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THE EFFECT OF WELFARE REFORM ON PRENATAL CARE AND BIRTH WEIGHT

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ABSTRACT

Welfare reform has resulted in a dramatic decline in welfare caseloads and some have claimed that a significant number of low-income women may be without health insurance as a result. The loss of insurance may reduce low-income, pregnant women's health care utilization, and this may adversely affect infant health. Welfare reform also may affect healthcare utilization and health of pregnant women and infants because of welfare-induced changes in family disposable income, time available for health investments, and levels of stress. In this paper we examine the effect of welfare reform on prenatal care utilization and birth weight of low-educated women and their infants. We find that a 50 percent reduction in the caseload, which is similar to that which occurred in the 1990s, is associated with a zero to seven percent decrease in first trimester prenatal care; a zero to five percent decrease in the number of prenatal care visits; and a zero to 10 percent increase in low birth weight.

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Introduction:

The Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA) eliminated the entitlement to cash assistance, required recipients of cash assistance to meet new work requirements, and instituted lifetime time limits on participation in the welfare program. The primary goal of the policy was to decrease dependence on government assistance and to increase economic self-sufficiency. Many state welfare reform efforts that preceded PRWORA shared this emphasis. Partly as a consequence of these efforts, the number of welfare recipients dropped 59% between January 1993 and March 2001, from about 14.1 million recipients in 1993 to 5.4 million recipients in 2001.

The decline in the welfare caseload has the potential to reduce significantly the prevalence of health insurance among low-income women. Indeed, evidence from studies of former welfare recipients suggests that many women and children who left welfare are without insurance (Moffitt and Slade 1997; Guyer 2000; Garrett and Holahan 2000). In addition, many women who were diverted from entering welfare may be without health insurance because of a lack of employment or because health insurance benefits are not available on the mostly low-wage jobs these women will hold.

If welfare reform leads to a loss of health insurance coverage, the loss of insurance may reduce low-income, pregnant women's health care utilization, and this may adversely affect infant health. Still, even in the absence of any effect of welfare reform on the incidence of health insurance coverage (i.e., the proportion uninsured), reductions in the welfare caseload may affect health care utilization as more low-income women gain private health insurance through their employers. A greater reliance on private health insurance instead of Medicaid, with its larger deductibles and coinsurance rates, may reduce low-income women's health care utilization. In these ways, welfare reform, through its impact on insurance status, may have adversely affected the health care utilization of pregnant women and the health of infants.

Welfare reform also may affect healthcare utilization and health of pregnant women and infants without inducing significant changes in insurance status. The greater work effort by low-income women induced by welfare reform may change family disposable income, time available for health investments, and levels of stress, all of which may affect the health of low-income women and infants.

Given the dramatic decline in welfare caseloads and the possibility that a significant number of low-income, pregnant women and infants may be without health insurance as a result, it is remarkable that there is so little information currently available to policy makers as to the effect of welfare reform on the health care utilization and health of low-income, pregnant women and infants. In fact, we are aware of only one other paper that examined this issue, and this paper focused on pre-PRWORA reforms (Currie and Grogger 2002). Knowledge of such unintended consequences of PRWORA, however, would be relevant to the debate over the efficacy of the current welfare policy. Indeed, the original debate over welfare reform led to bipartisan support to ensure through provisions in the legislation that welfare reform did not affect health insurance coverage—presumably because of the adverse consequences on health. Evidence that this objective was or was not achieved would surely affect current thinking about this policy. In sum, the effect of welfare reform on the health care utilization and health of low-income, pregnant women and infants is a significant policy issue that is under-researched.

In this paper we examine the effect of welfare reform on prenatal care utilization and birth weight of low-educated women and their infants. We focus on low-educated women because they are the most likely to be affected by welfare reform. We use data from vital records (i.e., Natality detail files) in the United States from 1992 to 2000 and a quasi-experimental research design to obtain estimates of the effect of welfare reform on these outcomes.

Welfare Reform, Prenatal Care Utilization and Infant Health:

Welfare reform had several intended effects that may have unintended consequences for prenatal care utilization and infant health. Changes in federal and state welfare policy that made welfare temporary and require recipients to work essentially eliminated the guarantee in the AFDC program of long term income support to unmarried women with children. As a result, potential participants and those on welfare would have a greater incentive to make choices that would improve their abilities for self-support. Therefore, we would expect low-income women to have fewer births and fewer out-of-wedlock births (i.e., increase in marital births), to be more likely to marry, and to increase their labor market activity. All of these potential changes may affect prenatal care use and infant health.

A decrease in the number of births may lead to a change in the composition of women who give birth before and after welfare reform; welfare reform may have a "selection" effect.[1] For example, welfare reform may cause women who are most likely to receive prenatal care and least likely to have an adverse birth outcome not to have a child, or to delay having a child. As a result, welfare reform will be associated with a decrease in prenatal care utilization and a worsening of birth outcomes among low-income women, as the pool of low-educated women who give birth becomes more disadvantaged. "Selection" effects of this type are difficult to address by public policy intended to mediate adverse effects of welfare reform on pregnant women. It is important to recognize, however, that any effects due to changes in the composition of pregnant women (i.e., selection) are effects of welfare reform. Estimated effects of welfare reform will be a combination of the effects due to the change in the composition of pregnant women, and the effects due to changes brought about through the mechanisms described above—for example, because of changes in health insurance status associated with greater labor market

[1] There is relatively little evidence as to the effect of welfare reform on fertility. See Grogger et al. (2002) for a review. In a recent paper, Joyce, Kaestner and Korenman (2002) report that welfare reform in the 1990s had no effect on fertility.

activity. But it is correct to attribute both effects to welfare reform. In other words, we are estimating reduced form effects.

