences models can be found in the appendix.

30 Control states include all US states, except Arizona, California, Illinois, Kansas, Maine, Michigan, Missouri, and Oregon.
treatment and control groups. Hence, the difference-in-differences parallel trends assumption becomes more realistic due to pre-treatment similarities between the treatment and control regions. I cluster standard errors at the state level.\footnote{This strategy proves invalid when few control states receive positive weights, resulting in a small number of clusters (Cameron and Miller, 2015). All 41 control states receive positive weights in my weighted difference-in-differences specifications for Arizona, Kansas, and Michigan. The specification for Maine, however, only allocates positive weights to 3 states. Therefore, I also estimate standard errors in the style of Donald and Lang (2007). Results are available in the online appendix.}

7 Results

7.1 Adult TANF Receipt

Figure 3 shows the estimated effect of stricter time limit policies on monthly adult TANF receipt in Arizona. In the top panel, the line labeled “Arizona” shows the number of adult TANF recipients per 1000 adult residents in Arizona by month. This ratio hovers around 0.4 before Arizona first shortens its time limit policy in 2010. After the policy change, the ratio decreases to less than 0.3 and continues to decrease to less than 0.2 by 2016.

Synthetic Arizona, Arizona’s synthetic control group, matches the Arizona’s pre-treatment trends well. The two groups’ trajectories track each other closely before the policy change and diverge immediately thereafter. The middle panel of Figure 3 presents placebo-treatment effects as described in Section 6. A thin line represents each placebo-treatment effect. The thick line represents the actual treatment effect, which is simply the vertical distance between the Arizona and Synthetic Arizona in the top panel. The actual treatment effect average over the post-treatment period is -0.198, which constitutes a 48% decrease from the baseline mean. In 6 of the 79 post-treatment periods, Arizona’s treatment effect is larger in absolute value than any of the placebo-treatment effects, which implies that $\hat{\lambda} = 0.0263$. Similarly, Arizona’s treatment effect is larger in absolute value than all but 1 placebo-treatment effect in 37 periods, all but 2 placebo-treatment effects in 32 periods, and all but 4 placebo-treatment effects in the remaining 4 post-treatment periods. This implies $\hat{\lambda}$ values of 0.0526, 0.0789, and 0.1053, respectively.

The bottom panel of Figure 3 displays each $\hat{\alpha}_s$, the ratio of post- and pre-treatment
RMSPE in each state. Arizona’s post- to pre-treatment RMSPE ratio is striking: the post-treatment RMSPE is nearly 20 times as large as the pre-treatment RMSPE, a greater ratio than that of any control state. Thus, if the time limit policy change was randomly assigned within the data, the probability of obtaining a post- to pre-treatment RMSPE ratio as large as Arizona’s is $1/38 = 0.0263$. Taken together, the panels in Figure 3 suggest that Arizona’s stricter time limit policies caused large decreases in adult TANF receipt.

Similarly, Figure 4 shows the estimated effect of stricter time limit policies in Kansas. The number of adult TANF recipients per 1000 adults in Kansas remains around 0.5-0.6 before Kansas first shortens its time limit policy in 2011. Then, the number of adult TANF recipients per 1000 decreases to less than 0.4 and continues to decrease to less than 0.2 by 2016. Synthetic Kansas matches Kansas’s trajectory well until the policy change, at which time the groups’ trajectories diverge. The average treatment effect over the post-treatment period is -0.168, a 31% decrease relative to the baseline mean. Kansas’s treatment effect is larger than all of the placebo-treatment effects in 1 post-treatment period and larger than all but 1, 2, and 3 placebo-treatment effects in 4, 17, and 11 of the 123 post-treatment periods, respectively. Additionally, Kansas’s post- to pre-treatment RMSPE ratio is greater than that of any control state. This evidence implies that Kansas’s stricter time limit policies led to decreases in adult TANF receipt.

Finally, Figure 5 shows the estimated effect in Michigan. Michigan’s number of adult TANF recipients per 1000 fluctuates a bit before 2010, likely due to TANF policy changes that occurred in Michigan around this time. In April 2007, Michigan reinstated a former TANF eligibility requirement of attending a work orientation program (Michigan Pub. A. 9, 2007). Michigan also experienced large increases in its number of adult TANF recipients when it received ARRA funds in fiscal years 2009 and 2010. Because of this, Michigan’s pre-treatment trajectory does not match that of Synthetic Michigan nearly perfectly, as is the case in Arizona and Kansas. Still, the groups’ pre-treatment series match fairly well before Michigan begins to enforce its time limit policy in 2011 and the series diverge. The average treatment effect during the post-treatment period is -0.235, a 38% decrease relative to the baseline mean.

Michigan’s treatment effect is larger than that of any other state in 10 periods, larger
than that of all but 1 state in 6 periods, all but 2 states in 27 periods, and all but 3 states in 5 of the 123 post-treatment periods. In part due to Synthetic Michigan’s imperfect fit during the pre-treatment period, 13 control states have a larger post- to pre-RMSPE ratio than Michigan. Nonetheless, the Michigan’s large effect size relative to that of the placebo-treatments indicates that Michigan’s new time limit policy decreased adult TANF receipt.

