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 despair mortality among low-educated females, thus contributing to the rising trends in despair mortality among the disadvantaged over the last two decades. Our back-of-the-envelope calculations suggest that time limits may have driven roughly 7.5% of low-educated female despair mortality increases through 2017.

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

JEL Classification: H23, H53, H71, I12, I14, I38, J22

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1 Introduction

The incidence of deaths from drug overdoses, suicides, and alcohol liver diseases (ALD) has increased substantially in the U.S. over the past two decades, to the extent 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.

The underlying causes of this increase in mortality have been attributed by some to 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 attribute the rise to demand factors that are 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 effects of person-specific and place-specific factors have also been evaluated (Finkelstein et al. (2018)). For the vast majority of investigations concerning the rising mortality rates, there is a consensus that the increase is much greater among and potentially even entirely attributed to low-educated Americans (Case and Deaton (2022); Novosad et al. (2022)).

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 to date has evaluated whether the U.S. welfare reform, one of the largest changes in the history of the U.S. social safety net, has played a role. 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 by up to five years. This restriction is in sharp contrast to pre-reform eligibility criteria that ensured low-income parents welfare payments as long as they resided with a minor in the household. It has been argued that 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. Time limits did reduce welfare take-up (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)). While this serves as concrete evidence that time limits affected low-income families, it is not clear whether this would positively or negatively affect despair mortality. This primarily depends on whether the decline in welfare take-up was accompanied by greater economic independence, stability, or horizon, or alternatively, greater financial distress. There is evidence of increased employment in response to time limits (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)), but the evidence on earnings is less conclusive (Fang and Keane (2004); Grogger (2003); Lichtman-Sadot (2023); Mazzolari and Ragusa (2012)).[1, 2]

[1]Looking at welfare reform more broadly, in terms of its transition from the AFDC to TANF (rather than just at time limits), there are numerous studies with mixed results on its effects on divorce and family structure (Acs and Nelson (2004); Bitler et al. (2004); Blau et al. (2004); Fitzgerald and Ribar (2004); Moffitt et al. (2020); Low et al. (2020)). Thus, although Case and Deaton (2015) specifically discuss changes in family structure as potential contributors to deaths of despair, it is not clear how welfare reform should have affected these.

[2]The effects of welfare reform on measures related to well-being have been investigated extensively over the last two decades. While improved well-being can be considered to reduce despair-related mortality, adverse well-being effects can potentially exacerbate it. The results of this vast literature are mixed. Some positive effects include evidence of decreased female and youth crime rates (Corman et al. (2014, 2017)), reduced food insecurity and welfare dependency and greater educational attainment among children exposed

The evidence on the heterogeneous effects of welfare payment restrictions has grown in recent years. Despite substantial evidence of favorable labor market changes among disadvantaged parents following these reforms, some broader assessments have also found that households representing more extreme cases lost access to welfare payments while remaining poverty-stricken and in need of these transfers. Greenstein (2016) finds that between 1995 and 2005 the rate of children living in extreme poverty conditions went up from 2.1 to 3 percent, representing an increase from 1.5 to 2 million children. Trisi and Sherman (2016) show that during this same period the income of the poorest tenth of children of single mothers fell by a mean of 18 percent. Parolin (2021) uses CPS data to decompose the decline in cash assistance between 1993 and 2016 and finds that only a fifth of this decline can be attributed to families that improved their economic well-being, while over half is due to reduced participation that is not accompanied by reduced demand for cash support. Lastly, Bitler et al. (2006) specifically examine time limits as part of their evaluation of Connecticut's Jobs First waiver in the mid-1990's. They find a reduction in income for a nontrivial share of the income distribution after time limits took affect. These findings support the notion that while welfare payment time limits may have improved the living standards of many disadvantaged families, this was accompanied by many other families experiencing deeper poverty and lack of access to cash transfers that can alleviate this.[3] These conditions could have led to increased despair among disadvantaged families.

In addition to potential despair increases among disadvantaged families due to income reductions, greater despair could have also developed from stress resulting from

to welfare reform (Corman et al. (2014); Hartley et al. (2022); Vaughn (2018); Corman et al. (2022)). Adverse effects include reductions in maternal educational attainment (Corman et al. (2014)), decreased parent-child engagement and parental time devoted to child health care (Baltagi and Yen (2016); Reichman et al. (2020)), increased adolescent delinquency and risky behavior (Dave et al. (2021)), and reduced health insurance coverage and utilization (Bitler et al. (2005); Kaestner and Kaushal (2003)), which may have also driven worse prenatal care and birth outcomes for infants of mothers exposed to the reform (Kaestner and Chan Lee (2005); Leonard and Mas (2008)).
Evidence of all these diverse effects of welfare reform does not pose a threat to interpreting our results on the effects of welfare time limits on mortality, as we are proposing to estimate this effect through multiple potential channels. What is crucial here is that we are capturing the effect of a substantial decline in the social safety net, even though this effect may operate through diverse channels and mechanisms.

[3]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.

increased employment, even if this was accompanied by higher income levels. This could be plausible for this vulnerable population that often juggles between substantial household finance constraints and childcare responsibilities. 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.[4]

Two studies are closely related to our work in their evaluation of the relationship between two different 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. By evaluating the two other main social safety net programs available to disadvantage families in the U.S., 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[5] 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.

[4]Recall that in the Canadian context there is no concern for changes in health insurance coverage due to increased labor force participation.

[5]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)).

While 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 to the best of our knowledge.

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. 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 results show increases in female despair mortality in response to exposure to time limits among deceased that are recorded to have a high school education or less. We also find greater precision in the estimated effect when narrowing the sample to states that had a poverty rate exceeding the median as of 1989. 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). This translates into over 23,000 deaths over a 20-years period.

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 lays out our empirical strategy and discusses our identification. Section 5 presents our main results, followed by robustness checks in Section 7. Section 8 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. In light of the continuous debate on the potential of welfare payments to distort household work decisions, beginning in the 1990s, the U.S. welfare system experienced a major reform. 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 the 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. Lifetime time limits on the receipt of welfare payments were among the more common waivers, ranging from 21 to 60 months. Under AFDC, eligibility for welfare payments was guaranteed until one's youngest child reached age 18. Upon implementation of PRWORA, welfare payments were federally limited to five years. States could offer lifetime welfare payments for more than five years at their own expense or restrict time limits even further than what was federally provided (using the funds for other welfare-related measures). 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.

The vast majority of welfare payment recipients has historically been and remains to date females, and more specifically single mothers. Despite this, some of our analysis will examine whether there was a male mortality response to welfare payment time limits, and this is due to Lichtman-Sadot (2023), which finds male labor market responses to welfare payment time limits that appear to be driven primarily through male partnerships to female adult recipients. Given the evidence in Lichtman-Sadot (2023), we do not rule out the possibility of a male mortality effect. However, we hypothesize that such an effect should be smaller and/or less precise if they exist, considering the substantially smaller representation of males that could potentially be exposed to welfare payment time limits, either directly or through partnerships to females.

3 Data

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.[6] 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 California and Maryland.[7]

Restricted-use mortality data was made available from the Center for Disease Con-

[6]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.

[7]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.

trol and Prevention (CDC) National Center for Health Statistics for the years 1989-2017. Based on these records, we sum the number of deaths for each gender by state and year of the deceased, and four age ranges: 25-34, 35-44, 45-54, and 55-64. Our deceased span ages 25-64, as in numerous 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.[8] We obtain mortality rates per 100,000 population by using population bases for these specific cells (at the state, year, gender, age group, and education level) from the CPS data.

