Saving Babies: The Efficacy and Cost of Recent Changes in the Medicaid Eligibility of Pregnant Women

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A key question for health care reform in the United States is whether expanded health insurance eligibility will lead to improvements in health outcomes. We address this question in the context of the dramatic changes in Medicaid eligibility for pregnant women that took place between 1979 and 1992. We build a detailed simulation model of each state's Medicaid policy during this era and use this model to estimate (1) the effect of changes in the rules on the fraction of women eligible for Medicaid coverage in the event of pregnancy and (2) the effect of Medicaid eligibility changes on birth outcomes in aggregate *Vital Statistics* data. We have three main findings. First, the changes did dramatically increase the Medicaid eligibility of pregnant women, but did so at quite differential rates across the states. Second, the changes lowered the incidence of infant mortality and low birth weight; we estimate that the 30-percentage-point increase in eligibility among 15–44-year-old women was associated with a decrease in infant mortality of 8.5 percent. Third, earlier,

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targeted changes in Medicaid eligibility, which were restricted to specific low-income groups, had much larger effects on birth outcomes than broader expansions of eligibility to women with higher income levels. We suggest that the source of this difference is the much lower take-up of Medicaid coverage by individuals who became eligible under the broader eligibility changes. Even the targeted changes cost the Medicaid program $840,000 per infant life saved, however, raising important issues of cost effectiveness.

Will the extension of health insurance to the uninsured improve their health? This is a key question underlying the recent debate over health care reform. Although insurance coverage may be a necessary precondition for improvements in the utilization of medical care, expansions in eligibility for insurance may not translate into increased utilization of care, or even into increases in insurance coverage. It is also possible that increased utilization of care will not result in improvements in health or that any improvements that do result come only at very high costs.

This paper sheds light on these issues by investigating the relationship between the health of newborns and recent changes in the eligibility of pregnant women for public insurance under the Medicaid program. At nine infant deaths per 1,000 births, the U.S. infant mortality rate is among the highest in the industrialized world (U.S. House of Representatives 1992, pp. 1116–17). This high rate is thought to reflect large numbers of unhealthy newborns. Hence, to the extent that adequate prenatal care improves neonatal health, there is scope for a decrease in this rate through the promotion of prenatal care (Institute of Medicine 1985).

In an effort to increase the use of prenatal care, the past decade has seen a rapid expansion in the eligibility of pregnant women for Medicaid, a federal-state matching entitlement program that provides health insurance for the poor. Until the early 1980s, eligibility for Medicaid was tied to the receipt of cash welfare payments under the Aid to Families with Dependent Children (AFDC) program; eligibility rose and fell with changes in the generosity of that program. This linkage had the effect of limiting eligibility to very low income women in single-parent households. Recent extensions of eligibility to other groups provide a case study of whether changes in health insurance eligibility can actually improve infant health.

We identify the effect of eligibility changes by exploiting the fact that they occurred at very different rates across the states. The backbone of our analysis is a detailed simulation model of each state's Medicaid eligibility rules for pregnant women over the 1979–92 period. We apply this model to data from the Current Population Surveys (CPS) in order to quantify the effects of changes in the rules on eligibility and on actual Medicaid coverage. We then use aggregate *Vital Statistics* data to examine the effect of Medicaid policy changes on two widely used indicators of infant health: the incidence of low birth weight and infant mortality. Using these estimates in conjunction with data on Medicaid expenditures from the Health Care Financing Administration (HCFA), we then examine the costliness of the Medicaid eligibility changes. Finally, we use information on the use of medical services by pregnant women from the National Longitudinal Survey of Youth (NLSY) to ask how the policy changes affected the use of birth inputs.

We have three major findings. First, we estimate that the Medicaid eligibility changes of the 1979–92 period increased the fraction of 15–44-year-old women eligible for public insurance in the event of pregnancy from 12.4 to 43.3 percent, an increase of 250 percent. Second, increases in Medicaid eligibility were associated with a reduced incidence of low–birth weight births and with a decrease in infant mortality. Third, all Medicaid eligibility changes are not created equal. In particular, we divide the changes into two categories. "Targeted changes" applied to specific low-income groups. They included changes in eligibility for cash welfare under the AFDC program and changes that allowed pregnant women in families with incomes below AFDC eligibility thresholds to receive Medicaid coverage regardless of family structure. "Broad changes" extended Medicaid coverage to all women with incomes less than specified levels (e.g., 185 percent of the federal poverty level). Most of these women had incomes much higher than the AFDC income cutoffs, which suggests that the two types of policies may have had different effects.

In fact, we find that targeted eligibility changes had sizable and significant effects on birth outcomes, but broad eligibility changes had little effect. We suggest that the source of this difference is in the differential effects that these policies had on Medicaid coverage: the broader changes resulted in much lower take-up rates. Both types of changes were associated with large increases in Medicaid program costs, however; we estimate that the program spent $840,000 per infant life saved under the targeted changes and $4.2 million under the broad changes. These high costs were incurred despite the fact that the targeted changes increased the use of prenatal care, which was their stated goal. This finding raises the critical question of whether extending eligibility for public health insurance is a cost-effective means of improving health.

The rest of the paper is laid out as follows: In Section I, we provide background information about our measures of newborn health. In Section II, we discuss the Medicaid policy changes and their effects on eligibility. Section III investigates the effect of these changes on birth outcomes. Section IV examines the role of Medicaid take-up in explaining the differential effects of different types of Medicaid policy. Section V investigates the cost effectiveness of the policy changes using information on Medicaid expenditures and individual use of prenatal care. Section VI concludes the paper with a discussion of the policy implications of our findings.

I. Background on Birth Outcomes in the United States

The infant mortality rate and the incidence of low birth weight are two of the most frequently examined indicators of infant health. Figure 1 plots the trends in these measures over the 1980s and early 1990s. The incidence of low birth weight, defined as the number of live births per 1,000 weighing less than 2,500 grams (approximately 5.5 pounds), declined from 68.7 in 1979 to 66.6 in 1984, but then rose to 71.1 by 1992. In contrast, infant mortality declined steadily throughout the decade. These differing trends underscore the fact that although they are related, low birth weight and infant mortality measure different aspects of birth outcomes.

Low birth weight is a key indicator of the underlying health of the fetus. Children with a low birth weight are at high risk of neonatal mortality and experience postneonatal mortality rates 10–15 times those found among infants with a normal birth weight (U.S. Office of Technology Assessment 1987*a*). Horbar et al. (1993) found that in a sample of very low birth weight children weighing between 601 and 1,300 grams at birth, each increase in birth weight of 100 grams was associated with a decrease of approximately 10 percent in the probability of death, other things being equal.

