What Accounts for Recent Declines in Welfare Caseloads in the U.S.? The Role of Time Limits

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Abstract

U.S. welfare caseloads declined dramatically after the 1996 welfare reform, and have not risen despite the recession of 2001-03. About two-thirds of the drop in welfare participation through 2003 is not explained using standard methods. In this paper I examine to what extent time limits - the provision restricting individuals to at most 5 years of federal benefits - can explain the observed decline. This provision reduces caseloads directly by cutting off benefits after a time-limited usage post reform, but also discourages welfare participation prior to reaching that limit in order to preserve the option of using benefits in the future. The incentive to "bank" benefits for the future is higher the lower the remaining coverage. Estimating the propensity to bank is complicated by the endogeneity of remaining coverage to the propensity to stay on welfare. Fortunately, the interaction of state-specific eligibility rules with individual characteristics is a very strong predictor of coverage in a first stage regression. When those interactions are used to define instrumental variables, time limits are found to explain more than half of the residual drop in welfare participation not explained by business cycle effects and other policy changes.

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1 Introduction

The last decade has witnessed significant changes in U.S. welfare policy. Aid to Families with Dependent Children (AFDC), the main welfare program providing cash-assistance to low-income single-parent families, was replaced by Temporary Assistance for Needy Families (TANF) as a result of the Personal Responsibility and Work Opportunity Reconciliation Act of 1996 (PRWORA).[1] A substantial departure from previous policy is the introduction of lifetime limits on the number of years of benefit receipt which can be paid out of federal funds. Among other changes, the reform also strengthened requirements for mandatory participation in work-related activities and imposed sanctions, in the form of financial penalties for non-compliance with program requirements.[2] Under the new TANF program, welfare participation among single mothers has dropped dramatically, from 23 percent in 1996 to 8 percent in 2003.

The dramatic decline in welfare participation has received much attention in the economic literature,[3] which agrees on the importance of three major explanations: welfare reform; the strong economy of 1996-2000; and changes in other policies affecting work-related incentives, mainly the significant expansion of the Earned Income Tax Credit (EITC) after 1993.[4] There are however some open issues. First, most empirical studies attribute a sizeable portion of the welfare drop to "unexplained time effects." Second, traditional empirical models cannot explain the recent puzzling observation that, despite substantial increases in the unemployment rate since 2000, welfare participation has not increased, but has actually slightly declined at the national level from 10 percent in 2000 to 8 percent in 2003.[5] Finally, there is

[1] The exact timing and intensity of the change from one program to the other vary by state because of two factors. First, some states gradually changed their welfare systems beginning in the early 1990s, because they were granted waivers from the AFDC rules. Second, the 1996 legislation devolved major program design elements to the individual states, which implemented their specific TANF programs in a 17-month period, between September 1996 and January 1998.

[2] Moffitt (2003) provides a detailed summary of the changes from AFDC to TANF.

[3] For a comprehensive review of this literature, see Blank (2002).

[4] Another explanation for falling caseloads that has received increasing attention in the literature is the shifting to other programs, mainly to Supplemental Security Income (Schmidt and Sevak (2004) and Duggan and Kearney (2004)).

[5] One way to address this puzzle has been to recognize that caseload stocks are the outcome of flows into and out of the rolls. Accounting for the link between stock and flows reveals inertia in welfare caseloads (Klerman and Haider (2002)), and this insight has been used by Grogger (2003) to explain the slow response of caseloads to the increasing unemployment rates in 2000-2002.

no consensus on what features of welfare reform have been most responsible for the decline in welfare participation and in other welfare-related outcomes. Persuasively disentangling the effects of detailed reform categories has posed a challenge because of the restricted timing of, and limited variation in, state policy choices.[6]

The goal of this paper is to go inside the "black box" of policy changes in order to investigate the explanatory power of one specific aspect of recent welfare reforms: the elimination of the entitlement status of welfare through the imposition of time limits on welfare receipt. Cash aid was an entitlement under the AFDC program, so that all poor, single-parent families with at least one child under age 18 could receive benefits as long as they satisfied the eligibility criteria. In contrast, as a result of welfare reform, federal law now limits families to 60 months of federal aid over their lifetimes. Many states have exercised their authority under PRWORA to set even shorter limits.[7] Several studies using only cross-state variation in the time of implementation of time limits estimate that they have had no significant effect on welfare use (Council of Economic Advisers (1997), Council of Economic Advisers (1999), Ziliak, Figlio, Davis, and Connolly (2000)). The shortcoming of these studies is that they constrain the effects of time limits to be independent of personal characteristics and they ignore the interaction of the time-limit rule with the growing stock of individuals facing binding time limits.

My approach to identifying the effects of time limits on welfare participation is to exploit the interaction of cross-state variation in the timing and form of time limit implementation with individual variation in characteristics that should predict different impacts of time limits. Time limits may have behavioral effects whose intensity depends on personal time-varying characteristics. If families are forward-looking, they may reduce their current welfare use even before reaching the limit in order to preserve their benefits as insurance against a negative event in the future (this will be called the "banking" effect of time limits). This paper contributes a direct test for the predictions of the dynamic model in Grogger and Michalopoulos (2003) on the relation between the "option value" of banking benefits for the future and a measure of individual remaining "coverage," which is defined as the ratio between the stock of remaining months of eligibility[8] and the

[6] For a review of the studies investigating the impact of specific welfare program components on different welfare-related outcomes, see Grogger, Karolyn, and Klerman (2002).

[7] See section 4.3.2 and Table A3 for details on the time limit policies implemented at state level.

[8] The stock of remaining months of eligibility is defined as the difference between the initial stock of available benefits in the state of residence and the number of periods of

eligibility horizon over which benefits may be used.[9] The predictions of the model give rise to a nonlinear schedule of incentives for the decision to participate in welfare, which is tested on micro data that incorporate detailed information on coverage.[10] Using a woman's actual welfare participation history to construct the remaining months of her time limit clock poses an identification issue, because of the potential endogeneity of current to prior participation. I instrument for the actual incentive measures using the same functional form, but isolating exogenous variation in individual coverage at each point in time arising from differences in the timing and nature of time limit policies, individual exposure to time limits and remaining periods of categorical eligibility.

This paper also investigates the mechanical effects of time limits, which should reduce welfare participation once recipients exhaust their benefits and lose eligibility. Given that states have multiple ways to allow needy individuals to continue to receive benefits beyond the state-wide time limit (primarily through extensions or exemptions),[11] an interesting empirical question is the degree to which time limits have been enforced. I investigate this question by estimating the effect on welfare participation of having exhausted benefits relative to the statewide most stringent time limit. Given the rules that regulate exemptions and extensions, also in this case it is necessary to deal with the endogeneity of current to prior participation.

In regressions that account for the potential endogeneity of an individual's past use, time limits are found to account for two-thirds of the residual drop in welfare participation between 1996 and 2003 not explained by business cycle effects, the welfare reform as a bundle and other policy changes. The estimation results also show that time limits influence welfare participa-

participation already used since the clock started to tick.

[9] Given that AFDC/TANF benefits are only available if minor children live in the home, the eligibility horizon over which the remaining benefits may be used is defined as the number of periods until a woman's youngest child turns 18.

[10] Grogger and Michalopoulos (2003) and Grogger (2001, 2002, 2004) test for (and find evidence consistent with) a more restrictive testable prediction: time limits should have the greatest behavioral effects among the families with the youngest children. This is a creative and clever way to identify the impact of time limits, but it is only valid if the remaining benefits are the same for everyone living in the same state, *i.e.* at the moment of time limit implementation, or in following periods for those with no welfare use post reform.

[11] See Bloom, Farrell, and Fink (2002) for a survey of how extension policies (that temporarily continue assistance even though a family has reached the time limit) and exemptions (which temporarily stop the time limit clock) differ from state to state, not only in terms of how the policies are designed, but also on how they are actually implemented by the welfare offices.

tion not only through their mechanical effect, but also through behavioral effects consistent with the "banking" hypothesis. Evidence that welfare-eligible individuals preserve their benefits for future periods is relevant to the literature on targeting efficiency and optimal program design. Time limits, when operating through their behavioral effect, can be thought of as a restriction on the intertemporal allocation of time and income, and Nichols and Zeckhauser (1982) show that targeting transfers through restrictions on recipients can be useful in forcing the especially needy to identify themselves.[12] Evidence of banking effects shows that time limits could succeed in making cash assistance a transitional support for people who are suffering temporary difficulties because of a job loss, a change in their family's circumstances, or some personal problem, while excluding people who are in periods in which they can sustain themselves through work.[13]

The remainder of the paper proceeds as follow. Section 2 describes the empirical puzzle and quantifies how much of the drop in welfare participation remains to be explained when using traditional approaches that only identify the effect of time limits using cross-state variation in their implementation. Section 3 presents the theoretical framework from which testable implications of the effects of time limits are derived. Section 4 presents the empirical setting and describes the individual level data drawn from the Survey of Income and Program Participation, and the state-level economic and policy variables. Section 5 reports the main results and some robustness checks. Finally, section 6 concludes and mentions possible extensions to this research.

2 Background: How much is left unexplained

The number of women receiving AFDC/TANF benefits has declined at record rates. After peaking in 1993, the prevalence of welfare participation among single mothers has dropped dramatically, from 23.2 percent in 1996 to 9.9 percent in 2000. It continued to decline in the early 2000s, reaching the low level of 7.9 percent in 2003, despite the increase in the unemployment rate from 4 percent in 2000 to 6 percent in 2003. The dra-

[12] They focus their analysis on programs that incorporate traditional restrictions, such as means-tested in-kind transfers, commodity-specific taxes and subsidies, and ordeals that impose a cost to qualify for a transfer (*e.g.* work requirements).

[13] This was exactly the goal that Ellwood (1988), who is recognized as the first to introduce the concept of time limits into the policy-making and research communities, had in mind when recommending to "convert welfare into a transitional support system whose duration might vary from eighteen months to three years."

matic and persistent decline in welfare participation since the middle of the 1990s is the subject of a now-extensive literature. Previous studies typically use data on state welfare caseloads or individual welfare participation from multiple years and regress them against controls for economic, policy and demographic factors (Council of Economic Advisers (1997), Council of Economic Advisers (1999), Ziliak, Figlio, Davis, and Connolly (2000), Schoeni and Blank (2000), Fang and Keane (2004), Grogger (2001)). Three primary factors have been estimated to explain caseload changes through 2000: the strong labor market, changes in the generosity of the EITC, and changes in welfare policy. Policy variables are typically represented as dummy variables that equal zero prior to the implementation of a specific policy (a waiver or a TANF program), and equal one in each year thereafter. Standard specifications generally include state fixed effects to remove long-term state-specific differences and year fixed effects to remove unobserved factors changing over time. Most studies that estimate this kind of specification attribute a sizeable portion of the change in welfare participation to unexplained time effects, as captured by year dummies (and state-specific time trends if included).[14]

This section uses data from the Survey of Income and Program Participation to quantify the drop in welfare participation attributed to unexplained time effects in a standard specification that measures policy effects only through cross-state variation in the date of implementation of the reform as a bundle. The share of the unexplained drop does not decrease substantially in a specification that captures the effects of time limits through the inclusion of a dummy for the date in which states started to count time limits. The estimated specification is a linear probability model, which relates the monthly welfare utilization indicator p_{its} for individual i in month t living in state s to socio-demographic characteristics (X_{it}), policy and economic factors (V_{ts}), state effects (μ_s), year effects (ν_y) and state-specific linear and quadratic trends ($\psi_s(t+t^2)$):[15]

$$p_{its} = \mathbf{X}_{it}\boldsymbol{\theta} + \mathbf{V}_{ts}\boldsymbol{\phi} + \mu_s + \nu_y + \psi_s(t+t^2) + \varepsilon_{its} \tag{1}$$

Summary statistics of the variables included in X_{it} and in V_{ts} are re-

[14] A notable exception is Fang and Keane (2004). Their specification successfully explains both levels and changes in the welfare participation rate without including state and year dummies. Also in their specification, though, there are variables that could be picking up time effects, such as months elapsed since the implementation of certain policies.

[15] The sample is restricted to single-mothers so that results are comparable to the ones presented in section 5. For a discussion of the data, I refer the reader to section 4.

ported in Table 1 and in Table 2. Average welfare participation across states in 1990 was 28.2 percent. Table A1 presents (and Figure 1 plots) changes in yearly average welfare participation rates across states (relative to the 1990 level) as predicted by the variables for time effects included in (1), *i.e.* year dummies and state-specific trends ($\nu_y + \psi_s(t + t^2)$). In a specification that only includes time effects (column (1) of Table A1 and the solid line in Figure 1), these are the actual changes in yearly average welfare participation rates across states. The decline in welfare participation since 1996 through 2003 is more than 15 percentage points. Columns (2) through (5) of Table A1 correspond to specifications that include more control variables. The estimated average time effects should become smaller in magnitude and statistically less significant the more complete the specification.

Column (2) of Table A1 corresponds to a specification that includes socio-demographic characteristics (X_{it})[16], dummies for the state of residence (μ_s), the (state monthly) unemployment rate (as a measure of business cycles) and the real AFDC/TANF maximum monthly benefit available for a family of three (as a measure of the generosity of the program). These controls are found to explain together 10 percent of the 1996-2003 drop.[17]

Column (3) adds controls for the generosity of the Earned Income Tax Credit, which provides a wage subsidy to low-income workers.[18] I use measures for the combined federal and state phase-in rate and the federal maximum credit (both vary by family size). Meyer and Rosenbaum (2001) show that the expansions of the EITC in the 1990s have substantially increased the employment of female family heads. EITC generosity adds sizable power to the explanation of participation drops in the late 1990s (as in Grogger (2001)), and the explanatory power seems to increase over time. Figure 1 shows that the unexplained time effects decrease in absolute value at an in-

[16] The socio-demographics I control for in this specification are the same as in section 5.

[17] Previous studies using state unemployment rates to proxy for changes in the economy find larger effects of the business cycle. Part of this difference may have to do with differences in sample periods. Council of Economic Advisers (1999) reports that falling unemployment accounts for 26 to 36 percent of the 1993-6 decline in welfare caseloads, but only 8 to 10 percent of the 1996-8 decline. My sample is extended through 2003, and the lower explanatory power of the business cycle may be due to a lower structural impact of the economy on caseloads in the early 2000s. Some studies find a bigger role of business cycles when including lagged values of the unemployment rate (Ziliak, Figlio, Davis, and Connolly (2000)) or when including a richer set of economic variables.

[18] Below a certain threshold, the subsidy is a fraction of the earned income as expressed by the "phase-in" rate. Once a worker's earnings exceed a threshold that depends on the phase-in rate and the maximum credit, the EITC effectively provides a lump-sum transfer. Once earnings exceed a further threshold, the credit is phased out until it reaches zero at the break-even level of earnings.

creasing rate. On the other hand, including measures for the EITC seems to exacerbate the unexplained rise in caseloads in 1993-1995, which means that the expansion in generosity of the EITC predicts that participation should have fallen in these years. When adding the effects of the EITC expansion to the specification, the 1996-2003 drop in welfare participation attributed to unexplained time effects is 11.2 percentage points, which means that so far we are able to explain 26 percent of the total 1996-2003 drop.

Column (4) of Table A1 adds two dummy variables for state-level welfare reforms. The first is a dummy for the implementation of waivers,[19] and the second is a dummy for whether the state TANF plan has been implemented. Except for the more generous financial work incentives, the new rules included first in state waivers and then in TANF programs (work requirements, time limits, "diversion" strategies and sanctions) are expected to decrease participation. Controlling for the implementation of welfare reform is found to add explanatory power to the model, as graphically shown by the fact that the time effects from this last specification are smaller in absolute value starting in 1996. This specification though still attributes almost two-thirds of the 1996-2003 drop in participation to unexplained time effects.