We focus on those who were reported to have a high school education or less in their death record as our treatment group. 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, the U.S. welfare reform was more likely to directly affect single mothers as opposed to fathers, although not exclusively, as demonstrated in Lichtman-Sadot (2023). For these reasons, we examine mortality separately by sex.

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)

[8]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-Z64, 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 also when limiting the sample from 1999 onward - see 7.

that had missing or unknown education levels for over 5% of despair deaths.[9, 10] 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.[11]

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. Ap-

[9]This results in excluding 18-25 percent of state-year cells from our analysis, depending on the dependent variable (the base sample size changes due to singletons dropped from the regression). 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.

[10]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.

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

pendix 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 smaller ranges of exposure to time limits in most sample years.

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 2 presents trends across age groups over time in despair mortality among those with low educational attainment (i.e. high school degree or less) and exposure to time limits beginning during the age of parenthood by sex. Mortality rates increase all throughout our sample period. For males, the most dramatic changes are for ages 25-34, although the changes are very large and similar across all age ranges. For females,the largest despair mortality increase is for the 45-54 age group, while there is a sizable but notably smaller increase among the 25-34 and 55-64 age groups. Our explanatory variable measuring years of exposure to time limits is also rising over time, as more states introduce time limits and more years elapse since the introduction of time limits. However, when examining Figure 2 more closely, 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.

4 Empirical Strategy

4.1 Regression Specifications

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.[12] 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.[13] This results in the following regression specification:

[12]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.

[13]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) and Lindo 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 specifications 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.[14]

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.[15] 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.

In addition, in order to interpret the coefficient estimates in equations 1 as causal, we need to ensure that our results are not driven by other policies that were implemented at

[14]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.

[15]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.

the state level and may have also affected despair mortality. 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.

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 3A which plots values for the following characteristics as of 1990-1992: 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.[16]

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 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 3A shows this for state-level policies. In the first row of Figure 3B we examine three employment-related policies that were implemented or expanded during this period and affected low-income households: the EITC, minimum wages, and work requirement expansions, which were part of TANF.[17] The EITC and minimum wages

[16]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).

[17]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 expansions and waivers ("Work Requirement Expansions") timings are

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.[18] 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 changes in the real minimum wage between 1992 and 2016.

Some of the correlations in the graphs in Figure 3B appear stronger than those presented in Figure 3A. However, only for real minimum wage changes is the p-value below 0.3 (equal to 0.15).[19] We note that if a correlation between time limit introduction timing and changes in the minimum wages is driving our results that despair mortality increases in response to time limits, then this would be inconsistent with Dow et al. (2020) who find that higher minimum wages *decreased* despair mortality. As can be seen in Figure 3A, during our sample period, all states increased real minimum wages, with the exception of three states (Georgia, Oklahoma, and Wyoming) that experienced a very small decline. Thus, if the correlation between minimum wage changes and time limit introductions is driving our increased despair mortality results in any way (despite the state-year fixed effects in the regression specification), then this would have to be driven by three states that decreased real minimum wages to a very small extent and also appeared to have introduced time limits relatively early. This seems highly implausible.[20]

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. How-

from the DHHS - https://web.archive.org/web/20170201085413/https://aspe.hhs.gov/pdf-report/state-implementation-major-changes-welfare-policies-1992-1998.

[18]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).

[19]The correlation coefficient for minimum wage changes is 0.21.

[20]Nevertheless, we ran an analysis that exclude these three states from our regression analysis and our results are robust to this exclusion (not shown).

ever, 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.[21]

An additional threat to our identification can arise if there is a migration response 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 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 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 wish to increase the validity of our explanatory variable and empirical framework by presenting evidence that even in this broad population of

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

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. We restrict the sample to females ages 25-50 and run regressions with an indicator variable for welfare payment receipt over the past year.

Our regression framework replicates to a great extent the empirical framework presented in Section 4. Our sample period is 1989-2017, as in our mortality analysis. 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 limits 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.

Results are presented in Table 2 and show statistically significant reductions in the probability of receiving welfare payment in response to each year of exposure to time limits. Moreover, the magnitude of the effect is 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. 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, when we checked the samples in Table 2, less than 75% of these females had at least one child under 18 present in the household.

Our estimates suggest that each year of exposure to time limits on average reduced welfare payment receipt by 0.1 percentage points. With the mean years of exposure in the sample being 6.177, this implies a mean decline of 0.62 percentage points. We note that this reduction, although statistically significant, is much smaller than past estimates of the effects of time limits on welfare payment receipt.[22] This is also reas-

[22]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 declined by 0.66

suring, as our sample is much less focused on females potentially eligible for welfare payments than past studies, which generally focus on single mothers with a high school education or less, and our explanatory variable is a very rough measure of exposure to time limits.

5.2 Male vs. Female Despair Mortality Trends

Figure 2 shows rising despair mortality rates for low-educated males and females over our sample period. Furthermore, the magnitudes of these increases are relatively similar. Male rates increased from a mean across states of 10-40 per 100,000 population in 1989 to 120-140 per 100,000 population in 2017, while female rates increased from a mean of roughly 10 per 100,000 in 1989 to 50-70 per 100,000 in 2017. In tandem with these increasing despair mortality rates over the years, Figure 2 also shows steady increases in exposure to time limits over the sample period and this is by definition constructed relatively similarly for males and for females. With these clear increases over time in despair mortality and exposure to time limits, a positive correlation between despair mortality and time limit exposure is not surprising and should not necessarily imply that exposure to time limits increased despair mortality.

One finding that may be more indicative of a causal relationship between exposure to time limits and despair mortality is the existence of a stronger correlation between these two variables for females as opposed to males. We know that females were much more prone to be directly affected by limitations on welfare payments than males. Thus, if the correlation between exposure to time limits and despair mortality is stronger for females than it is for males, despite the overall trends being relatively similar, then this is evidence that variation across age groups in mean years of exposure to time limits has greater power in explaining the variation across age groups in despair mortality

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)).

for females as opposed to males. This is exhibited in Figure 4. 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 whose mortality rates vary substantially by level. What may come as a surprise in Figure 4 is the much stronger correlation exhibited in the female data as opposed to the male data. This is despite the years of exposure variable being constructed very similarly for both sexes. This is evidence that this variable is explaining rising trends in despair mortality that are unique to females and less relevant to males.

5.3 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. We do not hypothesize a-priori what the results of the fourth panel should be, as it is not clear whether male mortality should have been affected by time limits or not. Nevertheless, we do hypothesize that if there is an effect, it should be smaller in magnitude and/or with less precision in comparison to the results in the first panel, which evaluates the female deaths with the same criteria.

Each panel in Table 3 presents the coefficient estimate β from equation 1 for each dependent variable. 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 female despair deaths by 9.3 percent (1.32 multiplied by 7.08) annually on average 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 increases in the respective mortality 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, for 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.

5.4 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 effect 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 began to time limits. We replace 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. We further exploit the fact that a non-parametric regression specification can allow us to test for pre-existing 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 have introduced time limits.

The results of our dynamic analysis are presented in Figure 5. 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.[23] 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 5 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

[23]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.

time limits.

Figure 5 presents a close-to-zero and not statistically significant 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.

6 Results Discussion

6.1 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 5.3, this implies that 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 in the respective age group. 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 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=7.6) of the increase in this rate during the sample period. This is substantially smaller than the 65% 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 pandemic.

With low-educated females ages 25-64 having a base population of 33.4 to 38 million

during our sample period,[24] 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.