In contrast, infant mortality rates reflect not only the health of the fetus as measured by birth weight but also the effect of any interventions that occur during or shortly after birth. New technologies have had dramatic effects on the survival rate of low–birth weight infants. Buehler et al. (1985) report that improvements in birth weight–specific mortality rates accounted for 91 percent of the overall decline in neonatal mortality between 1960 and 1980.[1]

These interventions, however, are often very expensive. Schwartz (1989) reports that although babies weighing less than 2,500 grams

[1] More recently, Horbar et al. (1993) report that as much as half of the decline in national infant mortality reported between 1989 and 1990 may be attributable to the introduction of a new therapy for artificially replacing an essential substance in the lung (pulmonary surfactant) that is not manufactured by the fetus in significant quantities until the thirty-third week. This therapy was introduced in October 1989.

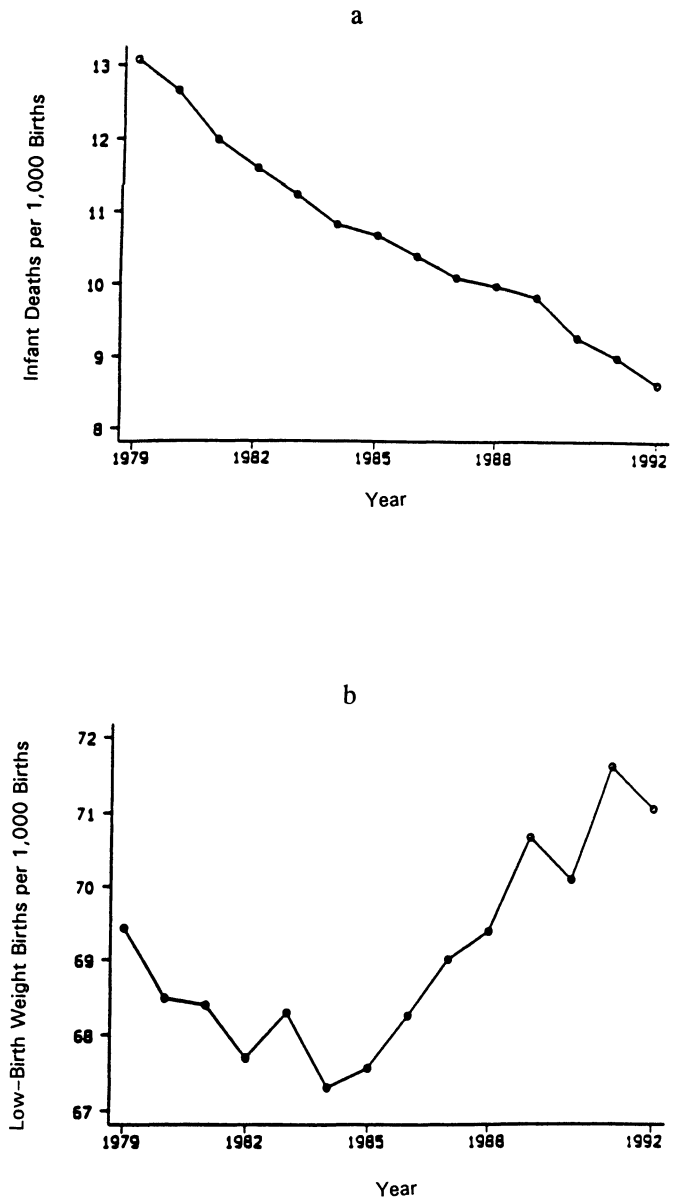

FIG. 1.—*a,* Infant mortality over time. *b,* Low birth weight over time

account for only 9 percent of neonatal hospital caseloads, they account for 57 percent of the cost of neonatal hospital care. The average cost of caring for a surviving low–birth weight baby was $9,712 compared to $678 for an infant weighing more than 2,500 grams. These costs rise as birth weight falls; in 1984, the cost of saving an infant with birth weight below 1,000 grams was $118,000 (U.S. Office of Technology Assessment 1987*b*). Moreover, survivors are at high risk of handicaps such as cerebral palsy of significant degree, major seizure disorders, blindness, deafness, and learning disorders (U.S. Office of Technology Assessment 1987*b*; Chaikind and Corman 1990; McCormick et al. 1992).

The high cost of caring for low–birth weight infants, and their uncertain future should they survive, have led policy makers to emphasize the prevention of low birth weight through the promotion of appropriate prenatal care. There are a number of ways that early prenatal care can improve fetal health. For example, approximately two-thirds of all low–birth weight births are preterm, and Creasy, Gummer, and Liggins (1980) found that over 60 percent of these cases could have been identified using inexpensive ($10–$20) screenings in the first prenatal care visit. Several clinical studies cited in the Institute of Medicine's influential 1985 report suggest that providing appropriate prenatal care to women identified by these screenings (at a cost of between $400 and $500 per woman) could reduce the incidence of low birth weight by more than 20 percent.

As has been noted by a number of economists, however, studies based on differences in outcomes among women who do and do not receive prenatal care are likely to be biased by selection; see Harris (1982) for an extensive discussion. Compared to clinical studies, studies based on survey data that attempt to control for this selection typically find much smaller effects of prenatal care on birth weight (Rosenzweig and Schultz 1982, 1983, 1988; Corman, Joyce, and Grossman 1987; Grossman and Joyce 1990; Frank et al. 1991). These different findings may also reflect the fact that clinical studies focus on the gains that could be attained under ideal circumstances, whereas surveys reflect the impact of prenatal care as it is practiced in the field.

In summary, the available clinical evidence suggests that while both reductions in the incidence of low birth weight and high-tech neonatal care can reduce infant mortality rates, the former is the more cost-effective policy. Decreasing the incidence of low birth weight through increases in the use of prenatal care was the primary motivation for the changes in the Medicaid coverage of pregnant women that took place during the 1980s.

II. Medicaid Policy Changes

A. *Background*

Historically, Medicaid eligibility for women and children has been closely tied to participation in AFDC. This linkage with AFDC restricted access to the program in three ways. First, despite the existence of the AFDC–Unemployed Parents program, which provides benefits to households in which the primary earner is unemployed, AFDC benefits are generally restricted to female-headed households.[2] Second, income cutoffs for cash welfare vary across states and can be very low. For example, in Texas, the cutoff for a family of four in 1979 was only 24 percent of the poverty line. Third, the stigma of applying for cash welfare programs may have prevented eligible families from receiving Medicaid benefits (Moffitt 1992).

However, from the inception of the Medicaid program, states have had the option of extending Medicaid benefits to some groups of pregnant women who were not on AFDC.[3] These options expanded rapidly during the 1980s in a manner that is detailed in the appendix to Currie and Gruber (1994). In brief, eligibility changes during this era can be divided into two types. The first type provided coverage to narrowly defined groups of low-income persons. This category includes changes in AFDC eligibility, which carried with them changes in Medicaid eligibility as well as policies that expanded pregnancy coverage to several specific groups: first-time pregnant women with income below AFDC cutoffs (who did not qualify under the traditional program because they did not yet have a child); teenagers in families with income less than the AFDC cutoff, regardless of their family structure; two-parent families with income below AFDC cutoffs; and the "medically needy"—those with incomes above the AFDC cutoff who had large medical expenses that brought their net incomes below these cutoffs.[4] Because these eligibility changes were narrowly

[2] Not every state had an AFDC-UP program over our sample period, and eligibility requirements are strict. As a result, as of 1990, only 5 percent of the AFDC caseload qualified under this program (U.S. House of Representatives 1992).

