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ABSTRACT

We investigate the relationship between welfare reform and health insurance, health care utilization, and self-reported measures of health status for women aged 20-45, using nationally representative data from the Behavioral Risk Factor Surveillance System. We present estimates from both difference-in-difference models (applied to single women and single women with children) and difference-in-difference-in-difference models (using married women and single women without children as comparison groups). 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. We find no statistically significant effects of reform on health status. Overall, effects are somewhat larger for Hispanics compared to blacks and low educated women.

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1 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 health utilization and health status 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 & 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 & Duchovny (2003) and Aizer & 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 status and health care utilization, we begin by analyzing the impacts of reform on health insurance coverage. Second, our models include controls for these and

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

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 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. 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 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 construct a wide range of health measures including preventive health care utilization (e.g., checkups, breast exams, Pap smears), and 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

[2]For a summary of these "welfare-leaver" studies, see Dion & Pavetti (2000) and Families USA Foundation (1999).

[3]See the reviews cited above. As noted below, in Bitler, Gelbach & Hoynes (2003*a*) 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 & Black (2002)); smoking (Evans, Ringel & Stech (1999) and Gruber & Zinman (2001)); risky behaviors (Dee & Evans (2001); and health care utilization and health status (Busch & Duchovny (2003)).

dummy variables for state implementation of welfare waivers and TANF. To focus our analysis on groups likely to be impacted, we present results for two groups: all single women and single women with children.[5] We further refine these groups by presenting results separately for blacks, Hispanics, and low-education (high school education or less) 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 introduce comparison groups and estimate difference-in-differences-in-differences (DDD) models. We present estimates using several different comparison groups including married women and single women without children.

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. We generally find no statistically significant association between welfare reform and health status. These effects are typically, though not uniformly, robust to choice of sample (single women or single women with children) and source of identification (DD or DDD). An exception to the overall robustness is that the results for low-education single women with children are almost all insignificant. For health insurance, our results generally suggest that welfare waivers (and sometimes TANF) are associated with a reduced probability of simultaneous employment and insurance coverage. Unfortunately, we do not have a satisfactory explanation for this result, which runs contrary to what we would expect. Overall, 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 2 describes the changes in welfare programs and their expected effects on health insurance and health outcomes. In section 3, we discuss previous literature on welfare reform and health. In section 4 and section 5, we describe our data and discuss our empirical model. We report results in section 6 and then conclude in section 7.

[5]We examine impacts both on all single women and on the more natural welfare-eligible group of single women with children because the BRFSS has incomplete data on the presence of children. This issue will be explained more fully below.

2 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 one-for-one 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 or adjunctive eligibility for Medicaid. Thus, a family that received AFDC benefits would also be be eligible for health insurance through Medicaid. Conversely, if a family left AFDC, its members also 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 & 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

[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] This is not precisely correct. 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.

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. One representation of these pathways is presented in Figure 1.

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 & 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 (e.g., Currie & 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. A decline in insurance can lead to less health service utilization—for example less preventive care and prenatal care (Nathan & Thompson (1999), Lyons (1999))—and 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 et al. (2003*a*) 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

[8]For recent summaries of the experimental and nonexperimental studies of welfare reform and family income, see the reviews by Blank (2002), Grogger et al. (2002), and Moffitt (2002).

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.

3 Literature Review

As noted above, the welfare reform literature is very large and is well reviewed elsewhere (Blank (2002), Grogger et al. (2002), and 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 more broadly. 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 the waiver program. In their review of the experimental literature, Grogger et al. (2002) conclude that in the majority of studies, reform is associated with decreases in women's health insurance coverage, with smaller declines for children. Findings vary, however, with some studies reporting an increase or no statistically significant change in health insurance. Some studies examine health utilization and health status for children (e.g., Fraker, Ross, Stapulonis, Olsen, Kovac, Dion & Rangarajan (2002) and Morris, Huston, Duncan, Crosby & Bos (2001)) but little evidence is available for women. The main limitations of experimental studies are the inability to obtain nationally representative estimates and to account for effects of changes in entry behavior that result from welfare reform. Further, information on health outcomes come almost exclusively from surveys that are conducted on subsamples of the experimental sample and can have low response rates.

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 (e.g., 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 for several reasons. First, leavers are a selected sample. The typical leaver

[9]Leaver studies that examine health insurance are reviewed in Greenberg (1998) and include Ellwood & Lewis (1999), Loprest (1999), Danziger, Corcoran, Danziger & Heflin (2000), Garrett & Holahan (2000), and Tweedie (2001).

study examines families leaving welfare in the mid-1990s after welfare caseloads have already fallen by 20 to 30 percent. Second, leaver studies cannot examine the impacts on nonentrants—those families that would have gone on aid prior to welfare reform but did not. Recent work suggests that this is an important channel through which the caseload has fallen (Grogger, Haider & Klerman (2003)). Third, one cannot tell why families left welfare and who would have left absent reform. Fourth, examining a single cohort of leavers makes it impossible to separately identify impacts of leaving welfare from other events occurring at the same time such as the strengthening labor market and other policy changes (e.g., expansion of the EITC).