One of the most well-documented consequences of welfare reform has been an increase in labor market activity among low-income, unmarried women (Kaushal and Kaestner 2001; Moffitt 2002). An increase in labor market activity may affect prenatal care utilization and infant health. First, an increase in work activity and the associated increase in earnings will decrease eligibility for Medicaid. This will result in a loss in health insurance unless there is an equal increase in employment-related, private insurance coverage. There may even be a decline in health insurance coverage among those who exit welfare and who remain eligible for transitional Medicaid benefits because of administrative hurdles associated with applying for Medicaid benefits (Ellwood 1999). In both cases, the loss of health insurance may decrease prenatal care utilization and worsen birth outcomes. On the other hand, greater work activity may increase private insurance coverage and depending on the relative changes in Medicaid and private insurance, health insurance coverage may remain unchanged. However, the higher copayments associated with private insurance may decrease prenatal care utilization and adversely affect infant health. Alternatively, the quality of prenatal care may be better with private insurance resulting in improvements in infant health.

Greater work activity will also affect other aspects of a woman's life such as disposable income, stress levels, feelings of self-esteem, and time available for health investments. All of these factors may affect prenatal care utilization and infant health. Disposable income may increase, for example, because of greater earnings or the addition of other family members' income, and this may result in an increase in quantity and quality of prenatal care and improve infant health. Alternatively, a decrease in disposable income, say, because of increased child care costs, may result in a decrease in the quantity and quality of prenatal care and worsen infant health. Changes in income may also affect other determinants of infant health besides prenatal care such as nutrition. Greater work activity may increase stress levels and behaviors used to reduce stress such as smoking and drinking. These changes will adversely affect infant health. Alternatively, employment may increase self esteem and feelings of self worth that affect the "wantedness" of the child and investments in the child. As a result prenatal care may increase and there may be a reduction in unhealthy behaviors (e.g., smoking) resulting in an improvement in infant health. Finally, greater labor force participation will tighten time constraints and may create difficulties scheduling and obtaining prenatal care, or reduce the time available for other health augmenting behaviors (e.g., diet and nutrition). These changes will also adversely affect infant health.

In sum, there are a variety of ways that welfare reform may affect prenatal care utilization and infant health. Some considerations suggest that welfare reform may positively affect these outcomes, while others suggest a more harmful effect. In this paper, we examine the reduced form effect of welfare reform on prenatal care utilization and infant health. While it is important to understand the mechanisms by which welfare reform affects these outcomes—if there is an effect—the first order question is whether or not there is an effect of welfare reform. To date, there is no evidence bearing on this question for post-1996 reforms.

Empirical Framework:

As described above, changes in welfare policy and the resulting changes in the welfare caseload may affect low-income women's health insurance coverage, stress, income, and time constraints, and all of these factors may affect their use of medical services, health behaviors and health. In this paper, we focus on maternal health care utilization, as measured by prenatal care, and infant health, as measured by birth weight and gestation, and we restrict the sample to low-income women who are at high-risk of welfare receipt. Perhaps the most straightforward way to assess the effect of welfare reform on prenatal care use and infant health is to obtain estimates of the association between the welfare caseload and these outcomes. This is the strategy pursued by

Currie and Grogger (2002) for the pre-PRWORA period. Accordingly, we begin by estimating the following model:

$$Y_{ijt} = \beta_j + \delta_t + \gamma \text{CASELOAD}_{jt} + Z_{jt}\Delta + X_{ijt}\Gamma + u_{ijt}$$

(1) $i = 1, \ldots, N$ (persons)
$j = 1, \ldots, 51$ (states)
$t = 1992, \ldots, 2000$ (years)

Equation (1) relates outcome Y (e.g., prenatal care utilization or birth weight) of pregnant woman "i", in state "j", and in year "t" to the welfare caseload in that state nine-months prior to the month of birth. To measure the caseload, we use the natural logarithm of the number of cases (i.e., families) participating in welfare.[2] There are several points to note about equation (1). First, it includes controls for year (δ_t) and state effects (β_j). Thus, to a large extent, equation (1) addresses the fundamental identification problem associated with this analysis. The only identifying assumption we invoke is that unmeasured state-specific time trends are uncorrelated with the welfare caseload. Note, however, that we also include time-varying state characteristics (Z_{jt}) such as the current and one-year lag of the state unemployment rate and the current and one-year lag of Medicaid income-eligibility threshold for pregnant women, which also control for within-state variation in these outcomes. Finally, equation (1) includes a set of controls (X_{ijt}) measuring personal characteristics (e.g., age, race) of the women in the sample.

One disadvantage of equation (1) is that it assumes that welfare reform was responsible for all of the change in the welfare caseload. In fact, evidence suggests that most of the change in the caseload was not due to policy, particularly prior to the implementation of PRWORA in 1996 (Blank 2002). Moreover, it is possible that those who leave welfare because of government policy may have different experiences than those who leave for other reasons such as robust

[2] An alternative is to use the number of cases divided by population; all results are qualitatively the same when this measure is used.

economic growth. For example, women who leave welfare because of welfare policy may make more use of transitional benefits such as Medicaid and child care than those who leave for other reasons. Thus, the former women group may be more likely to have health insurance and less stressful environments than the latter group, and therefore would also likely have different prenatal care use and infant health. The upshot is that estimates of the association between the welfare caseload and prenatal care and infant health do not necessarily provide estimates of the association between welfare reform and these outcomes.

To estimate the effect of welfare reform per se, we use the following model:

$$\mathrm{Y}_{\mathrm{ijt}} = \beta_{\mathrm{j}} + \delta_{\mathrm{t}} + \gamma_1\,\mathrm{C(POLICY)}_{\mathrm{jt}} + \gamma_2\,\mathrm{C(RESIDUAL)}_{\mathrm{jt}} + \mathrm{Z}_{\mathrm{jt}}\Delta + \mathrm{X}_{\mathrm{ijt}}\Gamma + \mathrm{u}_{\mathrm{ijt}} \tag{2}$$

where the variables C(POLICY) and C(RESIDUAL) are the portions of the welfare caseload related to welfare policy and other factors, respectively. These variables are derived from the following regression model:

$$\begin{aligned} \mathrm{CASELOAD}_{\mathrm{jt}} &= \rho_{\mathrm{j}} + \sigma_{\mathrm{t}} + \alpha_{\mathrm{j}}(\mathrm{TANF})_{\mathrm{jt}} + \lambda_{\mathrm{j}}(\mathrm{WAIVER}) + \mathrm{Z}_{\mathrm{jt}}\Pi + \mathrm{v}_{\mathrm{jt}} \\ \mathrm{C(POLICY)}_{\mathrm{jt}} &= \alpha_{\mathrm{j}}(\mathrm{TANF})_{\mathrm{jt}} + \lambda_{\mathrm{j}}(\mathrm{WAIVER})_{\mathrm{jt}} \\ \mathrm{C(RESIDUAL)}_{\mathrm{jt}} &= \mathrm{CASELOAD}_{\mathrm{jt}} - \mathrm{C(POLICY)}_{\mathrm{jt}} \end{aligned} \tag{3}$$

