Welfare Reform and Health[*]

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Abstract

We investigate the impact of welfare reform on health insurance coverage and health care utilization of single women aged 20–45, using nationally representative data from the Behavioral Risk Factor Surveillance System. We present estimates from both difference-in-difference models and difference-in-difference-in-difference models (using married women as a comparison group). We find that welfare reform is associated with reductions in health insurance coverage and specific measures of health care utilization, as well as an increase in the likelihood of needing care but finding it unaffordable. Overall, effects are somewhat larger for Hispanics compared to blacks and low educated women.

I. Introduction

Evaluating the impacts of state and federal welfare reform is the subject of a large and growing literature. The recent welfare reform period in the United States started with state waivers from the former Aid to Families with Dependent Children (AFDC) program in the early 1990s. This period of active state experimentation culminated in the passage of the 1996 Personal Responsibility and Work Opportunity Act (PRWORA), which eliminated AFDC and replaced it with Temporary Assistance for Needy Families (TANF). This federal reform dramatically changed the economic incentives facing low-income individuals with children or considering having children. In particular, these reforms imposed lifetime time limits, strengthened work requirements, and limited the eligible population.

A number of recent studies have shown that state waivers and TANF implementation have played a role in the dramatic declines in welfare caseloads and increases in the employment of less skilled women.[1] Now that this first wave of research has established these important results, there is increasing interest in broadening our evaluation of welfare reform by examining the impacts on family well-being. Our paper makes an important contribution by examining the impact of welfare reform on the health care utilization of adult women.

Little is known about the effects of welfare reform on health care utilization and overall health status. Several recent studies do, however, examine the impact of welfare reform on health insurance coverage using data from the Current Population Survey (CPS). Kaestner and Kaushal (2003) find that welfare reform led to a decrease in Medicaid coverage. The overall (negative) effects of reform on health insurance coverage were attenuated, they find, due to increases in private health insurance coverage. In a closely related literature, several papers examine state expansions in Medicaid eligibility for immigrants and parents that occurred around the time of

PRWORA. Borjas (2003) and Royer (2003) find that more restrictive Medicaid policies did not lead to substantially reduced health insurance coverage among immigrants, because the loss in public coverage was offset by substantial increases in private insurance coverage. Busch and Duchovny (2003) and Aizer and Grogger (2003) find that parental Medicaid expansions led to increases in overall coverage, with small changes in private coverage. The results in these studies inform our analysis in two ways. First, while the focus of our analysis is the largely unexplored area of health care utilization, we begin by analyzing the impacts of reform on health insurance coverage. Second, our models include controls for these and other changes to state Medicaid and non-Medicaid public health insurance programs.

There are likely many pathways through which welfare reform can affect health. First, as illustrated above, welfare reform may lead to a decline in Medicaid coverage. This is consistent with the observation that families leaving the welfare rolls also appear to stop receiving Medicaid and Food Stamps even when they remain eligible for these programs.[2] This loss in public coverage may be offset by increased private coverage either due to increases in mother's employment or coverage from another family member. These changes in insurance may subsequently impact health care utilization and health outcomes.

Second, welfare reform may impact families' economic resources. While the evidence is more mixed on this point, a number of researchers have found that welfare reform has led to an increase in the average incomes of low-skill families.[3] These changes in families' economic circumstances could then affect health care utilization and health status directly. Third, reform-induced increases in employment will change parents' time endowment, which may affect choices about health care utilization, diet, and health (Haider, Jacknowitz, and Schoeni 2003).

Fourth, welfare reform could lead to increases (or decreases) in stress, which is associated with health outcomes.

To examine this issue, we use data from the Behavioral Risk Factor Surveillance System (BRFSS), a monthly individual survey conducted by states in partnership with the Centers for Disease Control and Prevention (CDC). The BRFSS is designed to produce uniform, state-representative data on preventive health practices and risky behaviors and covers all civilian, noninstitutionalized persons age 18 and over. It is thus important to note that the BRFSS is limited to the adult population; as a consequence we cannot use it to examine the impact of reform on child health. We use the BRFSS data to analyze the impacts of reform on single women aged 20–45 over the period 1990–2000. This time period allows for examination of both state AFDC waivers and state implementation of federal welfare reform (PRWORA). We are able to analyze a wide range of health utilization measures (checkups, breast exams, and Pap smears), and reports of having needed care but not having been able to afford it. We also examine, but we do not report results on, measures of physical and mental health status (self-rated overall health status, days limited from usual activities, and days mental health is not good).[4]

We estimate pooled cross-section models where the impacts of welfare reform are captured by dummy variables for state implementation of welfare waivers and TANF. To focus our analysis on the group likely to be impacted, our main estimates are for single women. We further refine these groups by presenting results separately for blacks, Hispanics, and low-education (high school education or less) single women. All empirical models include controls for state, year, and month fixed effects; state labor market variables; and state programs affecting health care coverage (Medicaid, SCHIP, other state-funded programs). The impacts of welfare reform

in this standard difference-in-differences (DD) framework are identified through variation in the timing and incidence of reform across states. To control for the possible correlation of state welfare policies with unmeasured state trends in health, we use married women as a comparison group and estimate difference-in-differences-in-differences (DDD) models.[5]

Our results generally show that welfare reform is associated with decreases in health insurance coverage and health care utilization, as well as an increase in the likelihood of needing care but finding it unaffordable. In addition, we find no statistically significant association between welfare reform and self reported health status. Importantly, these effects are robust to the source of identification (DD or DDD). The findings suggest that TANF had larger effects than waivers, and that impacts for Hispanics were somewhat larger than impacts for blacks and low-education women. Efforts to relate these larger effects among Hispanics to immigration policy reforms are suggestive but not conclusive.

The remainder of the paper proceeds as follows. Section II describes the changes in welfare programs and their expected effects on health insurance and health outcomes. In section III, we discuss previous literature on welfare reform and health. In section IV and section V, we describe our data and discuss our empirical model. We report results in section VI and then conclude in section VII.

II. Welfare Reform in the 1990s and Implications For Health

Beginning in the early 1990s, many states were granted waivers to make changes to their AFDC programs. As shown in the top panel of Table 1, about half of the states implemented some sort of welfare waiver between 1993 and 1995. On the heels of this state experimentation,

PRWORA was enacted in 1996, replacing AFDC with TANF. While waiver and TANF policies varied considerably across states, overall the reforms are viewed as welfare-tightening and pro-work. More specifically, the welfare-tightening elements of reform include work requirements, financial sanctions, time limits, family caps, and residency requirements.[6] The loosening aspects of reform include liberalized earnings disregards (which promote work by lowering the tax rate on earned income while on welfare), increased asset limits, and expanded eligibility for two-parent families.

