A Re-Examination of the Impact of Welfare Reform on Health Insurance Among Less-Skilled Women*

John C. Ham
University of Southern California, Federal Reserve Bank of San Francisco and IZA

Lara Shore-Sheppard
Williams College and NBER

September 2007

There has been substantial interest in whether welfare reform affected the ability of women, particularly single mothers, to obtain health insurance coverage. We examine the assumptions used in most of the existing literature on welfare reform and health insurance for women—that either married mothers or single women without children can act as a valid control group for single mothers—and find that they are not consistent with the data. We thus estimate models of health insurance coverage for four different groups of less-skilled women, defined by marital status and presence of children, separately, using a rich specification of policy, labor market, and demographic variables along with state and year dummy variables. Our results provide some evidence that welfare reform was associated with a reduction in the probability of Medicaid coverage and a somewhat offsetting increase in private coverage among single mothers with less than a high school education. The evidence indicates that any effect was concentrated among minority women, particularly Hispanic immigrants.

* Prepared for the UKCPR Conference "Ten Years After: Evaluating the Long-Term Effects of Welfare Reform on Children, Families, Welfare, and Work." We gratefully acknowledge the support of the National Science Foundation. We would like to thank Anna Aizer, Sarah Hamersma, and James Ziliak for providing data on parental Medicaid expansions and estimated welfare system parameters. Sarah Hamersma, James Ziliak, Lucie Schmidt, Tara Watson, and conference participants provided useful comments on an earlier draft of the paper. We are grateful to Eileen Kopchik for her expert research assistance. Any opinions, findings, and conclusions or recommendations in this material are those of the authors and do not necessarily reflect the views of the National Science Foundation, the Federal Reserve Bank of San Francisco or the Federal Reserve System. We are responsible for any errors.

I. Introduction

The enactment of the Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA) in 1996 represented not only an “end to welfare as we know it” but the completion of a process begun in the late 1980s: the separation of Medicaid from cash assistance. Under the Aid to Families with Dependent Children (AFDC) program, receipt of Medicaid was automatic for welfare recipients, and to a large extent leaving AFDC meant losing not only cash assistance but Medicaid coverage. Recognizing the work disincentive inherent in this arrangement, the authors of PRWORA instructed states to set income standards for Medicaid that were related to AFDC standards in effect at the time of PRWORA's passage rather than to eligibility for the states' new Temporary Assistance to Needy Families (TANF) programs. Consequently, former welfare recipients would be permitted to keep their public health insurance even if they were not eligible for cash assistance. Despite this precaution, it is possible that welfare reform led to a loss of Medicaid coverage through a variety of channels. Indeed, case studies of thirteen states done by researchers at the Urban Institute (Holahan, Wiener, and Wallin 1998) showed declines in welfare caseloads were accompanied by declines in Medicaid enrollment. Women leaving welfare may have been unaware that they could keep their Medicaid coverage, potential applicants for welfare may have been deterred by the stricter welfare policies, missing Medicaid eligibility in the process, and in some states the processes of applying for welfare and applying for Medicaid were sufficiently de-linked that a welfare applicant would need to go to a different office and provide different documentation in order to qualify for Medicaid. Moreover, restrictions on receipt of public programs by newly arrived immigrants may have led even eligible immigrants to avoid Medicaid (the so-called “chilling hypothesis” (Fix and Passel

1999)).[1] The possible effects of welfare reform on health insurance coverage were not limited to the effect on public coverage. The intent of welfare reform was for family heads to leave the welfare rolls and begin work. Thus welfare reform had implications for private coverage–as mothers began to work they increased their chances of obtaining health insurance through an employer. To the extent that former or potential welfare recipients are unable to find jobs offering health insurance benefits, however, the impact on private coverage may be small. Similarly, if welfare reform affected the probability of marriage, the probability of receiving coverage through a spouse's employer may have increased, although evidence to date suggests any effect of welfare reform on marriage probabilities is likely to be small (see Blank 2002 for a review).

While it is clear that welfare reform had the potential to affect health insurance coverage, evaluating the impact of welfare reform on health insurance coverage is complicated by several factors. As has been noted throughout the literature on welfare reform, welfare reform did not take place in isolation, but rather occurred at a time of significant policy activity and macroeconomic change. Increases in the Earned Income Tax Credit (EITC) were raising the return to working, expansions in Medicaid eligibility and the introduction of the State Children's Health Insurance Program (SCHIP) had extended some access to public insurance to welfare nonrecipients, specifically to children and pregnant women, and the economy was growing rapidly. These changes also had implications for health insurance coverage. Increases in the EITC, like welfare reform, increase the likelihood of labor force participation among potential welfare recipients and thus potentially the likelihood of private coverage, again with the caveat

[1] For evidence on the "chilling effect" with respect to health outcomes, see the paper by Kalil

that low-wage jobs are much less likely to offer employer-sponsored coverage. Growth in the economy might also be expected to increase labor force attachment. Finally, after a period of minimal growth in the early 1990s, health care spending trends had resumed their upward movement, leading to increases in the price of private health insurance.

In this paper, we use data from the 1990-2001 Surveys of Income and Program Participation (SIPP) to examine the impact of welfare reform and other policies on health insurance coverage among less-skilled women–the group of women most likely to be affected by welfare reform—where we define "less-skilled" variously as having a high school education or less or as having less than a high school education. While this issue has not gone unstudied in the substantial literature on the effects of welfare reform, our paper makes several important contributions. First, we consider the question of identification of the welfare reform effect in greater detail than has been done in previous studies. The results from studies conducted thus far have been conflicting, and thus the question of identification is an important one. The approach taken in most previous nonexperimental studies is to identify the effect of welfare reform using state-level variation in the existence and timing of welfare reforms, or to modify this approach by introducing the possibility of an untreated comparison group Studies using this latter approach (including Kaestner and Kaushal 2003, Bitler, Gelbach, and Hoynes 2005, and Cawley, Schroeder, and Simon 2005) have typically chosen single mothers (or in the case of Bitler, Gelbach, and Hoynes single women more generally) as the group affected by welfare reform (the treatment group) and have used married mothers (or married women more generally in the case of Bitler, Gelbach, and Hoynes) or single women without children as a comparison group. We

---

and Ziol-Guest in this volume.

test whether this treatment group-comparison group strategy is supported by the data, and find that neither married mothers nor single women without children can be used as a comparison group for the treatment group of single women with children. We therefore estimate our models separately for four groups: single mothers, married mothers, single childless women, and married childless women.

Since we reject the untreated comparison group approach, we attempt to circumvent the problem of potential other factors affecting health insurance that change at the time of welfare reform by controlling for a richer set of covariates than has been done in previous studies. In particular, while various studies have controlled for Medicaid or SCHIP expansions or expansions in the EITC along with parameters of the state welfare program, the state minimum wage, and the state unemployment rate, they have not controlled for all of these factors simultaneously. We include not only these variables, but also two additional measures of the state's labor market: the 25th percentile of weekly wages and the male labor force participation rate. Moreover, we include a measure of health care costs at the state level to attempt to account for the potential role of rising health care costs in health insurance coverage probabilities. Despite our rich set of controls, unobservable changes across states occurring at the time of welfare reform that are correlated with welfare reform remain a possibility, so our results concerning the effects of welfare reform must be considered in light of this caveat.

Third, we focus on the effects of welfare reform among Hispanic women, a group that is of particular importance for evaluating welfare reform due to its overrepresentation among less-educated women and the fact that PRWORA targeted immigrants specifically. We estimate our models for the four groups defined by marital status and presence of children for all low-educated Hispanic women and then separately for immigrant and native Hispanics. Our results indicate that much of the observed effect of welfare reform on health insurance for single mothers is attributable to the effect on Hispanic immigrant women. Fourth, we consider the impact of persistence (or dynamics) in insurance coverage in addition to estimating cross-sectional relationships, primarily by including indicators for the lagged insurance state in our model. The problem with static models is that they assume that policy changes must have their full impact immediately. The specification including lagged insurance state allows the effects of policy changes through the natural dynamics of the model to grow over time as families become more familiar with them. We found that allowing for this type of dynamics was important in a related study (Ham and Shore-Sheppard 2005) of the effect of Medicaid expansions on children's coverage, and find that these dynamics are quite important here. Finally we investigate whether take-up and participation are more likely once programs have been in place for some time; again we found this was an issue for children in our related study.

II. Background

A. Trends in Insurance Coverage and Transition Rates

Despite all of the changes in policies and macroeconomic conditions occurring in the mid- to late-1990s, trends in the rate of health insurance coverage (overall and public and private) for less-skilled women show no obvious breaks. Figures 1-3 show estimates of coverage (public and private) or uninsured rates by month in the Survey of Income and Program Participation (SIPP) for the period 1986-2003 for four groups of less-skilled women (defined here as women with a high school degree or less) who were between 21 and 44 years old at the start of the respective SIPP panel: married women with children, married women without children, single women with children, and single women without children.   Each point in the figure is the mean rate for a month from a particular panel, calculated using the weight for that year in the panel. Because the SIPP is composed of overlapping panels, most months have data from more than one panel. The data are sparse in early 1990, 1995, and 2000, however, as those years were only covered by at most one panel (the 1990, 1993, and 1996 panels, respectively). Another caveat is that since the SIPP, like all panel data sets, suffers from attrition, means from later in each panel are likely to be more noisy as they are estimated from fewer observations. In addition to plotting the estimated rates, we plot the trend smoothed using a locally weighted regression smoothing method (lowess).

The trends in the fraction uninsured are broadly consistent for all four groups, remaining constant or falling slightly before 1990 but rising fairly steadily through the end of the period. The similarity in patterns of uninsurance masks fairly substantial differences in insurance trends by type of insurance, however. Single mothers experienced a marked decline in public (Medicaid) coverage beginning in late 1993, with their levels of private coverage remaining fairly constant, while single women without children experienced sharply declining levels of private coverage and little change in public coverage.[2] Married women (both mothers and non-mothers) also experienced steady declines in the level of private coverage over the entire period observed in the data and little change in coverage by Medicaid. These national-level trends are broadly consistent with the hypothesis of a negative effect of welfare reform on Medicaid for

[2]Women who report a disability or who report receiving Supplemental Security Income are excluded from the sample. Including these women increases the reported rate of Medicaid coverage for single women without children substantially.

single mothers and a limited positive effect on private insurance, although they are hardly definitive. In addition, the differences in the trends across groups even prior to welfare reform cast doubt on the use of either single childless women or married mothers as a comparison group. Given the differences in the trends before welfare reform, the assumption that the trends in coverage would have been the same across the groups in the absence of welfare reform seems unlikely to hold.

Turning to trends in the empirical transition rates (Figures 4-5), it appears that the rates of both coverage loss and coverage gain increased after 1990 for all four groups, although the estimated rates are quite noisy. (As transition rates are much lower than coverage levels, these two figures are plotted on different scales than Figures 1-3.) The fact that *both* insurance entry and exit rates increased suggests that the stability of insurance relationships declined over this period. This increase in insurance "churning" has also been observed for children (see Ham, Li, and Shore-Sheppard 2006), although for children the increased churning was not accompanied by significant overall coverage loss.

B. Literature Review

Not surprisingly, the impact of welfare reform on health insurance has been examined by several groups of researchers; however the results of these studies have been conflicting. In this review, we focus on the studies that are closest to ours in terms of goals and research methods, leaving aside papers that analyzed experimental welfare reforms and studies of welfare leaver behavior. One of the earliest studies to focus on the impact of welfare reform on the probability that individual women are covered by various types of health insurance (rather than focusing on the Medicaid caseload as in Ku and Garrett 2000) was the study by Kaestner and Kaushal

(2003). Using the March CPS, Kaestner and Kaushal relate public and private insurance coverage of individuals to measures of welfare reform, particularly the welfare caseload. To account for unobserved factors that may be correlated both with welfare reform and with individual insurance coverage, they estimate their models using single mothers as their "treatment" group and married mothers and unmarried childless women as two different "comparison" groups. We discuss this identification strategy further below, but for now we note that for married mothers or single non-mothers to serve as comparison groups for the treatment group of single mothers, it must be the case that welfare reform did not affect either married mothers or single non-mothers. For example, this strategy rules out the possibility of spillovers to "untreated" individuals via the labor market (thus increases in labor supply due to welfare reform cannot affect the labor market outcomes of women not directly affected by welfare reform) as well as ruling out the possibility that less-skilled women who have no children or are married might change their marriage, fertility, or labor market behavior in response to welfare reform. In the case of married mothers, this strategy assumes that those who get their health insurance through their husband have the same trends in coverage as single mothers. Kaestner and Kaushal find that decreases in the caseload are associated with reductions in Medicaid coverage but find no significant impact on private coverage. We do not focus on welfare caseloads in our paper, as it is likely that welfare reform may affect health insurance through other mechanisms besides the pure caseload effect. Moreover, the caseload picks up demand conditions as well as welfare program changes, so unless demand conditions have been controlled for sufficiently, it is not clear what the caseload variable is measuring.

Our approach is closest in style to the approaches of Bitler, Gelbach, and Hoynes (2005), DeLeire, Levine, and Levy (2006), and Cawley, Schroeder, and Simon (2005). All three of these studies estimate the relationship between welfare reform and the probability that a less-skilled woman has health insurance coverage (overall and of various types), and all three use variation in the timing of welfare waivers and TANF implementation across states to identify the effect. This is the general approach we take as well.

Bitler, Gelbach, and Hoynes use data on single and married women from the Behavioral Risk Factor Surveillance System (BRFSS) and estimate models for three groups: black women, Hispanic women, and women with a high school education or less. Because the BRFSS does not distinguish between types of insurance and it does not provide enough family structure information to permit the identification of which women in the sample have children, Bitler, Gelbach, and Hoynes simply estimate a model of whether a woman is covered by health insurance and distinguish women only by marital status and not by the presence of children. Since the SIPP allows us to distinguish women with children from those without children, we are able to test whether women with and without children can be pooled, and find that they cannot be. (The BRFSS does have detailed information on utilization of health care, however, and this is a focus of the Bitler, Gelbach, and Hoynes analysis that other researchers are unable to examine.) Bitler, Gelbach, and Hoynes find that only the sample of Hispanic women shows statistically significant evidence of an effect of welfare reform: both the waiver and TANF variables have negative coefficients, indicating a reduction in the probability of (overall) insurance coverage associated with reform. This finding of a significant effect of welfare reform for Hispanic women is consistent with the findings of significant reform effects for immigrants in Borjas (2003) and Kaushal and Kaestner (2005) since Hispanic women make up the majority of immigrants, although Borjas and Kaushal and Kaestner do not separately distinguish Hispanics from other immigrants. (These papers are discussed further below.[3])

While Bitler, Gelbach, and Hoynes do not explicitly consider the quality of the comparison group approach, they do discuss the difficulty of identifying an effect of reform when there is little variation in the timing of the reform across states. Like Bitler, Gelbach, and Hoynes, our data are at the monthly level, so we are able to take more advantage of timing variation than can studies using the annual CPS; nevertheless their concern about the meager variation in the time of state implementation is relevant for our study also.

