The Social Safety Net and Mortality: Evidence from the Introduction of Welfare Payment Time Limits

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Abstract

We ask whether a decline in the social safety net can contribute to despair mortality (from drugs, suicide, or alcohol liver disease). We exploit the introduction of lifetime time limits on the receipt of welfare benefits to disadvantaged households, a substantial component of the U.S. welfare reform, which began in the 1990s. Our findings suggest that exposure to time limits beginning in the age of parenthood increased suicide and alcohol-related liver disease mortality among low-educated females. Our back-of-the-envelope calculations suggest that time limits contributed roughly 7.5% to low-educated female despair mortality increases through 2017.

Keywords: Welfare reform, deaths of despair, time limits, mortality

JEL Classification: H53, H71, I12, I14, I38

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1 Introduction

Deaths from drug overdoses, suicides, and alcohol liver diseases (ALD) have increased substantially in the U.S. over the past two decades, such that some population segments experienced a decline in overall life expectancy (Case and Deaton, 2017). Case and Deaton (2015) named this phenomenon "deaths of despair" and argue that rising trends in these causes of death are a result of worsening economic and social factors.

Researchers have established the significant role of supply factors related to either the pharmaceutical industry (Alpert et al., 2018, 2022; Arteaga and Barone, 2022; Evans et al., 2019; Fernandez and Zejcirovic, 2018; Kolodny et al., 2015; Quinones, 2015; Van Zee, 2009) or physician and insurance incentives and practices (Barnett et al., 2017; Buchmueller and Carey, 2018; Eichmeyer and Zhang, 2022; Schnell and Currie, 2018; Khan et al., 2019; Powell et al., 2020). Others have attributed the rise to demand factors driven by broad economic conditions or specific labor demand shocks (Betz and Jones, 2018; Carpenter et al., 2017; Hollingsworth et al., 2017; O'Brien et al., 2022; Pierce and Schott, 2020). Some argue, though, that these demand factors have a marginal role (at most) in increasing drug mortality trends (Ruhm, 2019; Currie and Schwandt, 2021). More recently, Giles et al. (2023) present evidence that a decline in religious participation could have also contributed to earlier mortality increases. Lastly, the consumption of ones' local peers matters, as (Finkelstein et al., 2018) found moving to a state with a higher rate of risky prescription opioid use increases an individual's probability of risky use.

Case and Deaton (2017) conjecture that changes in the social safety net could have also increased deaths of despair. Despite this argument, to our knowledge no study has evaluated the role of the U.S. welfare reform, one of the largest changes in the history of the U.S. social safety net. Our project fills this gap by investigating this vast reform to the U.S. welfare system that began during the 1990s and substantially reduced disadvantaged families' eligibility for welfare payments. Of the many reform components, we focus on the introduction of lifetime time limits, which restricted parents' welfare

payment eligibility to five years. This restriction is in sharp contrast to pre-reform eligibility criteria that sent low-income parents welfare payments as long as there was a minor in the household. Welfare payment time limits were the greatest departure from past welfare policy in the U.S. (Grogger, 2002, 2003, 2004; Mazzolari, 2007), thus representing a substantial decline in the social safety net available to disadvantaged families.

A priori, the effect of welfare payment time limits on despair mortality is ambiguous. While there is substantial evidence that time limits reduced welfare take-up[1] and improved labor market outcomes,[2] the overall effect of time limits on disadvantaged families' well-being is less clear. Reduced welfare take-up accompanied by improved labor market outcomes could imply greater economic independence, stability, or horizon. Alternatively, reductions in welfare take-up could have resulted in reduced cash support among families that were still in need of these payments. Indeed, the potential for heterogeneous effects resulting from welfare reform has been documented,[3] along with more general statistics of increases in child poverty over the reform's period.[4] Moreover, while improved labor market outcomes, even when resulting in greater household income, can enhance some disadvantaged families' overall well-being, it can also lead to adverse effects among other vulnerable families who are struggling to juggle between increased household financial constraints and childcare responsibilities.[5]

[1]See Chan (2013, 2017, 2018); Fang and Keane (2004); Farrell et al. (2008); Fitzgerald and Ribar (2004); Grogger (2003, 2004); Keane and Wolpin (2010); Lichtman-Sadot (2023); Mazzolari (2007); Mazzolari and Ragusa (2012); Swann (2005).

[2]See Chan (2013, 2017); Fang and Keane (2004); Fang and Silverman (2009); Farrell et al. (2008); Grogger (2003); Keane and Wolpin (2010); Lichtman-Sadot (2023); Low et al. (2020); Mazzolari and Ragusa (2012); Pepin (2022).

[3]See Parolin (2021) for welfare reform more broadly and Bitler et al. (2006) for a specific assessment of time limits in the context of Connecticut's Jobs First waiver in the mid-1990's. For additional studies that present adverse effects on household income or poverty rates following welfare reform, see Hicks et al. (2023) for Canada and Waldfogel (2007) for the U.S.

[4]E.g., Greenstein (2016) and Trisi and Sherman (2016).

[5]Indeed, Hicks et al. (2023) find that expanding job search requirements for welfare payment recipients in Canada resulted in increased employment for these mothers and even reductions in poverty rates due to the new employment income. However, this was accompanied by increased consumption of mental health medication and the authors find other measures of increased stress, such as reduced time available for non-urgent medical care. Recall that in the Canadian context there is no concern for changes in health insurance coverage due to increased labor force participation.

Two studies are closely related to our work in their evaluation of the relationship between two social safety net programs and mortality. Dow et al. (2020) find that Earned Income Tax Credit (EITC) expansions decreased non-drug suicides. Heflin et al. (2019) find that participation in the Supplemental Nutrition Assistance Program (SNAP) reduces premature adult all-cause mortality and suggestive evidence for reductions in despair mortality. Both these studies support Case and Deaton (2017)'s hypothesis regarding the potential for a negative correlation between the level of the social safety net and despair deaths. However, they do not contribute to solving the puzzle of rising death trends over the last few decades, as the EITC was expanding during Dow et al. (2020)'s sample period[6] and SNAP participation has also increased as more states facilitated access to this benefit. Thus, increases in despair deaths cannot be driven by these specific changes in the social safety net. The evidence in Dow et al. (2020) and Heflin et al. (2019) actually suggests that rising death rates may have been mitigated due to EITC and SNAP expansions.

Our project addresses two important questions. First, we explore the role of the social safety net and its effects on despair deaths. Second, we investigate whether a decline in the social safety net contributed to the rising trends in despair mortality. The first question has been partially addressed in Dow et al. (2020) and Heflin et al. (2019) through different social safety net programs. The second has not been empirically addressed to date in the U.S. context to the best of our knowledge. For the UK, Berman and Hovland (2024) is a recent working paper that finds evidence of increased mortality - including drug overdoses - in response to austerity measures that began in 2010 and included reductions in welfare payments. The findings in Berman and Hovland (2024) complement our work by demonstrating the potential for welfare payment reduction to increase mortality from one component despair mortality, drug overdoses.[7] Both works reinforce the external validity of our findings and strengthen

[6]Dow et al. (2020) use variation across states in state-level EITC supplements. Only three states discontinued their EITC supplements during 2000-2014 (Agan and Makowsky, 2023). This is accompanied by over 15 states introducing state-level EITC supplements (based on data from Kleven 2019).

[7]Our paper does not find statistically significant effects of welfare payment time limits on drug overdose mortality and this may be driven by differences between the U.S. and the UK in the nature of the opioid

the notion that declines to the social safety can adversely affect mortality not just in the context of one particular country.

Our analytical framework exploits two main sources of variation to identify the effect of time limits on despair mortality. One is variation across states regarding the timing of time-limit introduction. The second is variation across cohorts in the degree of exposure to time limits (upon introduction), as time limits should have only affected individuals beginning in the age of parenthood. Combining these two sources of variation generates variation in exposure to time limits within state-year units that strengthens our empirical design considerably. In particular, we are able to control for any contemporaneous state-level conditions or policies that may have also affected mortality. Moreover, this relatively rich variation across cohorts within state-year units allows us to more confidently rule out that our results are simply driven by simultaneous rising trends in both exposure to time limits and despair mortality. Lastly, our regression results demonstrate statistically significant responses to exposure to time limits among low-educated females and null responses among low-educated males, despite similar rising trends in overall despair mortality for both genders (see Figure 2). This suggests that our variation is indeed capturing something that is unique to females and much less relevant to males, e.g. welfare payment time limits. Our results are further validated with findings of null effects among high-educated females, who should not be affected by welfare payment restrictions.

Our estimates suggest that time limits contributed approximately 7.5 percent of the rise in despair mortality among females with a high school degree or less between the years preceding time limits (1989-1992) and the end of our sample period (2014-2017). The magnitude of the estimated effects is greater when the sample is narrowed to states that had a poverty rate exceeding the median as of 1989. The estimated effect translates into more than 23,000 deaths over a 20-year period. Considering that the observed increases in despair mortality may be accompanied by reductions in mortality

epidemic and its sources. Berman and Hovland (2024) do not evaluate suicide and alcohol-related liver disease mortality, the despair mortality components that drive our mortality responses to time limits, and this is likely due to small cell sizes in their analysis.

among other disadvantaged households that experience improved overall well-being in response to welfare payment time limits, our results likely underestimate the overall adverse effects of this policy on mortality.

The remainder of the paper is organized as follows. In Section 2, we provide background on the U.S. welfare reform and the time limits imposed on eligibility for welfare payments. Section 3 discusses the data used for this study. Section 4 presents our empirical strategy and discusses our identification. Section 5 presents our main results, followed by robustness checks in Section 6. Section 7 concludes.

2 Background

In the U.S., the welfare system provides cash transfers to needy families thereby assisting disadvantaged households to meet their housing, food, and healthcare needs. With the passage of the Personal Responsibility and Work Opportunity Act (PRWORA) of 1996, the Aid to Families with Dependent Children (AFDC) was replaced with the more restrictive Temporary Assistance for Needy Families (TANF), which transitioned the U.S. welfare system from a federal entitlement program to a work-conditioned and time-limited benefit program.

The curtailing of AFDC benefits actually preceded PRWORA, given state-led reforms that became popular in the early 1990s. These reforms were implemented under waivers that were granted federally to states who wished to change their welfare program. Time limits on the receipt of welfare payments were among the more common waivers, ranging from 21 to 60 months. Under AFDC, eligibility for welfare payments was guaranteed until one's youngest child reached age 18. Upon implementation of PRWORA, welfare payments were federally limited to five years. States could offer lifetime welfare payments for more than five years at their own expense or restrict time limits even further than what was federally provided (using the funds for other welfare-related measures). In this paper, we exploit this variation generated across states in the timing of time-limit implementation. The policies range from Arkansas,

Colorado, and Georgia which had adopted time limits already in 1992 to Vermont (the last state) which implemented time limits only in 2014. Figure 1 maps the variation across states in the year time limits were introduced.

Most welfare payment recipients have historically been and remains to date females, and more specifically single mothers. While there is evidence that welfare payment time limits also affected disadvantaged male labor market outcomes primarily through male partnerships to female adult recipients (Lichtman-Sadot, 2023), this potential exposure to time limits should be relevant to a substantially smaller fraction of low-educated males in comparison to low-educated females. We therefore do not expect to find meaningful (if any) effects on males when using aggregate data, as in our mortality analysis. If mortality responses to time limits are found among females but not among males, despite use of the same empirical strategy and similar variation for both genders, then this is evidence that our empirical strategy and variation are capturing changes that are more relevant to females, as opposed to males.

3 Data

3.1 Welfare Payment Time Limits

The Welfare Rules Database (WRD) from the Urban Institute documents the initial time limit implementation for each state. Figure 1 presents a map of the U.S. with the varying shades in each state representing the year its first time limit was introduced.[8] Iowa and Washington D.C. are excluded from the analysis as time limits were implemented on an individual basis in the former and repealed four years after introduction in the latter. We 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

[8]We 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.

California and Maryland.[9]

3.2 Mortality Data

Restricted-use mortality data was made available from the Center for Disease Control and Prevention (CDC) National Center for Health Statistics for the years 1989-2017.[10] We focus on those who were reported to have a high school education or less in their death record. We also utilize those who are documented as having a college degree or more as a placebo group, under the assumption that high-education individuals should not have been affected by changes to welfare payment eligibility. Despair mortality rates vary substantially based on gender, so there is reason to believe that different policies differentially affect these rates by gender. Furthermore, despite evidence that the U.S. welfare reform affected disadvantaged males (Lichtman-Sadot, 2023), it was substantially more likely to directly affect disadvantaged mothers, and when examining aggregate data, it is highly plausible for male effects to be null. For these reasons, we examine mortality separately by sex.

We sum the number of deaths for each gender by state, year, and low versus high education level of the deceased, and for four age ranges: 25-34, 35-44, 45-54, and 55-64. Our deceased span ages 25-64, as in other research examining despair mortality (Case and Deaton, 2022). Our despair mortality measures are composed of deaths resulting from drugs, suicide, or ALD. We follow the ICD coding used by the CDC to calculate the number of annual deaths attributed to these causes for each state-sex-age range-education level combination.[11] We obtain mortality rates per 100,000 population by

[9] A review of the vast literature on time limit implementation across states has revealed several discrepancies across studies documenting the timing of time limits. We thus independently constructed our database of time limit introductions using the Urban Institute WRD, in addition to several rounds of communication with the Center on Budget and Policy Priorities. In a recent study by Pepin (2022) evaluating time limit introductions and expansions in the 2010's (and discussing introductions prior to this period), all time limit introductions are identical to the documentation in our database.

[10] 1989 is the first year with the education level of the deceased.

[11] Causes of death are based on ICD-9 codes for years prior to 1999 and on ICD-10 for years from 1999 onward. Drug-related ICD-9 codes: E850-E858, E950.0-E950.5, E962.0, E980.0, E980.0-E980.5. Suicide ICD-9 codes: E950-E959. Alcohol liver disease ICD-9 codes: 571.0-571.3. Drug-related ICD-10 codes: X40-X44, X60-64, X85, Y10-Y14. Suicide ICD-10 codes: X60-X84, Y87.0-Y87.2. Alcohol liver disease ICD-10 codes: K70.0-K70.4, K70.9. Given the change in the ICD causes of death coding, all results are presented

using population bases for these specific cells (at the state, year, gender, age group, and education level) from the CPS data. Our analysis is for mortality rates at relatively narrow age ranges (10 years). Nevertheless, concern can still arise that our results are affected by different state-specific composition changes in the age distribution (as noted in Ruhm (2000) and Finkelstein et al. (2024)) . We therefore present results of robustness checks with age-adjusted mortality rates in Section 6.6, and these are highly similar to our baseline results.