During this same period, public health insurance for low-income families was expanding. Historically, eligibility for Medicaid for the non-elderly and non-disabled was tied directly to receipt of cash public assistance. In particular, the AFDC income eligibility limits adopted by a state would also be used for Medicaid, and AFDC conferred automatic eligibility for Medicaid. Thus, a family that received AFDC benefits would also be eligible for health insurance through Medicaid. Conversely, if a family left AFDC, its members generally would lose Medicaid coverage.[7] However, in a series of federal legislative acts beginning in 1984, states were required to expand Medicaid coverage for infants, children, and pregnant women beyond the AFDC income limits, leading to large increases in eligibility (Gruber 1997).

PRWORA further weakened the link between AFDC and Medicaid by requiring states to cover any family that meets the pre-PRWORA AFDC income, resource, and family composition eligibility guidelines (Haskins 2001). This so-called 1931 program (named after the relevant section of the Social Security Act, as amended by PRWORA) also allowed states to expand eligibility for parents beyond the 1996 AFDC/Medicaid limits. Aizer and Grogger (2003) report that by 2001 about half the states had taken advantage of this program and expanded Medicaid access for parents above the welfare income cutoffs. PRWORA also contained language

restricting immigrant access to means-tested transfer programs (including Medicaid). As discussed in Borjas (2003), many states responded by providing immigrant access to Medicaid using newly created, state-funded "fill-in" programs. In 1997, Congress established the State Children's Health Insurance program (SCHIP), which allows states to provide public health insurance to children up to 200 percent of the poverty level (and subsequently to higher levels). Our empirical model includes controls for these expansions.

In the context of these dramatic changes in the U.S. social safety net, we start by discussing the expected effects of these reforms on our target population—women potentially eligible for welfare. There are many pathways through which welfare reform may affect health outcomes.

First, welfare reform reduces welfare caseloads, leading to a decline in Medicaid coverage. The AFDC caseload has declined more than 60 percent since its peak in 1994 (U.S. Department of Health and Human Services 2002). During this time period, the number of nondisabled adults and children on Medicaid also fell. Between 1995 and 1997, the number of nondisabled adults on Medicaid fell by 10.6 percent, with larger reductions among cash welfare recipients (Ku and Bruen 1999). The noncash Medicaid caseload (especially children), on the other hand, grew, reflecting the separation of AFDC eligibility from Medicaid eligibility described above.

This expected loss in public coverage may be offset by increased private coverage due to increases in mother's employment or coverage from another family member. However, these low-skill workers are likely to be employed in industry-occupation cells with traditionally low rates of employer-provided health insurance (Currie and Yelowitz 2000). In sum, the first prediction is that welfare reform should be associated with a decrease in Medicaid coverage, an increase in private insurance, and likely a decrease in overall insurance.

This pathway of decreased insurance coverage can lead to changes in health. A decline in insurance can then lead to less health service utilization—for example less preventive care and prenatal care (Nathan and Thompson 1999). This decline in health care utilization may subsequently impact health outcomes.

Second, welfare reform may impact families' economic resources. While the evidence is less clear on this topic, research suggests that welfare reform has led to an overall increase in the incomes of low-skill families.[8] However, Bitler, Gelbach, and Hoynes (2003b) use experimental data and show that reform has heterogeneous impacts across the income distribution, with some evidence of reductions at the lowest income levels. These changes in a family's economic well-being could then have direct impacts on health care utilization and health status.

Third, increases in employment lead to changes in a parent's time endowment which in turn can affect choices about health care utilization, diet, and health. Fourth, welfare reform could lead to increases (or decreases) in stress, which in turn can affect health.

Discussion of these pathways illustrates that the impacts of welfare reform on health insurance coverage and health care utilization are more direct than the impacts on health status. This interpretation is consistent with the health production model in Grossman (2001). In particular, health is a durable capital stock that will change slowly with investment (time, nutrition, exercise, health services). Health services, on the other hand, are goods consumed each period and therefore would be expected to change more quickly in response to changes in prices, income, and time constraints. Consequently, we focus here on health insurance coverage and health care utilization, with less attention to health status. Identifying the impacts of welfare reform on health status will likely require data from a longer follow-up period.

III. Literature Review

As noted above, the welfare reform literature is very large and is well reviewed elsewhere (Blank 2002; Grogger, Karoly, and Klerman 2002; Moffitt 2002). Here, we focus on the much smaller literature on welfare reform and health. Most studies in this area examine the impacts on health insurance while little is known about impacts on health utilization and health status. The literature comes from three sources: randomized experimental analyses of state AFDC waivers, welfare leaver studies, and nonexperimental analyses of administrative and household survey data.

The first source is experimental analyses of changes to state AFDC programs (waivers). The results in these studies compare outcomes for families randomized into the existing AFDC program to families randomized into a waiver program. Findings from these studies vary, and they also have limitations related to the inability to obtain nationally representative estimates and to account for effects of changes in entry behavior that result from welfare reform.

The second source of information on welfare reform and health is leaver studies—national-level or state-level studies that examine the characteristics of families leaving welfare. The studies uniformly show that welfare leavers experience a decline in insurance coverage. While private insurance coverage increases, public health insurance (such as Medicaid) declines more, leading to an increase in the percent uninsured.[9] These leaver studies provide an excellent snapshot of the experiences of those families that have left welfare. However, as discussed in Blank (2002), such studies cannot identify causal impacts of welfare reform.

The last source of information uses nonexperimental methods to examine the impact of reform on health insurance using administrative or household survey data. Ku and Garrett

(2000) examine the impact of pre-PRWORA welfare waivers on Medicaid caseloads and find

that waivers led to (statistically insignificant) declines in the adult and child Medicaid caseload.[10]

Several recent studies use the Current Population Survey (CPS) to examine the impact of

welfare reform, and other changes to Medicaid, on health insurance coverage. Kaestner and

Kaushal (2003) find that declines in the AFDC caseload are associated with reductions in

Medicaid, increases in employer-provided health insurance, and overall increases in uninsurance.

DeLeire, Levine, and Levy (2003) examine low-education women and find that waivers from the

AFDC program and TANF implementation in states that formerly had waivers are associated

with increases in health insurance.

Borjas (2003) and Royer (2003) find that more restrictive Medicaid policies did not reduce

health insurance coverage among immigrants, because the loss in public coverage was offset by

increases in private insurance coverage. Royer also examines impact on pregnant immigrants and

finds a temporary reduction in prenatal care, but no effect on birth outcomes.

Aizer and Grogger (2003) and Busch and Duchovny (2003) use the CPS to examine parental

Medicaid expansions through the 1931 program. Aizer and Grogger (2003) find that these

Medicaid expansions led to increases in health coverage of women (with some crowdout of

private insurance coverage). They also find that expanding parental coverage leads to increases

in the health insurance coverage of children—possibly arising from an increase in benefits

relative to costs associated with taking up coverage. Busch and Duchovny (2003), in one of the

only other papers to use the BRFSS, find that these parental Medicaid expansions led to increases

in health care utilization, with no significant effects on health status.[11]

Overall, while the evidence is somewhat mixed, it generally suggests that health insurance coverage declines with reform. Below, we replicate this finding and then extend the literature to examine the impacts on several measures of health utilization.