The specification of equation (2) allows the effect of the caseload to differ by the underlying cause of changes in the caseload. We have created two broad categories of causes: policy and other factors (residual). To construct these two caseload variables, we estimate equation (3) using monthly data on the welfare caseload.[3] Equation (3) has been estimated by several other researchers interested in the effect of welfare policy on the caseload (Blank 2002). In our case, we allow the effect of welfare policy, as measured by whether a state has an AFDC waiver (WAIVER) or has implemented the Temporary Assistance to Needy Families (TANF) program, to differ by state. This modification results in state-specific estimates of the effect of policy, and

[3] To estimate equation (3), we use individual level data.

for each state we have at least two and potentially three observations depending on whether the state had an AFDC waiver.

Finally, we also estimate the reduced form, which is obtained by substituting equation (3) into (2).

$$Y_{ijt} = \tilde{\beta}_j + \tilde{\delta}_t + \gamma_1 \alpha \text{TANF}_{jt} + \gamma_1 \lambda \text{WAIVER}_{jt} + Z_{jt}\tilde{\Delta} + X_{ijt}\Gamma + u_{ijt} \quad (4)$$

In equation (4), the symbol ~ denotes a reduced form parameter. One aspect to note about equation (4) is that it restricts the effects of policy on the welfare caseload (α and λ) to be constant across states. In her review of the literature, Blank (2002) reports a range of estimates of the effect of policy on welfare caseloads, although most of the estimates suggest that between 10 and 30 percent of the decline in the caseload was due to policy. In our analysis, we found that 25 percent of the decline in the caseload was due to policy: eight percent due to AFDC waivers and 17 percent due to TANF.[4] Thus, we expect the coefficients on TANF and AFDC waivers from equation (4) to be opposite in sign and between one-tenth and one-third the size of the coefficient on C(POLICY) in equation (2). The relatively small size of the expected effects may make it difficult to detect reliably a statistically significant effect. A point we return to below.

As noted, the primary identifying assumption underlying equations (1), (2) and (4) is that unmeasured state-specific trends are not correlated with welfare reform. To address the possibility that this assumption does not hold, we also estimate these equations using a sample of women who are in some ways similar to the women in the target group, but who are less likely to be affected by welfare reform. For example, if we focus on low-educated, unmarried women as the target group, an appropriate comparison group may be low-educated married women since relatively few married women receive AFDC/TANF. The rationale underlying this empirical strategy is that we expect the effect of welfare reform to be larger for women most likely affected

[4] To obtain this particular estimate, we constrained the effect of policy in equation (3) to be constant across states.

by the policy change. Therefore, if welfare reform does have a causal effect, we should observe a distinct pattern to the estimates: larger estimates for the group most affected. On the other hand, similar sized effects for both the target and comparison group suggests that welfare reform is associated with changes in prenatal care and infant health because of the influence of some omitted variable correlated with welfare reform; in this case the association does not reflect a causal relationship.

The comparison group approach is commonly referred to as a difference-in-differences (DD) estimator. It can be explicitly incorporated into a regression model as in equation (5):

$$\begin{aligned} \mathrm{Y_{ijt}} &= \beta_{\mathrm{j}} + \delta_{\mathrm{t}} + \gamma_1 \,\mathrm{TREAT_i} + \gamma_2 \mathrm{CASELOAD_{jt}} + \gamma_3 (\mathrm{TREAT_i \text{ x } CASELOAD_{jt}}) \\ &\quad + \mathrm{Z_{jt}}\Delta + \mathrm{X_{ijt}}\Gamma + \mathrm{u_{ijt}} \end{aligned} \tag{5}$$

Equation (5) is quite similar to equation (1) and it illustrates the logic of the difference-in-differences procedure. There are only a few new variables to define. First, there is the variable $\mathrm{TREAT_i}$, which equals one if a woman is likely to be affected by welfare reform, and zero otherwise. We use a woman's marital status to determine whether or not she is likely to be affected by welfare reform. Low-educated, unmarried women who are pregnant have high rates of welfare participation and are therefore classified as being in the target, or treatment, group. Low-educated, married women who are pregnant have very low rates of welfare participation. So we classify them as the comparison group (i.e., treat=0). Second, there is an interaction between the treatment group indicator and the welfare reform policy variable. The key parameter of interest is γ_3, which measures the effect of welfare reform on low-educated, unmarried women, holding constant state effects (β_j), year effects (δ_t), and state-year effects ($\mathrm{Reform_{jt}}$).[5]

The primary identification assumption of the difference-in-differences model is that state-year effects (γ_2) are the same for women who are affected by welfare reform (i.e., unmarried

[5] The least restrictive specification of equation (5) includes interactions between the target group indicator (TREAT) and all the other variables in the model. Estimates from this model would be identical to those obtained by estimating two separate models. This is the specification we adopt.

women) and for women who are unaffected by welfare reform (i.e., married women). In practice, two conditions need to be satisfied for this procedure to be valid. The comparison group has to contain a greater proportion of women who are likely to be affected by welfare reform than the treatment group, and in the absence of welfare reform, within-state changes in prenatal care utilization and infant health have to be the same for both the treatment and comparison group.

