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Welfare reform, time limits, and infant health

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ABSTRACT

This paper offers evidence that welfare time limits contributed to a deterioration of infant health. We use the fact that the dates at which TANF recipients were first subject to timing out varied by state. We show that by 2000 there was a marked difference in TANF duration spells depending on whether the state employed the 60-month Federally imposed time limit, or a shorter limit, differences that were not present under AFDC. There were significant increases in infant mortality when time limits became binding in a state. These increases occurred primarily among mothers who could have plausibly timed-out of TANF: poorly educated and unmarried women with at least one previous live-birth. There is some evidence that the population of mothers affected by time limits were less likely to seek prenatal care in the first trimester, suggesting a possible role for reduced medical care in explaining the deterioration in infant health.

Welfare reform enacted under the Personal Responsibility and Work Opportunity Reconciliation Act of 1996 (PRWORA) sought to end a culture of dependency. Reform of the U.S. welfare system was designed to reduce a moral hazard problem: by insuring against the effects of low income and joblessness, welfare (AFDC in particular) had reduced the incentives to work. This tradeoff lies at the heart of debates over welfare reform: how to aid those in need while preserving incentives for people to take care of themselves. To strengthen work incentives and reduce welfare dependency, the Temporary Aid to Needy Families (TANF) program that replaced AFDC placed a 60 month federal cap on lifetime benefits—a dramatic and controversial change in policy. Reform also devolved many aspects of welfare policy from federal to state-level control, including the discretion to impose intermittent (per-spell) caps, and to impose shorter lifetime caps. By the end of the year 2000, 21 states had begun expelling families from TANF as they reached these new time limits.[1]

Critics of welfare reform feared it would have a devastating impact on the poor. In Senate hearings over the proposal, Senator Daniel Moynihan stated that between 2001 and 2005 the federal 5-year time limit would force nearly 5 million children off welfare, half of them black, and called dropping millions of children from this life support system "the most brutal act of social policy we have known since the reconstruction." (U.S. Senate Finance Committee Hearings, 1996). The Clinton administration's Assistant Secretary of Housing and Urban Development, Andrew Cuomo, had in 1994 warned that ending, rather than decreasing welfare payments would leave women and children on the streets, and asked "Then what do we do? Watch them wither and die?" (DeParle, 1994).

Our focus is on the effect of welfare reform on infant health, a particularly well-measured outcome not directly targeted by PRWORA. Welfare reform could affect the health of infants through a number of channels. The most optimistic observers

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[1] Throughout we treat the District of Columbia as a State.

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hoped that employment and an improved ethic of personal responsibility might improve health. The prospect of reduced welfare support could deter some from having children, alter the timing or number of pregnancies, or induce changes in marital status. But welfare reform also creates opposing forces. Work requirements may reduce the time available to attend to health needs. Both the threat and the realization of benefit cutoffs could adversely affect mothers' health. Most directly, families expelled from TANF lose benefits, and this economic deprivation may affect infant health. In 2002, 13.8% of recent TANF leavers reported themselves without income from any source (Loprest, 2003). Even though Medicaid benefits are in principle still available to these families, Medicaid take-up rates remain low (Aizer, 2003).

We first show that there is testable exogenous variation in welfare policy across the states. Well before the 2001–2003 economic downturn, states reached TANF agreements with the federal government, and adopted different TANF time limits that would push their first cases off TANF after spell durations and at dates that differ by state in a pre-determined way. The TANF cut-offs imposed a constraint binding in practice: by 2003 the 20 states with shorter time limits indeed had shorter TANF durations. We examine how the infant mortality rate changed after a state's time limit became binding, a date which varies across states.

TANF time limits allow us to isolate the impact on infant mortality of changes in TANF use that are not caused by contemporaneous economic or social changes, nor by omitted individual characteristics that might affect both spell length and infant mortality. For our purposes, a crucial aspect of these time limits is that they were set well before the recession of 2001. Their enactment prior to the recession precludes their being influenced by the severity of the subsequent economic downturn.[2,3] Time limits are more attractive for our purposes than changes in case loads following welfare reform because case loads are endogenous, depending on a number of factors, such as the economy, that could also influence infant mortality.

While we control for a variety of observable characteristics of the mother, and of the state economy, we are cognizant that there could be state and year variation in infant health for reasons other than time limits. Welfare reform was a complex mixture of policy changes, and it occurred against a backdrop of other economic and policy changes which could, in theory, have affected the quality of medical care in states differentially over this time period (Moffitt and Ver, 2001). These complex sets of changes in a relatively short time frame across a limited number of states do not provide a promising environment in which to try to isolate a single causal path.

To help distinguish the impact of TANF time limits from other changes that may have occurred, we contrast infant mortality between first versus higher-order births. A mother's initial entry onto TANF is typically associated with first birth. Biology and time limits being what they are, first births are unlikely to run into TANF time limits. Subsequent births are the most likely to be directly affected by TANF time limits. We contrast the health of higher-order versus first-born infants as time limits bind, allowing us to identify the effects of time limits beyond state-specific changes that might affect all infants irrespective of birth-order. Finally, we argue that any divergence in infant health between first and higher order births when time limits bind should occur for mothers who are plausibly on welfare, those with little formal education and who are unmarried.

As the time limits began to bind extensively after 2000, states in which TANF families faced shorter time limits experienced a greater increase in infant mortality than did states with longer time limits. We show that these effects are not typical of recessions, nor do they represent a fixed difference across states prior to welfare reform, nor are they due to shifts in the population of mothers in states with short time limits. Among low-educated mothers, this differential change in infant mortality occurred in higher order births but not in first births. By contrast, the gap in infant mortality that emerges between higher order and first births when time limits bind is not present amongst women that had attended college by the time of birth. These complementary findings increase our confidence that we have identified an effect of welfare reform on infant mortality. Moreover, the results are robust to the inclusion of a variety of prominent alternative policies that may be related to welfare, including family caps, sanctions, and State EITC. We also find some evidence that prenatal care was delayed for the affected group over this period, suggesting a possible mechanism explaining these changes in infant health.

The benefits of welfare reform have entailed some cost in terms of infant mortality. Despite the good intentions behind the formal delinking of health from welfare benefits, the evidence indicates that infant mortality rates increased in states that had imposed shorter limits on the duration of TANF benefits. The reform of welfare has not forged a complete escape from welfare's fundamental tradeoff.

1. Related literature

There is evidence that infant mortality fell when the nation first provided welfare to single mothers. Fishback et al. (2007) report that increases in New Deal public assistance led to reductions in infant mortality. These original welfare policies were initially aimed at an arguably less selected group of mothers: widows, who possibly started with greater economic and social resources than later welfare generations.

To our knowledge, this is the first study that seeks to examine the effect of TANF time limits on infant mortality. There are related but still small literatures on other birth outcomes, the health of mothers, and on the effect on infant mortality

[2] No state extended formal time limits during the recession. If short states differentially weakened administrative enforcement of time limits during the recession, this would attenuate differences between short and long states.

[3] We will show that the time limits are uncorrelated with changes in infant mortality in the prior 1990–1991 recession.

of other earlier welfare programs, WIC in particular.[4] These studies generally find a positive association between welfare programs and various measures of infant health. Kaestner and Lee (2005) find a positive relationship between state-level welfare caseloads and first trimester care, number of prenatal visits and birthweight. Currie and Grogger (2003) find that declines in welfare caseloads are associated with an increase in low birthweights and a decline in prenatal care.[5] In a study using data up to 2000, possibly before there were a critical number of cases that were closed due to reaching time limits, Bitler et al. (2005) find no statistically significant effects on health status, but do find that welfare reform is associated with reductions in health insurance coverage and some measures of healthcare utilization.