IV. Data

A. BRFSS

We analyze the impact of welfare reform on the health of adult women using data from the Behavioral Risk Factor Surveillance System (BRFSS). The BRFSS is a nationally-representative telephone survey of the civilian, noninstitutionalized adult population conducted by the CDC in partnership with the states, Washington, D.C., and some territories. The BRFSS provides detailed information on health care utilization and health status as well as limited data on health insurance coverage. The BRFSS also provides demographic variables, including age, race and ethnicity, marital status, and education.

Few household or individual survey datasets cover health utilization and health status. For example, the CPS, which is often used in nonexperimental analyses, provides detailed economic and demographic data, but beyond health insurance coverage, it has little health information that spans the period of recent welfare reforms.[12] The National Health Insurance Survey (NHIS) has much more information on health, but the public-use data files do not include the state identifiers that are necessary to create state welfare reform variables, and the survey was redesigned as of 1997, right when reform was implemented in many states.

The BRFSS was first fielded in 1984 in 20 states but now covers all states and is designed to yield uniform, state-representative data on risky behavior and preventive health practices by all persons 18 and older.[13] The BRFSS is administered monthly, and people are equally likely to be

surveyed in each calendar month. For this reason, we do not drop states for which there are no observations in a subset of months within a given year. States must follow CDC-approved methods for sampling, and participating states are required to ask core questions every year. States are also permitted to select questions from modules that are not asked every year, as well as to propose their own questions. The BRFSS is a telephone survey, so households with no phones are excluded.[14]

The BRFSS differs from most other large household or individual surveys along several dimensions. First, no proxy answers are permitted, and only one adult (aged 18 or older) is interviewed per household. These factors may lead to a lower response rates than for other surveys. Further, the BRFSS does not produce fully imputed data, that is, there is no attempt to allocate responses for persons who cannot or refuse to answer specific questions. This item non-response is fairly minor for most of our outcomes of interest, so we simply ignore observations with missing items.

We base our analysis on the BRFSS survey years 1990–2000.[15] For each outcome variable, we restrict the sample to the set of states that asked each question in every year. Since some questions were asked only starting in 1991, 1992, or 1993, and since some states do not include all questions in all years, this selection rule means the set of states in our sample differs slightly across outcomes. While we would prefer a constant sample of states, ceteris paribus, the countervailing advantage of this approach is that we have a balanced panel of states for each outcome. Our estimates therefore may be interpreted as representing average treatment effects for the states in the sample (assuming the usual conditions for this result hold; see Heckman and Robb 1985 for a discussion).[16]

To identify a population at higher risk of being impacted by welfare reform, our empirical analysis is based on the sample of single women aged 20-45. We present separate results for sub-samples of black, Hispanic, and low-education single women.[17] Our low-education sample includes those with a high school education or less. An analysis of single women aged 20–45 using the March CPS in the pre-welfare reform period (1988–1992), shows that about 24 percent of black single women, 18 percent of Hispanic single women, and 19 percent of low-education single women had some AFDC income in the previous year.

Given welfare eligibility rules, the treatment group used in most welfare reform studies is single women with children. While we have re-estimated our models on the sample of single women with children (Bitler, Gelbach, and Hoynes 2004), we concentrate here on all single women because of data limitations in the BRFSS. Specifically, the BRFSS provides marital-status data for all states in all years but its information on presence of children is incomplete. The only variable available for the full 1990–2000 period measures the presence of a child aged 5–13 in the household. However, given the apparent large effects of welfare reform on the employment of women with young children (Meyer and Rosenbaum 2000), dropping women who coreside with children younger than five is undesirable.[18] The number of all children younger than 18 is available, but only for years 1993–2000. Further, California must be dropped from any sample of single women with children because of missing data in 1995. This is especially problematic for interpreting the results for the Hispanic subsample.[19]

We construct three sets of outcome variables concerning health insurance coverage, health utilization, and health status. Respondents are asked about their insurance at the time of the interview. We use this to create an indicator variables for whether the respondent is insured now.

We use this any-coverage variable because the BRFSS has no specific questions concerning source of insurance coverage (such as, Medicaid or employer-provided health insurance).[20] [21]

The health care utilization outcomes we analyze include three separate variables indicating whether a women has had a checkup, breast exam, or Pap smear in the last year. These are important measures of preventive care for adult women. Further, because checkups and breast exams are conducted in the clinician's office and Pap smears require lab work, these measures might respond differentially to changes in insurance status. Another health care utilization measure we examine is whether a woman reports having needed care but not having been able to afford it. As indicated above, we focus on health care utilization because there may not have been adequate time after reform to identify impacts on health status. Nonetheless, the health status outcomes we analyze include whether self-reported health status is fair or poor, the number of days in the last month the respondent reported that her mental or physical heath limited her from her usual activities, and the number of days in the last month her mental health was not good.

We report means for our outcomes and demographic controls in Tables 2 and 3 for each of our three subgroups: black, Hispanic, and low-education single women.[22] The descriptive data show that the health variables vary considerably across our subgroups. Black women are more likely to be covered by health insurance, have higher utilization rates, and are in better health than Hispanic and low-education women. For example, 76 percent of blacks have health insurance, compared to 67 percent of low-education women. A large share of single women said they needed to see a doctor in the previous year and could not afford it (21 percent of black women and about 26 percent of Hispanic and low-education women).

B. Welfare Reform Variables and Other State-Level Controls

We include two kinds of state-level control variables in our models. Our key variables of interest are indicators of whether states have implemented welfare reform. We also include a series of state-level variables in all of our regressions to control for economic opportunities in the state, state welfare benefit generosity, and state generosity in the provision of public health insurance.

Our welfare reform variables can be classified into two categories: those related to state waivers in the pre-PRWORA era and those related to post-PRWORA TANF programs. Our main focus is on simple dummy variables indicating whether or not the given reform—waiver or TANF—is in place (implemented) in a state. Following the convention in the literature, we code a waiver as being in place only if it was "major", in the sense of involving a significant deviation from the state's AFDC program, and if it was in effect statewide. Our primary data source for the dating of state reforms is a set of tables available on the website of the Assistant Secretary for Planning and Evaluation (ASPE) for the Department of Health and Human Services.[23] For TANF, we construct a dummy variable indicating whether the state TANF plan has been implemented. In general, we code states as having implemented a policy in a given month if the policy was implemented by the last day of the previous month.

Some of our outcomes (insurance status and health status) refer to women's conditions contemporaneously. For these variables, our welfare reform variables refer to the policy in place at the beginning of the previous month. By contrast, the health care utilization outcomes measure whether women obtained (or did not obtain) care during the previous year. For these retrospective questions, we construct the state welfare reform variables using the 12-month period before the month before the interview month. If a reform (either waiver or TANF) is

implemented at some time during the 12-month recall period, the reform variable is the fraction of the 12 month period that the reform is in place; otherwise the variable is coded as 0.