[3] These programs also covered some costs of newborns. The Consolidated Omnibus Reconciliation Act of 1986 mandated that children born to mothers with Medicaid coverage be covered themselves for 60 days postpartum. In earlier years, some states made no distinction between expenditures on the mother and expenditures on the baby, which implies that the baby would have received treatment under the mother's coverage. However, even in states with separate accounts for mother and child, it may take several days to establish the child's account; in the meantime the child would be covered under the mother's policy.

[4] In some states, medically needy thresholds are somewhat above AFDC thresholds; they are never more than 33 percent higher. Since higher-income (but sick) families can qualify, medically needy eligibility changes are not as narrowly targeted to very

targeted to the existing low-income population, we label them "targeted changes."

Beginning in April 1987, income cutoffs for pregnant women were also greatly liberalized. States were first given the option and then required to cover pregnant women with income levels that greatly exceeded AFDC income limits in most states. These expansions applied to all women, regardless of family structure. By April 1990, a uniform minimum threshold had been established: all states were required to cover pregnant women with incomes up to 133 percent of the poverty line, and states had the option of covering women with incomes up to 185 percent of the poverty line. In fact, using state-only funds (no federal matching), some states have even expanded coverage beyond these levels. In what follows, we shall denote these relaxations of the income requirements as "broad eligibility changes."

B. *Effects on Eligibility*

Our analysis begins with a detailed simulation of the effects of Medicaid policy on eligibility in 49 states and the District of Columbia, over the 1979–92 period.[5] The construction of our simulation model is described in detail in the appendix to Currie and Gruber (1994). We analyze eligibility using 14 years of CPS data since the CPS is the largest available annual data source with the requisite information about income and demographic characteristics.[6]

Figure 2 shows the fraction of 15–44-year-old women in the CPS who would have been eligible for Medicaid coverage in each year had they become pregnant. We estimate that the percentage eligible rose from 12.4 percent to 43.3 percent between 1979 and 1991. The eligibility increases of the early 1980s show that these estimates are sensitive to business cycle effects. During the recession years, many women became eligible because they fell into poverty, so eligibility increased even as eligibility criteria became stricter in the early years of the Reagan administration. In the estimation below, we therefore control for these business cycle effects with a full set of year dummies.

There is a moderate increase in eligibility associated with increases

low income groups as the other policies in our "targeted" category. But the results presented below are not sensitive to the inclusion or exclusion of medically needy eligibles in the group of targeted eligibles.

[5] We exclude Arizona from the analysis because it does not have a conventional Medicaid program.

[6] One limitation of both the CPS and the NLSY data used below is that income is measured annually, whereas eligibility for Medicaid is determined on the basis of monthly income. This will lead to some measurement error in our eligibility calculations. If this measurement error is random, it will be corrected by the instrumental variables procedure that is used in the empirical work.

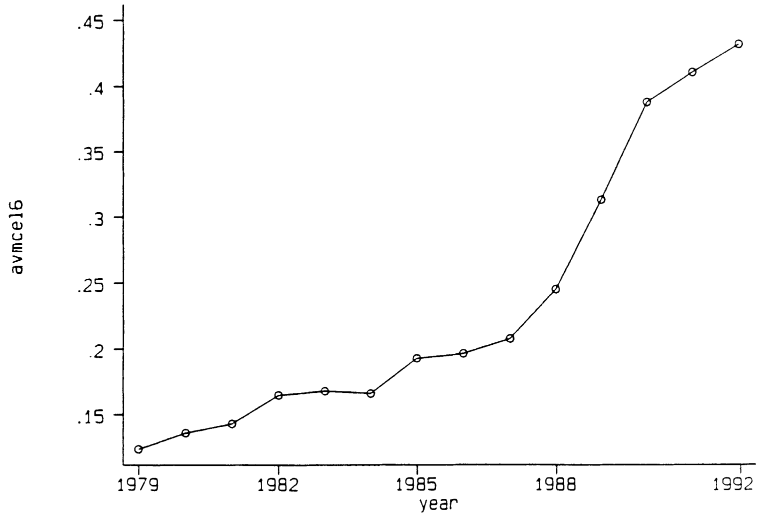

FIG. 2.—Medicaid eligibility trends

in the coverage of unborn children and two-earner families that were mandated in the Deficit Reduction Act of 1984. But the figures clearly show that the most dramatic changes in the number of eligibles were associated with the relaxation of income restrictions in the late 1980s and early 1990s: eligibility increased over 100 percent between 1987 and 1992.

The aggregate time trends shown in figure 2 mask considerable variability in the growth of eligibility across states, as is shown in table 1, which presents the fraction of 15–44-year-old women eligible for Medicaid in each state in 1979, 1986, and 1992. For 1992, we show eligibility under the targeted and broad eligibility criteria separately, as well as overall eligibility. Between 1979 and 1986, the growth in eligibility under the targeted changes was dramatic in states such as Colorado, Mississippi, and North Carolina. On the other hand, eight states experienced reductions in eligibility over this period. The growth in eligibility between 1986 and 1992 was positive for all states, but there was also substantial variation: the growth in eligibility was 10 times greater in Mississippi than it was in Virginia or Washington.

It is this substantial change across states and within states over time that provides the identifying variation for our study. If states are ranked by the fraction of the 15–44-year-old female population that is eligible, the rank in 1992 and rank in 1979 are actually uncorrelated (correlation coefficient −.016). Thirty-three states experienced a change in ranking of at least 10 positions: Washington fell from the tenth most generous state to the least generous, and Mississippi rose