6.2 The Role of the Opioid Pandemic

Table 3 exhibits a null effect of welfare payment time limits on deaths resulting from drug overdoses, while suggesting increases in deaths resulting from suicide and ALD. We wish to verify whether indeed the increase in mortality is not driven by drug overdoses. To do this, we conducted several tests, which we outline below.

Our first test utilizes the regression specification outlined in equation1 but interacts the $YrsExposure_{ast}$ variable with an indicator for being a triplicate state. Triplicate states had policies in place that restricted pharmaceutical prescription writing, and as a result of this, the marketing of OxyContin upon its launching in 1996 was much less aggressive.[25] Alpert et al. (2022) find that triplicate states had substantially less exposure to opioid drugs, and this reduced the death rates from opioids and other drug overdoses as well. An interaction of our main explanatory variable for exposure to time limits with a triplicate state indicator could further validate our null effects found for drug overdoses in Table 3 if we find similar null effects even for non-triplicate states, for which the opioid pandemic was much more dramatic.

We present the results of our triplicate state analysis in Table 4, and at first glance, the results in the second column are consistent with welfare payment time limits not affecting deaths resulting from drug overdoses. The coefficient estimates for years exposure to time limits for non-triplicate states are far from being statistically significant. The coefficients on the interaction terms are also not statistically significant.

When proceeding to examine the regression results in Table 4 for suicide and ALD deaths, some interesting patterns arise. It appears that the coefficients on the interaction term between years of exposure and the triplicate state indicator are negative and statistically significant, and that this differential effect cancels out positive and sta-

[24] This base population calculation includes the entire United States and not just the states in the regression sample. It is based on our base population configurations using CPS data.

[25] There were five triplicate states: California, Idaho, Illinois, New York, and Texas (Alpert et al. (2022))

tistically significant effects of exposure to time limits for non-triplicate states. Thus, the results in Table 4 suggest that reduced access to opioid medication decreased the adverse effects of exposure to time limits on suicide and ALD deaths. While at first glance this may seem surprising, a review of the public health literature reveals that excessive use of opioids can lead to a greater risk of suicide or alcohol abuse. Witkiewitz and Vowles (2018) provide a review of the interaction between alcohol and opioid use and cite evidence showing that this is common (although under-studies according to Witkiewitz and Vowles (2018)). In particular, among a cohort of 5,307 adult patients (18 or older) with opioid use disorder in an electronic health record database, 23.4% also had a diagnosis of alcohol use disorder (Hser et al. (2017)). Furthermore, among adults in treatment for alcohol or drug use disorder approximately 68% had used prescription opioids non-medically in the 30 days prior to intake (Price et al. (2011)). Evidence of the interaction between opioid or substance abuse and suicide risk is also prevalent, with suicidal behavior acknowledged as a significant problem for people with co-occurring disorders seeking addiction treatment (Yuodelis-Flores and Ries (2015)). Rizk et al. (2021) provide a theoretical framework explaining how chronic opioid use can lead to neurobiological changes that elevate the risk of suicidal behavior.

We further explore the potential role of the opioid pandemic in the effect of time limits on despair mortality by running the same regressions as specified in equation 1 but limiting the sample years to varying years before 2017, our latest year in the base sample. If we were to observe results that are similar to those in Table 3 when looking at a time period that is before the surge in drug overdose deaths (but when there is already sufficient exposure to time limits), then this would be evidence that drug overdose deaths are indeed not affected by time limits. It can also be evidence that the opioid pandemic did not play a role in the adverse effects of time limits on despair mortality, as suggested in Table 4.

The results of this exercise are presented in Appendix Table A2 and suggest otherwise. Results are not statistically significant for samples that run through earlier years, which implies that either drug overdoses were affected by time limits or that the opioid

pandemic played a role in increasing the effect of time limits on despair mortality. In particular, a marginally statistically significant effect that is consistent with the results in Table 3 only begins to appear when the sample is limited to years through 2013. When limiting the sample to years through 2014 and onwards, the effects are highly consistent with those presented in Table 3.

Our last exercise calculates the contribution of time limits to rising trends in despair mortality that excludes drug overdoses. This is the same calculation that is presented in Section 6.1 but now we take the coefficient estimate from running a regression on deaths resulting from suicide or ALD (combined) and the mortality rates are for these two combined causes of death as well.[26] The coefficient estimate when examining low-education females is still not statistically significant at a conventional level, although the p-value is 0.106. For high-poverty states, the results become statistically significant and suggest that for each year of exposure to time limits, annual low-educated female deaths resulting from ALD or suicide increased by 1.12 percent.

Taking the estimate for for all states in the first column of Appendix Table A3 (0.743 percent increase) and multiplying that by the mean years of exposure in the sample (7.08) implies a 5.26 percent mean increase in annual deaths resulting from ALD or suicide. With a mean mortality rate in our sample of 18.75, this implies an addition of 1 death per 100,000 low-educated females. However, 1 death per 100,000 low-educated females is much less than the 3.5 deaths per 100,000 low-educated females that we found in Section 6.1 when calculating the contribution to despair mortality based on the estimate for despair mortality in Table 3. While the estimate in Appendix Table A3 is rather noisy (recall the 0.106 p-value), even if we add on to this estimate one standard error of the estimate, or alternatively, take the more precise and larger estimate for high-poverty states, we are still rather far from a 3.5 death increase per 100,000 population (although we get closer to 2).

This suggests that the adverse effects of welfare payment time limits on despair

[26]We note that some suicide deaths include drug overdose as a secondary cause of death. These are included in the drug deaths when we run regressions separately by each cause of death, as in Table 3.

mortality may not be confined solely to suicide and ALD deaths and may include drug overdoses as well. It may be that this effect is rather small in comparison to overall high mortality rates resulting from drug overdose (recall, we are only missing roughly 2 deaths per 100,000 population). If we take, for example, the statistically insignificant estimate of years of exposure on drug mortality for states with a high poverty rate (second panel of Table 3), it is 0.86 percent and far from being statistically significant. However, taking just this noisy estimate translates into 1.6 deaths per 100,000 population. This example demonstrates that a small increase in drug mortality in response to time limits is still possible despite being statistically non-detectable.

To summarize, at first glance, the results in Table 3 appear to imply that the effects of welfare payment time limits on despair mortality are not related to drug overdoses or the opioid pandemic. However, a closer analysis can suggest that the results are nevertheless related to the opioid pandemic in two ways. First, it appears that widespread use of opioids contributed to the adverse effects that exposure to time limits had on suicide and ALD deaths. Second, the gap between the death rate contribution calculated for all despair mortality as opposed to ALD and suicide mortality raises the possibility that time limits may have also adversely affected drug mortality, although this is statistically undetectable in our analysis.

7 Robustness Checks

7.1 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 5 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 and far from being statistically significant.

7.2 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 in Table 3, as males experienced similar (if not greater) upward trends in despair mortality but we do not find an effect of the time limit explanatory variable 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 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 5, 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).

7.3 1999 Start Year or Lower Thresholds for Missing Education Level

The criteria for our baseline sample is a start-year of 1989 - the earliest year we were able to obtain our restricted mortality data with reported education levels. We apply a threshold of 5% for excluding state-year units that had too many missing education levels for their despair deaths. 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 two panels

of Table 6, we limit the sample period to 1999 onward, rather than 1989 onward. In the bottom two panels of Table 6, we change the threshold for missing education levels to 4% of deaths, rather than 5% of deaths. Both changes 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.

7.4 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 (using 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 greater imprecision in our mortality rate measures as exposure to time limits increases 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). These population values do not suffer from potential small cell errors 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 7. 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 7. This reassures us that our null effects for the male population are not driven by imprecise population bases.

