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Abstract

Evaluations of changes to the Medicaid program have focused on increases in the generosity of income cutoffs for eligibility. Previous research shows that despite dramatic increases in the number of births paid for by Medicaid, many eligible women are still getting inadequate prenatal care. States have addressed this problem by adopting administrative measures designed to simplify the Medicaid application process and encourage the use of prenatal care. At the same time, recent declines in welfare caseloads may have caused many women to lose their Medicaid coverage by effectively increasing administrative barriers to obtaining care.

We examine the effects of changes in income eligibility, administrative reforms, and welfare caseloads on the use of prenatal care and infant health using data from birth certificates covering all U.S. births between 1990 and 1996. We find that increases in income cutoffs were associated with increased use of prenatal care among whites, while decreases in welfare caseloads were associated with reduced use of prenatal care, especially among blacks. Reduced form estimates suggest that changes in income cutoffs were associated with a reduced probability of fetal death among blacks and disadvantaged whites. None of the administrative reforms we consider had consistently positive effects. These results suggest that the administrative reforms undertaken to date have been insufficient to break the close link between participation in welfare and access to prenatal care under the Medicaid program.

Medicaid, a public insurance program that largely serves poor women and children, has undergone extensive changes since the mid-1980s. An important goal of these changes is to improve infant health by encouraging pregnant women to obtain adequate prenatal care. To the extent that prenatal care improves birth outcomes, policies that encourage the use of prenatal care will reduce infant mortality, which remains high in the U.S. compared to other developed countries.

Previous research evaluating changes to the Medicaid program has focused on increases in the generosity of the income cutoffs for Medicaid eligibility (c.f. Currie and Gruber, 1996). Although these eligibility expansions have increased the fraction of births paid for by Medicaid from 15 to nearly 40 percent (National Governor's Association, 1997), many women still fail to obtain adequate prenatal care.

In order to address this problem, states have adopted a variety of administrative measures to reduce non-price barriers to care. The objective of these policies is to encourage the use of prenatal care by simplifying the Medicaid application process for pregnant women. Common reforms included: presuming that pregnant women were eligible for Medicaid while their applications were being processed and/or expediting the processing of applications for pregnant women; "outstationing" Medicaid eligibility workers in hospitals that serve low income women; dramatically shortening and simplifying application forms; and eliminating the requirement for face-to-face interviews by allowing mail-in applications from pregnant women.

At the same time, many states experienced sharp declines in their welfare caseloads; the welfare rolls have fallen by 50 percent since 1993, at least in part because of recent welfare reforms (Administration for Children and Families, 1999; U.S. Council of Economic Advisers, 1997, 1999; Grogger and Michalopoulos, 1999; Moffitt, 1999; Grogger, 2000). These declines had the potential to

reduce Medicaid coverage because many poor women obtained Medicaid coverage "automatically" when they enrolled in the Aid to Families with Dependent Children (AFDC) program. Thus, the loss of AFDC effectively raised the administrative bar for women seeking Medicaid coverage, by requiring them to go through a separate and unfamiliar application process (Ellwood and Kenney, 1995).

This paper compares the effects of these changes on the utilization of prenatal care. We also examine the reduced form effects of these policy changes on birthweight, an important indicator of infant health, and on the probability of fetal death. In order to conduct these analyses, we merge information about income cutoffs, administrative reforms, and welfare caseloads with the Vital Statistics Detail Natality files for 1990 to 1996, which contain information about virtually every birth that occurred in the United States over this period, and with fetal death records. The available literature suggests that the effects of the policies we examine may vary with race and socio-economic status. Thus, we conduct separate analyses of the effects of the administrative reforms by race as well as for disadvantaged mothers (unmarried teens and high school dropouts).

We find that higher income cutoffs and higher welfare caseloads increased the utilization of prenatal care and reduced fetal deaths. None of the administrative reforms we consider had consistently positive effects. These results suggest that administrative reforms undertaken to date have been insufficient to break the close link between participation in welfare programs and access to prenatal care under the Medicaid program.

2. Background

2.1 Changes to the Medicaid Program and to Welfare

Eligibility for the Medicaid program used to be largely restricted to participants in AFDC, which meant that income cutoffs were very low. Beginning in the late 1980s, Congress enacted several laws breaking the link between welfare and Medicaid eligibility. By April 1990, states were required by federal law to provide Medicaid coverage to pregnant women with incomes up to 133% of the poverty level, and states had the option of receiving federal matching funds to cover women with incomes up to 185% of the poverty level.

Studies of the first years of these expansions of the income cutoffs show that many newly eligible, uninsured, pregnant women did not take up coverage in time to benefit from improved prenatal care. For example, Currie and Gruber (1996) suggest that as many as half of these women did not take up coverage in time. Moreover, non-participation was concentrated among women who were not income-eligible for AFDC, suggesting that simply increasing the income eligibility cutoff did not break the link between welfare receipt and Medicaid coverage.

Administrative barriers to care have been extensively criticized in the public health literature (c.f. Brown, 1989; Kalmuss and Fennelly, 1990, and Aved et al., 1993). In response, state governments have adopted a range of administrative reforms designed to make it easier for pregnant women to enroll, irrespective of their welfare status. Table 1 shows the number of states that had adopted these measures in each half year between 1990 and 1996.[1] The for these data is the National Governor's Association's (NGA) Maternal and Child Health newsletters, which began to publish information

[1] In addition to the measures shown in Table 1, most states also eliminated asset tests for Medicaid eligibility by 1990, meaning that applicants no longer had to document the value of assets such as cars and life insurance policies. Since this change occurred before our sample period, we do not evaluate it.

regarding the administrative reforms in 1990. The table shows that the law often changed in mid-year. Hence, we merge the NGA data to the Vital Statistics data by half year rather than years. Finally, Table 2 indicates that many states have continued to raise the income eligibility thresholds for pregnant women beyond the 133% of the federal poverty line that is currently required by federal law.

At the same time that Medicaid eligibility for pregnant women was expanding, a number of states were reforming their welfare systems under waivers from the Aid to Families with Dependent Children (AFDC) program. These welfare reform efforts culminated in the passage of the federal Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA) of 1996, which required states to replace AFDC with the new Temporary Assistance for Needy Families Program by July 1997. If the administrative reforms to the Medicaid program were successful in breaking down barriers to Medicaid access, then we would not expect changes in the welfare rolls to have much effect on the use of prenatal care among Medicaid-eligible pregnant women. The reason is that the women remain eligible for Medicaid coverage in the event of pregnancy whether or not they are welfare recipients. However, if administrative barriers to accessing the program remain important, then we might expect the welfare reforms to have the unintended consequence of reducing access to prenatal care under the Medicaid program. Hence, other things being equal, the effect of the welfare rate on the utilization of prenatal care provides a test of the importance of administrative barriers to accessing Medicaid.