Numerous studies show that the Supplemental Feeding Program for Women, Infants and Children (WIC) reduced infant mortality, raised birthweight, and increased gestational age (Devaney et al., 1992). Brien and Swann (2001b) find positive WIC effects on birth outcomes among blacks, but not among whites. It is commonplace in this literature to observe both that blacks are overrepresented among program beneficiaries, and that despite this, black infant health outcomes are persistently worse than those of whites. Indeed, the difference in infant mortality between the US and the rest of the developed economies is concentrated among US minorities.

Studies of the impact of national welfare programs are often overshadowed by difficulties of identification. Using time-series data alone, it is difficult to distinguish between welfare reform and contemporaneous trends. With individual micro-data, variation in welfare enrollment and use among those at risk of becoming beneficiaries is clouded by unobserved variables that make one person, but not her observationally identical clone, a beneficiary. These unobserved individual, family, or neighborhood characteristics may lead to biased estimates of the effect of welfare take-up on infant health.[6] The daunting methodological challenges to an informative research design may help explain why, in her 1998 review of the effects of welfare on child outcomes, Janet Currie finds no studies to review on the effect of AFDC on infant mortality (Currie, 1998).[7]

2. Welfare reform allowed cross-state variation in TANF time limits

Welfare reform is sometimes mistakenly thought of as a discrete change in federal policy uniformly and simultaneously applied to all states. Welfare reform was well underway before passage of PRWORA in 1996, which ended AFDC as an uncapped entitlement and replaced it with TANF. An increasingly flexible system of state by state waivers from federal AFDC standards began with the first state waivers in October 1992. By February 1997, at least 30 states had implemented major AFDC waivers, and at least 8 of these placed time limits on AFDC benefits (CEA, 1999, Tables A and B). This period of administrative experimentation under waivers became the first step towards the formal devolution of power to the states under PRWORA. States were given fixed federal block grants, and as residual claimants, considerable discretion to allocate them and to design welfare programs subject to a set of minimum and maximum standards.

The ideological core of welfare reform was to replace welfare as a system of entitlements with a temporary aid system that would encourage personal responsibility with employment as the goal. Perhaps the most striking and contentious aspect of welfare reform was the federal imposition of a 5-year lifetime cap on receipt of Federally funded TANF benefits. At the time welfare reform was enacted, Pavetti (1996) of the Urban Institute testified before Congress that 48% of current AFDC recipients had received benefits for more than 5 years, and so would be left exposed by TANF's Federal time limit.

At their option, states could extend Federally funded TANF assistance beyond the five-year limit for up to 20% of the average monthly caseload during the current or preceding fiscal year, on the basis of hardship or abuse. At their discretion, states could also cap TANF benefits at shorter durations, both per lifetime and per spell. Many states limited intermittent or lifetime benefits to 24 months, reaching as low as 12 months under some conditions in Texas. In the supplementary Electronic Appendix Table 1, we report time limits by state. Twenty states chose time limits more stringent than the 5-year federal cap.

3. Testing whether time limits are a binding constraint

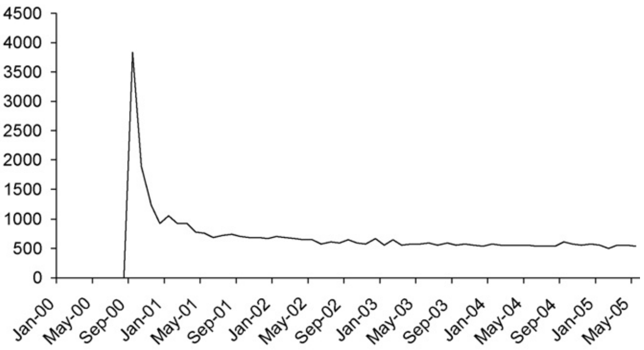
In practice, states have 18, 21, 24, 36, 48, or 60 months time limits. We use this variation in time limits across states to estimate the effect of involuntarily losing TANF benefits. For example, the timing is such that all but one of the states with time limits at or below 48 months began expulsions before 2001. According to the Department of Health and Human Services (DHHS), the earliest time limit expulsions under TANF began in November, 1997. Among states exercising caps, the mean date at which the first family hit the cap was October, 2000. The standard deviation of first cut-off across states is 21 months. By February 2002, more than 30% of the states had yet to have a family hit its time limit.

[4] For example, see Bitler et al., 2005, 2006; Brien and Swann (2001a,b), and Devaney et al. (1992).

[5] An earlier set of studies examined AFDC and NIT effects. Currie and Cole (1993) use both IV techniques and sibling comparisons and find that conditional on income, AFDC had no significant impact on low birth weight. In their study of the Gary Negative Income Tax experiment, Kehrer and Wolin (1979) find that the estimated impact of the NIT after controls is to increase birthweight of newborns in most maternal age groups. Note that Almond et al., 2002 find that low birth weight has modest effects beyond those of gestational age on infant mortality.

[6] Complicating matters, Danielson and Klerman (2004) document the sluggish response of caseloads to policy changes, suggesting that analyses seeking to evaluate the instantaneous impact on caseloads of a sharp change in policy may underestimate the effect in steady-state.

[7] See Blank (2002), Moffitt (2003b), and Currie (2006a) for additional comprehensive reviews of PRWORA.



Source: Ohio Department of Jobs and Family Services.

Fig. 1. Cases terminated in Ohio after reaching 36 month time limit.

The number of involuntary exits due to TANF time limits remains an open question, in part because of the scarcity of data on flows onto and off of welfare.[8] After enactment, some analysts were projecting that as many as 2 million families and 3.8 million children would hit the 5-year limit (Duncan et al., 2000), a fear made hollow by the ensuing boom.

While state-level data on monthly time limit terminations are not generally available, we can learn something about the time pattern of time limit terminations by examining the case of one state – Ohio – for which we were able to obtain information on monthly expulsions. Fig. 1 shows the number of cases terminated in Ohio after reaching its 36-month time limit. The first cases reached the time limit in September 2000. In that month there was a spike in the number of terminations, reflecting the stock of long-term recipients reaching time limits. The number of expulsions then declined, reaching a steady state of approximately 500 terminations per month. As Danielson and Klerman (2004) suggest, not all states follow this pattern.

Others have used aggregate caseload statistics published by the DHHS and state welfare agencies. Using this monthly caseload (stock) data from the DHHS, Danielson and Klerman (2004) find evidence of an anticipatory drop in welfare participation in short time-limit states. Such a drop is consistent with the forward-looking behavior of welfare participants first highlighted by Grogger (2002) and Grogger and Michalopoulos (2003). The authors conclude that a relatively small share of the decline in caseloads during a period including the 1990's boom can be accounted for by changes in welfare policy. Specifically, they estimate that time limits contributed to 12% of the reduction in the welfare caseload from its 1992 peak. Fang and Michael (2004) estimate a similar share. Including both mechanical reductions (hitting time limits) as well as eligibility banking of the type studied by Grogger et al. and Fang and Keane, Mazzolari (2007) finds in SIPP data that time limits caused 38% of the drop in welfare use between 1996 and 2003. The decline in welfare participation since the mid-1990s has been so large that even a moderate share of that decline represents a substantial number of cases affected by policy. In addition, there is some evidence that caseloads became more cyclically sensitive after welfare reform.

The use of stock data is not ideal since time limits should most directly influence exit rates, but the stock is confounded by entry. Bloom, Farrell and Fink (2002) surveyed states directly to count the number of families who timed-out. They estimate that 176,455 families had reached a state time limit by September of 2001. For a number of reasons, this figure is likely to underestimate the number of families that lost benefits due to time limits. Administrative and reporting differences across states affect these counts. First, this figure does not include cases that were closed just prior to reaching time limits. In Georgia, for example, many cases are closed in month 47 of the 48-month time limit because recipients do not attend a meeting to determine whether an extension will be made. Second, as the authors note, this estimate does not include cases that were closed due to time limits after receiving an extension. Third, the estimate does not include closed cases from two states with short time limits: Delaware and Nevada. Fourth, with each passing year, more families in more states reached their respective state limits. Because we do not have information on the number of timed out cases in short time-limit states that occurred nationally by the end of 2003, we surveyed several state welfare agencies to update the Bloom et al. (2002) estimate. While we were not able to obtain this figure from many of them, those that did have it available yield a count approximately double that in September 2001.[9] The information cooperating agencies provided us leads us to believe that doubling the Bloom et al. (2002) figure of 176,455 (approximately 353,000) families is a reasonable and conservative estimate of the number of families exiting TANF due to time limits alone by the end of 2003. Reassuringly, this count is very close to the one implied by the estimates in Danielson and Klerman (2004).