Education levels are not fully populated for all death records in the mortality data. To deal with this, we exclude all state-year cells (for all age ranges and both sexes) that had missing or unknown education levels for over 5% of despair deaths.[12, 13] We then take the deaths with missing/unknown education levels and allocate them across education categories based on the imputation procedure in Case and Deaton (2017). Specifically, we use the education distribution of observed death records within each year-state-age group cell.[14]

Figure 2 uses our data to show trends in aggregate despair mortality rates by sex, education level, and ten-year age groups spanning ages 25-64 during our sample period, 1989-2017. The figure confirms past investigations concerning rising despair mortality rates that have shown that the increase is much greater among low-educated Americans (Case and Deaton, 2022; Novosad et al., 2022). Furthermore, in addition to having greater despair mortality rates at the start of the sample period, males experienced greater increases as opposed to females. Lastly, Figure 2 presents variation across different age groups in the changes in mortality. For example, while the sharpest increase

also when limiting the sample from 1999 onward - see Section 6.

[12]This results in excluding 18-25 percent of state-year cells from our analysis, depending on the dependent variable. Dow et al. (2020) deal with missing/unknown education levels by entirely excluding four states that had very high levels of missing/unknown education levels during their sample period. We view our exclusion approach as preferable within our setting, as our regression specifications include state-year fixed effects (in contrast to specifications in Dow et al. (2020) that have separate state and year fixed effects). As such, we can exclude cells based on missing/unknown education levels at the state-year level, which increases the precision of the exclusion criteria.

[13]The 5% threshold was selected to balance between not having too many unknown education levels and not having to exclude too many state-year units. As we demonstrate in our robustness checks, our results are robust to lower thresholds of exclusions. When the exclusion threshold is greater, our results maintain qualitative consistency, although precision declines substantially.

[14]In the absence of using this imputation procedure, our results are similar although with lower precision.

in male low-educated despair mortality is among ages 25-34, among low-educated females this age group actually experienced the least dramatic increase. Our analysis will exploit and take into account all these aspects of variation - across gender, age groups, and education levels.

3.3 Explanatory Variable

The main explanatory variable is the mean number of years each age group's 10 cohorts were exposed to time limits, under the assumption that exposure begins when the age of parenthood begins. We calculate the age of parenthood using CPS data. Specifically, we take the range between the 2.5^{th} and 97.5^{th} percentile of the empirical distribution for the ages of all individuals earning up to 200% of the poverty threshold in the CPS March supplement data between 1992 and 2017 that have at least one child under 18. We do this separately by sex, resulting in ages 21-48 and 24-53 being the age of parenthood for females and males, respectively.

We calculate for each cohort in every year the number of years they have been exposed to time limits based on exposure beginning at the latest of either when time limits are introduced or at the minimal age of parenthood, conditional on beginning before the maximal age of parenthood. Cohorts that were above the maximal age of parenthood when time limits were introduced in their state receive zero years of exposure to time limits all throughout the periods they appear in the sample. Finally, we calculate for each age range (recall that our observations are for 10-year age ranges) the mean exposure to time limits across the ten cohorts comprising it. Appendix Figure A1 provides a graphical example of the female years of exposure to time limits for several cohorts in two states that introduced time limits in different years - Florida, which introduced time limits in 1996, and Michigan, which introduced in 2008. Appendix Figure A2 provides two visual presentations of the variation in the female years of exposure variable - both across age groups and within state-year units. Our identification relies to a large extent on the variation within state-year units, and Appendix Figure A2B does indeed demonstrate such variation, even among states that

introduced time limits in later years and have less cohorts exposed to time limits in most sample years.

3.4 Summary Statistics and Data Plots

Table 1 presents summary statistics of the data utilized. In addition to crude death rates for all despair-related causes of death, we also present the breakdown for drug-related, suicide, or ALD deaths separately as reference to the composition of despair-related deaths across genders and age groups. As can be seen, mortality rates from despair causes of death are much higher among low-educated as opposed to high-educated and also among males as opposed to females.

Figure 3 presents trends across age groups over time in exposure to time limits beginning during the age of parenthood by sex. As with mortality rates (see Figure 2), our explanatory variable measuring years of exposure to time limits is also rising over time, as more states introduced time limits and more years elapsed since the introduction of time limits. However, there is variation across age ranges in the years of exposure over time. Moreover, when comparing Figure 3 with Figure 2 in Section 3.2, one can notice that the magnitudes of the changes in time limit exposure over time across age ranges match much more closely the magnitudes of the changes in female despair mortality across age ranges than the corresponding changes in male despair mortality. Specifically, the increase in exposure to time limits over the years is greatest for ages 45-54 and smallest for ages 25-34, which matches the female differences across age ranges in despair mortality changes over time but much less so the male differences. This suggests that despite both despair mortality and time limit exposure rising over time, the correlation between time limit exposure and despair mortality across age ranges will be stronger for females than for males.

This stronger correlation between our dependent and explanatory variables for females as opposed to males is exhibited in Figure 4. The figure plots mean despair mortality rates (across states) for each year and age range along with their mean exposure to time limits, separately for low-educated females and males. All despair

mortality measures are relative to a normalized value of one for the mortality rate for the first year in the data, 1989, thus facilitating the comparison between males and females who have very different levels of mortality rates. The correlation exhibited is much stronger in the female data as opposed to the male data. This is despite the similarity in the years of exposure variable constructed for both sexes. This is evidence that our years of exposure variable may have explanatory power for rising trends in despair mortality that are unique to females and less relevant to males.

4 Empirical Strategy

4.1 Regression Specification

Our regression analysis explores how mortality rates for different age ranges changed over time as a function of their exposure to time limits beginning at the age of parenthood. The analysis is separate for each gender and for high versus low educated individuals, and observations in the dataset are at the age group-state-year level.[15] Variation is not only across time and states, as four different age groups are accounted for in each state-year combination, and these had different exposures to time limits based on when their age of parenthood began.

Given the relatively narrow population segments, we have a substantial number of observations with zero death counts for deaths of despair. Since standard OLS/WLS regressions with so many zero counts can potentially bias estimates, our specification is run using a Poisson Pseudo Maximum Likelihood (PPML) regression model.[16] This results in the following regression specification:

[15]Our education levels are either a high school education or less or a college degree and above, thereby excluding deaths with more than 12 years of schooling but less than a college degree.

[16]The relatively large share of zero death counts for our fine population segments can suggest using the inverse hyperbolic sine (IHS) function, as in Dow et al. (2020). However, more recently, Norton (2022) and Chen and Roth (2024) have presented disadvantages of using the IHS in terms of obtaining the correct marginal effects. Due to this argument, we present our results for this study with PPML regression specifications. Our results are nevertheless robust to using IHS transformations as dependent variables instead.

$$Deaths_{ast} = \exp\{\beta YrsExposure_{ast} + \gamma_a + \theta_{st} + \mu_c + \rho_a t_t + \ln(population)_{ast}\} + \epsilon_{ast} \quad (1)$$

The dependent variable is the number of deaths for age group a in state s in year t. Because this is the count of deaths and not the rate, the regression controls for the natural log of the relevant population and constrains its coefficient to be equal to one. The regression further controls for age group (γ_a) and state-year (θ_{st}) fixed effects, as well as age-group-specific linear time trends ($\rho_a t_t$). It also controls for cohort fixed effects in 10-year intervals (μ_c) – each age group in a specific year is assigned a dummy variable depending on whether one of its birth years is 1940, 1950, 1960, 1970, 1980, or 1990 (which are mutually exclusive given the 10-year age ranges). The coefficient of interest in equation 1 is β. It estimates the percent change in deaths in response to each year of exposure to time limits.[17]

4.2 Identification

The regression specification outlined in equation 1 relies on two main sources of variation: across states in the timing of time-limit introductions and across cohorts in the extent of their exposure to time limits beginning at the age of parenthood. The latter source of variation is exogenous as the cohort of the deceased was determined before welfare reform.[18] For the former source of variation to be considered plausibly exogenous, we need to verify that welfare time limits were not correlated with state-level characteristics prior to their introduction.

We rule out a correlation between the timing of time limit introductions and state-level characteristics prior to or on the eve of these introductions in Figure 5A which

[17] With the Poisson specification, coefficient estimates are interpreted as the percent increase (divided by 100) as long as the coefficient estimates are sufficiently small (< 0.2), meaning a 0.1 coefficient estimate implies 10% more deaths on average in response to an additional year of exposure to time limits.

[18] The youngest cohort analyzed in the mortality data was 25 years old in 2017, so they were born in 1992, the year the first time limits were introduced.

plots values for the following characteristics as of 1989-1992 (depending on data availability): despair mortality, population, unemployment, poverty, median household income, voting patterns, racial composition, and educational attainment. All correlation coefficients are relatively small in magnitude and the lowest p-value is 0.35.[19] Appendix Figure A3 presents similar plots to those in Figure 5A but rather than the values as of 1989-1992, the vertical axes are differences between 1989-1992 and 2008-2010 (exact years depend on data availability), and a lack of correlation can rule out the possibility that one of our main sources of variation is correlated with other state-level changes during the sample period that may be driving despair mortality changes. Here too the correlations are weak, with the exception of changes in the democratic vote rate, the only characteristic for which the p-value of the test for a correlation is under 0.1.

In order to interpret the coefficient estimates in equations 1 as causal, we also need to ensure that our results are not driven by other policies that were implemented at the state level and may have also affected despair mortality. To a large extent, our regression specification addresses this concern as it includes state-year fixed effects. Thus, we overcome an often-raised challenge when evaluating the effects of the U.S. welfare reform: that there were many other policies taking place during the same period that may have also affected disadvantaged households (and possibly their health and mortality). Given the inclusion of state-year fixed effects in our regression specifications, in order for other state-level policy measures to jeopardize our results, their intensity needs to be correlated with our varying measures of exposure to time limits across cohorts.

Despite our ability to control for state-level policies that changed over time through the inclusion of state-year fixed effects within our regression framework, we further

[19]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. 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).

confirm our identification assumptions by presenting evidence that the timing of time limit introductions is not correlated with other policy measures during this same period. Figure 5B shows this for state-level policies. In the first row of Figure 5B we examine three employment-related policies that were implemented or expanded during this period and affected low-income households: the EITC, minimum wages, and JOBS sanctions, which were part of TANF.[20] The EITC and minimum wages have been shown to decrease despair mortality (Dow et al., 2020). In the bottom row we examine three drug-related policies: Prescription Drug Monitoring Programs (PDMP), access to Naloxone, and legalization of marijuana for medical use.[21] All graphs plot the year of the stated policy's introduction along with the year time limits were introduced, with the exception of the graph for minimum wage, which plots the level of the real minimum wage as of 1992.

Our results can also be compromised if over time there is negative selection of those who have a high school education or less and this may be correlated with the introduction of time limits. Indeed, Novosad et al. (2022) show how this negative selection can bias changes in mortality measures over time among less educated Americans. However, our regression specification addresses this concern through the inclusion of cohort fixed effects, which control for the different compositions of high versus low educated in each cohort. Furthermore, in Novosad et al. (2022) this selection concern is most pronounced when examining those without a high school education, as opposed to examining those with a high school education or less in our analysis.[22]

An additional threat to our identification can arise if there is a migration response

[20]State-level EITC introductions are from Leigh (2010) and supplemented for more recent years with the data used in Kleven (2019). Minimum wages through 2013 are obtained from data for Meer and West (2016) and for later years from the US Department of Labor (https://www.dol.gov/agencies/whd/state/minimum-wage/history). JOBS sanction 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.

[21]PDMP data is from http://pdaps.org/datasets/prescription-monitoring-program-laws-1408223332-1502818372 and has also been utilized in Williams et al. (2019). Naloxone data is from http://pdaps.org/datasets/laws-regulating-administration-of-naloxone-1501695139. Medical marijuana legalization data is from Williams et al. (2019).

[22]Case and Deaton (2022) also make a similar argument against concern for selection when evaluations of the low-educated include high school graduates.

to time limits and individuals negatively or positively select into or out of states with time limits. We check for this in Appendix Table A1 using CPS March Supplement data. Our regression results rule out such migration patterns.

5 Results

5.1 Can our Explanatory Variable and the Broad Population Base Explain Reductions in Welfare Payment Receipt?

Our analysis focuses on despair mortality within a relatively broad population base - females with a high school education or less. We do not directly observe income levels for these females or whether they ever had any children and yet we argue that we can detect for this population changes that are related to welfare payment receipt or eligibility. Moreover, our explanatory variable is a measure of cumulative exposure to time limits based on the age of parenthood for disadvantaged adults. This is a rough measure of exposure to time limits that has to our knowledge never been examined in evaluations of time limit effects. It does not, for example, take into account one's age of youngest child, which has been shown to have substantial explanatory power in analyzing the effects of welfare payment time limits (Grogger, 2002, 2003, 2004; Low et al., 2020; Lichtman-Sadot, 2023; Mazzolari, 2007; Michalopoulos, 2004).

With this in mind, we increase the validity of our explanatory variable and empirical framework by presenting evidence that even in this broad population of females with a high school education or less, and while using our explanatory variable, we can obtain explanatory power and show a negative relationship between exposure to time limits and welfare payment receipt. We do this using the CPS March Supplement data for 1989-2017 (same period used for the mortality analysis) and the empirical framework presented in Section 4. Table 2 presents results both for an individual-level analysis and an aggregate analysis in columns 1-2 and 3-4, respectively. The aggregate analysis is at the state, year, and age-group level, as in our mortality analysis.

For the individual-level analysis, we restrict the sample to females ages 25-48[23] and run regressions with an indicator variable for welfare payment receipt over the past year as the dependent variable. With our dependent variable no longer being a count variable, the specification is OLS, rather than PPML. Our explanatory variable for these regressions is the number of years of exposure to time limits, and this is a function of each female's state of residence (and therefore the year she experienced time-limit introduction) and the year she was born. Besides our years of exposure variable of interest, our regression specification includes state-year fixed effects, age group fixed effects, cohort fixed effects, and age-group-specific linear time trends, as in equation 1.