To account for variation in economic opportunities, we control for a vector of state-level labor market variables; these variables include current and one-year lags of unemployment and aggregate employment growth rates. We also include the real maximum welfare benefit level for a family of three to control for the state's public assistance program generosity (holding constant welfare reform activity). The time periods used to construct these state-level control variables match the reporting period for each health outcome variable. For example, current health insurance status regressions include this year's benefit level, and this and last year's unemployment rate. The health utilization variables (which refer to last year) include last year's welfare benefit, and one-and two-year lags of the unemployment rate.

We construct four state-level variables to control for expansions in public health insurance coverage during this period: (1) the percent of the federal poverty level at which a pregnant woman loses eligibility for Medicaid coverage; (2) the percent of the federal poverty level at which a 14-year-old child loses Medicaid eligibility; (3) percent of the federal poverty level at which a parent loses eligibility under post-PRWORA 1931 expansions; and (4) the percent of the poverty level at which a child of 14 loses eligibility for SCHIP coverage through a separate or combination state plan (states with Medicaid SCHIP plans are captured with our control for Medicaid eligibility for a child or 14).[24] We control for child health insurance expansions as they may impact women because they reduce the cost of family (or parent-only) coverage.[25]

The means of the state-level and demographic variables are provided for our three samples in Table 3.

V. Empirical Model

A standard approach in the non-experimental welfare reform literature is to use pooled cross-sections and run regressions of outcome measures on demographic covariates, state-level controls, policy variables, and state and year fixed effects. We follow this basic approach.

We estimate linear regression models where y_{ist} indicates an outcome for individual i in state s in year t and has the following form:[26]

(1) $y_{ist} = X_{ist}\delta + L_{st}\alpha + R_{st}\beta + \gamma_s + v_t + \varepsilon_{ist}.$

Here, X_{ist} is a vector of demographic characteristics, including controls for the woman's age and its square, race and ethnicity, and dummy variables for her completed education level. L_{st} is a vector of the state-level labor market variables described above that control for economic opportunities in the state. L_{st} also includes the real maximum welfare benefit level for a family of three and the four measures of the generosity of state's public health insurance describe above. The γ_s terms represent state fixed effects and the v_t terms represent year and calendar month fixed effects. The state (time) fixed effects control for unobserved factors that differ across states and not over time (over time and not across states). Unobservable determinants are captured by ε_{ist}. All regressions are weighted.

Our main focus is on the coefficients of R_{st}, the welfare reform implementation dummies for the waiver and TANF programs. For waiver states, when TANF is implemented the waiver dummy is turned back to 0. Therefore, the reform coefficients represent the treatment effect of reform relative to the counterfactual, no-reform state of the world.

Some observers object that the simple dummy-variable approach taken here assumes that reform effects occur instantaneously at the time of implementation. However, this objection is on

target only if one assumes that reform's effects are constant (over time and across states). In our view, this assumption would be unreasonable even if instantaneous effects could be presumed. Detailed aspects of state reforms and economic conditions are difficult to observe. Moreover, there is no reason to think that different demographic groups will respond to the same reforms in the same way. Given all this, we strongly believe that the coefficients on R_{st} should be interpreted as averages of heterogeneous treatment effects over the post-reform period.

The model in (1) applied to the sample of single women is essentially a difference-in-difference (DD) model specification where the impacts of welfare reform are identified using differences across states in the timing and presence of reform. The top panel of Table 1 reports the first year for which we coded observations in each state as subject to a waiver (the table uses March 1 as the cutoff date for each year). The table also lists the states that never implemented major statewide waivers according to ASPE. It is clear from the table, as well as previous literature, that there is substantial variation in the implementation of state waivers across states and time. Unfortunately for empirical research, variation in TANF implementation was much less extensive—all states implemented their TANF programs within a 16-month period. The bottom panel of Table 1 shows that for all states, the first March of TANF implementation occurred in either 1997 or 1998.

This limited variation is a potentially important complication, as discussed in Bitler, Gelbach, and Hoynes (2003a). Estimated TANF effects estimated with the above methodology can be regarded as the average treatment effect over the 16-month period during which there is variation in TANF implementation status. However, all states implemented by January 1998, so within this period there is no comparison group, and thus no identifying variation, after this month. This

is unimportant if TANF treatment effects are constant over time, in which case the identified estimate for the 16-month period is appropriate for all years. But if one believes TANF impacts vary over time, effect levels or bounds on effects for later years are identified only by making assumptions about secular time effects after all states have implemented. To address this problem, we estimated models with more detailed measures of reform, rather than simply using implementation dummies, as regressors. We discuss this latter approach below, noting for now only that it turns out to be largely unilluminating. In general, we feel that the TANF estimates are best regarded as suggestive, rather than conclusive.

One potential concern that arises in DD-type studies is that even in the absence of policy changes, underlying trends in the outcome variables of interest could lead to spurious estimates of policy effects. To address this concern, we follow the conventional strategy of introducing a comparison group who should not have been affected by welfare reform, leading to a difference-in-difference-in-difference (DDD) estimator. The advantage of this approach is that it provides within-state variation in reform. Thus if the comparison groups are valid, then they net out any within-state trends in health outcomes common to the treatment and comparison groups in a state. Our main DDD results compare single women to married women. We estimate these DDD models in a general way, allowing all parameters in equation (1) to vary across the treatment and comparison groups. The DDD estimate is the difference in the estimated βs between the treatment and comparison groups.[27]

Lastly, we note that our standard error estimates allow arbitrary correlation within state-year cells. Hence, our precision is not spuriously driven by the fact that we use microdata while the policy variation occurs at the state-year or state-month level.

VI. Results

We first present our main findings for the impact of waivers and TANF on the health insurance and health care utilization of single women. In section IV.B we briefly discuss the coefficients on variables other than the reform dummies.

A. Results for Single Women

Our main result are presented in three tables. All models are estimated for single women aged 20-44. The tables present these estimates for blacks (Table 4), Hispanics (Table 5) and those with a high school education or less (Table 6). Each table has the same structure. Coefficients in rows 1 and 4 are the DD estimates of the waiver (row 1) and TANF (row 4) dummies in the regressions for single women. Each of the five columns provides estimates for a different outcome variable. Coefficients in rows 2 and 5 are similarly defined estimates using the comparison group of married women. Rows 3 and 6 provide the DDD estimates. All reported estimates are coefficients from linear probability models, and we provide the pre-reform means for the treatment and comparison groups (rows 9 and 10) to help interpret the magnitude of the estimated treatment effects. The model also includes controls for the age of the woman and its square, race/ethnicity (if applicable), educational attainment (if applicable), state labor market conditions, state public assistance programs (other than reform variables), state fixed effects, year fixed effects, and calendar-month fixed effects.

First consider the DD estimates for the sample of black single women (rows 1 and 4 of Table 4). The results in the first column shows that among black single women, welfare reform leads to

a small negative, statistically insignificant reduction in overall health insurance coverage. The point estimates imply a larger negative effect for TANF.