The last source of information uses nonexperimental methods to examine the impact of reform on health insurance using administrative or household survey data. Following the literature on welfare reform and AFDC/TANF caseloads, Ku & Garrett (2000) examine the impact of welfare reform on Medicaid caseloads. In particular, they pool state-level administrative data on Medicaid caseloads from the pre-PRWORA period and examine the impact of welfare waivers on adult and child Medicaid caseloads. The results show that AFDC waivers lead to a (statistically insignificant) decline in the 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 & 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 & 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) uses a DDD strategy to examine the impacts of welfare reform on health insurance in the immigrant population. His source of identification rests on cross-sectional differences in the generosity of state-only "fill-in" programs to replace benefits for immigrants made ineligible for federal-funded public assistance programs. Borjas finds that immigrants who are not citizens and lived in less-generous fill-in states were considerably less likely to be covered by Medicaid in the post-reform era. However, this decrease in Medicaid coverage was accompanied by a significant increase in employer-provided coverage, resulting in no overall decline in insurance for immigrants over the time period. Royer (2003) has a similar finding in an analysis of pregnant immigrants. Royer also uses Natality

[10]Currie & Grogger (2002), using Natality data for 1990–1996, find that declines in welfare caseloads are associated with declines in prenatal care. Also using Natality data, Kaestner & Lee (2003) find that reform is associated with a small decline in the use of prenatal care and a small increase in the incidence of low birthweight for low-education women.

data to look at health care utilization and health status and finds a temporary reduction in prenatal care, but no affect on birth outcomes.

Aizer & Grogger (2003) and Busch & Duchovny (2003) use the CPS to examine parental Medicaid expansions through the 1931 program. Aizer & 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 & Duchovny (2003), in one of the only 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 health utilization and health status.

4 Data

4.1 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 has the range of demographic variables usually provided on household and individual survey data, 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

[11] More than a year after completing our first draft, we received a copy of Kaestner & Tarlov's (2003) paper, which uses 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.

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

necessary to create state welfare reform variables.

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, i.e., 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 sample of women aged 20–45 from 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, *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 & Robb (1985) for a discussion).[16]

We construct subsamples to reflect groups at higher risk of being impacted by welfare reform.

[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. Furthermore, the nature of the health-related questions in the youth survey changes from year to year, making it difficult to construct consistent measures over time.

[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 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.

Given welfare-eligibility rules, the treatment group used in most studies is single women with children. While the BRFSS provides marital-status data for all states in all years, its information on presence of children is incomplete. First, the only information available for the full 1990–2000 period concerns the presence of a child aged 5–13 in the household. Given the apparent large effects of welfare reform on the employment of women with young children (Meyer & Rosenbaum (2000)), dropping women who coreside with children younger than five is undesirable.[17] The number of all children younger than 18 is available, but only for years 1993–2000. This restricts the sample significantly, especially for analyzing the waiver period. We take the more comprehensive approach of considering children aged 0–17. This approach does come with a cost, as we have to drop all years without comprehensive data on children. 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. Because of these issues, we present results both for the full set of single women (all years for which data are otherwise available) and for the subset of single women living with children (only 1993–2000, for which complete data on presence of children are available).

We further refine our sample of single women and present separate results for black, Hispanic, and low-education women.[18] Low-education women include 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. Unfortunately, the California BRFSS data for 1995 do not include any information on children, and therefore California is dropped from the sample of single women with children. Because of the importance of California for the Hispanic sample, we do not present results for the Hispanic subsample of single women with children.

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

[17] 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.

[18] 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.

insured and not working (thus the first variable is the sum of the other two). We choose these variables because the BRFSS has no specific questions concerning source of insurance coverage (e.g., Medicaid or employer-provided health insurance).[19]

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. 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 select these outcomes both because they were available in a large cross-section of states and because they are potentially important indicators of health or stress.

We report means for our outcomes and demographic controls in Tables 2 and 3.[20] We report means for our five subgroups: black, Hispanic, and low-education single women, and black and low-education single women with children. 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. Among single women with children, 76 percent of blacks have health insurance, compared to 68 percent of low-education women. A large share of single mothers said they needed to see a doctor in the previous year and could not afford it (27 percent of the low-education group and 22 percent of blacks).[21]

[19]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 (e.g., see Swartz (1986) and Bennefield (1996)).

[20]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.

[21]One should use caution in using this table to make comparisons between the means for all single women and single women living with children because the sample period varies across these subgroups, and because California data are included in the former but not the latter.

4.2 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.[22] 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, namely 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

[22]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.

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).[23] We control for child health insurance expansions as they may impact women because they reduce the cost of family (or parent-only) coverage.[24]

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

5 Empirical Model

A standard approach in the nonexperimental 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:[25]

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

[23]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 & Grogger (2003), Busch & 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.

[24]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.

[25]As described above, the outcome variables examined here are primarily indicator variables such as insurance status and poor/fair health. Sensitivity checks show that the estimates are not sensitive to the linear probability model assumptions.

Here, X_{ist} is a vector of demographic characteristics, including controls for the person'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 ν_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.

Given this DD model specification, 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 researchers, 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 & Hoynes (2003*b*). 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 the treatment group 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.[26] To try to address this problem, we tried using 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 try to address this concern, we follow the conventional strategy of introducing comparison groups of women who should not have been affected by welfare reform. We consider two basic comparison groups: married women, and single women without children. 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. We estimate 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.

Lastly, we note that we adjust our standard errors to 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.

6 Results

In the next subsection, we report estimates for our first treatment group—all single women in a given demographic group. We report results for the subset of single women living with children in subsection 6.2. In subsection 6.3 we briefly discuss the coefficients on variables other than the reform

[26] In Bitler et al. (2003*b*), we argued that with household income, bounds on treatment-effect heterogeneity are identified because one can reasonably assume that time effects on household income are non-negative (since one would not expect income in the absence of welfare reform to be falling over the January 1998–March 2000 period, given the very strong economic performance of that time). Unfortunately, we do not have such strong priors on trends for the outcomes we consider in this paper. An alternative approach, which we do not pursue here, would be to compare estimated coefficients on year dummies across the treatment and comparison groups for years after all states have implemented TANF.

dummies.

6.1 Results for single women

Results for blacks, low-education women, and Hispanics are reported in Tables 4–6. 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 nine columns then provides estimates for a different outcome variable. Coefficients in rows 2 and 5 are similarly defined estimates for a separate regression estimated using the comparison group—all married women of the given demographic group. 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 three columns show that among black single women, welfare reform leads to small negative but statistically insignificant reductions in overall health insurance coverage. The point estimates imply a larger negative effect for TANF. The negative TANF effect comes from a reduction in the propensity to be insured and not employed that is larger than the increase in the propensity to be insured and employed.