For the aggregate analysis, we evaluate 10-year age groups for age ranges 25-64, as in the mortality analysis. Our explanatory variable is identical to the explanatory variable in equation 1. Our welfare payment measure begins with a weighted sum of welfare payment cases at the state, year, and age-group level. Due to the weights, this variable is not an integer, thereby not suited to a Poisson regression. We therefore take the rate of welfare payment receipt (based on the weighted sum and dividing this by the relevant population base) and deal with the large share of zero values by employing an Inverse Hyperbolic Sine (IHS) transformation. This is our dependent variable within an OLS framework in columns (3)-(4) of Table 2.

Results in Table 2 show statistically significant reductions in the probability of receiving welfare payment in response to each year of exposure to time limits. The magnitude of the effect is slightly greater when limiting the sample to states that had a 1989 poverty level that exceeded the median, as is demonstrated by comparing the results of the first and second columns and the third and fourth columns. This is very reassuring. Despite the sample being solely limited to females with a high school education or less, some of which are likely not mothers, we observe meaningful reductions in welfare payment receipt. In fact, only roughly 75% of the females in the individual-level samples in columns 1-2 in Table 2 had at least one child under 18

[23]Recall that the female age of parenthood is 21-48 and our mortality data begins at age 25.

present in the household. Furthermore, even at the aggregate level analysis in columns (3)-(4), the level of our mortality analysis and with an explanatory variables that is not individualized, we observe statistically significant declines.

Our estimates suggest that each year of exposure to time limits on average reduced welfare payment receipt by 0.1 percentage points (for the sample of all states). With a mean of roughly 9 years of exposure in the sample (for post-time-limit introduction observations), this implies a mean decline of roughly 1 percentage point. For the sample of females ages 25-48, this decline represents 13 percent of pre-time-limit mean welfare payment receipt levels. We note that this reduction, although statistically significant, is much smaller than past estimates of the effects of time limits on welfare payment receipt.[24]. This is also reassuring, as our sample has a smaller share of females potentially eligible for welfare payments than past studies that generally focus on single mothers with a high school education or less. Furthermore, our explanatory variable is a rougher measure of exposure to time limits.[25]

[24]Fang and Keane (2004) use CPS data and find that absent time limits the drop in welfare payment receipt would have been 2.5 percentage points less. Grogger (2003) in his analysis with CPS data finds that for mothers with a youngest child who is 13, time limits reduced welfare payment by 0.66 percentage points, but as one's age of youngest child is smaller, welfare payment declines resulting from time limits increased and were as high as 6 percentage points for mothers with a youngest child who was 3. Other studies that examine datasets besides the CPS to evaluate the effect of welfare payment time limits on female welfare payment receipt find effects that are 25-60 percent of pre-treatment means (Chan, 2013; Keane and Wolpin, 2010; Lichtman-Sadot, 2023; Low et al., 2020; Mazzolari, 2007; Pepin, 2022; Swann, 2005)

[25]While the results in Table 2 can suggest a "first stage" that is part of an instrumental variable (IV) approach, we note two important factors that do not enable us to carry out such an analysis. First, the aggregate analysis in columns (3)-(4), which is at the same level as the mortality analysis, represents the relevant results for a "first stage". However, although the declines are statistically significant at the 5% level, they are not sufficiently strong for a "first stage". Second, while the decline in welfare payment is likely to be one important channel through which despair can increase due to the introduction of time limits, this is not the only channel. As discussed above, welfare payment time limits represent a substantial decline in the social safety net available to disadvantaged families. This safety net is not always and necessarily utilized but its presence and one's ability to rely on it can have a profound effect on an individuals' well-being. Furthermore, increased stress can also result from greater employment within these highly fragile households. This may not necessarily be a direct response to declining welfare payment receipt but rather a response to the possibility of being ineligible for welfare payments sometime in the foreseeable future. Taken as a whole, the results in Table 2 strengthen the validity of our analytical framework - in terms of the sample and explanatory variable used - but cannot provide the foundations for an alternative specification within an IV framework.

5.2 Regression Results

We present results from equation 1 in Table 3 in four panels: first, for females with a high school education or less; then for these same females but in states that had poverty levels above the median as of 1989; in the third panel, for females with a college degree or more; and lastly, in the fourth panel for males with a high school education or less. If the first panel exhibits some effects of time limits on mortality, then we expect the coefficient estimates in the second panel to be of greater magnitude and/or precision, as we should be capturing more females that are potentially affected by time limits in higher-poverty states. The third panel is a placebo test, as we do not expect females with a college degree or more to be affected by time limits. Males were not directly affected by welfare payment limitations to the same extent that females were, and despite evidence of male labor market responses to welfare payment time limits (Lichtman-Sadot, 2023), the share of these affected males in the overall population of males with a high school education or less should be much smaller than the share of affected females within the population of females with a high school education or less. We thus expect null to potentially very small effects in the fourth panel that presents regression results for the male low-educated population.

Each panel in Table 3 presents the coefficient estimate β from equation 1 for four dependent variables - despair mortality, drug overdoses, suicides, and ALD. The results are aligned with our hypotheses. The top panel suggests that despair deaths increased for females with a high school education or less by 1.32 percent for each year of exposure to time limits. Given a mean exposure rate of 7.08 in the regression sample (see bottom row of each panel in Table 3 for means of time limit exposure in each sample), this implies that time limits increased average annual female despair deaths by 9.3 percent (1.32 multiplied by 7.08) between 1989 and 2017. The top panel also suggests that of all three despair mortality components, suicides may have been most strongly affected by time limits with a p-value of 0.18 on the estimate for β, in comparison to p-values exceeding 0.6 for the coefficient estimates of drugs and ALD.

The second panel of Table 3 presents larger effects on despair, suicide, and ALD

when limiting the sample to high-poverty states. The coefficient on the effect of time limits on suicides is statistically significant at the 10 percent level and for ALD the p-value is 0.108. Both coefficient estimates suggest a one percent increase in the respective mortality rate for each year of exposure to time limits. For females with a college degree or more, there is no evidence of time limit effects on despair mortality. The last panel of Table 3 shows no effects of time limits on male despair mortality.

The summary statistics in Table 1 show very large differences between death rates among low-educated individuals as opposed to their high-educated counterparts. For some age groups and particular causes of death, death rates among low-education individuals are more than five times those among high-educated individuals. This can raise concern that the null effects observed in Table 3 for high-education deaths are due to lack of statistical power rather than there being no effect among those that we do not expect to be affected by time limits. However, this argument does not consider that some of the low-education death rates for specific causes of deaths are lower than the high-education despair mortality rates. For these causes of deaths – namely, ALD for females - we observe marginally statistically significant responses to time limits among the low-educated, despite the small death rates, thereby ruling out the argument that with such low death rates there is insufficient statistical power to detect an effect.

5.3 Dynamic Effects

Equation 1 estimates the effect of each additional year of exposure to time limits and makes the parametric assumption that this effect is constant. We wish to examine the effects of time limits non-parametrically, and this will also allow us to evaluate whether the effects change over time or to what extent they persist in the long term, more than 15 years after exposure to time limits began. To this end, we estimate the following regression specification:

$$Deaths_{ast} = \exp\{\sum_{j=-3,j\neq 0}^{4} \alpha^j TLExposure_{ast}^j + \gamma_a + \theta_{st} + \mu_c + \rho_a t_t + \ln(population)_{ast}\} + \epsilon_{ast} \quad (2)$$

Equation 2 replaces the $YrsExposure_{ast}$ variable from equation 1 with four indicator variables for the following levels of years of exposure to time limits: $(0, 5], (5, 10], (10, 15]$, and more than 15, represented by $TLExposure_{ast}^j$ for $j \in [1, 2, 3, 4]$, respectively.

We further exploit the fact that a non-parametric regression specification can allow us to test for trends prior to the introduction of time limits. We therefore calculate for each cohort the number of years they appear prior to the introduction of time limits, conditional on being at the age of parenthood when time limits are introduced (and therefore being exposed to time limits when they are introduced). Based on means for this calculation of years prior to exposure to time limits at the age group level, we construct three indicator variables for the following levels of years prior to exposure to time limits: $(0, 3], (3, 6]$, and more than six. We note that we are relatively constrained in terms of the number of years we can examine prior to time-limit introduction, as our data only begins in 1989 and by 1998, 40 states in our data already introduced time limits. The changes in mortality rates for these periods, prior to time-limit introductions, are represented in equation 2 by the coefficients α^j for $TLExposure_{ast}^j$ for $j \in [-1, -2, -3]$. Age groups with zero exposure to time limits (across all ten cohorts) are the excluded group in this regression specification.

The results of our dynamic analysis are presented in Figure 6. The figure plots the coefficient estimates α^j for $j \in [-1, -2, -3, 1, 2, 3, 4]$ from equation 2 along with their 95 percent confidence intervals. Two main conclusions arise. First, we can rule out pre-existing trends in despair mortality among cohorts that will be treated when time limits are introduced. This is demonstrated by the pre-time-limit-introduction coefficient estimates, which are not statistically significant and out of which two are

very close to zero.[26] Second, despair mortality responses to time limit exposure appear to be positive even in the long-term. The right-most coefficient estimate plotted in Figure 6 suggests that despair mortality among low-educated females with more than 15 years of exposure to time limits is roughly 10 percent greater than despair mortality among the same females with zero exposure to time limits.

Figure 6 presents a close-to-zero and statistically insignificant coefficient estimate for the short term effect of time limits (< 5 years of exposure to time limits). This can be due to two possibilities: either the effects of time limits on mortality are cumulative and take time to materialize in a statistically significant manner, or this null result can be due to many of the 10-year age-range observations with these low levels of mean years of exposure to time limits also including cohorts that have zero exposure to time limits. Under the latter possibility, the null result may just be due to changes in mortality among cohorts with zero exposure included in the estimation of less than five years of exposure to time limits. Unfortunately, we are unable to determine which of the two possibilities is driving the null results. Thus, limitations in our data do not enable us to determine whether exposure to time limits affects mortality already in the short term or not.

5.4 Quantifying the Effects of Time Limits on Despair Mortality

The results in Table 3 suggest an increase of 1.32 percent in the number of low-educated female despair deaths in response to each additional year of exposure to time limits. As discussed in Section 5.2, this implies that annual female despair mortality increased by 9.3 percent on average during our sample period due to welfare payment time limits. The mean female despair death rate in our sample is 37.8 per 100,000 females. Thus, our estimates suggest that time limits on welfare payments contributed on average 3.5 (9.3 percent of 37.8) deaths per 100,000 low-educated females ages 25-64 annually. Among

[26]The coefficient estimate for more than zero and less than three years prior to exposure to time limits is not statistically significant at conventional levels but does have a p-value that is smaller than 0.15.

low-educated females ages 25-64 in high-poverty states, a similar calculation based on the 1.77 point estimate and mean despair death rate and time limit exposure presented in the second panel of Table 3 yields that for high-poverty states, time limits increased low-educated despair mortality by an average of 5.5 deaths per 100,000 females ages 25-64.

To grasp at the potential contribution that time limits on welfare payments had to increased despair mortality during our sample period, we first calculate the difference in the female low-education despair mortality rate between the early years of the sample period, when no time limits were in place, and the last years of the sample period. Specifically, in our regression sample, the mean despair mortality rate among low-educated females in 1989-1992 was 13.2 per 100,000 population. During 2014-2017, this was 59.5, thereby increasing by 46.3. Therefore, an increase of 3.5 in the low-educated female despair mortality rate represents 7.6 percent (3.5/46.3=0.076) of the increase in this rate during the sample period. This is substantially smaller than the 79% contribution to drug overdose deaths due to OxyContin's launch and marketing in states that did not monitor physician prescription practices that is found in Alpert et al. (2022). However, it is only slightly smaller than the 10-15 percent increase in drug overdose mortality rates that Pierce and Schott (2020) find in response to an interquartile shift in their local measure of exposure to greater import competition from China.

Giles et al. (2023) find that blue law repeals in the U.S., which led to a decline in religious participation, explain 40% of the rise in despair mortality prior to the introduction of OxyContin and the sharp increase in deaths resulting from drug overdoses. While this also represents a much larger contribution to increased despair mortality than our 7.6% calculated contribution, we note that their estimated effects were substantially smaller than ours. Blue law repeals increased annual despair mortality rates by a mean of 2 deaths per 100,000 population, as opposed to our 3.5 estimated effect on despair mortality rates, and this represented a 4 percent effect, as opposed to our 9.3 percent effect. This comparison exhibits how calculations of the contribution to

despair mortality can vary substantially over time, especially with despair mortality in later years including the surge that resulted from the opioid epidemic.

With low-educated females ages 25-64 having a base population of 33.4 to 38 million during our sample period,[27] an increase of 3.5 deaths per 100,000 population represents roughly 1100-1300 deaths annually, or over 23,000 deaths over a 20-year period.

6 Robustness Checks

6.1 Cancer Mortality

We present a placebo check for our results by examining a different cause of death that should not have been affected by the introduction of welfare payment time limits. We choose cancer mortality, the leading cause of death (along with heart disease) for individuals under age 65 in developed countries. The top panel of Table 4 presents the results for the four different populations examined in Table 3 - females with a high school education or less in all sample states, females with a high school education or less in high-poverty states as of 1989, females with a college degree or more, and males with a high school education or less. All four coefficient estimates presented are very small in magnitude and not statistically significant.

6.2 Male Mortality with Female Explanatory Variable

We argued that the fact that we do not observe effects for males strengthens our causal interpretation of the results presented, as males experienced similar (if not greater) upward trends in despair mortality but we do not find an effect of exposure to time limits on their deaths. This argument is with the caveat that the explanatory variable for exposure to time limits differs between males and females, as we found that the age of parenthood for females is 21-48 whereas for males it is 24-53. We therefore perform

[27] This base population calculation includes the entire United States and not just the states in the regression sample (which excludes Washington, D.C. and Iowa). It is based on our base population configurations using CPS data.

a robustness check where we run equation 1 for the sample of male deaths, but the explanatory variable is now the female explanatory variable that assumes that the age of parenting is 21-48 rather than the male explanatory variable.

These results are presented in the bottom panel of Table 4, and still exhibit null effects for males. Recall that this is in comparison to the corresponding results for females in the top panel of Table 3 that show a statistically significant 1.32 percent increase in female despair mortality for each additional year of exposure to time limits (and a suggestive positive effect on suicide mortality).