The approach of DeLeire, Levine, and Levy (2006) is in many ways similar to that of Bitler, Gelbach, and Hoynes. The main differences between the two papers are the data set and sample used, and the identification strategy. DeLeire, Levine, and Levy use the CPS and construct a sample of women 18-64 with less than a high school education. They argue that the assumption that married women were unaffected by welfare reform is not plausible, although they do not test related assumptions, as we do. Nevertheless, our approach is quite similar to theirs, since they estimate models separately for various groups of women in the population, including single mothers, single childless women, and married mothers, as well as various racial sub-groups. In their sample of women with less than a high school education, they find no relationship between welfare reform and any kind of insurance for single mothers. They do, however, find that welfare waivers were associated with a reduction in public insurance for single childless women and an increase in private insurance (and therefore decrease in uninsurance) for Hispanic women of all family types. They also find evidence of an increase in

[3] See also the paper by Kalil and Ziol-Guest in this volume for an examination of the

private coverage and overall insurance coverage associated with TANF for single childless women and Hispanic women of all family types. Relative to DeLeire, Levine, and Levy, we make five main contributions. First, we test explicitly assumptions about whether women with different family structures can be used as comparison groups for the treatment group of interest: single women with children. Second, we estimate our models for more finely disaggregated groups, including native Hispanic and immigrant Hispanic single mothers. Third, we include a richer set of covariates, particularly variables measuring the impact of the EITC and the extent of parental Medicaid expansions. Including these covariates increases the likelihood that the effect we are labeling welfare reform is indeed an effect of that policy. Fourth, we use the SIPP monthly data, which not only allows us to consider dynamic issues in insurance choice, but also has some advantages with respect to accuracy and timing of reported health insurance data. Finally, we estimate models where individuals react to policy changes with a lag.

Cawley, Schroeder, and Simon (2005) also use the SIPP in their analysis. Like Kaestner and Kaushal (2003) and Bitler, Gelbach, and Hoynes (2005), Cawley, Schroeder, and Simon use a treatment-comparison methodology, using single mothers as the treatment group and married mothers as the comparison group. (As mentioned above, we show that this identifying assumption is rejected in the SIPP data.) In addition, Cawley, Schroeder, and Simon use individual fixed effects in their estimation of the effect of welfare reform on insurance status. While individual fixed effects have the advantage that they control for unobserved person-specific effects that may be correlated with the explanatory variables, such as a woman's taste for insurance or taste for work, including individual fixed effects makes the identification of the

---

impact of welfare reform on immigrant children's health taking health insurance as exogenous.

policy effect more challenging. In a fixed effects specification, identification of the effect of welfare reform comes from individuals who are observed both before and after welfare reform has occurred. Since we reject pooling married and single women and women with and without children, identification of fixed effect models comes from a relatively small number of women. Consequently, our preferred specification does not include individual fixed effects. As long as welfare reform is not correlated with *unobserved individual-specific characteristics* (controlling for state-specific characteristics), then omitting individual fixed effects will not lead to bias in our estimated coefficients. Nevertheless, we also estimate some models including individual fixed effects for comparability.

One important (observed) individual-specific characteristic that is correlated with welfare reform is immigrant status, since PRWORA denied newly arrived immigrants access to Medicaid and TANF. Three papers—Borjas (2003), Kaushal and Kaestner (2005), and Royer (2005)—have investigated the impact of welfare reform on health insurance specifically for subsets of the immigrant population—all immigrants and child immigrants in the case of Borjas, unmarried immigrant women and their children in the case of Kaushal and Kaestner, and low-income recent mother immigrants in the case of Royer. All three sets of authors use a treatment-comparison methodology. Borjas compares immigrants to natives in states that did and did not choose to offset PRWORA's immigrant provisions before and after the implementation of TANF. He finds increases in private insurance that more than compensated for the loss of public insurance for non-citizen immigrants relative to natives. While he attributes this effect to PRWORA, it is interesting to note that the descriptive statistics he provides indicate that large declines in Medicaid and increases in private coverage occurred prior to PRWORA's passage.

By distinguishing between welfare waivers and TANF implementation, we are able to investigate this phenomenon. In addition, we focus specifically on women, the primary targets of welfare reform, rather than all individuals, a group which is likely to be experiencing other changes besides the changes instigated by welfare reform. Moreover, while we present results both for immigrants and natives, we do not impose the restriction that trends in health insurance coverage in the absence of welfare reform would have been the same in the two groups, as Borjas does.

Kaushal and Kaestner (2005) do not assume similar trends for immigrants and natives; however, they assume that married women and men provide good comparison groups for unmarried women, an assumption that we show is unlikely to be the case, at least in the case of married women. Royer's (2005) identification strategy is similar to that of Borjas in using the differences by state in the existence of a "fill-in" program, though she compares post-TANF-enactment immigrants to pre-enactment immigrants rather than to natives. She finds evidence of a reduction in Medicaid coverage and health insurance overall due to TANF, although perhaps due to the small size of her sample, none of her coefficients are statistically significantly different from zero.

III. Data

As noted above, our primary data source is the Survey of Income and Program Participation (SIPP), a series of longitudinal data sets collected for a random sample of the U.S. population by the Census Bureau. The SIPP has several advantages over other data sets that have been used to examine health insurance and welfare reform. Unlike the March Current

Population Survey (CPS), which asks about health insurance coverage over the entire previous year, in each interview the SIPP asks about coverage by month for the previous four months. Since many researchers have noted that the CPS does not appear to be eliciting information about the entire previous year's coverage, the shorter recall period may lead to a greater degree of accuracy in reporting health insurance coverage status.[4] In addition, the information on family composition and demographics in the SIPP directly corresponds to the reference period for the insurance information. By contrast, the demographic and family composition information in the CPS is for the point at which the survey was taken, while the insurance information is for the previous year. While the data used by Bitler, Gelbach, and Hoynes, the BRFSS, does not suffer from these drawbacks, the BRFSS does not separately identify the source of coverage (public or private), nor does it include information on the presence of children in the household. Further, since SIPP is a panel data set, it also allows us to examine persistence in insurance coverage. As with any data set, the SIPP does have some flaws: the number of women in the data is smaller than in both the CPS and the BRFSS (although each woman potentially contributes many months of data), there are a few months in the period we study which were not covered by any panel, and like all panel data sets the SIPP suffers from attrition.[5]

The SIPP is collected in a series of panels, each one containing approximately 17,000

---

[4]Bennefield (1996) finds that health insurance coverage in the early 1990s is measured more accurately in the SIPP than in the CPS, due in part to the shorter recall period. See Swartz (1986) for a discussion of health insurance measurement issues in the CPS and Ham and Shore-Sheppard (2005) for a discussion of the SIPP versus the CPS and an attempt to reconcile results from the two data sets.

[5] We must assume that the attrition is random with respect to the estimated equations of interest—the incidence of public insurance, private insurance, and no insurance. This assumption can be relaxed in the fixed effects specification if the latent probability of attrition is person-specific and time-invariant.

households, on average. For ease of interviewing, the entire sample is randomly split into four rotation groups, and one rotation group is interviewed each month. Each rotation group in a SIPP panel is interviewed once every four months about employment and program participation during the previous four months (termed a wave). We use the 1990, 1991, 1992, 1993, 1996, and 2001 panels, which cover the period from October 1989 to December 2003 (we use the 1986, 1987, and 1988 panels as well in our illustration of trends; the 1989 panel is not used because it was ended after only three waves). The length of each SIPP panel varies: 32 months for the 1990 and 1991 panels, 40 months for the 1992 panel, 36 months for the 1993 panel, 48 months for the 1996 panel, and 36 months for the 2001 panel. A new panel is introduced each year or every few years, which often yields more than one panel with data covering a particular point in time.

Our base analysis sample is composed of women with a high school degree or less, who are between 21 and 44 the first month they are observed, and who live in states that are identified in the SIPP. (Through most of the panels, 41 states and the District of Columbia are identified, with that number increasing to 45 plus DC in the latest panel—the others are grouped for confidentiality.) Further, we only include women who do not report a disability or the receipt of Supplemental Security Income (SSI), since recipients of SSI are eligible for Medicaid and we want to avoid confounding the effects of changes in welfare policy with changes in SSI or the reported incidence of disability.[6] We chose high school or less education to capture women most likely to consider participating in welfare, although in many specifications we narrow our sample further to just women with less than a high school education. We further reduce the sample in

[6] In fact, there is evidence that welfare reform may have affected SSI participation (see

this way because women with a high school education are a fairly heterogeneous group and include many women with very low probabilities of ever participating in welfare. Our age range is chosen to minimize the possibility that an individual would be affected by the Medicaid expansions for children (which in some states extended to individuals over 18) and to capture women in their childbearing years (when they are most likely to participate in welfare). To address the possibility that our dynamic results may be biased by spurious transitions (for example when a woman is erroneously coded as having public insurance in a given period although in fact she does not have public insurance in that period nor in the preceding or following periods), we recode the data to eliminate any spells of one month duration except for those occurring at the beginning or end of the sample period.

Another measurement issue in the SIPP is that of "seam bias." Census Bureau researchers have shown that there are a disproportionate number of transitions in the fourth month of a wave (see, e.g. Young 1989, Marquis and Moore 1990). The approach to this problem that has been used in the past is to use only the fourth month of data from each wave, discarding the other three months. However, this approach has several disadvantages (outlined in Ham, Li, and Shore-Sheppard 2007). Instead, we use the data in monthly form and follow the suggestion of Ham, Li, and Shore-Sheppard (2007) to include a dummy variable for the fourth month in each equation, since they find that this procedure works remarkably well in that it mimics a much more complicated estimation scheme aimed at dealing with the seam bias.

Using the state of residence information available in the SIPP, we link information from other sources to our data, including welfare and welfare reform variables, the Medicaid

---

Schmidt and Sevak 2004).

eligibility limits applying to pregnant women, children, and to parents (for states with these expansions), state-level *Medicare* expenditure data, the EITC maximum credit applying to each family, the monthly unemployment rate in the state, the 25th percentile of real weekly earnings in the state and the male labor force participation rate (both measured monthly) and the minimum wage in the state. (We provide information about the sources of these variables in the Data Appendix.)

Means by marital status and the presence or absence of children for each of these variables are presented in Table 1. The four groups are quite similar in terms of most of the policy variables, indicating little systematic difference between groups in their states of residence (and the time periods observed). One clear difference is in the value of the maximum EITC, which is larger for women with children. Interestingly, the sample of single mothers has a somewhat lower likelihood of having a parental Medicaid expansion in their state. In terms of demographics, the married women samples tend to be older than the unmarried women samples, unmarried women (especially mothers) tend to be more likely to be black, and the two groups of mothers tend to have a greater proportion Hispanic than the two groups of non-mothers.

V. Empirical Model

We begin our empirical work with a straightforward static linear probability model of the form that has become standard in the literature on welfare reform's effects on health insurance (see, e.g. Bitler, Gelbach, and Hoynes 2005, eq. 1):

(1) $$I_{kist} = R_{st}\beta_k + L_{st}\alpha_k + X_{ist}\delta_k + \gamma_{ks} + \nu_{kt} + \varepsilon_{kist}\ ,\ k=p,m,n\ .$$

In this equation, I_{kist} refers to a dummy variable equal to 1 if woman i living in state s had insurance type k in month t (k = private (p), Medicaid (m), or uninsured (n)). Thus equation (1) is estimated separately for each type of insurance. R_{st} denotes dummy variables for the implementation of welfare reform, L_{st} is a vector of other characteristics of state policy and labor market, X_{ist} is a vector of characteristics of the woman and her family, γ_{sk} represents state fixed effects, ν_{kt} represents year fixed effects, and ε_{kist} is an error term that has an unrestricted covariance across observations from state s (i.e. we cluster on state). The vector X_{ist} includes the woman's age, race, ethnicity, education, marital status, and the number of children in her family.

Again following the existing literature, we represent welfare reform with a dummy variable for the presence of a major statewide waiver and a dummy variable for the implementation of TANF. As our data are monthly, these dummy variables "turn on" in the month of implementation, rather than merely indicating year of implementation. The waivers were implemented in various states over the period 1993-1996, and "turn off" once the state has implemented its TANF program (the earliest states implemented TANF in 1996, but most states' implementations occurred in 1997). The waivers and TANF are expected to be negatively associated with the probability of Medicaid participation but may be positively associated with the probability of having private coverage, so that the prediction for overall insurance coverage is ambiguous.

As discussed earlier, many changes were taking place over the time period we study in addition to welfare reform. These changes are captured in L_{st}. To measure the generosity of the welfare system in a state, we include the estimated guarantee for a family of three in a state and the estimated tax rate on earned income. These measures, which come from Ziliak (2007) following the methods of Fraker, Moffitt, and Wolf (1985) and McKinnish, Sanders, and Smith

(1999), arguably more accurately portray the actual welfare circumstances facing the average recipient in a state than the statutory rates.[7] More generous welfare benefits would be expected to increase the probability of Medicaid participation (particularly pre-TANF) but would be expected to be negatively associated with private insurance since the likelihood of working is expected to be lower in high benefit states. Along with welfare reform, the mid- to late-1990s saw a substantial increase in the EITC. The federal government increased the phase-in rate (the negative income tax rate) substantially, and many states followed suit with their own earned income credits. Over the period we study, the combined average federal-state phase-in rate more than tripled. As a result of this change and changes in the phase-out rates, the maximum credit also rose substantially. We include the maximum credit in the state in our models. Expansions of the EITC have ambiguous effects on insurance coverage–they increase the return to working, which increases the probability of having private coverage, but reduce the incentive to be on welfare, which reduces the probability of public insurance. Of course, to the extent that the jobs obtained do not offer insurance as a benefit, there would be little effect on private insurance coverage except via an income effect: conditional on working family incomes will be higher, which should increase the demand for private insurance purchased in the non-group market.