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 positive coefficient for these measures represents an increase 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.

The last three columns report results for the health status variables: self-rated health (equal to

one if health is fair or poor), days limited from usual activities, and days in poor mental health. A positive coefficient for any of these measures implies a adverse impact of reform. The medical-outcomes literature provides very compelling evidence that self-rated health is an important predictor of health outcomes (such as mortality). While the mechanism is not clear, it is clear that self-rated health is an independent predictor of mortality even when controlling for background and other health status variables (Idler & Benyamini (1997)). The results show no significant effects 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.

Overall, the DD results for single black women show that reform is associated with an insignificant decline in insurance coverage, significant declines in utilization, and insignificant and mixed impacts on health status. This pattern of stronger findings for utilization compared to health status is consistent with the health production model in Grossman (2001). In particular, health is a durable capital stock that changes slowly with investment (time, nutrition, exercise, health services). Health services, on the other hand, are goods purchased each period and therefore would be expected to change more quickly in response to changes in prices, income, etc. These varying results across utilization and health status have also been found in other recent studies on health and public programs (Royer (2003) and Kaestner & Tarlov (2003)).

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 & Schmidt (2001) and used recently in Schoeni & Blank (2003).[27] 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 et al. (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

[27] Blank and Schmidt code strength of work incentives according to state-benefit generosity as well as state policies on sanctions, time limits, and work requirements.

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.

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. We estimate models (not shown) using state-specific linear time trends; the results are quantitatively and qualitatively similar to those discussed above. As noted, we also estimate DDD models in which the comparison group for single women is married women. The results for married women as well as the DDD estimates are provided in Table 4. The DDD results 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, with mixed and insignificant effects for health status.

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 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. Contrary to expectations, however, reform is associated with a reduction in being employed and insured. Waivers and TANF also lead to reductions in utilization for Hispanics, although the waiver effects are less consistent and significant in the DDD models. 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 results in the final three columns show more consistent adverse impacts on health status, but again the results are not significant.

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 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.[28] While the interactions are rarely significant, their sign would suggest 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).[29] 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, significant negative impacts on utilization, and no significant effects for health status. The DDD results are very similar, although there is some loss in precision in some cases.

6.2 Results for single women living with children

We next turn to our second treatment group—single women with children. For this treatment group, we consider two comparison groups—married women with children and single women without children. We remind readers of two cautions before presenting these results. First, the BRFSS measures the presence of any children in the household rather than the presence of own children. Second, selecting households with children requires limiting the sample to 1993–2000 and dropping California from the analysis.[30] We do not present results for single Hispanic women with children because of the large

[28]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.

[29]Specifically, Tumlin, Zimmerman & 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.

[30]As noted above, the BRFSS does not provide the children-related data we need for California in 1995. Because 1995 is such an important year in a welfare reform study (it comes the year before PRWORA is enacted, at a time when

share of Hispanics living in California who are excluded from those regressions.

Results for black single women with children are presented in Tables 7 and 8. Table 7 presents results with married women with children as a comparison group. These results are quite similar to the results for single black women. The DDD results show that reform is associated with reductions in insurance and utilization, and mixed and largely insignificant effects for health status. For example, waivers are associated with an insignificant 1.9 percentage-point reduction in insurance coverage, while TANF is associated with a significant 10.5 percentage point reduction in insurance coverage. Again, however, this reduction in insurance is partially driven by a reduction in the likelihood of being simultaneously employed and insured. Unfortunately, we do not have a satisfactory explanation for this result, which runs contrary to what we would expect. Checkups, Pap smears, and breast exams all decrease with reform, while needing to see a doctor but finding it unaffordable increases (insignificantly).

Table 8 presents results with black single women living without children as the comparison group. The results for insurance and health status are very similar to the results using married women with children as the comparison group. However, because of declines in preventive health care utilization for the comparison group, the DDD estimates for utilization are somewhat different.

The final results examine impacts on low-education single women with children. We use married women with children as the comparison group in Table 9 and single women without children for Table 10. Comparing these tables to the findings for blacks, or for all low-education single women, leads to several observations. First, most coefficients are statistically insignificant, though a few are significant. This is true in both the DD and the DDD models. Second, there is no consistent pattern in the results. Some point estimates imply decreases in insurance and utilization, others imply the opposite patterns.

6.3 Other state-level controls

We briefly summarize the coefficients on the other controls for insurance coverage, health care utilization, and health status. Appendix Table 1 contains the coefficients on all of the other controls except for the (state, year, and calendar-month) fixed effects for the insurance coverage regressions estimated among single black women. For comparison's sake, this table repeats the point estimates for the main reform variables. For black single women, expanding Medicaid eligibility beyond AFDC-eligibility

numerous states have recently enacted waivers), we feel it is better to drop California and maintain a balanced panel than to include the state without this pivotal year.

levels via 1931 plans is associated with an increase in coverage. The coefficient has the same sign for low-education single women (not shown) but is insignificant and opposite-signed for Hispanic single women (not shown).

Higher Medicaid poverty thresholds are associated with an increased probability of being insured and employed for black single women; effects are more mixed for the other groups and are generally insignificant. The other economic and reform variables have no consistent association with insurance status with the following exceptions: among Hispanics, unemployment rates are strongly negatively associated with the probability of being insured and working; among both Hispanics and low-education women, unemployment rates are positively associated with the probability of being insured and not working.

The demographic patterns are generally consistent with what one would expect. Older women are more likely, and lower-education women less likely, to have coverage. Among low-education women, blacks are more likely than women of other groups to be insured, and Hispanics less likely. Whites are more likely to be working and insured and less likely to be not working and insured.