Some researchers have tried to code the adoption of specific program components rather than the adoption of a single policy change. For example, specifically regarding time limits, a set of studies have tried to identify the impact of time limits on state welfare caseloads, using as a policy measure a dummy to indicate if time limits were in place. Council of Economic Advisers (1997), Ziliak, Figlio, Davis, and Connolly (2000), Moffitt (1999) and Blank (2001) focus on the waiver period, while Council of Economic Advisers (1999) and MaCurdy, Mancuso, and OBrien-Strain (2002) extend the analysis to post-TANF data. All these studies report that time limits had little effect on caseloads, sometimes of the wrong sign (Blank (2001)). Only Council of Economic Advisers (1997) estimates that time limits have a negative statistically significant effect on caseloads.

In column (5) of Table A1, I add a dummy for time limit implementation. Given that time limit implementation (defined as the date since limits started to be counted) does not overlap completely either with waiver or with TANF dates,[20] this dummy identifies the separate effect of time limit

[19] In the first half of the 1990s, many states received waivers from the federal welfare requirements to test a variety of welfare reform strategies. These waivers varied substantially across states, and in many cases they differed greatly from the rules under AFDC. Many features later incorporated in PRWORA appeared first in state waivers.

[20] See section 4.3.2 and Table A3.

implementation from the collective effect of other reforms (as captured by the waiver and TANF dummies) under the assumption that the effects of time limits are the same across the population. The resulting estimate is similar to that found in previous studies that use state-level policy variation to estimate the effects of time limits: the coefficient is negative but statistically insignificant. As shown in Figure 1, Panel B, measuring the effects of time limits through a dummy for their implementation leaves us with almost the same amount of unexplained change in welfare participation as when not controlling for them.

In the remainder of the paper, I show how time limits are able to explain a relevant part of the otherwise unexplained drop in welfare participation, when we allow their effects to vary with individual characteristics.

3 The predicted effects of time limits

Moffitt (1983) and others have described an individual's decision to participate or not in AFDC as a single point-in-time comparison between the relative benefits and costs of participation. While the traditional static model was sufficient to analyze the consumer's behavior under the old entitlement regime, under time limits the consumer's problem is inherently dynamic, because current behavior affects future options and future utility. In this section I briefly present the dynamic model of welfare receipt in Grogger and Michalopoulos (2003) and derive the testable implications of time limits to be investigated in the empirical analysis.

In a dynamic framework, a forward-looking woman facing time limits is predicted to make welfare participation decisions by comparing the value of current period welfare benefits to the value of current period potential earnings plus the "option value" of conserving a month of benefit eligibility for the future, as an insurance to smooth consumption in case a negative event happens (a negative health event, a downturn in the economy that makes it difficult to find or keep a job, etc.). The main result of the Grogger-Michalopolous model is that this option value is an increasing function of the time horizon over which benefits may be used (*i.e.*, the number of years until the woman's youngest child turns 18) and a decreasing function of the stock of remaining months of eligibility. The model abstracts from a number of important considerations such as job search, welfare stigma and human capital formation, in order to focus on the effects of time limits.

3.1 The Grogger-Michalopolous model

Consider the optimization problem of a consumer living in state s at a time $t > \overline{T}_s$, where $\overline{T}_s$ is the period in which the state began counting months toward time limits. For simplicity, let's set the beginning of the optimization problem at $t = 1$ coinciding with the date of implementation of time limits. The utility maximization problem is limited to the eligibility horizon T, *i.e.* the number of periods until the consumer's youngest child turns 18, given that (under both AFDC and TANF) benefits are available only to families with minor children in the home. T is taken as given, but note the implications of relaxing this assumption below.

At the beginning of her period of eligibility, the consumer is endowed with an initial stock of benefits N_s, denominated in periods of benefit receipt and equal to the state time limit. The consumer's problem in each period t is to choose hours of work (h_t) and whether to utilize welfare ($P_t = 1$) or not ($P_t = 0$), so as to maximize the expected present value of lifetime utility, subject to time limits and her time and budget constraints. Each period the consumer has a total time available $\overline{L}$ to allocate between work and leisure (L_t) so that $h_t = \overline{L} - L_t$. If the consumer works in period t, she receives a gross wage of w_t, which depends on her personal characteristics (X_t) and on a stochastic *i.i.d.* element that becomes known at the beginning of time t. After she observes w_t, the consumer chooses how much to work and whether to utilize welfare or not. If she decides to participate in welfare, she receives a benefit G, but benefits are taxed away at a rate τ (the benefit reduction rate) as hours of work (and earnings) increase.

If the price of consumption C_t is normalized to one, and we assume that there is no non-labor income other than from welfare and that there is no borrowing or saving,[21] then the consumer faces the current period budget constraint $C_t = P_t[w_t(1-\tau)h_t + G] + (1 - P_t)w_t h_t$.

The consumer's current period utility function is given by $U(L_t, C_t)$. It is increasing and concave in both leisure (L_t) and consumption (C_t) and it satisfies the Inada condition $U_C \to -\infty$ when $C \to 0$. A formal statement of the consumer's problem is:

[21] Assuming no borrowing or saving is realistic for a low income population (Edin and Lein (1997)).

$$\max_{\{h_t, P_t\}_{t=1}^{T}} \quad \sum_{t=1}^{T} \rho^{t-1} E[U(\overline{L} - h_t, C_t)]$$

$s.t.$

$$C_t = P_t[w_t(1-\tau)h_t + G] + (1-P_t)w_t h_t$$

$$S_1 = N_s$$

$$S_t = N_s - \sum_{\tau=1}^{t-1} P_t \quad t = 2, ..., T$$

$$S_{T+1} \geq 0$$

where $\rho \in (0,1)$ is the discount factor. S_t is the state variable, giving the stock of benefits remaining at the beginning of period t, equal to the total stock of benefits minus how many periods the consumer has already participated since the implementation of time limits. Utilizing benefits at time t *depletes* the remaining stock by one unit.

The solution in Grogger and Michalopoulos (2003) is characterized in terms of the optimal value function $V_t(S_t) = \max_{\{h_t, P_t\}} [U(L_t, C_t) + \rho E V_{t+1}(S_{t+1})]$ (under time, budget and time limits constraints) and in terms of the indirect utility function $v(w_t, A_t)$, where A_t is non-labor income. A consumer optimally decides to participate in welfare if the current period utility gain that results from current period welfare utilization is bigger than the "option value" of preserving a month of welfare eligibility $\Delta_{t+1}(S_t)$, *i.e.* if:

$$v(w_t(1-\tau), G) - v(w_t, 0) \ \geq \rho[EV_{t+1}(S_t) \ - EV_{t+1}(S_t - 1) \equiv \Delta_{t+1}(S_t)$$

For low wage realizations, utilizing benefits raises current-period utility, so that $v(w_t(1-\tau), G) > v(w_t, 0)$. As long as the remaining stock of benefits is less than the remaining number of periods of the eligibility horizon ($S_t < T - t + 1$), utilizing benefits today reduces the expectation of maximized utility beginning in period $t+1$ ($EV_{t+1}(S_t) > EV_{t+1}(S_t - 1)$). That is, one is better off getting to time $t+1$ with more available months of eligibility remaining, because this increases one's scope to use welfare to smooth consumption in the following periods in the face of adverse wage shocks.

The simple stock depletion rule implies that in each period the decision on P_t entails a trade-off between a higher current-period utility (at least for low wage realizations) and the reduction of the stock of benefits available for the future. Optimal behavior in this dynamic model is to try to time the use of one's S_t periods of eligibility left to coincide with those periods when the realization of w_t is relatively low.

The main result of the existence of an incentive to preserve benefits for the future is based on the crucial assumptions that the future matters ($\rho > 0$, and high enough to avoid myopic behavior)[22] and that there is uncertainty about the occurrence of negative shocks.[23]

In contrast, we can relax the assumption that the eligibility horizon is fixed. Additional births increase the consumer's period of eligibility by increasing the number of years before the youngest child in the family turns 18. They have no effect on the family time limit, though. Thus additional births increase the family's reluctance to use welfare, all else equal.

3.2 Testable Implications

Some interesting special cases follow from the properties of the option value and from the specific time limit policies implemented at the state level. These different cases have empirical implications that can be tested with data on the welfare participation decision of families categorically eligible for benefits.

First, if at any time period t the remaining stock of benefits S_t (weakly) exceeds the remaining number of periods in the eligibility horizon $T - t + 1$ (from now on defined as T_t for simplicity), then the consumer's problem is effectively static. This result has the following empirical implication:

ZERO EFFECT HYPOTHESIS: when the clock is running ($t > \overline{T}_s$), *individuals with remaining stock of benefits exceeding their eligibility horizon* ($S_t \geqslant T_t$) *should behave as if time limits were not implemented.*

Second, as long as $S_t < T_t$, forward-looking individuals have an incentive to bank their benefits for periods of adverse wage shocks, as represented in

[22] Swann (2003) estimates a dynamic model for individual welfare participation and finds a high discount factor (ranging from 0.81 to 0.84).

[23] Another implicit assumption is that welfare eligible individuals are aware of the existence of time limits and of how many periods they have left. The state time limit policy is generally communicated to recipients when they enroll and in many states the number of months remaining on a recipient's clock is routinely announced with each benefits check (Bloom, Farrell, and Fink (2002)). Despite this, on data from the 2002 National Survey of America's Families, Zedlewski and Holland (2003) find that 16% of welfare recipients report that they were not told they had a time limit, and 21% report they were told but that they did not know how many more months they could receive benefits. The fact that welfare offices do contact families when the limit approaches (how and how long before vary by state) makes this last group likely to be made up of people still far from reaching their limit, so that we can assume large awareness of time limits, at least when they are approaching.

the model by the positive option-value of preserving benefits Δ_{t+1}. This negative incentive on current welfare participation is *decreasing* in S_t and *increasing* in T_t.[24] This gives rise to the following testable implication:

BANKING HYPOTHESIS: (1) *when time limits are implemented, individuals with a remaining stock of benefits less than the eligibility horizon* $(0 < S_t/T_t < 1)$ *should participate in welfare less than people not facing time limits. (2) This negative incentive is decreasing in* S_t *and increasing in* T_t.

When the time limit hits, then the individual should be removed from the welfare rolls if time limits are enforced. Grogger and Michalopoulos (2003) assume full enforcement of time limits and impose a terminal non-negativity condition on the stock variable $(S_{T+1} \geq 0)$.

There are factors though that can explain why some families who reach the time limit continue to receive TANF assistance, mainly extensions and exemptions.[25] Tabulations of the size of the caseload that has reached the time limit indicate that states are not dropping time-limited families from the rolls as frequently as their policies announced they would (Bloom, Farrell, and Fink (2002)). So, it is an interesting empirical question to investigate the extent to which time limits have been enforced at the state level, and this can be done by estimating the impact on welfare participation of having exhausted benefits. It is possible to easily extend the model in Grogger and Michalopoulos (2003) to allow for the realistic possibility that some families continue to receive (a portion of) the TANF benefit after reaching the limit. In particular, this possibility can be modeled as random for each individual, given the discretion of caseworkers in granting exemptions and extensions.[26] From this extension we can draw the following hypothesis to

[24] This comes from the fact that Δ_{t+1} is decreasing in S_t and increasing in T_t. For a proof of the second result, see Grogger and Michalopoulos (2003). The first result can be proved following the same steps.

[25] Most states allow for some extensions, usually because the family faces a particular hardship or because the parent was unable to find work despite "diligent efforts." Some states have exemptions that stop the clock for recipients that are incapacitated or are victims of domestic violence (Bloom, Farrell, and Fink (2002)).

[26] It's enough to define the total stock of benefits N_s as the time limit in state s, defined assuming that the consumer is not granted exemptions while receiving benefits. Only before reaching her limit is the consumer sure to receive full benefits G. When benefits have been exhausted $(S_t \leq 0)$, the consumer finds out *at the beginning of period* t if she is still granted assistance, and which portion γ of that. The budget costraint becomes $C_t = P_t[w_t(1-\tau)h_t + \gamma_t G] + (1-P_t)w_t h_t$ where:

$$\gamma_t = \mathbb{I}(S_t > 0) + \gamma \mathbb{I}(S_t \leq 0)$$
$$\gamma | X_t \sim i.i.d.\ [0, 1]$$

When S_t turns negative, the model is static, because the consumer's future options only be empirically tested:

ENFORCEMENT HYPOTHESIS: if time limits are enforced, then exhausting benefits relative to the statewide time limit ($S_t \leq 0$) should lower welfare participation. This means that exemptions, extensions, or intermittent time limits are not so widespread or generous to allow full continuation of welfare recipiency beyond the time limit.

3.3 Tests in the literature

The original contribution of this paper is to test for these hypotheses on detailed micro data that include information on welfare participation history. Using this information, I define the remaining stock of benefits S_{its} at each date t, for each woman i living in state s. In regressions that account for the potential endogeneity of an individual's past usage, I then estimate how welfare participation varies with this variable and with the individual eligibility horizon T_{it}, defined by the number of months until i's youngest child turns 18.

Grogger and Michalopoulos (2003) use data from a welfare reform demonstration in Florida and Grogger (2001, 2002, 2004) uses nationally representative data to test one of the implications of the model. That is, time limits, when first implemented, should have the greatest behavioral effects among the families with the youngest children, because, for given initial endowment of benefits, they face the longest eligibility horizon and the greatest incentive to bank benefits. The identification strategy in these papers is to use mothers with older children as a comparison group, and to attribute any incremental participation rate change among mothers with younger children to the effect of time limits. These studies find that time limits have statistically significant and economically sizable negative effects on welfare use, in contrast to previous work that constrained the effects of time limits to be independent of age. For example, using data from the Current Population Survey, Grogger (2004) estimates that behavioral responses to time limits account for 12 to 13 percent of the decline in welfare use during the late 1990s. A potential criticism of the difference-in-difference approach used in these studies is that it relies on the strong assumption that all other unmeasured time-varying factors affect behavior of mothers with old and young children in the same way.[27] Another shortcoming of this approach is that

depend on the discretion of the caseworker, and not on her current behavior.

[27] Recognizing this problem, Grogger (2004) interacts a number of important determinants of welfare use with the age of the youngest child to avoid attributing to time limits the effects of other factors that may vary with the age of the children.

it is theoretically valid only at the moment of time limit implementation. As time passes since the limit is introduced, this approach only captures the incentive to bank benefits perceived by people who have not used any of them. In contrast, the incentive to preserve months of eligibility among families whose stock of benefits has depleted because of usage depends not only on the age of the youngest child, but also on how many benefits they are left with.

Another study that has recently characterized the effects of time limits as the result of the same conceptual framework is Fang and Keane (2004). The empirical strategy in this study improves on the approach presented above because it allows the stock of remaining benefits to deplete over time. The authors estimate a reduced form specification for welfare utilization which includes variables intended to capture both behavioral and mechanical effects of time limits, such as the minimum stock of benefits that a woman *would* possess if she always received welfare since her clock started, and a dummy for whether a time limit *could* be binding (under the same assumption of continuous participation). Time limits are found to be one of the policy variables that contribute to the overall 23-percentage point decrease in the welfare participation rate from 1993 to 2002, but their estimated contribution (11%) is much smaller than the one of work requirements (57%) and the EITC (26%). The shortcoming of the approach used in this study is that a woman's stock of benefits is defined under the extreme scenario of continuous participation, so that the characterization of the incentives and the constraints faced by potential recipients worsens as time passes since the limit is introduced.

In the next section I show how, having data that include information on welfare participation history, I improve on the previous approaches in two ways. First, I estimate the effects of the remaining benefits on welfare utilization using an instrumental variables specification (instead of a reduced form specification) that use both differences in state time limits and in individual exposure to the new policy as sources of identifying variation. Second, I obtain relevant instruments by allowing the depletion of the stock of remaining benefits to depend on the likelihood of past usage, as predicted by some observable characteristics.

4 Empirical strategy

4.1 Modelling the welfare participation decision

In this section, I present my strategy to estimate heterogeneous effects of the time limit policy. Recall that the Grogger-Michalopolous model predicts a nonlinear schedule of incentives for the decision to participate in welfare that depends on the remaining benefits (S) and on the eligibility horizon (T). In a reduced form specification for the probability that individual i living in state s in month t receives welfare,[28] the predictions of the model (as presented in the previous section) can be tested by including a set of nonlinear functions of the ratio between remaining benefits and the eligibility horizon ($\frac{S_{its}}{T_{it}}$), only defined if time limits are implemented.