[8] The Current Population Survey has limited information on the duration of cash assistance. The Survey of Income and Program Participation samples are too small to make informative cross-state comparisons for our purposes.

[9] We obtained information from Connecticut, Florida, Indiana, Massachusetts, Georgia, Ohio, and Texas. We also obtained counts from Nevada and Delaware, states that were missing from the Bloom et al. (2002) analysis.

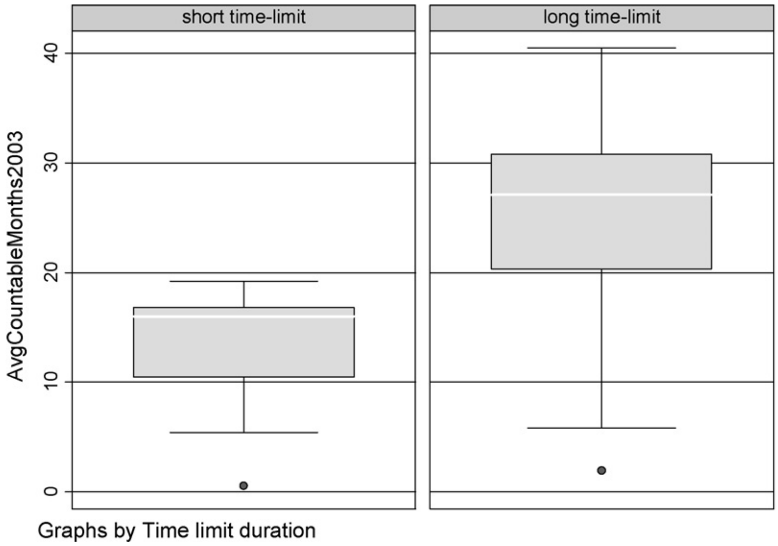


Fig. 2. Relationship between TANF time limits and the average monthly countable months by State in 2003. *Notes*: The *y*-axis is the average monthly number of countable months in 2003 for TANF recipients. Sample of states is weighted by number of births in 1988. Short time limit states limit TANF recipients to 48 months or less.

Of course, counts of time limit exits do not account for the indirect ways that time limits can affect welfare recipients and state welfare agencies. For example, time limits may dissuade families from participating in TANF in the first-place. Families (or case workers) may be forward-looking and therefore seek to conserve eligibility by cycling in and out of TANF, or be searching for employment which in several states stops the counter. Mazzolari (2007) finds that these type of behavioral changes, banking welfare eligibility, account for a declining share of the caseload reduction between 1998 and 2003. The share falls below 20% by 2003 as more time limits become mechanically binding. States sensitive to mechanically pushing families off TANF by the clock can find other categories in which to classify exits, and other doors through which those approaching time limits may be seen to exit. No family reaches time limits in Oregon because – without exception – the state finds they fail the work-requirement before that point. These short comings of direct measures suggest that potential usefulness of an alternative approach.

An additional way to assess whether time limits are binding is to compare TANF durations in short and long time-limit states. Such an approach is attractive because the measure incorporates anticipatory exits and rationing (whereby families seek at conserve eligibility months by cycling in and out of welfare), as well as involuntary exits. Fig. 2 compares average countable months for the cross-section of spells of TANF recipients in short versus long time-limit states.[10] Consistent with the view that time limits did shorten TANF spells, states adopting shorter time limits have shorter average TANF durations in 2003. TANF spell length is more than 1 year shorter in short time limit than in long time-limit states, a difference that is significant. Less than 2.5% of all TANF families in short time-limit states had spells that exceed 4 years in 2003, compared to 12.5% of all TANF families in long time-limit states. Since these durations are measured at the same point in time, these differences cannot be accounted for by the national cycle.

Shifts in spell lengths reflect the enactment of various time limits in each state. These mark changes from the states' experiences under AFDC. The left hand graphs in Fig. 3 compare TANF countable months in 2003 across sets of states with time limits of 24 months (Panel A), 36 months (Panel B) and 48 months (panel C). In each case, durations in the 60 month states are presented for a baseline. The right-side graphs show for each set of states in 1995 a corresponding AFDC spell length: months since case first opened. Panel A shows that while, on average, the 1995 distributions of AFDC durations were virtually identical in states with 24 month limits and in states with 60 month limits, by 2003 states with the 24 month time limits exhibited a marked leftward shift in the distribution of durations relative to states with 60 month limits. As one would expect, the shift in the distribution of durations occurs before the 24-month duration threshold. Similarly, in 2003 states with 36 and 48-month time limits also exhibit leftward shifts in the distribution of durations occurring at the 36 and 48 month thresholds respectively, as seen in Panels B and C.[11] TANF time limits succeeded in shortening TANF durations. This pattern is consistent with Mazzolari's (2007) finding that hitting time limits accounts for a 4.7%age point decline in welfare participation, or about 30% of the overall decline. Together with a decline in inflow, this would lead to

[10] Countable months are the cumulative number of months families have received cash assistance from TANF that are deemed to count against the TANF time limit. Average TANF countable months are obtained from the Department of Health and Human Services.

[11] Comparing 60 month states in 1995 and in 2003 suggests that there has not been enough increased inflow during the recent downturn to increase the share of short duration spells.

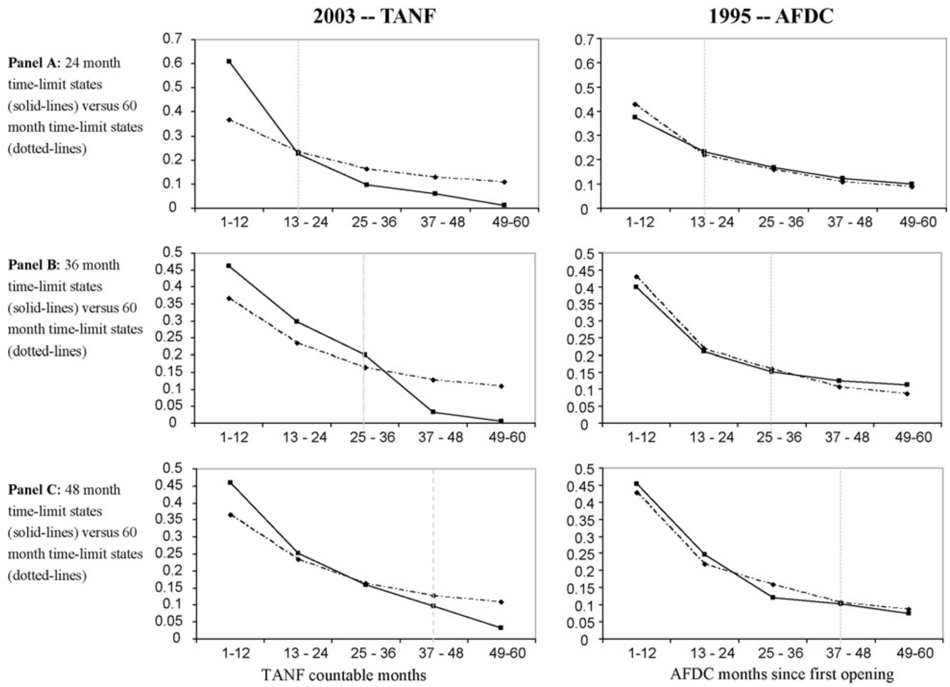


Fig. 3. Comparison of welfare duration distributions by time limit and welfare regime. AFDC months since first opening and TANF countable months. In each panel, the dotted-lines represent 60 month time-limit states. The solid lines represent 24 month time-limit states in Panel A; 36 month time-limit states in Panel B; and 48 month time-limit states in Panel C. *Notes*: Authors calculations based on aggregate administrative data of average TANF countable months in 2003 and of months since first opening AFDC case in 1995, as reported by DHHS. The *y*-axis is the proportion of families in each category. Distributions are the average caseload share in each duration category, weighting by the number of nonexempt families on welfare at the state. Oregon is excluded from the 24 month time limit group as no families reach this limit because—without exception-they fail the work-requirement before that point. Vertical line denotes the 24, 36 and 48 months limit in panels A–C respectively.

the decline in caseloads already observed. We now turn to the question of what effect the time constraint had on infant health.