8 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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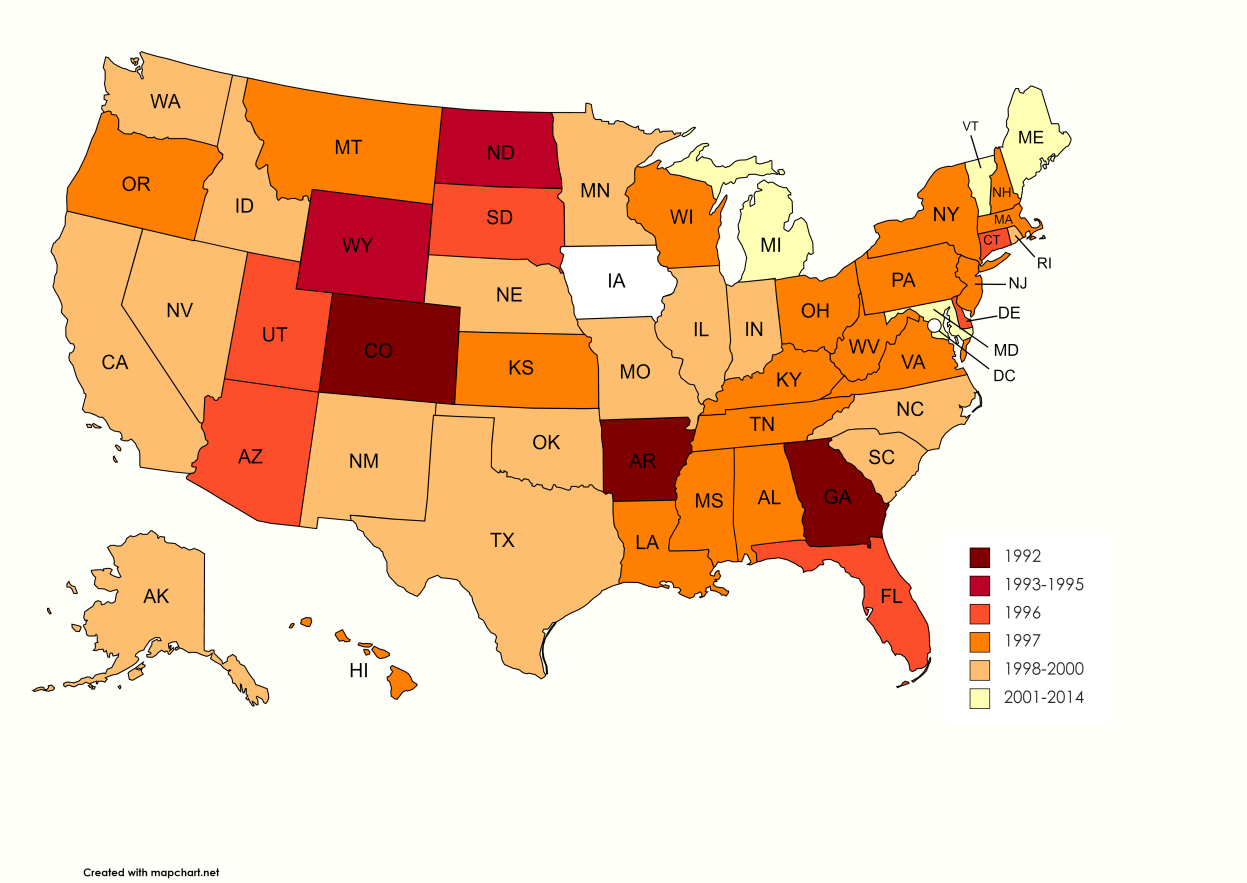
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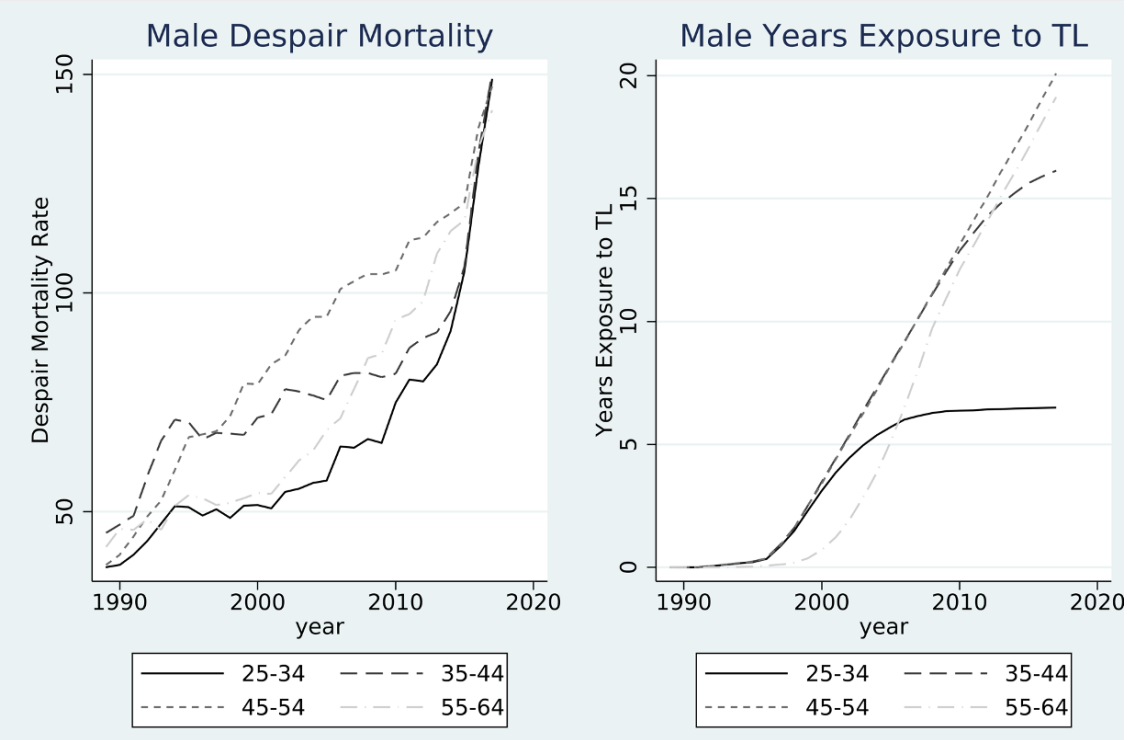
Figure 1: Time Limit Introduction Year