7.2 Adult Self-Reported Welfare Receipt

Figure 6 shows the estimated effect of stricter time limits on adults’ self-reported annual welfare use. In the top-left panel, the line labeled “Arizona” shows the scaled probability of reporting welfare use within the past year. Arizona’s control group matches Arizona’s pre-treatment trend well. Arizona and its control group diverge after Arizona first shortens its time limit policy in 2010, but no clear positive or negative effect of time limits emerges during the post-treatment period. This is reflected in Table 4, which presents coefficient estimates and their standard errors. The estimated effect in Arizona is negative but statistically insignificant.

The remaining panels in Figure 6 display the estimated effects of stricter time limits on annual welfare use in Kansas, Maine[^32] and Michigan. While Maine and Michigan’s trajectories do not match those of their control groups as well as Arizona and Kansas’s do, pre-treatment trends appear parallel across all states. Kansas, Maine, and Michigan exhibit decreases relative to their control groups during the post-treatment period. Table 4 shows that Kansas and Michigan’s estimated treatment effects are statistically significant at standard levels. Kansas and Michigan’s time limit policies are associated with 24% and 15% decreases in annual welfare use, respectively.

Figure 7 shows graphs that test the plausibility of the parallel trends assumption. I estimate event-study models similar to Equation (5) but include treatment-year interactions in the years preceding and following the policy changes. I display 95% confidence bands and omit the year before the policy change so that coefficient estimates can be interpreted.

[^32]: Note that results in Maine are limited to those using ACS data due to the unreliability of administrative TANF counts in Maine.
as the relative change in adults’ scaled self-reported annual welfare use relative to the year before their state shortened its time limit. Figure 7 provides little evidence of any changes in welfare use prior to states’ policy changes. Kansas and Michigan, for which estimates from weighted difference-in-differences models are statistically significant, exhibit large decreases in welfare use during the post-treatment period. Taken together, estimates generated by ACS data provide support for the negative effects of time limits on welfare use found in Section 7.1.

8 Discussion and Conclusion

Overall, the results suggest that the introduction of stricter time limit policies causes decreases in welfare participation. The consistency of signs of estimates across datasets, empirical specifications, and state policy environments is reassuring.

Most of the estimates in this paper are larger in magnitude than those of the existing literature based on the effects of PRWORA, which suggests that stricter TANF policies may have larger effects in the post-PRWORA regime, but it is difficult to compare estimates of the effects of welfare reform in 1996 with more recent TANF reforms as policy changes differed considerably. Further, much of the existing literature estimates anticipatory effects of time limits, and expected effect sizes before time limits can be reached are smaller than those of the more recent policies that I study. Nonetheless, comparing the large decreases in welfare use that I find with changes in states’ proportions of adult TANF recipients who have received benefits for fewer months than the treatment states’ new TANF time limits allow (Figure 1) suggests that both mechanical and behavioral effects are at work.

I do not find systematic effect size differences among states based on their TANF caseload characteristics and policies at baseline or the new time limit policies that they implement. While effect sizes differ across state policy environments, this lack of systematic differences provides support for the external validity of large negative effects of time limits on welfare caseloads: other states that implement similar policies can expect to see their caseloads decline, regardless of their demographic and policy environments.

Results suggest that there is room for more research on how time limits affect other
outcomes, such as labor force participation decisions, child outcomes, and participation in other social safety net programs as spillover effects of stricter time limit policies into these areas may mitigate or amplify state budget savings from decreased welfare participation. For example, states need not pass through child support payments to TANF recipients. Instead, states may split these child support revenues with the federal government at the state’s Medicaid matching rate (US Department of Health and Human Services, Office of Child Support Enforcement, 2017). Hence, any decreases in TANF participation among child support recipients could lead to decreases in state revenues. Before fiscal year 2012, Michigan passed through up to $50 of child support income to TANF recipient families each month. Since fiscal year 2012, Michigan has not allowed TANF recipients to receive any child support income. Nonetheless, quantitative evidence suggests that Michigan’s child support collection revenues have been declining due to decreased TANF participation: estimated child support state revenues in Michigan totaled over $45 million in fiscal year 2008 but were less than $15 million in fiscal year 2015 (US Department of Health and Human Services, Office of Child Support Enforcement, 2017). Similarly, Arizona’s Department of Economic Security projects deficits in its Child Support Enforcement Administration Fund of over $5 million and $13 million during fiscal years 2018 and 2019, respectively, and cites decreased TANF eligibility as the “most [notable]” reason for this inauspicious revenue forecast (Arizona Department of Economic Security, 2017). Likewise, if stricter time limits induce increased labor supply among low-income individuals, state expenditures may increase through their Earned Income Tax Credit (EITC) programs. Additional research regarding these possible spillover effects will allow policymakers to consider both direct and indirect effects of time limits to implement optimal policy.
9 References


Haider, Steven J. and Jacob Alex Klerman. “Dynamic Properties of the Welfare Caseload.”