4. Comparison of long and short time-limit states

We begin by assessing whether the time-path of infant health measures differs between states with long and short time limits. After this analysis, we will then impose more structure by estimating the effect of a state's time limit becoming binding on the infant mortality rate.

The first suggestive evidence linking TANF time limits to changes in infant mortality during the 2001–2003 period is presented in Fig. 4. This figure plots the percentage change in the infant mortality rate against average countable TANF months in 2003.[12] The longer the average TANF spell in process in 2003 in a state, the greater the decline in infant mortality between 2000 and 2003. But the relationship may not be causal; the average TANF duration in 2003 and the change in infant mortality between 2000 and 2003 could be jointly determined by other factors. For example, if infant mortality were pro-cyclical, states which experienced more precipitous downturns may have independently had both higher average number of countable months among their recipients and lower infant mortality.

It is more informative to consider the relationship between state time limits and the change in infant mortality over this period because time limits are not endogenous to the magnitude of the economic downturn. We use the time limits on TANF receipt, pre-set by each state in the late 1990s, to contrast the difference in infant mortality between states with short ($\leq$48 months) and long ($\geq$60 months) TANF time limits. Time limits can shorten spells by inducing eligibility savings behavior, by deterring entry, and by directly truncating spells. Focusing on TANF time limits allows us to isolate the impact on infant mortality of changes in TANF use that are not caused by contemporaneous economic or social changes, nor by omitted individual characteristics that might affect both spell length and infant mortality.[13]

[12] The infant mortality data are as reported by the CDC in its Vital Statistics Report.

[13] One could also estimate the relationship between changes in infant mortality and changes in state caseloads, instrumenting caseload with the state time limit policy. We do not take this approach because changes in caseloads predominantly are a result of voluntary exit from welfare, due in part to a strong economy in the 1990's. Because most of the variability in caseloads over time is accounted by the entry and voluntary exit of recipients, we do not have a strong enough first-stage to estimate a two-stage least squares model of this kind. Because we are interested in the effects of involuntary exits from

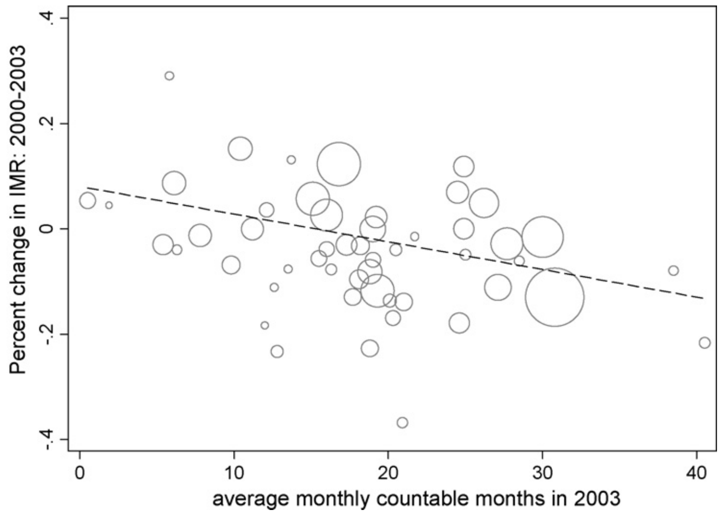


Fig. 4. Average monthly number of countable months and the change in the infant mortality rate by State between 2000 and 2003. Notes: *y*-axis is the percent change in the infant mortality rate by state from 2000 to 2003. *x*-Axis is the average monthly number of countable months in 2003 for TANF recipients. The points on the scatter plot are in proportion to the number of births in 1988. The estimated regression line has intercept 0.080 (S.E. = 0.036), slope −0.0052 (S.E. = 0.0016), and R^2 = 0.18.

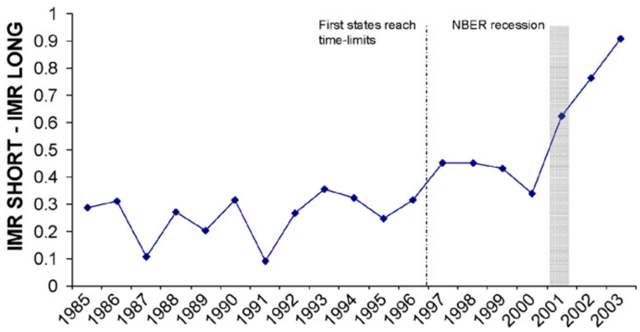


Fig. 5. Infant mortality rate of short-time limit states relative to the infant mortality rate of long time-limit states. *Notes*: Authors' calculation based on the National Vital Statistics Report published by the CDC. Sample weighted by the number of births in 1988.

The median year families first began exiting TANF in short time-limit states was 1998, while the median year families first began exiting TANF in long-time limit states was 2002. Between 1997 and 2002 a large number of families timed-out of TANF in short time-limit states, while timed-out exits in long time-limit states were limited during this interval. Indeed, we have shown that even by 2003, there were substantial differences in TANF durations between states with short time limits and those with long time limits, differences that were not present under AFDC. If time limits affect infant health, we expect to see the health of infants born to mothers likely affected by time limits changing in short time-limit states relative to long time-limit states after 1997.[14]

Fig. 5 shows the difference in the infant mortality rate of states with short time limits versus those with long time limits, weighting the sample by the number of births in the state in 1988. Inspection of the figure shows that the difference in infant mortality showed little trend from 1985 through 1996. Following the reform, there is a small increase in the infant mortality rate in states with more stringent limits, despite the on-going boom. Beginning in 2001, states with stringent time limits experienced large and persistent increases in infant mortality relative to states with long limits. As the figure suggests, short time-limit states do not fare worse than their long counterparts in every recession: there is little here to suggest that the earlier (1990–1991) recession exacerbated infant mortality. Short time-limit states are associated with a large increase in infant mortality (relative to long time-limit states) during the 2001–2003 downturn, but not during the previous 1990–1991

welfare on infant health, the ideal data would be a measure of state-level involuntary exits from welfare, which we would then instrument with state time limit policy, interacted with calendar year. Given the absence of reliable data on involuntary exits from welfare, our next best alternative is to estimate the reduced form model where we estimate the relationship of time limit policy on infant mortality.

[14] Making a similar distinction across States among those terminating benefits, Mazzolari (2007) finds that Short states decreased welfare use by 55% by 2003, almost all caused by hitting time limits. In contrast, she finds that welfare use in Long states declined by only 10%, all via eligibility banking.

Table 1
Sample characteristics; 1995–2002.