The annual state-level data on welfare caseloads that we analyze comes from the U.S. Department of Health and Human Services (various years); we divide the number of recipients by the state population in order to obtain a welfare utilization rate.[2] Over our sample period the mean

[2] Since the number of recipients includes children as well as women, it makes more sense to divide

utilization rate was 4.5 percent, but it varied greatly between states and within states over time (Blank, 1997; Wallace and Blank, 1999). For example, the utilization rate in Massachusetts rose from 4.4 percent in 1990 to 5.4 percent in 1993, falling to 3.9 percent by 1996. In California, it rose from 6.4 percent in 1990 to 8.5 in 1995, and stood at 8.3 percent in 1996. Welfare rates are shown for selected states and years in Table 2.

Of course welfare rates also reflect economic conditions facing poor mothers and their children. We expect that the administrative link between welfare and Medicaid will facilitate use of prenatal care, while poor economic conditions will be associated with lower utilization of prenatal care, other things being equal. Economic conditions will be controlled for by the inclusion of annual state-level unemployment rates from the Bureau of Labor Statistics' Local Area Unemployment Statistics program in our models. If economic conditions were perfectly controlled for using unemployment rates, then we might expect higher welfare rates to be associated with better birth outcomes through an income effect, in addition to any effect on birth outcomes via prenatal care. That is, if welfare makes families better off other things being equal, then this may have a positive effect on birth outcomes.[3]

However, to the extent that the unemployment rate is not a perfect control for economic conditions, higher welfare rates may be a sign of hardship among low income families. In this case, higher welfare rates would be associated with worse birth outcomes, and we could expect our estimate of the effects of welfare rates to understate the true importance of the administrative link between welfare and Medicaid. In what follows, we will show that the welfare rate has a significant positive

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by state population than to divide by the number of women in the state, for example.

[3] We also estimated models including state poverty rates but found that this variable was not statistically significant in any of our models.

effect on both utilization of prenatal care among both whites and blacks, as well as a positive effect on birth outcomes.

2.2 Non-Monetary Barriers to Care

The fact that many people eligible for social programs do not participate in them suggests that income eligibility is not the only barrier to care. For example, Blank and Ruggles (1996) find that only two-thirds of those eligible for AFDC and Food Stamps participate. Blank and Card (1991) find similar results for participation in the Unemployment Insurance program. Economists have focused on two explanations for eligible non-participation: "welfare stigma" (c.f. Moffitt, 1983) and transactions costs associated with enrollment in the program. For example, Blank and Ruggles find that those with short expected welfare stays are the least likely to enroll, presumably because the expected benefits of enrollment do not outweigh the costs of applying. The rates of non-participation in Medicaid are even higher than those of non-participation in other programs, which suggests that barriers to obtaining medical care under this program may be especially great.

Pregnant women who are not automatically eligible for Medicaid due to participation in welfare must go through a separate application process. They may be required to show birth certificates and/or citizenship papers, rent receipts and utility bills to prove residency, and pay stubs as proof of income. Many states have a time limit on the number of days the applicant can take to provide documentation and applicants are often required to return for several interviews. The available evidence suggests that up to a quarter of Medicaid applications are denied because applicants do not fulfill these administrative requirements: They cannot produce the necessary documentation within the required time or fail to attend all of the required interviews (GAO, 1994).

While it may be quite difficult for individuals to overcome these barriers, hospitals have both the incentive (since they must provide care to women in labor), and the means to do so (Saywell, 1989). Many hospitals have established Medicaid enrollment offices on site. These offices assist people in completing applications and tell them how to obtain necessary documentation (GAO, 1994). Hospitals in at least 32 states and the District of Columbia also employ private firms to help them enroll eligible patients in the Medicaid program. Thus, it is not surprising that births can be covered by Medicaid even when prenatal care is not.

Even those who are covered by Medicaid may have difficulty obtaining preventive care, since Medicaid typically pays about half of what private health insurance would pay. One study of new mothers who had arrived in emergency rooms to deliver with "no physician of record" found that 64 percent of the women cited their inability to find a doctor willing to accept them as the largest barrier to obtaining prenatal care (Aved et al., 1993). These problems may be even more severe for minority mothers. American cities are highly segregated by race and income (Massey and Denton, 1993) and urban blacks often live in parts of the city that are shunned by physicians in private practice and hence are more likely to be served by large urban teaching hospitals (Fossett *et al*., 1992).

Finally, we ask whether responses to administrative reforms vary by socioeconomic status, where lower socioeconomic status is proxied using unmarried teen motherhood and/or less than a high school education. While it may seem that barriers such as lengthy application forms would prove most daunting to the most disadvantaged women, it is also possible that these women face other barriers that are likely to reduce the effectiveness of the administrative reforms. For example, some poor mothers do not use the prenatal care that is available to them. One study of low-income women in New York City found that the two most commonly cited reasons for failing to obtain adequate prenatal care were not

lack of insurance coverage, but "feeling depressed and not up to going for care" and "needing time and energy to deal with other problems" (Brown, 1989). We also estimate models separately for women of high socio-economic status, who are unlikely to be eligible for Medicaid coverage. We proxy high SES by selecting married, college educated women. Dubay et al. (2001) report that in 1992, 95% of such women had private health insurance coverage.

In summary, the literature suggests that there are many non-monetary barriers to accessing the Medicaid program. Moreover, the importance of these barriers may vary with the characteristics of the potential patients. The administrative reforms that have been implemented by the states address some, but by no means all, of these problems. Thus, the extent to which they will be successful in increasing participation must be assessed empirically. If the administrative reforms have not been successful in easing access to Medicaid among those who are not on welfare, then the declines in the welfare caseloads that have accompanied welfare reform can be expected to have affected the utilization of prenatal care among Medicaid eligible pregnant women.

2.3 The Importance of Prenatal Care

Many studies have shown that women who use prenatal care earlier and more faithfully have healthier babies. But it is not obvious that this association reflects usage of prenatal care per se, rather than other characteristics of the mother that make her both more likely to use prenatal care and more likely to bear a healthy baby. Clinical studies have attempted to demonstrate a link between prenatal care and improved infant health. These studies generally focus on birthweight, and distinguish between two types of infants, those who are born prematurely (but may be of normal weight given their gestational age) and those who are of low weight given their gestational age. Premature babies (who

are often low birthweight) account for less than seven percent of total births, but consume half of all hospital delivery charges (Marbella et al., 1998).

The results of these studies have been disappointing. First, prenatal care has been shown to be effective in reducing the risk of low birthweight, but only among relatively mature full-term infants. It has not been shown to reduce the risk of premature birth, or the incidence of inter-uterine growth retardation among infants who are born prematurely (Alexander and Korenbrot, 1995). Large-scale survey-based studies that have attempted to control for unobserved differences between women who do and do not seek prenatal care also typically find little or no effect of prenatal care (c.f. Rosenzweig and Schultz, 1982, 1983, 1988; Frank et al., 1991; Corman et al., 1997).