Prior to and following welfare reform, there were significant changes in eligibility for the Medicaid program and the introduction of SCHIP: throughout the 1990s, pregnant women with incomes below 133 percent of the poverty line (or higher in some states) were eligible for Medicaid, while some states took advantage of the flexibility afforded by waivers and PRWORA to fund Medicaid coverage for parents with higher incomes than had previously been eligible.

[7]We also tried using the statutory maximum benefit for a family of 3 and found similar

(See Aizer and Grogger 2003 for a review of the changes.) In addition, income limits for older teenagers were raised substantially, particularly after the introduction of SCHIP. To capture these changes in public insurance eligibility we include three variables: the income eligibility limit for pregnant women in the state, the income eligibility limit under any parental expansion, and the income limit for 18-year-olds. Since increasing the limit for older children was an important part of SCHIP implementation in most states, we view this last variable as a way of measuring a state's commitment to SCHIP that is superior to the commonly used dummy variable for whether the state had an SCHIP plan since the latter variable exhibits very little variation across states or over time.

Along with changes in Medicaid eligibility, another significant feature of the health insurance landscape over the period we study is increasing health care costs. Since higher health care costs are likely to be reflected in higher prices for private health insurance, rising costs may spur losses in private coverage. As it is difficult to measure health care costs at the state level explicitly, we use the average level of *Medicare* spending per enrollee in a state as our proxy for the cost of health care. Finally, L_{st} includes four characteristics of the labor market in the state: the minimum wage, the monthly unemployment rate, the 25th percentile of the weekly wage and the male labor force participation rate (both measured monthly).

One potential drawback of the static model is that it implicitly assumes that a woman makes a new decision each period about whether or not to obtain public or private insurance, and that this decision is does not depend (in a structural sense) on last period's decision. However, insurance outcomes are closely related to job outcomes—families often gain access to private

---

results.

insurance when members find a job, and can lose private insurance when they are laid off. Since there is substantial persistence in labor market histories of disadvantaged women (see, e.g., Eberwein, Ham and LaLonde 1997, Chay and Hyslop 1998, and Ham, Li and Shore-Sheppard 2007), we would expect this persistence to carry over into insurance determination. Also, the static model does not incorporate the notion of fixed costs: a woman on Medicaid has already paid the fixed costs of enrolling and is more likely to be on Medicaid next month.

A simple dynamic model is obtained by adding several (three) lags of the dependent variable to (1):

(2) $$I_{kist} = \sum_{l=1}^{3} \lambda_l I_{kist-l} + R_{st}\beta + L_{st}\alpha + X_{ist}\delta + \gamma_s + \nu_t + \varepsilon_{ist}\text{ , } k{=}p{,}m{,}n$$

where $I_{kist\text{-}l}$ equals one if the individual had insurance type *k* *l* months ago and zero otherwise, and for notational simplicity we drop the '*k*' subscript on the coefficients and errors. As we note below, this model allows the short-run, medium-run, and long-run effects of variables affecting insurance status to differ substantially. We expect the lagged dependent variables to be correlated with the error term and treat the lagged dependent variables as endogenous. To do this, we use the three lags of all time-varying variables in the equation as instruments. Note that unlike a standard time series dynamic model, we assume a short panel length T, and our asymptotic distributions for the parameters are obtained by letting number of individuals $I \to \infty$ for fixed T. Thus the standard concerns about stability do not apply here.[8] Moreover, we feel that the short-run and intermediate run policy effects in (5) below are more relevant than a long-run

[8] For this equation, the standard time series requirement is that all of the characteristic roots of $1 - \lambda_1 w - \lambda_2 w^2 - \lambda_3 w^3 = 0$ lie outside the unit interval. If we set $\lambda_2 = \lambda_3 = 0$ (as we do below) then this reduces to the more familiar condition $|\lambda_1|$<1, and in fact only one of our

policy effect calculated as if $T \to \infty$ given how quickly policy has been changing over the last decade.[9]

It turns out that we cannot reject the null hypothesis that $\lambda_2 = \lambda_3 = 0$ in (2), and thus our model reduces to

(3) $I_{kist} = \lambda_1 I_{kist-1} + R_{st}\beta + L_{st}\alpha + X_{ist}\delta + \gamma_s + \nu_t + \varepsilon_{ist}$, *k=p,m,n.*

The immediate impact of a change in one of the explanatory variables is given by its coefficient. However, permanent changes today in an independent variable will have additional effects in the future due to the presence of the lagged dependent variable. To see this, note that if we substitute out once for I_{kist-l} in (3) we have

(4)
$$\begin{aligned} I_{kist} = {} & \lambda_1 I_{kist-2} + (R_{st} + \lambda_1 R_{st-1})\beta + (L_{st} + \lambda_1 L_{st-1})\alpha + (X_{ist} + \lambda_1 X_{ist-1})\delta \\ & + \gamma_s(1+\lambda_1) + (\nu_t + \lambda_1 \nu_{t-1}) + (\varepsilon_{ist} + \lambda_1 \varepsilon_{ist-1}). \end{aligned} .$$

Thus the estimated effect of a permanent change in the *j*th component of R_{st-1},(i.e. R_{jst-1}), on I_{kist} is $(1+\hat{\lambda}_1)\hat{\beta}_j$. By repeated substitution it is straightforward to show that the estimated medium-run effect L periods in the future of a permanent change in R_{jst-L} on I_{kist} is

(5)
$$\hat{P}\hat{E}_L = \sum_{l=0}^{L} (\hat{\lambda}_1)^l \hat{\beta}_j \ .$$

We estimate these effects separately for implementing waivers and implementing TANF. For each policy change we consider the effect for L= 6, 12, 24, (i.e. the effects 6, 12, and 24 months

---

estimates of λ_1 violates this time series stability condition.

[9] In an earlier version of the paper, we also estimated a somewhat richer specification of the dynamic model by adding the lagged value of an alternative insurance type (other than the current type). However, we found we were never able to reject the null hypothesis that the coefficient on the lag of the alternative type was zero, so we have eliminated this model in the interest of preserving space.

after the change). We calculate the standard error for the policy effect in (5) using the Delta method. To do so we take the square root of

(6)

$$V(P\hat{E}_L) = [\partial P\hat{E}_L / \partial\hat{\lambda}_1, \partial P\hat{E}_L / \partial\hat{\beta}_j]\, V\, [\partial P\hat{E}_L / \partial\hat{\lambda}_1, \partial P\hat{E}_L / \partial\hat{\beta}_j]^{transpose}, \quad \text{where}$$

$$V = \begin{bmatrix} V(\hat{\lambda}_1) & Cov(\hat{\lambda}_1, \hat{\beta}_j) \\ Cov(\hat{\lambda}_1, \hat{\beta}_j) & V(\hat{\beta}_j) \end{bmatrix}.$$

To implement (6) note that

(7) $$\partial P\hat{E}_L / \partial\hat{\lambda}_1 = \sum_{l=1}^{L} l\hat{\lambda}_1^{l-1}\hat{\beta}_j \text{ and } \partial P\hat{E}_L / \partial\hat{\beta}_j = \sum_{l=0}^{L} \hat{\lambda}_1^{l}.$$

In addition to dynamics at the individual level, we also consider the possibility that it may take some time for individuals to respond to the welfare reforms. This may be because local implementation of policies may be delayed following state passage, or because adjustments involving the labor market are rarely instantaneous. Women on welfare who are encouraged by the reform to find work may need time to find jobs, implying that they would continue on Medicaid after reform. On the other hand, to the extent that the policy's arrival was known ahead of time, women likely to be affected may have begun the adjustment process prior to the actual implementation of the law, reducing any delays. We examine this issue by replacing the simple reform implementation dummy variables in R_{ist} with a set of dummy variables indicating the time since the reform was implemented.

In all of our models, we estimate each equation separately by group (married and single women with and without children), allowing each group to have its own set of coefficients. However, we begin by assessing the comparison group identification strategy used by several previous researchers. In terms of equation (1), this strategy involves pooling the comparison and treatment group data, including a dummy for treatment group, and then interacting this dummy with the welfare reform variables in R_{st} . The coefficient on this interaction is then interpreted as giving the marginal effect of welfare reform for the treatment group. We discuss this approach below before moving on to estimation of the models separately by group.

VI. Assessing the Comparison Group Identification Strategy

As noted in the literature review, several papers (including Kaestner and Kaushal 2003, Bitler, Gelbach, and Hoynes 2005, and Cawley, Schroeder, and Simon 2005) suggest both that the effect of welfare reform is felt solely by less-educated single mothers, and that a comparison group (either less-educated married mothers or less-educated single childless women) can be used to eliminate the influence of omitted variables varying by state and year that are correlated with welfare reform and that affect health insurance coverage of less-educated women. In order for this approach to eliminate the influence of these omitted variables successfully, it must be the case that both the "treatment group" and the "comparison group" would have had the same state-level trends in insurance in the absence of the reform. This assumption is untestable. However, it is possible to test the assumptions that the state effects are the same for both groups and that the year effects are the same for both groups. If these assumptions are rejected, it casts doubt on the identifying assumption made in much of the literature that the state-by-year effects would be the same for both groups. In addition to providing indirect evidence about the identifying assumption, including interactions of state and year with treatment group status provides direct evidence about whether the *specification* used in the previous research, which constrains the different groups to have the same time and state effects, is consistent with the data.

We show the results from relaxing the constraints on time and state effects in Tables 2a and 2b. The top panel in each table gives the results for no insurance, the second panel gives the results for Medicaid, and the bottom panel gives estimates for private coverage. We use a different comparison group in each table. In Table 2a we show the results using all mothers, where the treatment group is single mothers and the comparison group is married mothers. The numbers presented are the coefficients on the welfare reform variables and their interactions, with the coefficients on the interactions between presence of welfare reform and the treatment group showing the treatment effects given the assumption being tested. (Other variables in the model include all of the demographic, policy, and labor market variables discussed previously.) For comparison, column 1 gives the fully pooled model, which is easily rejected. The model corresponding to the model used in the previous literature is in column 2 of each panel. According to this model, the implementation of TANF was associated with an increase in the likelihood that a single mother was uninsured, a reduction in the probability of having Medicaid, and an increase in private coverage, while the introduction of welfare waivers shows little effect.[10] However, this model is decisively rejected in the data in favor of models that allow single and married mothers to have different time effects, different state effects, or both. Including different time and state effects in the insurance status models typically reduces the magnitude of the welfare reform coefficients to such an extent that they are rarely statistically distinguishable from zero.

---

[10] The coefficients in the private and Medicaid regressions do not add up to 1 minus the coefficient in the uninsured regression because some women report having both types of insurance in a single month.

We repeat this exercise assuming that single childless women are the comparison group. (Single mothers are again the treatment group.) Here as well, the specifications restricting the two groups to have equal year and state effects are decisively rejected in favor of allowing these effects to differ. Finally, since the presence of welfare reform itself might plausibly have led the year and state effects to differ across groups, we estimate similar models of health insurance for the four groups using only data prior to welfare reform (results not shown). We again find that the constraints of either equal year or state effects are decisively rejected in the data. Consequently, we conclude that at least in this context, models of health insurance should be estimated separately for the four groups of women in the data: married mothers, married non-mothers, single mothers, and single non-mothers.[11] Consequently, we take this approach throughout the remainder of the paper.

VII. Results

A. Static OLS models

Our results from estimation of equation (1) for the four groups of women are presented in Tables 3a-3d. We find that there is at best only weak evidence of an effect of welfare reform for women with a high school education or less. While none of the coefficients on waivers or TANF are statistically different from 0 at conventional levels, we find a negative association between

[11] Of course, it is possible that a less flexible specification—such as permitting each group to have its own state, year, and reform effects but restricting the coefficients on the other variables to be the same—would still yield consistent parameter estimates. We conducted extensive tests of these "partially pooled" models, and found the implied constraints on the coefficients to be decisively rejected in virtually every case. Also, we could have considered married women with children as a comparison group for completeness, but omit it since this group is not used as a comparison group in the literature (for good reasons).

the implementation of welfare waivers and Medicaid participation for single mothers that has a *p*-value of 0.102. Other policies show greater evidence of an impact, especially for married mothers.[12] For these women, higher levels of the EITC are associated with lower probability of Medicaid participation, higher probability of private coverage, and higher probability of coverage overall. These estimates are consistent with the EITC encouraging greater attachment to the labor force, allowing women to gain private health insurance (on their own or from their spouses). Similarly, higher levels of parental Medicaid expansions are associated with higher probability of Medicaid participation and lower probability of being uninsured for married mothers.

Along with explicit policies, there is evidence that labor market conditions and health care costs play a role in coverage levels. In particular, better labor market conditions as measured by lower unemployment rates or higher levels of the 25th percentile of weekly earnings are associated with higher private insurance probabilities and lower Medicaid probabilities for several of the groups. For married mothers only, we find that higher Medicare spending levels in the state are associated with lower probability of private coverage. Finally, the demographic variables are often highly significant. Across all four groups, age and education are positively associated with private insurance and negatively associated with public insurance, while having more children (when applicable), or being black or Hispanic, are negatively associated with private insurance and positively associated with public insurance.

Since there is a clear correlation between education and insurance outcomes, and since

[12] We recognize that we need to provide more significant digits in some cases. While time constraints prevented us from making this change in this draft, the results do allow one to see the sign and significance of the variable.

women with a high school education are fairly heterogeneous, we next examine whether the results change when we restrict our samples to women with less than a high school education.[13] The coefficients on the variables other than the main variables of interest change little, though fewer of them are statistically significant—not surprising given the fact that there are far fewer women with less than a high school degree. Thus to save space we report only the coefficients on the welfare reform variables. The coefficients on these variables are larger (in absolute value) in several cases than they are when women with a high school degree are included. For the first time we find some evidence (albeit fairly imprecisely measured) that implementation of welfare reform is associated with changes in insurance coverage—in particular, reductions in Medicaid and increases in private coverage for single mothers.