6.3 1999 Start Year or Lower Thresholds for Missing Education Level

Our baseline sample starts in 1989, the earliest year that restricted mortality data report education levels. In 1999 the CDC changed the ICD code categorization for causes of death. For this reason, some evaluations of mortality trends begin in 1999, so as to have consistent codes for causes of death over time. In the top two panels of Table 5, we limit the sample period to 1999 onward, rather than 1989 onward. Our sample applies a threshold of 5% for excluding state-year units that had too many missing education levels for their despair deaths. In the bottom two panels of Table 5, we change the threshold for missing education levels to 4% of deaths, rather than 5% of deaths. Both changes to the sample - a 1999 start year and a lower threshold for missing education levels - decrease the sample size, so this may also affect statistical power. Despite this, the main results that are presented in Table 3 are maintained.

With the 1999 sample, a positive effect of exposure to time limits on despair deaths is still observed and the coefficient estimates are similar in size to those presented in Table 3, but statistical precision declined substantially, with a p-value of 0.117 for despair deaths among low-education females in all states. For suicides, the positive effects are also maintained, although the p-value for high-poverty states is 0.126.

When the sample excludes state-year units with missing education despair deaths

that exceed 4% of all these deaths, results are also maintained for despair deaths, with increases in response to time limits that are similar to those presented in Table 3, although slightly smaller. For suicides (or any of the despair mortality components), the effects are no longer statistically significant.

6.4 All Education Levels as Population Base

Equation 1 is a PPML regression with a dependent variable that is the death counts and these are normalized by including in the regression specification the natural log of the relevant population base and constraining the coefficient estimate for this to equal one. The relevant population bases are at the state, year, sex, age group and education level. We obtain these from the CPS data (summing the relevant weights).

Some concern may arise regarding the precision of these population bases. First, these small cells likely produce imprecise values. However, this should not bias our regression estimates unless this imprecision is correlated with our explanatory variable, and we are not aware of such a possibility. Second, and of potentially greater relevance, is the fact that the CPS sampling excludes incarcerated individuals. For males with a high school education or less incarceration rates can be quite substantial and have experienced a rising trend during our sample period (Neal and Rick, 2016). As such, the null effects that we observe for males in our regression results may not be driven by our explanatory variable's ability to capture something that is more relevant for females and neutralizing rising despair mortality trends during our sample period. Rather, it can be argued that these male null effects may be due to increasing imprecision in our male population base as incarceration rates increased (and this is correlated with greater time limit exposure over time).

To address this concern, we conducted the same analysis that is presented in Table 3 but rather than our CPS education-specific population bases, we used population bases provided by the CDC for all education levels (and still at the state, year, sex, and age group level).[28] These population values do not suffer from potential small cell errors

[28]https://wonder.cdc.gov/Deaths-by-Underlying-Cause.html

and include incarcerated individuals as well. If our female results are maintained using these population values, while still obtaining null effects for males, then this is evidence that our imprecise CPS education-level population values are not biasing our results. We present this in Table 6. Surprisingly, the coefficient estimates for the effect of years of exposure to time limits on female low-education mortality are for the most part of greater magnitude and with higher precision. This can imply that indeed using the small-cell population bases from the CPS suffers from imprecision. Moreover, the null effects for male despair mortality are maintained, as demonstrated in the bottom panel of Table 6. This reassures us that our null effects for the male population are not driven by imprecise population bases.

6.5 Accounting for the Staggered Introduction of Time Limits

Recent econometric literature has raised concerns that difference-in-differences designs with staggered adoption and two-way fixed effects, as in equations 1 and 2, can produce biased estimates in the presence of heterogeneous treatment effects (Goodman-Bacon, 2021; Callaway and SantÕAnna, 2020) or individuals systematically sorting into different levels of treatment (Callaway et al., 2024). Sun and Abraham (2021) propose an "interaction-weighted" estimator that allows for treatment effects to differ according to the timing of the intervention evaluated - i.e., the introduction of time limits in our setting. We test for the robustness of our results to the Sun and Abraham (2021) estimation method.

The Sun and Abraham (2021) procedure is applied to event study specifications in linear regression models. The dynamic estimation presented in Section 5.3 is similar to an event study framework by evaluating the effects of time limits for different time intervals relative to the introduction of time limits. However, the estimates are from a PPML regression model, which is not linear. To implement the Sun and Abraham (2021) procedure we therefore specify a linear regression model that deals with our zero

death count cells using an Inverse Hyperbolic Sine (IHS) transformation as the dependent variable. Although Norton (2022) and Chen and Roth (2024) have demonstrated that IHS transformations can result in incorrect marginal effects, our results are not sensitive to this, and when we estimate a linear regression with an IHS transformation, rather than equation 2, our results are highly similar. With this in mind, we apply the Sun and Abraham (2021) "interaction-weighted" procedure on a variation of equation 2 that is linear and the dependent variable is an IHS transformation of the despair mortality rate.[29, 30]

All states introduced time limits during our sample period, and as such, we use later-treated states as the "control cohort" in the Sun and Abraham (2021) "interaction-weighted" procedure. Specifically, we estimate and then average separate treatment effects by year of time-limit introduction for states that introduced time limits until 1999, while using states that introduced time limits from 2000 onward as a comparison group. We choose 2000 as the cutoff year to ensure a sufficient sample size (five states in the "comparison group").[31] The results of our test are presented in Figure 7. Along with the "interaction-weighted" coefficient estimates, the OLS coefficient estimates are also presented as a comparison. The results resemble to a large extent our baseline dynamic estimate analysis in Figure 6.

6.6 Age-Adjusted Mortality Rates

While our analysis evaluates changes in mortality rates for relatively narrow age ranges, concern can still arise that state-specific changes in the age distribution within these age brackets biases the results. To address this, we rerun our analysis with mortality measures that adjust for the state-year-sex-educational attainment age distribution

[29] Given that we take the despair mortality rate and not the death count, as in our PPML specification, we no longer control for the population of the relevant cell in the linear regression specification.

[30] IHS transformations of mortality or birth rates are also found in the analyses in Hollingsworth and Rudik (2021) and Palmer (2020), respectively.

[31] Using only the last three states to introduce time limits (from 2008 onward) as the comparison group yields similar estimates, with the exception of the long-term effect (15+ years post time-limit introduction), which is negative and with a p-value exceeding 0.9 and a substantially larger standard error for the 5-10 years post time-limit introduction estimate.

from the 1990 census (the largest sample near the start of our sample period, and still prior to the introduction of welfare payment time limits). These age-adjusted mortality rates are for five-year age range population shares, as in Finkelstein et al. (2024) and in the spirit of Ruhm (2000). Table 7 presents these results, and they are highly similar to our baseline results in Table 3, thereby ruling out the possibility that potential shifts in the age distribution are biasing our results.

6.7 Accounting for Other Welfare Reform Components

When evaluating the U.S. welfare reform, confining the analysis to welfare payment time limits has some advantages and disadvantages. The main advantage is that this is a specific policy that was substantial among the various welfare reform policy components and its variation is relatively well-identified, in terms of the introduction of time limits across states, as opposed to other policy components that may not have been as substantial or did not provide as much variation in their implementation. The disadvantage is that such an analysis focuses on only one of many components that comprised the U.S. welfare reform and does not provide a more complete picture of the reform's implications.

Attempting to address concerns that other welfare reform policies are not accounted for is challenging from several perspectives. First, some policy components have very little variation in implementation.[32] Second, for some policy components the best measure may be a continuous number (e.g., the monetary value of benefits for families and how this changed over time) and given that our main explanatory variable is already continuous, it is not straightforward to integrate this into our regression framework (e.g., an interaction term would be difficult to interpret). With these limitations in mind, we decided to incorporate additional aspects of the U.S. welfare reform by making use of a classification of the degree welfare reform sanctions were stringent that is provided in Grogger and Karoly (2005). This classification has the advantage of

[32]See Moffitt et al. (2020) for a discussion of this.

incorporating all important policy components into a single indicator variable.[33] We performed our mortality analysis for a variation of equation 1 that interacts the measure of exposure to time limits with an indicator for the state being classified as having stringent sanctions.[34]

The results of our analysis are presented in Table 8 for low-educated females in all states and high-poverty states in the top and bottom panels, respectively. Each column is a different dependent variable in terms of the causes of death examined. When despair mortality is the dependent variable (first column), the coefficient estimates for the interaction terms are not statistically significant, thereby suggesting that stringent sanctions do not generate a stronger response to time limits. However, when evaluating suicide and ALD mortality, the two causes of death which seem to be driving the mortality response to time limits (see Table 3), the results suggest that the effect is driven by states with stringent welfare reform sanctions. This can be seen through the statistically significant coefficient estimates for the interaction term when evaluating suicides or when examining the p-value for testing the significance of the sum of the time limit exposure variable and the interaction term variable ("P-Value for Joint Significance"). This suggests that stringent sanctions generated a greater adverse effect on mortality as opposed to non-stringent sanctions, although the results are relatively noisy and with the caveat that this is not observed for despair mortality.

Overall, despite the challenges mentioned above when attempting to integrate additional welfare reform components into the analysis and the problematic nature of the classification from Grogger and Karoly (2005),[35] Table 8 is an attempt to expand the analysis beyond the effects of welfare payment time limits on mortality. The relatively

[33]This is with the exception of the monetary value of the welfare payment benefit, a limitation of this measure that is mentioned in Grogger and Karoly (2005).

[34]14 out of the 49 states in the sample are classified as having "stringent" welfare reform sanctions. We integrate the four sources for sanction severity that are presented in Table 4.2 in Grogger and Karoly (2005) and classify a state as "stringent" if at least three of the four sources categorized the sanctions as stringent (on a scale of "stringent", "intermediate", and "lenient").

[35]Grogger and Karoly (2005) discuss the inconsistency of the classifications across the four sources they present and summarise: "If analysts cannot agree on what a strict sanction policy is, the effects of a 'strict' sanction policy may vary across studies for reasons that have more to do with measurement than with real behavior."

noisy results in Table 8 do not allow us to credibly compare the magnitude of the effects between the overall population (in Table 3) and just those in states with stringent policies. However, a simple comparison of the suicide and ALD coefficient estimates suggests effects in stringent states that are more than 50% larger than those presented for all states (in Table 3).

6.8 Additional Population Breakdowns

Our analysis is at the state-year-age group-gender-education level. This is a very granular level of analysis, which potentially constrains our ability to evaluate the effect of time limits on mortality for even more granular population levels, such as by race or other geographic breakdowns beyond the state level. In particular, the nature of despair mortality is such that we already have many observations in our analysis with zero or very low death counts. An additional breakdown would increase this even further, thereby potentially increasing extreme cases of changing mortality rates from zero to positive numbers, which could bias the analysis. Furthermore, our analysis includes imputation of education levels among the deceased (see Section 3), and evaluation of more granular cells than those already used for the analysis can also reduce the precision of this imputation procedure. Lastly, all regression specifications control for the relevant population base for which death counts are accounted for. However, as these are not provided directly, but rather generated from CPS data, the smaller the cell sizes, the greater the imprecision in this measure. Indeed, the evidence in Section 6.4 suggests that our current population measures at the state-year-age group-gender-education level are already at the limit of noisiness.

Still, racial breakdowns of the effect of time limits on despair mortality are of interest because increases in these causes of death have been primarily attributed to the non-Hispanic White population, although in more recent studies with data from the last 10+ years this is not exclusively the case, and there is evidence of increases among Black and Hispanic individuals as well (Case and Deaton, 2022). Nevertheless, given that at least until more recently despair mortality was categorized as a phenomenon

more relevant for White communities, and that welfare payments are generally thought to be disproportionately relevant for Black females, this implies that an analysis by race should be of value. Unfortunately, our analysis of White or Black deaths was not conclusive. As described above, we believe that this is due to lower death counts and population levels when examining finer population cells than those that we are already analyzing. However, we do note that our review of past literature suggests that the share of Black individuals among females potentially eligible for welfare payments seems to be roughly one third. In particular, in Pepin (2022)'s sample using 1990 census data to evaluate the effect of time limits in the 2000's and 2010's, of the 120,000 heads of households and their spouses who are AFDC recipients, 30% were Black. In Grogger (2004)'s sample that takes female-headed households from the 1979-1998 March CPS data, 35% are either Black or Non-White (31% report welfare use). Thus, despite Black individuals being disproportionately represented among females who are either receiving or potentially eligible for welfare payments, the vast majority of this population is still White.

A more granular geographic breakdown could also be of interest because it enables examining more homogeneous localities, rather than states, which could be fairly large and as a result more heterogeneous (for example, in terms of poverty rates). However, any breakdown (e.g., at the commuting zone or metropolitan statistical area level) would still need to not cross state borders because of the variation across states in time-limit introduction. As such, we are again constrained by the small cell sizes that our analysis already incorporates, and as in the racial breakdown, breaking down the states into more granular geographic units did not produce conclusive results.

7 Concluding Remarks

We have examined the causal effect of lifetime time limits on the receipt of welfare payments on deaths of despair. Our results suggest that this substantial reduction in the social safety net provided to disadvantaged families in the U.S. contributed to the

rising trends in despair mortality over the past two decades. Our findings therefore highlight the potential role of social safety nets in improving the overall welfare and well-being of disadvantaged individuals beyond just satisfying their basic economic and material needs.

Our results have some limitations. First, we are unable to include in our analysis the precise mechanisms driving these increased mortality rates. As mentioned in the Introduction, deaths of despair can increase upon the introduction of time limits through numerous channels, including the psychological stress of having a weaker social safety net, the stress resulting from increased labor force participation, in particular when it involves heads of very fragile families, or the decreased income experienced by some families that can result in greater financial hardships. Second, the adverse effects of time limits may be under-estimated for some population segments, due to the potential for time limits to decrease despair through greater positive engagement in the labor market, and we are not aware of a way to evaluate this. Lastly, death is a very extreme outcome and finding evidence that it increased due to the introduction of time limits can imply that less extreme adverse outcomes may have also been affected by disadvantaged individuals. Future research may be able to shed light on these aspects as well.

In this paper, we present the first results (to the best of our knowledge) that link the rising trends in despair mortality rates to a decline in the social safety net. Furthermore, we are able to do this as a function of prolonged exposure to this decline, in accordance with the notion that despair mortality results from cumulative factors that build up over time.