TABLE 1

ELIGIBILITY BY STATE OVER TIME

STATE	1979 OVERALL (1)	1986 OVERALL (2)	1992 Overall (3)	1992 Targeted (4)	1992 Broad (5)
Alabama	.124	.195	.528	.109	.420
Alaska	.037	.205	.306	.213	.093
Arkansas	.108	.151	.453	.119	.334
California	.199	.286	.510	.307	.203
Colorado	.065	.191	.379	.129	.250
Connecticut	.160	.185	.311	.177	.134
Delaware	.102	.110	.373	.087	.286
District of Columbia	.268	.220	.494	.279	.215
Florida	.047	.132	.491	.176	.315
Georgia	.079	.169	.393	.179	.214
Hawaii	.199	.185	.372	.194	.177
Idaho	.083	.162	.455	.158	.297
Illinois	.076	.187	.357	.210	.147
Indiana	.033	.132	.422	.112	.311
Iowa	.101	.174	.493	.190	.303
Kansas	.113	.148	.319	.144	.175
Kentucky	.093	.167	.517	.186	.332
Louisiana	.148	.210	.505	.224	.281
Maine	.128	.199	.518	.152	.366
Maryland	.134	.157	.370	.156	.214
Massachusetts	.179	.165	.381	.221	.160
Michigan	.145	.228	.440	.213	.227
Minnesota	.107	.219	.440	.200	.240
Mississippi	.052	.211	.595	.213	.381
Missouri	.053	.151	.388	.111	.277
Montana	.166	.145	.380	.142	.238
Nebraska	.183	.177	.303	.112	.191
Nevada	.090	.083	.381	.191	.190
New Hampshire	.018	.070	.295	.149	.146
New Jersey	.131	.146	.422	.180	.241
New Mexico	.103	.164	.540	.192	.347
New York	.245	.254	.489	.291	.199
North Carolina	.027	.145	.490	.168	.323
North Dakota	.115	.165	.369	.172	.197
Ohio	.121	.176	.336	.169	.167
Oklahoma	.113	.142	.449	.202	.247
Oregon	.165	.176	.312	.173	.140
Pennsylvania	.134	.173	.336	.197	.140
Rhode Island	.232	.183	.475	.216	.258
South Carolina	.127	.213	.544	.210	.334
South Dakota	.096	.143	.337	.131	.208
Tennessee	.089	.173	.499	.145	.355
Texas	.026	.145	.485	.151	.333
Utah	.182	.267	.302	.129	.173
Vermont	.265	.258	.485	.272	.212
Virginia	.034	.264	.298	.119	.179
Washington	.168	.268	.294	.192	.102
West Virginia	.104	.207	.527	.212	.315
Wisconsin	.129	.229	.337	.163	.174
Wyoming	.058	.129	.335	.206	.128

NOTE.—The figures are the fraction of 15–44-year-old women in each state/year who were eligible for Medicaid overall (cols. 1, 2, and 3), under targeted eligibility rules only (col. 4), and under broad eligibility rules only (col. 5). Tabulated from the March 1980, 1987, and 1993 CPS.

TABLE 2

CHARACTERISTICS OF THE POPULATION COVERED UNDER "TARGETED" AND "BROAD" CHANGES, CPS DATA

Characteristic	Full Sample 1979 (1)	Targeted Changes (2)	Full Sample 1986 (3)	Broad Changes (4)
Income	36,148	5,393	36,037	18,135
	(27,170)	(5,902)	(30,821)	(7,906)
Poor (%)	16.1	80.1	19.7	11.6
Number of kids	1.08	.77	1.01	1.19
	(1.31)	(1.36)	(1.20)	(1.28)
White (%)	86.4	75.2	84.6	82.5
Age	27.9	25.0	29.2	28.7
	(8.30)	(7.06)	(8.28)	(7.44)
Married (%)	54.9	33.3	52.5	48.8
Working (%)	70.5	54.2	73.6	74.5
Received public assistance (%)	5.4	10.1	5.7	2.64
Uninsured (%)	14.4	37.6	17.0	25.6
Employer-provided health insurance (%)	32.2	10.0	35.2	33.7
Private health insurance (%)	76.8	41.5	73.2	66.6

NOTE.—Data are taken from 1980 and 1987 samples of the CPS. Cols. 1 and 3 provide means for the full sample of 15–44-year-old women in each of those years. Col. 2 provides means for women who were not eligible for Medicaid in 1979 but would have been eligible under a targeted program in 1992. Col. 4 provides means for women who were not eligible for Medicaid in 1986, would not have been eligible under a targeted program in 1992, but would have been eligible under broad eligibility guidelines in 1992. Income is personal income for older children living at home and family income for family heads, spouses, or children. Working is defined as working at least one week in the previous year.

from the forty-third most generous state to the state that makes the highest fraction of its population eligible.

C. *Targeted versus Broad Eligibility Changes*

Throughout this paper, we shall distinguish between the effects of the targeted and broad eligibility changes. One reason for doing so is presented in table 2, which highlights the heterogeneity between the populations affected by the two types of changes. Column 1 shows the characteristics of the entire sample for 1979, before any of the eligibility changes that we are studying. We then identify the individuals affected by the targeted eligibility changes in column 2 by applying the 1992 rules for targeted eligibility to this 1979 sample, after inflating all elements of income to 1992 levels. We break out the subset of people who were not eligible in 1979 but would have been made eligible under the targeted changes in the Medicaid program over the entire 1979–92 period. In columns 3 and 4, we pursue a similar exercise for the broad changes using 1986 data. In order to focus on the broad changes, we exclude women who would have become eligible under the targeted changes. For female heads, spouses, and children, we report family income; for women over age 18 who are not family heads or spouses, income is individual income.

Individuals who would have been covered by either the targeted or broad changes were disadvantaged relative to the full sample. Those who would have become covered only under the broad changes, however, are more similar to the full sample than to those who would have become eligible under targeted changes. The former have higher income and are much less likely to be in poverty or to receive public assistance than the latter; in fact, among the group who would have become eligible under the broad changes, the poverty rates and incidence of receipt of public assistance are below the means for the full sample. The broad group is also older, more likely to be white and married, and more likely to be working. While both groups are much more likely to be uninsured than the average female, those who would have become eligible under the targeted changes have a 50 percent higher probability of being uninsured than those who would have become eligible under the broad changes.[7] In summary, table 2 suggests that the two types of changes affected very different populations and may, as a result, have had very different effects on birth outcomes.[8]

III. Eligibility and Birth Outcomes

A. *Methodology*

We examine the effect of the eligibility changes on birth outcomes using aggregate data from *Vital Statistics,* which reports the incidence of low birth weight (less than 2,500 grams) and the infant mortality rate in each state and year. Our empirical strategy is to regress these

[7] This raises the possibility that the broad changes may have crowded out the private insurance coverage of this population; evidence that this is the case is presented in Cutler and Gruber (1996).

[8] State Medicaid policies could differ in other ways besides rules governing eligibility. For example, states may cover different services. However, the most important services are covered in all states, with variation mostly in peripheral services such as eyeglass prescriptions. States may also differ in terms of fees paid to Medicaid providers. Currie, Gruber, and Fischer (1995) show that fees paid to obstetrician/gynecologists do have a significant effect on infant outcomes. But including the ratio of Medicaid to private-sector fees did not change our conclusions regarding the effects of eligibility. Finally, states may differ in other aspects of eligibility besides income cutoffs, such as whether individuals are subject to asset tests or whether they are presumed to be eligible while their application is being processed. We have rerun our basic models including controls for whether states dropped their asset tests for pregnant women or made them presumptively eligible. Neither of these variables had a significant effect on birth outcomes, and their inclusion did not affect the estimated effects of the eligibility variables.

state/year outcomes on an index of Medicaid eligibility generosity: the fraction of 15–44-year-old women in that state and year who would have been eligible for Medicaid coverage in the event of pregnancy. That is, the regression asks the following question: As Medicaid makes a larger fraction of pregnant women eligible in a state and year, do birth outcomes improve?

A potential drawback to this strategy, however, is that the actual fraction eligible depends on economic and demographic characteristics of the state, which may also be correlated with birth outcomes. Figure 2 showed, for example, that the recession of 1982 was associated with increases in Medicaid eligibility despite the adoption of stricter eligibility criteria. Similarly, the fact that in 1992 Mississippi had the highest fraction eligible of any state reflects both the generosity of the state program and the relative poverty of Mississippians.