The empirical specification relates the monthly welfare utilization indicator p_{its} for individual i in month t living in state s to observed socio-demographic characteristics (X_{it}), a set of state-level policy and economic factors (V_{ts}), and a vector of functions $\mathbf{L}(\frac{S_{its}}{T_{it}}) = (L_1(\frac{S_{its}}{T_{it}}), .., L_4(\frac{S_{its}}{T_{it}}))$. The sample is restricted to single mothers because they are the primary target for cash assistance under the U.S. law,[29] and the specification takes the form:[30]

$$p_{its} = \mathbf{X}_{it}\boldsymbol{\theta} + \mathbf{V}_{ts}\boldsymbol{\phi} + \mathbf{L}\left(\frac{S_{its}}{T_{it}}\right)\boldsymbol{\pi} + \eta_i + \nu_{ts} + \varepsilon_{its} \tag{2}$$

The parameters of interest are the coefficients on the time limit variables

[28] The model predicts that the participation choice at t depends on the full distribution of wages. In a reduced-form, I assume that the probability distribution depends on current socio-demographic characteristics.

[29] Restricting the analysis to single mothers avoids a parameter heterogeneity problem (since childless women are ineligible for welfare, and tranfers to married couples account for a very low portion of the program because of stricter eligibility criteria), but may give rise to sample selection bias, if time limits, and welfare reform generally, alter marriage and childbearing incentives and change the composition of the population of single mothers. In their comprehensive review of the empirical literature on the effects of welfare reforms, Grogger, Karolyn, and Klerman (2002) conclude that despite "TANF and the welfare reforms under waivers in the pre-TANF period aimed specifically to change family formation—to increase marriage, to decrease separation or divorce, and to decrease nonmarital fertility".."the evidence from both the experimental and econometric studies is insufficient to draw any firm conclusions about the effects of welfare reform on marriage or fertility."

[30] The main results presented in section 5 are the ones derived from the estimation of a linear probability model with heteroskedasticity-robust standard errors (given that heteroskedasticity is an inherent property of the linear regression estimator when applied to a binary dependent variable). As a specification check, I repeat the analysis using probit models.

$\mathbf{L}(\frac{S_{its}}{T_{it}})$ only defined if the event $\mathbb{I}(t \geq \overline{T}_s)$ is true, and included to capture the heterogeneous effects of time limits:

$$\mathbf{L}(\frac{S_{its}}{T_{it}}) = \mathbb{I}(t \geq \overline{T}_s) \{ \mathbb{I}(0 < \frac{S_{its}}{T_{it}} < 1), \frac{S_{its}}{T_{it}} \mathbb{I}(0 < \frac{S_{its}}{T_{it}} < 1), \mathbb{I}(\frac{S_{its}}{T_{it}} \geq 1), \mathbb{I}(\frac{S_{its}}{T_{it}} \leq 0) \} \tag{3}$$

Figure 2 summarizes how people are categorized into different groups that correspond to different predicted effects of time limits. First, people can either live in states and months in which time limits are not implemented, or in states and months in which time limits are implemented ($t \geq \overline{T}_s$). If time limits are implemented, further heterogeneity is due to the relative magnitude of remaining months of eligibility versus the eligibility horizon. If remaining benefits are lower than the eligibility horizon, but positive (which means that the event $\mathbb{I}(0 < \frac{S_{its}}{T_{it}} < 1)$ is true), then the *banking effect hypothesis* predicts that the individual should be less willing to take up benefits to preserve them for the future, but that this incentive should be lower (so participation should be higher) the bigger the ratio $\frac{S_{its}}{T_{it}}$.[31] If remaining benefits are bigger than the eligibility horizon (equivalently, if the event $\mathbb{I}(\frac{S_{its}}{T_{it}} \geq 1)$ is true), the *zero effect hypothesis* predicts that an individual should behave in the same way as if time limits were not implemented. Finally, if an individual has exhausted her benefits relative to the most binding time limit in the state (corresponding to the event $\mathbb{I}(\frac{S_{its}}{T_{it}} \leq 0)$),[32] then, if time limits are enforced, this should predict a drop in participation (*enforcement hypothesis*). Explicitly formulating the specification in (2) in terms of the relevant variables that capture the effects of time limits yields:

$$p_{its} = other\ in\ (2) + \mathbb{I}(t > \overline{T}_s) \{ \alpha\, \mathbb{I}(0 < \frac{S_{its}}{T_{it}} < 1) + \beta \frac{S_{its}}{T_{it}} \mathbb{I}(0 < \frac{S_{its}}{T_{it}} < 1) + \gamma\, \mathbb{I}(\frac{S_{its}}{T_{it}} \geq 1) + \delta\, \mathbb{I}(\frac{S_{its}}{T_{it}} \leq 0) \} \tag{4}$$

The baseline group is given by people living in states and months in which time limits are not implemented. The hypotheses presented in section 3 can be formulated in terms of the coefficients in (4) in the following way:

[31] The model predicts that the option value to bank benefits is decreasing in S_{its} and increasing in T_{it}, so that a reasonable approximation in the empirical specification is to test if it is decreasing in the ratio $\frac{S_{its}}{T_{it}}$.

[32] The event $\mathbb{I}(\frac{S_{its}}{T_{it}} \leq 0)$ is equivalent to the event $\mathbb{I}(S_{its} \leq 0)$ given that we restrict the sample to the population of single-mothers with minor children, so that $T_{it} > 0$.

Box 1: Predictions of the Grogger-Michalopolous Model

Hypothesis	Event	Expected Coefficient
Banking effect	$0 < \frac{S_{its}}{T_{it}} < 1$	
intercept		$\alpha \leq 0$
slope		$\beta \geq 0$
Zero Effect	$\frac{S_{its}}{T_{it}} \geq 1$	$\gamma \simeq 0$
Enforcement	$\frac{S_{its}}{T_{it}} \leq 0$	$\delta < 0$

As regards the banking effect hypothesis, there is a prediction to be tested on the flat effect of facing an incomplete coverage ($S_{its} < T_{it}$) under time limits ($\alpha \leq 0$). Given this flat negative effect, a marginal increase in the ratio between remaining benefits and eligibility horizon should make the incentive to preserve benefits weaker and participation more likely to happen ($\beta \geq 0$). The predictions of the model on the banking effect of time limits are summarized in Figure 3A.

The strategy for identifying the effects of time limits is based on three sources of variation:

(1) *state variation*: different states implemented different time limit policies. Each state s has a specific total stock of benefits available denominated in months of benefit receipt (N_s), and started to count months toward time limits at a specific date ($\overline{T}_s$).[33]

(2) *time variation*: first, individuals may live in a period when time limits are not implemented ($t < \overline{T}_s$) or in a period when they are implemented ($t \geq \overline{T}_s$); second, if time limits are implemented, there is variation in the length of the exposure to time limits, defined by the number of periods elapsed since the implementation of the policy ($t - \overline{T}_s$).

(3) *individual variation*: different individuals have different eligibility horizons (T_{it}) and remaining eligibility (S_{its}). The eligibility horizon is the horizon over which the remaining periods of eligibility may be used, and it

[33] I use cross-state program differences to identify policy effects, and I take individual location as exogenous. Meyer (2000) shows that welfare induced migration is modest in magnitude when considering the incentive to move given by interstate benefit differentials. Cross-state differences in time limit policies should not add a new strong source of welfare induced migration given that, despite differences in state time limits (see Table A3), there is a 60-month lifetime limit on federal aid. Only two states have no time limit on benefits (Michigan and Vermont) and one state (New York) allows those who reach the limit to transition to a state funded safety net program.

is defined as the number of months until the woman's youngest child turns 18, given that AFDC/TANF benefits are available only to families with a minor in the home. If A_{it}^{y} denotes the age in months of the youngest child of woman i observed at time t, then:

$$T_{it} = (12 * 18) - A_{it}^{y} \tag{5}$$

Remaining periods of eligibility depend on the total stock of benefits made available in the state of residence and on welfare participation since the implementation of time limits, and are defined as:

$$S_{its} = N_s - \sum_{\tau=\overline{T}_s}^{t-1} p_{i\tau s} \tag{6}$$

Let E_{its} be the maximum number of months a woman i could have received benefits since her state started her clock. Then, welfare participation history since the introduction of time limits (the second term of (6)) can be written as the fraction of E_{its} the woman actually spent on welfare (k_i), so that:

$$S_{its} = N_s - k_i \; * \; E_{its} \tag{7}$$

Given that AFDC/TANF benefits are only available if minor children live in the home, then E_{its} is equal to the minimum between the number of months elapsed since the implementation of time limits ($t-\overline{T}_s$) and the age in months of a woman's oldest child (A_{it}^{o}):

$$E_{its} = \min\{(t - \overline{T}_s), A_{it}^{o}\}$$

E_{its} is a measure of the "exposure" to the risk of participating in welfare and it identifies an exogenous source of variation in the remaining stock of benefits S_{its}. On the contrary, the fraction k_i of E_{its} that individual i actually spent on the rolls is potentially endogenous to current welfare participation. The next section deals with the endogeneity issues in the specification in (2).

4.2 Endogeneity issues and solutions

The key issues for the estimation of (2) involve the composite disturbance term, which consists of three independent components: an individual component (η_i), a state-month component (ν_{ts}) and an idiosyncratic component (ε_{its}).

4.2.1 Endogeneity of the remaining stock of benefits

There is an immediate concern with including the actual remaining stock of benefits S_{its} in a model that explains current welfare participation p_{its}, because S_{its} depends on the individual welfare participation in previous periods, which is likely to be correlated with some unobservable factors that also explain current welfare participation and fall into η_i. For example, a woman with a strong preference for welfare, or with some unobserved barriers to work, is more likely to have received benefits in the past and to have a low S_{its}, but she is also more likely to be receiving benefits today. If cross-state variation in the total stock of available benefits is exogenous (and I will go some way toward assuring this assumption by including state-specific fixed and time-varying effects in the final specification), the only endogenous element in the actual remaining stock of benefits S_{its} as defined in (7) is a woman's propensity to participate in welfare (k_i). My strategy is to simulate the incentive schedule $\mathbf{L}(\frac{S_{its}}{T_{it}})$ for each woman by using only exogenous variables (N_s and E_{its}) and by replacing the problematic individual specific element k_i with an exogenous propensity to participate in welfare.

I therefore simulate remaining benefits (Z_{its}) for woman i belonging to group j (defined by socio-demographic characteristics that are good predictors of pre-reform welfare participation) as:

$$Z_{its} = N_s - k_j * E_{its} \qquad i \in j \tag{8}$$

where k_j is the average welfare recipiency rate in the socio-demographic group j, calculated at the *national level* and in the *pre-reform period* to avoid any endogeneity to the reforms. I then instrument the four endogenous regressors $\mathbf{L}(\frac{S_{its}}{T_{it}})$ with the simulated variables:

$$\mathbf{L}(\frac{Z_{its}}{T_{it}}) = \mathbb{I}(t \geq \overline{T}_s)\,\{\,\mathbb{I}(0 < \frac{Z_{its}}{T_{it}} < 1),\ \frac{Z_{its}}{T_{it}}\mathbb{I}(0 < \frac{Z_{its}}{T_{it}} < 1),\ \mathbb{I}(\frac{Z_{its}}{T_{it}} \geq 1),\ \mathbb{I}(\frac{Z_{its}}{T_{it}} \leq 0)\,\}$$

The set of k's that calibrate E_{its} are chosen to increase precision. There is a sizable improvement in the fit of the first stage regressions when using observable characteristics to define the likelihood of participation, versus using a single weight for every i, such as $k = 0$, $k = 1$ or k equal to the average welfare participation in the whole population. Setting $k = 0$ means that the only useful identifying variation comes from N_s, while setting $k = 1$ allows use of E_{its} as a source of identifying variation. In both approaches though the fit of the first stage is good only for a specific subset of people, *i.e.* those who have never or who have always used welfare since the implementation of time limits. These two extreme choices share the problems that, in a reduced form specification, characterize the approaches taken respectively in Grogger (2001, 2002, 2004) and in Fang and Keane (2004).

The IV estimation results presented in section 5 are reported for simulated remaining benefits Z_{its}, where E_{its} is calibrated using a set of four weights, that represent average national pre-reform welfare participation by education (two categories: less than an high school degree, and high school degree or more) and number of children (two categories: one or two children, more than two children). Table 3A documents the relationship between the actual $\frac{S}{T}$ and the predicted $\frac{Z}{T}$. Obviously, the reported Z is defined from only one of the many possible set of weights we can calculate using the four socio-demographic characteristics that have the highest predictive power for pre-reform welfare participation (number of children, age of the youngest child, education and race). Using more than two variables or different combinations of them is equivalent in terms of the fit of the first stage regressions. Importantly, second stage results are robust to alternative set of weights.

4.2.2 Policy endogeneity

There is a second endogeneity issue, which arises if the unobservable determinants of welfare use that vary between states and over time (ν_{ts}) are correlated with the state level policy variables I include in (2), such as the time limit variables (total stock of available benefits N_s and date of implementation $\overline{T}_s$). There is indeed some evidence that suggests that this could be the case: MaCurdy, Mancuso, and OBrien-Strain (2002) find that the imposition of termination time limits (and full-family sanctions) between 1992 and 1996 in the five largest states "explains" substantial increases in welfare caseloads between 1989 and 1992. I deal with this problem in the same way as in previous studies (Council of Economic Advisers (1997), Council of Economic Advisers (1999), Grogger (2001, 2002, 2004)), *i.e.* by including in the specification state fixed effects μ_s and state-specific trends $\psi_s(t + t^2)$[34] (or alternatively state-by-year effects $\psi_s \nu_y$, where $\nu_y = \nu_y(t)$ is a dummy for all the months t's that belong to year y). These variables should soak up unobservables at the state level (fixed and changing over time).

[34] Specifications with cubic and quartic state trends were also estimated, but F-statistics on the joint significance of state trends of order higher than the second did not reject zero effects.

4.2.3 Final specification

I therefore perform an instrumental variables estimation of the following equation (which is a variant of (2)):

$$p_{its} = \mathbf{X}_{it}\boldsymbol{\theta} + \mathbf{V}_{ts}\boldsymbol{\phi} + \mathbf{L}(\frac{S_{its}}{T_{it}})\boldsymbol{\pi} + \mu_s + \psi_s f(t) + \nu_y + \eta_i + \widetilde{\nu}_{ts} + \varepsilon_{its} \quad (9)$$

where $\mathbf{X}_{it}$ includes all of the socio-demographic characteristics used to define Z_{its}, *i.e.* the age of the oldest child, education and number of children.[35] The corresponding first-stage regressions (reported in Table 3B) are:

$$L_c(\frac{S_{its}}{T_{it}}) = \mathbf{X}_{it}\boldsymbol{\theta}^c + \mathbf{V}_{ts}\boldsymbol{\phi}^c + \mathbf{L}(\frac{Z_{its}}{T_{it}})\boldsymbol{\pi}^c + \mu_s^c + \psi_s^c f(t) + \nu_y^c + \eta_i^c + \widetilde{\nu}_{ts}^c + \varepsilon_{its}^c \qquad c = 1, 2, 3, 4 \quad (10)$$

4.3 Data

4.3.1 SIPP data

The individual-level data used in this paper come from the Survey of Income and Program Participation (SIPP). The SIPP consists of several separate panels, each of which constitutes an independent sample of the U.S. population. In this study I use data from the 1990, 1991, 1992, 1993, 1996 and 2001 panels, whose combined sampling periods extend from October 1989 to August 2003.[36] In each panel, data are collected at four-month intervals, referred to as "waves". At each interview, respondents are asked to provide information covering the 4 months since the previous interview, so that the data are on a monthly basis.

[35] We have estimated numerous variants of (9) including different possible nonlinear functions of the age of the oldest child, education and number of children (such as polinomials and interactions with year dummies) and we have found no significant change in the estimated coefficients of $\mathbf{L}(S_{its}/T_{it})$. This is evidence that our results should not be driven by some nonlinear relation between p_{its} and the variables in $\mathbf{X}_{it}$ used to define Z_{its}.