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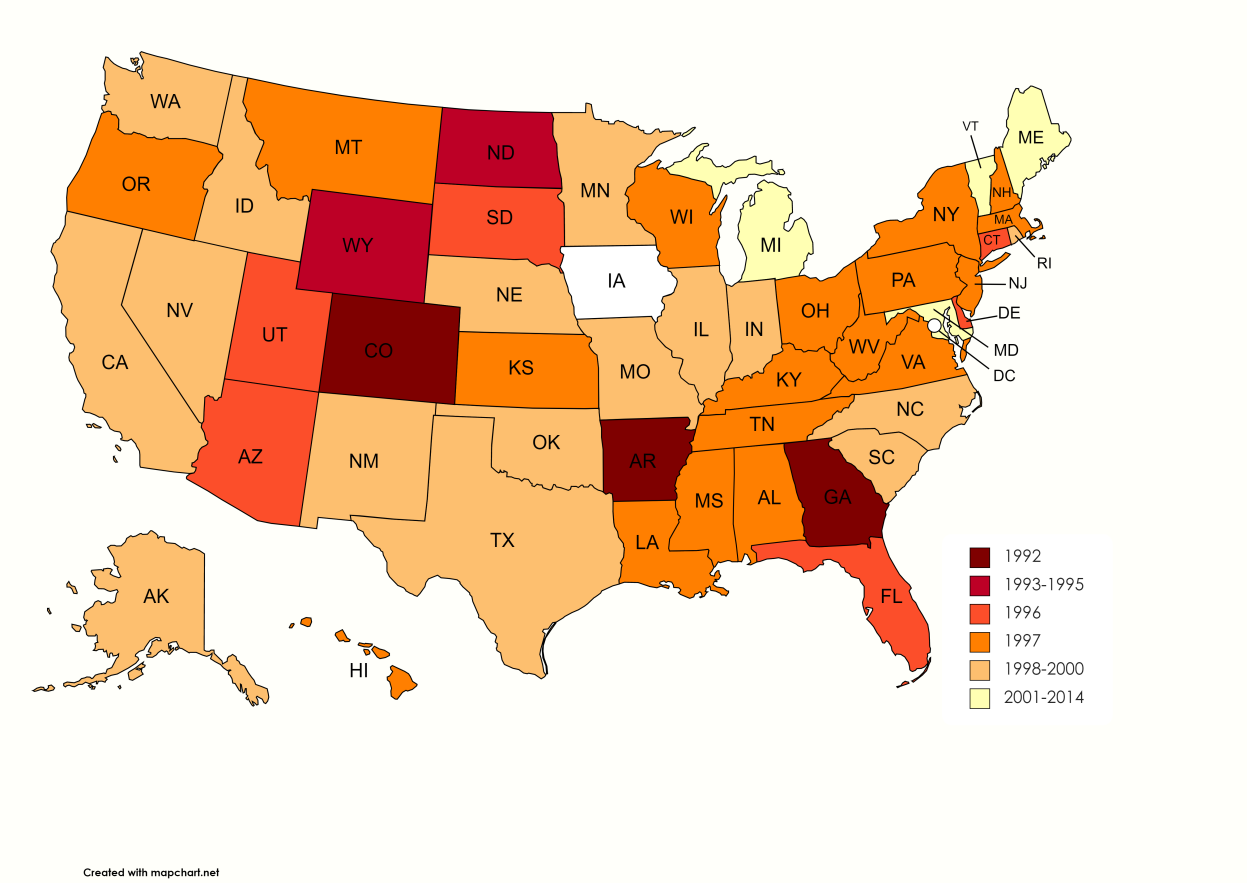
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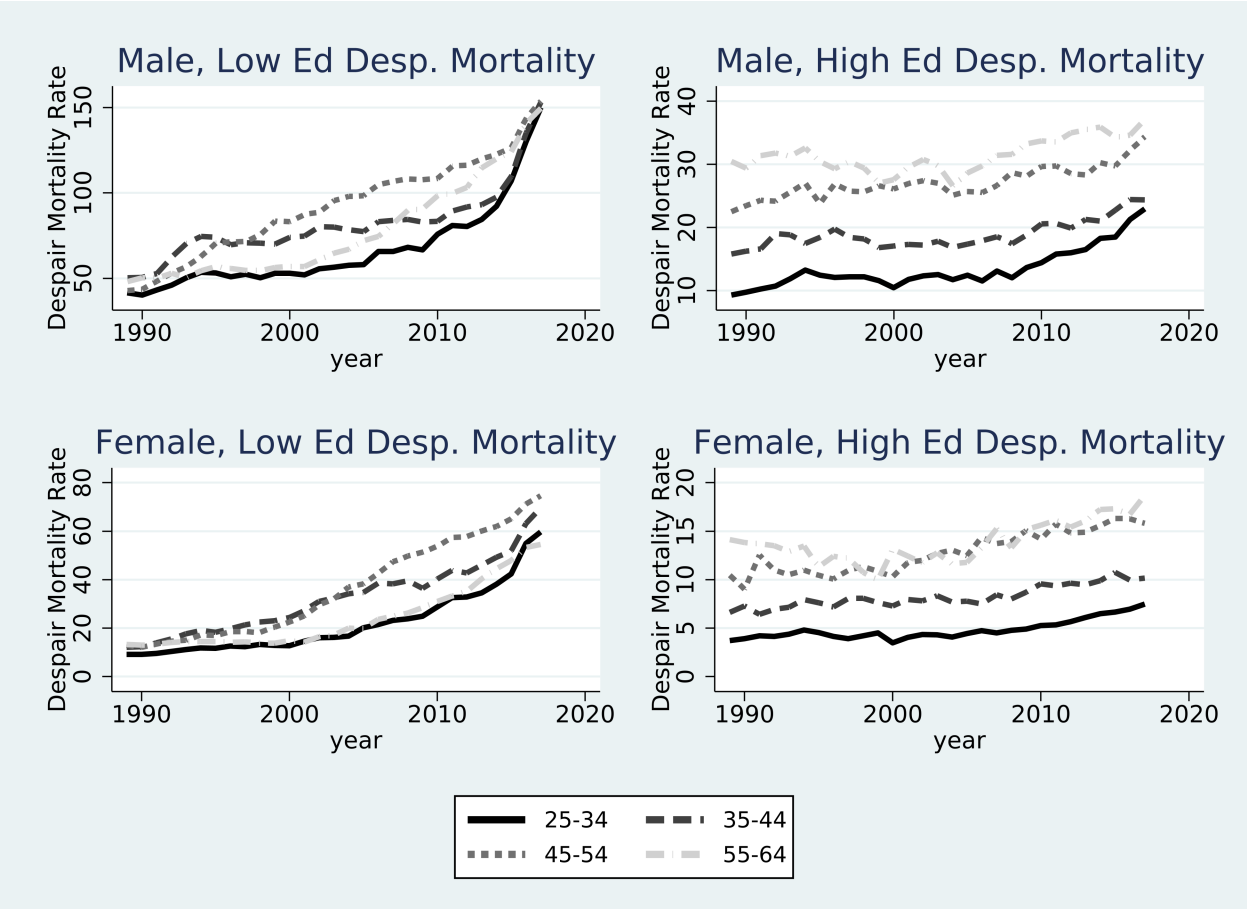
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Figure 1: Time Limit Introduction Year



Notes: The figure presents 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.

Figure 2: Trends in Despair Mortality by Sex, Age Range, and Education Level



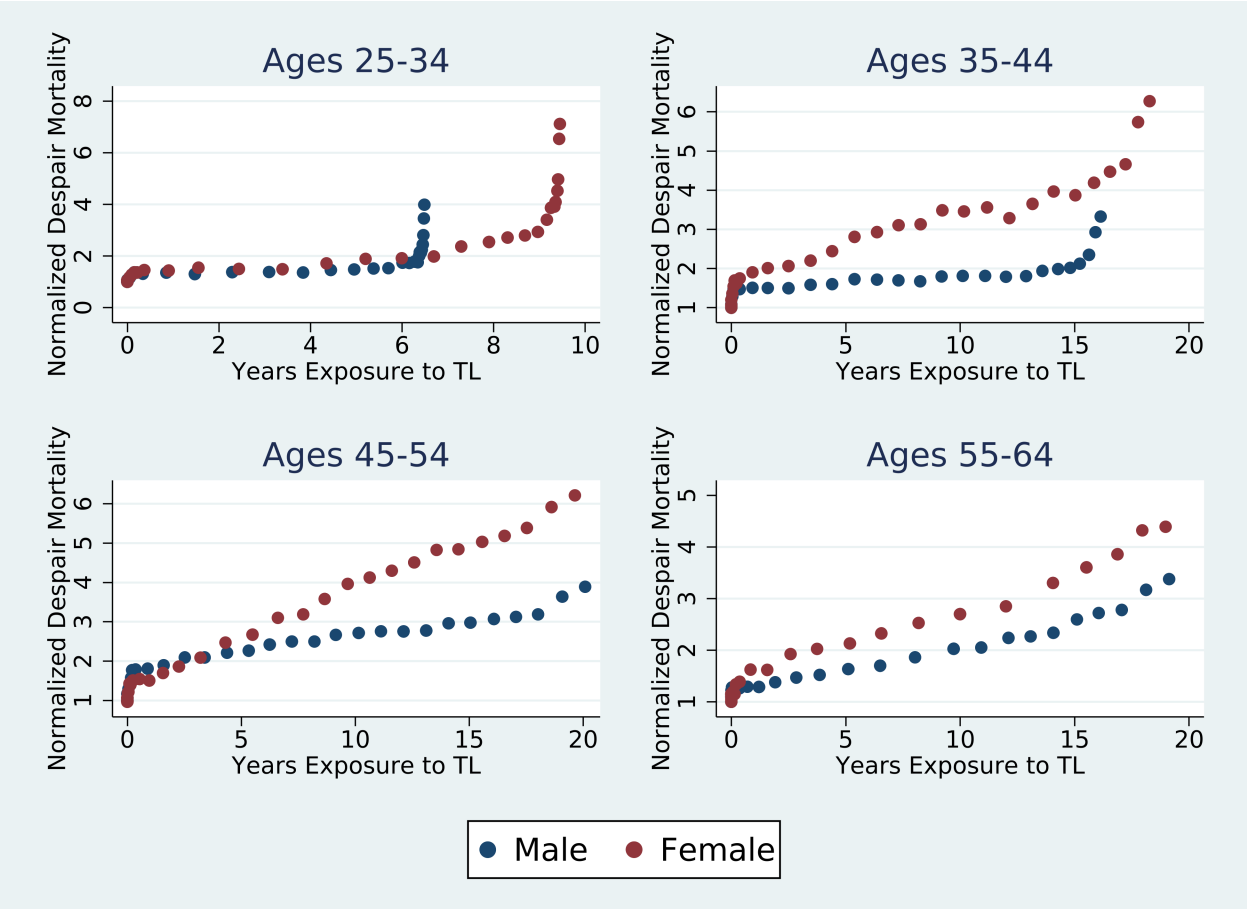
Notes: The figures present means across states, excluding Iowa and Washington D.C. (see sample restrictions in Data Section). Each line represents the age range indicated in the legend in the bottom. The low-education population is those with a high school degree or less, while the high-education population is those with a college degree or more.

Figure 3: Trends in Time Limit Exposure by Sex and Age Group



Notes: Both figures present means across states, excluding Iowa and Washington D.C. (see sample restrictions in Data Section). Each line represents the age range indicated in the legend in the bottom. Years of exposure to time limits begins in the age of parenting minors, as long as the start is before the end of years parenting minors. These ages are 24-53 and 21-48 for males and females, respectively.

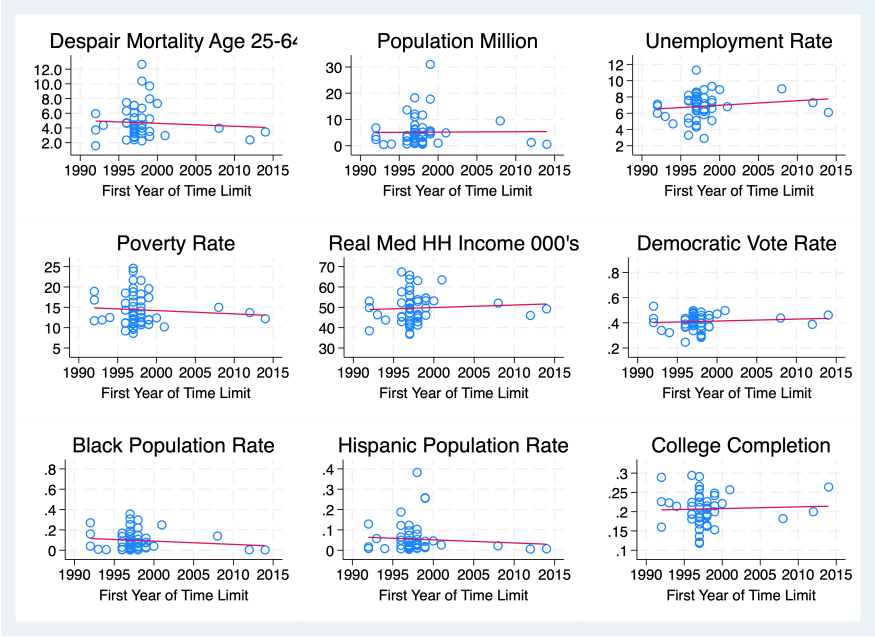
Figure 4: Relation between Mean Annual Despair Mortality Rate (1989=1) and Time Limit Exposure by Sex and Age Group



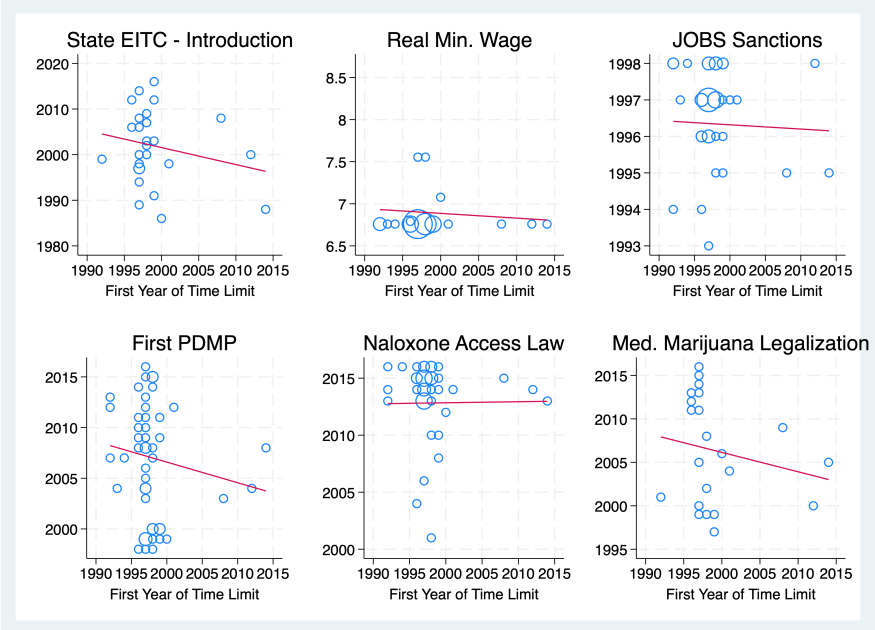
Notes: The figure presents the relationship between the despair mortality rate among the low-educated (high school education or less) and the years of exposure to time limits for each sex, age group, and year cell (so these are weighted means across states in each year for each age group-sex combination). For each age group and sex, the mean despair mortality is normalized by dividing by the values in the earliest year, 1989.