Data from the 1994 Current Population Survey indicated that 60 percent of unmarried mothers with less than a high school degree, and 37 percent of unmarried mothers with a high school degree received welfare in the past year. In contrast, low-educated, married mothers have very low rates of welfare participation. Again, data from the 1994 Current Population Survey reveal that only 11 percent of married mothers with less than high school degree, and three percent of married mother with a high school degree received welfare in the past year. Thus, our target and comparison groups satisfy, to a reasonable degree, the first of the two criteria, although the less than perfect classification will lead to estimates biased toward zero. The magnitude of the bias depends on the proportion of each group at risk. In our example, 60 (37) percent of the target group is at risk and 11 (3) percent of the comparison group is at risk. Therefore, the difference-in-differences estimate will be approximately 50 (34) percent of the true estimate.

As to the second criteria, given the similarity of their education levels, within-state variation in outcomes may be similar for the two groups, although it is difficult to document whether or not this assumption is true. If it is, estimates obtained using equation (5) may be given a causal interpretation. If not, we will obtain biased estimates. One factor that at first may appear to contradict this last assumption about the adequacy of the married women comparison group is the recent changes in Medicaid eligibility that may have affected married women, because of their higher income, more than unmarried women. However, for pregnant women, most eligibility changes were implemented by 1993, the first year of our analysis. In addition, we include the Medicaid income eligibility threshold for pregnant women in our model.

Data:

The principal source of data for this analysis is the National Natality Files for 1992 through 2000. These files contain information on all births in the United States collected from birth certificates. The Natality Files contain information on prenatal care use, birth weight, and information on maternal characteristics including age, race, education, marital status, medical risk factors and reproductive history. Unfortunately, birth certificates do not provide information about health insurance coverage, so we are prevented from analyzing this outcome. We use only observations for singleton births for women ages 19 to 39. Our sample is limited to low-educated (12 or fewer years of education) women—the primary target group of welfare reform. Given that we use education and marital status to define target and comparison groups we restrict the sample to women ages 19 to 39 since education and marital status are not effective indicators of risk for younger women. To reduce the number of observations and ease computation, we select a 50 percent random sample, which results in samples of close to two million observations.

The large sample sizes are important because DD estimates of the effect of welfare reform on prenatal care utilization and birth weight will be biased toward zero by what may be called measurement error resulting from the fact that not all women in the target group are at risk of being affected by welfare reform. Thus, the large sample sizes will allow us to detect reliably small effects. In addition, the large sample size allows us to obtain separate estimates for various subgroups; specifically, we divide the sample in two depending on whether a woman has 12 or fewer than 12 years of education. One possibility is that welfare reform will have larger effects on women with fewer than 12 years of education because a larger proportion of this group is at risk of welfare reform. Alternatively, it may be the case that welfare reform will have a larger effect on more educated (12 years of education) women because of a larger behavioral response by this group; for example, these may be more likely to work in response to welfare reform because of better labor market opportunities.

We examine the effect of welfare reform on prenatal care utilization and infant health. We examine three variables that measure different aspects of prenatal care: the total number of prenatal visits received; a dichotomous indicator of whether or not the mother received prenatal care in the first trimester (early care); and a dichotomous indicator of whether or not the mother received prenatal care starting in the third trimester (late care including no care). We measure infant health using the natural logarithm of birth weight; a dichotomous indicator of whether or not the birth was of low weight (less than 2500 grams); and a dichotomous indicator of whether or not the birth was premature (gestation less than 37 weeks). Table 1 presents sample means of the dependent variables by treatment group status.

The Natality files also include information about the mother's characteristics. We use this information to control for variation in outcomes due to differences in family preferences for child health, family income and the price of other infant health inputs (e.g., wages). Since we do not have direct measures of these variables, we use exogenous determinants of these factors for which we do have measures. Thus, in addition to the mother's education and marital status, which we use to stratify the sample, we also include in the model age of the mother (single year categories), race (non-Hispanic white, non-Hispanic black, Hispanic), and birth order of the child (a dummy variable indicating first born).

The data on the welfare caseload and welfare policies came from the Administration for Families and Children of the Department of Health and Human Services (http://www.acf.dhhs.gov). The welfare caseload is measured as the number of families receiving benefits in state j in year t and month k. We measure welfare reform policy using dummy variables indicating whether or not the state had an AFDC waiver or TANF program in effect. Both the caseload and policy variables are merged to the vital records data with a nine-month lag.

Results:

Estimates of the parameters of equations (1), (2) and (5) are obtained by ordinary least squares regression. Robust standard errors have been calculated under the assumption that there is clustering within state (Bertrand et al. 2002). All models include controls for age, race, whether this is the first born child, current and one-year lag of state Medicaid generosity for pregnant women and infants, current and one-year lag of state unemployment rate, state dummy variables, and year dummy variables. There are four samples: unmarried women with fewer than 12 years of education, unmarried women with 12 years of education, married women with fewer than 12 years of education and married women with 12 years of education. The first two samples are considered to be target groups—those likely to be affected by welfare reform—and the latter two samples are comparison groups—those unlikely to be affected by welfare reform. For each sample, we estimate three models. The first model includes the natural logarithm of the welfare caseload; the second model divides the caseload into that due to policy and that due to other factors; and the third model includes indicators of whether or not the state had an AFDC waiver or TANF. The dummy variable for AFDC waiver is set to zero at the time the state implements TANF.

Table 2 presents the estimates for women with less than 12 years of education. The top panel shows estimates for unmarried women and the bottom panel estimates for married women. In each panel, there are three rows, which present estimates from the three models [equations (1), (2) and (5)]. Estimates in each column pertain to a different dependent variable and column headings are labeled. The last column refers to a model in which the dependent variable is low birth weight and an indicator of premature birth is included as an explanatory variable. This model may be interpreted as an analysis of weight for gestational age.

Estimates in the top panel of Table 2 indicate that the welfare caseload is positively associated with earlier prenatal care and a greater number of prenatal care visits, and negatively (positively) associated with birth weight (low birth weight) and premature birth. Most of the estimates are statistically significant at the 0.05 level. The magnitudes of the estimated effects are small—between two and nine percent. A one unit change in the natural logarithm of the caseload represents an approximate 100 percent change in the caseload. So, a 50 percent decrease in the caseload, which is approximately the decline in the caseload during the 1990s, is associated with the following:

- a 2.7 percentage-point (4 percent of mean) decrease in first trimester care;
- a 1.0 percentage-point (9 percent of mean) increase in last trimester care;
- a 0.3 (3 percent of mean) decrease in the number of prenatal care visits;
- a 0.4 percent decrease in birth weight;
- a 0.25 percentage-point (3 percent) increase in low birth weight;
- and a 0.3 percentage-point (2 percent) increase in premature birth.