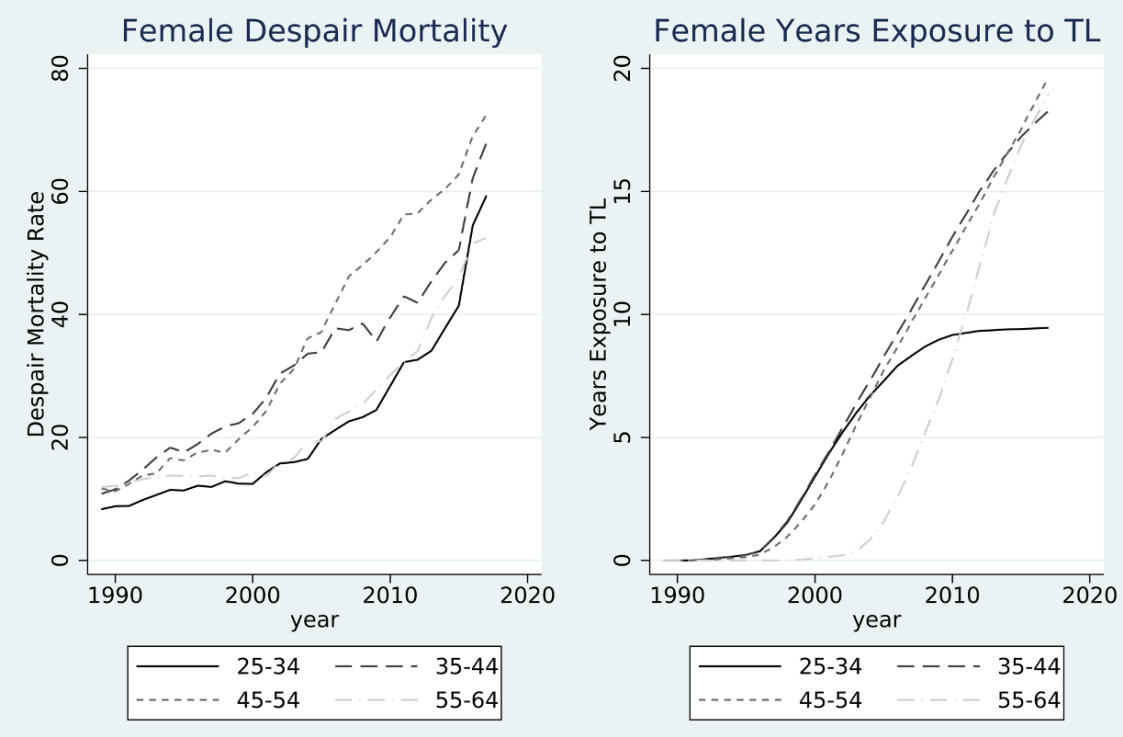
Notes: The figure presents the year of time-limit introduction and the number of months individuals were eligible for welfare payments during their lifetime upon the implementation of time limits, respectively. Iowa and Washington, D.C. are excluded from the sample. See Data Section for additional details.

Figure 2: Trends in Mortality and Time Limit Exposure by Age Range

(A) Males



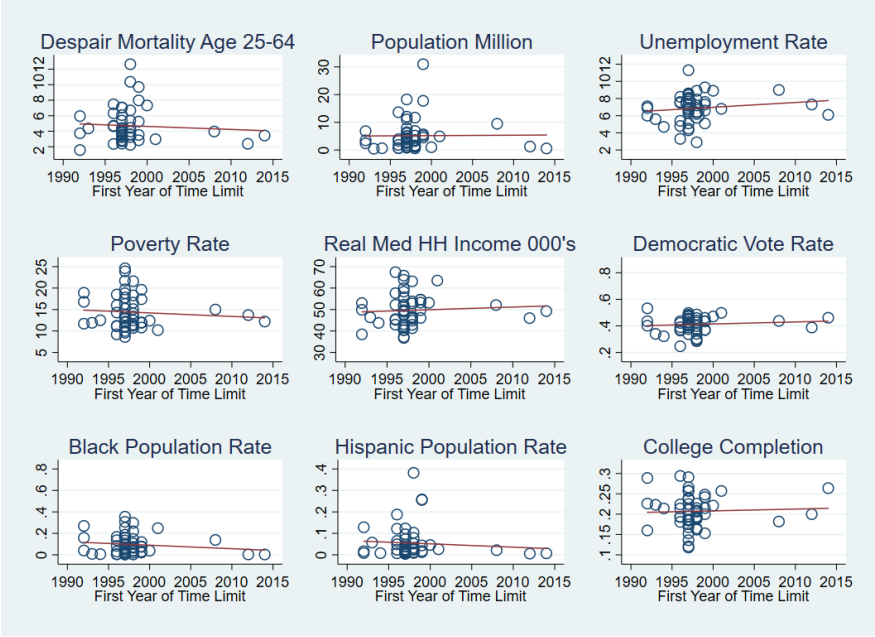
(B) Females



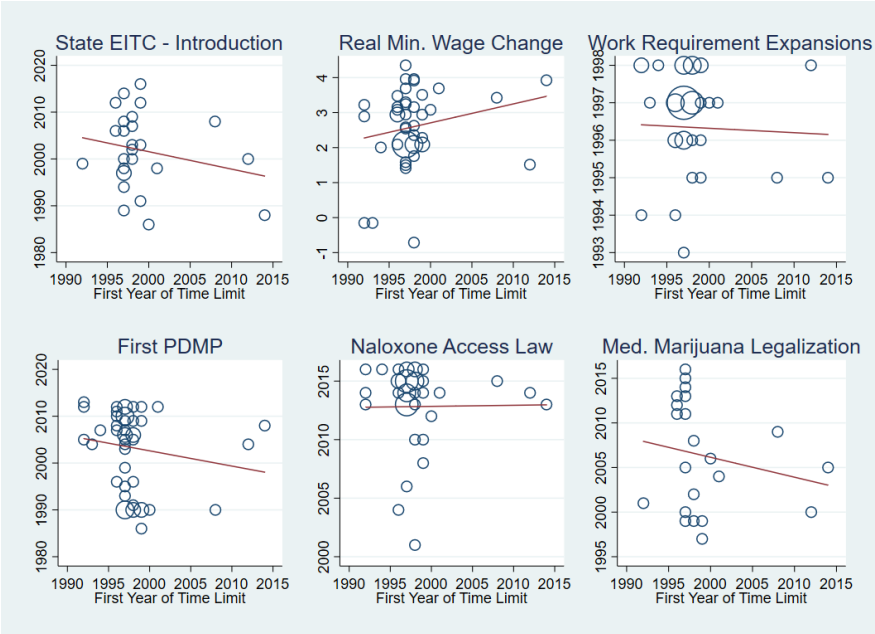
Notes: Both figures present means across states, excluding Iowa and Washington D.C. (see sample restrictions in Data Section). Each line represents the age range indicated in the legend in the bottom. Mortality rates are for the low-education population (high school degree or less). Years of exposure to time limits begins in the age of parenting minors, which is 24 and 21 for males and females, respectively.