[36] We choose to begin the sample in 1990 for two reasons. First, there was essentially no activity in welfare waivers until the early 1990s, so adding earlier years would do little to identify effects of the reform. Second, by starting in 1990, we are able to use data that include both the early 1990s and the early 2000s recessions.

To focus on the primary recipients of welfare, I restrict the sample to single mothers between the ages of 15 and 55.[37] I also exclude women living in 9 states that are not separately identified in the SIPP, which precludes me from merging state-level data.[38] Given the "seam" problem,[39] I use only one observation from each wave, pertaining to the month before the interview. Table A2 presents details on the duration and size of each SIPP panel once the sample restrictions are applied. Table 1 presents summary statistics of the socio-demographic characteristics used in the empirical analysis.[40]

The dependent variable in the empirical specification is a dummy variable equal to one if a woman is receiving income from AFDC/TANF program in a specific month, and zero otherwise. Given that I include separate observations for the same woman in different months,[41] variance estimates in all baseline regressions presented in section 5 are corrected for arbitrary correlation across repeatedly-observed individuals.[42]

The first main advantage of using samples drawn from the SIPP is the availability of data on monthly welfare participation. This is the appropriate variable of interest given that eligibility for welfare is determined on a monthly basis and time limits are expressed in terms of available months of benefit receipts.[43] The second reason for using the SIPP is that it includes information sufficient to calculate the remaining stock of available benefits S_{its} for each woman i in each month t. To define this variable, we need information on prior welfare participation since the implementation of time

[37] The determination of whether a woman has children and how many she has (and of which ages) is obtained by matching the adult's identification number with the number of a child's parent or guardian, instead of using, as common in the literature, the less precise measure of the number of children living in a family.

[38] The excluded states are Alaska, Idaho, Iowa, Maine, Montana, North Dakota, South Dakota, Vermont and Wyoming.

[39] Blank and Ruggles (1996) show that respondents tend to give the same response for all four months within a wave.

[40] All summary statistics (and regressions in section 5) are weighted using month-person weights. The use of weights is necessary, given that two of the samples used in the analysis oversample the low-income population (the 1990 and 1996 panels).

[41] In particular the same woman shows up on average 6 times.

[42] I employ a Huber-White covariance matrix estimator that corrects for arbitrary correlation across individuals (other than for heteroskedasticity).

[43] The fact that respondents are interviewed every four months also reduces misreporting due to recollection problems which can be serious in yearly datasets. Underreporting though is still a problem in the SIPP, as in all survey data. In particular, if underreporting changed over time (Primus, Rawlings, Larin, and Porter (1999)), then time series comparisons are problematic. The main concern in the present study arises from the possibility that the elimination of the entitlement for benefits could have increased underreporting for TANF participation.

limits,[44] and this is obtained in the SIPP using both retrospective questions asked at the beginning of the panel[45] and in-sample information. Once I define the number of months of welfare participation accumulated by each individual since the implementation of time limits, I can build remaining benefits S_{its} as the difference between the state time limit and the number of periods of participation already used since the clock started to tick.[46]

4.3.2 Welfare Reform and other state-level variables

The welfare reform variables I use in this paper are based on my reading of various reports prepared for the U.S. Department of Health and Human Services[47] and of classification schemes presented in previous papers.[48]

PRWORA prohibits states from using federal TANF funds to provide benefits to adults beyond a 60-month lifetime time limit (except that 20 percent of a state's caseload can be exempted). As shown in Table A3, only thirty states have adopted the simple PRWORA standard of a 60 month lifetime time limit. The rest of the states have adopted some other type of

[44] Information on welfare participation history is not available in the Current Population Survey, which is the dataset most widely used in the literature.

[45] A recipiency history module is asked to all respondents in the first wave of the survey. The module asks if respondents not currently in welfare have ever received benefits, and if so when for the first time and for how many times. Importantly, in SIPP 2001 respondents with welfare participation experience at the beginning of the panel are also asked when was the last time they received benefits. Welfare recipients in the first wave of the panel are asked when they started to receive those benefits (and if there were other times they received benefits).

[46] The major source of imprecision in defining this variable comes from the fact that states allow exemptions that temporarily stop the clock for some welfare recipients. In this case, welfare participation occurs without depleting the stock of remaining benefits. Even if states define broad groups to whom exemptions can be applied, it is not feasible to include this information in defining S_{its} for two reasons. First, not all of the categories are based on observable characteristics. Second, welfare workers have large discretion in granting exemptions to recipients who apply for them (Bloom, Farrell, and Fink (2002)). As a result, I define S_{its} abstracting from the existence of extensions, and then I test for the extent to which they were eventually granted by estimating the relationship between having exhausted benefits ($S_{its} < 0$) and current participation. In this way though it is impossible to separate the effect of exemptions from the one of extensions.

[47] The main sources are the chapter "Specific Provisions of State Programs" in the fifth report to Congress (http://www.acf.hhs.gov/programs/ofa/annualreport5/chap12.htm) and the report "State Implementation of Major Changes to Welfare Policies, 1992-1998" (http://aspe.hhs.gov/hsp/Waiver-Policies99/policy_CEA.htm). Detailed information on time limit policies is obtained from Bloom, Farrell, and Fink (2002).

[48] See Council of Economic Advisers (1999), Blank (2001), Bitler, Gelbach, and Hoynes (2002) and Fang and Keane (2004).

plan and, in fact, most of these states have implemented time limits that are stricter than those required by PRWORA, sometimes dramatically so. For example, eight states impose a shorter lifetime limit than 60 months (ranging between 21 and 48 months). Eleven states impose not only a lifetime limit but also a shorter limit over fixed calendar intervals.[49] Five states impose no lifetime limit, but three of them impose intermittent limits. Finally, eight states have relaxed the time limits implicit in PRWORA by adopting "reduction" rather than termination policies.[50] In the empirical exercise presented above, the state time limit N_s is denominated in months of benefit receipt and it corresponds to the most binding time limit imposed in the state (*i.e.*, the intermittent time limit if in place).

There is also state variation in the timing of implementation of time limit policies. I define the state implementation date $\overline{T}_s$ as the month when the state started to count periods of receipt toward time limits. Six states implemented time limits under waivers and continued them after PRWORA, so that the actual counting date is prior to TANF implementation. Even when time limits where implemented for the first time or restarted under TANF, there are ten states that started to count months toward time limits after their initial TANF implementation date. Given that the dates since time limits started to be counted do not overlap completely either with waiver or with TANF dates, I can identify the collective effect of other welfare reform policies by including a dummy for the implementation of "major" (in the sense of involving a significant deviation from the state's AFDC program) statewide waivers and a dummy for the date of implementation of a state TANF program (both reported in Table A3). The waiver dummy is set to zero when the TANF implementation dummy turns on.[51]

The other state-level economic and policy variables included as explanatory variables in the empirical analysis presented in section 5 are the monthly

[49] This policy is referred to as "intermittent time limit" and it is implemented either by limiting individuals to receive no more than x months of receipt in every y months of calendar time, or by obliging recipients to stay out of the program for z months after receiving benefits for x months.

[50] A reduction limit is a limit only for adults, so that children can continue to receive benefits beyond 60 months (paid for out of state funds).

[51] In most cases, the waiver concept becomes meaningless after TANF implementation, because states are given broad control over their welfare policies under PRWORA. It is also practically difficult to know if states who had waivers and then implemented TANF kept the waivers in effect. Importantly for this paper, if a state continued a time limit waiver, then participants' time clocks in that state were running prior to TANF implementation. This information is used to define cross-state variation in the implementation of time limits.

unemployment rate,[52] the AFDC/TANF maximum monthly benefit for a family of three,[53] and two measures of the EITC generosity, *i.e.* the federal maximum credit and the combined federal and state phase-in rate.[54] Table 2 presents summary statistics (by year) of the policy and economic variables presented in this section, calculated in the sample of single-mothers drawn from the SIPP.

5 Results

This section describes the estimated effects of time limits when they are allowed to differ across individuals depending on the ratio between remaining benefits (S_{its}) and eligibility horizon (T_{it}). Table 4 presents estimates of the effects of the incentive schedule captured by $\mathbf{L}(\frac{S_{its}}{T_{it}})$ in (3) on the probability of receiving AFDC/TANF benefits. I only report the estimated coefficients of the four variables in $\mathbf{L}(\frac{S_{its}}{T_{it}})$, whose predicted signs are shown in Box 1 (section 4). The first column of Table 4 reports ordinary least squares (OLS) estimates of (2), while further columns present linear instrumental variables (IV) estimates from specifications that are variants of the equation in (9). The sample is given by 103,768 monthly observations on 16,539 single mothers between the ages of 15 and 55. The dependent variable is a monthly indicator for receiving income from AFDC/TANF programs. The first and second columns of Table 4 include state and year fixed-effects, the third column adds state-specific linear trends, the fourth column adds state quadratic trends, while the fifth does not include either state fixed effects or state-specific trends but adds state-by-year fixed effects. The observable individual socio-demographic characteristics ($\mathbf{X}_{it}$) included in the control set of all specifications are: mother's age at time t, number of children between 0 and 5, between 6 and 12 and between 13 and 18, ages of the youngest and the oldest child, marital status[55], presence of another adult in the house-

[52] Data from Bureau of Labor Statistics, available at http://www.bls.gov/lau/home.htm.

[53] Benefit levels are available by year and expressed in 1996 dollars. Data are from Blank (2001) (for the period 1990-1995) and from the 2000 and 2003 editions of *The Green Book* (U.S. House of Representatives) (for the period 1996-2003).

[54] These measures are available by year and family size and were kindly provided by Hanming Fang and Michael P. Keane. The inclusion of these measures is only a rough way to control for the effects of the EITC, given that the main goal of this paper is to investigate the impact of time limits. For an excellent methodology for summarizing the features of the complex, nonlinear budget sets created by the EITC, see Meyer and Rosenbaum (2001).

[55] I consider four possible marital conditions: never married, separated, divorced and widowed.

hold, education,[56] race,[57] Hispanic origin, nativity,[58] cohort of entry in the U.S.[59] and living in a metropolitan area. The state-level economic and policy variables ($\mathbf{V}_{ts}$) are the unemployment rate, the real AFDC/TANF maximum benefit for a family of three, the federal and state EITC phase-in rate and the federal maximum credit, and the waiver and TANF implementation dummies. These variables vary by state and time, some at monthly level (unemployment rate and waiver and TANF dummies), the others on an yearly basis. The EITC parameters also vary by family size.[60] All baseline regressions are weighted using SIPP month-individual weights, and standard errors are adjusted to allow arbitrary heteroskedasticity, and arbitrary correlation across observations on the same woman.

The OLS estimates presented in the first column of Table 4 are largely at odds with the predictions of the Grogger-Michalopolous model. Women facing incomplete coverage under time limits (corresponding to the event $0 < \frac{S_{its}}{T_{it}} < 1$) are estimated to be more likely to receive benefits relative to a baseline given by women living in a state-month when time limits are not implemented. Additional to this flat effect that applies to everyone with incomplete coverage, a higher $\frac{S_{its}}{T_{it}}$ is estimated to decrease participation. These estimates contradict the banking effect hypothesis, but they could be affected by an omitted variable bias. When remaining periods of eligibility are calculated using actual welfare participation history, it is likely that a woman who participated more in the past has both a low remaining stock of benefits (so that it is more likely that $S_{its} < T_{it}$) and a higher propensity to participate today, because of some unobserved characteristics (such as seriously ill children, an history of abuse or domestic violence, a low quality of education) that make it difficult for her to leave welfare for work.[61] For

[56] I consider 8 possible educational attainments: at most 6^{th} grade, 7^{th} to 8^{th} grade, 9^{th} grade, 10^{th} grade, 11^{th} grade, high school degree, at least some college.

[57] White, Black, American Indian and Asian.

[58] An individual is defined as foreign-born if born outside the U.S. and not of American parents.

[59] The year of entry in the U.S. is a variable available by classes, but the intervals available in subsequent SIPP panels are different. I define 7 consistent cohorts of entry in the U.S.: before 1970, 1970-1979, 1980-1983, 1984-1993, 1994-1995 and 1996 or after.

[60] The AFDC/TANF maximum benefit only varies by state and year and so it is not identified in the specification in column (5) of Table 4, which includes state-by-year fixed effects.

[61] Evidence supporting the existence of an endogeneity bias of this kind is provided by estimating (2) through a period just after the imposition of time limits. Soon after the implementation of time limits, the remaining periods of eligibility S_{its} are still predominantly defined by the state time limit N_s, so that the endogeneity bias should almost disappear. In fact, the estimated coefficients on a sample period 1990-1997 are $\widehat{\alpha} = -0.047$

the same reason, we cannot rely on the OLS estimate that having run out of benefits ($S_{its} < 0$) leads to a higher probability of participation as evidence that time limits are not enforced. We know that states grant extensions to people who have exhausted their benefits but can prove to the welfare office to be facing a particular hardship or not to be able to find work despite diligent efforts. So, the event $S_{its} < 0$ is likely to be endogenous to some unobserved factors that also make it more likely for woman i to be granted to continue to receive benefits. The OLS estimate is also at odds with the zero-effect hypothesis: women who face non-binding time limits ($S_{its} > T_{it}$) are estimated to participate more than women living in states where, and months when, time limits were not implemented. This result cannot be explained by the same endogeneity issue presented above, and I will come back to it when showing how it persists in the IV estimations.

The importance of using only exogenous variation in the remaining stock of benefits S_{its} (as arising from differences in state policies and in individual exposure to time limits) is emphasized by the fact that the IV estimates reported in Table 4 deliver coefficients that support the predictions of the model on the way in which banking efforts should affect the current decision to receive benefits. The IV estimation results are reported for simulated remaining benefits Z_{its} as defined in (8), where the individual exposure to the risk of participating in welfare since the introduction of time limits (E_{its}) is calibrated using a set of four weights (k's), that represent average national pre-reform welfare participation by education and number of children, but results are robust to different sets of k's. The fit of the first stage regressions (equations in (10) including state specific linear and quadratic trends) is reported in Table 3B. Each column presents the estimated coefficients of the four instruments $\mathbf{L}(\frac{Z_{its}}{T_{it}})$ on each endogenous variable $L_c(\frac{S_{its}}{T_{it}})$ for $c = 1, 2, 3, 4$. The instrumental variables are statistically significant predictors of the endogenous variables and the F-statistics on the joint significance tests show how weak instruments are not a concern here.

The signs of the estimated coefficients of the incentive functions are the same across the different IV specifications, and, as opposed to the OLS estimates, they consistently confirm the main prediction of the Grogger-Michalopolous model on the existence and direction of banking effects ($\widehat{\alpha} \leq 0$ and $\widehat{\beta} \geq 0$). Time limits are estimated to be enforced ($\widehat{\delta} \leq 0$), again as opposed to the OLS estimate. However, as in the case of the OLS estimation,

and $\widehat{\beta} = 0.038$, on a sample period 1990-1998 are $\widehat{\alpha} = -0.026$ and $\widehat{\beta} = 0.006$, while on a sample period 1990-2000 they start to show the same counterintuitive signs as in Table 4 ($\widehat{\alpha} = 0.002$ and $\widehat{\beta} = -0.03$).

the IV estimation delivers the unlikely result that women not constrained by the time limit policy, because their remaining benefits are more than the ones they can use, participate more than women living in states and months when time limits are not implemented. This contradicts the prediction that non-binding time limits should have no behavioral effect (zero-effect hypothesis) and raises the concern of the existence of correlation between unobservable determinants of welfare use and the timing and nature of states' welfare reforms. This concern, though, is not supported by the fact that the estimated coefficients are qualitatively the same across specifications that include additional controls for policy endogeneity. This suggests that this result could be evidence of other factors, such as a decline in the stigma of being on welfare for those that do not face binding time limit constraints. Another explanation is that limiting the total available benefits (as done by a newly introduced time limit policy) may discourage potential recipients from *ever* applying for benefits. If this is the case, women with $S_{its} > T_{it}$ would be the ones not affected by this effect, because for them the system is at least as generous as before the introduction of time limits. This may help explain the puzzling estimation result: women with $S_{its} > T_{it}$ may be predicted to participate more than before time limit implementation because people with $S_{its} = N_s < T_{it}$ are discouraged from ever entering in welfare. To reduce the concern of policy endogeneity, the remainder of the section reports results only for the specification that includes state-specific linear and quadratic trends that should soak up the effects of unobserved factors that vary over time at the state level.