To obtain estimates of the effect of the welfare caseload on those at risk, or what is sometimes to referred to as the effect of the treatment on the treated, we would inflate these estimates by a factor of 1.67 since only 60 percent of the sample is at risk. But even after this adjustment, the effects remain relatively small—roughly between three (premature birth) and fifteen (last trimester care) percent of the sample means—given such a large reduction in the caseload.

The second row of the top panel examines whether changes in the caseload have different effects depending on the source of change. We consider two underlying causes: policy and other factors. Overall, the estimates in the second row suggest that changes in the caseload due to policy had larger effects on prenatal care and smaller effects on infant health than changes in the caseload due to other factors. However, none of the pairs of estimates are statistically different from each other. In other words, we cannot reject the hypothesis that changes in the caseload due to policy had the same effect as changes in the caseload due to other reasons.

Finally, in row three, we present the reduced form estimates. As suggested above, we expect these estimates to be opposite in sign to the estimates of the effect of the caseload and smaller—approximately 10 to 30 percent the magnitude of the caseload estimates. For the most part, the estimates are in line with expectations. However, none of the estimates are statistically significant. This may reflect the fact that we do not have sufficient statistical power to detect such small effects. Even though we have very large samples, in reality we only have 451 state-year observations on welfare policy, and we have accounted for this fact when constructing the standard errors (Bertrand et al. 2002). The important point, however, is that the estimates in row three are not inconsistent with those in row one.

We also investigated whether the effects of the welfare caseload and welfare policy differed by demographic characteristics. Specifically, we re-estimated equations (1), (2) and (5) for a sample of young (ages 19 to 29) women and for a sample of non-Hispanic, black women. These estimates are presented in Appendix Table 1. Overall, the estimates in Appendix Table 1 are quite similar to those in Table 2. Therefore, we do not discuss them in detail.

The bottom panel of Table 2 presents the estimates for married women with fewer than 12 years of education. This is a group that is unlikely to significantly affected by welfare reform. In general, we observe very similar estimated effects as those in the top panel. For example, estimates in row one suggest that a 50 percent decrease in the caseload is associated with the following:

- a 2.3 percentage-point (3 percent of mean) decrease in first trimester care;
- a 0.3 percentage-point (4 percent of mean) increase in last trimester care;
- a 0.3 (2 percent of mean) decrease in the number of prenatal care visits;

- a 0.4 percent decrease in birth weight;
- a 0.25 percentage-point (4 percent) increase in low birth weight;
- and a 0.5 percentage-point (5 percent) increase in premature birth.

Here too, the effects of the caseload do not differ by the underlying cause of changes in the caseload, as we cannot reject the equality of estimates in row two. Finally, few of the estimates associated with the two policies measures in row three are statistically significant, although the sign and magnitudes are generally consistent with the expectation that these estimates should be opposite in sign and smaller than those in row one.

The similarity of the estimates in the top and bottom panels of Table 2 is inconsistent with a causal effect of welfare on prenatal care and infant health for the affected group. Relatively few married women are at risk of welfare receipt and therefore welfare reform should have relatively little effect on their behavior and outcomes. The fact that changes in the welfare caseload are correlated with prenatal care and infant health for this group suggests that this correlation reflects the influence of other unmeasured trends. This raises questions about the association between the welfare caseload, and by extension welfare reform, and prenatal care and infant health of the affected group—low-educated unmarried women. Perhaps the same unmeasured factors are influencing their behavior and outcomes. The difference-in-differences procedure is one way to eliminate this confounding, although it assumes that the unmeasured influences are the same for the target and comparison groups.

The difference-in-differences (DD) estimates are presented in Table 3. The DD estimates in row one are small in magnitude and except for one are not statistically significant. A 50 percent decrease in the caseload is associated with the following:

- a 0.5 percentage-point (1 percent of mean) decrease in first trimester care;
- a 0.7 percentage-point (6 percent of mean) increase in last trimester care;
- a 0.05 (0.5 percent of mean) decrease in the number of prenatal care visits;

- a 0.1 percent decrease in birth weight;
- a 0 percentage-point (0 percent) increase in low birth weight;
- and a 0.4 percentage-point (3 percent) increase in premature birth.

Only the effect of the welfare caseload on last trimester care is statistically significant. DD estimates in row two are consistent with earlier findings and suggest that the effect of the caseload does not differ by whether the caseload change is associated with policy or other factors. And DD estimates of the effect of the policy variables are also small and statistically insignificant except for the effect of TANF on premature birth. In this case, TANF is associated with an (counterintuitive) improvement in birth outcomes, as measured by a decrease in premature birth.

In sum, the estimates in Tables 2 and 3 suggest that welfare reform had at most relatively small effects on the prenatal care use and infant health of unmarried women with less than 12 years of education. Estimates of the effect of the caseload and welfare policy obtained using just the target group indicated that decreases in the caseload such as those observed during the 1990s adversely affected prenatal care use and infant health. The effects (treatment on the treated) were relatively modest in size given the large (e.g., 50 percent) decrease in the caseload—approximately seven percent of the mean with a range of between three and fifteen percent. Importantly, the magnitudes of these estimates are appropriately inflated to reflect the fact that not all members of the target group are at risk. However, similar effects were observed for a sample of married women, a group not likely to be significantly affected by welfare reform. Indeed, difference-in-differences estimates suggest the decrease in the welfare caseload had virtually no effect on prenatal care use and infant health except for a six percent increase in last trimester care. Inflating the DD estimates by a factor of two to adjust for the classification error and to obtain estimates of the effect of treatment on the treated does not change the conclusion. Further, the significant effect of the caseload on last trimester care is not a robust finding since there was no association between the welfare caseload and first trimester care or the number of prenatal care visits.