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

(A) State-Level Characteristics

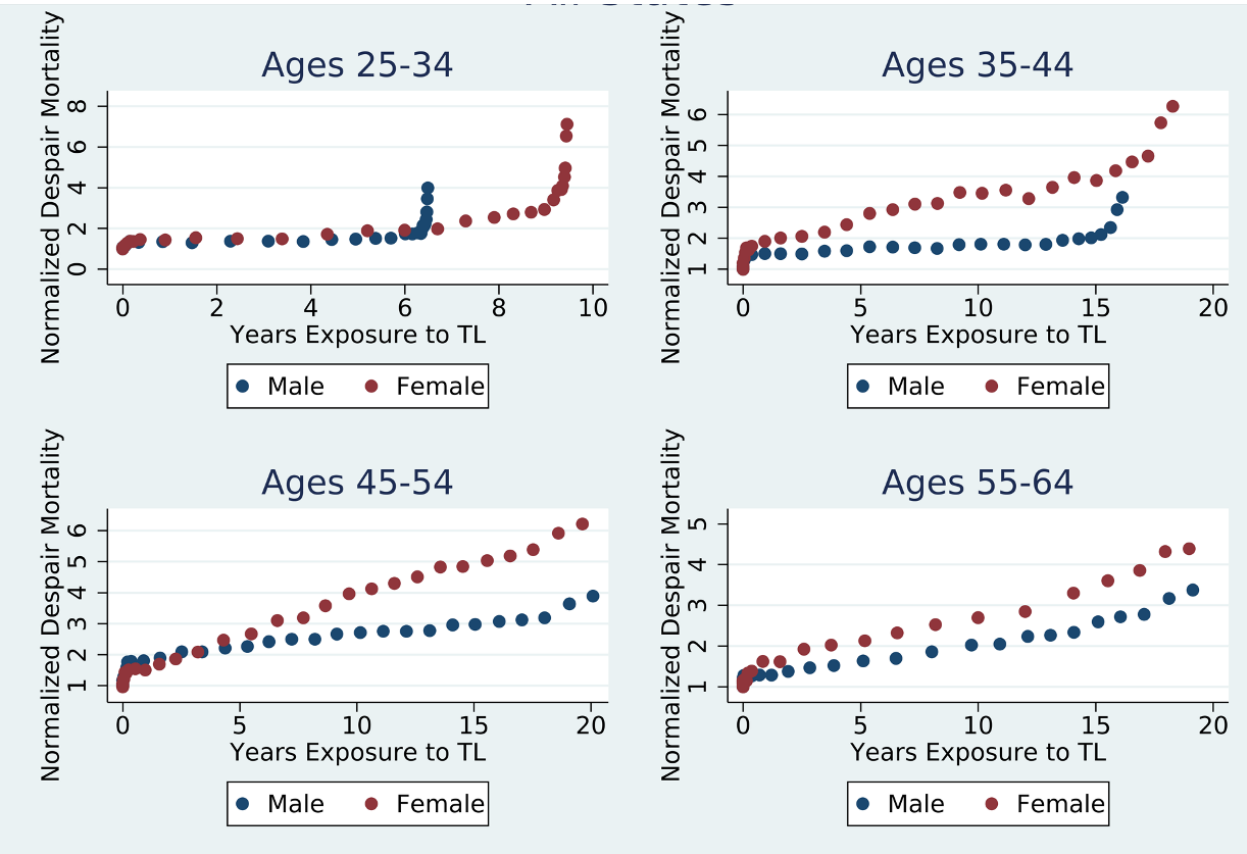


(B) State-Level Policies



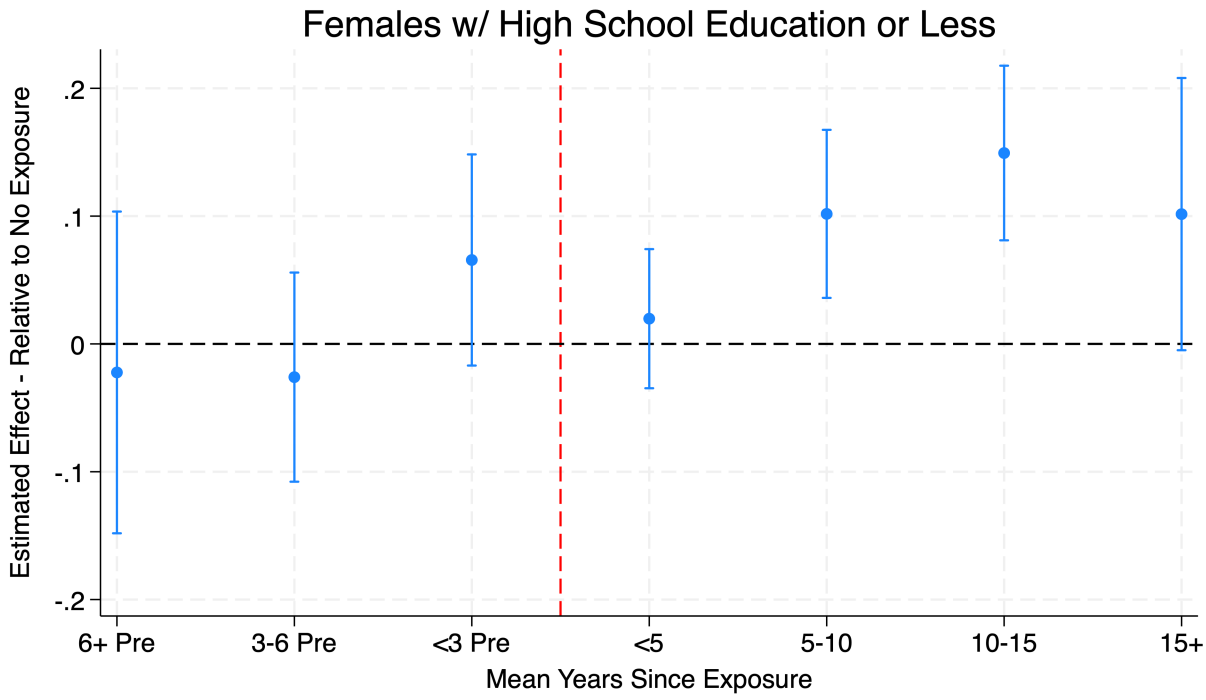
Notes: Larger circles indicate more states.
The top panel plots for each state the value of the stated state-level characteristics as of 1990-1992 and the years time limits were introduced. The bottom panel plots for each state the year the stated policy was introduced or substantially expanded and the years time limits were introduced, or real minimum wage changes between 1992 and 2016.
The fitted linear regression line is indicated in red.
Work requirements are based on JOBS (Jobs Opportunities and Basic Skills Training Program) waivers or expansions. For work requirements, 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 4: Despair Mortality Rate by Sex as a Function of Time Limit Exposure



Notes: The figure presents 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. For each age group, the mean despair mortality is normalized by dividing by the values in the earliest year, 1989.

Figure 5: Non-Parametric Despair Mortality Response to Time Limit Exposure - Low-Education Females



Notes: The figure presents the coefficient estimates, along with their 95% confidence intervals for the dynamic regression specification discussed in Section 5.4. 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.

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

	Female Low Education - All States	Female Low Education - High Poverty States
Years of Exposure to Time Limits	-0.00102**	-0.00124***
	(0.000388)	(0.000386)
Observations	408,850	249,311
R-Squared	0.037	0.038
Mean Welfare Receipt (Pre-TL)	0.0837	0.0881
Mean Years of Exposure to TL	6.177	6.176
State-Year Fixed Effects	Yes	Yes
Age Group Fixed Effects	Yes	Yes
Cohort Fixed Effects	Yes	Yes
Age Group Specific Linear Time Trends	Yes	Yes

Notes: The table presents the coefficient estimate β from a variation of equation (1) - an OLS regression (rather than PPML) with 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. The sample is females with a high school education or less from the March CPS Supplement 1989-2017 data. The second column limits 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	7.101	7.114	7.101	7.101

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: Triplicate State Analysis

	Despair	Drugs	Suicide	ALD
Females - High School Education or Less				
Mean Years Since	0.0134**	0.00195	0.00759*	0.00362
Exposure to Time Limits	(0.00573)	(0.00927)	(0.00439)	(0.00747)
Triplicate State X Mean	-0.00392	0.000711	-0.0221**	-0.00533
Years Exposure to TL	(0.00634)	(0.00630)	(0.00944)	(0.0189)
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	0.238	0.794	0.174	0.928
Females - HS Education or Less - States w/ 1989 Poverty $\geq$ Median				
Mean Years Since	0.0182**	0.00876	0.0124**	0.0122**
Exposure to Time Limits	(0.00717)	(0.0110)	(0.00527)	(0.00549)
Triplicate State X Mean	-0.00979	-0.00382	-0.0265**	-0.0309**
Years Exposure to TL	(0.00762)	(0.00890)	(0.0104)	(0.0135)
Observations	2,032	4,248	2,032	2,032
Mean Mortality Rate	39.44	23.38	10.80	8.594
P-Value for Joint Significance	0.259	0.629	0.265	0.172