As regards the interpretation of the estimated coefficients, recall that the predicted combined effect of the two elements (flat and marginal) that make up the banking effect hypothesis is as shown in Figure 3A. Women with very low coverage $\frac{S_{its}}{T_{it}} = 0.1$ are 7.5 percentage points less likely to use welfare than women with full coverage (as represented by the group not facing time limits). When coverage increases the negative predicted effect decreases in magnitude until it becomes positive for a coverage more than one-half (the maximum value of $\frac{S_{its}}{T_{it}}$ that guarantees $\widehat{\alpha} + \widehat{\beta}\frac{S_{its}}{T_{it}} \leq 0$ is reported in the last row of Table 4). It is true that this contradicts the prediction that all women with incomplete coverage should feel the incentive to participate less, but inspection of the empirical distribution of $\frac{S_{its}}{T_{it}}$ (plotted in Figure 4) shows that 85% of the women with incomplete coverage actually have a $\frac{S_{its}}{T_{it}} \leq 0.5$, so are predicted to participate less than women not facing time limits.

In the specification presented in the last column of Table 4 I run a test for overidentifying restrictions. Using a multidimensional set of k's allows me to reformulate the specification in (9) in a model that is overidentified, so that I can test empirically for the exogeneity of the selected instruments.[62] Results from a test for overidentifying restrictions is reported in the last row of column (6): the Hansen's J statistic takes the value 3.2 and exogeneity is not rejected at the 1% significance level.

Table 5 tests the sensitivity of the statistical significance of the estimated coefficients to different variance estimates. In baseline estimations standard errors are adjusted to allow not only for heteroskedasticity, but also for arbitrary correlation across observations on the same woman. To better cope with the potential correlation over time of the outcomes for the same woman, standard errors are also adjusted to allow for arbitrary *autocorrelation*.[63] The significance of the estimated coefficients is preserved as shown in column (2) of Table 5, and standard errors actually decrease slightly relative to a correction for arbitrary correlation across individuals (column (1)). Another concern is the correlation of the dependent variable across states. To address this problem column (3) of Table 5 reports standard errors which are adjusted for arbitrary correlation across states, and again the significance of the coefficients is preserved.

Table 6 tests the sensitivity of the estimated coefficients to the model specification. The results presented so far are based on a linear probability model, and they are reported for the preferred specification (including state linear and quadratic trends) in the first column of Table 6. The second column presents the coefficient estimates of the relevant variables when a probit model is estimated. To ease the interpretation of the probit estimates, the third column reports the change in the probability of receiving welfare with respect to each of the four relevant variables, holding all the other explanatory variables at their mean in the sample. The partial effect from changing the dummy $\mathbb{I}(0 < \frac{S_{its}}{T_{it}} < 1)$ from zero to one is calculated when the dummies $\mathbb{I}(\frac{S_{its}}{T_{it}} > 1)$ and $\mathbb{I}(\frac{S_{its}}{T_{it}} < 0)$ are set to zero,[64] and it

[62] I define 3 instruments for each $L_c(\frac{S_{its}}{T_{it}})$, given by $L_c(\frac{Z^1_{its}}{T_{it}})$, $L_c(\frac{Z^2_{its}}{T_{it}})$ and $L_c(\frac{Z^3_{its}}{T_{it}})$ where Z^1_{its} is predicted remaining eligibility using 4 k's, Z^2_{its} is predicted remaining eligibility using only 2 k's (by education) and Z^3_{its} is predicted remaining eligibility using only 2 k's (by number of children).

[63] To generate adjusted standard errors, I use Stata's -bw(4)- option after the -ivreg2- command, which reports standard errors that are robust to arbitrary autocorrelation (AC). The option performs a kernel-based AC estimation (Newey-West kernel) on panel data, where the cross-section variable is the individual and the time-series variable is the month of the year.

[64] This insures that the partial effect of $\mathbb{I}(0 < \frac{S_{its}}{T_{it}} < 1)$ can be interpreted relative to the baseline of time limits not implemented, as in the linear case.

takes into account the direct effect of the dummy and the effect through the term $\frac{S_{its}}{T_{it}}\mathbb{I}(0 < \frac{S_{its}}{T_{it}} < 1)$. Column 3 of Table 6 reports the partial effect of $\mathbb{I}(0 < \frac{S_{its}}{T_{it}} < 1)$ when $\frac{S_{its}}{T_{it}} = 0.3$,[65] while Figure 5 plots the partial effect as a function of $\frac{S_{its}}{T_{it}}$ in the range $(0, 1)$, and the confidence interval at the 95%. As in the linear case, the banking hypothesis is supported by a probit estimate. Incomplete coverage is associated with a decrease in the probability of participating in welfare and the magnitude of the effect decreases when coverage increases. The negative effect vanishes for coverage greater than one-half. Probit estimates deliver similar results to the linear case also when analyzing the other two hypotheses. In particular, the enforcement hypothesis is supported in a probit specification: having exhausted benefits is predicted to reduce welfare, and the effect is large but not precisely estimated. Also, non-binding time limits ($\frac{S_{its}}{T_{it}} > 1$) are associated with an increase in the probability of receiving welfare benefits, which is at odds with the zero effect hypothesis.

It is now useful to quantify how much allowing for heterogeneous time limit effects help explain the 1996-2003 drop in welfare participation. Section 2 shows that a specification for individual welfare participation that controls for changes in socio-demographic characteristics, business cycle effects, the increasing generosity of the Earned Income Tax Credit, and the implementation of the reform as a bundle still attributes almost two-thirds of the 1996-2003 drop in participation to unexplained time effects. Adding a control for the effects of time limits through a dummy for their implementation left almost the same amount of unexplained change in welfare participation as when not controlling for them (column (5) of Table A1). Table A4 (as an extension of Table A1), presents changes in yearly average welfare participation rates across states (relative to the 1990 level) as predicted by the variables capturing time effects (year dummies and state-specific trends) in a linear specification for the probability to participate in welfare as a function of individual socio-demographic characteristics, state-level economic and policy variables and the incentive schedule $\mathbf{L}(\frac{S_{its}}{T_{it}})$. Column (3) presents the estimated time effects from an ordinary least squares regression, while column (4) presents results from a linear instrumental variables estimation. When estimating heterogenous time limit effects, an OLS estimate decreases the explanatory power of the model (because of the endogeneity bias), while an IV estimate more than doubles it (Figure 6). The share of the 1996-2003 welfare participation drop attributed to time effects in column (4) is one third of the share attributed to time effects in a specifi-

[65] The mean of $\frac{S_{its}}{T_{it}}$ in the range $(0, 1)$.

cation where the effects of time limits are constrained to be the same across the population (column (2)).

6 Conclusions

Recent changes in the U.S. welfare policy provide a unique opportunity to investigate the determinants of program participation. This paper focuses on the effects of the controversial provision in the 1996 reform restricting individuals to at most five years of federal benefits. The draconian aspect of this provision is to lead to the cut-off of benefits after the time-limit is reached, if the policy is strictly enforced and the needy applicant is not granted an extension. On the other hand, time-limited provision of benefits may also have behavioral effects that can promote targeting efficiency. If potential recipients are forward-looking, then they should "bank" their benefits by going off the rolls during good (labor market) times and saving their benefits for bad times. Time limits can thus succeed in making cash assistance a transitional support for people who are suffering temporary difficulties because of a job loss or some personal problem, while keeping out of the program people who are in periods in which they can sustain themselves through work.

The main contribution of this paper is to estimate the behavioral ("banking") effect of time limits using detailed micro data that include information on welfare participation history. Periods of welfare participation since the introduction of time limits are subtracted from the total stock of benefits available at the state level to define remaining periods of eligibility for each woman at each date. The dynamic model of welfare participation in Grogger and Michalopoulos (2003) predicts that the incentive to bank benefits is higher the longer the horizon of potential need to smooth consumption through benefits (defined by the periods until the youngest child turns 18) and the shorter the periods of remaining eligibility. In regressions that account for the potential endogeneity of an individual's past usage, time limits are found to influence welfare participation not only through their direct effect, but also through a banking effect.

The results show that time limits are a major explanation of the dramatic drop in welfare participation since the middle of the 1990s. This result is at odds with some previous studies reporting that time limits had little effect on caseloads. This is due to my original identification strategy, which allows time limit effects to increase with past welfare participation, while previous studies made the implicit assumption that the effects of time limits are constant across the population and in time. The results of this paper confirm findings in Grogger (2001, 2002, 2004) that time limits play a sizable role in explaining welfare participation, but they are obtained from an identification strategy that avoids making the strong assumptions required in their difference-in-differences approach.

My paper provides evidence of the existence of a behavioral effect of time limits on current welfare participation. There are many research questions that can be addressed starting from here.

Banking may only occur if a woman has currently an alternative way of financing consumption. Assessing which alternative sources of income a family that is banking benefits is relying on is an important policy question, to evaluate if the reform succeeded in making benefits perceived as temporary while promoting a transition from welfare to work. If the incentive to bank benefits increases the fraction of income coming from labor earnings, then time limits may not only promote targeting efficiency, but also improve welfare of the individuals who decide to stay out of the rolls by giving them an incentive to start working earlier than they would have in the absence of a time limit.[66] An interesting future extension of the present work could then be an analysis of the income sources of women banking benefits. For example, I can use the same approach presented in this paper, but change the outcome variable from welfare participation to labor supply or portion of income coming from a specific source.

Finally, there is a further way in which time limits could affect the behavior of single mothers and which I am currently exploring in another paper. Time limits may discourage women from *ever* participating in a welfare program. Stigma arising from receipt of welfare benefits has commonly been proposed as an explanation for the low take-up rate among eligible women. If this stigma depends heavily on whether one has ever received benefits, then welfare eligible individuals would decline to make use of the program if the years of potential eligibility are shortened enough. I plan to test for this effect by examining whether cross-state differences in the number of months an individual can receive welfare benefits explain drops in the number of women who ever claim benefits. These research issues provide a fascinating agenda for future work.

[66] Starting to work earlier increases an individual's lifetime welfare under the assumption that the choice to work in the labor market implies delayed benefits in terms of human capital accumulation. Fang and Silverman (2004) prove that under this assumption the imposition of time limits may improve the well-being of welfare recipients with present-biased preferences, by providing them with a commitment to work.

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Table 1: Descriptive Statistics of Individual Level Characteristics

	mean	standard error	min	max
Welfare participation	0.228	0.001	0	1
Age	33.159	0.027	15	55
Number of children 0-5 years old	0.607	0.002	0	6
Number of children 6-12 years old	0.709	0.003	0	6
Number of children 13-17 years old	0.427	0.002	0	6
Age of the youngest child	6.872	0.016	0	17
Age of the oldest child	9.256	0.016	0	17
Woman never married	0.403	0.002	0	1
Woman separated	0.195	0.001	0	1
Woman widow	0.041	0.001	0	1
Woman divorced	0.362	0.002	0	1
At most sixth grade	0.024	0.000	0	1
Seventh or eigth grade	0.024	0.000	0	1
Nineth grade	0.034	0.001	0	1
Tenth grade	0.056	0.001	0	1
Eleventh grade	0.081	0.001	0	1
High School graduate	0.392	0.002	0	1
At least some years of College	0.389	0.002	0	1
White	0.625	0.001	0	1
Black	0.341	0.002	0	1
American Indian	0.012	0.000	0	1
Asian	0.022	0.000	0	1
Hispanic	0.161	0.001	0	1
Foreign-born	0.113	0.001	0	1
Cohort of entry in the US: before 1970	0.149	0.004	0	1
Cohort of entry in the US: 1970-1979	0.262	0.004	0	1
Cohort of entry in the US: 1980-1984	0.188	0.004	0	1
Cohort of entry in the US: 1985-1993	0.261	0.004	0	1
Cohort of entry in the US: 1994-1995	0.057	0.002	0	1
Cohort of entry in the US: after 1995	0.031	0.002	0	1
Remaining benefits S / eligibility horizon T				
between zero and one	0.283	0.001	0	1
more than or equal to one	0.046	0.001	0	1
less than or equal to zero	0.009	0.000	0	1
Not in a state-period with time limits	0.661	0.001	0	1

Notes: The sample is given by 103,768 observations on 16,539 single-mothers between the ages of 15 and 55. The sample period is January 1990- August 2003. Figures are weighted.
Source: Survey of Income and Program Participation panels (1990, 1991, 1992, 1993, 1996, 2001)

Table 2: Descriptive Statistics of State-level Policy and Economic Variables, by year

	1990	1991	1992	1993	1994	1995	1996	1997	1998	1999	2000	2001	2002	2003
Welfare participation rate	0.285	0.287	0.293	0.310	0.303	0.302	0.232	0.197	0.158	0.122	0.099	0.094	0.081	0.079
Unemployment Rate (%)	5.73	6.96	7.67	7.09	6.24	5.73	5.46	5.02	4.58	4.29	4.10	4.80	5.88	6.11
Maximum Welfare Benefit ($	474.99	460.90	449.28	431.98	416.15	404.50	384.66	374.83	372.93	367.44	367.56	360.84	358.48	347.48
EITC Maximum Credit ($)	953	1,213	1,353	1,472	2,278	2,594	2,818	2,891	2,966	3,019	3,079	3,178	3,296	3,346
EITC Phase-in Rate	0.142	0.174	0.183	0.193	0.273	0.357	0.378	0.379	0.377	0.377	0.380	0.384	0.386	0.386
Waiver Implementation	0	0	0.201	0.271	0.320	0.463	0.317	0.126	0	0	0	0	0	0
TANF Implementation	0	0	0	0	0	0	0.471	0.874	1.000	1.000	1.000	1.000	1.000	1.000
TIME LIMIT Implementation	0	0	0	0	0	0.002	0.128	0.631	0.953	0.961	0.963	0.967	0.970	0.971

Notes: The sample is given by 103,768 observations on 16,539 single-mothers between the ages of 15 and 55. The sample period is January 1990-August 2003. Figures are weighted.
Source: Survey of Income and Program Participation panels (1990, 1991, 1992, 1993, 1996, 2001)

Table 3A: Predicted remaining benefits Z and predicted coverage Z/T

Given the remaining benefits $S_{its} = N_s - \sum_{\tau=\bar{T}_s} p_{i\tau s} = N_s - k_i E_{its}$, the predicted remaining benefits are defined as $Z_{its} = N_s - k_j E_{its} \quad i \in j$ where k_j is average (national pre-reform) welfare participation in the socio-demographic group j defined by education (two categories) and number of children (two categories) taking the 4 following values:

	1 or 2 children	More than 2 children
Less than HS degree	41.23	63.25
HS graduate or more	18.98	39.72

The coverage measure is defined as $\frac{S_{its}}{T_{it}}$ where the eligibility horizon is $T_{it} = (12 * 18) - A_{it}^{y}$