The next four columns of the table examine the impacts of reform on health care utilization for black single women. Three measures correspond to whether the woman has had preventive services in the past 12 months, including: a checkup, a Pap smear, and a professional breast exam. A negative coefficient for these measures represents an decrease in health care utilization associated with reform. The final utilization measure is a dummy equal to one if the woman reports she needed to see a doctor in the past 12 months but that it was unaffordable. Here a positive coefficient suggests an adverse impact of reform. Looking across the columns, the results consistently show that welfare reform is associated with reductions in health care utilization. For example, the incidence of breast exams and Pap smears falls by about 7–10 percent in the reform period relative to baseline. In addition, the propensity to have needed a doctor's care but to have found it unaffordable increases 7–16 percent relative to baseline.[28]

A well-known disadvantage of the dummy-variable approach to measuring welfare reform is that it does not allow one to discern which aspects of welfare programs are driving the results. For example, it is possible that increases in earnings disregards lead to increases in employment and income, which in turn leads to some improvement in self-rated health. Simultaneously, other, more punitive aspects of welfare reform may drive people off welfare and lead to losses in income and thus self-rated health. We explored models with separate TANF estimates for states with weak, mixed, and strong work incentives, as coded by Blank and Schmidt (2001) and used recently in Schoeni and Blank (2003).[29] Those results, not shown here, are largely insignificant and provide no consistent pattern to help identify specific policy effects. It is possible that some

other index of TANF severity/generosity would provide estimates more in line with expectations. However, in comparing four sets of analysts' approaches to characterizing state sanction policies, Grogger, Karoly and Klerman (2002) find considerable disagreement: the four ratings agree for only 25 states; as an extreme example, Pennsylvania is characterized as lenient by two, moderate by one, and strict by the fourth. Except when there is a very strong theoretical reason to believe that a particular policy will affect a given outcome in a known direction, we are pessimistic that detailed characteristics of reforms can be profitably used in this fashion (Haider, Jacknowitz, and Schoeni 2003).

It is possible that the impacts of reform are, to some extent, capturing state trends in health that are correlated with state waiver and TANF implementation. To address this concern, we estimate DDD models with married women as a comparison group.[30] The results for married women as well as the DDD estimates are provided in Table 4. The DDD results (provided in Row 3 for waivers and Row 6 for TANF) tell much the same story as that for black single women in isolation. While there are some differences in statistical significance (in both directions), the general pattern of the results is a decline in health insurance coverage and health utilization.

Table 5 presents the results for single Hispanic women. Overall, the results for Hispanics show similar patterns to the results for blacks, with estimated effects being somewhat larger in magnitude. Using the DD model, waivers are associated with a (statistically significant) 5 percentage-point reduction in insurance coverage for single Hispanic women, and a (statistically insignificant) 2 percentage-point reduction in the DDD estimates. TANF is associated with a 9 (14) percentage point decline in insurance in the DD (DDD) estimates.   Columns 2-4 in Table 5 show that waivers and TANF also lead to reductions in utilization for Hispanics, although the

waiver effects are less consistent and significant in the DDD models. For example, the DDD results suggest that waivers led to a 1 percentage point increase in checkups while TANF led to a 14 percentage point reduction, while waivers (TANF) led to a 4 (14) percentage point reduction in breast exams. The larger effects on utilization for TANF are consistent with the larger insurance effects of TANF for this group. More generally, the relative magnitudes of the utilization effects compared to the insurance effects seem reasonable.

The estimates in Table 5 show that several of the estimates for the comparison group are significant. Some operate in the same direction as the expected effects of reform (for example, negative impacts of waivers on utilization) and others operate in the opposite direction of reform (for example, an increase in any-coverage following TANF). The fact that there are significant estimates for the comparison group suggests that there are unmeasured state trends that are correlated with welfare reform. Of course, the DDD estimates are appropriate only if married women are a valid control group.

Comparing the black and Hispanic results, the negative impacts of TANF are somewhat larger for Hispanics (the waiver results are more similar). This is consistent with the descriptive evidence cited above on the sharp declines in public insurance coverage among immigrants. It is possible that this occurs because of changes in welfare and Medicaid policies for recent immigrants. We explore this in two ways. We cannot use citizenship status (as in Borjas 2003) because we do not observe citizenship or immigrant status in the BRFSS. Instead, we re-estimated the models in Table 5 adding interactions of the reform variables with the share of Hispanic women aged 20–45 in the state-year cell who are noncitizens (tables not shown here). These noncitizen shares were calculated from the 1990 Census and 1994-2000 CPS and may be regarded as rough estimates of the probability that a given Hispanic woman in the BRFSS

sample is a recent immigrant.[31] While the interactions are rarely significant, their sign suggests that larger effects are present in states with a large share of noncitizen Hispanics. For example, the DD model in Table 5 shows that TANF is associated with a 9.2 percentage point reduction in any insurance coverage. In the interaction model, the estimated TANF reform effect for a hypothetical state with a noncitizen share of zero is -0.058. Every increase of one percentage point in the noncitizen share increases the magnitude of this negative effect by 0.00087. Thus a hypothetical state with a noncitizen share of one would have an estimated effect of -0.145. We also experimented with adding a control for the state immigrant Medicaid "fill-in" programs that were examined by Borjas (2003) and Royer (2003).[32] Adding this variable, however, did not significantly affect the main coefficients of interest. The bottom line is that there is some limited evidence that the findings for Hispanics are explained by the noncitizen share across states. However, because of the lack of data in the BRFSS on citizenship or immigrant status, this finding is at most suggestive.

The final set of estimates for single women is presented in Table 6, where the sample is all single women with a high school education or less. These results are qualitatively similar to the results for black single women. The DD results show insignificant negative impacts on insurance and significant negative impacts on utilization. The DDD results are very similar, although there is some loss in precision in some cases.[33]

C. Other State-Level Controls

Here we summarize, but do not report, the results on the other control variables in the insurance regression. For black single women, expanding Medicaid eligibility beyond AFDC-eligibility levels via 1931 plans is associated with an increase in any-coverage. The coefficient

has the same sign for low-education single women but is insignificant and opposite-signed for Hispanic single women. Higher Medicaid poverty thresholds are associated with an (insignificant) increase in the probability of being insured for black single women. The effects are more mixed for the other groups but again are generally insignificant. The other economic and reform variables have no consistent association with insurance status and the demographic patterns are generally consistent with what one would expect. For details, see Bitler, Gelbach, and Hoynes (2004).

VII. Conclusion

This paper presents estimates of the impact of welfare reform on health insurance coverage and health care utilization during a time of fundamental change in welfare programs, falling AFDC/TANF caseloads, and declines in Medicaid participation. Despite policymakers' evident interest in the insurance status and health of low-income families, to date we know little about how health related variables has been affected by recent state and federal welfare reforms.