Notes: The table presents the coefficient estimate β from a variation of equation (1) that interacts the $YrsExposure_{ast}$ variable with an indicator for being a triplicate state (see Section 6 for details). Triplicate states are: California, Idaho, Illinois, New York, and Texas (Alpert et al. (2022)). 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 5: 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	7.50e-05	0.000974	0.000565	0.00414
	(0.00237)	(0.00335)	(0.00359)	(0.00471)
Observations	4,256	2,032	4,256	4,256
Mean Mortality Rate	150	152.8	88.28	198.3
	Males - High School Education or Less - Female Explanatory Variable			
	Despair	Drugs	Suicide	ALD
Mean Years Since Exposure to Time Limits	0.00381	-0.00915	-0.00125	0.00500
	(0.00555)	(0.00947)	(0.00347)	(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 6: 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 7: 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. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

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

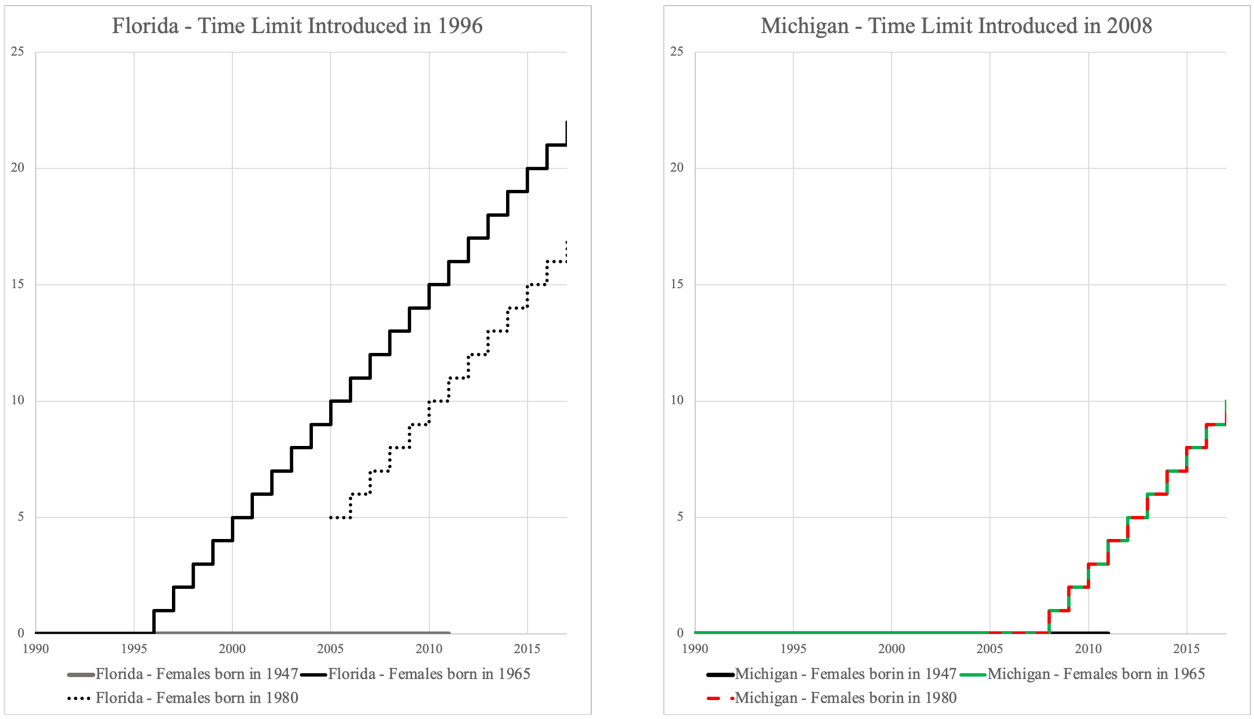
The data is from Current Population Survey (CPS)'s Annual Social and Economic Supplement (ASEC, also known as the "March Supplement" after the month it is administered in). This survey contains a question as to whether a respondent has changed residences in the course of the past year, and if so whether 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.

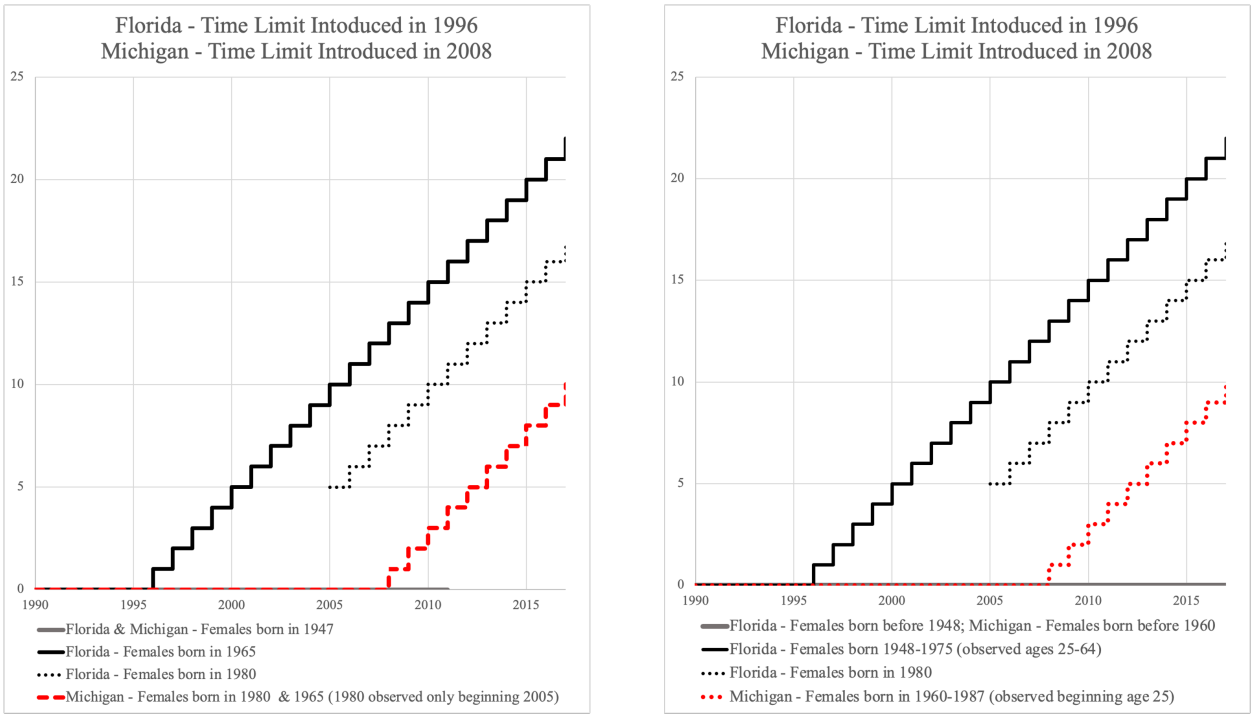
The dependent variable of "did the respondent migrate across state lines" is then regressed separately on each of these explanatory variables. We would expect statistically significant coefficients if the introduction of or presence of time limits in some states but not others caused individuals to migrate. 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