Interestingly, these results are broadly consistent with the findings of Bitler, Gelbach, and Hoynes (2005, Table 6) and Cawley, Schroeder, and Simon (2005, Table 2) despite our finding that their assumption of similar trends for married and single mothers in the absence of welfare reform is unlikely to hold in the data, and despite some differences in specification and sample composition. Our results are not as consistent with the results of DeLeire, Levine, and Levy (2006) (hereafter DLL). Notably, comparing our results in Table 4 to the results in the first row of their Table 5, the results for married mothers are similar, but the results for unmarried women differ in several ways. While we find a statistically significant reduction in the probability of having Medicaid associated with the presence of a welfare waiver for single mothers, they find a positive and insignificant coefficient. Our results for TANF and for private insurance for single

[13] The argument that high school educated women should not necessarily be included in the definition of low-skilled women likely to be affected by welfare reform is made persuasively by DeLeire, Levine, and Levy (2006).

mothers are more similar, with substantially overlapping confidence intervals. However, our results for unmarried non-mothers again differ, particularly for private insurance. DLL find a positive and statistically significant coefficient on TANF, a result that essentially drives their overall conclusion of a positive effect of welfare reform on insurance coverage. We attempted to replicate this finding by changing our sample to match theirs more closely, without success. We were able to replicate their finding of a negative and statistically significant association between welfare waivers and Medicaid for single childless women by including women reporting disability in our sample, but even with that sample change and increasing the age range to match theirs (18-64) we were unable to find any evidence of a substantial positive association between TANF and private insurance for single childless women. One obvious possible explanation for the different results is our use of different data sets, in which the insurance variables are measured in different ways. Since SIPP is a monthly data set, our insurance variable represents whether a woman reports having insurance in a given month. DLL use the March supplement to the CPS, where the reference period for insurance coverage is much less clear. While a detailed comparison of the implications of the different data sets is beyond the scope of this paper, we note that we did carry out such a comparison for children's health insurance in Ham and Shore-Sheppard (2005). There we concluded that it was possible to explain some, but not all, of the differences between results based on SIPP and results based on the CPS, and that CPS respondents seemed to be indicating whether they had insurance at the end of the previous calendar year. [14]

One striking difference between some of the results in the existing literature occurs for

[14] Even if seam bias were to affect all individuals in the SIPP, the reported insurance

Hispanic women. While all of the authors who examine Hispanic women separately find large effects of welfare reform for Hispanic women, the nature of these effects differ. For example, Bitler, Gelbach, and Hoynes (2005) find evidence that waivers and TANF led to sizeable losses in insurance coverage for Hispanic women, while DeLeire, Levine, and Levy (2006) find the opposite. In addition, the literature on the immigrant population has found similarly conflicting results. We estimate our model for all Hispanic women with less than a high school education, and then further divide this population into immigrant and native-born. These estimates are reported in Table 5. We find evidence that both the welfare waivers and TANF were associated with reductions in Medicaid coverage and increases in private coverage for all unmarried Hispanic mothers with less than a high school education, although the standard errors are large enough that the estimates are not always statistically significant at standard confidence levels. Consistent with the literature on the impact of welfare reform for immigrant women, when we divide the sample of Hispanic women into immigrants and native-born, we find the effects to be largely concentrated among immigrant women. This evidence is consistent with a so-called "chilling effect," with immigrants being dissuaded from participating in public programs even if they were eligible for the programs. However, it is striking that the effects on insurance coverage are similar for both the waivers and TANF, despite the fact that only TANF singled out immigrants for differential treatment. There are several possible explanations for the similarity between the waiver and TANF results. Immigrants may have been more responsive to welfare reform provisions in general (perhaps out of concern for being labeled a public charge), or conditions in low-skill immigrant labor markets may have been improving faster than conditions

period would still be more clearly defined and at the sub-annual level.

in labor markets dominated by low-skill natives over this time period, so that what we have identified is not solely an effect of welfare reform, but welfare reform combined with positive conditions in markets employing immigrant labor that are not captured by the three (state-specific, monthly) labor market variables we include in the model. Although we are unable to distinguish between the explanations using our data, our results highlight the importance of accounting for the presence of welfare waivers in examinations of TANF. Moreover, while the evidence for a reduction in Medicaid coverage and a (smaller) increase in private coverage among low-skill immigrant single mothers that is associated with welfare reform is fairly compelling, we are reluctant to attribute these changes solely to the exclusion of immigrants in PRWORA.

For comparison, we estimate our models for white (non-Hispanic) and black (non-Hispanic) women with less than a high school education (Table 6). Aside from two somewhat puzzling results—a *reduction* in private coverage associated with waiver implementation for unmarried mothers and an increase in private coverage associated with TANF for *married* mothers—we find little evidence of welfare reform effects for white women. For black women, we found evidence of Medicaid reductions and private coverage increases for several of the groups, although only the Medicaid results for married mothers and the private coverage results for married non-mothers and unmarried mothers are statistically distinguishable from zero. Thus significant evidence of welfare reform impacts for the group theoretically most likely to have been affected—single mothers—is found primarily, if not exclusively, among less-educated Hispanic immigrant women.[15]

[15] The sizeable effect on Medicaid for black married mothers is striking, but difficult to

B. Static models with individual fixed effects

As a robustness check, we estimate our model including individual fixed effects. The use of individual fixed effects places considerable demands on the data; in particular, identification depends on women who are present both before and after welfare reform, the number of whom is quite small for detailed subgroups in the SIPP. As a result, we use the entire sample of women with less than a high school education rather than restricting our sample to Hispanic women only. In order to restrict our attention to the effects of welfare reform, we removed from our sample women who changed marital status or motherhood status or state of residence.[16] The results from the fixed effects models are reported in Table 7. It is immediately apparent that while most results are similar to the results from Table 4, this is not the case for the unmarried mothers. The statistically significant evidence of both a negative effect on Medicaid coverage and a positive effect on private coverage disappears, and in fact the only coefficient that is statistically different from zero is the estimated effect of welfare waivers on private coverage, but that estimate is unexpectedly negative. We see several possibilities to explain the differences in results between the OLS and fixed effects models. Obviously, one possibility is that the fixed effect model is the true model, and in that case one would conclude that welfare reform had essentially no effect on health insurance for low-educated women. However, we are hesitant to draw that conclusion based on the evidence presented here. As identification in fixed effects

---

explain. Perhaps black married mothers were more likely than other mothers to be receiving AFDC through the Unemployed Parent program, or were more dissuaded by welfare reform from enrolling in Medicaid's pregnancy or parental expansion provisions.

[16] We checked whether our results differed if we did not make these exclusions, and found they did not. In addition, estimating the cross-sectional model on this restricted sample yielded results that differed by 0.015 or less from the results in Table 4, and with similar levels of statistical significance.

models comes from women who are observed in both regimes, the identification here is based on relatively small samples of women—for example only 376 single mothers experience a change in the presence of a waiver. Thus it may be the case that the fixed effects estimation involves substantial small sample bias. Another concern given the small sample size is that the women observed in both regimes are unrepresentative in some way, so that the models utilizing variation across women, as well the variation within a woman's history, better measure the true effect. Unfortunately, we are unable to resolve this question with our data. Given the small sample size and oddly signed results in several of the fixed effects models, we find the OLS results more compelling, but we need to be cautious in drawing strong conclusions from them given the lack of agreement with the fixed effects results.

C. Dynamic models

Turning to the models that include a lagged dependent variable (Table 8), it is immediately apparent that there is a substantial amount of persistence in insurance status. The coefficients on the lagged dependent variable are all 0.7 or higher. In fact, in many models including the lagged dependent variable accounts for virtually all of the variation in insurance status, leaving little variation for the policy variables to explain. This is evident in the small size and lack of statistical significance of the coefficients on the welfare reform variables. Nevertheless, we calculate the predicted effects of the reforms 6, 12, and 24 months after their implementation to calculate medium run effects (bottom panel of Table 8). While few of the predicted effects are statistically distinguishable from zero, it is useful to note their increasing size with time. (Of course, the latter is true by definition given the existence of a nonzero, positive coefficient on the lagged dependent variable.)

D. Models with a Delay in Response to Reform Implementation

Finally, in Table 9 we return to the static model, but let the effect of a policy change depend on how long the change has been in place (for the reasons discussed in Section V). Specifically, we parameterize the waiver and TANF variables with groups of dummies for months since implementation. This model places considerable demands on the data, and in over half of the cases we are unable to reject the null hypothesis that the welfare reform variables have constant effects over time. Moreover, there is no clear pattern to the results and the coefficients are rarely statistically distinguishable from zero. (Given the 96 coefficients one would expect about 10 coefficients to be statistically significant at the 10% level simply as a result of sampling error.)

VIII. Conclusions

Since the goal of welfare reform was to move welfare (and hence Medicaid) recipients off welfare to employment, there has been substantial interest in whether reform affected women's ability to obtain health insurance coverage. In this paper we examine two assumptions that have been made in most previous investigations of the effect of welfare reform's implementation on the insurance status of single mothers. Specifically, we ask whether either married mothers or single women without children can act as a valid comparison group for single mothers. We find that the answer to this question is a resounding no.

We then estimate our models on the four different groups of less-skilled women separately. Since there is no comparison group available, we include a rich specification of demographic, policy, and labor market variables along with state and year dummy variables.

Nevertheless, we interpret our results cautiously, as the many changes occurring at the time of welfare reform complicate the identification of welfare reform substantially. Overall, our results provide some evidence that welfare reform was associated with a reduction in the probability of Medicaid coverage and a somewhat, though not entirely, offsetting increase in private coverage among single mothers with less than a high school education. The evidence indicates that any effect was concentrated among minority women, particularly Hispanic immigrants.

Estimating a simple dynamic model indicates that insurance status is quite persistent, particularly in the case of private insurance coverage. These findings suggest that it would be worthwhile to consider a richer dynamic specification, such as a multi-spell, multi-state duration model of health insurance, as we considered for children's health insurance in Ham, Li and Shore-Sheppard (2006). However, we find no evidence that individuals respond to the implementation of different aspects of welfare reform with a lag.

Our evidence of a limited effect on Medicaid and private insurance for some groups is largely consistent with the findings of Bitler, Gelbach, and Hoynes (2005) and Cawley, Schroeder, and Simon (2005), although we show that the identifying assumptions used to obtain those findings do not hold in the SIPP data. Unlike DeLeire, Levine, and Levy (2006), who find little effect of welfare reform for single mothers but some effects elsewhere, we find the effect of welfare reform to be largely but not exclusively concentrated in the single mother population, and concentrated further among Hispanic immigrant single mothers. Consistent with DeLeire, Levine, and Levy, however, we find stronger effects when we concentrate on the population with less than a high school education. Our results are broadly consistent with the results from the literature on the effect of welfare reform on immigrants, although we do not find the greater than

100 percent "crowd-in" that Borjas (2003) finds when he compares all immigrants to all natives. Moreover, although the welfare waivers did not have an explicitly immigrant focus, we find that the effects of the waivers and TANF were similar for immigrant single mothers, suggesting that the greater responsiveness of immigrants to welfare reform can be attributed to other factors besides the immigrant provisions of PRWORA. Further research on immigrant health insurance is necessary to determine what those factors might be.

References

Aizer, Anna and Jeffrey Grogger. 2003. "Parental Medicaid Expansions and Health Insurance Coverage." NBER Working Paper 9907.

Bennefield, R.L, 1996. "A comparative analysis of health insurance coverage estimates: data from CPS and SIPP." U.S. Bureau of the Census, SIPP Working Paper No. 218.

Bitler, Marianne P., Jonah B. Gelbach, and Hilary W. Hoynes. 2005. “Welfare Reform and Health.” *Journal of Human Resources* 40(2): 309-334.

Blank, Rebecca M. 2002. “Evaluating Welfare Reform in the United States.” *Journal of Economic Literature* 40(4):1105-1166.

Borjas, George. 2003. "Welfare Reform, Labor Supply, and Health Insurance in the Immigrant Population," *Journal of Health Economics* 22(6):933-58.

Cawley, John H., Mathis Schroeder, and Kosali I. Simon. 2005. “Welfare Reform and the Health Insurance Coverage of Women and Children.” *Forum for Health Economics & Policy*, Forum: Frontiers in Health Policy Research vol. 8, article 5. http://www.bepress.com/fhep/8/5.

Chay, Kenneth Y. and Dean R. Hyslop. 1998. “Identification and Estimation of Dynamic Binary Response Panel Data Models: Empirical Evidence using Alternative Approaches.” Center for Labor Economics, UC Berkeley, Working Paper No. 5.

DeLeire, Thomas, Judith Levine, and Helen Levy. 2006. “Is Welfare Reform Responsible for Low-Skilled Women’s Declining Health Insurance Coverage in the 1990s?” *Journal of Human Resources*, 41(3): 495-528.

Eberwein, Curtis, John Ham and Robert LaLonde. 1997. “The Impact of Being Offered and Receiving Classroom Training on the Employment Histories of Disadvantaged Women: Evidence From Experimental Data.” *The Review of Economic Studies*, 1997, 64(4): 655-682.

Fix, Michael and Jeffrey S. Passel. 1999. “Trends in Noncitizens’ and Citizens’ Use of Public Benefits Following Welfare Reform: 1994–97.” Research report, March. Washington, DC: The Urban Institute. Available online at http://www.urban.org/url.cfm?ID=408086.

Fraker, Thomas, Robert Moffitt, and Douglas Wolf. 1985. “Effective Tax Rates and Guarantees in the AFDC Program, 1967–1982.” *Journal of Human Resources* 20(2): 251–263.

Ham, John C., Xianghong Li, and Lara D. Shore-Sheppard. 2007. “Analyzing Movements over Time in Employment Status and Welfare Participation while Controlling for Seam Bias using SIPP.” University of Southern California working paper, August.

_____, _____, and _____. 2006. "The Dynamics of Children's Health Insurance, 1986-1999." Williams College working paper, July.

Ham, John C. and Lara D. Shore-Sheppard. 2005. “The Effect of Medicaid Expansions for Low-Income Children on Medicaid Participation and Private Insurance Coverage: Evidence from the SIPP.” *Journal of Public Economics* 89(1): 57-83.

Holahan, John, Joshua Wiener, and Susan Wallin. 1998. “Health Policy for the Low-Income Population: Major Findings from the Assessing the New Federalism Case Studies.”Assessing the New Federalism Occasional Paper No. 18, Urban Institute. Washington, DC: Urban Institute.

Kaestner, Robert and Neeraj Kaushal. 2003. “Welfare Reform and Health Insurance Coverage of Low Income Families.” *Journal of Health Economics* 22(6):959–81.

Kalil, Ariel and Kathleen M. Ziol-Guest. 2007. “Welfare Reform and Health among the Children of Immigrants.” Working paper.

Kaushal, Neeraj and Robert Kaestner. 2005. “Welfare Reform and Health Insurance of Immigrants.” *Health Services Research* 40(3): 697-722.