These results are puzzling given that there are good biological reasons for expecting prenatal care to be effective. First, we know that smoking is a leading cause of both prematurity and growth retardation. So one would expect prenatal care that was successful in persuading pregnant women not to smoke to have an impact. Second, several recent studies have shown that antibiotic treatment of vaginal infections results in significant reductions in the incidence of preterm birth and improvements in birth outcomes. Similarly, studies have shown that maternal stress is associated with an increased risk of preterm birth, either because stress hormones encourage contractions or because they suppress the immune system and lead to infection. Interventions that decrease stress by teaching exercise and relaxation techniques have also been shown to be effective in some trials (Armson and Moutquin, 1998).

Perhaps routine prenatal care is often ineffective because it does not emphasize smoking cessation, stress reduction and/or the diagnosis and treatment of mild infections. A study of a national sample of mothers found that many had not received advice about reducing or eliminating alcohol,

smoking, and illegal drugs, eating the proper foods, and taking vitamin supplements. The correlation between getting good advice and positive outcomes was greatest for those at highest risk of bad outcomes: women with household incomes less than $6000, teenagers, women who received otherwise inadequate prenatal care, those using public clinics, smokers, and women with a previous history of negative birth outcomes (Kogan et al., 1994). One thing that standard prenatal care does do is closely monitor maternal weight gain. Since we know that low maternal weight gain is associated with low birthweight in full term infants, this emphasis may account for the positive effects of prenatal care on birthweight among relatively mature infants that have been documented in the clinical literature. It is possible that the value of prenatal care will increase as providers become better trained in techniques of stress management, diagnoses and control of minor infection, and smoking cessation.

2.4 Measures of Prenatal Care

We use two common measures of prenatal care that can be constructed from information available on the birth certificate.[4] The first is whether care began after the first trimester. Timeliness is an important element of prenatal care adequacy. The first visit is used to establish baselines for such things as maternal weight and blood pressure which can then be used to track the progress of the pregnancy. The second measure is whether the mother received inadequate prenatal care where adequacy is assessed by taking into account when prenatal care began, and the total number of visits conditional on the length of the pregnancy. This measure is constructed by the National Center for Health Statistics and appears on the Vital Statistics Detailed Natality files. In principle, it is a better

[4] In an earlier version of this paper we also looked at whether the mother received any prenatal care, but we found that the vast majority of mothers reported receiving prenatal care at some point during their pregnancies.

measure than whether the care began in the first trimester. However, it may be measured with more error.

2.5 Birthweight as a Measure of Infant Health

Birthweight is a key indicator of the underlying health of newborns. Many previous analyses have focused on low birthweight, which is defined as birthweight less than 2500 grams, about 5.5 pounds. In 1987, the Office of Technology Assessment (OTA, 1987a) found that children of low birthweight were at high risk of neonatal mortality and experienced post-neonatal mortality rates 10 to 15 times greater than those found among infants of higher birthweight. Low birthweight survivors were also at higher risk of handicaps such as cerebral palsy, seizure disorders, blindness, deafness and learning disorders (McCormick et al., 1992; OTA, 1987b, Chaikind and Corman, 1990).

Typically, medical problems are more severe for the lightest infants. For example, Horbar et al. (1993) found that in a sample of very low birthweight children (usually defined as birthweight less than 1500 grams), each increase in birthweight of 100 grams was associated with a decrease of approximately 10% in the probability of death, other things being equal. Moreover, recent improvements in medical technology have decreased the critical birthweight below which infants are at high risk of death and impairment (Cutler and Meara, 1999). Thus, in addition to examining the incidence of low birthweight, we also examine the incidence of very low birthweight.

2.6 Fetal Deaths as an Outcome Measure[5]

Birth certificates record information only for infants who are born alive. However, improved

[5] Most of the discussion of fetal deaths is taken from U.S. DHHS (2000).

prenatal care and delivery may increase the probability that infants are born alive. If the infants who are saved are disproportionately low birthweight, then the estimated effects of the policy variables on birthweight may be biased downwards by fetal selection.

Fetal deaths are also increasingly becoming a subject of research interest in their own right as more attention is paid to the health of the fetus in utero. The World Health Organization defines fetal deaths as deaths after 28 weeks gestation. However, the U.S. tracks fetal deaths after 20 weeks. Fetal deaths after 20 weeks of gestation are almost as common as infant deaths at 7 per 1,000 live births plus fetal deaths in 1995. Like infant mortality, the fetal death rate is much higher among blacks than among whites at 12.7 per 1,000 in 1995. Fetal deaths also decline substantially with education from 6.5 per 1,000 among high school dropout mothers to 4.8 per 1,000 among college educated mothers. The Healthy People 2010 Objectives include a call for the reduction of the U.S. fetal death rate to 4 per 1,000 live births plus fetal deaths (U.S. DHHS, 2000).

Unlike birthweight, which measures the underlying health of the fetus, fetal deaths reflect both underlying health conditions, and the quality of medical care received during pregnancy and delivery. Fetal deaths are often associated with maternal complications of pregnancy such as problems with amniotic fluid levels and blood disorders. If these conditions are detected, the death of the fetus can often be prevented by either proper care of the pregnant woman or emergency delivery. Rates of fetal mortality are 35% greater than average in women who smoke, and 77% higher in women who drink during pregnancy. Fetal deaths after 20 weeks may also be associated with congenital defects, though these deaths are more common prior to 20 weeks gestation.

3. Methods

We assume that mothers decide whether or not to participate in Medicaid prenatally by weighing the costs against the perceived benefits of participation. The costs of participation include the time and effort necessary to apply, as well as any stigma associated with participation. Benefits include free prenatal care, which is expected to result in a healthier infant. This simple conceptual model implies that anything that increases the value of the subsidy provided by the program will increase participation rates. Likewise, anything that decreases the cost of participation or the stigma associated with participating will also increase participation. Moreover, the same factors that influence prenatal participation in Medicaid will also influence infant health, if prenatal care is at all effective.

Many of the administrative reforms adopted in the 1990s were intended precisely to lower the costs and/or raise the benefits associated with participation in Medicaid by pregnant women. Specifically, presumptive eligibility and expedited application processing seem likely to raise the benefit provided by the program by giving health care providers an incentive to provide care even prior to a final determination of eligibility. Reforms such as shortening applications should reduce the cost of applying for Medicaid. Outstationing and allowing mail-in applications (rather than face-to-face interviews) both reduce costs and may reduce stigma. On the other hand, falling welfare rolls can be expected to increase the costs of applying for Medicaid, other things being equal.