Panel A	(1) All states	(2) Short time-limit state	(3) Long time-limit state	Difference between short and long time-limit states
Number of states	51	20	31	
Number of births	28,497,855	12,867,084	15,630,771	
Infant mortality rate (×1000)	6.04	6.29	5.85	0.44 (0.45)
Unemployment rate	4.93	4.65	5.16	−0.51 (0.33)
Mother is non-Hispanic White (Yes = 1)	0.634	0.632	0.636	−0.005 (0.082)
Mother is non-Hispanic Black (Yes = 1)	0.160	0.179	0.143	0.036 (0.034)
Mother is Hispanic (Yes = 1)	0.206	0.189	0.220	−0.031 (0.099)
Mother does not have a HS degree (Yes = 1)	0.223	0.231	0.215	0.016 (0.036)
Mother is less than 25 years (Yes = 1)	0.381	0.410	0.358	0.051 (0.019)
Mother is older than 34 years (Yes = 1)	0.125	0.112	0.135	−0.023 (0.009)

Notes: Authors' calculation based on linked birth and death certificate micro-data. Sample consists of singleton births from White/non-Hispanic mothers, Black/non-Hispanic mothers, and Hispanic mothers. Short time-limit states are states with lifetime or intermittent time limits that are less than 60 months.

downturn of the AFDC era. Note also that the waiver period of 1992 to 1997 is not associated with an increased divergence in infant mortality between short and long time-limit states.[15]

5. Evidence from linked birth and death certificate records

We have shown that the infant mortality gap between long and short time-limit states was relatively stable through 2000, but then diverged. Was this divergence a result of time limits? Individual data are not available with the requisite information on both time limited exits and infant mortality needed for the most direct test, so to address this question we ask whether infant mortality increased among infants born to classes of mothers likely to have timed-out when time limits became binding. Using linked birth and death certificate micro-data from the Center for Disease Control, we compare the birth outcomes of women who are relatively likely to be on welfare and who have timed out, to those of women also likely to be on welfare but unlikely to have timed out, before and after time limits become binding. The most promising contrast is among low-educated unmarried mothers between those having their first child (who were unlikely to have timed out of TANF near the birth of their first born that establishes TANF eligibility) and those having at least their second child (who could have plausibly timed-out).

The linked birth and death certificate data contain a near universe of infant births and deaths. We begin our analysis in 1995, the first year for which uninterrupted data are available, and end the analysis in 2002, the last year of data available to us. We sampled all singleton births of White/non-Hispanic mothers, Black/non-Hispanic mothers, and Hispanic mothers for whom birth weight and gestation are recorded, resulting in 28,497,855 birth records under analysis. Summary statistics are reported in Table 1.

The micro-data allow us to focus on the subset of mothers for whom we believe time limits were binding, but because they are only available for the period 1995–2002 they do not allow us to fully account for possible pre-existing time trends. However, the analysis from the aggregate data in Fig. 5 suggests that pre-existing trends do not appear to be a major concern. Prior to welfare reform, infant mortality in short and long time-limit states do not appear to have different trends.

In Table 2, we estimate linear probability models of the probability that an infant dies within the first year of life.[16] The independent variable of interest, Post-binding, is an indicator that is 1 if the state's time limit was binding for the entire year, as reported by Bloom et al. (2002), and 0 otherwise. The key finding here is in Column (1) of Table 2 which shows that a binding time limit is associated with an increase in infant mortality of approximately 0.16 infant deaths per thousand births (t-ratio = 2.3), controlling for state and year fixed-effects. The estimated coefficient on the post-binding indicator is largely invariant to the addition of controls for the logarithm of the state unemployment rate and for the mother's age, education, race and ethnicity (Columns 2 and 3).

In column (4) we interact the post-binding indicator with an indicator for whether the birth occurred during the recession (post-2000). Almost the entire effect of binding time limits on infant mortality appears to occur during the 2001–2002 period, perhaps suggesting that expulsion from TANF is particularly harmful to infants during economic downturns. It is also possible that this positively estimated interaction is coming about because time limits affect mothers and/or infants with a lag. This could be the case because it takes time for affected mothers to have children, or because it takes time for the number of timed-out mothers to grow sufficiently to detect them in the data.

It is common to observe differences between blacks and whites in health and in the utilization of medical care. Approximately 27% of all black births took place under Medicaid in 2000, while the corresponding figure for whites was about 10%. In 2000, 10.9% of black mothers were TANF beneficiaries, while only 2.8% of white mothers were. If TANF cutoffs were driving

[15] In Fig. 1 of the Electronic Appendix we plot the same series after controlling for the log of the state unemployment level. The pattern observed in Fig. 5 is unaffected.

[16] The findings are robust to estimating probit models.

Table 2
Linear probability models of infant mortality; dependent variable is indicator for infant death, 1995–2002.

Explanatory variables	(1)	(2)	(3)	(4)	(5) White/ Non-Hispanic	(6) Black/ Non-Hispanic	(7) Hispanic
Post-binding	0.159 (0.069)	0.164 (0.065)	0.157 (0.069)	0.059 (0.057)	0.079 (0.071)	0.709 (0.170)	0.016 (0.117)
Post-binding × post-2000				0.205 (0.116)			
Year dummies?	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State dummies?	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Log unemployment?		Yes	Yes	Yes	Yes	Yes	Yes
Mother characteristics?			Yes	Yes	Yes	Yes	Yes
Mean of the dependent variable (×1000)	6.05	6.05	6.05	6.05	4.96	11.8	4.91
Observations	28,497,855	28,497,855	28,497,855	28,497,855	18,073,262	4,547,617	5,876,976
R^2	0.0002	0.0002	0.002	0.002	0.001	0.001	0.0004

Notes: Standard errors clustered by state in parentheses. Post-binding is 1 in the year after TANF recipients could have first have timed out, and 0 otherwise. See text for details. All estimates are multiplied by 1000. All models are estimated with a constant. Mother characteristics controls are age dummies (7), education dummies (5), and race/ethnicity dummies (4).

our results, we would expect to see a larger aggregate impact on blacks simply because a larger proportion of blacks are TANF beneficiaries, and so exposed to the risk of cut-off. Assuming that cutoff effects across individuals did not depend on race, we would expect to see that the effect of TANF cutoffs on infant mortality should be approximately four times greater for blacks than for whites in the aggregated state data.[17,18]

We find effects almost nine times greater in magnitude for blacks than for whites. Column (6) of Table 2 presents this estimate for the sub-population of black mothers. As we would expect if the effect were driven by welfare reform, the effect is large, estimated at 0.71 (t-ratio = 4.2). For whites (column 5) the time limit effect is substantially smaller, estimated at 0.079, and insignificant.

Among Hispanics (column 7), adverse consequences of welfare reform on the infant mortality rate are negligible and insignificant.[19] Black infant mortality has been differentially affected, and more so than would have been expected from differences in TANF participation alone.[20] The fact that we observe no relationship between changes in Hispanic infant mortality rates after time limits bind also suggests that the post-binding effect is not the result of restriction of benefits paid to immigrants as a result of welfare reform.

6. Effects by education, marital status, and live-birth order

We now turn to an estimation strategy capable of distinguishing the effects of other policy, economic, or social changes from those of time limits. Typically, it is not possible for a woman to enroll in TANF unless she is already pregnant or a mother. It should be impossible for a mother to reach a 2–5 year time limit on TANF receipt during the first year of life of her first infant that establishes eligibility. We use this differential impact on higher-order births to help identify time limit effects and distinguish them from other possible causes. Table 3 tests the hypothesis that the post-binding effect is concentrated among mothers who already have had a live-birth, since it is precisely this group of women who could have timed-out.

We focus on subsets of the population more likely to be TANF beneficiaries. Table 3 first presents the estimated post-binding effect for low-educated – those with less than a high-school degree – and unmarried mothers who have previously had a live-birth.[21] We argue that post-binding the effect should only be present in this sub-sample of mothers. Consistent with our hypothesis, when focusing on this cut of the data, the infant mortality rate increased by 0.51 following binding time limits (t-ratio = 2.38), as seen in column (2).

As "falsification" tests, we also present estimates of the post-binding effect for three groups of mothers whom we do not believe should be affected by time limits: low-educated and unmarried mothers giving birth for the first time, married mothers, and highly educated mothers—those who have attended at least some college. We do not believe that time limits should

[17] There is some evidence that blacks were more likely to be cut-off from TANF for non-employment reasons. See, for example, www.clasp.org/publications/welfare_reform_and_racial.pdf. If blacks were more likely to become uninsured following TANF cutoff, this would aggravate racial differentials. Evidence is limited. For California, Aizer (2007) finds blacks less likely to enroll in Medicaid, conditional on being eligible. For Wisconsin, Cancian et al. (2001) find insignificant racial differences in Medicaid take-up among those leaving welfare.