We use BRFSS data covering the 1990–2000 period to examine these questions for single women ages 20–45. We estimate models separately for black, Hispanic, and low-education (high school education or less) subgroups. We examine the impact of state waivers and TANF implementation on health insurance, preventive health care utilization (such as checkups, breast exams, and Pap smears), and reports of having needed care but not having been able to afford it.

We present two empirical models for identifying the impacts of reform. We present DD estimates in which the effects of reform are identified through variation in the timing and incidence of reform across states. To account for possible unobserved state trends in health that

might be correlated with reform, we introduce a comparison group (married women) and also estimate DDD models.

The results are generally consistent across the different treatment groups and models. We find that welfare reform is associated with a reduction in insurance coverage. Reform is also associated with a reduction in health care utilization, and an increase in the likelihood of needing care but finding it unaffordable. We find no statistically significant effects of reform on health status. Overall, effects are somewhat larger in magnitude for TANF compared to state waivers, and somewhat larger in magnitude for Hispanics compared to blacks and low-education women.

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1 The welfare reform literature that has developed in the last several years is enormous. For comprehensive summary of this research, see the excellent reviews by Blank (2002); Grogger, Karoly and Klerman (2002); and Moffitt (2002).

2 For a summary of these "welfare-leaver" studies, see Dion and Pavetti (2000), as well as Klein and Fish-Parcham (1999).

3 See the reviews cited above. As noted below, in Bitler, Gelbach, and Hoynes (2003b) we find considerable heterogeneity in the effects of Connecticut's Jobs First waiver on earnings, transfer payments, and income.

4 BRFSS data have been used recently in studies of disability and employment (Carpenter 2003); drinking (Dee 2001, Ruhm 2000, and Ruhm and Black 2002); smoking (Evans, Ringel and Stech 1999 and Gruber and Zinman 2001); risky behaviors (Dee and Evans 2001); and health care utilization and health status (Busch and Duchovny 2003).

5 A more targeted estimation sample would be single women with children. The BRFSS, however, has incomplete data on the presence of children in the family. In our earlier working paper (Bitler, Gelbach, and Hoynes 2004) we present estimates for both single women and single women with children. These data limitations will be explained more fully below.

6 Family cap policies prevent welfare benefits from increasing when a woman gives birth while receiving aid. Residency-requirement policies mandate that unmarried teen parents who receive aid must live in the household of a parent or other guardian.

7 States could and did set up Medically Needy programs that allowed states to provide Medicaid benefits to families above the AFDC income cutoff if they had high medical expenses. States were also required to provide transitional Medicaid coverage for families leaving AFDC due to an increase in earnings.

8 For recent summaries of the experimental and nonexperimental studies of welfare reform and family income, see the reviews by Blank (2002); Grogger, Karoly, and Klerman (2002); and Moffitt (2002).

9 Leaver studies that examine health insurance are reviewed in Greenberg (1998) and include Ellwood and Lewis (1999); Loprest (1999); Danziger et al.(2000); Garrett and Holahan (2000); and Tweedie (2001).

10 Currie and Grogger (2002) and Kaestner and Lee (2003) use natality data and find that declines in welfare caseloads are associated with declines in prenatal care and small increases in the incidence of low birthweight for low-education women.

11 In addition, Kaestner and Tarlov (2003) use the BRFSS to examine the impact of welfare reform on health. They find that welfare reform is associated with a decrease in binge drinking, an increase in exercise, and insignificant effects on overall health status. We received this paper more than a year after completing our first draft using the BRFSS data.

12 Self-reported health status questions were not added to the CPS until the middle of the period we examine.

13 There is a "youth" risk behavioral survey, in which children in grades 9–12 are asked about risky behaviors. Unfortunately, this survey does not ask about health insurance or health care utilization, and the health-related questions change from year to year.

14 While 95 percent of households in the U.S. have phones, coverage is lower for persons living in the south, for some racial groups, and for those in lower socioeconomic groups (U.S. Bureau of the Census 1994).

15 We choose 1990 primarily because the number of participating states increased substantially in that year. As shown in Table 1, our analysis period contains all years when major state welfare reforms were taking place.

16 States that were excluded from all the analysis include D.C. (missing for all of 1995), Rhode Island (missing for all of 1994), and Wyoming (missing for all of 1993). The last missing year for other states was 1990 for Alaska, 1992 for Arkansas, 1991 for Kansas, 1991 for Nevada, and 1990 for New Jersey; thus these states are excluded from analysis of variables that were collected in the years they were missing. Furthermore, states that did not ask a question for part of the period it was asked in most other states were excluded from those samples. A list of states and years in the sample for each outcome is available on request.

17 The black sample includes all non-Hispanic blacks (Hispanics may be of any race). The low-education sample includes members of all racial and ethnic groups and therefore includes some of the women in the black and Hispanic samples.

18 Tabulations of the pre-reform (1989–1992) period using the March CPS show that about one quarter of single women with no child aged 5–13 do have a child under 18, and 14 percent of single women with no child aged 5–13 have a child under 5. Among the group with a child under 18 but no child 5–13, about 20 percent have some AFDC income, compared to 27 percent among single women with a child aged 5–13. Among the group with a child under 5 and no child 5–13, 30 percent have some AFDC income.

19 A second BRFSS shortcoming is that the child variables concern the presence or number of children in the entire household, rather than by parent, so they do not allow for the identification of single women living with own children.

20 In our earlier working paper (Bitler, Gelbach, and Hoynes 2004) we created a proxy for public and private insurance by using current employment status to construct two additional variables: insured and working and insured and not working.

21 The BRFSS health insurance survey question differs from the questions in the CPS. In the CPS, respondents are asked about coverage through different sources and uninsurance is then calculated as a residual. In the BRFSS it is directly measured. An additional advantage of the BRFSS over the CPS is that the design of the questions has not changed over this period (Swartz 1997). Lastly, there is some debate about whether CPS respondents respond as though the reference period for the health insurance questions is the preceding year or the time of the survey (for example, see Swartz 1986; and Bennefield 1996)

22 Recall that the number of non-missing observations varies across the outcome variables. The means in Tables 2 and 3 are for the superset of observations that are defined for any of the health

outcomes of interest and also have data for all the right-hand side control variables. The number of observations for each health outcome used is reported in the tables of regression results.

23 Specifically, these tables classify a waiver as "major" only if it related to one of the following policies: termination time limits, work exemptions, sanctions, increased earnings disregards, family caps, or work requirement time limits (Crouse 1999). More specific details regarding our construction of reform variables are available on request in a data appendix.

24 The first two variables control for the Medicaid expansions of the late 1980s through the early 1990s and are based on National Governor's Association Maternal and Child Health Updates and information generously provided by Aaron Yelowitz. The 1931 expansion variable is based on data from Aizer and Grogger (2003), Busch and Duchovny (2003), the National Governor's Association, and the State Policy Documentation Project. The SCHIP variable is based on data from the websites of the Centers for Medicare and Medicaid and the National Governor's Association. State Medicaid 1931 plans that did not expand eligibility above the former AFDC program-eligibility cutoffs are captured by our control for state-level maximum AFDC/TANF benefits. Benefit levels are quite collinear with the AFDC income-eligibility thresholds for 1996 and the statutory 1931 minimum eligibility threshold, states' 1988 AFDC income-eligibility cutoffs.