These considerations led us to specify an empirical model of the following form:

(1) $P_{sti}^{*} = aADMIN_{st} + bCUTOFF_{st} + cWELFARE_{st} + dUNEMP_{st} + eX_{sti} + u_s + v_t + w_{st} + e_{sti}$,

for $s = 1, \ldots, S$, $t = 1,\ldots,T$; and $i = 1,\ldots,N_{st}$, where P^{*} is participation in prenatal care by the i^{th} expectant mother in the s^{th} state at time t. $ADMIN_{st}$ is a vector of dummy variables indicating whether each of the administrative reforms was in effect in state s at period t; $CUTOFF_{st}$ is a variable giving the income eligibility cutoff in effect in state s during period t as a percentage of the poverty level;

WELFARE gives the rate of participation in welfare among women in the state; and *UNEMP* is included in order to control for business cycle conditions. The terms *a*, *b*, *c*, *d* and *e* are coefficients to be estimated from the data. The term X_{sti} is the vector of observable exogenous maternal characteristics that influence participation, such as maternal age and education.

The unobservable determinants of prenatal care utilization are decomposed here into three mutually independent components for the purposes of the regression analysis. The first component, u_s, is a state effect, common to all pregnant women in state *s*. This term can be thought of as representing characteristics of a state which change only slowly over time such as the demographic composition of the state. The term v_t denotes a period effect, common to pregnant women at period *t*. This term can be thought of as representing things like the technology available to women at date *t*. The term w_{st} is a state specific time trend, which will control for characteristics of states that are trending over time and that might be correlated with both changes in policy and birth outcomes. Finally, e_{sti} is a person-specific error term, capturing all purely idiosyncratic factors that influence participation. Since our dependent variables are zero-one indicators, we present logit estimates below.

If the policy variables increase the utilization of prenatal care, and if the marginal prenatal care obtained has an effect on infant health, then these variables should have an impact on our outcome measures. We examine these effects by estimating reduced form models of the effects of the policy variables on low birthweight, very low birthweight, and the probability of fetal death. These logit models are of the same form as equation (1).

The large size of the VSDN data set offers many advantages that are discussed further below. However, it also raises a specification issue, first analyzed by Moulton (1986). Since our dependent variables vary at the level of the individual, and the policy variables vary only between state-period cells,

logit estimates will tend to overstate the precision with which the coefficients of the policy variables are estimated. To account for this problem, we adapt Moulton's approach to our setting by computing standard errors that allow for arbitrary dependence among the individual-level error terms within each state-year cell.

A limitation of these models is that we cannot be certain that any measured effect of the right hand side variable is coming through enrollment in Medicaid, rather than through some other channel. The Vital Statistics data we use does not have information about Medicaid coverage, so it is not possible to address this issue directly. Hence, we also estimate a set of auxiliary regressions examining the effect of our policy variables on aggregate Medicaid caseloads. These regressions take the form:

(2) $MEDCASE_{st} = aADMIN_{st} + bCUTOFF_{st} + cWELFARE_{st} + dUNEMP_{st} + u_s + v_t + w_{st} + \varepsilon_{st}$,

where MEDCASE is the non-elderly, non-disabled, non-medically needy adult Medicaid caseload in the state and year. These data are reported to the Health Care Financing Administration (HCFA) by each state and come from HCFA 2083 forms. This caseload can be further divided into recipients who were also receiving cash payments, and those who were not.[6] There should be a direct relationship between receipt of cash benefits and Medicaid recipiency since people on welfare or social security are automatically entitled to Medicaid benefits. On the other hand, the non-cash caseload should be

[6] These data can be downloaded directly from HCFA's web page. We use numbers from 4 HCFA tables for each year: Categorically needy adults in families with dependent children who received cash payments; categorically needy adults in families with dependent children who did not receive cash payments; caretaker/pregnant women of other coverage groups from pre-1988 legislation; caretaker/pregnant women of other coverage groups from 1988 or later legislation. These 4 groups plus medically needy adults in families with dependent children add up to the total number of adults in families with dependent children. A few states with 1115 waivers did not break down their Medicaid caseload in a way that allowed us to identify adults consistently throughout the sample period. These states were: Tennesee, Oregon, Hawaii, and Arizona. These 4 states are excluded from these caseload regressions. We obtained very similar results if we used log(caseload/population) rather than log(caseload) as the dependent variable.

affected primarily by the Medicaid income eligibility cutoffs rather than by the state's welfare caseload. Both parts of the caseload may be affected by the administrative reforms.

4. The Data

The first source of data for this study is the Vital Statistics Detailed Natality Files for 1990 to 1996. The natality data is collected from birth certificates, and is a census of virtually all of the approximately 4 million births that occur in the United States each year. The large sample size facilitates the analysis of relatively rare outcomes, such as very low birthweight, and enables us to conduct separate examinations by race and socioeconomic status. Data about fetal deaths come from the Vital Statistics Fetal Deaths Detail Record, which follow much the same format as the birth records. By combining data from these two sources, we have the universe of pregnancies which lasted at least 20 weeks and terminated in either a birth or a fetal death.

These data are merged to the policy variables that were in effect 6 months prior to the birth. The rationale for this is that if the policy measure is to affect prenatal care, then it must be in place during the pregnancy. This decision results in the loss of 6 months of data from 1990, since we have data on the administrative reforms only from 1990 on.

Table 3 shows the fetal deaths, prenatal care and outcomes data, by race. In addition, in order to examine the effects of socioeconomic status without reference to race, figures for whites are broken out separately for unmarried teen mothers and highschool dropouts as well as for married college graduates (those with 16 years of education or more).[7] These figures are calculated using the pooled

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We attempted a similar decomposition for blacks, but found that the results for all blacks tended to resemble those for the black unmarried teen moms and high school dropouts. On the other hand, results for disadvantaged whites tend to resemble those for blacks, making this an interesting

VSDN data for 1990 to 1996. We focus only on singleton births, since multiple births are much more likely to be of low birthweight for reasons that are unlikely to be related to use of prenatal care. We divide the children using the race of the mother, in order to avoid issues regarding classification of children born to mothers and fathers of differing race (approximately 4%), or classifying children whose father's race is not reported (approximately 16%). In this we are following the National Center for Health Statistic's practice as of 1989. Hispanics, Asians and a small residual "other" category consisting largely of Native Americans, Pacific Islanders, and persons who did not report maternal race are not included in this analysis.

For "all whites", we use a 25% random sample, while for blacks, we use all of the available observations. This algorithm yields sample sizes for blacks of approximately the same size as those available for whites. Thus, if we find effects for whites but not for blacks, then this pattern of results cannot be attributed solely to differences in sample size. Similarly, the sample of white unmarried teen mothers and dropouts includes all of the teen mothers and dropouts in the data, that is, it is not a subset of the "all whites" data set.