[18] The black-white infant mortality rate gap declined consistently beginning in 1965 through 1997, when it flattened. Although our analysis is largely silent on the question, it would be interesting to explore whether PRWORA contributed to this flattening.

[19] This is another manifestation of the well-known finding that Hispanic infants tend to be more robust. [Abraido-Lanza et al., 2005]. What is notable here is that this "Hispanic paradox" is thought to be concentrated among first generation immigrant families- the same group targeted for exclusion from welfare under PRWORA.

[20] The cyclical racial differences in selection and behavior found by Dehejia and Lleras-Muney (2004) would have led to increased infant mortality among whites but not blacks. The patterns they note appear to be outweighed here by the differential TANF effects across race.

[21] When we include mothers with a high school degree or less with the less educated the results are qualitatively similar.

Table 3
Linear probability model of infant death; Estimated coefficient on the post-binding indicator by live-birth order, martial status, and education; linear probability models.

	(1) Mean of the dependent variable (×1000)	(2) Higher order-birth	(3) First live-birth	(4) *p*-Value on test of equality of (2) and (3)
No high-school degree				
Unmarried (*n* = 3,788,004)	9.24 [95.7]	0.502 (0.212)	−0.042 (0.281)	0.18
Married (*n* = 2,554,842)	6.25 [78.8]	−0.033 (0.207)	−0.273 (0.418)	0.66
Attended at least some college				
Unmarried (*n* = 1,950,037)	7.41 [85.7]	0.046 (0.351)	0.047 (0.354)	0.997
Married (*n* = 10,628,077)	3.70 [60.7]	0.009 (0.081)	0.095 (0.122)	0.50

Notes: Authors' calculation based on linked birth and death certificate micro-data on singleton births for years 1995–2002. See text for details on sample selection. All estimates are multiplied by 1000. Standard errors clustered by state are in parentheses. Each of the four rows corresponds to a single model estimated over the indicated sub-sample. The linear-probability model of infant mortality includes the following regressors: a constant, year dummies (7), age dummies (7), race/ethnicity of mother dummies (2), state dummies (50), the log state unemployment rate, a post-binding dummy, and the interaction of all of these variables with an indicator for whether it was the first live-birth for that mother. Column (2) is the estimated coefficient on the post-binding indicator. Column (3) is the sum of the estimated coefficient on the post-binding indicator with the estimated interaction of post-binding and first birth. Column (4) is the *p*-value on the interaction of post-binding and first birth.

affect married mothers because barriers to TANF enrollment are much higher for married parents than single parents.[22] In none of these cases did the infant mortality rate grow significantly more after time limits became binding, suggesting that our finding of excess infant mortality amongst the "affected" group is not driven by general trends.[23] In particular, we can reject equal effects of time limits on first compared to subsequent births. Overall, the estimates support our basic hypothesis: the increase in the infant mortality rate after time limits began binding occurred among women who were both at risk for being on welfare, and who could have plausibly timed-out.

7. Magnitudes

Using the estimate of the post-binding effect for higher-order births among unmarried mothers with less than a high-school degree, we estimate that there were approximately 465 excess deaths associated with binding TANF time limits.[24] This estimate represents a 6% increase in the mean infant mortality rate among unmarried mothers lacking a high-school degree, although clearly not all of these mothers transited TANF.

How sizable are these deaths in proportion to the number of mothers who did transit TANF? Because the Federal government does not release data on exits from TANF, or on time limited exits, and because infant birth and mortality data do not indicate welfare status, we must rely on estimates of the number of affected births. To help put this into perspective, consider our estimate of the number of families that were cut-off due to time limits—353,000. If these mothers gave birth to an average of 28,240 infants per year – corresponding to a fertility rate of 80 per thousand – the implied increase in the infant mortality rate amongst the affected group is 42%, assuming a baseline infant mortality rate of 15 deaths per 1000 births.[25,26] This is a large change in a relatively small group, but possible to overlook among millions of births. We do not consider this effect an entirely implausible outcome for a single uneducated mother with few resources, cut off from TANF, and now with a second infant. However, we believe that the true number of affected families is higher than 353,000, suggesting a smaller relative effect of time limits on infant mortality, although the precise number is unavailable. Because we are also unsure of the fertility rate for the affected group as well as the baseline infant mortality rate, we present the implied percent increase in the infant mortality rate for several combinations of time limit affected group size, baseline infant mortality rate, and fertility rate in Table 4. The implied effects range from a 20% increase in the infant mortality rate (assuming that there were 450,000 families affected by time limits, the fertility rate is 90 births per 1000 women, and the baseline infant mortality rate is 22 per 1000 births) to 48% (assuming that there were 353,000 families affected

[22] Marital status was imputed in several large states during much of the sample period. States that imputed marital status are California (imputed until 1997), Connecticut and Nevada (imputed until 1998), and Michigan and New York (imputed to the present). Imputation will result in misclassification error, thus potentially attenuating any differences between the married and unmarried group of mothers.

[23] In Electronic Appendix Fig. 2 we plot the infant mortality rate by year, separately for short and long time-limit states. We stratify by marital status, birth-order, and education. The divergence in infant mortality between short and long time limit states, seen in Fig. 5, is only present among unmarried and low-educated having at least their second child.

[24] This estimate is the excess number deaths implied by the post-binding estimate in the sub-sample of higher-order births to low-educated and unmarried mothers in Table 3 (0.502). To compute the implied excess number of deaths we multiply this estimate (divided by 1000) by the number of births in this sub-population under binding time limits through 2002 (927,002).

[25] The age-specific fertility rate of 30–34 year old women in 2002 was 85.1.In 1998, the infant mortality rate for children born to black non-Hispanic mothers without a high-school degree was 15.27.

[26] To calculate this implied magnitude, we multiply the size of the affected group (353,000) by the fertility rate (0.8) and by the average number of post-binding years (2.6).The resulting quantity is then multiplied by the baseline infant mortality rate (15/1000) in order to obtain the number of infant deaths that would have occurred in the absence of binding time limits. The percent increase in the infant mortality rate is 465 (excess deaths) divided by the baseline number of deaths.

Table 4
Implied magnitude of effect for various combinations of time limit affected group size, baseline infant mortality rate, and fertility rate.

Size of the affected group	Baseline IMR	Fertility rate (%)		
		70	80	90
353,000	15	48	42	37
	22	33	29	26
400,000	15	43	37	33
	22	29	25	23
450,000	15	38	33	29
	22	26	23	20

Note: Fertility rate refers to the number of births per 1000 capita.

by time limits, the fertility rate is 70 births per 1000 women, and the baseline infant mortality rate is 15 deaths per 1000 births).[27]

We expect the increase in infant mortality we observe here to overstate the permanent change in a new steady state because states will have worked through the one time expulsion of an accumulation of long-term beneficiaries during the transition to TANF time limits, as in Ohio. In this regard, note Moffitt and Stevens (2001) findings that while at least before 2000 welfare reform itself had had little effect on the skill distribution of beneficiaries; long-term beneficiaries tend to be the most disadvantaged in terms of labor market skills. As time limits bind, they are likely to impact an accumulation of the particularly vulnerable.

8. Through what channels does the infant mortality effect operate?

Welfare reform of the 1990s explicitly sought to avoid the type of outcome we have documented here. While it may seem straightforward that a reduction in resources going to the poor should exacerbate their health outcomes, welfare reform was aimed at deeper cultural changes with arguably offsetting effects. Stronger families and increased employment were expected, after some transition, to end a culture of dependency and ameliorate any number of social pathologies.