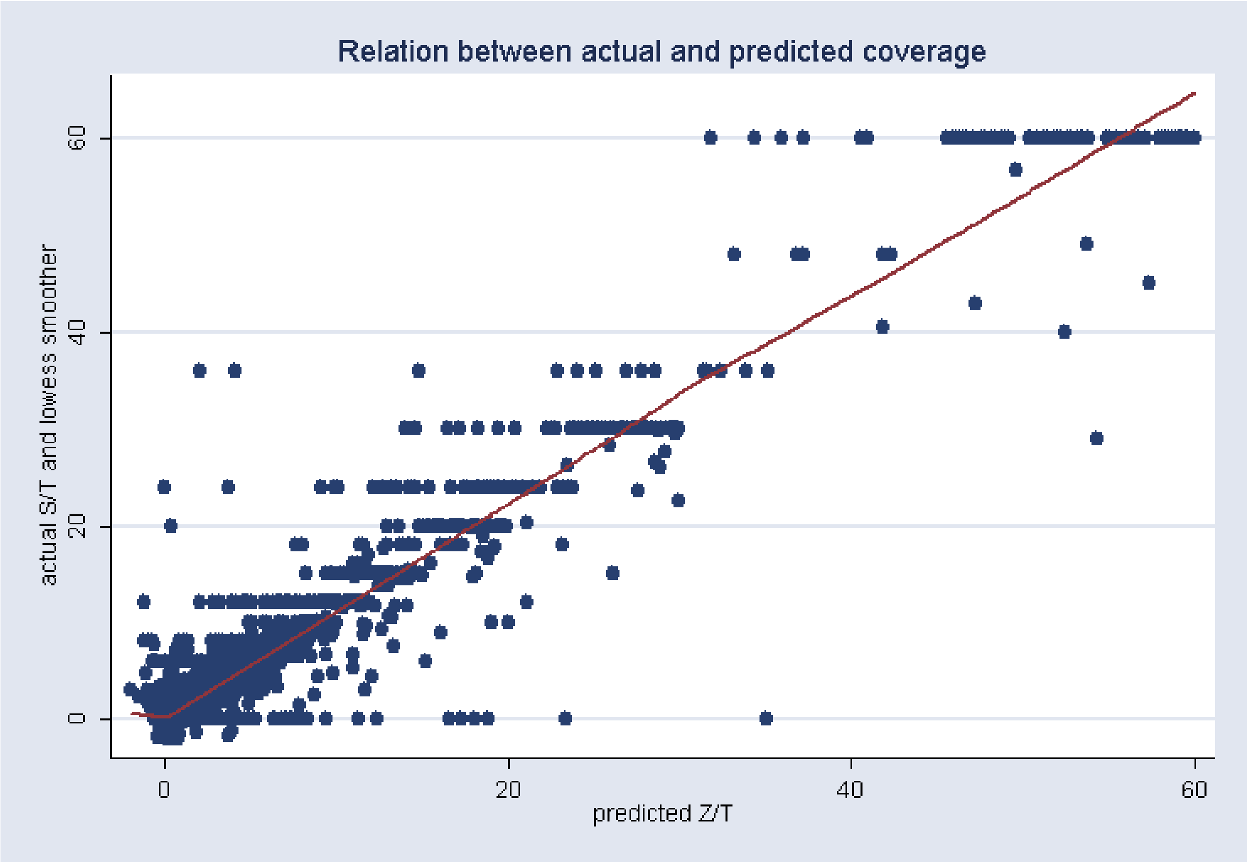


Table 3B: First Stage regressions on the incentive schedule defined by time limits $\mathrm{L}\left(\frac{S_{its}}{T_{it}}\right)$

Dependent variable	*banking effect I(0<S/T<1)*	*banking effect (S/T * I(0<S/T<1))*	*zero effect I(S > T)*	*enforcement effect I(S<0)*
banking effect I(0<Z/T<1)	0.173***	-0.084***	-0.159***	-0.004
	(0.040)	(0.018)	(0.018)	(0.034)
banking effect (Z/T * I(0<Z/T<1))	-0.476***	0.538***	0.513***	-0.066***
	(0.018)	(0.016)	(0.017)	(0.009)
zero effect I(Z > T)	-0.922***	-0.256***	0.953***	-0.038
	(0.041)	(0.018)	(0.019)	(0.034)
enforcement effect I(Z<0)	0.047	-0.075***	-0.112***	0.091**
	(0.046)	(0.020)	(0.021)	(0.039)
R-squared	0.95	0.81	0.83	0.08
F test (of joint significance of the instruments)	8192.934	2456.155	9141.318	20.423
Prob > F	0.00000	0.00000	0.00000	0.00000

Notes: OLS regressions. All specifications include all controls also included in the second stage presented in column (4) of Table 4. Each column reports the coefficients on the four instruments for each of the four endogenous variables $L_c(S/T)$, for $c=1, 2, 3, 4$.

$S_{its} = N_s - \sum_{\tau=\overline{T}_s} p_{i\tau s} = k_i E_{its} = N_s - k_i E_{its}$ defines remaining benefits

$Z_{its} = N_s - k_j E_{its} \quad i \in j$ is predicted remaining benefits as defined in Table 3A.

Standard errors (in parentheses) account for heteroskedasticity and presence of multiple observations per person. Estimates are weighted. The sample is given by 103,768 observations on 16,539 single-mothers between the ages of 15 and 55. The sample period is January 1990- August 2003. Asterisks denote coefficients significantly different from zero at the 10% (*), 5% (**), and 1% (***).

Source: SIPP panels (1990, 1991, 1992, 1993, 1996, 2001)

Table 4: Estimates of the relationship between welfare participation and the incentive schedule defined by time limits $\mathbf{L}\left(\frac{S_{its}}{T_{it}}\right)$

Dependent variable: dummy for individual monthly recipiency of income from AFDC/TANF

	(1) OLS	(2) IV	(3) IV linear state trends	(4) IV linear and quadratic trends	(5) IV state by year effects	(6) IV (overidentified) linear and quadratic trends
banking effect $L_1(S/T)=I(0<S/T<1)$ (α)	0.027*	-0.036**	-0.098***	-0.104***	-0.124***	-0.100***
	(0.014)	(0.017)	(0.017)	(0.017)	(0.016)	(0.016)
banking effect $L_2(S/T)= (S/T * I(0<S/T<1))$ (β)	-0.060***	0.128***	0.209***	0.209***	0.219***	0.196***
	(0.020)	(0.032)	(0.036)	(0.037)	(0.036)	(0.035)
zero effect $L_3(S/T)= I(S > T)$ (γ)	0.071***	0.105***	0.093***	0.088***	0.073***	0.088***
	(0.014)	(0.016)	(0.016)	(0.016)	(0.015)	(0.016)
enforcement effect $L_4(S/T)= I(S<0)$ (δ)	0.383***	-0.363*	-1.031***	-1.139***	-1.051***	-1.245***
	(0.033)	(0.194)	(0.341)	(0.390)	(0.339)	(0.358)
Hansen J statistic						3.250
Prob > j						0.77689
OTHER CONTROLS						
SOCIO-DEMOGRAPHICS X_{it}	YES	YES	YES	YES	YES	YES
POLICY AND ECONOMIC VARIABLES V_{st}	YES	YES	YES	YES	YES	YES
STATE FIXED EFFECTS μ_s	YES	YES	YES	YES	NO	YES
YEAR FIXED EFFECTS v_y	YES	YES	YES	YES	YES	YES
TOTAL BANKING EFFECT NEGATIVE IF $\alpha+\beta(S/T)<0$, i.e. IF:		$S/T < 0.281$	$S/T < 0.468$	$S/T < 0.497$	$S/T < 0.566$	$S/T < 0.510$

Notes: The specifications in columns (2) through (5) are based on linear instrumental variables estimation where the remaining benefits *(S)* are endogenous, and the four functions of *S* in ***L****(S/T)* are instrumented by the same four functions of the predicted remaining benefits *(Z)*. *Z* is predicted using state time limits and exposure to time limits (calibrated by national average propensity to participate in welfare in the pre-reform period by education and number of children). In column (6) the specification is overidentified by breaking down the instruments (using different calibration).

Standard errors (in parentheses) account for heteroskedasticity and presence of multiple observations per person. Estimates are weighted.

The sample is given by 103,768 observations on 16,539 single-mothers between the ages of 15 and 55. The sample period is January 1990- August 2003. Asterisks denote coefficients significantly different from zero at the 10% (*), 5% (**), and 1% (***).

Source: SIPP panels (1990, 1991, 1992, 1993, 1996, 2001)

Table 5: Alternative adjustments of standard errors

Dependent variable: dummy for individual monthly recipiency of income from AFDC/TANF

	(1)	(2)	(3)
Standard errors adjusted for	arbitrary correlation among individuals	arbitrary autocorrelation among individuals	arbitrary correlation among states
anticipatory effect I(0<S/T<1)	-0.104***	-0.104***	-0.104***
	(0.017)	(0.011)	(0.025)
anticipatory effect (S/T * I(0<S/T<1))	0.209***	0.209***	0.209***
	(0.037)	(0.019)	(0.034)
zero effect I(S > T)	0.088***	0.088***	0.088***
	(0.016)	(0.010)	(0.015)
enforcement effect I(S<0)	-1.139***	-1.139***	-1.139***
	(0.390)	(0.207)	(0.296)
OTHER CONTROLS			
SOCIO-DEMOGRAPHICS X_{it}	YES	YES	YES
POLICY AND ECONOMIC VARIABLES V_{st}	YES	YES	YES
STATE FIXED EFFECTS μ_s	YES	YES	YES
YEAR FIXED EFFECTS ν_y	YES	YES	YES
STATE LINEAR AND QUADRATIC TRENDS $\mu_s(t+t^2)$	YES	YES	YES

Notes: The specifications in columns (2) through (5) are based on linear instrumental variables estimation where the remaining benefits *(S)* are endogenous, and the four functions of *S* in ***L****(S/T)* are instrumented by the same four functions of the predicted remaining benefits *(Z)*. All specifications include all controls also included in the specification presented in column (4) of Table 4.
Standard errors (in parentheses) account for heteroskedasticity and arbitrary correlation among individuals (column 1), arbitrary autocorrelation among individuals (column 2), arbitrary correlation among states (column 3). Estimates are weighted. The sample is given by 103,768 observations on 16,539 single-mothers between the ages of 15 and 55. The sample period is January 1990- August 2003. Asterisks denote coefficients significantly different from zero at the 10% (*), 5% (**), and 1% (***).
Source: SIPP panels (1990, 1991, 1992, 1993, 1996, 2001)

Table 6: Estimates of the relationship between welfare participation and the incentive schedule defined by time limits $\mathbf{L}\left(\frac{S_{its}}{T_{it}}\right)$

Linear versus Probit Specification

Dependent variable: dummy for individual monthly recipiency of income from AFDC/TANF

		(1)	(2)	(3)
	Model	*Linear*	*Probit*	*Probit* (marginal effects)
banking effect	I(0<S/T<1)	-0.104***	-0.258***	-.0214**
		(0.017)	(0.072)	(0.011)
banking effect	(S/T * I(0<S/T<1))	0.209***	0.567***	0.141***
		(0.037)	(0.175)	(0.043)
zero effect	I(S > T)	0.088***	0.172**	0.044**
		(0.016)	(0.088)	(0.022)
enforcement effect	I(S<0)	-1.139***	-1.938	-0.487
		(0.390)	(1.180)	(0.298)

Note: The specifications are based on linear instrumental variables estimation where the remaining benefits *(S)* are endogenous, and the four functions of *S* in ***L****(S/T)* are instrumented by the same four functions of the predicted remaining benefits *(Z)*. All specifications include all controls also included in the specification presented in column (4) of Table 4. Standard errors (in parentheses) account for heteroskedasticity and presence of multiple observations per person. Estimates are weighted.
The sample is given by 103,768 observations on 16,539 single-mothers between the ages of 15 and 55. The sample period is January 1990- August 2003. Asterisks denote coefficients significantly different from zero at the 10% (*), 5% (**), and 1% (***).
Source: SIPP panels (1990, 1991, 1992, 1993, 1996, 2001)

Table A1: Changes in welfare participation rates predicted by average time effects across states
Effects of time limits constrained to be the same across the population

	(1) only time effects	(2) add demographics, economy and AFDC benefit level	(3) add EITC generosity	(4) add waiver and TANF dummies	(5) add TL implementation dummy
A- estimated time effects relative to 1990					
year91	0.003	0	0.002	0	0
year92	0.008	0.009	0.015	0.015	0.014
year93	0.026	0.02	0.029	0.03	0.029
year94	0.023	0.021	0.044	0.047	0.046
year95	0.02	0.018	0.031	0.035	0.034
year96	-0.049	-0.051	-0.033	-0.021	-0.022
year97	-0.083	-0.085	-0.063	-0.044	-0.041
year98	-0.121	-0.118	-0.092	-0.071	-0.064
year99	-0.155	-0.149	-0.12	-0.098	-0.091
year00	-0.178	-0.169	-0.137	-0.116	-0.108
year01	-0.188	-0.175	-0.14	-0.12	-0.112
year02	-0.201	-0.188	-0.149	-0.129	-0.122
year03	-0.201	-0.187	-0.145	-0.126	-0.118
B- absolute changes in welfare participation predicted by time effects					
Δ 93-03	-0.227	-0.207	-0.174	-0.156	-0.147
Δ 94-03	-0.224	-0.208	-0.189	-0.173	-0.164
Δ 95-03	-0.221	-0.205	-0.176	-0.161	-0.152
Δ 96-03	-0.152	-0.136	-0.112	-0.105	-0.096

Notes: The dependent variable is a dummy for individual monthly welfare participation. The sample is restricted to single mothers between 15 and 55. The sample period is January 1990- August 2003. All specifications include year dummies and state-specific linear and quadratic trends (*time effects*).

Column 1 only includes time effects as regressors. Column 2 adds individual time-varying socio-demographic characteristics (see text for a list), monthly state unemployment rate and state maximum AFDC benefit level for a family of three. Column 3 adds federal and state EITC phase-in rate and federal EITC maximum credit. Column 4 adds a dummy for implementation of a major statewide waiver and a dummy for implementation of a state TANF program. Column 5 adds a dummy for implementation of a state time-limit policy.

Panel A reports changes in yearly average welfare participation rates as predicted by year dummies and state trends, relative to the 1990 welfare participation rate of 0.282.
Panel B reports the 9j-03's (j=3, 4, 5, 6) changes in welfare participation attributed to time effects in each specification.

Source: SIPP panels (1990, 1991, 1992, 1993, 1996, 2001)

Table A2: Sampling period and size of the samples drawn from the 1990-2001 SIPP panels

Panel	Sampling period	Number of waves	Number of observations	Number of individuals
1990	Jan90-Aug92	8	16,905	3,401
1991	Oct90-Aug93	8	9,019	1,843
1992	Oct91-Aug94	10	15,354	2,837
1993	Oct92-Dec95	9	15,341	2,894
1996	Dec95-Feb00	12	39,357	6,306
2001 [(a)]	Oct00-Aug03	8	28,595	5,112

Notes: (a) figures for the Sipp 2001 refer to the 8 waves released in August 2004
The sample is restricted to single mothers between the ages of 15 and 55. Observations for women living in Alaska, Idaho, Iowa, Maine, Montana, North Dakota, South Dakota, Vermont and Wyoming are excluded. Only one observation for each wave is kept (the one pertaining to the month before the interview).