Table 3 shows striking racial differences, some of which have been documented in previous studies. Blacks are three times more likely than whites to have children of very low birthweight. The racial difference is not as great for fetal deaths, but it is striking that the rates of fetal death are very similar for blacks and for disadvantaged whites. Whites are more likely than blacks to be reported to have received "adequate" prenatal care, judged in terms of both the timing of the initiation of prenatal

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comparison. Also, there are few married college-educated blacks in some states. We should note that over our sample period, marital status was imputed for California, Connecticut, Michigan, Nevada, and New York, and it was also imputed in Texas until 1994. Given the large number of people in these states, it was not practical to exclude them from our sample.

care and the number of visits; they are also much more likely to have begun care in the first trimester.

While the majority of women receive adequate prenatal care, and initiate care in the first trimester, there are again large differences by race. The table also shows that those who receive inadequate prenatal care are much more likely than other women to bear infants of low or very low birthweight, although as discussed above, this may reflect selection into prenatal care rather than a causal connection. However, even among this subset, disadvantaged women tend to have worse birth outcomes than other women.

5. Results

5.1 Effects of Policy on Medicaid Caseloads

The relationship between the policy variables and (non-elderly, non-disabled, non-Medically Needy) adult Medicaid caseloads are shown in Table 4. The first column indicates that the income cutoff for pregnant women had no significant effect on the overall caseload while the welfare rate is significant at the 95% level of confidence. The point estimate suggests that the Medicaid caseload would go up by about a quarter for each one percentage point increase in the welfare rate. None of the other policy variables are statistically significant.

Dividing the caseload into those receiving cash payments and those who are not produces very reasonable estimates of the effects of the eligibility cutoffs for pregnant women and welfare rates. In the population that receives cash payments, the income cutoff is irrelevant and Medicaid caseloads are estimated to rise by about a third with each percentage point increase in the welfare rate. In the population that does not receive cash benefits, the welfare rate is irrelevant, and the caseload rises with the income cutoffs. Specifically, an increase of 100% in the Medicaid income cutoffs is estimated to

increase the Medicaid caseload by about two thirds. In these regressions, the other policy variables do not have significant effects. These results suggest that welfare rates and Medicaid income cutoffs did affect Medicaid caseloads, so that the effects we observe in the Vital Statistics data (below) could plausibly be driven by changes in Medicaid caseloads.

5.2 Effects of Policy on Prenatal Care

Estimates of the effects of the Medicaid policy variables and welfare rates on our measures of utilization of prenatal care are shown in Table 5. These models are based on equation (1) and include all of the control variables listed in the table notes. We did not include information about the father's age and education in these regressions because it is often missing, and because when it is present it is highly correlated with the mother's information. The control variables generally have the effects one would anticipate on the basis of the previous literature. For example, older mothers, more educated mothers, and married mothers are more likely to get prenatal care than teens, less educated women, or unmarried women. Utilization of prenatal care also decreases with parity.

Table 5 suggests that increases in the income cutoff increase the adequacy of prenatal care for whites though not for blacks. They have no statistically significant effect on initiation of care in the first trimester, so the increase in adequacy must reflect a greater number of visits after the first trimester for whites. Income cutoffs have been normalized so that a cutoff of 100% of poverty is equal to 1, 200% of poverty is equal to 2 and so on. Since logit coefficients are difficult to interpret, Table 5 also gives the derivatives implied by the coefficient estimates. An asterisk denotes derivatives that are statistically significant at the 95% level of confidence. These figures imply that a change in the income cutoff from 100 to 200% of poverty would have increased the probability of adequate prenatal care by .4

percentage point among whites.

Increases in the welfare rate also had significant effects on the utilization of prenatal care. A 2 percentage point change in the welfare rate (such as occurred in California between 1990 and 1996) was associated with a one percentage point increase in the probability that prenatal care was initiated in the first trimester among whites, and with a .6% increase in the probability that prenatal care was adequate. But the same change was associated with an almost 2 percentage point increase in both probabilities among blacks.

The significant effects of increases in the welfare rate can be compared to the generally small and insignificant effects of the unemployment rate. The fact that welfare rates are significant determinants of prenatal care utilization, while a more general indicator of economic conditions is not, suggests that welfare rates may in fact impact prenatal care through the administrative link to the Medicaid program that was illustrated in Table 4. That is, women who lose access to welfare also appear to reduce utilization of prenatal care, presumably because it is more difficult for them to access the Medicaid program.

In contrast to the income cutoffs and welfare rates, the administrative reforms we consider have mixed effects. Mail-in has a significantly positive effect on both measures of prenatal care utilization among blacks, but not among whites. Shorter forms have a positive effect on the probability of getting care in the first trimester among whites but not among blacks. Outstationing has a puzzling negative effect on the probability of first trimester prenatal care among blacks, but given that there is little variation in this outcome over our sample period, this result may reflect an omitted variable that coincided with the federal mandate to outstation in 1991.

5.3 Reduced Form Estimates of Effects of Policy Variables on Outcomes

Table 6 shows estimates of the effects of the policy variables on the probabilities of low birthweight , very low birthweight, and fetal death. These models are of the same form as those in Table 5, and once again, the coefficients on the control variables followed the patterns that one would expect given the existing literature. For example, married mothers, and mothers who are more educated, have healthier babies, while older mothers are more likely to have unhealthy babies. Also, males are more likely to suffer low or very low birthweight than females. The probability of low or very low birthweight is also highest for first born children but does not change appreciably with increasing parity after that.

Table 6 suggests that increasing the income eligibility cutoffs for Medicaid has little effect on birthweights among either whites or blacks. However, the probability of fetal deaths among blacks is significantly reduced. Although the derivative is small, it translates into a large number of fetal deaths averted. Specifically, an increase in the income cutoff from 100 to 200% of poverty (in line with what occured in many states) would be associated with a decrease of approximately 1,720 fetal deaths per year among black women, on a base of about 16,000 fetal deaths per year.

Since we found an effect of the income cutoffs on prenatal care among whites but not among blacks, this result suggests that if income cutoffs are reducing fetal deaths among blacks it is not through their effects on either the early initiation of prenatal care or the number of visits. It is possible that the quality of prenatal care improves with Medicaid coverage in a way that is not captured by our relatively crude measures. Our data cannot speak to this issue.

Alternatively, additional information given on the death certificates suggests that a significant fraction of the fetal deaths that occur after 20 weeks occur during attempted deliveries. Hence, the

outcome may have been affected by improvements in the delivery technology available to the mother, which in turn is affected by the mother's insurance status. These improvements would be expected to affect the probability of survival, but not birthweight. As discussed above, the fraction of births covered by Medicaid increased from 15 to 40% over the period we examine. Hence, the improvement in fetal deaths is all the more remarkable given that only a quarter of women were directly affected (though of course those affected may have been disproportionately black).