Tables 4 presents estimates of the effect of the welfare caseload and welfare policy on the prenatal care and infant health of women with 12 years of education. Estimates in row one of the top panel suggest that changes in the welfare caseload are not statistically correlated with prenatal care use of this group, but are correlated with infant health. The caseload is positively (negatively) associated with birth weight (low birth weight). Estimates suggest that a 50 decrease in the caseload is associated with a 0.5 percent decrease in birth weight and a 0.3 percentage-point (3 percent) increase in low birth weight. Inflating these estimates to reflect the fact that only a portion of the group is likely affected by welfare reform would increase them by a factor of three, but they would still be relatively small. Estimates in row two of the top panel are similar to those in row one and we are unable to reject the hypothesis that the all changes in the caseload have the same effect regardless of the source of change—policy or other factors. Finally, estimates of the effect of welfare policy in row three are very small and not statistically significant. Again, we caution that since we expect the effects of policy to be one-tenth to one-third the size of the effects of the caseload, we may not have enough statistical power to detect the very small effects implied by the caseload estimates. This is particularly true given that we allow for clustering at the state level in the calculation of standard errors.

The bottom panel of Table 4 presents the estimates for the sample of married women with 12 years of education. In general, estimates in the bottom panel are similar to those in the top panel, but smaller in magnitude. There are few statistically significant effects; changes in the caseload are positively related to prenatal care use and birth weight. A 50 percent decrease in the caseload is associated with a 0.8 percentage-point (1 percent) decrease in first trimester care and a 0.2 percent increase in birth weight. The policy variables, TANF and AFDC waivers, are not statistically related to prenatal care and infant health, except for a negative association between AFDC waivers and birth weight.

Table 5 presents the DD estimates for unmarried women with 12 years of education. DD estimates in row one indicate that decreases in the caseload are associated with a significant decrease in birth weight and a significant increase in low birth weight. For example, a 50 percent decrease in the caseload is associated with a 0.3 percent decrease in birth weight and 0.2 percentage-point (2 percent) increase in low birth weight. The classification error associated with the target and comparison groups in this analysis suggest that these estimates should be inflated by a factor of three to obtain estimates of the effect of the treatment on the treated. Doing so, however, does not significantly change the characterization of them as relatively small effects given a 50 percent decrease in the welfare caseload. DD estimates of the effect of policy are very small and not statistically significant. In sum, estimates in Tables 4 and 5 suggest that welfare reform may have a small effect on infant weight of unmarried women with 12 years of education.

Conclusion:

There is a widespread belief that welfare reform has lead to a significant decline in health insurance coverage among low-income families. This belief is based on several pieces of information: studies of welfare "leavers," which find that a substantial proportion of former welfare recipients are uninsured in the year after leaving welfare (Moffitt and Slade 1997; Guyer 2000; Garrett and Holahan 2000; Garrett and Hudman 2002); studies of the effect of welfare reform on Medicaid enrollment, which find a significant decline in Medicaid enrollment among low-income women and children after the implementation of welfare reform (Families USA Foundation 1999; Kronebusch 2001; Ku and Garrett 2000); and evidence that administrative hurdles limit enrollment in Medicaid for low-income families not receiving public assistance (Ellwood 1999). As a result, welfare reform may have adversely affected the health care utilization and health of low-income pregnant women and infants. In addition, welfare reform may have altered work activity, stress levels, family income and other aspects of life for low-educated, unmarried women that may affect prenatal care use and birth weight. In this paper we provided evidence as to whether or not welfare reform has adversely affected prenatal care use and birth weight.

Our findings indicate that welfare reform had at most relatively small effects on the prenatal care use and infant health of low-educated unmarried women. Among unmarried women with less than 12 years of education, decreases in the welfare caseload were associated with less prenatal care and lower weight infants. The size of the effects depends on the estimation strategy. Simple, before and after estimates (Table 3) appropriately inflated to reflect that only part of the target group is at risk indicate that the 50 percent decrease in the welfare caseload over the 1990s is associated with the following: a seven percent decrease in first trimester care; a 15 percent increase in last trimester care; a five percent decrease in the number of number of prenatal care visits; and a five percent increase in low birth weight. DD estimates suggest even smaller effects: a two percent decrease in first trimester care; a 10 percent increase in last trimester care; a one percent decrease in the number of prenatal care visits; and virtually no change in birth weight.

Among unmarried women with 12 years of education, estimates indicate similarly small effects. In this case, there is little evidence that welfare reform affected prenatal care. Estimates from Table 4 suggest that a 50 percent decrease in the caseload was associated with a one percent decrease in birth weight and a 10 percent increase in low birth weight. DD estimates indicate smaller effects for the same outcomes of one percent and six percent, respectively.

Our results are broadly consistent with those of Currie and Grogger (2002). For example, they report that a one percentage point decrease in the welfare rate (caseload divided by population) is associated with a 1.2 percentage-point decrease in first trimester care for low-income (SES) white women. A one-percentage point change in the welfare rate represents approximately a 15 percent change, and a 1.2 percentage point change in first trimester care represents approximately a two percent change. So the elasticity between changes in first trimester care and the welfare caseload is 0.13. A similar calculation using our estimates suggests an elasticity of the welfare caseload and first trimester care of between zero and 0.14. Currie and

Grogger (2002) find no effect of the welfare caseload on birth weight for low-income white women. We find effects although they are small. A 50 percent decrease in the welfare caseload is associated with a zero to 10 percent increase in low birth weight.

The relatively small effects of changes in welfare policy and welfare caseload on the prenatal care use and birth weight of low-educated women may appear surprising given the large reported declines in health insurance among this group. However, recent evidence by Kaestner and Kaushal (2003) suggest smaller declines in insurance coverage than earlier estimates that are consistent with the relatively small effect found here. Kaushal and Kaestner (2003) reported that the approximately halving of the caseload in the 1990s was associated with a one to two percentage-point increase in the proportion of low-educated, unmarried women who are uninsured. This figure is significantly less than those implied by earlier studies. More importantly, the smaller estimate of the effect of welfare reform on the number of uninsured suggests that average effects of welfare reform on prenatal care and birth weight should be small since the health insurance coverage of only a small part of the sample has been affected by welfare reform. While welfare reform may affect prenatal care utilization and birth weight through channels other than health insurance, the primary causal link between welfare reform and utilization of services and birth weight is through health insurance. The small effects of welfare reform on prenatal care that we find are consistent with the relatively small number of women whose health insurance is adversely affected. However, these small effects for the total sample may mask relatively large effects for those women who actually lost insurance because of welfare reform.