Findings for health care utilization and health status (not shown) are quite mixed, with one exception: the number of days in the last month with poor mental health is positively associated with the unemployment rate for black and Hispanic single women, and negatively associated with the employment growth rate for low-education women.

7 Conclusion

This paper presents estimates of the impact of welfare reform on health insurance coverage, health care utilization, and health status 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 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 and single women with children 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 (e.g., checkups, breast exams, and Pap smears), self-rated health status, days limited from usual activities, and number of days with poor mental health.

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 comparison groups (married women, single women without children) 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 (though these results do not conform to expectations when we look at simultaneous coverage and employment). 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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Figure 1
Expected Effects of Reform on Health

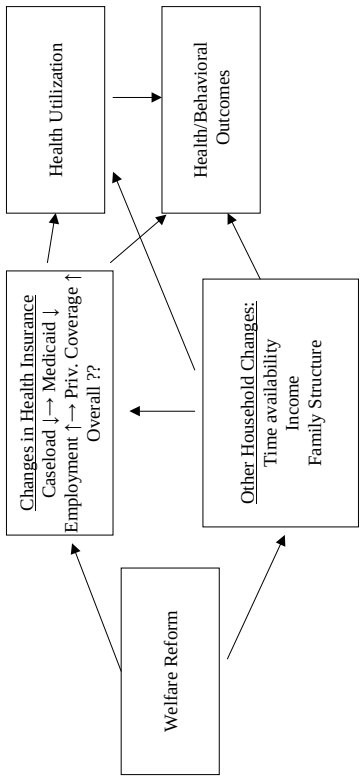

Table 1: State implementation of AFDC waivers and TANF programs, by March 1

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

	1997	1998
First Year for which TANF Implemented by March 1	Alabama	Alaska
	Florida	Colorado
	Kansas	DC
	Kentucky	Idaho
	Louisiana	Minnesota
	Maine	New Mexico
	Nevada	New York
	New Hampshire	North Dakota
	Oklahoma	Pennsylvania
	South Carolina	Rhode Island
	Wyoming	Arkansas
	Arizona	California
	Connecticut	Delaware
	Georgia	Hawaii
	Indiana	Illinois
	Iowa	Mississippi
	Maryland	New Jersey
	Massachusetts	Wisconsin
	Michigan	
	Missouri	
	Montana	
	Nebraska	
	North Carolina	
	Ohio	
	Oregon	
	South Dakota	
	Tennessee	
	Texas	
	Utah	
	Vermont	
	Virginia	
	Washington	
	West Virginia	

Note: See text for data sources and explanation.

Table 2: BRFSS sample summary statistics for outcome variables, all single women 20–45

	Single women			Single women with children	
	Black	Hispanic	Low ed.	Black	Low ed.
Have any health insurance (private or public)	0.763	0.633	0.673	0.764	0.682
	(0.425)	(0.482)	(0.469)	(0.425)	(0.466)
Have HI and employed now	0.544	0.413	0.458	0.527	0.446
	(0.498)	(0.492)	(0.498)	(0.499)	(0.497)
Have HI and are not employed now	0.218	0.220	0.213	0.235	0.233
	(0.413)	(0.414)	(0.409)	(0.424)	(0.423)
Last checkup was in the last 12 months	0.843	0.699	0.718	0.847	0.737
	(0.363)	(0.459)	(0.450)	(0.360)	(0.440)
Needed to see a doctor, could not afford to in the last year	0.207	0.261	0.265	0.218	0.272
	(0.406)	(0.439)	(0.441)	(0.413)	(0.445)
Last Pap. smear was in the last year	0.798	0.654	0.691	0.801	0.711
	(0.402)	(0.476)	(0.462)	(0.400)	(0.453)
Last professional breast exam was in the last year	0.722	0.566	0.631	0.711	0.638
	(0.448)	(0.496)	(0.482)	(0.453)	(0.481)
General health is fair or poor	0.143	0.195	0.172	0.148	0.167
	(0.350)	(0.396)	(0.378)	(0.355)	(0.373)
Days in last month poor health kept R from usual activities	1.983	1.922	2.299	2.037	2.284
	(5.605)	(5.505)	(6.139)	(5.770)	(6.206)
Days in last month poor mental health	4.575	4.800	5.532	4.740	5.757
	(8.470)	(8.321)	(9.192)	(8.697)	(9.477)
Maximum available sample size	26,527	12,680	60,872	13,049	29,004

Note: Tabulations from the BRFSS, 1990–2000 (columns 1–3) or 1993–2000 (columns 4 and 5) with standard errors in parentheses. Weighting is based on *finalwt* variable. Black subgroups all defined as non-Hispanic. Sample is all single women 20–45 in the subgroup defined in the column label for whom the variables are reported. Child indicator is available only for 1993-2000 and was not reported for California. Child indicator denotes a child under 18 is present in the household. Low-education denotes high school dropout or high school graduate with no college. 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