To the extent that relevant state- and year-specific characteristics are not captured by state and year dummies (i.e., they are not constant within a state or across states within a year), the coefficient on the fraction eligible will be biased by omitted variables. Suppose, for example, that a state recession is associated with both increases in eligibility and a higher incidence of low birth weight. Then this source of variation in eligibility could induce a spurious positive correlation between Medicaid eligibility and low birth weight.

In order to overcome this potential problem, we instrument the actual fraction eligible with a measure of the generosity of Medicaid in a state and year that depends only on the state's eligibility rules. To create our instrument, which we label the "simulated fraction eligible," we first take a sample of 3,000 women from the CPS in each year. We then calculate the fraction of this sample of women who would be eligible for Medicaid in each state. By using the same group of women in each state simulation, we obtain an estimate of the fraction eligible that depends only on the legislative environment and is independent of other characteristics of states. This measure can be thought of as a convenient parameterization of legislative differences affecting women in different states and years: the generosity of state Medicaid policy can be naturally summarized in terms of the effect it would have on a given, nationally representative, population. Furthermore, we reduce the sampling variability in our estimates that derives from having relatively small cells for some states in the CPS.[9]

[9] Note that the use of a national sample does not affect the consistency of our estimates, since any set of weights will still yield a measure that is a function only of state rules. An alternative approach would be to estimate the fraction eligible in each year using a state-specific sample drawn from a particular base year. However, this procedure relies on stronger assumptions than ours: it would be necessary to assume not only that state rules are legitimate instruments but also that state-specific conditions

A final estimation issue is that these models treat state Medicaid policy as though it were exogenous to birth outcomes; there is in fact some evidence that states with high proportions of low–birth weight births and high fractions of women who delayed obtaining prenatal care were more likely to adopt optional Medicaid expansions (Gold, Singh, and Frost 1993). The models we estimate include state fixed effects in order to control for potentially spurious correlations between time-invariant state characteristics and Medicaid policy. We also control for some time-varying state characteristics in subsection *D* below.

B. Overall Results

Table 3 presents the basic estimates from models that include a full set of state and year dummies as well as our eligibility measure. When we use the overall actual fraction eligible in column 1 of panel A, we find a negative but insignificant effect on the incidence of low birth weight. In column 2, we instrument the actual fraction eligible using the simulated fraction eligible. The coefficient on eligibility rises and becomes significant at the 10 percent level.[10] The point estimate suggests that a 30-percentage-point increase in eligibility (roughly the magnitude of the eligibility increases that actually occurred over this time period) would lead to a reduction of 1.9 percent in the incidence of low birth weight. We conclude that there is some evidence of an effect of the eligibility changes on the incidence of low birth weight but that the effect is relatively small.

In contrast, there is a sizable and significant effect of increasing Medicaid eligibility on infant mortality, regardless of the estimation strategy pursued. The instrumental variables regression indicates that the 30-percentage-point rise in eligibility that took place was associated with an 8.5 percent decline in the infant mortality rate. In columns 5 and 6, we show estimates from models of infant mortality that include both our eligibility measure and the incidence of low birth weight. The strong effect of eligibility on mortality is apparent even after we condition on the positive correlation between eligibility changes and improvements in birth weight.[11]

in the base year are instruments. This assumption would be violated if, e.g., trends in birth outcomes were correlated with the state-specific level of income in the base year as it would be if changes in policy had their largest impact in places in which the population was poorer to begin with.

[10] Our first-stage fit is excellent, with an F-statistic on the instrument of over 10,000.

[11] Our finding of stronger effects on infant mortality than on the incidence of low birth weight is consistent with that of Hanratty (1992), who examined the introduction of National Health Insurance in Canada and found significant effects on mortality but

TABLE 3

OLS REGRESSIONS OF LOW BIRTH WEIGHT AND INFANT MORTALITY ON ELIGIBILITY USING *VITAL STATISTICS* DATA FOR EACH STATE AND YEAR

	LOW BIRTH WEIGHT		INFANT MORTALITY			
	Actual Eligibility (1)	Simulated Instrumental Variables (2)	Actual Eligibility (3)	Simulated Instrumental Variables (4)	Actual Eligibility (5)	Simulated Instrumental Variables (6)
	A. Models Using Fraction Eligible					
Fraction eligible	−2.711	−4.347	−1.875	−3.031	−1.741	−2.822
	(2.124)	(2.601)	(.571)	(.702)	(.563)	(.691)
Low birth weight					.049	.048
					(.010)	(.011)
Adjusted R^2	.968	.968	.915	.914	.917	.917
	B. Models Using Fraction Eligible under Targeted Changes Only					
Fraction eligible	−10.12	−17.81	−2.818	−4.088	−2.340	−3.273
	(3.191)	(4.294)	(.864)	(1.161)	(.858)	(1.159)
Low birth weight					.047	.046
					(.011)	(.011)
Adjusted R^2	.968	.968	.915	.914	.917	.917
	C. Models Using Fraction Eligible under Broad Changes Only					
Fraction eligible	.435	−.345	−.142	−1.031	−.171	−1.009
	(3.348)	(3.836)	(.927)	(1.064)	(.903)	(1.036)
Low birth weight					.065	.065
					(.017)	(.017)
Adjusted R^2	.981	.981	.923	.922	.927	.926
Mean of dependent variable	68.12	68.12	10.66	10.66	10.66	10.66

NOTE.—All regressions include a full set of state and year dummies. Standard errors are in parentheses. Each panel (A–C) presents the results from a separate regression. N = 600 for full sample and targeted changes only; N = 300 for broad changes.

Thus there is evidence that eligibility for health insurance improves health, as measured by birth outcomes. In terms of their stated goal of reducing infant mortality, the Medicaid policy changes of the 1980s were a success. We shall explore the cost of this success below. First, however, we examine the heterogeneous effects of the different types of eligibility policies pursued over this period.

C. *Differential Effects of Targeted and Broad Policy Changes*

Panels B and C of table 3 present models in which the incidence of low birth weight and the infant mortality rate in each state and year are functions of the fraction of women eligible under the targeted and the broad eligibility changes, respectively. The model using targeted changes is estimated over the full set of years (1979–92), whereas the model using broad changes is estimated using the years 1987–92, since broad policy changes were not made before 1987. All the regressions include a full set of year and state dummies, as discussed above.

Table 3 shows that targeted eligibility had much stronger effects on both measures of infant health than broad eligibility. Using the instrumental variables estimates, we find that a 30-percentage-point increase in eligibility under targeted programs would have been associated with a highly significant 7.8 percent decline in the incidence of low birth weight; a similar increase in eligibility under the broad programs would have decreased the incidence of low birth weight by only 0.2 percent. Similarly, a 30-percentage-point increase in targeted eligibility would have been associated with an 11.5 percent decline in infant mortality, compared to a 2.9 percent decline under the broad policy changes. Again, the findings for infant mortality persist when we condition on the incidence of low birth weight.