10 Tables and Figures

Figure 1: Characteristics of Adult TANF Recipients Over Time

Figure 2: Proportion of Adults by Time on TANF

(a) Proportion of adults who have received TANF for 36 months or less in Arizona and Alabama between fiscal years 2007 and 2016.

(b) Proportion of adults who have received TANF for 48 months or less in Kansas and Alabama between fiscal years 2007 and 2016.

(c) Proportion of adults who have received TANF for 60 months or less in Maine and New York between fiscal years 2007 and 2016.

(d) Proportion of adults who have received TANF for 60 months or less in Michigan and New York between fiscal years 2007 and 2016.

Proportion of adult TANF recipients by months on TANF. Information retrieved from US Department of Health and Human Services, Office of Family Assistance (2018).
Figure 3: Synthetic Control Estimates of Stricter Time Limit Policies on Adult TANF Receipt in Arizona

(a) Adults/1000 receiving TANF within each month in Arizona and its synthetic control group.

(b) Placebo tests for estimated treatment effect of stricter time limit policies in Arizona. The thick blue line represents Arizona, and the thin gray lines represent control states.

(c) Ratio of post- and pre-treatment RMSPE of placebo tests.
Figure 4: Synthetic Control Estimates of Stricter Time Limit Policies on Adult TANF Receipt in Kansas

(a) Adults/1000 receiving TANF within each month in Kansas and its synthetic control group.

(b) Placebo tests for estimated treatment effect of stricter time limit policies in Kansas. The thick blue line represents Kansas, and the thin gray lines represent control states.

(c) Ratio of post- and pre-treatment RMSPE of placebo tests.
Figure 5: Synthetic Control Estimates of Stricter Time Limit Policies on Adult TANF Receipt in Michigan

(a) Adults/1000 receiving TANF within each month in Michigan and its synthetic control group.

(b) Placebo tests for estimated treatment effect of stricter time limit policies in Michigan. The thick blue line represents Michigan, and the thin gray lines represent control states.

(c) Ratio of post- and pre-treatment RMSPE of placebo tests.
Figure 6: Weighted Difference-in-Differences Estimates on Adults’ Self-Reported Annual Welfare Use

Adults’ scaled self-reported annual welfare use in treatment states and their control groups.
Figure 7: Weighted Difference-in-Differences Event-Study Estimates on Adults’ Self-Reported Annual Welfare Use

Changes in adults’ scaled self-reported welfare use in treatment states relative to their control groups. Bars represent 95% confidence bands.
Table 1: State TANF Time Limit Policy Changes

<table>
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<th>Policy</th>
<th>Month Law Enacted</th>
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<td>Arizona</td>
<td>60- to 36-month time limit</td>
<td>July 2010</td>
</tr>
<tr>
<td>Arizona</td>
<td>36- to 24-month time limit</td>
<td>August 2011</td>
</tr>
<tr>
<td>Arizona</td>
<td>24- to 12-month time limit</td>
<td>July 2016</td>
</tr>
<tr>
<td>Kansas</td>
<td>60- to 48-month time limit</td>
<td>November 2011</td>
</tr>
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<td>Kansas</td>
<td>48- to 36-month time limit</td>
<td>July 2015</td>
</tr>
<tr>
<td>Kansas</td>
<td>36- to 24-month time limit</td>
<td>July 2016</td>
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<tr>
<td>Maine</td>
<td>N/A to 60-month time limit</td>
<td>January 2012</td>
</tr>
<tr>
<td>Michigan</td>
<td>Enforced 60-month time limit</td>
<td>November 2011</td>
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Table 2: State TANF Policies

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Policies for nonelderly nonexempt adults in standard state TANF programs as of July 2009. Maximum income levels are for initial eligibility of three-person families. Maximum benefit amounts are for three-person families. If the state continues to provide TANF benefits to families in compliance beyond 60 months, the time limit is listed as "-". Information retrieved from Welfare Rules Database, Urban Institute (2018).

* Lifetime time limit for adults only
Table 3: Adult Welfare Use

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Estimates of the effects of time limits on welfare participation. “Monthly TANF Receipt” indicates the state’s number of adult TANF recipients per 1000 adults. These are synthetic control estimates generated by the administrative data. The ranking of the treated state’s post- to pre-treatment RMSPE ratio, relative to those of the placebo-treatments, is listed in curly braces. “Annual Self-Reported Welfare Use” indicates the percent of individuals reporting welfare use within the past year. These are weighted difference-in-differences estimates generated using ACS data. Standard errors are clustered at the state level and listed in parentheses. Pre-treatment means are listed in brackets.
A Appendix

Table A1: Adult Annual Self-Reported Welfare Use

<table>
<thead>
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<th>State</th>
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</tbody>
</table>

Difference-in-differences estimates of the effects of time limits on welfare participation. “Annual Self-Reported Welfare Use” indicates the percent of individuals reporting welfare use within the past year in the ACS data. Standard errors are clustered at the state level and listed in parentheses. Pre-treatment means are listed in brackets.