Figure 5: Year of Time-Limit Introductions and State-Level Characteristics/Policies

(A) State-Level Characteristics

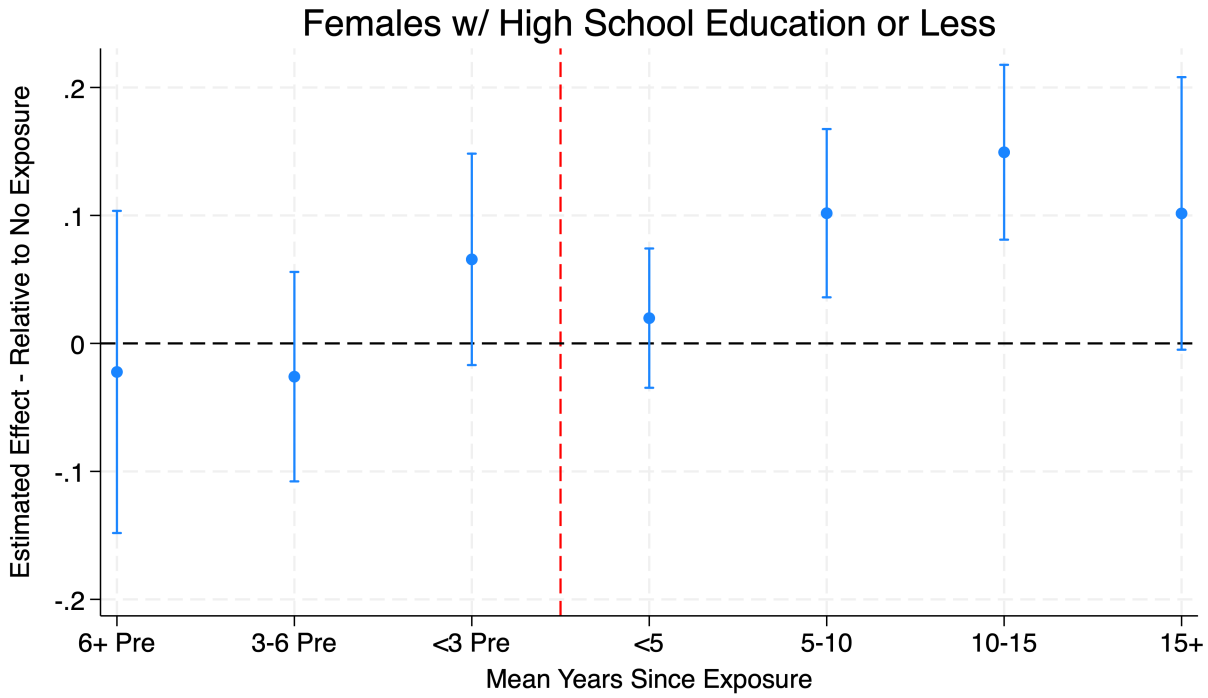


(B) State-Level Policies



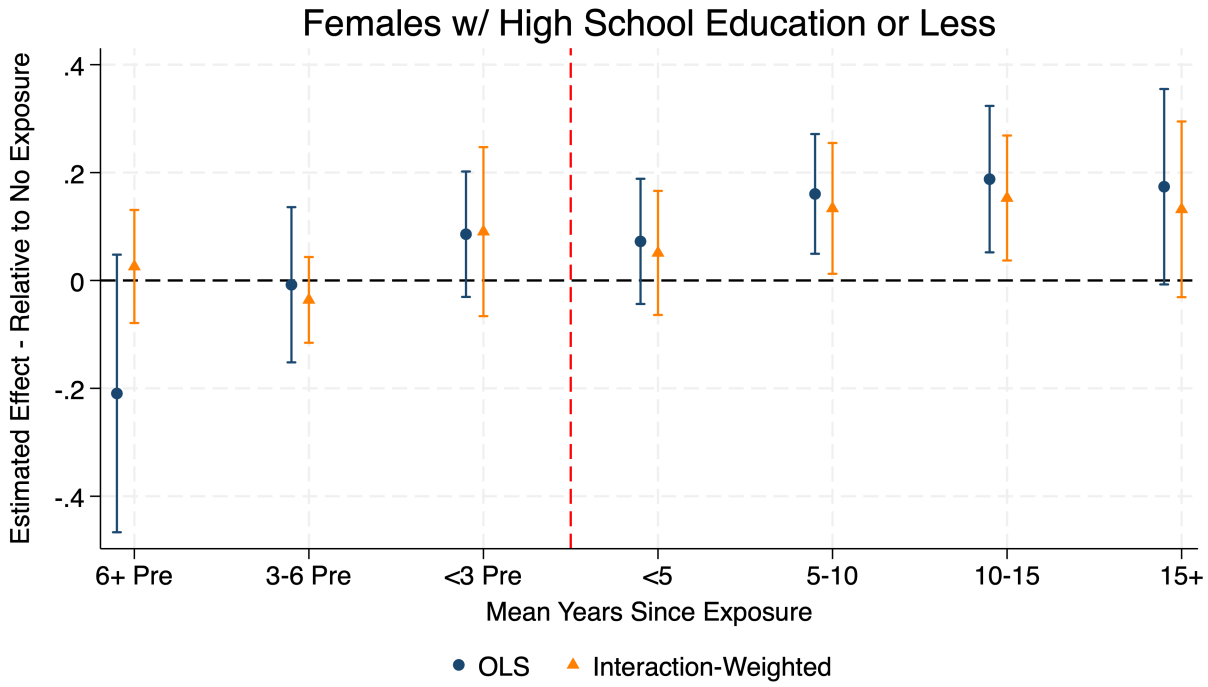
Notes: Larger circles indicate more states.
The top panel plots for each state the value of the stated state-level characteristics as of 1990-1992 and the years time limits were introduced. The bottom panel plots for each state the year the stated policy was introduced or substantially expanded and the years time limits were introduced, or real minimum wage changes between 1992 and 2016.
The fitted linear regression line is indicated in red.
JOBS represents Jobs Opportunities and Basic Skills Training Program. For JOBS Sanctions, all states are covered. Only a subset of states introduced EITC or legalized cannabis medical use. States that did not introduce these policies are not plotted.

Figure 6: Non-Parametric Despair Mortality Response to Time Limit Exposure - Low-Education Females



Notes: The figure presents the coefficient estimates for $TLExposure^{j}_{ast}$ for $j \in [-1, -2, -3, 1, 2, 3, 4]$ from equation 2, along with their 95% confidence intervals for the dynamic regression specification discussed in Section 5.3. Each coefficient estimate represents the change in despair mortality (multiply by a hundred for percent change) in response to the range of years of exposure indicated on the horizontal axis, relative to zero years of exposure.

Figure 7: Non-Parametric Despair Mortality Response to Time Limit Exposure - Low-Education Females - Linear Regression with IHS Transformation as Dependent Variable



Notes: The figure presents the coefficient estimates for $TLExposure^{j}_{ast}$ for $j \in [-1, -2, -3, 1, 2, 3, 4]$ from a variation of equation 2 that is linear and uses an IHS transformation of the despair mortality rate as the dependent variable. See Section 6.5 for additional details regarding the regression specification and procedure applied. The vertical lines are 95% confidence intervals for each coefficient estimate.

Each coefficient estimate represents the change in despair mortality (multiply by a hundred for percent change) in response to the range of years of exposure indicated on the horizontal axis, relative to zero years of exposure.

The figure provides results by two estimation methods: (i) OLS (blue circle) (ii) Sun and Abraham (2021) Interaction-Weighted (orange triangle)

The Interaction-Weighted analysis uses Sun and Abraham (2021)'s `"eventstudyinteract"` STATA implementation.

Table 1: Summary Statistics

	Age Range 25-34		Age Range 35-44		Age Range 45-54		Age Range 55-64	
	High School or Less	College Degree or More	High School or Less	College Degree or More	High School or Less	College Degree or More	High School or Less	College Degree or More
Despair Male	85.16	16.58	97.04	21.88	105.70	30.66	88.31	34.66
	(45.39)	(9.81)	(43.39)	(10.01)	(45.27)	(13.10)	(44.06)	(18.51)
Despair Female	30.14	5.61	44.08	10.06	47.10	15.10	29.85	15.41
	(23.65)	(4.22)	(27.87)	(5.91)	(30.51)	(8.30)	(20.35)	(11.54)
Suicide Male	44.10	11.03	43.24	14.30	42.01	19.46	36.08	21.17
	(17.05)	(7.22)	(15.41)	(7.32)	(15.69)	(9.11)	(15.72)	(12.18)
Suicide Female	9.23	3.65	11.84	5.57	12.04	7.94	8.70	7.77
	(5.77)	(3.34)	(6.79)	(3.83)	(6.66)	(5.16)	(5.51)	(7.62)
Drugs Male	41.19	5.91	46.26	7.04	40.24	7.17	20.28	5.02
	(39.60)	(5.84)	(36.40)	(5.66)	(31.38)	(5.72)	(21.73)	(4.90)
Drugs Female	20.97	2.69	28.67	5.15	27.08	7.20	13.43	5.82
	(20.37)	(2.71)	(22.19)	(4.11)	(21.56)	(5.74)	(12.62)	(6.11)
ALD Male	2.32	0.39	11.30	1.99	27.32	5.84	34.57	10.13
	(3.12)	(1.07)	(7.92)	(2.42)	(13.87)	(4.72)	(19.85)	(9.99)
ALD Female	2.16	0.17	7.61	1.09	12.58	2.59	10.79	4.33
	(4.06)	(0.55)	(8.81)	(1.82)	(11.62)	(2.91)	(9.49)	(6.05)
Explanatory Variable - Mean Years Since Reform (Across Cohorts in Age Range)								
Mean Male	4.27		8.10		8.75		7.29	
(SD)	(2.72)		(6.25)		(7.24)		(7.36)	
Max Male	6.50		16.50		24.90		25.00	
Mean Female	5.80		8.55		8.25		5.72	
(SD)	(3.93)		(6.83)		(7.20)		(7.35)	
Max Female	9.50		19.50		25.30		25.00	

Notes: The mortality measures are means across states and years of mortality rates per 100,000 residents in the relevant population base (based on age group, sex and education level). Number of observations for each age group-education level and sex is 1,064, which covers 49 states over 29 years (1989-2017) after excluding state-year units with unknown/missing education levels for over 5% of despair deaths. Mean Years Since Reform refers to time limit exposure beginning at the age of parenting which is 21-48 and 24-53 for females and males, respectively. Number in parenthesis are standard deviations of the means presented.

Table 2: Years Exposure to Welfare Payment Time Limits and Welfare Receipt

	Individual Level Ages 25-48 OLS Estimation		Aggregate Level Ages 25-64 OLS Estimation w/ IHS	
	Female Low Educ All States	Female Low Educ High Poverty States	Female Low Educ All States	Female Low Educ High Poverty States
Mean Years Since Exposure to Time Limits	-0.00124*** (0.000393)	-0.00144*** (0.000391)	-0.00112** (0.000436)	-0.00143** (0.000673)
Observations	378,617	231,180	5,684	2,900
R-Squared	0.036	0.038	0.703	0.723
Mean Welfare Receipt (Pre-TL)	0.0870	0.0915	0.0526	0.0551
Mean Years of Exposure to TL	9.129	9.172	9.181	9.282
State-Year Fixed Effects	Yes	Yes	Yes	Yes
Age Group Fixed Effects	Yes	Yes	Yes	Yes
Cohort Fixed Effects	Yes	Yes	Yes	Yes
Age Group Specific Linear TT	Yes	Yes	Yes	Yes

Notes: The table presents the coefficient estimate β from two variations of equation (1). The sample is females with a high school education or less from the March CPS Supplement 1989-2017 data, excluding those residing in Washington, D.C. and Iowa.
In the first two columns, individual-level data is used for an OLS regression (rather than PPML) with an indicator for welfare receipt as the dependent variable. Years of Exposure to Time Limits refers to time limit exposure beginning at the age of parenting which is 21-48.
In the last two columns, data is aggregate at the state, year and age group level, as in the mortality data used for the analysis. Observations are weighted using CPS weights, thereby creating aggregate welfare payment receipt measures that are not integers. These measures are converted to welfare payment rates by dividing by the relevant population for each cell. Then an inverse hyperbolic sine (IHS) function is applied to these rates and this is the dependent variable of the OLS regression that is a variation of equation (1).
The second and fourth columns limit the sample to states that had a 1989 poverty rate that exceeded the median.
Standard errors clustered at the state level are in parenthesis. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 3: Mortality and Exposure to Time Limits

	Despair	Drugs	Suicide	ALD
Females - High School Education or Less				
Mean Years Since Exposure to Time Limits	0.0132**	0.00198	0.00615	0.00313
	(0.00574)	(0.00922)	(0.00461)	(0.00731)
Observations	4,256	4,248	4,256	4,236
Mean Mortality Rate	37.79	22.58	10.45	8.322
TL Exposure Mean	7.08	7.10	7.08	7.10
Females - HS Education or Less - States w/ 1989 Poverty $\geq$ Median				
Mean Years Since Exposure to Time Limits	0.0177**	0.00859	0.0105*	0.00915
	(0.00714)	(0.0108)	(0.00588)	(0.00570)
Observations	2,032	2,032	2,032	2,032
Mean Mortality Rate	39.44	23.38	10.80	8.594
TL Exposure Mean	7.86	7.86	7.86	7.86
Females - College Degree or More				
Mean Years Since Exposure to Time Limits	-0.00290	-0.00932	-0.00509	-0.00570
	(0.00532)	(0.00828)	(0.00589)	(0.00850)
Observations	4,232	4,104	4,216	3,824
Mean Mortality Rate	11.61	5.404	6.292	2.277
TL Exposure Mean	7.12	7.28	7.13	7.37
Males - High School Education or Less				
Mean Years Since Exposure to Time Limits	-0.00109	-0.0135	-0.00129	-0.00784
	(0.00833)	(0.0132)	(0.00518)	(0.00899)
Observations	4,256	4,248	4,256	4,256
Mean Mortality Rate	94.05	37.06	41.36	18.88
Mean TL Exposure	7.10	7.11	7.10	7.10