	Single women			Single women with children	
	Black	Hispanic	Low ed.	Black	Low ed.
Waiver implemented	0.138	0.224	0.154	0.164	0.145
	(0.345)	(0.417)	(0.361)	(0.370)	(0.352)
TANF implemented	0.355	0.380	0.344	0.547	0.558
	(0.478)	(0.486)	(0.475)	(0.498)	(0.497)
Share of last yr., waiver impl.	0.141	0.228	0.151	0.169	0.147
	(0.320)	(0.390)	(0.330)	(0.339)	(0.316)
Share of last year, TANF	0.311	0.334	0.302	0.482	0.494
	(0.445)	(0.453)	(0.442)	(0.475)	(0.476)
State has fill-in Med. like program for inel. immigrants	0.185	0.225	0.189	0.270	0.271
	(0.389)	(0.417)	(0.391)	(0.444)	(0.444)
Income limit (% of FPL), 1931 or 1115 family Med. elig.	7.4	9.6	10.4	10.5	15.5
	(33.8)	(35.8)	(39.1)	(41.0)	(48.4)
Income limit (% of FPL), pregnant women's Medicaid elig.	173.9	189.4	176.4	176.6	176.1
	(37.4)	(41.4)	(39.0)	(36.0)	(35.6)
Income limit (% of FPL), child of 14's Medicaid elig.	51.6	49.9	51.2	75.9	78.8
	(74.9)	(60.1)	(72.9)	(81.8)	(81.6)
Income limit (% of FPL), child of 14's SCHIP elig.	32.8	50.2	34.8	47.4	49.8
	(76.2)	(92.5)	(79.2)	(87.3)	(89.7)
Share of non-citizen women 20–45, rac./ethn. group	0.043	0.409	0.107	0.043	0.085
	(0.063)	(0.145)	(0.174)	(0.066)	(0.142)
Real max. AFDC/TANF benefits, family of three	4.485	5.780	5.083	4.026	4.316
	(1.884)	(2.261)	(2.050)	(1.599)	(1.649)
State unemployment rate	5.561	6.050	5.674	4.857	4.797
	(1.413)	(1.591)	(1.550)	(1.007)	(1.046)
State employment growth rate	1.865	2.029	1.886	2.353	2.456
	(1.443)	(1.644)	(1.564)	(0.949)	(1.082)
Age	31.2	29.9	31.0	31.3	31.1
	(7.5)	(7.5)	(7.6)	(7.2)	(7.2)
High school dropout, no GED	0.121	0.276	0.263	0.140	0.285
	(0.326)	(0.447)	(0.440)	(0.347)	(0.451)
High school diploma or GED only	0.373	0.299	0.737	0.420	0.715
	(0.484)	(0.458)	(0.440)	(0.494)	(0.451)
Some college/technical school, no 4 year degree	0.334	0.283	0.000	0.332	0.000
	(0.472)	(0.451)	(0.000)	(0.471)	(0.000)
Maximum available sample size	26,527	12,680	60,872	13,049	29,004

Note: Tabulations from the BRFSS, 1990–2000 (columns 1–3) or 1993–2000 (columns 4 and 5) with standard errors in parentheses. Weighting is based on *finalwt* variable. Black subgroups all defined as non-Hispanic. Sample is all single women 20–45 in the subgroup defined in the column label for whom the variables are reported. Child indicator is available only for 1993-2000 and was not reported for California. Child indicator denotes a child under 18 is present in the household. Low-education denotes high school dropout or high school graduate with no college. See text for more information.

Table 4: Comparing single women to married women in the black (non-Hispanic) sample

	Insurance status			Utilization				Health status		
		Insured and:			Pap	Breast	Needed care,	Health	Days	Days
	Any	Employed	Not Employed	Checkup	Smear	Exam	Unaffordable	Fair/poor	Limited	Depressed
Waivers										
Single	-0.003	-0.023	0.024	-0.006	-0.056***	-0.054***	0.033*	0.004	-0.111	0.334
	(0.016)	(0.020)	(0.019)	(0.018)	(0.017)	(0.021)	(0.019)	(0.013)	(0.212)	(0.380)
Married	0.009	0.014	-0.008	0.028	0.011	0.023	0.002	-0.006	-0.214	-0.341
	(0.020)	(0.029)	(0.024)	(0.025)	(0.027)	(0.029)	(0.022)	(0.017)	(0.284)	(0.372)
Difference	-0.012	-0.037	0.032	-0.035	-0.067**	-0.077**	0.031	0.011	0.103	0.675
	(0.026)	(0.035)	(0.030)	(0.030)	(0.032)	(0.035)	(0.029)	(0.022)	(0.354)	(0.532)
TANF										
Single	-0.017	0.036	-0.056	-0.018	-0.064**	-0.071**	0.015	0.003	0.330	-0.407
	(0.021)	(0.037)	(0.035)	(0.034)	(0.029)	(0.036)	(0.032)	(0.021)	(0.291)	(0.569)
Married	0.031	-0.039	0.063	0.097**	0.026	-0.019	-0.013	0.041	0.613	-0.054
	(0.033)	(0.047)	(0.040)	(0.043)	(0.038)	(0.037)	(0.037)	(0.026)	(0.458)	(0.677)
Difference	-0.048	0.075	-0.119**	-0.115**	-0.091*	-0.052	0.028	-0.039	-0.283	-0.354
	(0.040)	(0.060)	(0.053)	(0.055)	(0.048)	(0.051)	(0.049)	(0.034)	(0.542)	(0.884)
Num. obs.										
Single	24,091	22,966	22,966	24,956	23,687	24,381	24,087	21,372	21,284	21,185
Married	10,475	10,005	10,005	10,937	10,333	10,737	10,474	9,309	9,282	9,233
Pre-reform Mean										
Single	0.753	0.522	0.229	0.849	0.809	0.737	0.211	0.143	1.823	4.251
Married	0.850	0.658	0.192	0.857	0.839	0.799	0.167	0.108	1.556	3.126

Note: ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively. All figures are OLS coefficients and associated standard errors. See text for list of included covariates.