Ku, Leighton and Bowen Garrett. 2000. "How Welfare Reform and Economic Factors Affected Medicaid Participation: 1984-1996." Discussion Paper 00-01, Urban Institute. Washington, DC: Urban Institute.

Marquis, Kent H. and Jeffrey C. Moore. 1990. “Measurement Errors in the Survey of Income and Program Participation (SIPP) Program Reports.” 1990 Annual Research Conference Proceedings, Washington, D.C.: U.S. Bureau of the Census.

McKinnish, Terra, Seth Sanders, and Jeffrey Smith. 1999. “Estimates of Effective Guarantees and Tax Rates in the AFDC Program for the Post-OBRA Period.” *Journal of Human Resources* 34(2): 312–345.

Royer, Heather. 2005. “The Response to a Loss in Medicaid Eligibility: Pregnant Immigrant Mothers in the Wake of Welfare Reform.” Working paper.

Schmidt, Lucie and Purvi Sevak. 2004. “AFDC, SSI, and Welfare Reform Aggressiveness: Caseload Reductions versus Caseload Shifting.” *Journal of Human Resources* 39(3): 792-812.

Swartz, Katherine. 1986. "Interpreting the estimates from four national surveys of the number of people without health insurance." *Journal of Economic and Social Measurement* 14 (3), 233– 242.

U.S. House of Representatives Committee on Ways and Means. 1987-2000. *Green Book: Background Material and Data on Programs Within the Jurisdiction of the Committee on Ways and Means*. U.S. Government Printing Office, Washington, DC.

Young, Nathan. 1989. “Wave-Seam Effects in the SIPP.” In *Proceedings of the Section on Survey Research Methods*. Alexandria, VA: American Statistical Association. Available online at http://www.amstat.org/sections/SRMS/Proceedings/papers/1989_069.pdf.

Ziliak, James P. 2007. "Making Work Pay: Changes in Effective Tax Rates and Guarantees in U.S. Transfer Programs, 1983-2002." *Journal of Human Resources* 42(3): 619-642.

Data Appendix

Information about the policies and economic conditions studied in our analysis was gathered from a variety of sources and merged to the SIPP data by date, state, and (in some cases) family size or structure. Information about the sources of each of these variables is given below.

Medicaid and SCHIP eligibility limits: Information on the Medicaid and SCHIP pregnancy and child expansions was obtained from a variety of sources, particularly the National Governors' Association Maternal and Child Health (MCH) Updates. We are grateful to Anna Aizer and Sarah Hamersma for giving us information they collected on parental Medicaid expansions. Information on Medicaid and SCHIP eligibility limits for 18-year-olds, for pregnant women, and for parents was merged to the SIPP data by month and by state of residence.

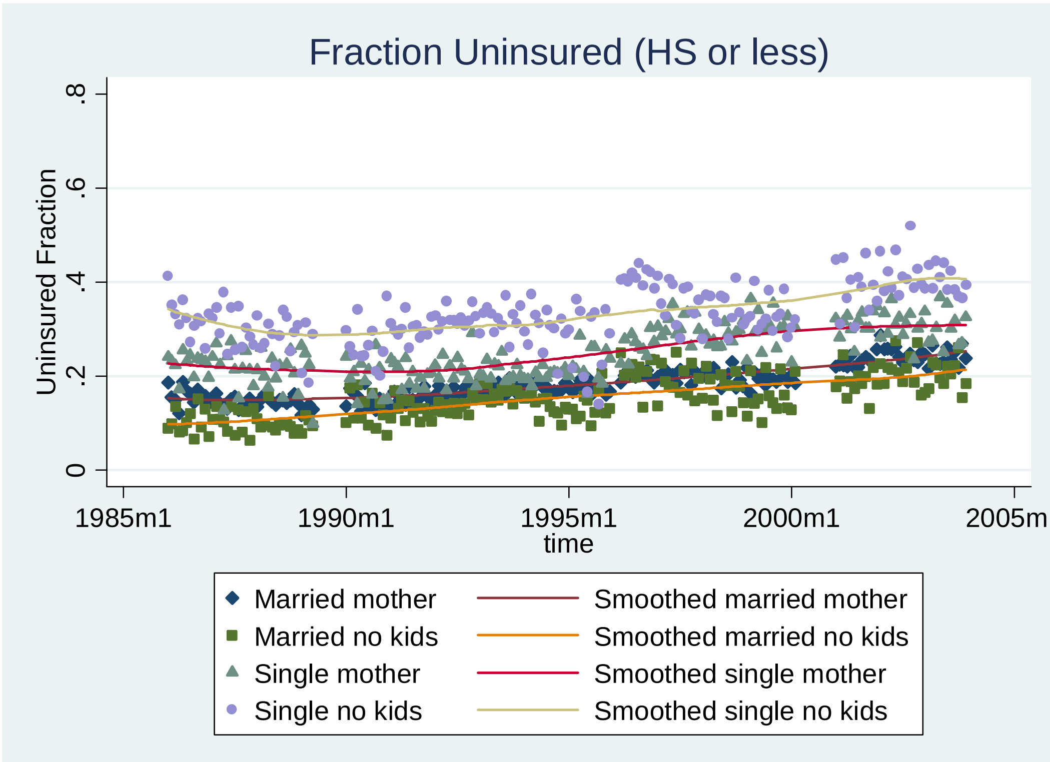
Economic conditions: State-level monthly unemployment rates were obtained from the Bureau of Labor Statistics, as were data on the level of the minimum wage in each state (measured annually). The 25^{th} percentile of real weekly earnings and the male labor force participation rate for each state in each month were calculated using data from the Current Population Survey Merged Outgoing Rotation Groups.

AFDC/TANF rules: We are grateful to James Ziliak for giving us his estimated welfare benefits and tax rates on earned and unearned income, and for passing along the estimates from earlier authors. Information on the maximum benefit available by state, year, and size of family was obtained primarily from the Green Book and from the Urban Institute for years prior to welfare reform and from the Urban Institute’s Welfare Rules Database for the years following welfare reform. Information on the welfare waivers states were granted and the TANF implementation dates came from the Council of Economic Advisors, the State Policy Documentation Project, and the Office of the Assistant Secretary for Planning and Evaluation in the Department of Health and Human Services. Dating of the welfare waivers and TANF implementation was at the monthly level.

EITC parameters: The maximum credit for the EITC for each state, year, number of children, and filing status was calculated using the NBER’s TAXSIM tax calculator. Data on the EITC initial phase-in rate (also by state, year, number of children, and filing status) came from Jon Bakija's IncTaxCalc tax calculator.

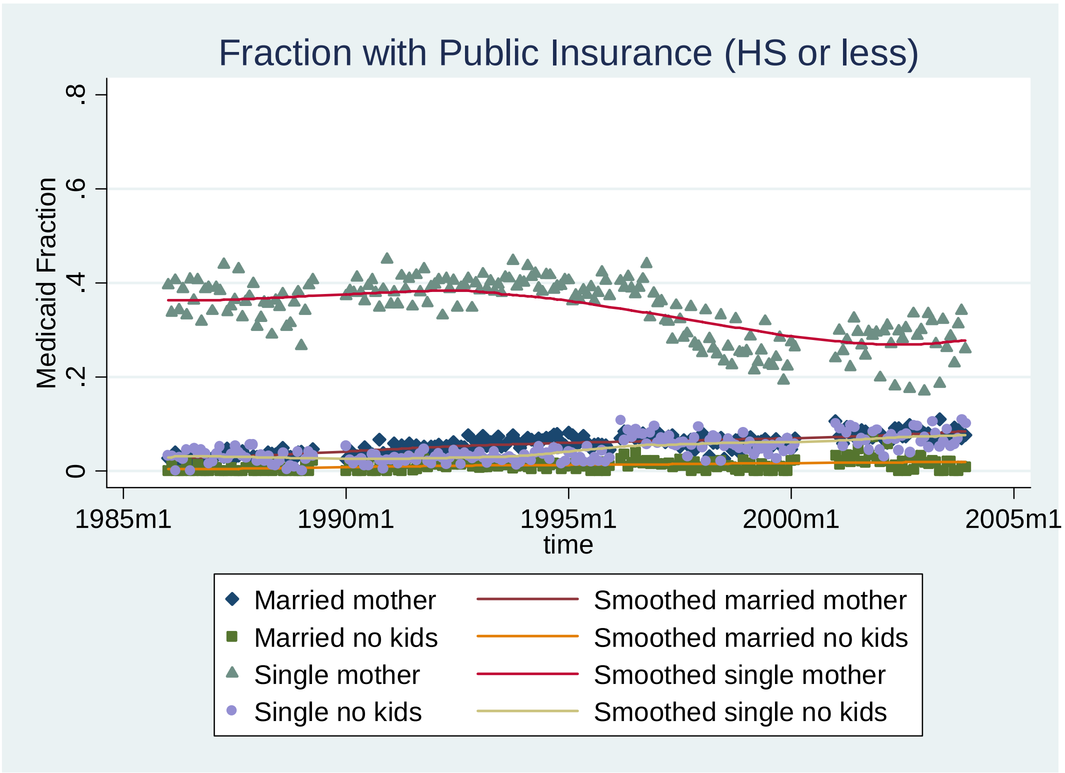
Health care costs: We used Medicare expenditures on personal health care per enrollee by state and year as our proxy for health care costs. These data were obtained from the Centers for Medicare and Medicaid Services.

Figure 1



Fraction Uninsured (HS or less)
Uninsured Fraction
0
.2
.4
.6
.8
1985m1
1990m1
1995m1
2000m1
2005m1
time
Married mother
Married no kids
Single mother
Single no kids
Smoothed married mother
Smoothed married no kids
Smoothed single mother
Smoothed single no kids

Figure 2



Fraction with Public Insurance (HS or less)
Medicaid Fraction
0
.2
.4
.6
.8
1985m1
1990m1
1995m1
2000m1
2005m1
time
Married mother
Married no kids
Single mother
Single no kids
Smoothed married mother
Smoothed married no kids
Smoothed single mother
Smoothed single no kids

Figure 3

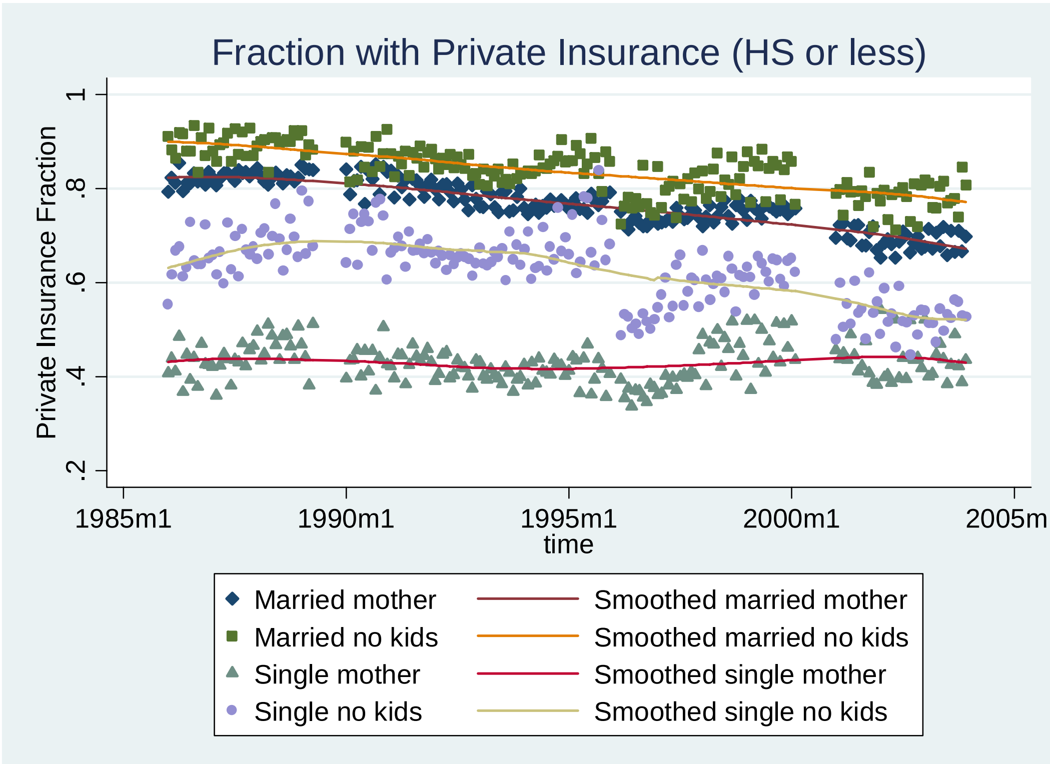
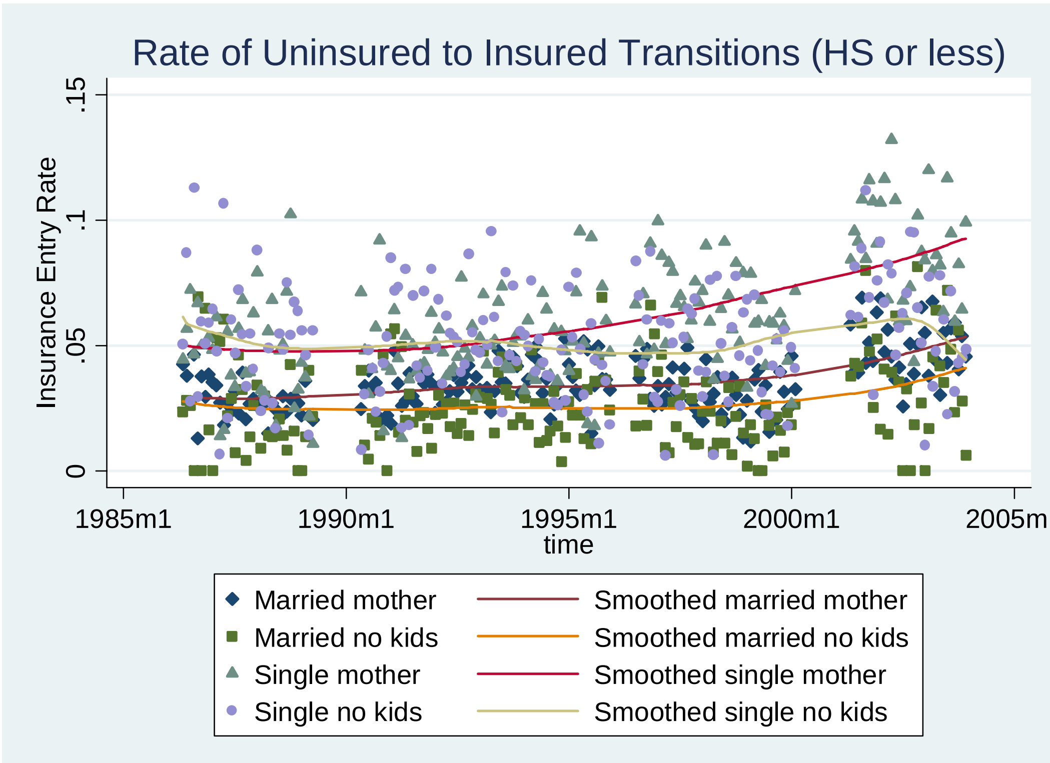
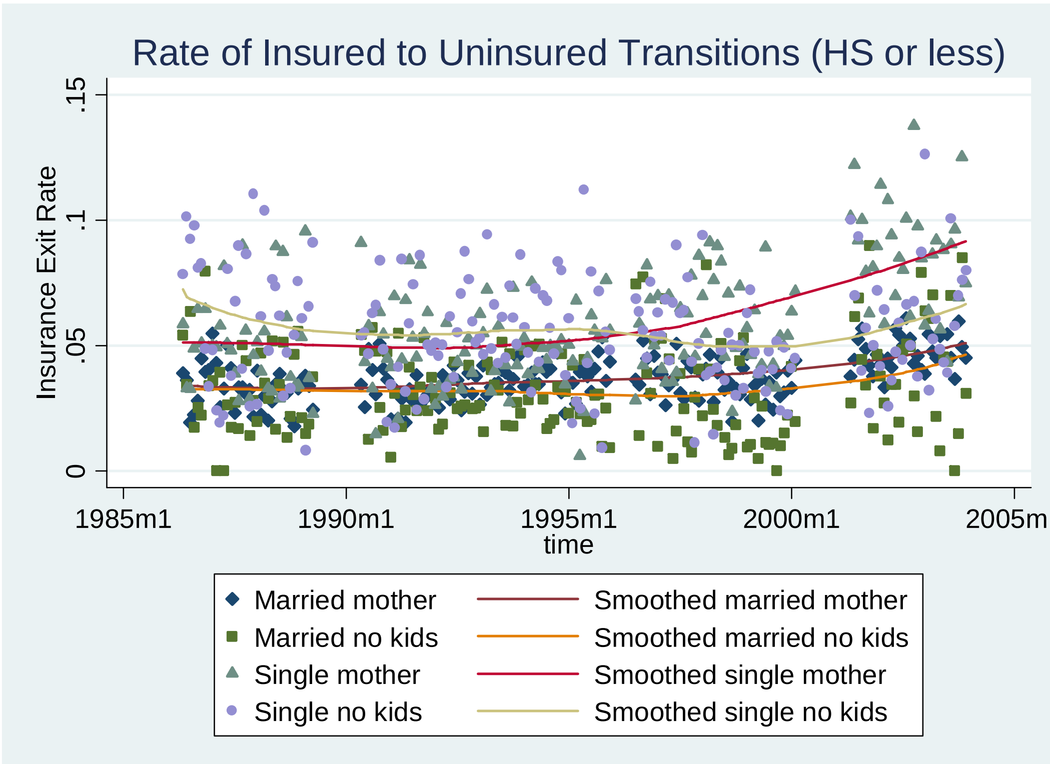