Welfare rates also appear to have little effect on birthweights, but are estimated to reduce fetal deaths among both blacks and whites. Table 5 showed that welfare rates also increased prenatal care utilization for both groups. Hence, this set of findings is consistent with the hypothesis that higher welfare rates are linked to improved utilization of prenatal care which in turn reduces the probability of fetal death. The magnitudes of the effects are modest, however. For example, the coefficient for blacks implies that a 2 point increase in the welfare rate (which would be represent a large percentage increase) would lead to a 10% reduction in the incidence of fetal deaths.

Turning to the administrative reforms, we find a significant effect of mail-in on birthweight. Allowing women to mail in forms is estimated to reduce both the incidence of low birthweight and the incidence of very low birthweight among whites, as well as reducing the probability of fetal deaths among blacks. However, in Table 5, we found that mail-in affected prenatal care only among blacks. The derivatives suggest that allowing white women to mail-in applications reduces the incidence of low birthweight by one percentage point, or about 20%. The effect on the probability of black fetal death is 1/10 of a percentage point, or a reduction of about 5%. The other administrative reforms have either insignificant or inconsistent and/or wrong-signed effects.

5.4 Estimates by Socioeconomic Status

We have estimated all of our models on two subsamples of whites, married college-educated women and unmarried teen mothers or highschool dropouts. This comparison is similar in spirit to "difference-in-difference" estimation since changes in Medicaid policy should have little effect on married, college-educated women who are unlikely to use Medicaid. Conversely, welfare rates should have the largest effects on unmarried teens or dropouts, since these women are most likely to use welfare. It is less clear what the expected magnitude of the effects of income cutoffs and administrative reforms are for this group of disadvantaged women. As discussed above, the income cutoffs we consider generally applied to women with incomes above the poverty line but below 185% of poverty, and the most disadvantaged women may be least able to take advantage of administrative reforms.

Table 7 shows the estimated effects of our policy variables on our two measures of the probability of obtaining prenatal care in these subsamples. We find that the derivatives associated with increases in the income cutoffs are in fact much larger for the disadvantaged. The derivatives indicate that an increase from 100 to 200 percent of poverty would have increased the probability that prenatal care was obtained in the first trimester by 3 percentage points. Turning to prenatal care adequacy, the derivatives are again much larger for the disadvantaged than for college educated women (1.7 percentage points compared to .2 percentage points), although the income cutoffs are also estimated to have a small positive effect for the latter group.

The second row of Table 7 shows that disadvantaged women are strongly affected by welfare policy, while the married college-educated women are not. The estimated derivatives of the welfare rate on prenatal care adequacy and first trimester prenatal care are twice as large for the disadvantaged group as they were for the full sample of whites (though the coefficent in the model for adequacy is

significant only at the 90% level of confidence).

In contrast to Table 5, being permitted to mail in forms now has a negative effect where it is significant, and outstationing is estimated to have positive effects on prenatal care utilization which are stronger for the disadvantaged than for married college-educated women. Finally, presumptive eligibility has a significant, but wrong-signed effect in models of prenatal care adequacy among disadvantaged women.

Table 8 shows the estimated effects of policy on birth outcomes using these two sub-samples. The Medicaid income eligibility cutoff has a positive effect on low birthweight for disadvantaged women and a positive effect on very low birth weight for the married college-educated women. It seems unlikely that these estimates reflect any causal effect. It is possible that states in which women were more likely to have low birthweight infants for other reasons were also more likely to raise income eligibility cutoffs for Medicaid.

Welfare rates have no consistently estimated positive effect on birthweights, but as in Table 6, they appear to reduce the incidence of fetal death. This effect is significant for disadvantaged women but not for married college-educated women. There is little consistent evidence that administrative reforms reduced the incidence of poor birth outcomes in these two groups.

5.5 Extensions and Caveats

While our measures of prenatal care capture important aspects of prenatal care and are frequently used in the literature, it should be kept in mind that they are primarily quantitative measures which say little about the quality of care received. A second important caveat to our results, is that due to the limitations of our sample, we have been unable to conduct a direct assessment of effect of

eliminating asset tests. Also, given the lack of variation in the outstationing measure over our sample period, we believe that the results regarding this variable should be regarded with caution.

It is possible that our policy measures are correlated with some omitted variable that also affects birthweights. Suppose for example, that higher Medicaid income cutoffs induced some women who would have aborted to carry their infants to term, and that these infants are both less likely than others to receive adequate prenatal care and more likely than others to be of low birthweight (c.f. Grossman and Jacobowitz, 1981, Grossman and Joyce, 1990).[8] Then omitting abortion rates from our models will cause us to under-estimate the positive effects of the higher income cutoffs. Unfortunately, it is difficult to test this hypothesis since abortion rates are available for only two years of our sample period.

Anti-smoking policies could also affect birthweight since smoking is the single most important preventable cause of low birthweight. And it is possible that anti-smoking policies such as cigarette tax increases coincided with changes in Medicaid policy. For example, legislators seeking to pay for the higher income cutoffs for Medicaid might raise cigarette taxes. In this case, reductions in the incidence of low birthweight or fetal deaths which were really due to reductions in smoking would be falsely attributed to increases in the generosity of the Medicaid program.

In order to test these hypotheses, we re-estimated all of our models including the cigarette tax for each state and year. William Evans was kind enough to provide us with time-series data on state cigarette taxes (in cents per pack). However, while cigarette taxes did have significant effects on birthweight, their inclusion in our models did not change the findings reported above.

[8] On the other hand, Currie, Nixon and Cole (1996) find no evidence that restrictions on the Medicaid funding of abortion affected the distribution of birthweights.

6. Conclusions

Our results show that changes in income eligibility cutoffs increased Medicaid caseloads and had some effect on the adequacy of prenatal care among white women. The derivatives were greater for disadvantaged women than for married college-educated women. We find no evidence that these income cutoffs were related to improvements in birthweight, though they were associated with reductions in the incidence of fetal death among black women. This result suggests that the reductions in fetal deaths may reflect increased access to life saving technologies at the time of the birth rather than additional utilization of prenatal care or improvements in the underlying health of the fetus.

These results are broadly consistent with those of Currie and Gruber (1996) who found that increases in income cutoffs had larger effects on infant mortality than on birthweight. They are also consistent with previous findings that increases in Medicaid eligibility have increased the amount of medical care that eligible women receive when giving birth (c.f. Currie and Gruber, 2001, Ellwood and Kenney, 1995). It is also possible that the Medicaid expansions improved hospital maternity care more generally by providing more money for this service, and that this improvement resulted in fewer fetal deaths.