Notes: The table presents the coefficient estimate β from equation (1). Mean Years Since Exposure to Time Limits refers to time limit exposure beginning at the age of parenting which is 21-48 and 24-53 for females and males, respectively. The mean mortality rate is the mean across all observations of the total number of deaths per 100,000 in each unit of observations. The number of observations varies across columns due to singletons being dropped. Standard errors clustered at the state level are in parenthesis. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 4: Robustness Checks - Cancer Mortality and Male Despair Mortality w/ Female Explanatory Variable

	Mortality from Cancer			
	Female Low Education - All States	Female Low Education - High Poverty States	Female High Education	Male Low Education
Mean Years Since Exposure to Time Limits	2.04e-05 (0.00237)	0.00113 (0.00356)	0.000960 (0.00469)	0.00391 (0.00471)
Observations	4,256	2,032	4,256	4,256
Mean Mortality Rate	149.5	152.3	87.72	198
	Males - High School Education or Less - Female Explanatory Variable			
	Despair	Drugs	Suicide	ALD
Mean Years Since Exposure to Time Limits	0.00381 (0.00555)	-0.00915 (0.00947)	-0.00125 (0.00347)	0.00500 (0.00522)
Observations	4,256	4,248	4,256	4,256
Mean Mortality Rate	94.05	37.06	41.36	18.88

Notes: The table presents the coefficient estimate β from equation (1). Mean Years Since Exposure to Time Limits refers to time limit exposure beginning at the age of parenting. In the top panel this is ages 21-48 or 24-53 for females and males, respectively. In the second panel, this is ages is 21-48. The mean mortality rate is the mean across all observations of the total number of deaths per 100,000 in each unit of observations. Standard errors clustered at the state level are in parenthesis. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 5: Female Mortality and Exposure to Time Limits - Different Sample Criteria

	Despair	Drugs	Suicide	ALD
High School Education or Less - 1999 Sample				
Mean Years Since	0.0123	0.00558	0.00959*	0.00110
Exposure to Time Limits	(0.00784)	(0.0104)	(0.00560)	(0.0113)
Observations	3,180	3,180	3,180	4,036
Mean Mortality Rate	44.79	28.24	11.33	8.469
HS Education or Less - States w/ 1989 Poverty $\geq$ Median - 1999 Sample				
Mean Years Since	0.0163*	0.0106	0.0112	0.0128
Exposure to Time Limits	(0.00920)	(0.0118)	(0.00728)	(0.0107)
Observations	1,588	1,588	1,588	1,588
Mean Mortality Rate	45.71	28.43	11.58	9.398
High School Education or Less - 4% Threshold				
Mean Years Since	0.0113**	0.00129	0.00310	0.000807
Exposure to Time Limits	(0.00573)	(0.00966)	(0.00480)	(0.00767)
Observations	3,812	3,808	3,812	3,796
Mean Mortality Rate	38.52	23.20	10.59	8.319
HS Education or Less - States w/ 1989 Poverty $\geq$ Median - 4% Threshold				
Mean Years Since	0.0153**	0.00861	0.00753	0.00717
Exposure to Time Limits	(0.00728)	(0.0111)	(0.00621)	(0.00620)
Observations	1,800	1,800	1,800	1,800
Mean Mortality Rate	40.60	24.35	11.01	8.605

Notes: The table presents the coefficient estimate β from equation (1). Mean Years Since Exposure to Time Limits refers to time limit exposure beginning at the age of parenting which is 21-48 and 24-53 for females and males, respectively. The mean mortality rate is the mean across all observations of the total number of deaths per 100,000 in each unit of observations. The first two panels present results similar to the first two panels of Table 3, only the sample period begins in 1999. The bottom two panels present results similar to the first two panels of Table 3, only the threshold for excluding state-year units from the analysis due to missing education levels for deaths is 4 percent. Standard errors clustered at the state level are in parenthesis. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 6: Mortality and Exposure to Time Limits - All Education Levels as Population Base

	Despair	Drugs	Suicide	ALD
Females - High School Education or Less				
Mean Years Since Exposure to Time Limits	0.0145***	0.00174	0.00813*	0.00453
	(0.00439)	(0.00641)	(0.00425)	(0.00838)
Observations	4,256	4,248	4,256	4,236
Mean TL Exposure	7.082	7.095	7.082	7.115
Females - HS Education or Less - States with 1989 Poverty $\geq$ Median				
Mean Years Since Exposure to Time Limits	0.0192***	0.00827	0.0139***	0.0104
	(0.00598)	(0.00717)	(0.00502)	(0.00820)
Observations	2,032	2,032	2,032	2,032
Mean TL Exposure	7.863	7.863	7.863	7.863
Females - College Degree or More				
Mean Years Since Exposure to Time Limits	-0.00542	-0.00998	-0.00854	-0.0100
	(0.00671)	(0.00993)	(0.00659)	(0.00908)
Observations	4,232	4,104	4,216	3,824
Mean TL Exposure	7.120	7.281	7.128	7.369
Males - High School Education or Less				
Mean Years Since Exposure to Time Limits	0.00649	-0.00941	0.00491	0.00740
	(0.00504)	(0.0105)	(0.00379)	(0.00993)
Observations	4,256	4,248	4,256	4,256
Mean TL Exposure	7.101	7.114	7.101	7.101

Notes: The table presents the coefficient estimate β from equation (1). Population bases are for all education levels (based on state, year, sex, and age group), rather than the education level for which the incidence of deaths is counted for. Mean Years Since Exposure to Time Limits refers to time limit exposure beginning at the age of parenting which is 21-48 and 24-53 for females and males, respectively. The mean mortality rate is not presented, as the population base is not correct. Standard errors clustered at the state level are in parenthesis. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 7: Mortality and Exposure to Time Limits - Age-Adjusted Mortality Rates

	Despair	Drugs	Suicide	ALD
Females - High School Education or Less				
Mean Years Since Exposure to Time Limits	0.0133**	0.00201	0.00620	0.00338
	(0.00567)	(0.00908)	(0.00455)	(0.00727)
Observations	4,256	4,248	4,256	4,236
Mean Mortality Rate	38.26	22.85	10.58	8.415
Mean TL Exposure	7.08	7.10	7.08	7.10
Females - HS Education or Less - States with 1989 Poverty $\geq$ Median				
Mean Years Since Exposure to Time Limits	0.0177**	0.00840	0.0109*	0.00932*
	(0.00709)	(0.0107)	(0.00557)	(0.00544)
Observations	2,032	2,032	2,032	2,032
Mean Mortality Rate	39.92	23.67	10.93	8.671
Mean TL Exposure	7.86	7.86	7.86	7.86
Females - College Degree or More				
Mean Years Since Exposure to Time Limits	-0.00161	-0.00852	-0.00467	-0.0318
	(0.00521)	(0.00804)	(0.00554)	(0.00905)
Observations	4,232	4,104	4,216	3,824
Mean Mortality Rate	11.94	5.526	6.454	4.911
Mean TL Exposure	7.12	7.28	7.13	7.37
Males - High School Education or Less				
Mean Years Since Exposure to Time Limits	-0.000961	-0.0128	-0.00125	0.00856
	(0.00842)	(0.0133)	(0.00528)	(0.00949)
Observations	4,256	4,248	4,256	4,256
Mean Mortality Rate	95.34	37.41	41.99	19.16
Mean TL Exposure	7.10	7.11	7.10	7.10

Notes: The table presents the coefficient estimate β from equation (1) with age-adjusted mortality rates. See Section 6.6 for an explanation of constructing this. Mean Years Since Exposure to Time Limits refers to time limit exposure beginning at the age of parenting which is 21-48 and 24-53 for females and males, respectively. Standard errors clustered at the state level are in parenthesis. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 8: Interacting Exposure to Time Limits with State-Level Indicator for Stringent Welfare Sanctions

	Despair	Drugs	Suicide	ALD
	Female Low Education - All States			
Mean Years Since	0.0122**	0.000805	0.00293	8.00e-05
Exposure to Time Limits	(0.00563)	(0.00893)	(0.00481)	(0.00824)
Mean Years Since Exposure	0.00264	0.00314	0.00856*	0.00905
to TL X Stringent Sanctions	(0.00702)	(0.00953)	(0.00503)	(0.00775)
Observations	4,256	4,248	4,256	4,236
Mean Mortality Rate	37.79	22.58	10.45	8.322
P-Value for Joint Significance	0.0646	0.749	0.0203	0.216
Mean Years of Exposure to TL	7.082	7.095	7.082	7.115
	Female Low Education - High Poverty States			
Mean Years Since	0.0169**	0.00807	0.00435	0.00569
Exposure to Time Limits	(0.00682)	(0.00958)	(0.00643)	(0.00598)
Mean Years Since Exposure	0.00164	0.00106	0.0130**	0.00810
to TL X Stringent Sanctions	(0.00841)	(0.0111)	(0.00625)	(0.00775)
Observations	2,032	2,032	2,032	2,032
Mean Mortality Rate	39.44	23.38	10.80	8.594
P-Value for Joint Significance	0.0528	0.525	0.00114	0.0728
Mean Years of Exposure to TL	7.863	7.863	7.863	7.863

Notes: The table presents coefficient estimates from a variation of equation (1), only an additional explanatory variable is added which is the interaction between the years of exposure to time limits and an indicator for the state being classified as having stringent welfare reform sanctions (see Section 6.7). Mean Years Since Exposure to Time Limits refers to time limit exposure beginning at the age of parenting which is 21-48 for females. Standard errors clustered at the state level are in parenthesis. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Appendices

Appendix A The Explanatory Variable for Years Exposure to Time Limits

We provide additional details concerning the construction of our explanatory variable measuring years of exposure to time limits through several figures.

Appendix Figure A1 visualizes the years of exposure to time limits for different cohorts in different years over the sample period for two states that introduced time limits at different years - Florida in 1996 and Michigan in 2008. The last sub-figure on the bottom right demonstrates the variation in exposure across cohorts within state-year units. There are three separate lines for Florida, which introduced time limits in 1996 (note the solid gray line which has a value of zero all throughout the sample period). The dotted gray line represents the 1980 cohort in Florida, but there are actually several such lines that are slightly above or below each other (in term of years of exposure values) for cohorts ranging from 1976 through 1992, the last cohort that is observed in the data (those born in 1992 are 25 in 2017 - 25 is the youngest age and 2017 is the last year in our sample).

The last sub-figure on the bottom right of Appendix Figure A1 also provides a sense of the variation across states. One can compare the two lines for Michigan as opposed to the lines for Florida, and this is only a partial exhibition of the differences between the two states in the values of years of exposure, as Michigan also has additional lines for other cohorts (for example, those born after 1987) that are not in this figure.

Appendix Figure A2 demonstrates the variation in our years of exposure variable from additional perspectives. Appendix Figure A2A presents histograms for the values of the variable by age group. For age group 25-34, the values range from zero to a little over 10, but for the higher age groups, we can see values that are up to 20 and with the exception of the mass at zero (mostly for cells prior to time-limit introductions), this is fairly evenly distributed.

Appendix Figure A2B takes four states that introduced time limits in different years and presents the distribution of the standard errors for the years exposure variable within state-year units (so across four observations representing the four age groups). The figure demonstrates differing degrees of variation within state-year units, even for Michigan, a state that introduced time limits as late as 2008 and therefore has relatively low values of exposure to time limits for all age groups (which should result in low variation within many of its state-year units).

Appendix B Checking for Migration Effects of Time Limit Introductions

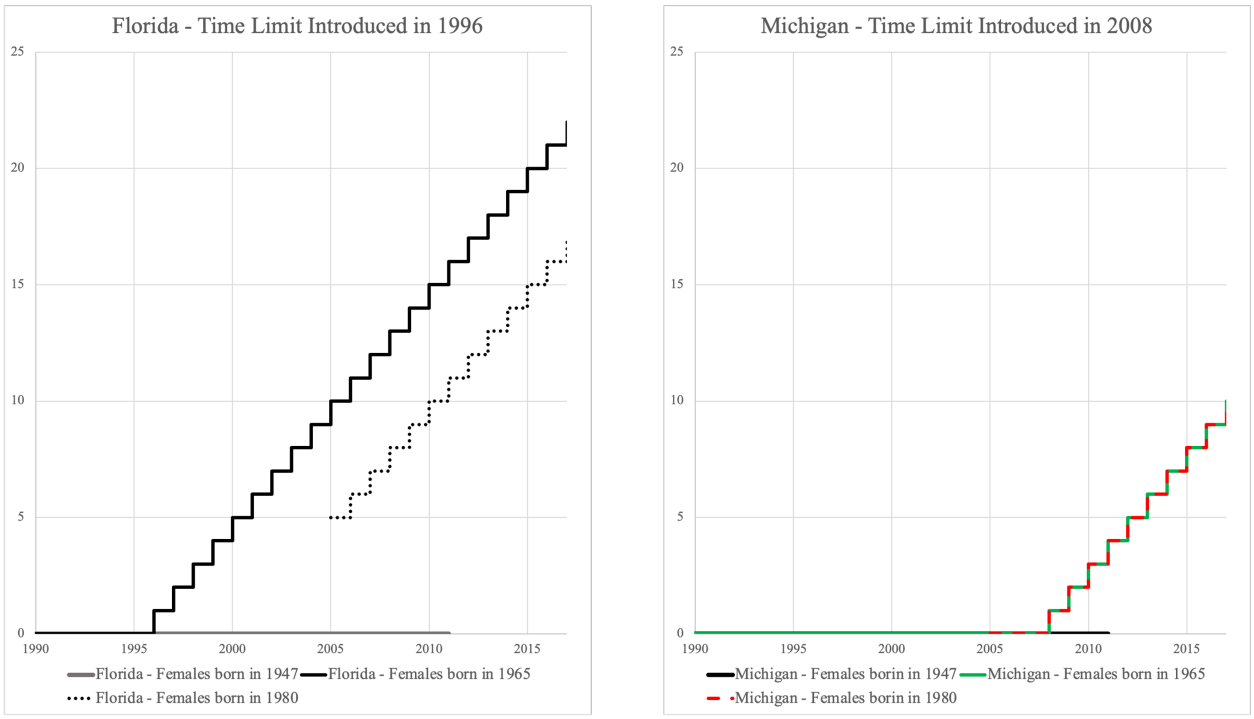
The data is from Current Population Survey (CPS)'s Annual Social and Economic Supplement (ASEC, also known as the "March Supplement" after the month it is administered in). This survey contains a question as to whether a respondent has changed residences in the course of the past year, and if so whether that individual has moved to a different state. With the standard variable of current state of residence, one can calculate the migration flows between any two pairs of states.