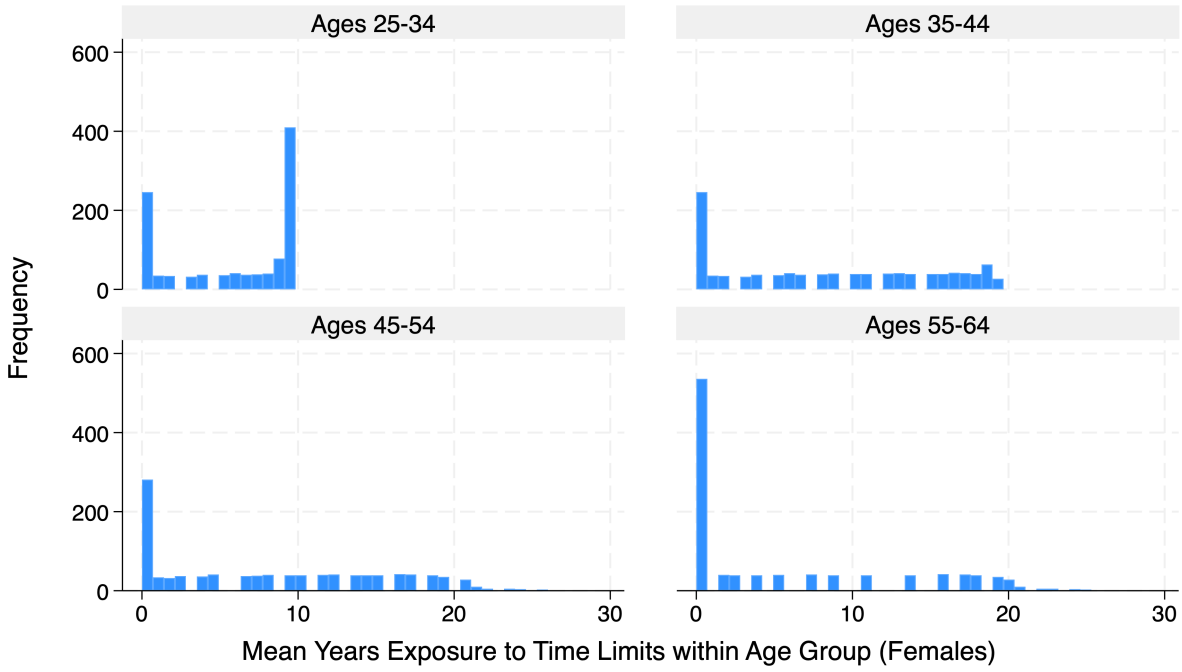
(B) Florida vs. Michigan Presented Jointly



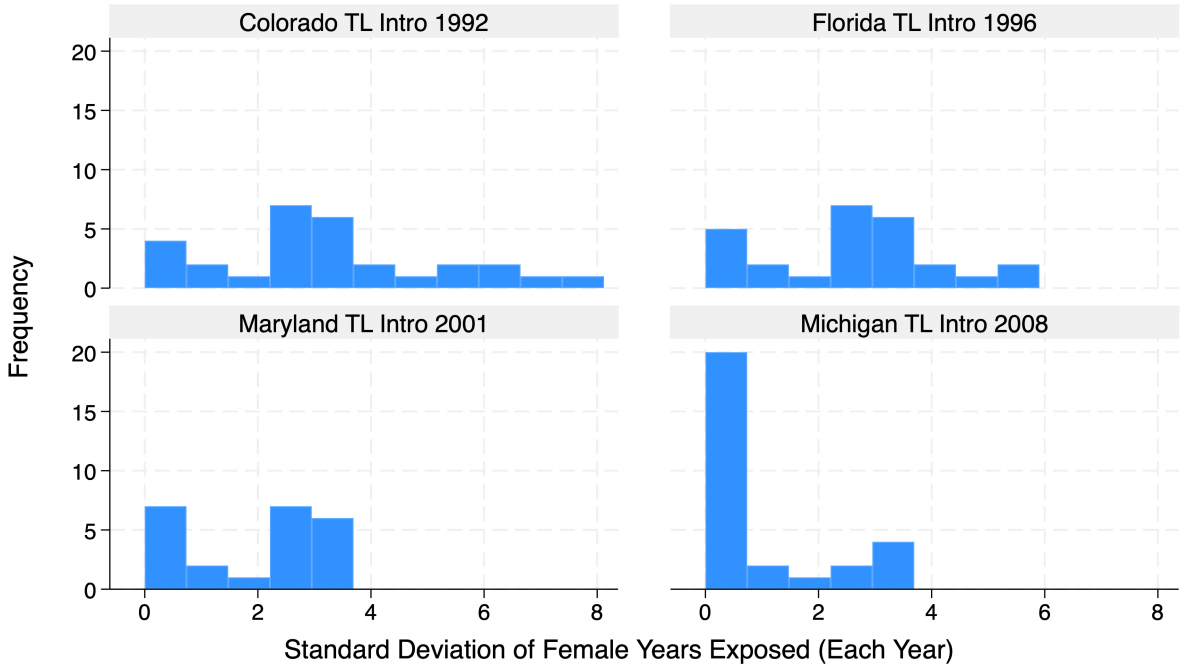
Notes: Both figures present the years of exposure calculated for different female birth cohorts in Florida and Michigan. The age of parenthood for females is assumed to be 21-48 (see Section 3 for justification), so years of exposure to time limits begins at age 21 or when time limits are introduced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

Appendix Table A2: Welfare Payment Time Limits and Despair Mortality - Sample Running Through Different Years

	Female Low Education - All States	Female Low Educ. - High Poverty States	Female High Education	Male Low Education
		Until 2006		
Mean Years Since	0.00806	0.00461	0.0250	-0.00664
Exposure to Time Limits	(0.0113)	(0.0152)	(0.0169)	(0.0202)
Observations	2,332	1,032	2,308	2,332
Mean Mortality Rate	23.47	23.91	10.24	72.89
		Until 2011		
Mean Years Since	0.00919	0.00708	0.00724	-0.000184
Exposure to Time Limits	(0.00695)	(0.00978)	(0.00639)	(0.0107)
Observations	3,188	1,476	3,164	3,188
Mean Mortality Rate	29.22	30.61	10.88	80.57
		Until 2013		
Mean Years Since	0.0120*	0.0147	-0.00233	-0.00124
Exposure to Time Limits	(0.00632)	(0.00899)	(0.00548)	(0.00961)
Observations	3,532	1,660	3,508	3,532
Mean Mortality Rate	31.48	33.26	11.05	83.62
		Until 2014		
Mean Years Since	0.0147**	0.0191**	-0.00378	-0.00123
Exposure to Time Limits	(0.00612)	(0.00857)	(0.00507)	(0.00934)
Observations	3,716	1,756	3,692	3,716
Mean Mortality Rate	32.85	34.55	11.20	85.56
		Until 2015		
Mean Years Since	0.0146**	0.0197***	-0.00534	0.00105
Exposure to Time Limits	(0.00589)	(0.00766)	(0.00520)	(0.00896)
Observations	4,072	1,940	3,868	4,072
Mean Mortality Rate	36.03	37.78	11.35	90.77

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 year through which the sample runs through is stated on the top of every panel. Standard errors clustered at the state level are in parenthesis. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Appendix Table A3: Welfare Payment Time Limits and Alcohol Liver Disease and Suicide (Combined) Mortality

	Female Low Education - All States	Female Low Educ. - High Poverty States	Female High Education	Male Low Education
Mean Years Since Exposure to Time Limits	0.00743	0.0112**	-0.00415	-0.00254
	(0.00459)	(0.00555)	(0.00507)	(0.00675)
Observations	4,256	2,032	4,228	4,256
Mean Mortality Rate	18.75	19.41	8.342	60.27

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.Standard errors clustered at the state level are in parenthesis. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$