One difference between our targeted and broad eligibility measures is that the fraction eligible under targeted programs includes those eligible for cash benefits under AFDC, whereas the broad group includes only those eligible for insurance. We have reestimated our models using a measure of targeted eligibility that excludes AFDC recipients. The results are similar, although the standard errors are

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mixed effects on birth weight. It does contrast with the finding of Fischer (1992), who also studied the effects of the Medicaid expansions from 1984 onward. He found strong effects on the incidence of low birth weight for blacks, but no effects on mortality for either race.

somewhat higher, since useful variation in the fraction eligible stemming from changes to the AFDC program is lost.[12]

D. *Robustness*

We address two potential concerns about the robustness of our results in this subsection. First, it is possible that our results could be driven by the experience of one or two outlying state/year observations. We have therefore reestimated all our models using robust regression techniques that first exclude influential outlying observations and then iterate toward a solution by down-weighting observations with larger residuals (Berk 1990). The results are reported in rows 1–3 of table 4. The coefficients are slightly smaller than those reported in table 3, but the only important inference that changes is that the overall results for low birth weight are no longer significant at even the 10 percent level.

A second concern is that there may be time-varying omitted variables that are correlated with both eligibility and birth outcomes.[13] While it is impossible to rule out all possible candidate variables, we consider several likely ones in parts 4–6 of table 4. The first is the abortion rate. There is a large body of literature documenting the relationship between the distribution of birth outcomes and the availability of abortion services (Glass et al. 1974; Lanman, Kohl, and Bedell 1974; Quick 1978; Grossman and Jacobowitz 1981; Corman and Grossman 1985; Joyce 1987; Grossman and Joyce 1990; Joyce and Grossman 1990). These studies all suggest that the women who are most likely to have unhealthy babies if abortion is not available are also most likely to choose abortion. Hence, if there were changes in the availability of abortion that coincided with the changes in Medicaid policy, the estimated effects of eligibility changes could be biased.[14] Estimates from models that include the abortion rate are

[12] These estimates are also potentially biased downward because non-AFDC eligibility changes are negatively correlated with changes in the generosity of AFDC when a fixed federal eligibility standard is imposed.

[13] Recall that state/year economic or demographic conditions have already been purged from the model when we use simulated eligibility as an instrument, and that any fixed state factors will be absorbed by our state effects.

[14] Blank, George, and London (1994) show that there was a negative relationship between restrictions on the Medicaid funding of abortion and the abortion rate over this period. However, Currie, Nixon, and Cole (1996) do not find any direct effect of these restrictions on birth weight. They do find that reductions in the availability of abortion services had a negative effect on average birth weight.

TABLE 4

ROBUSTNESS OF THE BIRTH OUTCOME RESULTS

Robust Regression	Low Birth Weight: Actual Eligibility (1)	Low Birth Weight: Simulated Instrumental Variables (2)	Infant Mortality: Actual Eligibility (3)	Infant Mortality: Simulated Instrumental Variables (4)
1. Fraction eligible	−.821	−1.725	−1.759	−2.420
	(1.638)	(1.990)	(.515)	(.631)
2. Targeted changes only	−6.837	−11.50	−2.791	−3.387
	(2.438)	(3.276)	(.777)	(1.049)
3. Broad changes only	2.178	2.843	.057	−.679
	(2.963)	(3.207)	(.848)	(.921)
4. Fraction eligible	−2.128	−3.256	−1.871	−3.039
	(2.115)	(2.259)	(.574)	(.706)
Abortion rate	−.131	−.129	−.001	.001
	(.039)	(.039)	(.011)	(.011)
5. Fraction eligible	−2.791	−4.479	−1.825	−2.954
	(2.126)	(2.604)	(.570)	(.700)
Neonatal intensive care beds (×100)	.161	.166	−.100	−.097
	(.161)	(.161)	(.043)	(.043)
6. Fraction eligible	−7.893	−10.91	−2.169	−2.851
	(2.510)	(3.068)	(.745)	(.910)
Maternal and child health spending ($1,000,000)	.125	.126	−.025	−.024
	(.059)	(.060)	(.018)	(.018)

NOTE.—Coefficients are taken from regressions similar to those reported in table 3. Each part (1–6) indicates a separate regression. All regressions except model 6 have 700 observations; model 6 has only 542 observations.

shown in part 4 of table 4.[15] While we do find that increases in the abortion rate lower the incidence of low–birth weight births, the inclusion of this variable has no effect on the estimated effect of eligibility.

As discussed above, changes in technology have had an important impact on mortality, conditional on birth weight. In our work so far, we have implicitly assumed that changes in technology were not correlated with changes in Medicaid policy in a state and year. However, according to the Institute of Medicine (1985), the adoption of new technologies is much faster in areas with specialized neonatal intensive care units (NICUs). If states created NICUs at the same time they adopted changes in the Medicaid program, then our estimates could once again be biased. Part 5 of table 4 shows that increas-

[15] The abortion rate data are discussed in Currie et al. (1996). Data from that paper have been updated using Henshaw and Van Vort (1994). The data for 1983, 1986, and 1989/90 are missing and have been interpolated using the surrounding years.

ing the number of NICU beds in the state significantly reduces infant mortality; data on NICU beds are taken from *Hospital Statistics* (American Hospital Association, various years). The inclusion of this variable has little effect on our estimates of the effects of eligibility changes.

Finally, as Grossman and Jacobowitz (1981) show, increasing the number of public clinics can have a significant effect on infant outcomes. It is possible that states either coordinated eligibility increases with increases in state aid to clinics or traded off expenditures under the two types of policies. In either case, our estimates would be biased. In an effort to control for this potential bias, we include state expenditures on maternal and child health centers, in millions of 1986 dollars, in part 6 of table 4. These data are taken from *Public Health Agencies* (Public Health Foundation, various years) but are missing for some years in some states and for all years beyond 1989; our sample is somewhat restricted by the inclusion of this variable. For this subsample of years, our estimates are stronger than those in the full sample and similar to those presented in table 3 for the targeted changes.

IV. Explaining the Heterogeneous Effects: The Role of Medicaid Take-up

Why were the targeted eligibility changes so much more successful than the broad changes in improving birth outcomes? We suggest that part of the answer may lie in the differential take-up of Medicaid by pregnant women made eligible under these different types of policies. As a number of researchers have emphasized, eligibility for social insurance and welfare programs does not automatically translate into coverage. For example, Blank and Ruggles (1993) find that only about two-thirds of women eligible for AFDC take up their benefits, and Blank and Card (1991) find a similar take-up rate for unemployment insurance.