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Table 1
Sample Means

Variable	Unmarried Education<12	Married Education<12	Unmarried Education=12	Married Education=12
First Trimester Care	0.63	0.71	0.71	0.86
Last Trimester Care or No Care	0.11	0.07	0.07	0.02
Number of Prenatal Care Visits	9.69	10.50	10.68	11.85
Log Birth Weight	8.06	8.10	8.06	8.11
Low Birth Weight (LBW)	0.09	0.06	0.09	0.05
Premature Birth - Gestation < 37 Weeks)	0.14	0.10	0.13	0.09
Number of Observations	1,295,500	1,307,297	1,825,775	3,330,590

Note: Number of observations refers to maximum number available for analysis. Some variables will have slightly less due to missing values.

Table 2. Estimates of the Effect of AFDC/TANF Caseload and Welfare Reform Policies on Prenatal Care and Birth Weight
Women with less than 12 Years of Education

Unmarried Women	First Trimester Care	Last Trimester Care or No Care	Number of Prenatal Visits	Log Birth Weight	LBW	Premature Birth	LBW / Premature
Log Caseload	0.053** (0.014)	-0.020** (0.007)	0.597** (0.247)	0.008** (0.002)	-0.005 (0.003)	-0.003 (0.004)	-0.005** (0.002)
Log Caseload Policy	0.061** (0.018)	-0.024** (0.009)	0.794** (0.322)	0.007** (0.003)	-0.004 (0.003)	-0.004 (0.005)	-0.003 (0.002)
Log Caseload Residual	0.038** (0.015)	-0.014 (0.007)	0.214 (0.145)	0.009** (0.003)	-0.008** (0.003)	0.001 (0.004)	-0.009** (0.004)
TANF	-0.005 (0.008)	0.002 (0.004)	-0.045 (0.072)	-0.001 (0.001)	-0.001 (0.002)	-0.003 (0.002)	-0.000 (0.002)
AFDC Waiver	-0.007 (0.006)	0.005 (0.003)	0.024 (0.043)	-0.001 (0.001)	0.001 (0.001)	-0.000 (0.001)	0.001 (0.001)
Married Women	First Trimester Care	Last Trimester Care or No Care	Number of Prenatal Visits	Log Birth Weight	LBW	Premature Birth	LBW / Premature
Log Caseload	0.045** (0.016)	-0.006 (0.006)	0.507** (0.266)	0.007** (0.002)	-0.005** (0.002)	-0.010** (0.004)	-0.002 (0.002)
Log Caseload Policy	0.052** (0.022)	-0.007 (0.009)	0.686** (0.297)	0.007 (0.003)	-0.006** (0.003)	-0.014** (0.005)	-0.002 (0.002)
Log Caseload Residual	0.029** (0.011)	-0.004 (0.005)	0.150 (0.140)	0.007** (0.003)	-0.004 (0.002)	-0.003 (0.003)	-0.003 (0.002)
TANF	-0.012 (0.008)	0.001 (0.004)	-0.076 (0.079)	-0.003** (0.001)	0.002 (0.001)	0.005** (0.002)	-0.000 (0.001)
AFDC Waiver	-0.008 (0.004)	0.003 (0.002)	-0.004 (0.046)	-0.001 (0.001)	0.001 (0.001)	0.001 (0.001)	0.000 (0.001)

Notes: All models include the following controls: age (dummy variables for each year of age), race (non-Hispanic white, non-Hispanic black, Hispanic), dummy variable for first born, dummy variables indicating current and one-year lag of Medicaid generosity for pregnant women, current and one-year lag of unemployment rate, state dummy variables, and year dummy variables. Robust standard errors are in parentheses (clustering on state). ** 0 < p-value < 0.05

Table 3
Difference-in-Differences Estimates of the Effect of AFDC/TANF Caseload and Welfare Reform Policies on Prenatal Care and Birth Weight
Unmarried Women with less than 12 Years of Education

Difference-in-Differences	First Trimester Care	Last Trimester Care or No Care	Number of Prenatal Visits	Log Birth Weight	LBW	Premature Birth	LBW / Premature
Log Caseload	0.009 (0.012)	-0.014** (0.006)	0.090 (0.098)	0.001 (0.002)	-0.000 (0.002)	0.008 (0.006)	-0.003 (0.002)
Log Caseload Policy	0.009 (0.013)	-0.017** (0.006)	0.108 (0.117)	0.001 (0.003)	0.001 (0.003)	0.010 (0.007)	-0.001 (0.002)
Log Caseload Residual	0.009 (0.013)	-0.010 (0.008)	0.063 (0.100)	0.002 (0.003)	-0.003 (0.004)	0.004 (0.006)	-0.006 (0.004)
TANF	0.007 (0.005)	0.001 (0.003)	0.030 (0.042)	0.002 (0.001)	-0.002 (0.002)	-0.008** (0.003)	0.000 (0.002)
AFDC Waiver	0.001 (0.004)	0.002 (0.002)	0.028 (0.026)	-0.000 (0.001)	0.000 (0.001)	-0.002 (0.001)	0.000 (0.001)

Notes: See table 2.