Our most striking results concern the effects of welfare rates on Medicaid caseloads, utilization of prenatal care and fetal deaths. We show that Medicaid caseloads rise with welfare caseloads, as one would expect given the structure of the programs. These increases are associated with increased use of prenatal care among both whites and blacks, with the effects for whites being concentrated among the most disadvantaged mothers. We find that both early initiation of prenatal care and overall adequacy of care go up with welfare rates. These increases in welfare rates are associated with reductions in the

incidence of fetal death among both blacks and disadvantaged whites. We cannot definitively rule out the hypothesis that changes in welfare rates reflect income effects rather than access to prenatal care. However, we find it suggestive that measures of unemployment have little effect on either utilization of prenatal care or birth outcomes.

Our estimates of the effects of various administrative reforms designed to reduce non-price barriers to obtaining prenatal care are disappointing. While we generally find positive effects of allowing applicants to mail in their applications variables measuring other administrative reforms are seldom significant, and are as likely to be wrong-signed as right-signed when they are.

The fact that welfare rates have an impact on the utilization of prenatal care indicate that the administrative link between welfare and Medicaid remains important, even in the face of dramatic expansions in income eligibility for the program, and the adoption of many administrative reforms intended to simplify enrollment. Thus, our results suggest that the administrative reforms undertaken to date were not sufficient to break the link between welfare and access to prenatal care under the Medicaid program. Although our sample period predates the most recent round of welfare reform, our results suggest that among pregnant women, cuts in the welfare rolls are likely to be accompanied by reduced use of prenatal care and that these cuts may have adverse effects on infant health.

Table 1: Number of States Adopting Various Strategies to Encourage the Medicaid Enrollment of Pregnant Women

	Income Cutoff >133% Poverty	Shortened Application	Presumptive Eligibility	Mail-in Application	Expedited Eligibility	Outstationing
Jan. 1990	20	19	23	1*	9	17
July 1990	23	25	25	2*	11	19
Jan. 1991	23	27	26	5*	13	24
July 1991	29	31	26	14	14	51
Jan. 1992	29	33	28	23	14	51
July 1992	31	28	28	17	13	51
Jan. 1993	32	26	30	16	13	51
July 1993	33	41	30	18	24	51
Jan. 1994	33	42	30	20	25	51
July 1994	33	42	30	20	25	51
Feb. 1995	33	43	30	19	26	51
Aug. 1995	34	43	31	26	30	51
Feb. 1996	35	41	31	31	30	51

Notes: Source is National Governors' Association, Maternal and Child Health Update, various issues. The dates in the table correspond to the dates of the issues. An asterisk indicates that the data comes from discussion in the text of the sources rather than being drawn from the tables in those sources.

Table 2: Welfare Rates and Unemployment Rates in 10 Large States

	1990 Welfare Rate	1990 Unemp. Rate	1993 Welfare Rate	1993 Unemp. Rate	1996 Welfare Rate	1996 Unemp. Rate
California	7.3	5.8	9.0	9.4	9.2	7.2
Florida	3.6	6.0	6.2	7.0	4.6	5.1
Illinois	6.4	6.2	6.8	7.5	6.2	5.3
Massachusetts	5.2	6.0	6.2	6.9	4.4	4.3
Michigan	8.1	7.6	8.3	7.1	6.0	4.9
New Jersey	4.7	5.1	5.1	7.5	4.1	6.2
New York	6.4	5.3	7.7	7.8	7.4	6.2
Ohio	6.8	5.7	7.4	6.5	5.6	4.9
Pennsylvania	5.3	5.4	6.0	7.1	5.2	5.3
Texas	4.1	6.3	4.9	7.2	3.9	5.6

Table 3: Incidence of Selected Outcomes by Race and Socioeconomic Status 1990-1996

	Whites	Blacks	White Low SES	White High SES
Fetal Death	.017	.028	.027	.004
Very Low Birthweight	.008	.026	.014	.005
Low Birthweight	.047	.119	.083	.032
Adequate Prenatal Care	.923	.868	.900	.990
First Trimester Care	.861	.688	.660	.952
Very Low Birthweight if Inadequate Care	.016	.037	.021	.008
Low Birthweight if Inadequate Care	.094	.184	.123	.046
% Missing Prenatal Care Adequacy	.033	.066	.044	.024
# Observations	3985968	4014935	1288916	3833406

Notes: Number of observations is not the same for all outcomes. The number shown here refers to the number of non-missing observations for birthweight for live births. Note that the "all white" column is a 25% sample of the available observations for whites while the numbers in the other three columns are based on all of the available observations.

Table 4: Effects of Policy Variables on Aggregate Adult Medicaid Caseloads

	Log(Medicaid)	Log(Cash)	Log(NonCash)
Income Cutoff	.287	.223	.664
(1=100% poverty)	(.185)	(.227)	(.277)
Welfare Rate	.233	.325	.194
	(.109)	(.134)	(.163)
Unemployment Rate	.021	-.028	.077
	(.038)	(.047)	(.057)
Mail-in	.072	.136	-.004
	(.070)	(.085)	(.104)
Shortened Forms	-.084	-.142	.038
	(.073)	(.089)	(.108)
Expedited Eligibility	-.060	-.062	-.005
	(.090)	(.110)	(.135)
Presumptive Eligibility	.124	.080	.223
	(.122)	(.149)	(.182)
Outstationing	.043	.104	.020
	(.096)	(.117)	(.143)
# Observations	329	329	329
R-squared	.605	.570	.709

Notes: Caseloads are for adults in families with dependent children who are not medically needed. The aggregate caseload can be divided into those who are also receiving cash payments and those who are not. Data for AZ, TN, HA, and OR are excluded because the number of adults was not reported. Regressions also included state effects, year effects, and state-year trends. Standard errors in parentheses.

Table 5: Effects on Prenatal Care - Logits

	1st Tri	1st Tri	Adequate	Adequate	
		White	Black	White	Black
Income Cutoff		-.008	.025	.109	.056
(1=100% Poverty)		(.028)	(.046)	(.055)	(.054)
Welfare Rate		.042	.045	.096	.076
		(.016)	(.017)	(.026)	(.022)
Unemployment Rate		.007	.002	-.003	.004
		(.006)	(.007)	(.009)	(.008)
Shortened Forms	.028	.027	-.012	.008	
		(.014)	(.015)	(.021)	(.017)
Mail-in		.017	.027	.014	.042
		(.011)	(.013)	(.021)	(.017)
Expedited Eligibility		-.021	-.001	-.033	-.036
		(.016)	(.020)	(.025)	(.025)
Presumptive Eligibility		-.038	-.015	-.034	-.041
		(.021)	(.028)	(.035)	(.034)
Outstationing		-.024	-.052	-.002	.014
		(.015)	(.018)	(.028)	(.023)
Pseudo R-squared	.112	.070	.114	.096	
# Observations (millions)		3.89	3.72	3.86	3.75
Derivatives					
Income Cutoff		-.001	.005	.004*	.006
Welfare Rate		.005*	.009*	.003*	.008*
Unemployment Rate		.001	...	...	...
Shortened Forms		.003*	.005	...	.001
Mail-in		.002	.005*	...	.004*
Expedited Eligibility		-.002	...	-.001	-.004
Presumptive Eligibility		-.004	-.003	-.001	-.004
Outstationing		-.003	-.010*	...	.001

Notes: Regressions also included: mother's age (19-24, 25-34, 35+), mother's education (dropout, high school, some college, college), mother's marital status, number of siblings (1, 2, 3, 4, 5+), missing parity, child's gender, year effects, state effects, and state-specific time trends. Standard errors in parentheses. An asterisk denotes a derivative that is statistically significant at the 95% level of confidence. A ... indicates a derivative that was smaller than .001 in absolute value.