25 For outcome variables that are measured at the time of the survey, we use the expansion variables measured as of the policy last month. For the health-utilization variables, which refer to last year, we use the average of the policy variables over the last year.

26 The outcome variables examined here are primarily indicator variables. Sensitivity checks show that the estimates are not sensitive to the linear probability model assumptions.

27 In our working paper (Bitler, Gelbach, and Hoynes 2004), we also present results for single women with children when we consider two additional comparison groups: single women without children and married women with children.

28 In results not reported here, we analyze self reported health and days limited and find no significant effects of welfare reform on health status. About half the point estimates suggest a deterioration in health with reform while the other half suggest improvements in health with reform. These insignificant results (which are reported in Bitler, Gelbach, and Hoynes 2004) are found for all subgroups and for DD and DDD models. As described in Section II, this is not surprising given that health status is a stock variable and as such, may take more time to respond to changes in prices and income. In fact, other recent studies on health and public programs have similar findings (Royer 2003; Kaestner and Tarlov 2003).

29 Blank and Schmidt (2001) code strength of work incentives according to state-benefit generosity as well as state policies on sanctions, time limits, and work requirements.

30 We also estimated models (not reported here) controlling for state-specific linear time trends; the results are quantitatively and qualitatively similar to those discussed above.

31 The share of women who are not citizens is constructed from the March CPS for 1994–2000, from the 1990 Census for 1990, and is a linear interpolation of the 1990 and 1994 values for intervening years. The March CPS does not provide data on citizenship status before 1994.

32 Specifically, Tumlin, Zimmerman, and Ost (1999) tabulate state safety-net policies for immigrants after enactment of PRWORA. We create a dummy variable indicating whether each state has a state-funded Medicaid-like program for immigrants who entered the U.S. after August 22, 1996 and were thus ineligible for federally-funded Medicaid for five years or for the state providing state-funded care to some unqualified immigrants also made ineligible for Medicaid post-PRWORA. These state fill-in plans should have been protective for unqualified immigrants and for entrants after August 1996.

33 As described above, data limitations in the BRFSS led us to choose all single women as the preferred treatment group. Despite those limitations, in results not presented here, we estimate DD and DDD models for single women with children (see Bitler, Gelbach, and Hoynes 2004 for the full set of results). The comparison groups for the DDD models are single women without children and married women with children. In these models, due to missing data, we limit the sample to 1993–2000 and drop California from the analysis. Because of the importance of California in the Hispanic sample, we do not examine results for single Hispanic women with children. Overall, the results are qualitatively similar to those presented above.

Table 1
State Implementation of AFDC Waivers and TANF Programs, by March 1

	Ever had a waiver:					Never had a Waiver
	1993	1994	1995	1996	1997	
First Year for which major waiver implemented by March 1	California Michigan New Jersey Oregon Utah	Georgia Illinois Iowa	Arkansas South Dakota Vermont	Arizona Connecticut Delaware Indiana Massachusetts Mississippi Missouri Montana Virginia Washington West Virginia Wisconsin	Hawaii	Alabama Florida Kansas Kentucky Louisiana Maine Nevada New Hampshire Oklahoma South Carolina Wyoming Alaska Colorado DC Idaho Minnesota New Mexico New York North Dakota Pennsylvania Rhode Island
	1993	1994	1995	1996	1997	1998
First Year for which TANF implemented by March 1					Alabama Florida Kansas Kentucky Louisiana Maine Nevada New Hampshire Oklahoma South Carolina Wyoming Arizona Connecticut Georgia Indiana Iowa Maryland Massachusetts Michigan Missouri Montana Nebraska North Carolina Ohio Oregon South Dakota Tennessee Texas Utah Vermont Virginia Washington West Virginia	Alaska Colorado DC Idaho Minnesota New Mexico New York North Dakota Pennsylvania Rhode Island Arkansas California Delaware Hawaii Illinois Mississippi New Jersey Wisconsin

Note: See text for data sources and explanation.

Table 2
BRFSS Sample Summary Statistics for Outcome Variables, All Single Women 20-45

	Subsample:		
	Black	Hispanic	Low Education
Have any health insurance (private or public)	0.763 (0.425)	0.633 (0.482)	0.673 (0.469)
Last checkup was in the last 12 months	0.843 (0.363)	0.699 (0.459)	0.718 (0.450)
Needed to see a doctor, could not afford to in the last year	0.207 (0.406)	0.261 (0.439)	0.265 (0.441)
Last Pap smear was in the last year	0.798 (0.402)	0.654 (0.476)	0.691 (0.462)
Last professional breast exam was in the last year	0.722 (0.448)	0.566 (0.496)	0.631 (0.482)
General health is fair or poor	0.143 (0.350)	0.195 (0.396)	0.172 (0.378)
Days in last month poor health kept respondent from usual activities	1.983 (5.605)	1.922 (5.505)	2.299 (6.139)
Days in last month poor mental health	4.575 (8.470)	4.800 (8.321)	5.532 (9.192)
Maximum available sample size	26,527	12,680	60,872

Note: Tabulations from the BRFSS 1990–2000 with standard errors in parentheses. Weighting is based on *finalwt* variable. Sample is all single women 20–45 in the subgroup defined in the column label for whom the variables are reported. Black subgroups all defined as non-Hispanic. Low-education denotes less than or equal to a high school education. Sample size is smaller than maximum possible because not all states collect data on every question in every year. Actual sample sizes correspond to Ns reported in regression results for each health outcome. See text for more information.

Table 3
BRFSS Sample Summary Statistics for Control Variables, All Single Women 20–45