8.1. Breaking the welfare-medicaid link

Before welfare reform, Medicaid benefits for the non-disabled were tightly and automatically linked to AFDC. Legislation broke this link, allowing the poor to qualify for Medicaid whether or not they received TANF benefits. Beginning in 1984, Medicaid coverage was extended to pregnant women and children under 6 with family incomes less than 133% of the federal poverty level. Many states have raised the income level to increase eligibility further. Currie and Gruber (1996a,b) report that this extension of Medicaid benefits to pregnant women reduced the infant mortality rate by 8.5%. If those leaving TANF took up the Medicaid benefits to which they were eligible, TANF time limits themselves would have no impact on Medicaid eligibility or utilization, perhaps avoiding adverse consequences for infant health.[28] But Medicaid take-up rates are surprisingly low, particularly for those leaving TANF. Currie and Grogger (2003) find that even women already on welfare appear to be easily deterred from applying for Medicaid benefits for which they are eligible. Those expelled from TANF are also less likely to utilize Medicaid benefits for which they are eligible. In chapter 13 of its Sixth Annual Report to Congress, the DHHS Office of Family Assistance reported that:

> Welfare status was closely associated with access to health care. Welfare leavers, especially those who were not working, had significantly greater problems with health care access than current recipients. The most important reason for loss of non-welfare benefits, including food stamps and Medicaid, was failure to appear for TANF eligibility re-determination.[29]

Hospitals accepting any Medicare funds must by law serve women in labor irrespective of their finances, so hospitals themselves have a direct interest in obtaining Medicaid benefits for those delivering. However, Ellwood and Kenney (1995) show that the take-up for Medicaid-paid prenatal care lags behind take-ups rates for Medicaid paid births. Medicaid eligibility does not in itself guarantee the application for, or the provision or availability of medical services[30].

[27] These figures may also be understated because our estimate of excess mortality excludes mothers with a high school degree or additional education who timed-out of TANF.

[28] About 40% of those receiving TANF benefits for two or more years report themselves in very poor health, as do half of those leaving TANF without a job [Loprest and Zedlowski, 2002].

[29] US Department of Health and Human Services, Office of Family Assistance. Memorandum No. TANF-ACF-IM2005-01. http://www.acf.dhhs.gov/programs/ofa/annualreport6/chapter13/chap13.htm. January 24, 2005.

[30] Incentives to providers affect Medicaid utilization in more nuanced ways. Currie et al. (1995) find that increases in Medicaid fees paid to obstetricians and gynecologists led to significant declines in infant mortality.

Table 5
Estimated coefficient on the post-binding indicator by live-birth order, martial status, and education; dependent variable is indicator for whether prenatal care was obtained in the first trimester.

	(1) Mean of the dependent Variable (×1000)	(2) Higher order-birth	(3) First live-birth	(4) p-value on test of equality of (2) and (3)
No high-school degree				
Unmarried (n = 3,679,913)	649.3 [477.2]	−19.4 (10.4)	−1.74 (6.71)	0.00
Married (n = 2,499,970)	728.2 [444.9]	−21.4 (10.7)	−16.6 (9.00)	0.26
Attended at least some college				
Unmarried (n = 2,104,565)	784.1 [411.4]	3.59 (4.52)	4.62 (3.81)	0.73
Married (n = 10,641,001)	932.8 [250.4]	−1.38 (2.27)	−1.64 (2.33)	0.69

Notes: Authors' calculation based on linked birth and death certificate micro-data on singleton births for years 1995–2002. All estimates are multiplied by 1000. Standard errors clustered by state in parentheses. Dependent variable is an indicator for whether prenatal care was obtained in the first trimester. See notes to Table 3 for estimation details.

Thirty-eight percent of poor children had no health insurance in 2002 (Currie, 2006a). Kaestner and Kaushal (2004) find that the 42% decrease in the welfare caseload was associated with a 7–9% decrease in Medicaid coverage; an increase in employer-sponsored, private insurance coverage of 6%; and a 2–9% increase in the proportion uninsured. The key question is whether time limits have induced a reduction in health-care access among those forced off TANF. Garrett and Hudman (2002) report that 41% of women who had left TANF between 1997 and 1999 were uninsured in 1999.[31] Twenty percent had employer-sponsored health insurance, and only 39% were enrolled in Medicaid. Currie, 2006a (p. 49) reports that "... census estimates suggested that as many as 1 million children lost Medicaid benefits as a result of their parents leaving welfare rolls", and – citing evidence from Wisconsin – notes that Medicaid enrollments dropped by 40–50% among those forced off of welfare. Welfare reform has increased the exit rate from welfare, which is associated with the loss of health insurance. The uninsured are less likely to obtain health care (Currie and Gruber, 1996a,b). Despite being delinked de jure, Medicaid benefits are linked de facto to TANF beneficiary status, and this may result in reduced pre-natal and infant health care.

8.2. Access to prenatal care

To shed more light on possible mechanisms, we turn to an analysis of the use of prenatal care. If there were fewer mothers on Medicaid because of the failure of delinking, we would expect that on average mothers would delay their first prenatal visits in short time-limit states. As Table 5 shows, among low-educated and unmarried mothers having at least their second child, the probability that prenatal care is accessed in the first trimester is reduced after time limits bind.

The falsification tests show a mixed picture. Consistent with the hypothesis that time limits lead to delayed prenatal care, there was no change in the timing of prenatal care following binding time limits among unmarried and low-educated mothers who had their first live-birth. Moreover, the timing of prenatal care did not shift among more educated mothers. We do see evidence of a delay in prenatal care in low-educated and married mothers following binding time limits, a sub-population we would not have expected to be affected. However, unlike unmarried mothers, the reduction in this population is occurring for mothers having both higher-order and first-births.

Overall the evidence, while certainly not definitive, suggest that reduced prenatal care is possibly one mechanism that could be influencing higher infant mortality for infants born to at risk mothers.[32]

8.3. Policy implications of an income gradient

Welfare reform has been haunted by the question of how those without benefits would endure a recession. The increased labor demand, rising incomes, and rising state and federal revenues during a boom can and did buffer many adverse consequences of welfare reform, in particular because unemployment is especially sensitive to the business cycle among the low-skilled (Hoynes, 2000). If the health effects we observe are due in part to economic deprivation, then the effects of time limits on infant health may have been less pronounced if they had coincided with better economic conditions. If that is the case, then we believe that there are several policy prescriptions to consider. TANF funding is frozen in nominal terms under the block grant program, and declining in real terms with inflation. During an economic downturn, the number of needy increases while funding remains fixed. As state tax revenues fall, the states tend to restrict welfare programs, eligibility, and

31 Aizer and Currie (2004), Bertrand et al., 2000; Blank and Ruggles (1996) and Currie (2006b) present interesting results on the role of information, time-inconsistent preferences, and high enrollment costs in determining welfare enrollment.

32 We have also considered gestation age and birth weight as outcomes. We find some evidence of reduced birth weight among newborns of at risk mothers. While this effect is more pronounced for higher-order births than first-births, the difference is not statistically significant. We find no evidence that gestation age changed when time limits became binding. We have also examined the timing of infant mortality. While there is some imprecision, much of the increase in infant mortality under time limits came from post-neonatal mortality.

funding. Even after accounting for reserves put aside during the boom, recessions mean less support for more people. As seen in Table 2, the deterioration in infant health under binding time limits occurred primarily during the recession. This may be because a recession is a particularly bad time to lose welfare benefits. In that case, infant mortality might be reduced by extending TANF benefits during recessions, a counter-cyclical policy of the type long implemented in the federal-state unemployment insurance system.[33]

9. Alternative explanations

The large estimated time limit effect we estimate naturally leads us to wonder whether it is too large to be solely attributable to the direct effects of time limits on infant health. This section considers various alternative explanations for the patterns in the data in terms of maternal selection, recessions, and state differences.