Table 4
Estimates of the Effect of AFDC/TANF Caseload and Welfare Reform Policies on Prenatal Care and Birth Weight
Women with 12 Years of Education

Unmarried Women	First Trimester Care	Last Trimester Care or No Care	Number of Prenatal Visits	Log Birth Weight	LBW	Premature Birth	LBW / Premature
Log Caseload	0.018 (0.010)	-0.006 (0.004)	0.209 (0.131)	0.009** (0.003)	-0.006** (0.002)	-0.002 (0.002)	-0.005** (0.002)
Log Caseload Policy	0.016 (0.012)	-0.008 (0.005)	0.261 (0.163)	0.011** (0.003)	-0.006** (0.002)	-0.005 (0.002)	-0.004** (0.002)
Log Caseload Residual	0.021 (0.011)	-0.003 (0.005)	0.110 (0.110)	0.006 (0.004)	-0.005 (0.003)	0.004 (0.004)	-0.006 (0.003)
TANF	0.004 (0.007)	-0.000 (0.003)	0.036 (0.051)	-0.002 (0.001)	0.001 (0.001)	-0.001 (0.001)	0.001 (0.001)
AFDC Waiver	-0.006 (0.006)	0.002 (0.003)	0.015 (0.052)	0.000 (0.001)	-0.001 (0.001)	0.001 (0.001)	-0.001 (0.001)
Married Women	First Trimester Care	Last Trimester Care or No Care	Number of Prenatal Visits	Log Birth Weight	LBW	Premature Birth	LBW / Premature
Log Caseload	0.015** (0.006)	-0.003 (0.002)	0.087 (0.103)	0.004** (0.002)	-0.001 (0.001)	-0.003 (0.002)	-0.000 (0.001)
Log Caseload Policy	0.014 (0.008)	-0.004 (0.003)	0.103 (0.116)	0.004 (0.002)	-0.001 (0.001)	-0.005** (0.002)	0.000 (0.001)
Log Caseload Residual	0.016** (0.006)	-0.001 (0.002)	0.057 (0.111)	0.003 (0.002)	-0.000 (0.002)	0.001 (0.002)	-0.001 (0.002)
TANF	-0.002 (0.004)	0.002 (0.001)	0.031 (0.036)	-0.002 (0.001)	0.000 (0.001)	0.000 (0.002)	-0.001 (0.001)
AFDC Waiver	-0.003 (0.003)	0.002 (0.001)	0.016 (0.034)	-0.001** (0.000)	0.000 (0.000)	0.000 (0.001)	0.000 (0.000)

Notes: See table 2.

Table 5
Difference-in-Differences Estimates of the Effect of AFDC/TANF Caseload and Welfare Reform Policies on Prenatal Care and Birth Weight
Unmarried Women with 12 Years of Education

Difference-in-Differences	First Trimester Care	Last Trimester Care or No Care	Number of Prenatal Visits	Log Birth Weight	LBW	Premature Birth	LBW / Premature
Log Caseload	0.003 (0.008)	-0.003 (0.004)	0.122 (0.074)	0.006** (0.001)	-0.004** (0.002)	0.002 (0.002)	-0.005** (0.002)
Log Caseload Policy	0.002 (0.009)	-0.004 (0.003)	0.158 (0.091)	0.007** (0.002)	-0.005** (0.002)	0.007 (0.003)	-0.004** (0.002)
Log Caseload Residual	0.005 (0.010)	-0.001 (0.004)	0.053 (0.079)	0.003 (0.003)	-0.004 (0.003)	0.003 (0.004)	-0.005 (0.003)
TANF	0.005 (0.006)	-0.002 (0.003)	0.005 (0.031)	-0.000 (0.001)	-0.002 (0.002)	0.000 (0.001)	0.001 (0.001)
AFDC Waiver	-0.002 (0.004)	-0.000 (0.002)	-0.001 (0.033)	0.002 (0.001)	0.000 (0.001)	-0.001 (0.001)	-0.000 (0.000)

Notes: See table 2.

Appendix Table 1
Estimates of the Effect of AFDC/TANF Caseload and Welfare Reform Policies on Prenatal Care and Birth Weight
Unmarried Women with less than 12 Years of Education: By Demographic Characteristics

Young Women (Ages 19 to 29)	First Trimester Care	Last Trimester Care or No Care	Number of Prenatal Visits	Log Birth Weight	LBW	Premature Birth	LBW / Premature
Log Caseload	0.050** (0.014)	-0.019** (0.006)	0.566** (0.245)	0.007** (0.002)	-0.004 (0.002)	-0.002 (0.004)	-0.004 (0.002)
Log Caseload Policy	0.055** (0.018)	-0.022** (0.009)	0.749** (0.319)	0.007** (0.002)	-0.004 (0.003)	-0.005 (0.005)	-0.002 (0.003)
Log Caseload Residual	0.038** (0.015)	-0.014 (0.008)	0.215 (0.145)	0.008** (0.003)	-0.006 (0.003)	0.003 (0.004)	-0.008** (0.004)
TANF	-0.006 (0.008)	0.002 (0.004)	0.047 (0.070)	-0.001 (0.001)	-0.001 (0.002)	-0.002 (0.002)	-0.000 (0.002)
AFDC Waiver	-0.006 (0.008)	0.005 (0.003)	0.025 (0.042)	-0.001 (0.001)	0.001 (0.001)	-0.000 (0.001)	0.002 (0.001)
Non-Hispanic Black Women	**First Trimester Care**	**Last Trimester Care or No Care**	**Number of Prenatal Visits**	**Log Birth Weight**	**LBW**	**Premature Birth**	**LBW / Premature**
Log Caseload	0.042** (0.018)	-0.020** (0.009)	0.195 (0.177)	0.018** (0.004)	-0.017** (0.006)	-0.016** (0.006)	-0.010 (0.005)
Log Caseload Policy	0.042** (0.020)	-0.028** (0.012)	0.271 (0.236)	0.019** (0.007)	-0.017** (0.008)	-0.012 (0.008)	-0.011 (0.006)
Log Caseload Residual	0.042** (0.018)	-0.005 (0.012)	0.065 (0.144)	0.017** (0.007)	-0.016 (0.009)	-0.022** (0.009)	-0.009 (0.009)
TANF	-0.000 (0.008)	0.005 (0.004)	0.084 (0.100)	-0.006** (0.003)	0.004 (0.005)	-0.003 (0.005)	0.004 (0.005)
AFDC Waiver	0.001 (0.009)	0.003 (0.004)	0.086 (0.055)	-0.000 (0.002)	0.001 (0.003)	-0.005 (0.003)	0.002 (0.003)

Notes: See table 2.