Fraction with Private Insurance (HS or less)
Private Insurance Fraction
.2
.4
.6
.8
1
1985m1
1990m1
1995m1
2000m1
2005m1
time
Married mother
Married no kids
Single mother
Single no kids
Smoothed married mother
Smoothed married no kids
Smoothed single mother
Smoothed single no kids

Figure 4



Rate of Uninsured to Insured Transitions (HS or less)
Insurance Entry Rate
0
.05
.1
.15
1985m1
1990m1
1995m1
2000m1
2005m1
time
Married mother
Married no kids
Single mother
Single no kids
Smoothed married mother
Smoothed married no kids
Smoothed single mother
Smoothed single no kids

Figure 5



Rate of Insured to Uninsured Transitions (HS or less)
Insurance Exit Rate
0
.05
.1
.15
1985m1
1990m1
1995m1
2000m1
2005m1
time
Married mother
Married no kids
Single mother
Single no kids
Smoothed married mother
Smoothed married no kids
Smoothed single mother
Smoothed single no kids

Table 2a: *Testing the Restrictions Implied by Using Married Mothers as a Control Group for Single Mothers*

	Uninsured				
	(1)	(2)	(3)	(4)	(5)
	Pooled	+Reform interactions	+Year interactions	+State interactions	Fully interacted
Major waiver implemented	0.005	0.004	0.008	-0.001	-0.0005
	(0.004)	(0.010)	(0.011)	(0.006)	(0.005)
TANF implemented	-0.001	-0.015	0.002	-0.005	-0.005
	(0.008)	(0.009)+	(0.011)	(0.007)	(0.007)
Major waiver *single		0.003	-0.010	0.019	0.015
		(0.032)	(0.036)	(0.012)	(0.012)
TANF*single		0.048	-0.010	0.015	0.014
		(0.017)**	(0.036)	(0.016)	(0.015)
Reject single/married same?		yes	yes	yes	yes
Reject year interactions 0?			yes	yes	yes#
Reject state interactions 0?				yes	yes

	Medicaid				
	(1)	(2)	(3)	(4)	(5)
	Pooled	+Reform interactions	+Year interactions	+State interactions	Fully interacted
Major waiver implemented	-0.010	-0.012	-0.019	-0.003	0.0002
	(0.004)	(0.006)+	(0.007)**	(0.014)	(0.005)
TANF implemented	-0.009	0.021	-0.016	-0.007	-0.007
	(0.004)	(0.008)**	(0.008)*	(0.006)	(0.007)
Major waiver *single		0.009	0.035	-0.017	-0.025
		(0.017)	(0.023)	(0.013)	(0.017)
TANF*single		-0.097	0.027	-0.004	-0.003
		(0.013)**	(0.028)	(0.014)	(0.013)
Reject single/married same?		yes	yes	yes	yes
Reject year interactions 0?			yes	yes	yes
Reject state interactions 0?				yes	yes

	Private				
	(1)	(2)	(3)	(4)	(5)
	Pooled	+Reform interactions	+Year interactions	+State interactions	Fully interacted
Major waiver implemented	0.002	0.006	0.010	0.003	-0.002
	(0.006)	(0.006)	(0.007)	(0.006)	(0.006)
TANF implemented	0.008	-0.007	0.012	0.010	0.010
	(0.009)	(0.010)	(0.009)	(0.009)	(0.009)
Major waiver *single		-0.016	-0.029	-0.008	0.004
		(0.019)	(0.023)	(0.014)	(0.015)
TANF*single		0.048	-0.014	-0.009	-0.010
		(0.012)**	(0.020)	(0.020)	(0.021)
Reject single/married same?		yes	yes	yes	yes
Reject year interactions 0?			yes	yes	yes
Reject state interactions 0?				yes	yes

The entries in the table are the coefficients on the waiver and TANF variables and their interactions with treatment group status. The coefficients on the interactions give the implied treatment effect. In addition to the variables listed, other variables in the model include the demographic, policy, and labor market variables discussed in the text. Robust standard errors in parentheses, clustered at the state level.

+ significant at 10%; * significant at 5%; ** significant at 1%

All F-test rejections at $p<0.01$, with the exception of those marked # rejected at $p<0.10$.

Table 1: *Summary Statistics*

	Married mothers		Married non-mothers		Unmarried mothers		Unmarried non-mothers	
	Mean	Std. Dev	Mean	Std. Dev	Mean	Std. Dev	Mean	Std. Dev
Uninsured	.1867019	.3896725	.168278	.3741147	.2504307	.4332623	.3415494	.474231
Medicaid	.0633027	.2435069	.0155365	.1236742	.3524941	.4777482	.0526077	.2232499
Private	.7579336	.4283348	.8188846	.3851156	.4153507	.4927838	.6112476	.4874687
Major waiver implemented	.1202346	.325236	.1079752	.3103505	.1197894	.324716	.112365	.3158161
TANF implemented	.319254	.4661882	.3025393	.4593596	.347028	.4760261	.336939	.4726656
Estimated welfare benefit, family of 3/1000	.3745879	.1501074	.3708903	.1460439	.3710996	.1500732	.3861558	.1466249
Welfare tax rate on earned income (%)	29.94706	14.48984	30.272	14.59512	29.25582	14.76287	30.60213	14.58541
Maximum EITC/1000	2.338031	1.126161	.1934675	.245957	2.296788	1.087737	.3615672	.6712848
Minimum wage	4.44916	.5841006	4.432664	.5589678	4.458917	.5936961	4.477486	.5955484
Unemployment rate (%)	6.044022	1.58336	6.00758	1.561679	6.015236	1.570971	6.00622	1.575193
25th percentile of weekly earnings	.3827771	.0951452	.3811766	.0926019	.3900225	.097397	.3912603	.098101
Male labor force participation rate in state	.7513724	.0426647	.7501796	.0432083	.7490033	.0421627	.7506277	.0416731
Medicare spending per enrollee/1000	4.627156	1.172656	4.577189	1.151089	4.714549	1.204437	4.687451	1.191957
Medicaid pregnancy expansion level (fract. FPL)	1.714665	.2879544	1.713146	.289184	1.722224	.2786013	1.724667	.2879194
Parental Medicaid expansion level (fract. FPL)	.1788595	.6132569	.176812	.6184015	.1664346	.5714281	.1847353	.6004209
Medicaid/SCHIP eligibility level for 18-year-olds	.8673861	.8066895	.8458562	.7954844	.8997425	.8112023	.9079	.8155982
Number of children in family	2.104257	1.057995			1.920213	1.074233		
Age	34.26657	6.143578	36.31562	7.678702	32.67877	6.481578	32.40503	7.719916
Age squared/100	12.11942	4.205182	13.77786	5.349373	11.09913	4.313209	11.09683	5.160419
Black	.0782479	.2685616	.0789993	.269739	.3202129	.4665596	.1731764	.3784011
Hispanic	.1860796	.3891713	.1088216	.3114167	.1899665	.3922756	.1455808	.3526867
Highest grade completed	11.20696	1.860949	11.51043	1.423331	11.17207	1.706269	11.47522	1.472959
N (person-months)	433852		106330		174715		134695	

Table 2a: *Testing the Restrictions Implied by Using Married Mothers as a Control Group for Single Mothers*

	Uninsured				
	(1)	(2)	(3)	(4)	(5)
	Pooled	+Reform interactions	+Year interactions	+State interactions	Fully interacted
Major waiver implemented	0.005	0.004	0.008	-0.001	-0.0005
	(0.004)	(0.010)	(0.011)	(0.006)	(0.005)
TANF implemented	-0.001	-0.015	0.002	-0.005	-0.005
	(0.008)	(0.009)+	(0.011)	(0.007)	(0.007)
Major waiver *single		0.003	-0.010	0.019	0.015
		(0.032)	(0.036)	(0.012)	(0.012)
TANF*single		0.048	-0.010	0.015	0.014
		(0.017)**	(0.036)	(0.016)	(0.015)
Reject single/married same?		yes	yes	yes	yes
Reject year interactions 0?			yes	yes	yes#
Reject state interactions 0?				yes	yes

	Medicaid				
	(1)	(2)	(3)	(4)	(5)
	Pooled	+Reform interactions	+Year interactions	+State interactions	Fully interacted
Major waiver implemented	-0.010	-0.012	-0.019	-0.003	0.0002
	(0.004)	(0.006)+	(0.007)**	(0.014)	(0.005)
TANF implemented	-0.009	0.021	-0.016	-0.007	-0.007
	(0.004)	(0.008)**	(0.008)*	(0.006)	(0.007)
Major waiver *single		0.009	0.035	-0.017	-0.025
		(0.017)	(0.023)	(0.013)	(0.017)
TANF*single		-0.097	0.027	-0.004	-0.003
		(0.013)**	(0.028)	(0.014)	(0.013)
Reject single/married same?		yes	yes	yes	yes
Reject year interactions 0?			yes	yes	yes
Reject state interactions 0?				yes	yes

	Private				
	(1)	(2)	(3)	(4)	(5)
	Pooled	+Reform interactions	+Year interactions	+State interactions	Fully interacted
Major waiver implemented	0.002	0.006	0.010	0.003	-0.002
	(0.006)	(0.006)	(0.007)	(0.006)	(0.006)
TANF implemented	0.008	-0.007	0.012	0.010	0.010
	(0.009)	(0.010)	(0.009)	(0.009)	(0.009)
Major waiver *single		-0.016	-0.029	-0.008	0.004
		(0.019)	(0.023)	(0.014)	(0.015)
TANF*single		0.048	-0.014	-0.009	-0.010
		(0.012)**	(0.020)	(0.020)	(0.021)
Reject single/married same?		yes	yes	yes	yes
Reject year interactions 0?			yes	yes	yes
Reject state interactions 0?				yes	yes

The entries in the table are the coefficients on the waiver and TANF variables and their interactions with treatment group status. The coefficients on the interactions give the implied treatment effect. In addition to the variables listed, other variables in the model include the demographic, policy, and labor market variables discussed in the text. Robust standard errors in parentheses, clustered at the state level.

+ significant at 10%; * significant at 5%; ** significant at 1%

All F-test rejections at $p<0.01$, with the exception of those marked # rejected at $p<0.10$.