9.1. Selection of mothers

Welfare reform may have changed fertility patterns, perhaps leading to a higher share of at risk mothers after time limits began to bind. Of course, a policy change that increases infant mortality by changing who gets pregnant rather than by changing the outcomes of pregnancy among a given set is still not yielding a good outcome. Welfare reform explicitly sought to reduce teenage and out-of-wedlock pregnancies. To the extent this may have reduced the number of pregnancies among higher-risk mothers, we would expect to see a reduction in infant mortality. Likewise, an increase in abortion among higher-risk pregnancies would also be expected to reduce infant mortality.[34] However, there is little evidence that welfare reform has had an effect on fertility or on the abortion rate (see Blank, 2002, and Moffitt, 2003a,b for reviews), though it is worth noting that studies on this question use data prior to when time limits themselves may have affected fertility.

While theoretically we would not expect time limits to lead to a shift towards mothers more at risk of experiencing infant mortality, we explored the possibility by conducting a simple share-shift analysis. Using education, age, marital status, race, smoking, and ethnicity as predictors of infant mortality risk, we found no evidence that time limits changed fertility towards more at risk mothers (Electronic Appendix, Table 2). At least along these observable dimensions, the evidence suggests that the infant mortality effect is coming about from changed behavior, not changed composition in the population of mothers.

9.2. The effect of recession

During the 2001 recession the unemployment rate increased somewhat more in short time limit than in long time-limit states. For the recession to drive the increase in infant mortality we observe in short time-limit states, it would have to be peculiar in two respects: it would have to affect higher-order births, rather than first births, and it would have to have an effect opposite to that observed in the literature.[35] There is some evidence that infant mortality falls during recessions, most likely due to selection of mothers (Dehejia and Lleras-Muney, 2004).[36] As have others before us, we find that infant mortality tends to fall significantly in a recession, making it unlikely that some unmeasured deepening of the recession in short time-limit states accounts for our result.

While we have seen that mothers' risk class did not change because of time limits, we do find significant evidence that during the recession mothers were more likely to come from lower risk classes. Dehejia and Lleras-Muney (2004) report that blacks tend to shift fertility pro-cyclically, while whites shift counter-cyclically.[37] They also report some evidence of cyclical selection into motherhood: less-educated single black mothers are less likely to have babies during recessions, raising the average health of black babies, but less-educated white mothers are more likely to have babies during recessions. Both their selection and their behavioral results indicate an increased risk of infant mortality during recessions for whites but a decreased risk among blacks. Against this background, our estimates of increased infant mortality in post-binding states during the post-2000 recession are more striking.

9.3. State differences

If TANF limits are correlated with any persistent state difference that affects infant mortality, we should see persistent differences between the infant mortality rate in short and longer time-limit states. Fig. 5 shows that the infant mortality

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33 It is possible that there is no interaction effect with the recession, but rather, we are observing a lagged response in infant health outcomes following the timing out of mothers for other reasons.

34 See Gruber et al. (1999) and Ananat et al. (2006) for estimates of the effects of abortion in the 1970s.

35 We know of no studies that link the business cycle to infant mortality by birth-order.

36 Over a longer time period, Joyce, and Joyce and Mocan find that infant mortality is not sensitive to the business cycle. During the most severe post-war recession, Chay and Greenstone (2003) find evidence that infant mortality fell as pollution decreased.

37 They also find that utilization of pre-natal care increases and neonatal and post-neonatal mortality decline during recessions.

rate is slightly higher in short time-limit states prior to 2001, but the difference is not significant. The figure also shows that infant mortality in short and long time-limit states were not trending differently before the year 1997.

An alternative explanation for the relationship between binding time limits and infant mortality is that time limits are correlated with some other policy that produced the pattern in infant mortality time that we have seen. The differential increase in infant morality among second and higher order births is a hallmark of time limits, and helps to distinguish it from many alternative explanations. In general, alternative channels that we have not considered would have to match the specific patterns observed in the data. They would have to affect short time-limit states disproportionately, and within short states, they would have to disproportionately affect higher-order births to poorly educated mothers. This restricts the set of alternative reasons that might account for the observed patterns in infant mortality.

Family caps are of particular interest because they do share with time limits the distinction of predicting a differential impact on higher-order births occurring under TANF. However, only 4.1% of all TANF families in the states with family caps were estimated to have hit this cap (Administration for Children and Families, May 2002, Table 10:14). As shown in Electronic Appendix, Table 3, which adds controls for a variety of state policies to the model in Table 3, Panel B, and column (2), the increase in the infant mortality rate after time limits bind is still present after accounting for the interaction of a set of year dummies with an indicator for whether the state implemented family caps.

The Earned Income Tax Credit (EITC) improved the income of poor working mothers. The federal expansion of EITC was nationwide in scope, and so by itself could not explain differential changes in infant health between short and long time-limit states. The basic results are essentially unchanged by the inclusion of State EITC policy variables interacted by year (Electronic Appendix, Table 3).

PRWORA requires that at least half of the TANF caseload participate in work-related activities at least 30 h per week—20 h if the child is under 6. While work requirements, both as written and as practiced, differ across states, formal sanction policies are not highly correlated with time limits.[38,39] Controlling for the interaction of sanction policies and year dummies does attenuate the post-binding effect, by about 30%, resulting in it falling below the threshold of significance (Electronic Appendix, Table 3). However, the interaction of binding time limits with the recession indicator remains large and highly significant.[40,41]

10. Conclusion

Welfare reform in the late 1990s placed a 5-year federal lifetime time limit on the receipt of welfare benefits under TANF, and allowed the states to set shorter limits. How do those who would have otherwise continued on TANF fare after reaching their time limit? PRWORA was enacted at a most favorable time, the beginning of one of the strongest booms in decades. As unemployment surged between 2000 and 2003, the number of families in poverty increased by 1.2 million. Meanwhile, the TANF caseload *declined* by 232,666. As the economic tide receded, a full consideration of the effects of welfare reform could begin. We present evidence that limiting the time mothers can spend on welfare has increased the rate of infant mortality. However, because the bulk of time limit exits occurred at the time of the recession, we are not able to say whether the increase in infant mortality was due to time limit exits alone, or the interaction of these exits with the recession. With the passage of more time one could seek to unravel whether the effects documented in this paper are the result of time limits alone, or their interaction with the business cycle. Specifically, if the difference in infant mortality between states with short and long time limits is counter-cyclical, that would suggest that economic conditions interact with time limits resulting in elevated infant mortality risk.

Before coming to an unexpected reversal in 2002, the U.S. infant mortality rate had declined each year since 1958, continuing an even longer trend. At its nadir, the infant mortality rate fell to 6.8 deaths per 1000 births, still high compared to other developed countries. The troubling reversal after 2001 remains little remarked upon and largely unexplained. We offer evidence that the increase in infant mortality during the 2001–2003 economic downturn is in small but significant part a product of welfare reform. However, welfare reform is far from the only factor at work. Between 2000 and 2002 the infant mortality rate among singletons for mothers giving their first live-birth increased by almost 2%, an increase we would not attribute to time limits. While time limits certainly do not account for all of the flattening in the infant mortality rate since 2000, the evidence suggests that they are responsible for part of this increase.

[38] We note that *de facto* sanction policy may differ from *de jure* sanction policy. We were unable to find reliable state-level information on exits from TANF due to sanctions in order to fully explore this possibility.

[39] Seven states, including both with long and short time limits, maintain children's benefits when cutting off adults. Mazzolari (2007) finds no decline in welfare participation in these states. This may help account for part of the better infant mortality record in some time limit states such as California and New York.

[40] As already mentioned, it is possible that our tests have more power after 2001 as there will be more mothers who have timed out of TANF by that time. Conversely, surges of the type we saw in Ohio suggest that studies of periods after the backlog of pre-reform beneficiaries have been worked through may have limited statistical power to discern effects of the type estimated here.

[41] Inclusion of the state's benefit-to-wage ratio in 1996 interacted by year dummies results in a similar pattern. The post-binding effect attenuates by about 45%, though the coefficient on the interaction of time limits with the recession indicator remains large and significant.

Appendix A. Supplementary data

Supplementary data associated with this article can be found, in the online version, at doi:10.1016/j.jhealeco.2008.05.013.

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