Table 5: Comparing single women to married women in the Hispanic sample

	Insurance status			Utilization				Health status		
		Insured and:			Pap	Breast	Needed care,	Health	Days	Days
	Any	Employed	Not Employed	Checkup	Smear	Exam	Unaffordable	Fair/poor	Limited	Depressed
Waivers										
Single	-0.050**	-0.011	-0.040*	-0.049*	-0.044	-0.081**	0.078**	-0.047	-0.004	-0.706
	(0.025)	(0.026)	(0.023)	(0.029)	(0.029)	(0.035)	(0.036)	(0.029)	(0.514)	(0.456)
Married	-0.030	-0.014	-0.015	-0.057**	-0.046**	-0.041	0.038*	-0.010	0.511	0.188
	(0.020)	(0.017)	(0.020)	(0.025)	(0.019)	(0.027)	(0.019)	(0.017)	(0.348)	(0.363)
Difference	-0.020	0.003	-0.025	0.007	0.003	-0.040	0.040	-0.037	-0.515	-0.894
	(0.032)	(0.031)	(0.030)	(0.038)	(0.035)	(0.044)	(0.041)	(0.033)	(0.621)	(0.583)
TANF										
Single	-0.092***	-0.025	-0.067*	-0.074	-0.102**	-0.069	0.050	-0.018	0.032	-0.514
	(0.035)	(0.056)	(0.034)	(0.046)	(0.041)	(0.050)	(0.052)	(0.028)	(0.482)	(0.589)
Married	0.046**	0.080***	-0.035	0.064*	-0.020	0.065	0.047	-0.042	-0.170	-0.501
	(0.022)	(0.024)	(0.026)	(0.035)	(0.026)	(0.042)	(0.043)	(0.029)	(0.422)	(0.535)
Difference	-0.139***	-0.105*	-0.032	-0.138**	-0.082*	-0.135**	0.003	0.024	0.202	-0.013
	(0.041)	(0.061)	(0.043)	(0.058)	(0.048)	(0.065)	(0.068)	(0.040)	(0.641)	(0.796)
Num. obs.										
Single	11,726	11,648	11,648	11,697	11,563	11,409	11,719	10,722	10,667	10,593
Married	13,258	13,170	13,170	13,328	13,105	13,012	13,257	12,059	12,011	11,941
Pre-reform Mean										
Single	0.661	0.408	0.253	0.749	0.668	0.596	0.255	0.160	1.669	5.355
Married	0.691	0.449	0.240	0.759	0.729	0.665	0.253	0.167	1.448	3.257

Note: ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively. All figures are OLS coefficients and associated standard errors. See text for list of included covariates.

Table 6: Comparing single women to married women in the low-education sample (all races/ethnicities)

	Insurance status			Utilization				Health status		
		Insured and:			Pap	Breast	Needed care,	Health	Days	Days
	Any	Employed	Not Employed	Checkup	Smear	Exam	Unaffordable	Fair/poor	Limited	Depressed
Waivers										
Single	-0.002	-0.004	0.003	-0.044***	-0.025	-0.056***	0.014	-0.004	-0.022	-0.028
	(0.012)	(0.013)	(0.011)	(0.013)	(0.017)	(0.019)	(0.017)	(0.009)	(0.223)	(0.346)
Married	-0.010	-0.001	-0.010	-0.025**	-0.024*	-0.008	0.022**	-0.008	0.047	-0.156
	(0.009)	(0.010)	(0.008)	(0.012)	(0.013)	(0.013)	(0.011)	(0.007)	(0.116)	(0.210)
Difference	0.008	-0.004	0.013	-0.019	-0.000	-0.048**	-0.007	0.004	-0.069	0.128
	(0.014)	(0.017)	(0.014)	(0.018)	(0.021)	(0.023)	(0.020)	(0.012)	(0.251)	(0.405)
TANF										
Single	-0.030	-0.002	-0.026*	-0.055**	-0.052*	-0.069**	0.037	-0.012	-0.402	-0.734
	(0.021)	(0.017)	(0.014)	(0.025)	(0.027)	(0.031)	(0.029)	(0.012)	(0.247)	(0.473)
Married	0.007	0.025*	-0.018	0.020	-0.025	-0.010	0.018	-0.012	0.173	-0.440
	(0.014)	(0.015)	(0.015)	(0.023)	(0.023)	(0.022)	(0.024)	(0.015)	(0.251)	(0.301)
Difference	-0.036	-0.027	-0.009	-0.075**	-0.027	-0.059	0.019	-0.000	-0.575	-0.293
	(0.025)	(0.023)	(0.021)	(0.034)	(0.035)	(0.039)	(0.037)	(0.019)	(0.352)	(0.561)
Num. obs.										
Single	54,787	52,979	52,979	56,165	53,902	54,045	54,743	48,570	48,283	47,890
Married	73,050	70,653	70,653	76,049	72,106	73,120	73,009	63,569	63,374	62,891
Pre-reform Mean										
Single	0.692	0.493	0.197	0.725	0.703	0.648	0.255	0.144	2.094	5.393
Married	0.818	0.555	0.262	0.722	0.711	0.696	0.186	0.095	1.464	3.738

Note: ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively. All figures are OLS coefficients and associated standard errors. See text for list of included covariates.

Table 7: Comparing single women living with children to married women living with children in the black (non-Hispanic) sample