Table 2b: *Testing the Restrictions Implied by Using Single Women Without Children as a Control Group for Single Mothers*

	Uninsured				
	(1)	(2)	(3)	(4)	(5)
	Pooled	+Reform interactions	+Year interactions	+State interactions	Fully interacted
Major waiver implemented	0.012	0.036	0.031	0.001	0.010
	(0.009)	(0.020)+	(0.024)	(0.018)	(0.017)
TANF implemented	-0.003	-0.002	0.002	-0.020	-0.010
	(0.012)	(0.015)	(0.018)	(0.017)	(0.017)
Major waiver *mother		-0.042	-0.032	0.019	0.004
		(0.029)	(0.037)	(0.022)	(0.020)
TANF*mother		0.002	-0.009	0.031	0.019
		(0.018)	(0.035)	(0.023)	(0.023)
Reject mothers/non-mothers same?		no	yes	yes	yes
Reject year interactions 0?			yes	yes	no
Reject state interactions 0?				yes	yes

	Medicaid				
	(1)	(2)	(3)	(4)	(5)
	Pooled	+Reform interactions	+Year interactions	+State interactions	Fully interacted
Major waiver implemented	-0.014	-0.038	-0.042	-0.004	-0.007
	(0.010)	(0.025)	(0.025)+	(0.008)	(0.008)
TANF implemented	0.001	0.035	-0.011	0.015	0.003
	(0.009)	(0.018)+	(0.028)	(0.015)	(0.013)
Major waiver *mother		0.041	0.048	-0.020	-0.017
		(0.032)	(0.033)	(0.010)	(0.011)
TANF*mother		-0.059	0.022	-0.025	-0.013
		(0.024)*	(0.044)	(0.022)	(0.020)
Reject mothers/non-mothers same?		yes	yes	yes	yes
Reject year interactions 0?			yes	yes	yes
Reject state interactions 0?				yes	yes

	Private				
	(1)	(2)	(3)	(4)	(5)
	Pooled	+Reform interactions	+Year interactions	+State interactions	Fully interacted
Major waiver implemented	-0.003	-0.001	0.007	-0.0007	-0.008
	(0.015)	(0.025)	(0.024)	(0.022)	(0.020)
TANF implemented	0.0005	-0.032	0.006	0.003	0.004
	(0.016)	(0.023)	(0.024)	(0.024)	(0.022)
Major waiver *mother		0.003	-0.017	-0.003	0.011
		(0.023)	(0.025)	(0.018)	(0.017)
TANF*mother		0.057	-0.010	-0.004	-0.003
		(0.019)**	(0.032)	(0.030)	(0.027)
Reject mothers/non-mothers same?		yes	yes	yes	yes
Reject year interactions 0?			yes	yes	yes
Reject state interactions 0?				yes	yes

The entries in the table are the coefficients on the waiver and TANF variables and their interactions with treatment group status. The coefficients on the interactions give the implied treatment effect. In addition to the variables listed, other variables in the model include the demographic, policy, and labor market variables discussed in the text. Robust standard errors in parentheses, clustered at the state level.

+ significant at 10%; * significant at 5%; ** significant at 1%

All F-test rejections at $p<0.01$, with the exception of those marked # rejected at $p<0.10$.

Table 3a: *Results for Married Mothers with High School or Less Education*

	(1) Uninsured	(2) Medicaid	(3) Private
Major AFDC waiver implemented	-0.0005	0.0002	-0.001
	(0.005)	(0.005)	(0.006)
TANF in effect	-0.005	-0.007	0.010
	(0.007)	(0.007)	(0.009)
Estimated welfare benefit, family of 3	-0.002	-0.106	0.088
	(0.120)	(0.049)*	(0.135)
Welfare tax rate on earned income	-0.0002	0.00003	0.0002
	(0.0002)	(0.0002)	(0.0003)
Maximum EITC	-0.015	-0.016	0.029
	(0.005)**	(0.002)**	(0.005)**
Minimum wage	-0.003	0.003	-0.001
	(0.009)	(0.005)	(0.007)
Unemployment rate	0.003	0.003	-0.005
	(0.002)	(0.003)	(0.003)+
25th percentile of earnings	-0.070	0.024	0.061
	(0.027)*	(0.021)	(0.032)+
Male labor force participation rate	-0.013	-0.009	0.028
	(0.018)	(0.015)	(0.018)
Medicare spending per enrollee	0.031	0.001	-0.036
	(0.012)*	(0.004)	(0.012)**
Medicaid pregnancy expansion level	-0.005	0.003	0.004
	(0.009)	(0.008)	(0.012)
Parental Medicaid expansion level	-0.006	0.014	-0.006
	(0.003)*	(0.006)*	(0.005)
Medicaid/SCHIP eligibility limit for 18-year-olds	-0.007	0.001	0.007
	(0.004)+	(0.003)	(0.004)*
Number of children in family	0.023	0.032	-0.052
	(0.006)**	(0.004)**	(0.004)**
Age	-0.033	-0.034	0.062
	(0.004)**	(0.003)**	(0.005)**
Age squared/100	0.040	0.044	-0.077
	(0.006)**	(0.004)**	(0.008)**
Black	-0.007	0.053	-0.035
	(0.014)	(0.011)**	(0.016)*
Hispanic ethnicity	0.111	0.008	-0.118
	(0.014)**	(0.019)	(0.019)**
Highest grade completed	-0.038	-0.014	0.052
	(0.005)**	(0.003)**	(0.003)**
4th month in wave	-0.001	0.001	0.0004
	(0.0004)**	(0.0002)**	(0.0004)
Person-months	433852	433852	433852
R-squared	0.11	0.08	0.18

Robust standard errors in parentheses, clustered at the state level.
+ significant at 10%; * significant at 5%; ** significant at 1%

Table 3b: *Results for Married non-Mothers with High School or Less Education*

	(1) Uninsured	(2) Medicaid	(3) Private
Major AFDC waiver implemented	0.002	-0.003	0.002
	(0.014)	(0.003)	(0.013)
TANF in effect	-0.019	-0.004	0.023
	(0.018)	(0.006)	(0.016)
Estimated welfare benefit, family of 3	-0.297	-0.007	0.307
	(0.157)+	(0.038)	(0.168)+
Welfare tax rate on earned income	0.0002	0.0002	-0.0003
	(0.0004)	(0.0001)	(0.0004)
Maximum EITC	0.075	0.012	-0.087
	(0.025)**	(0.017)	(0.032)**
Minimum wage	0.005	0.010	-0.014
	(0.015)	(0.003)**	(0.016)
Unemployment rate	-0.005	-0.001	0.005
	(0.005)	(0.001)	(0.005)
25th percentile of earnings	-0.089	-0.009	0.098
	(0.066)	(0.012)	(0.065)
Male labor force participation rate	-0.027	-0.008	0.029
	(0.031)	(0.010)	(0.033)
Medicare spending per enrollee	0.019	-0.007	-0.013
	(0.018)	(0.005)	(0.019)
Medicaid pregnancy expansion level	0.028	0.003	-0.024
	(0.021)	(0.007)	(0.021)
Parental Medicaid expansion level	-0.009	-0.004	0.014
	(0.008)	(0.003)	(0.008)+
Medicaid/SCHIP eligibility limit for 18-year-olds	-0.003	0.004	-0.001
	(0.007)	(0.002)*	(0.007)
Age	-0.003	-0.006	0.008
	(0.006)	(0.002)**	(0.006)
Age squared/100	-0.002	0.007	-0.004
	(0.009)	(0.002)**	(0.009)
Black	0.034	0.019	-0.050
	(0.019)+	(0.005)**	(0.019)*
Hispanic ethnicity	0.108	0.008	-0.115
	(0.035)**	(0.005)	(0.037)**
Highest grade completed	-0.053	-0.005	0.058
	(0.005)**	(0.001)**	(0.005)**
4th month in wave	-0.0004	0.00004	0.0004
	(0.001)	(0.0003)	(0.001)
Person-months	106330	106330	106330
R-squared	0.11	0.04	0.13

Robust standard errors in parentheses, clustered at the state level.

+ significant at 10%; * significant at 5%; ** significant at 1%

Table 3c: *Results for Unmarried Mothers with High School or Less Education*

	(1) Uninsured	(2) Medicaid	(3) Private
Major AFDC waiver implemented	0.014	-0.024	0.003
	(0.009)	(0.015)	(0.014)
TANF in effect	0.009	-0.010	0.001
	(0.016)	(0.013)	(0.020)
Estimated welfare benefit, family of 3	0.021	-0.258	0.264
	(0.179)	(0.211)	(0.145)+
Welfare tax rate on earned income	-0.0001	-0.0006	0.001
	(0.0005)	(0.0004)	(0.001)
Maximum EITC	-0.0003	-0.011	0.012
	(0.007)	(0.011)	(0.010)
Minimum wage	-0.007	-0.004	0.012
	(0.011)	(0.008)	(0.010)
Unemployment rate	-0.008	0.011	-0.004
	(0.005)	(0.004)*	(0.005)
25th percentile of earnings	-0.0002	-0.038	0.041
	(0.050)	(0.068)	(0.060)
Male labor force participation rate	-0.013	0.035	-0.010
	(0.034)	(0.045)	(0.034)
Medicare spending per enrollee	0.015	-0.028	0.011
	(0.017)	(0.021)	(0.023)
Medicaid pregnancy expansion level	-0.052	0.004	0.044
	(0.020)*	(0.022)	(0.027)
Parental Medicaid expansion level	-0.021	0.001	0.021
	(0.009)*	(0.006)	(0.007)**
Medicaid/SCHIP eligibility limit for 18-year-olds	0.016	0.0001	-0.016
	(0.006)*	(0.011)	(0.008)+
Number of children in family	-0.025	0.095	-0.068
	(0.007)**	(0.008)**	(0.005)**
Age	0.011	-0.065	0.047
	(0.006)+	(0.005)**	(0.006)**
Age squared/100	-0.018	0.072	-0.046
	(0.010)+	(0.008)**	(0.010)**
Black	-0.061	0.159	-0.088
	(0.008)**	(0.013)**	(0.017)**
Hispanic ethnicity	0.020	0.035	-0.055
	(0.023)	(0.043)	(0.032)+
Highest grade completed	-0.016	-0.039	0.054
	(0.002)**	(0.007)**	(0.006)**
4th month in wave	-0.003	0.005	-0.003
	(0.001)**	(0.001)**	(0.001)**
Person-months	174715	174715	174715
R-squared	0.06	0.20	0.15

Robust standard errors in parentheses, clustered at the state level.
+ significant at 10%; * significant at 5%; ** significant at 1%

Table 3d: *Results for Unmarried non-Mothers with High School or Less Education*

	(1) Uninsured	(2) Medicaid	(3) Private
Major AFDC waiver implemented	0.010	-0.007	-0.008
	(0.017)	(0.008)	(0.020)
TANF in effect	-0.010	0.003	0.004
	(0.017)	(0.013)	(0.022)
Estimated welfare benefit, family of 3	0.141	-0.040	-0.074
	(0.254)	(0.076)	(0.263)
Welfare tax rate on earned income	0.001	-0.00003	-0.001
	(0.0004)+	(0.0002)	(0.0004)*
Maximum EITC	0.022	0.005	-0.026
	(0.005)**	(0.005)	(0.004)**
Minimum wage	-0.001	0.005	-0.004
	(0.012)	(0.005)	(0.011)
Unemployment rate	-0.006	0.011	-0.003
	(0.008)	(0.003)**	(0.009)
25th percentile of earnings	-0.018	0.051	-0.026
	(0.051)	(0.031)	(0.052)
Male labor force participation rate	0.012	-0.003	-0.009
	(0.041)	(0.017)	(0.041)
Medicare spending per enrollee	0.024	-0.004	-0.017
	(0.019)	(0.010)	(0.019)
Medicaid pregnancy expansion level	-0.050	-0.021	0.065
	(0.036)	(0.009)*	(0.033)+
Parental Medicaid expansion level	-0.002	0.028	-0.025
	(0.012)	(0.013)*	(0.007)**
Medicaid/SCHIP eligibility limit for 18-year-olds	0.011	-0.002	-0.007
	(0.008)	(0.004)	(0.008)
Age	0.003	-0.006	0.002
	(0.006)	(0.003)+	(0.006)
Age squared/100	-0.014	0.007	0.007
	(0.010)	(0.005)	(0.010)
Black	0.080	0.054	-0.128
	(0.019)**	(0.009)**	(0.019)**
Hispanic ethnicity	0.119	0.012	-0.134
	(0.023)**	(0.014)	(0.029)**
Highest grade completed	-0.046	-0.012	0.058
	(0.006)**	(0.004)**	(0.007)**
4th month in wave	-0.001	0.002	-0.001
	(0.001)	(0.0005)**	(0.001)
Person-months	134695	134695	134695
R-squared	0.09	0.06	0.11

Robust standard errors in parentheses, clustered at the state level.
+ significant at 10%; * significant at 5%; ** significant at 1%

Table 4: *Results for Women with Less than a High School Education*

	Married Mothers			Married Non-Mothers		
	Uninsured	Medicaid	Private	Uninsured	Medicaid	Private
Major AFDC waiver implemented	-0.004	-0.001	0.005	-0.046	-0.003	0.052
	(0.020)	(0.013)	(0.017)	(0.058)	(0.013)	(0.061)
TANF in effect	-0.034	0.018	0.019	-0.102	0.021	0.091
	(0.018)+	(0.012)	(0.019)	(0.066)	(0.021)	(0.068)
Person-months	98170	98170	98170	17412	17412	17412
R-squared	0.10	0.10	0.14	0.18	0.08	0.20

	Unmarried Mothers			Unmarried Non-Mothers		
	Uninsured	Medicaid	Private	Uninsured	Medicaid	Private
Major AFDC waiver implemented	0.030	-0.059	0.028	-0.008	-0.021	0.033
	(0.019)	(0.033)+	(0.022)	(0.037)	(0.024)	(0.033)
TANF in effect	-0.014	-0.036	0.052	0.001	0.003	0.011
	(0.032)	(0.030)	(0.023)*	(0.048)	(0.027)	(0.042)
Person-months	51099	51099	51099	23801	23801	23801
R-squared	0.11	0.18	0.39	0.09	0.11	0.10

Robust standard errors in parentheses, clustered at the state level.
+ significant at 10%; * significant at 5%; ** significant at 1%

Table 5: *Results for Hispanic Women with Less than a High School Education, All and by Immigrant Status*

All Hispanic Women

	Married Mothers			Married Non-Mothers			Unmarried Mothers			Unmarried Non-Mothers		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Uninsured	Medicaid	Private	Uninsured	Medicaid	Private	Uninsured	Medicaid	Private	Uninsured	Medicaid	Private
Major AFDC waiver implemented	-0.008	0.010	0.002	-0.048	-0.028	0.070	0.083	-0.150	0.064	-0.016	-0.058	0.073
	(0.025)	(0.020)	(0.026)	(0.079)	(0.037)	(0.075)	(0.027)**	(0.056)*	(0.052)	(0.050)	(0.030)+	(0.064)
TANF in effect	0.012	0.027	-0.037	-0.079	0.027	0.063	0.019	-0.091	0.062	-0.013	-0.018	0.044
	(0.025)	(0.016)+	(0.028)	(0.066)	(0.043)	(0.050)	(0.046)	(0.059)	(0.047)	(0.065)	(0.040)	(0.064)
Person-months	43992	43992	43992	4591	4591	4591	17521	17521	17521	8211	8211	8211
R-squared	0.10	0.07	0.12	0.17	0.12	0.21	0.15	0.20	0.12	0.11	0.16	0.10