Source: Survey of Income and Program Participation panels (1990, 1991, 1992, 1993, 1996, 2001)

Table A3: State Welfare Policies
Features of State Time Limit Policies and Date of Implementation of major statewide Waivers, TANF Programs and Time Limits

	Lifetime Limit		Intermittent Time Limit				Implementation			
State	in place	duration (months)	in place	duration (months)	most binding TL (months)	reduction TL [i]	waiver [ii]	TANF	TL [iii]	Date Families first Exceed(ed) limit [iv]
Alabama	1	60	0		60	0		Nov-96	Nov-96	Nov-01
Alaska	1	60	0		60	0		Jul-97	Jul-97	Jul-02
Arizona	0	none	1	24 in 60	24	1	Nov-95	Oct-96	Nov-95	Nov-97
Arkansas	1	24	0		24	0	Jul-94	Jul-97	Jul-98	Jul-00
California	1	60	0		60	1	Dec-92	Jan-98	Jan-98	Jan-03
Colorado	1	60	0		60	0		Jul-97	Jul-97	Jul-02
Connecticut [a]	1	21	1		21	0	Jan-96	Oct-96	Jan-96	Oct-97
Delaware [a]	1	60	1	48 ineligible for 96	48	0	Oct-95 [2]	Mar-97	Mar-97	Oct-99
DC	1	60	0		60	0		Mar-97	Mar-97	Oct-98
Florida [a]	1	48	1	36 in 72	24	0	Jun-96	Oct-96	Oct-96	Oct-98
Florida [b]	1	48	1	24 in 60	36	0				
Georgia	1	48	0		48	0	Jan-94	Jan-97	Jan-97	Jan-01
Hawaii	1	60	0		60	0	Feb-97	Jun-97	Dec-96	Dec-01
Idaho	1	24	0		24	0	Aug-96	Jul-97	Jul-97	Jul-99
Illinois	1	60	0		60	0	Nov-93	Jul-97	Jul-97	Jul-02
Indiana	1	24	0		24	1	May-95 [3]	Oct-96	May-97	Jul-97
Iowa	1	60	0		60	0	Oct-93	Jan-97	Jan-97	Jan-02
Kansas	1	60	0		60	0		Oct-96	Oct-96	Oct-01
Kentucky	1	60	0		60	0		Oct-96	Oct-96	Oct-01
Louisiana	1	60	1	24 in 60	24	0		Jan-97	Jan-97	Jan-99
Maine	1	60	0		60	1	Jun-96	Nov-96	Nov-96	Nov-01
Maryland	1	60	0		60	1	Mar-96	Dec-96	Jan-97	Jan-02
Massachusetts	0	none	1	24 in 60	24	0	Nov-95	Sep-96	Dec-96	Dec-98
Michigan	0	none	0		.		Oct-92	Sep-96	----	----
Minnesota	1	60	0		60	0		Jul-97	Jul-97	Jul-02
Mississippi	1	60	0		60	0	Oct-95	Jul-97	Oct-96	Oct-01
Missouri	1	60	0		60	0	Jun-95	Dec-96	Jul-97	Jul-02
Montana	1	60	0		60	0	Feb-96	Feb-97	Feb-97	Feb-02
Nebraska	1	48	1	24 in 48	24	0	Oct-95	Dec-96	Dec-96	Dec-98
Nevada	1	60	1	24 ineligible for 12	24	0		Dec-96	Jan-98	Jan-00
New Hampshire	1	60	0		60	0	Jun-96	Oct-96	Oct-96	Oct-01
New Jersey	1	60	0		60	0	Oct-92	Jul-97	Mar-97	Mar-02

New Mexico	1	60	0		60	0		Jul-97	Jul-97	Jul-02
New York	0	60 [1]	0		60	1		Nov-97	Dec-96	Dec-01
North Carolina	1	60	1	24 ineligible for 36	24	0	Jul-96	Jan-97	Jul-96	Jul-98
North Dakota	1	60	0		60	0		Jul-97	Jul-97	Jul-02
Ohio	1	60	1	36 ineligible for 24	36	0	Jul-96	Oct-96	Oct-97	Oct-00
Oklahoma	1	60	0		60	0		Oct-96	Oct-96	Oct-01
Oregon	0	none	1	24 in 84	24	1	Feb-93	Oct-96	Jul-96	Jul-98
Pennsylvania	1	60	0		60	0		Mar-97	Mar-97	Mar-02
Rhode Island	1	60	0		60	1		May-97	May-97	May-02
South Carolina	1	60	1	24 in 120	24	0	May-96	Oct-96	Oct-96	Oct-98
South Dakota	1	60	0		60	0	Jun-94	Dec-96	Dec-96	Dec-01
Tennessee	1	60	1	18 ineligible for 3	18	0	Sep-96	Oct-96	Sep-96	Mar-98
Texas [a]	1	60	1	12 ineligible for 60	12	0	Jun-96 [4]	Nov-96	Sep-97	Jun-97
Texas [b]	1	60	1	24 ineligible for 60	24	0	"	"	"	Jun-98
Texas [c]	1	60	1	36 ineligible for 60	36	0	"	"	"	Jun-99
Utah	1	36	0		36	0	Jan-93	Oct-96	Jan-97	Jan-00
Vermont	0	none	0		.		Jul-94	Sep-96	----	----
Virginia	1	60	1	24 ineligible for 24	24	0	Jul-95 [5]	Feb-97	Oct-97	Oct-99
Washington	1	60	0		60	0	Jan-96	Jan-97	Aug-97	Aug-02
West Virginia	1	60	0		60	0		Jan-97	Jan-97	Jan-02
Wisconsin	1	60	0		60	0	Jan-96	Sep-97	Oct-96	Oct-01
Wyoming	1	60	0		60	0		Jan-97	Jan-97	Jan-02

[i] A reduction time limit (as opposed to the general case of termination time limit) means that the child portion of the welfare benefit continues after time limits exhaustion

[ii] Implementation of major statewide waivers.

[iii] Effective date for time limits (actual counting date for statewide time limits).

[iv] Denotes the month following the date families could potentially accumulate the maximum number of months of TANF assistance

Connecticut [a]: In October 2001 Connecticut imposed a new 60-month time limit that allows fewer exceptions than the previous 21 month limit, and that counts benefits received since October 1996.

Delaware [a]: In Januray 2000, Delaware introduced a new 36-month lifetime limit.

Florida [a] applies to women with age under 24 and who did not finish high school

Florida [b] applies to other women

Texas [a] applies to women has at least a high school diploma

Texas [b] applies to women who completed three or more years of high school without high school diploma

Texas [c] applies to women who have completed less than 3 years of high school

[1] New York state has a 60 month lifetime limit, but state funds would continue to provide the same benefits afterward. Fang and Keane (2002) treats NY as if it had no effective time limits

[2] Delaware began implementation of its termination time limit with a small number of cases in October 1995; the policy became universal in March 1997

[3] Indiana began implementation of its 24-month time limit policy for "job-ready" non-exempt cases in July 1995; beginning May 1997 the 24-month time limit was expanded to all non-exempt cases.

[4] Texas' 12, 24, or 36 month time limit began in one county in June 1996 and was expanded to the entire state by September 1997. The federal 60 month time limit was imposed beginning November 1996.

[5] Virginia's termination time limit began in five counties in July 1995 and was expanded to the entire state by October 1997.

Sources: CEA (1999), Bloom et al. (2002), Fang and Keane (2004), Bitler et al. (2004), ASPE webpage (http://aspe.hhs.gov/hsp/waiver-Policies99/W1tim_limt.htm#N5)

TableA4: Changes in welfare participation rates predicted by average time effects across states
Estimation of heterogenous time limit effects: OLS versus IV

	(1)	(2)	(3)	(4)
	only time effects	add individual socio-demographic characteristics and state-level economic and policy variables		
		add dummy for time limit implementation	add heterogeneous time limit effects	add heterogeneous time limit effects
model	*OLS*	*OLS*	*OLS*	*IV*
A- estimated time effects relative to 1990				
year91	0.003	0	-0.001	0
year92	0.008	0.014	0.013	0.012
year93	0.026	0.029	0.028	0.024
year94	0.023	0.046	0.042	0.034
year95	0.02	0.034	0.027	0.024
year96	-0.049	-0.022	-0.032	-0.028
year97	-0.083	-0.041	-0.054	-0.041
year98	-0.121	-0.064	-0.08	-0.059
year99	-0.155	-0.091	-0.111	-0.076
year00	-0.178	-0.108	-0.135	-0.076
year01	-0.188	-0.112	-0.139	-0.075
year02	-0.201	-0.122	-0.153	-0.075
year03	-0.201	-0.118	-0.153	-0.061
B- absolute changes in welfare participation predicted by time effects				
Δ 93-03	-0.227	-0.147	-0.181	-0.085
Δ 94-03	-0.224	-0.164	-0.195	-0.095
Δ 95-03	-0.221	-0.152	-0.18	-0.085
Δ 96-03	-0.152	-0.096	-0.121	-0.033

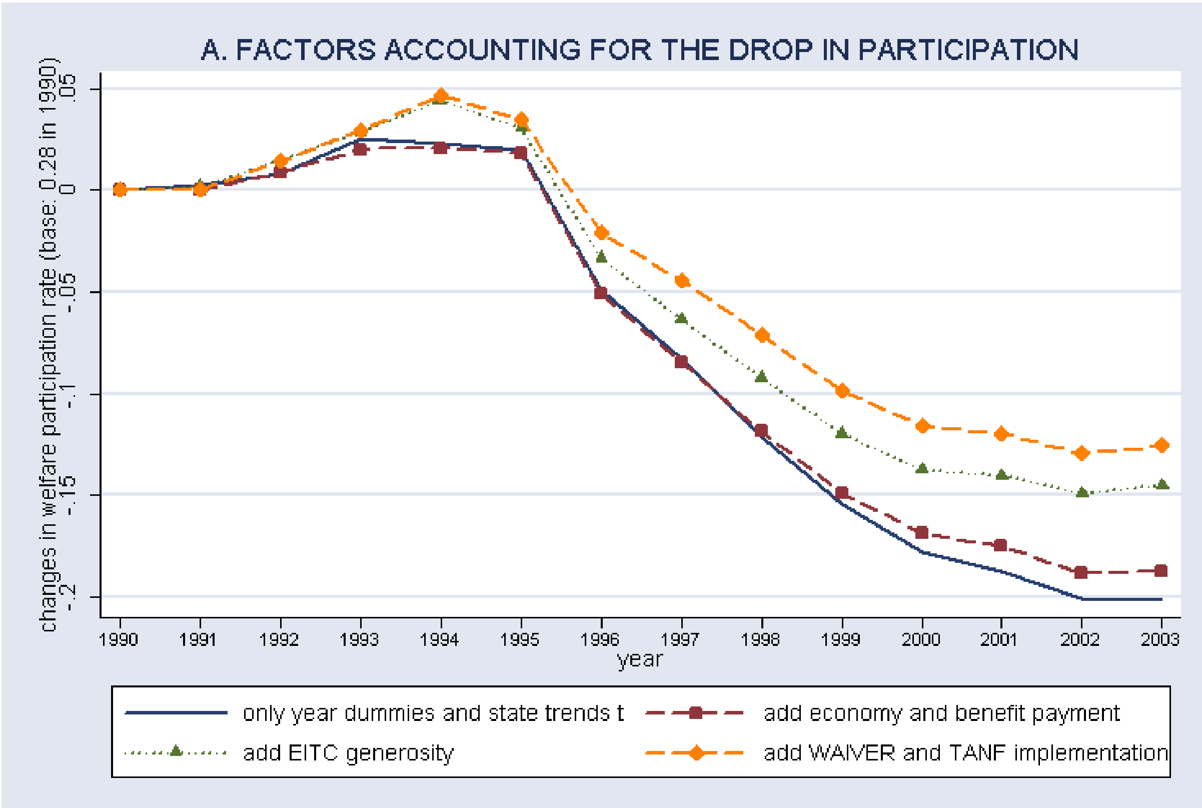
Note: The dependent variable is a dummy for individual monthly welfare participation. The sample is restricted to single mothers between 15 and 55. The sample period is January 1990- August 2003. All specifications include year dummies and state-specific linear and quadratic trends (*time effects*). Column 2-4 add individual socio-demographic characteristics and state-level economic and policy variables (see text for a list). Column 2 adds a dummy for implementation of a state time-limit policy, while column 3 and 4 estimate heterogenous time limit effects through the inclusion of four variables of the ratio between remaining benefits and eligibility horizon *(S/T)* .

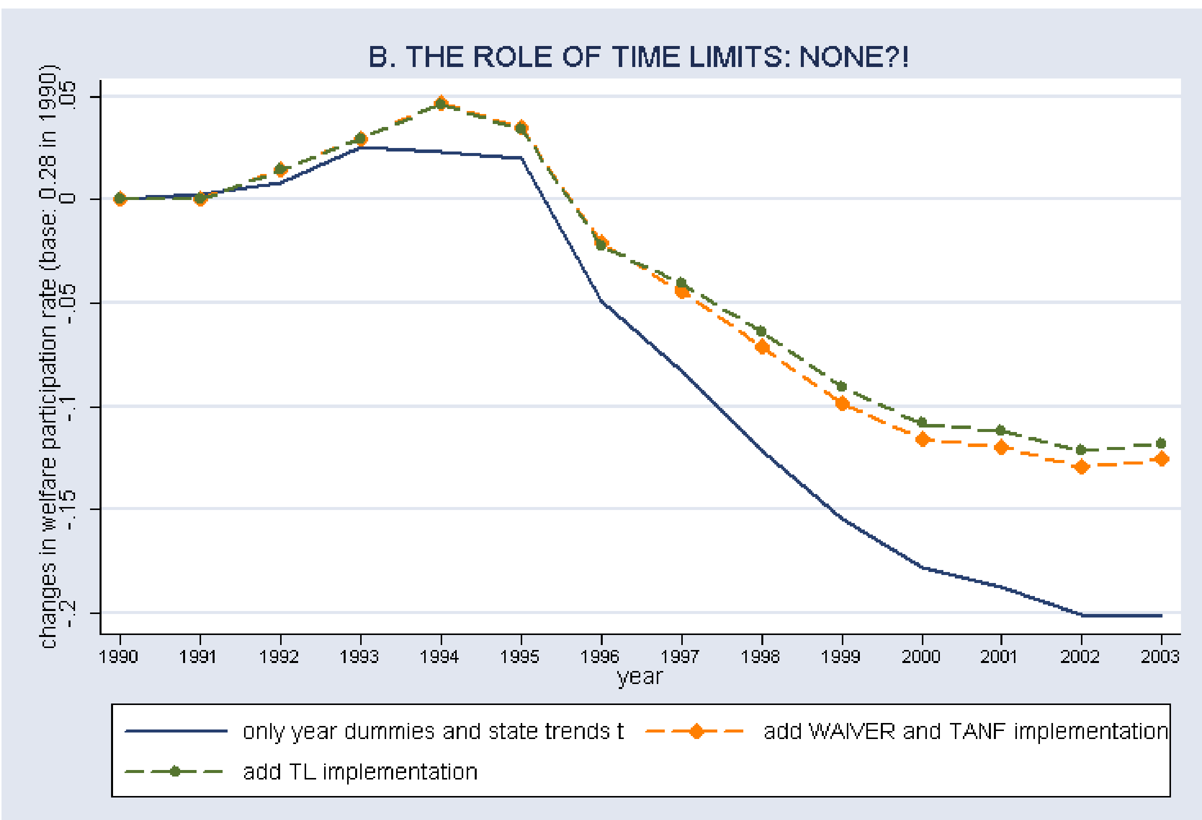
Panel A reports changes in yearly average welfare participation rates as predicted by year dummies and state trends, relative to the 1990 welfare participation rate of 0.282.

Panel B reports the 9j-03's (j=3, 4, 5, 6) changes in welfare participation attributed to time effects in each specification.