Table 6: Reduced Form Estimates of Effects on Birth Outcomes - Logits

	Very Low Birthweight		Low Birthweight		Fetal Death	
	White	**Black**	**White**	**Black**	**White**	**Black**
Income Cutoff	-.018	.026	-.008	-.012	-.020	-.026
(1=100% Poverty)	(.060)	(.043)	(.029)	(.026)	(.013)	(.012)
Welfare Rate	-.005	-.012	.001	.002	-.018	-.011
	(.026)	(.014)	(.011)	(.008)	(.005)	(.005)
Unemployment Rate	.016	.006	.007	.002	.004	.002
	(.011)	(.007)	(.005)	(.004)	(.003)	(.002)
Shortened Forms	-.006	-.013	.003	.004	-.003	.004
	(.024)	(.018)	(.011)	(.009)	(.006)	(.005)
Mail-in Forms	-.048	-.004	-.027	-.002	-.003	-.006
	(.020)	(.013)	(.009)	(.007)	(.003)	(.003)
Expedited Eligibility	-.028	.023	-.010	.005	.009	.006
	(.028)	(.018)	(.012)	(.010)	(.004)	(.004)
Presumptive Eligibility	-.035	.040	-.014	.008	.001	-.004
	(.043)	(.021)	(.014)	(.013)	(.005)	(.005)
Outstationing	.017	-.047	.029	-.015	-.002	.005
	(.031)	(.020)	(.014)	(.013)	(.007)	(.006)
Psuedo R-squared	.019	.010	.023	.014	.121	.102
#Observations (millions)	3.98	4.01	3.98	4.01	4.02	4.07
Derivatives						
Income Cutoff	-.001	.006	-.004	-.010	-.001	-.003*
Welfare Rate	...	-.003	...	.002	-.001*	-.001*
Unemployment Rate	.001	.002	.003	.001	...	...
Shortened Forms	...	-.003	...	.004	...	.001
Mail-in Forms	-.003*	-.001	-.012*	-.002	...	-.001*
Expedited Eligibility	-.002	.006	-.001	.005	.001*	.001
Presumptive	-.002	.010	-.006	.008	...	-.001
Outstationing	.001	-.012*	.013*	-.016	...	.001

Notes: See Table 5.

Table 7: Effects on Prenatal Care by Socioeconomic Status - Whites Only

	1st Tri Low SES	1st Tri High SES	Adequate Low SES	Adequate High SES
Income Cutoff	.139	-.074	.180	.180
(1=100% Poverty)	(.037)	(.049)	(.055)	(.091)
Welfare Rate	.055	-.031	.058	-.031
	(.023)	(.031)	(.030)	(.042)
Unemployment Rate	.011	.025	.004	.015
	(.009)	(.012)	(.012)	(.030)
Shortened Forms	.014	-.010	.011	-.063
	(.014)	(.020)	(.019)	(.042)
Mail-in Applications	-.008	.027	-.013	-.002
	(.014)	(.016)	(.017)	(.034)
Expedited Eligibility	.017	.025	-.011	-.044
	(.021)	(.027)	(.023)	(.051)
Presumptive Eligibility	-.014	.006	-.077	.160
	(.025)	(.028)	(.030)	(.059)
Outstationing	.048	.048	.079	.146
	(.019)	(.023)	(.021)	(.058)
Pseudo R-squared	.017	.032	.033	.029
#Observations (millions)	1.2	3.8	1.2	3.7
Derivatives				
Income Cutoff	.030*	.003	.017*	.002*
Welfare Rate	.012*	.001	.006	...
Unemployment Rate	.002	.001*	...	...
Shortened Forms	.003	...	.001	.001
Mail-in Applications	-.002	.001	-.001	...
Expedited Eligibility	.004	.001	-.001	...
Presumptive Eligibility	-.003	...	-.007*	.002*
Outstationing	.011*	.002*	.008*	.001*

Notes: See Table 5.

Table 8: Reduced Form Estimates of Effects on Birth Outcomes by Socioeconomic Status

	Very Low Birthweight		Low Birthweight		Fetal Death	
	Low SES	**High SES**	**Low SES**	**High SES**	**Low SES**	**High SES**
Income Cutoff	.001	.149	.095	.013	.131	-.101
(1=100% Poverty)	(.081)	(.076)	(.041)	(.028)	(.113)	(.100)
Welfare Rate	-.028	-.064	.006	-.026	-.177	.025
	(.044)	(.045)	(.020)	(.017)	(.062)	(.078)
Unemployment Rate	.024	.000	.012	.001	.035	-.044
	(.017)	(.018)	(.008)	(.006)	(.023)	(.027)
Shortened Forms	-.003	-.058	.008	-.035	-.034	.033
	(.030)	(.027)	(.013)	(.012)	(.054)	(.074)
Mail-in Applications	-.001	.011	-.034	.027	.029	-.034
	(.026)	(.028)	(.013)	(.011)	(.039)	(.035)
Expedited Eligibility	-.009	.008	.012	-.003	.028	-.051
	(.038)	(.041)	(.017)	(.014)	(.051)	(.057)
Presumptive Eligibility	.085	-.037	.041	.033	-.006	-.108
	(.060)	(.056)	(.021)	(.016)	(.049)	(.081)
Outstationing	.042	-.069	.033	-.036	.014	-.119
	(.040)	(.034)	(.018)	(.013)	(.074)	(.053)
Psuedo R-squared	.004	.014	.018	.018	.107	.060
# Observations (millions)	1.3	3.8	1.3	3.8	1.3	3.8
Derivatives						
Income Cutoff	...	.001*	.007*	...	.002	...
Welfare Rate	...	...	...	.001	-.003*	...
Unemployment Rate	...	...	.001	...	.001	...
Shortened Forms	...	...	.001	-.001*	-.001	...
Mail-in Applications	...	...	-.003*	.001*	...	...
Expedited Eligibility	...	...	.001	...	...	...
Presumptive Eligibility	.001	...	.003	.001*	...	...
Outstationing	.001	...	.002	-.002*	...	...

Notes: See Table 5.

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