The March CPS asks individuals whether they were covered by Medicaid at any point in the previous year.[16] We can therefore estimate the marginal take-up rate for these Medicaid policy changes; that is, for every 100 women made eligible for coverage of pregnancy,

[16] Unfortunately, prior to March 1988, health insurance coverage in the CPS was assigned according to whether one received coverage under the policy held by the head of the household. Thus those dependents deriving coverage from outside the household were counted as uninsured. After March 1988, each family member was asked about health insurance coverage from any source. This questionnaire change had its largest effect on children below the age of 15, so it should not significantly bias our results. Furthermore, the inclusion of year dummies will capture overall changes in the nature of responses.

how many additional women report coverage? It is important to note that the CPS measure may deviate from administrative measures of Medicaid coverage, since some eligible women may consider themselves covered even though they have not yet signed up for Medicaid. From the patients' point of view, whether they are actually holding a Medicaid card or not may be irrelevant since in many hospitals patients can be signed up when they receive care, or even ex post. For this reason, reported coverage may actually be superior to administrative data on persons who are holding Medicaid cards, because it more closely measures knowledge of eligibility, and it is knowledge that affects behavior.

Of women 15–44 years old, 6.5 percent had a child in any given year during our sample period, so that about 11.4 percent of women in the relevant age range were pregnant at some point during the year.[17] By this calculation, a take-up rate of .114 in the entire population would represent full take-up by pregnant women. This figure is only a lower bound, however, since some of the Medicaid eligibility changes (e.g., those associated with the adoption of the AFDC-UP program) covered not only pregnancy but also other conditions.

We examine the relationship between Medicaid coverage and eligibility using linear probability models that control for other observable characteristics, including race, marital status, employment status, and income.[18] Our data set consists of 526,830 observations over a 14-year period. All regressions include a full set of state and year dummies. In these models, as in our infant outcomes models, there is the potential for omitted variables bias from correlates of both eligibility and coverage (such as state-specific business cycles). Thus we estimate the model using instrumental variables, using our simulated eligibility measure as the instrument.

The results are shown in table 5. Overall, we find that making a woman eligible for Medicaid raises the odds that she will be covered by 3.9 percent. Relative to the baseline full take-up estimate of 11.4 percent, this is a take-up rate of 34 percent. This take-up rate is low relative to those estimated for other social insurance programs; this is a *marginal* take-up elasticity, whereas those studies report average take-up. Given that much of the population affected by these policy

[17] All women who give birth in a year must have been pregnant at some time during that year. In addition, between two-thirds and three-fourths of women whose pregnancies begin in one year will give birth in the next year. Hence, the percentage pregnant in any year is approximately $(1 + 0.75) \times 6.5 = 11.4$ percent.

[18] We use a linear probability model in order to facilitate the use of instrumental variables and for computational ease with our large sample size.

TABLE 5

MEDICAID ELIGIBILITY AND MEDICAID COVERAGE IN THE CPS

	1	2	3
Any eligibility	.039		
	(.009)		
Targeted eligibility		.056	
		(.016)	
Broad eligibility			.019
			(.018)
Age	.006	.006	.006
	(.0004)	(.0004)	(.001)
$Age^2/100$	−.009	−.009	−.010
	(.001)	(.001)	(.001)
White	−.085	−.084	−.081
	(.001)	(.001)	(.002)
Work	−.121	−.118	−.144
	(.001)	(.002)	(.002)
Married	−.121	−.117	−.124
	(.002)	(.004)	(.002)
Number of kids	.041	.041	.047
	(.001)	(.001)	(.001)
Family income/10,000	−.021	−.021	−.019
	(.001)	(.001)	(.0005)
Number of observations	526,830	526,830	215,722

NOTE.—Regressions also include a full set of state and year dummies. Standard errors are in parentheses. All models are estimated by instrumental variables, with simulated eligibility as an instrument.

changes was covered by private insurance, we would expect take-up to be less than full.[19]

In order to address this point further, we examine the results for the targeted and broad expansions separately in columns 2 and 3 (the last regression is run for 1987–92 only). Targeted eligibility has a significant and sizable take-up effect, whereas the effect of the broad policy changes is insignificant.

There are two possible explanations for lower take-up rates under the broad changes. First, the population eligible for the broad changes was less needy: as table 2 shows, this group had a higher overall rate of insurance coverage. But this insurance coverage differential is not large enough to account for the much lower take-up of the broad policies.[20] Second, given a level of need, the broader policy changes may have been less effective. It may be difficult to bring women who have never received any sort of social assistance into the

[19] See Cutler and Gruber (1996) for a more detailed discussion of this issue.

[20] That is, the take-up coefficient for the targeted changes is three times as large, but the noninsurance rate for the targeted eligibles (table 2) is only 50 percent greater.

Medicaid program, either because they do not know about it or because of stigma effects. Rymer and Adler (1987) report that many low-income families and their physicians are unaware that they can qualify for Medicaid even if they do not receive AFDC benefits. It may have been easier for program administrators to find and notify women eligible for the targeted changes because these women had more frequent interactions with government assistance programs, as illustrated in table 2.

V. Exploring the Cost Effectiveness of Medicaid Eligibility Changes

A. *Medicaid Payments per Infant Saved*

It is perhaps not surprising that the tremendous expansions of Medicaid eligibility of the 1980s induced improvement in birth outcomes. The more relevant question from a policy perspective is the cost effectiveness of this policy instrument. In this section, we use data on Medicaid expenditures to address this question.

States report payments made under the Medicaid program to the Health Care Financing Administration each year.[21] These reports break down expenditures according to the class of provider and the category of recipient. We examine total expenditures on physicians, hospital inpatient departments, and hospital outpatient departments and other clinics, for all nondisabled children and nondisabled/nonelderly adults. Unfortunately, these data are not available by type of service (i.e., childbirth) or by detailed population type (i.e., pregnant women and infants). However, it is reasonable to expect that if the expansions worked primarily by improving prenatal care, this would be reflected in higher payments to physicians, hospital outpatient departments, and clinics; if the expansions saved infant lives primarily through expensive interventions during and after birth, then we would expect to see an increase in payments to hospital inpatient departments.

We normalize expenditures using the state's 15–44-year-old female population. All figures are reported in thousands of 1986 dollars; we deflate expenditures on hospital inpatient and outpatient/clinic expenditures using the Consumer Price Index for hospital services, and expenditures for physician services using the Consumer Price Index for physician's services. Once again, we estimate both ordinary least squares (OLS) and instrumental variables models and disaggregate by the type of policy change (targeted vs. broad).

[21] We are grateful to Killard Adamache of Health Services Research for providing us with these data.

TABLE 6

MEDICAID ELIGIBILITY AND MEDICAID PAYMENTS: EVIDENCE FROM HCFA DATA

	Actual Eligibility			Simulated Eligibility		
	Overall (1)	Targeted (2)	Broad (3)	Overall (1)	Targeted (2)	Broad (3)
Total spending	.244	.301	.176	.202	.224	.284
	(.055)	(.085)	(.103)	(.068)	(.115)	(.118)
Physician spending	.054	.092	.034	.033	.092	.028
	(.011)	(.017)	(.019)	(.014)	(.023)	(.022)
Inpatient hospital spending	.163	.171	.162	.155	.125	.273
	(.049)	(.075)	(.091)	(.059)	(.101)	(.104)
Outpatient spending	.027	.038	−.019	.015	.001	−.016
	(.012)	(.019)	(.025)	(.015)	(.025)	(.029)

NOTE.—This table shows the coefficient on various Medicaid eligibility variables from regressions including year and state dummies. For example, the coefficient in col. 2 refers to the coefficient on the actual fraction made eligible under the targeted changes. Standard errors are in parentheses. The dependent variable is payments in thousands of 1986 dollars per 15–44-year-old woman. $N = 686$ for overall and targeted regressions; $N = 294$ for broad regressions.