Source: SIPP panels (1990, 1991, 1992, 1993, 1996, 2001)

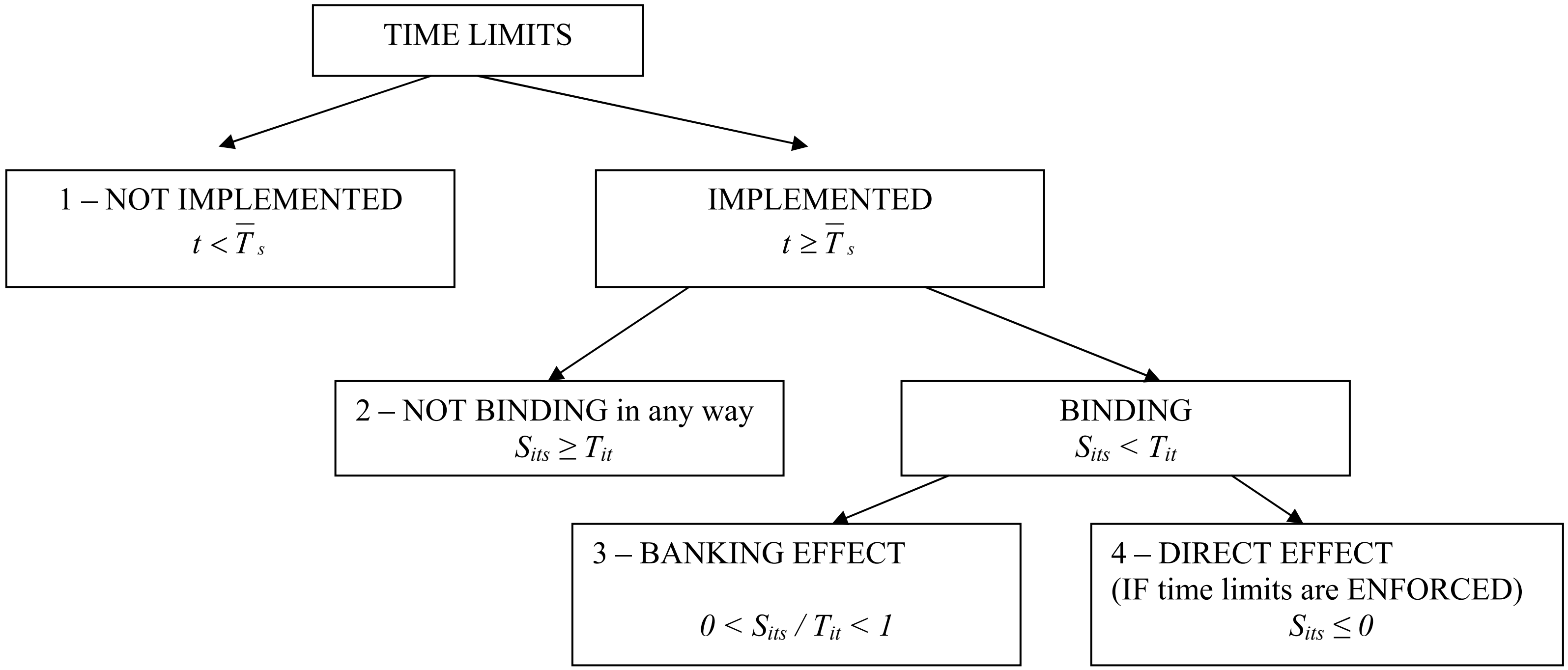
Figure 1: Estimated average time effects (year fixed effects and state specific linear and quadratic trends) from a regression of individual monthly welfare participation





Note: see Table A1 for details
Source: SIPP panels (1990, 1991, 1992, 1993, 1996, 20001)

Figure 2: Relevant variables to catch the effects of time limits



Legend: $\overline{T}_s$: date when state s starts to count the limits

S_{its}: stock of remaining benefits (state time limits minus periods of participation since implementation of time limits)

T_{it}: eligibility horizon (months until the youngest child turns 18)

Figure 3A: Banking effect of time limits (predicted)

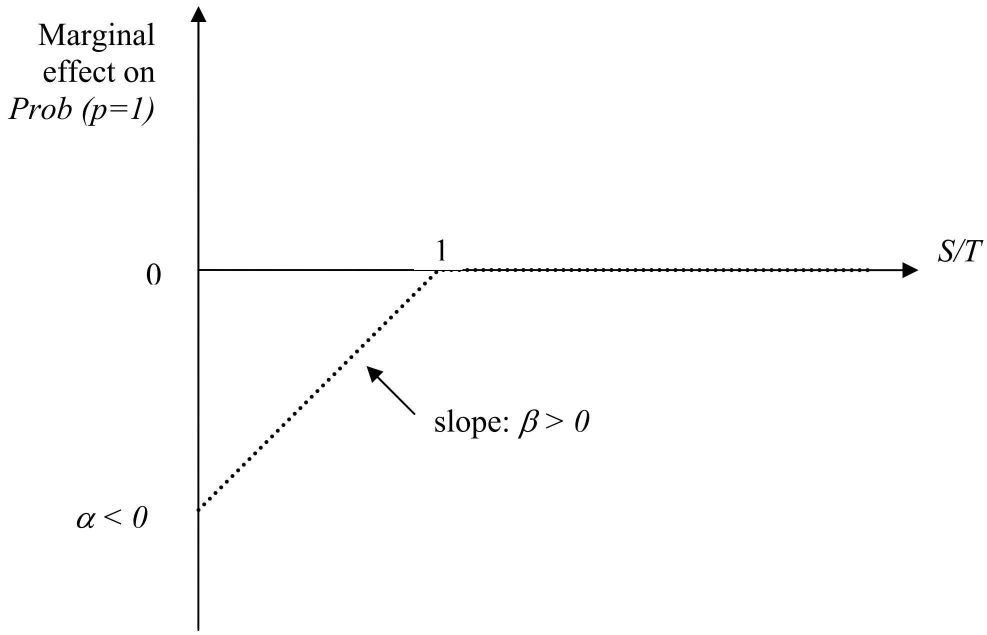


Figure 3B: Banking effect of time limits (IV estimates)

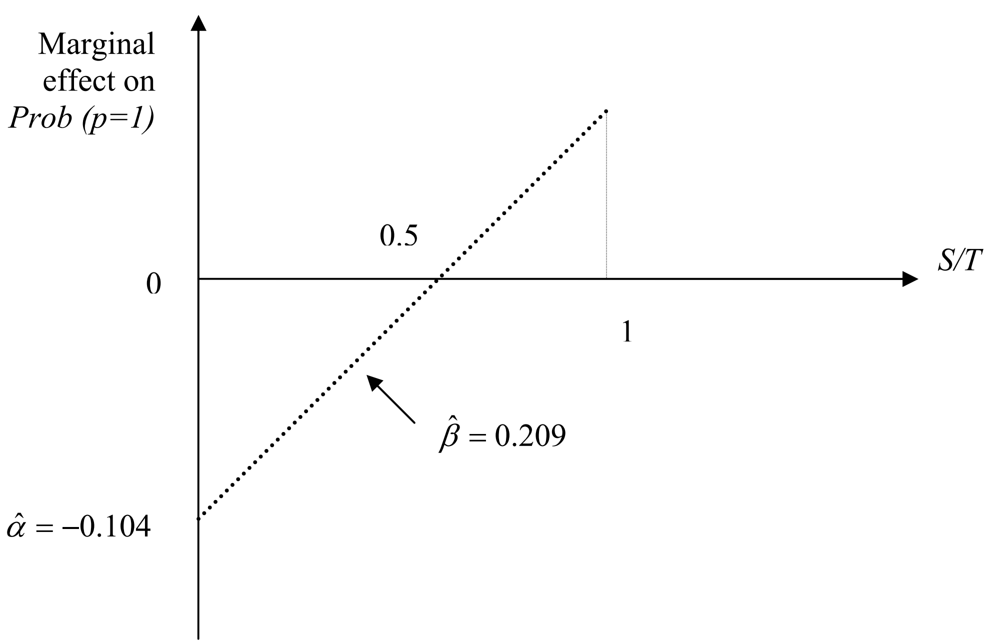


Figure 4: Empirical distribution of the coverage measure (S/T) when coverage is incomplete

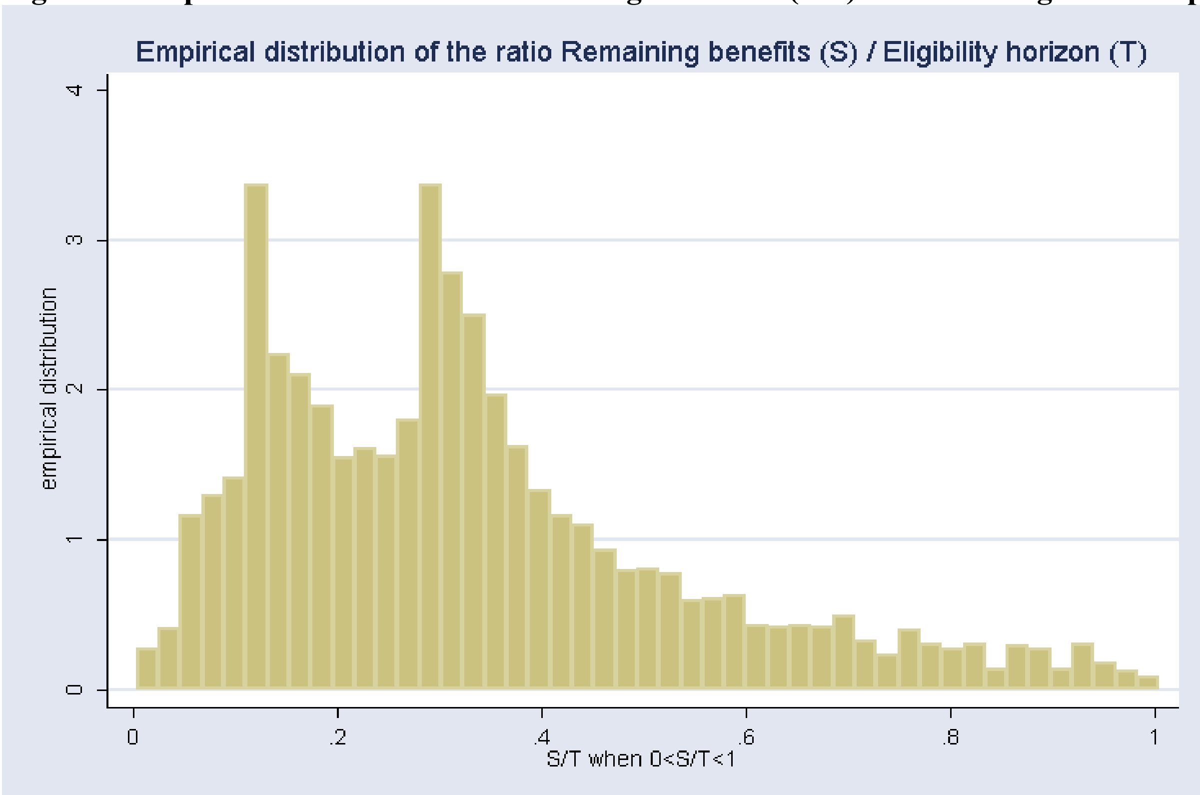


Figure 5: Probit estimates of the partial effect of an incomplete coverage (0<S/T<1) on the probability of receiving benefits

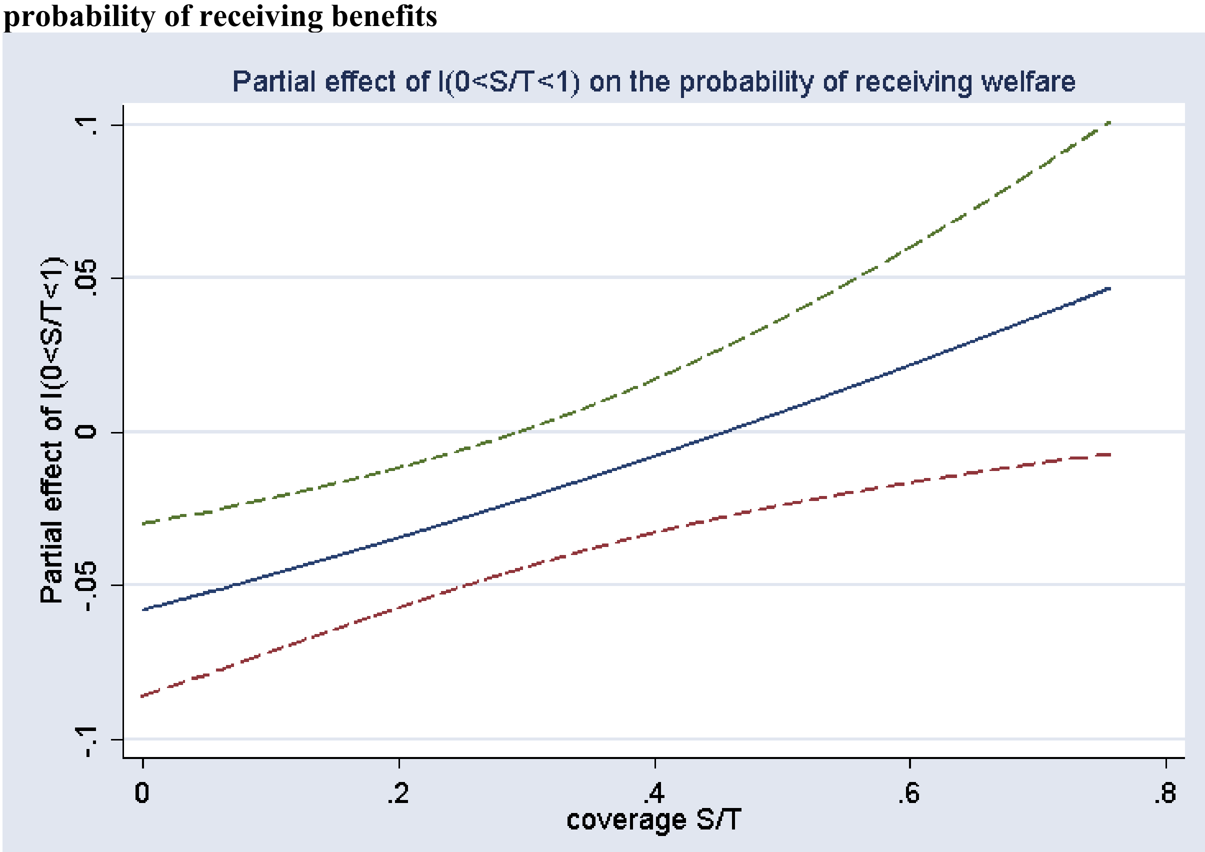
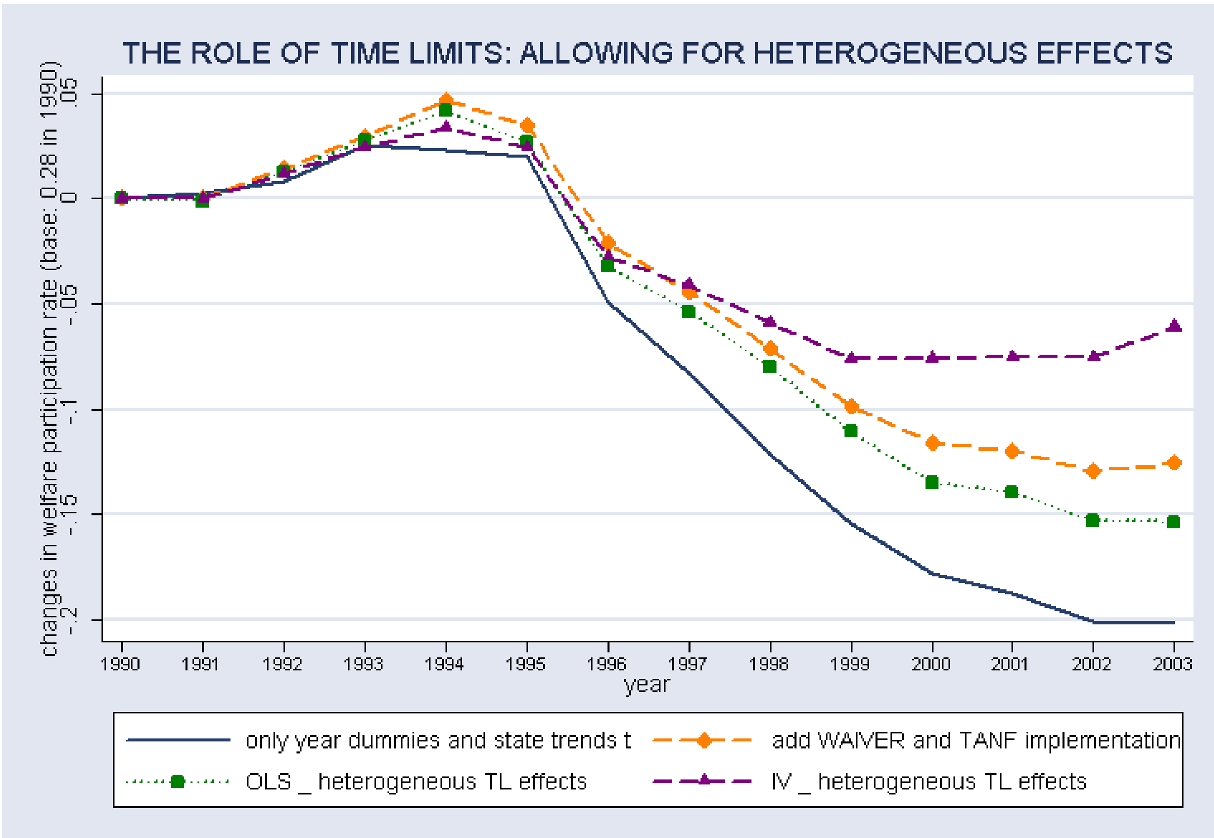


Figure 6: Estimated average time effects (year fixed effects and state specific linear and quadratic trends) from a regression of individual monthly welfare participation



Note*:* see Table A4 for details
Source: SIPP panels (1990, 1991, 1992, 1993, 1996, 2001)