	Subsample:		
	Black	Hispanic	Low education
Waiver implemented	0.138 (0.345)	0.224 (0.417)	0.154 (0.361)
TANF implemented	0.355 (0.478)	0.380 (0.486)	0.344 (0.475)
Share of last year, waiver implemented	0.141 (0.320)	0.228 (0.390)	0.151 (0.330)
Share of last year, TANF	0.311 (0.445)	0.334 (0.453)	0.302 (0.442)
State fill-in Medicaid like program for ineligible immigrants	0.185 (0.389)	0.225 (0.417)	0.189 (0.391)
Income limit (percent of FPL), 1931 or 1115 family Medicaid eligibility	7.4 (33.8)	9.6 (35.8)	10.4 (39.1)
Income limit (percent of FPL), pregnant women's Medicaid eligibility	173.9 (37.4)	189.4 (41.4)	176.4 (39.0)
Income limit (percent of FPL), child of 14's Medicaid eligibility	51.6 (74.9)	49.9 (60.1)	51.2 (72.9)
Income limit (percent of FPL), child of 14's SCHIP eligibility	32.8 (76.2)	50.2 (92.5)	34.8 (79.2)
Share of non-citizen women 20–45, race/ethnicity group	0.043 (0.063)	0.409 (0.145)	0.107 (0.174)
Real maximum AFDC/TANF benefits, family of three	4.485 (1.884)	5.780 (2.261)	5.083 (2.050)
State unemployment rate	5.561 (1.413)	6.050 (1.591)	5.674 (1.550)
State employment growth rate	1.865 (1.443)	2.029 (1.644)	1.886 (1.564)
Age	31.2 (7.5)	29.9 (7.5)	31.0 (7.6)
High school dropout, no GED	0.121 (0.326)	0.276 (0.447)	0.263 (0.440)
High school diploma or GED only	0.373 (0.484)	0.299 (0.458)	0.737 (0.440)
Some college/technical school, no 4 year degree	0.334 (0.472)	0.283 (0.451)	0.000 (0.000)
Maximum available sample size	26,527	12,680	60,872

Note: Tabulations from the BRFSS 1990–2000 with standard errors in parentheses. Weighting is based on *finalwt* variable. Sample is all single women 20–45 in the subgroup defined in the column label for whom the variables are reported. Black subgroups all defined as non-Hispanic. Low-education denotes less than or equal to a high school education.

Table 4
Results for Black (Non-Hispanic) Sample

	Insurance Coverage	Health Care Utilization			
		Checkup	Pap Smear	Breast Exam	Needed Care, Unaffordable
Waivers					
Single	-0.003	-0.006	-0.056***	-0.054***	0.033*
	(0.016)	(0.018)	(0.017)	(0.021)	(0.019)
Married	0.009	0.028	0.011	0.023	0.002
	(0.020)	(0.025)	(0.027)	(0.029)	(0.022)
Difference	-0.012	-0.035	-0.067**	-0.077**	0.031
	(0.026)	(0.030)	(0.032)	(0.035)	(0.029)
TANF					
Single	-0.017	-0.018	-0.064**	-0.071**	0.015
	(0.021)	(0.034)	(0.029)	(0.036)	(0.032)
Married	0.031	0.097**	0.026	-0.019	-0.013
	(0.033)	(0.043)	(0.038)	(0.037)	(0.037)
Difference	-0.048	-0.115**	-0.091*	-0.052	0.028
	(0.040)	(0.055)	(0.048)	(0.051)	(0.049)
Number of Observations					
Single	24,091	24,956	23,687	24,381	24,087
Married	10,475	10,937	10,333	10,737	10,474
Pre-reform Mean					
Single	0.753	0.849	0.809	0.737	0.211
Married	0.850	0.857	0.839	0.799	0.167

Note: ***, **, and * indicate statistical significance at the 1, 5, and 10 percent levels, respectively. All figures are weighted OLS coefficients (using BRFSS *finalwt* variable), with robust variance estimates to account for state-by-year clustering. Each column presents estimates from a separate regression. Sample includes all black non-Hispanic single women ages 20-45 from the 1990-2000 BRFSS. Regressions also include controls for demographic variables, state labor market variables, other state policy variables, and state, year, and calendar month fixed effects. See text for details.

Table 5
Results for Hispanic Sample

| | Insurance Coverage | Health Care Utilization: | | | |
		Checkup	Pap Smear	Breast Exam	Needed Care, Unaffordable
Waivers					
Single	-0.050**	-0.049*	-0.044	-0.081**	0.078**
	(0.025)	(0.029)	(0.029)	(0.035)	(0.036)
Married	-0.030	-0.057**	-0.046**	-0.041	0.038*
	(0.020)	(0.025)	(0.019)	(0.027)	(0.019)
Difference	-0.020	0.007	0.003	-0.040	0.040
	(0.032)	(0.038)	(0.035)	(0.044)	(0.041)
TANF					
Single	-0.092***	-0.074	-0.102**	-0.069	0.050
	(0.035)	(0.046)	(0.041)	(0.050)	(0.052)
Married	0.046**	0.064*	-0.020	0.065	0.047
	(0.022)	(0.035)	(0.026)	(0.042)	(0.043)
Difference	-0.139***	-0.138**	-0.082*	-0.135**	0.003
	(0.041)	(0.058)	(0.048)	(0.065)	(0.068)
Number of observations					
Single	11,726	11,697	11,563	11,409	11,719
Married	13,258	13,328	13,105	13,012	13,257
Pre-reform Mean					
Single	0.661	0.749	0.668	0.596	0.255
Married	0.691	0.759	0.729	0.665	0.253

Note: ***, **, and * indicate statistical significance at the 1, 5, and 10 percent levels, respectively. All figures are weighted OLS coefficients (using BRFSS *finalwt* variable), with robust variance estimates to account for state-by-year clustering. Each column presents estimates from a separate regression. Sample includes all Hispanic single women ages 20-45 from the 1990-2000 BRFSS. Regressions also include controls for demographic variables, state labor market variables, other state policy variables, and state, year, and calendar month fixed effects. See text for details.

Table 6
Results for Low-Education Sample (All Races/ Ethnicities)

	Insurance Coverage	Health Care Utilization			
		Checkup	Pap Smear	Breast Exam	Needed Care, Unaffordable
Waivers					
Single	-0.002	-0.044***	-0.025	-0.056***	0.014
	(0.012)	(0.013)	(0.017)	(0.019)	(0.017)
Married	-0.010	-0.025**	-0.024*	-0.008	0.022**
	(0.009)	(0.012)	(0.013)	(0.013)	(0.011)
Difference	0.008	-0.019	0.000	-0.048**	-0.007
	(0.014)	(0.018)	(0.021)	(0.023)	(0.020)
TANF					
Single	-0.030	-0.055**	-0.052*	-0.069**	0.037
	(0.021)	(0.025)	(0.027)	(0.031)	(0.029)
Married	0.007	0.020	-0.025	-0.010	0.018
	(0.014)	(0.023)	(0.023)	(0.022)	(0.024)
Difference	-0.036	-0.075**	-0.027	-0.059	0.019
	(0.025)	(0.034)	(0.035)	(0.039)	(0.037)
Number of observations					
Single	54,787	56,165	53,902	54,045	54,743
Married	73,050	76,049	72,106	73,120	73,009
Pre-reform Mean					
Single	0.692	0.725	0.703	0.648	0.255
Married	0.818	0.722	0.711	0.696	0.186

Note: ***, **, and * indicate statistical significance at the 1, 5, and 10 percent levels, respectively. All figures are weighted OLS coefficients (using BRFSS *finalwt* variable), with robust variance estimates to account for state-by-year clustering. Each column presents estimates from a separate regression. Sample includes all single women ages 20-45 with a high school degree or less from the 1990-2000 BRFSS. Regressions also include controls for demographic variables, state labor market variables, other state policy variables, and state, year, and calendar month fixed effects. See text for details.