	Insurance status			Utilization				Health status		
		Insured and:			Pap	Breast	Needed care,	Health	Days	Days
	Any	Employed	Not Employed	Checkup	Smear	Exam	Unaffordable	Fair/poor	Limited	Depressed
Waivers										
Single	0.000	-0.045*	0.046	0.004	-0.043*	-0.025	0.050*	-0.004	-0.030	0.419
	(0.018)	(0.024)	(0.028)	(0.019)	(0.023)	(0.025)	(0.026)	(0.018)	(0.259)	(0.413)
Married	0.019	0.045*	-0.022	0.024	-0.009	0.065*	0.012	-0.025	-0.735***	-0.085
	(0.023)	(0.025)	(0.025)	(0.027)	(0.029)	(0.033)	(0.026)	(0.016)	(0.283)	(0.406)
Difference	-0.019	-0.090**	0.068*	-0.020	-0.034	-0.090**	0.038	0.021	0.705*	0.504
	(0.030)	(0.035)	(0.037)	(0.032)	(0.037)	(0.042)	(0.036)	(0.024)	(0.384)	(0.579)
TANF										
Single	-0.047	-0.034	-0.014	-0.053*	-0.051	-0.077**	0.062*	-0.011	0.476	-0.245
	(0.029)	(0.034)	(0.030)	(0.030)	(0.044)	(0.039)	(0.036)	(0.024)	(0.359)	(0.677)
Married	0.058*	0.054	-0.000	0.058	0.010	0.028	-0.013	0.011	0.123	0.770
	(0.031)	(0.038)	(0.032)	(0.057)	(0.044)	(0.046)	(0.043)	(0.024)	(0.459)	(0.710)
Difference	-0.105**	-0.088*	-0.014	-0.111*	-0.062	-0.105*	0.075	-0.021	0.352	-1.014
	(0.042)	(0.051)	(0.044)	(0.064)	(0.062)	(0.060)	(0.056)	(0.035)	(0.582)	(0.982)
Num. obs.										
Single	13,850	13,232	13,232	13,446	13,679	13,089	13,852	14,457	14,396	14,332
Married	6,897	6,596	6,596	6,668	6,832	6,541	6,896	7,237	7,215	7,182
Pre-reform Mean										
Single	0.765	0.492	0.270	0.838	0.800	0.728	0.221	0.148	1.881	4.515
Married	0.844	0.647	0.196	0.831	0.829	0.790	0.169	0.112	1.553	3.107

Note: ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively. All figures are OLS coefficients and associated standard errors. See text for list of included covariates.

Table 8: Comparing single women living with children to single women living without children in the black (non-Hispanic) sample

	Insurance status			Utilization				Health status		
		Insured and:			Pap	Breast	Needed care,	Health	Days	Days
	Any	Employed	Not Employed	Checkup	Smear	Exam	Unaffordable	Fair/poor	Limited	Depressed
Waivers										
Single	0.000	-0.045*	0.046	0.004	-0.043*	-0.025	0.050*	-0.004	-0.030	0.419
	(0.018)	(0.024)	(0.028)	(0.019)	(0.023)	(0.025)	(0.026)	(0.018)	(0.259)	(0.413)
No children	0.039	0.043	0.010	-0.013	-0.012	-0.063*	-0.032	0.027	-0.094	-0.605
	(0.030)	(0.034)	(0.024)	(0.029)	(0.032)	(0.037)	(0.032)	(0.021)	(0.347)	(0.565)
Difference	-0.038	-0.087**	0.036	0.017	-0.031	0.038	0.082**	-0.031	0.064	1.024
	(0.035)	(0.042)	(0.037)	(0.034)	(0.040)	(0.044)	(0.041)	(0.028)	(0.433)	(0.700)
TANF										
Single	-0.047	-0.034	-0.014	-0.053*	-0.051	-0.077**	0.062*	-0.011	0.476	-0.245
	(0.029)	(0.034)	(0.030)	(0.030)	(0.044)	(0.039)	(0.036)	(0.024)	(0.359)	(0.677)
No children	0.069	0.092	-0.022	-0.078*	-0.051	-0.070	0.031	0.043	0.057	-0.508
	(0.053)	(0.061)	(0.031)	(0.047)	(0.056)	(0.077)	(0.077)	(0.027)	(0.657)	(0.924)
Difference	-0.116*	-0.125*	0.007	0.026	-0.000	-0.008	0.031	-0.054	0.419	0.264
	(0.060)	(0.070)	(0.043)	(0.056)	(0.071)	(0.086)	(0.085)	(0.036)	(0.748)	(1.146)
Num. obs.										
Single	13,850	13,232	13,232	13,446	13,679	13,089	13,852	14,457	14,396	14,332
No children	6,131	5,835	5,835	5,937	6,037	5,786	6,136	6,393	6,366	6,330
Pre-reform Mean										
Single	0.765	0.492	0.270	0.838	0.800	0.728	0.221	0.148	1.881	4.515
No children	0.749	0.586	0.160	0.824	0.775	0.733	0.181	0.131	1.672	3.712

Note: ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively. All figures are OLS coefficients and associated standard errors. See text for list of included covariates.

Table 9: Comparing single women living with children to married women living with children in the low-education sample (all races/ethnicities)

	Insurance status			Utilization				Health status		
		Insured and:			Pap	Breast	Needed care,	Health	Days	Days
	Any	Employed	Not Employed	Checkup	Smear	Exam	Unaffordable	Fair/poor	Limited	Depressed
Waivers										
Single	0.014	-0.006	0.017	-0.013	0.009	-0.036	-0.005	-0.001	0.261	0.073
	(0.016)	(0.017)	(0.017)	(0.018)	(0.022)	(0.024)	(0.020)	(0.012)	(0.316)	(0.430)
Married	-0.011	-0.004	-0.007	0.004	-0.020	0.003	0.033**	-0.012	-0.087	-0.338
	(0.010)	(0.012)	(0.010)	(0.014)	(0.014)	(0.016)	(0.015)	(0.008)	(0.119)	(0.241)
Difference	0.025	-0.002	0.023	-0.017	0.029	-0.039	-0.037	0.011	0.348	0.410
	(0.019)	(0.021)	(0.020)	(0.022)	(0.026)	(0.028)	(0.025)	(0.014)	(0.338)	(0.493)
TANF										
Single	0.020	-0.006	0.025	0.030	0.020	-0.042	0.003	-0.002	0.252	-0.077
	(0.022)	(0.021)	(0.021)	(0.032)	(0.038)	(0.043)	(0.033)	(0.017)	(0.430)	(0.542)
Married	0.039***	0.042**	-0.003	-0.005	-0.066***	-0.006	0.006	-0.004	0.082	-0.229
	(0.015)	(0.020)	(0.019)	(0.025)	(0.025)	(0.029)	(0.031)	(0.018)	(0.251)	(0.377)
Difference	-0.020	-0.047	0.029	0.035	0.086*	-0.037	-0.003	0.002	0.170	0.152
	(0.027)	(0.029)	(0.028)	(0.040)	(0.046)	(0.052)	(0.045)	(0.025)	(0.498)	(0.660)
Num. obs.										
Single	30,552	29,585	29,585	29,229	30,255	27,971	30,529	32,089	31,891	31,629
Married	46,604	45,217	45,217	44,492	46,277	42,474	46,579	49,323	49,174	48,790
Pre-reform Mean										
Single	0.703	0.476	0.225	0.739	0.719	0.653	0.261	0.149	2.074	5.481
Married	0.822	0.547	0.273	0.712	0.701	0.683	0.184	0.091	1.336	3.751

Note: ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively. All figures are OLS coefficients and associated standard errors. See text for list of included covariates.