These new variables can be merged with the data described above regarding the introduction of time limits by state and year. This merged data set then can include three explanatory variables of interest: 1) was a time limit introduced in last year's state of residence, 2) did a time limit exist in last year's state of residence, and 3) is there a time limit in the current state of residence.

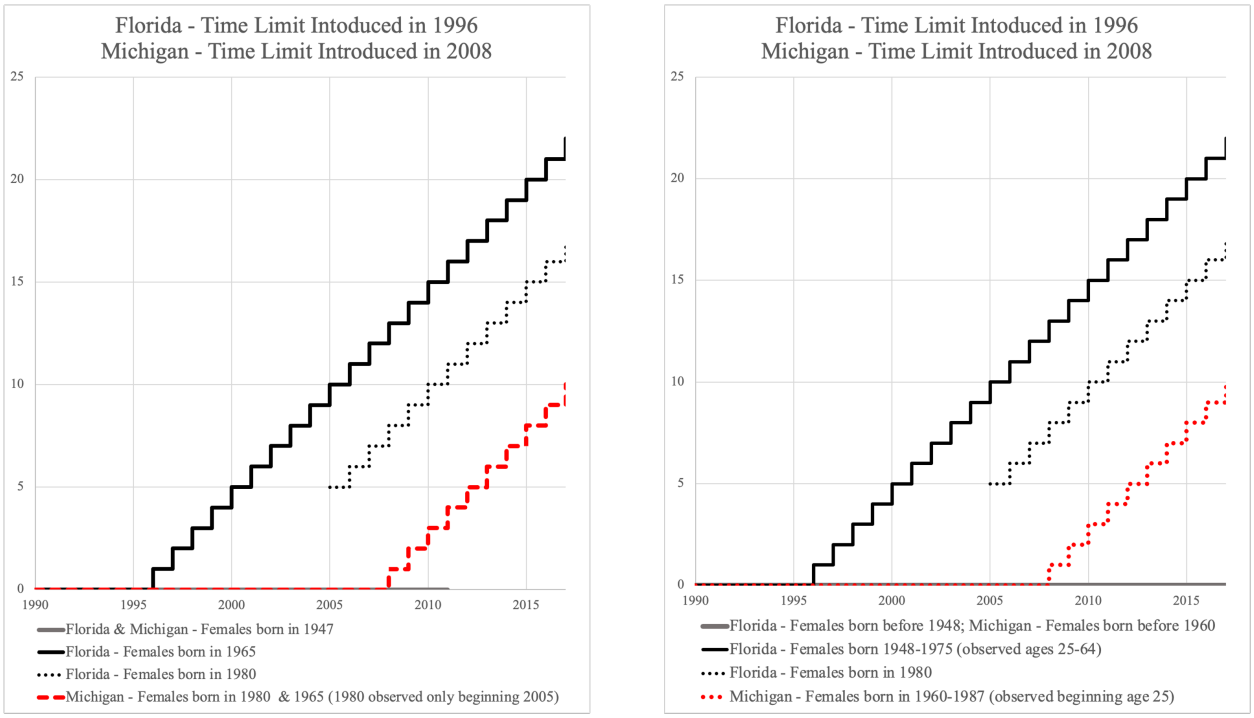
The dependent variable of "did the respondent migrate across state lines" is then regressed separately on each of these explanatory variables. We would expect statistically significant coefficients if the introduction of or presence of time limits in some states but not others caused individuals to migrate. Regression results are presented for all three explanatory variables in Table Appendix Table A1. As can be seen, all coefficient estimates are close to zero and not statistically significant.

Appendix Figure A1: Female Years of Exposure to Time Limits by Birth Cohort and State

(A) Florida vs. Michigan Presented Separately



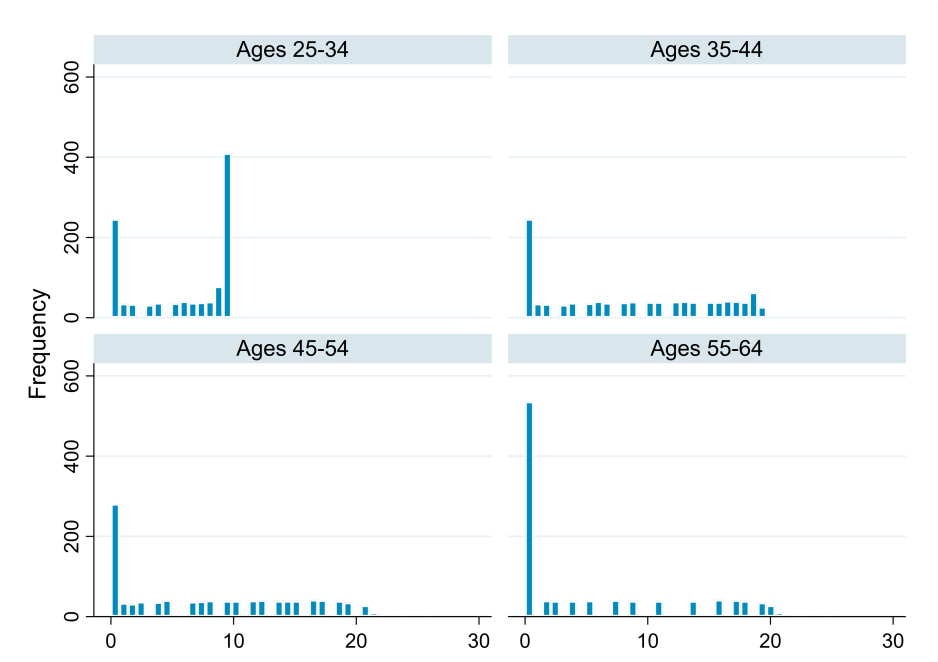
(B) Florida vs. Michigan Presented Jointly



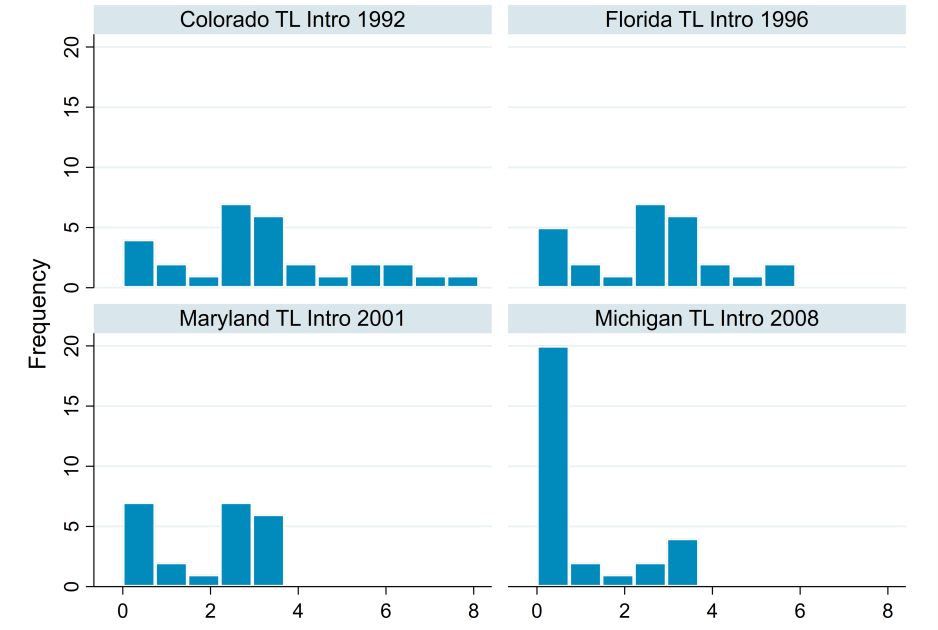
Notes: Both figures present the years of exposure calculated for different female birth cohorts in Florida and Michigan. The age of parenthood for females is assumed to be 21-48 (see Section 3 for justification), so years of exposure to time limits begins at age 21 or when time limits are introduced (whichever is later), unless the introduction occurred after age 48.

Appendix Figure A2: Variation in Female Years of Exposure to Time Limits

(A) Mean Years Exposure to Time Limits (Female) within Age Group

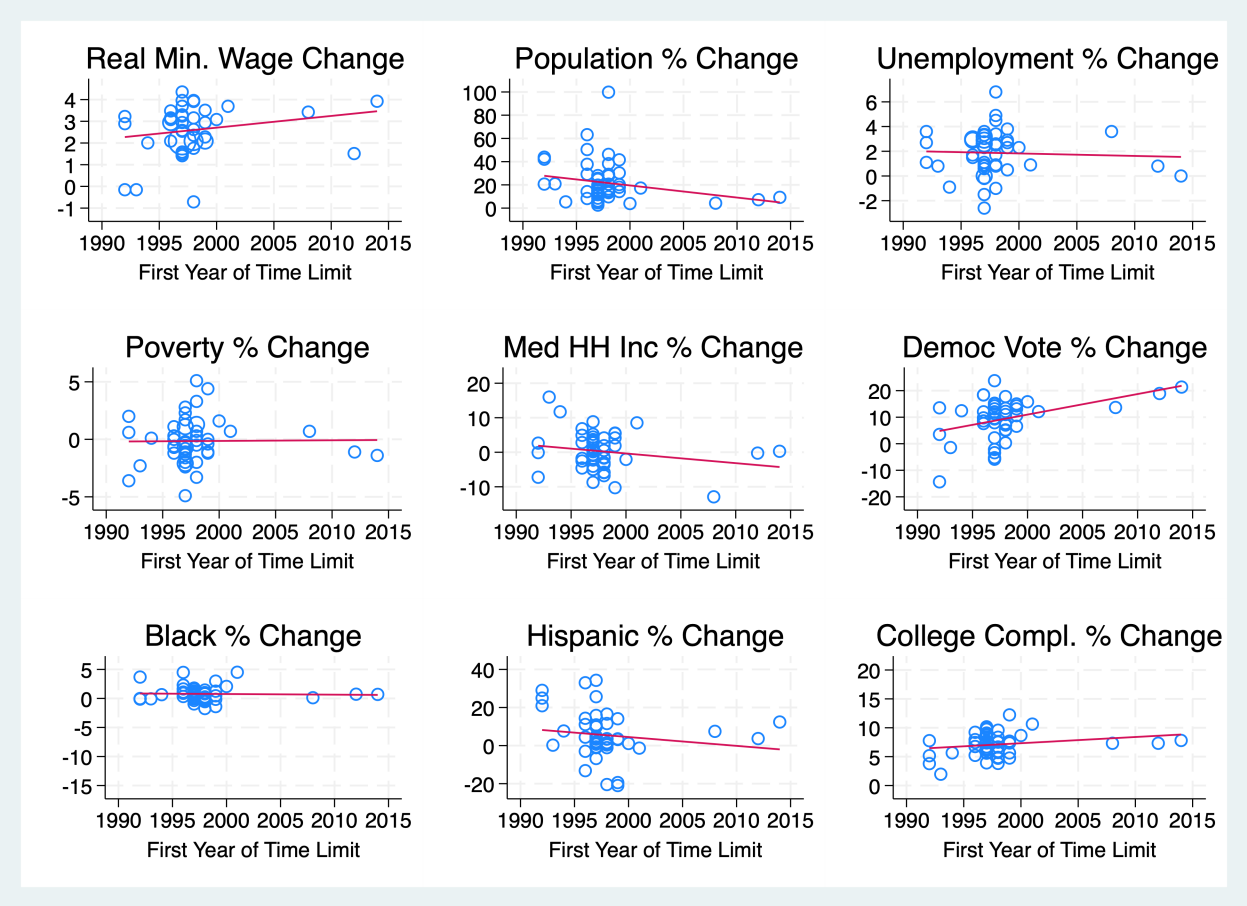


(B) Variation within State-Year Units



Notes: The top figure presents the frequency of values of the exposure to time limits variable by age group. The bottom figure presents the frequency of standard deviations for the years of exposure variable within state-year units (so across four observations representing the four age-groups in each state-year unit). Maryland has fewer state-year units due to dropping some of its state-year units because of high shares of despair deaths with unreported education levels (see Section 3).

Appendix Figure A3: Year of Time-Limit Introductions and Changes in State-Level Characteristics



Notes: The figure plots for each stated characteristic the difference in its values between 1989-1992 and 2008-2010 (exact years depend on data availability). The fitted linear regression is indicated in red.

Appendix Table A1: Time Limit Introduction and Migration Across States

	Dependent Variable: Migrated Over the Last Year (across states)		
Time Limits Introduced in State of Residence Last Year	-0.000571		
	(0.00137)		
Time Limits Existed in State of Residence Last Year		0.00113	
		(0.00131)	
No Time Limits in Current State of Residence			-0.000399
			(0.00138)
Observations	684,041	684,041	684,041
R-squared	0.158	0.158	0.158
Mean of dependent variable	0.0209	0.0209	0.0209
Mean of primary explanatory variable	0.0485	0.652	0.348

Notes: The dependent variable is an indicator equal to one if an individual migrated from another state over the last year. The sample is individuals from 1986 to 2019 (excluding 1995 due to low reliability of the migration variable - see https://cps.ipums.org/cps-action/variables/MIGRATE1description˙section) and includes parents where their youngest child is under 18 and who have only attained a high school diploma or less. We exclude observations that either reside in Iowa or Washington, DC or have migrated to those places. All regressions control for indicators for male, white, Black, and Asian American or Pacific Islander, a quadratic function of age, number of children, as well as state and year fixed effects.