Immigrant Hispanic Women

	Married Mothers			Married Non-Mothers			Unmarried Mothers			Unmarried Non-Mothers		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Uninsured	Medicaid	Private	Uninsured	Medicaid	Private	Uninsured	Medicaid	Private	Uninsured	Medicaid	Private
Major AFDC waiver implemented	-0.009	-0.0002	0.008	-0.036	-0.020	0.047	0.093	-0.149	0.067	-0.118	0.030	0.089
	(0.025)	(0.023)	(0.027)	(0.129)	(0.039)	(0.109)	(0.051)+	(0.065)*	(0.057)	(0.066)+	(0.034)	(0.075)
TANF in effect	0.029	0.020	-0.049	-0.019	0.018	-0.007	0.067	-0.153	0.093	-0.085	0.056	0.041
	(0.029)	(0.018)	(0.031)	(0.127)	(0.051)	(0.093)	(0.074)	(0.071)*	(0.054)+	(0.064)	(0.024)*	(0.066)
Person-months	32154	32154	32154	2820	2820	2820	9568	9568	9568	5115	5115	5115
R-squared	0.11	0.06	0.12	0.16	0.15	0.19	0.14	0.17	0.10	0.16	0.19	0.11

Native-born Hispanic Women

	Married Mothers			Married Non-Mothers			Unmarried Mothers			Unmarried Non-Mothers		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Uninsured	Medicaid	Private	Uninsured	Medicaid	Private	Uninsured	Medicaid	Private	Uninsured	Medicaid	Private
Major AFDC waiver implemented	-0.058	0.006	0.084	-0.170	-0.082	0.252	0.028	-0.073	0.039	0.216	-0.167	-0.052
	(0.041)	(0.039)	(0.054)	(0.191)	(0.044)+	(0.228)	(0.016)+	(0.077)	(0.078)	(0.128)	(0.053)**	(0.106)
TANF in effect	-0.138	0.052	0.110	-0.103	-0.049	0.152	-0.060	0.014	0.032	0.004	-0.019	0.044
	(0.068)*	(0.050)	(0.084)	(0.078)	(0.023)*	(0.084)+	(0.051)	(0.072)	(0.067)	(0.151)	(0.081)	(0.094)
Person-months	9724	9724	9724	1177	1177	1177	6699	6699	6699	2040	2040	2040
R-squared	0.17	0.17	0.21	0.43	0.28	0.48	0.20	0.31	0.24	0.25	0.33	0.29

Robust standard errors in parentheses, clustered at the state level.
+ significant at 10%; * significant at 5%; ** significant at 1%

Table 6: *Results for White and Black Women with Less than a High School Education*

	White											
	Married Mothers			Married Non-Mothers			Unmarried Mothers			Unmarried Non-Mothers		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Uninsured	Medicaid	Private	Uninsured	Medicaid	Private	Uninsured	Medicaid	Private	Uninsured	Medicaid	Private
Major AFDC waiver implemented	0.003	-0.0004	0.001	-0.058	-0.001	0.065	0.037	0.006	-0.050	-0.017	0.012	0.010
	(0.025)	(0.016)	(0.020)	(0.048)	(0.015)	(0.051)	(0.029)	(0.032)	(0.026)+	(0.043)	(0.028)	(0.047)
TANF in effect	-0.075	0.021	0.061	-0.109	-0.005	0.117	-0.028	0.019	0.006	-0.013	0.013	0.027
	(0.025)**	(0.019)	(0.023)*	(0.081)	(0.016)	(0.082)	(0.054)	(0.049)	(0.036)	(0.062)	(0.032)	(0.060)
Person-months	48797	48797	48797	11677	11677	11677	19634	19634	19634	11440	11440	11440
R-squared	0.08	0.16	0.14	0.17	0.07	0.18	0.16	0.22	0.13	0.11	0.13	0.11

	Black											
	Married Mothers			Married Non-Mothers			Unmarried Mothers			Unmarried Non-Mothers		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Uninsured	Medicaid	Private	Uninsured	Medicaid	Private	Uninsured	Medicaid	Private	Uninsured	Medicaid	Private
Major AFDC waiver implemented	0.100	-0.185	0.021	-0.026	-0.003	0.042	0.002	-0.062	0.075	0.010	-0.033	0.028
	(0.053)+	(0.075)*	(0.062)	(0.109)	(0.029)	(0.116)	(0.041)	(0.047)	(0.057)	(0.082)	(0.080)	(0.069)
TANF in effect	0.058	-0.162	0.101	-0.130	-0.106	0.245	0.020	-0.067	0.074	-0.035	0.037	-0.027
	(0.085)	(0.088)+	(0.094)	(0.146)	(0.065)	(0.123)+	(0.045)	(0.060)	(0.038)+	(0.107)	(0.098)	(0.099)
Person-months	5381	5381	5381	1144	1144	1144	13944	13944	13944	4150	4150	4150
R-squared	0.17	0.36	0.27	0.42	0.48	0.50	0.12	0.25	0.14	0.22	0.26	0.25

Robust standard errors in parentheses
+ significant at 10%; * significant at 5%; ** significant at 1%

Table 7: *Fixed Effects Results for Women with Less than a High School Education*

	Married Mothers			Married Non-Mothers		
	Uninsured	Medicaid	Private	Uninsured	Medicaid	Private
Major AFDC waiver implemented	-0.006	-0.001	0.010	-0.076	0.024	0.051
	(0.022)	(0.011)	(0.013)	(0.043)+	(0.007)**	(0.041)
TANF in effect	-0.032	0.022	0.018	-0.064	0.016	0.055
	(0.019)	(0.012)+	(0.013)	(0.036)+	(0.016)	(0.034)
Person-months (individuals)	82949 (3127)			11309 (587)		
Indiv. with waiver status change	807			81		
Indiv. with TANF status change	589			73		

	Unmarried Mothers			Unmarried Non-Mothers		
	Uninsured	Medicaid	Private	Uninsured	Medicaid	Private
Major AFDC waiver implemented	0.017	0.010	-0.034	0.026	-0.030	0.007
	(0.018)	(0.018)	(0.015)*	(0.035)	(0.025)	(0.036)
TANF in effect	-0.010	-0.022	0.023	0.035	-0.025	-0.005
	(0.028)	(0.027)	(0.022)	(0.050)	(0.026)	(0.044)
Person-months (individuals)	39332 (1803)			17189 (1050)		
Indiv. with waiver status change	373			167		
Indiv. with TANF status change	306			136		

Robust standard errors in parentheses, clustered at the state level.

+ significant at 10%; * significant at 5%; ** significant at 1%

Table 8: *Results Incorporating a Lagged Dependent Variable, Women with Less than a High School Education*

	Married Mothers			Married Non-Mothers			Unmarried Mothers			Unmarried Non-Mothers		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Uninsured	Medicaid	Private	Uninsured	Medicaid	Private	Uninsured	Medicaid	Private	Uninsured	Medicaid	Private
Lagged insurance value	1.006	0.965	0.889	0.867	0.745	0.854	0.735	0.856	0.949	0.998	0.967	0.856
	(0.078)**	(0.069)**	(0.048)**	(0.083)**	(0.080)**	(0.074)**	(0.124)**	(0.095)**	(0.066)**	(0.135)**	(0.196)**	(0.078)**
Major AFDC waiver implemented	-0.002	0.002	0.001	-0.016	0.002	0.014	0.006	-0.006	0.002	-0.012	0.002	0.014
	(0.002)	(0.002)	(0.002)	(0.010)	(0.003)	(0.011)	(0.006)	(0.08)	(0.003)	(0.005)*	(0.006)	(0.007)*
TANF in effect	0.006	-0.005	0.003	-0.032	0.003	0.035	-0.003	-0.012	0.010	-0.002	0.004	0.004
	(0.004)	(0.004)	(0.005)	(0.015)*	(0.003)	(0.015)*	(0.011)	(0.008)	(0.006)	(0.012)	(0.009)	(0.010)
Observations	93687	93687	93687	16280	16280	16280	48168	48168	48168	22043	22043	22043
R-squared	0.87	0.87	0.91	0.89	0.81	0.90	0.82	0.90	0.88	0.85	0.87	0.87

Robust standard errors in parentheses, clustered at the state level.
+ significant at 10%; * significant at 5%; ** significant at 1%

Predicted effects of:												
Waiver after 6 months	-0.009	0.009	0.005	-0.068	0.006	0.060	0.020	-0.027	0.010	-0.074	0.011	0.061
	(0.012)	(0.009)	(0.010)	(0.042)	(0.011)	(0.211)	(0.017)	(0.029)	(0.017)	(0.043)	(0.036)	(0.025)
Waiver after 12 months	-0.019	0.015	0.007	-0.097	0.007	0.084	0.024	-0.038	0.018	-0.147	0.019	0.085
	(0.022)	(0.017)	(0.014)	(0.060)	(0.013)	(0.391)	(0.019)	(0.036)	(0.028)	(0.136)	(0.076)	(0.036)
Waiver after 24 months	-0.040	0.026	0.009	-0.114	0.008	0.096	0.024	-0.044	0.028	-0.292	0.032	0.098
	(0.039)	(0.032)	(0.017)	(0.078)	(0.014)	(0.535)	(0.019)	(0.038)	(0.040)	(0.490)	(0.158)	(0.048)
TANF after 6 months	0.039	-0.030	0.015	-0.138	0.009	0.146	-0.008	-0.053	0.054	-0.011	0.023	0.018
	(0.032)	(0.024)	(0.023)	(0.054)	(0.018)	(0.056)	(0.037)	(0.033)	(0.027)	(0.073)	(0.055)	(0.045)
TANF after 12 months	0.080	-0.054	0.022	-0.196	0.010	0.203	-0.010	-0.074	0.093	-0.023	0.043	0.024
	(0.081)	(0.051)	(0.033)	(0.075)	(0.020)	(0.082)	(0.043)	(0.050)	(0.045)	(0.148)	(0.114)	(0.063)
TANF after 24 months	0.165	-0.089	0.028	-0.232	0.010	0.233	-0.010	-0.085	0.142	-0.045	0.071	0.028
	(0.240)	(0.110)	(0.041)	(0.105)	(0.021)	(0.109)	(0.044)	(0.065)	(0.084)	(0.304)	(0.242)	(0.072)

Standard errors calculated using the delta method from robust standard errors of estimates.

Table 9: *Results Allowing the Impact of Reform to Vary Over Time, Women with Less than a High School Education*

	Married Mothers			Married Non-Mothers			Unmarried Mothers			Unmarried Non-Mothers		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Uninsured	Medicaid	Private	Uninsured	Medicaid	Private	Uninsured	Medicaid	Private	Uninsured	Medicaid	Private
Major AFDC waiver 1-6 months	-0.001	-0.006	0.002	-0.058	0.028	0.040	0.023	-0.008	-0.013	0.061	-0.016	-0.042
	(0.020)	(0.013)	(0.019)	(0.033)+	(0.019)	(0.035)	(0.020)	(0.023)	(0.020)	(0.043)	(0.028)	(0.037)
Major AFDC waiver 7-12 months	0.013	-0.003	-0.0003	-0.117	0.013	0.106	0.014	-0.021	0.006	-0.003	-0.004	0.008
	(0.020)	(0.017)	(0.019)	(0.058)*	(0.022)	(0.059)+	(0.027)	(0.030)	(0.023)	(0.045)	(0.023)	(0.043)
Major AFDC waiver 13-24 months	-0.006	0.012	-0.004	-0.117	0.019	0.095	0.030	-0.034	-0.001	-0.041	0.033	-0.003
	(0.023)	(0.017)	(0.019)	(0.054)*	(0.019)	(0.058)	(0.035)	(0.043)	(0.029)	(0.042)	(0.025)	(0.038)
Major AFDC waiver >24 months	-0.012	0.030	-0.018	-0.182	0.024	0.154	0.006	0.054	-0.073	0.003	0.069	-0.079
	(0.028)	(0.018)	(0.025)	(0.065)**	(0.019)	(0.062)*	(0.032)	(0.027)+	(0.026)**	(0.046)	(0.025)**	(0.045)+
p-value for test of equality	0.68	0.06	0.82	0.05	0.87	0.01	0.69	0.15	0.01	0.001	0.05	0.04
TANF in effect 1-6 months	-0.033	0.020	0.014	-0.075	0.026	0.055	-0.046	0.016	0.036	0.010	0.017	-0.017
	(0.018)+	(0.012)+	(0.016)	(0.060)	(0.017)	(0.059)	(0.031)	(0.027)	(0.022)	(0.031)	(0.020)	(0.025)
TANF in effect 7-12 months	-0.012	-0.014	0.021	-0.073	0.037	0.042	-0.064	0.026	0.053	0.032	-0.014	-0.011
	(0.023)	(0.015)	(0.021)	(0.083)	(0.019)+	(0.080)	(0.040)	(0.037)	(0.028)+	(0.040)	(0.021)	(0.037)
TANF in effect 13-24 months	-0.010	-0.021	0.030	-0.040	0.036	0.011	-0.044	0.049	0.010	-0.003	-0.018	0.042
	(0.038)	(0.019)	(0.041)	(0.111)	(0.022)	(0.111)	(0.053)	(0.042)	(0.049)	(0.062)	(0.029)	(0.059)
TANF in effect >24 months	-0.022	-0.012	0.027	0.028	0.056	-0.072	-0.020	0.039	0.001	0.032	-0.066	0.051
	(0.059)	(0.030)	(0.066)	(0.159)	(0.030)+	(0.160)	(0.085)	(0.053)	(0.065)	(0.074)	(0.037)+	(0.064)
p-value for test of equality	0.25	0.001	0.77	0.81	0.30	0.70	0.77	0.66	0.33	0.10	0.02	0.55
Person-months	98170	98170	98170	17412	17412	17412	51099	51099	51099	23801	23801	23801
R-squared	0.10	0.10	0.14	0.18	0.08	0.21	0.11	0.18	0.09	0.09	0.12	0.10

Robust standard errors in parentheses, clustered at the state level.

+ significant at 10%; * significant at 5%; ** significant at 1%