Table 10: Comparing single women living with children to single women living without children in the low-education sample (all races/ethnicities)

	Insurance status			Utilization				Health status		
		Insured and:			Pap	Breast	Needed care,	Health	Days	Days
	Any	Employed	Not Employed	Checkup	Smear	Exam	Unaffordable	Fair/poor	Limited	Depressed
Waivers										
Single	0.014	-0.006	0.017	-0.013	0.009	-0.036	-0.005	-0.001	0.261	0.073
	(0.016)	(0.017)	(0.017)	(0.018)	(0.022)	(0.024)	(0.020)	(0.012)	(0.316)	(0.430)
No children	-0.011	-0.001	-0.003	-0.032	-0.080***	-0.065**	0.020	-0.003	-0.607**	-0.555
	(0.022)	(0.025)	(0.016)	(0.030)	(0.029)	(0.032)	(0.027)	(0.015)	(0.291)	(0.457)
Difference	0.025	-0.005	0.020	0.019	0.089**	0.029	-0.025	0.002	0.868**	0.627
	(0.027)	(0.030)	(0.023)	(0.035)	(0.036)	(0.040)	(0.033)	(0.019)	(0.430)	(0.628)
TANF										
Single	0.020	-0.006	0.025	0.030	0.020	-0.042	0.003	-0.002	0.252	-0.077
	(0.022)	(0.021)	(0.021)	(0.032)	(0.038)	(0.043)	(0.033)	(0.017)	(0.430)	(0.542)
No children	-0.043	-0.008	-0.031	-0.079*	-0.120**	-0.072	0.016	-0.036	-1.356***	-0.346
	(0.034)	(0.038)	(0.022)	(0.044)	(0.054)	(0.065)	(0.050)	(0.022)	(0.345)	(0.607)
Difference	0.063	0.002	0.057*	0.109**	0.140**	0.029	-0.013	0.034	1.608***	0.270
	(0.040)	(0.043)	(0.031)	(0.055)	(0.066)	(0.078)	(0.060)	(0.028)	(0.551)	(0.814)
Num. obs.										
Single	30,552	29,585	29,585	29,229	30,255	27,971	30,529	32,089	31,891	31,629
No children	13,852	13,388	13,388	13,240	13,644	12,629	13,856	14,639	14,556	14,429
Pre-reform Mean										
Single	0.703	0.476	0.225	0.739	0.719	0.653	0.261	0.149	2.074	5.481
No children	0.696	0.569	0.125	0.711	0.674	0.644	0.238	0.132	2.016	4.918

Note: ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively. All figures are OLS coefficients and associated standard errors. See text for list of included covariates.

Appendix Table 1: Black (non-Hispanic) single-women sample: Health insurance results

	Any Insurance	Insured and: Employed	Insured and: Not Employed
Any major waiver	-0.003	-0.023	0.024
	(0.016)	(0.020)	(0.019)
TANF	-0.017	0.036	-0.056
	(0.021)	(0.037)	(0.035)
Income limit (multiples of FPL), 1931/1115 family Med. elig.	0.015*	0.017	-0.001
	(0.009)	(0.018)	(0.015)
Income limit (multiples of FPL), pregnant women's Medicaid elig.	0.019	0.076**	-0.026
	(0.014)	(0.035)	(0.029)
Income limit (multiples of FPL), child of 14's Medicaid elig.	0.013	0.027*	-0.007
	(0.008)	(0.016)	(0.018)
Income limit (multiples of FPL), child of 14's SCHIP elig.	-0.004	0.001	-0.005
	(0.008)	(0.009)	(0.009)
Real max. AFDC/TANF benefits, family of three	-0.002	-0.006	0.008
	(0.017)	(0.022)	(0.019)
State unemployment rate	-0.008	-0.008	-0.002
	(0.009)	(0.010)	(0.009)
Lagged state unemployment rate	0.001	-0.013	0.015*
	(0.009)	(0.010)	(0.009)
State employment growth rate	-0.005	-0.006	0.002
	(0.006)	(0.008)	(0.007)
Lagged state employment growth rate	-0.001	-0.012	0.009
	(0.005)	(0.007)	(0.006)
Age	0.016***	0.055***	-0.038***
	(0.005)	(0.006)	(0.006)
Age squared	-0.000**	-0.001***	0.000***
	(0.000)	(0.000)	(0.000)
High school dropout, no GED	-0.189***	-0.500***	0.311***
	(0.015)	(0.015)	(0.017)
High school diploma or GED only	-0.156***	-0.287***	0.131***
	(0.011)	(0.014)	(0.012)
Some college/technical school, no 4-year degree	-0.083***	-0.172***	0.089***
	(0.010)	(0.013)	(0.010)
Pre-reform mean of dependent variable	0.767	0.568	0.198
N	24,091	22,966	22,966

Note: ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively. Coefficients are those for control variables in Table 4, specifications in columns 1–3. All specifications are weighted using BRFSS *finalwt* variable, with robust variance estimates to account for state-by-year clustering. Sample includes only single, black, non-Hispanic women aged 20–45. Economic and welfare reform variables refer to the year of the survey. Additional control variables are dummy variables for year, state, and calendar month.