The results are reported in table 6. As would be expected, overall increases in eligibility significantly increased Medicaid expenditures; regressions using the simulated eligibility measure indicate that an additional eligible woman was associated with an increase in expenditures of $202 per year. The majority of this spending comes through inpatient hospital costs, with smaller increases in physician and outpatient spending (the latter being insignificant). Increases in the fraction eligible under the targeted changes also increased spending significantly in both types of models.

The most striking finding of table 6, however, is that increases in broad eligibility also had a statistically significant effect on spending; in the instrumented model, spending per broad eligible is actually *higher* than spending per targeted eligible. This result is striking because the low take-up of eligibility under the broad expansions suggested that these types of changes would have little impact on Medicaid costs, but the spending numbers indicate otherwise. The two types of eligibility policies have quite different effects on the composition of spending, however. Among targeted eligibles, only about half of spending is on inpatient hospital services, whereas among broad eligibles, over 90 percent of spending is on these services; the sum of physician and other outpatient services is basically unchanged for the broad changes.

One explanation for the differing effects of the two types of policies on costs is that while use of prenatal care and other physician services is a function of individual take-up decisions, the use of expensive inpatient hospital services reflects decisions made by both individuals and hospitals. Hospitals have strong incentives to ensure that eligible women who arrive at the hospital to deliver are enrolled in the Medicaid program, since hospitals are required to treat any patient who comes to them for emergency care and are specifically prohibited from turning away women in labor if they participate in Medicaid (U.S. Office of Technology Assessment 1987*b*). Uncompensated charges to hospitals amounted to $15 billion in 1989 (Gruber 1994), and childbirth was the single largest component, accounting for 17.4 percent of these expenditures (Saywell et al. 1989).

This incentive for hospitals to sign up otherwise uninsured eligibles has always been present, but the incentive became greater with the broad changes, since they affected so many women. Indeed, the U.S. General Accounting Office (1994) reports that in recent years many hospitals have established offices, or contracted with private firms, in order to help Medicaid-eligible patients navigate the often tortuous path toward claiming benefits.

Once enrolled, women may receive much more expensive services than their uninsured counterparts; Wenneker, Weissman, and Epstein (1990) and Hadley, Steinberg, and Feder (1991) find that insured patients receive more intensive hospital treatment than uninsured patients along a number of margins. And in an evaluation of the extension of benefits to women in Massachusetts with incomes less than 185 percent of the poverty line, Haas, Udvarhelyi, and Epstein (1993) and Haas et al. (1993) found that, while newly eligible mothers were no more likely to use prenatal care services or to have higher–birth weight babies, they were more likely to have cesarean delivery, other things being equal.

Of course, if doctors made the same kinds of efforts to enroll potential Medicaid recipients, then payments to doctors might rise under the broad changes as well. In contrast to hospitals, however, doctors may have both fewer opportunities and lesser incentives to enroll women in the Medicaid program; doctors also have the option of denying care to the uninsured, which may be less costly than treating poor women and then attempting to get Medicaid reimbursement. Our results indicate that physician payments rose significantly only under the targeted changes, which suggests that pregnant women need to be aware of their eligibility and to actively seek coverage before they can gain increased access to physicians for prenatal care.

Together with the findings for birth outcomes, these spending results suggest that the broad eligibility changes had a much lower marginal return in terms of improving infant health. Using the instrumental variables estimates, we estimate that Medicaid spending increased by $224 for each woman who became eligible under the targeted changes. We also find that a one-percentage-point rise in targeted eligibility decreased the incidence of infant mortality by 0.041 deaths per 1,000 births. These findings imply that the cost of saving a life through the targeted eligibility changes was $840,000.[22] Using the same methodology, we calculate that the cost of saving a life through broad eligibility changes was $4.2 million, over five times as large.

B. Cost Effectiveness

These substantial costs per life saved are difficult to interpret in a vacuum. Are they large or small? On the one hand, these figures are very large relative to other investments society makes in children. For example, $840,000 would pay for 206 child/years of elementary/secondary education, 247 family/years of AFDC benefits for the typical two-person family, or 280 child/years of Head Start.[23]

On the other hand, studies of the value of an adult life generally arrive at figures that exceed our estimated cost of saving a life via the targeted expansions. For example, Manning et al. (1989) use data from studies of willingness to pay for a small change in the probability of survival to estimate a value of life of $1.66 million. Viscusi (1992) summarizes studies based on compensating differentials for risk of death on the job and concludes that the most reliable estimates range from $4 to $7 million per life saved. Judged by this metric, the targeted eligibility changes, and perhaps even the broad changes, were cost-effective policies. Furthermore, we do not value health improvements short of mortality reductions.

However, although the literature on compensating differentials suggests that the value of a life falls with age since fewer years of life are saved for older workers (Moore and Viscusi 1988), the value of a newborn life may be much less than that of a prime-age adult because investments in human capital have not yet been made. Also, the compensating differentials literature implies that the value of life

[22] This figure is calculated as follows. To generate 1,000 births, given the average fertility rate of .065 in our sample, would require 15,385 women. A one-percentage-point increase in targeted eligibility in this sample would therefore cost $34,462 ($224 for each of 153.85 women). This would reduce the number of infant deaths by 0.041. So, to reduce the number of infant deaths by one would cost $841,000. Note that to the extent that the newly eligible women (under either type of policy change) were getting treated for free when they were uninsured, the net cost to society of the Medicaid expansions is lower than the costs to the Medicaid program. Saywell et al. (1989) show that, in Indiana, the average cost of uncompensated care for pregnancy and childbirth in 1986 was $2,668. Subtracting this from the cost per birth of the targeted expansions lowers the cost to society per life saved to $814,000.

[23] Cost of education is average expenditures per student from the U.S. Department of Education (1991); AFDC costs are taken from the AFDC benefits data used in this paper; Head Start costs come from Stewart (1992).

rises with income because higher-income persons are willing to pay more to save their lives (Evans and Viscusi 1993). The impact of Medicaid policy, and particularly of the targeted policy changes, is concentrated among low-income populations. Hence, to value an infant life using estimates derived from studies of compensating wage differentials, we would have to somehow adjust both for differences in human capital and for differences in income levels.[24] Whether the Medicaid policy changes would appear cost-effective after these adjustments were made is unclear.

A second way to use the compensating differentials framework would involve viewing children as a consumption good and examining the trade-offs parents are willing to make in order to protect infants and unborn children from potential hazards such as dangerous chemicals. We are unaware of any studies of this issue.[25]

An alternative means of assessing cost effectiveness is to compare the cost of saving a life via Medicaid policy to the costs of saving a life via other government interventions. If the government mandates that at least $840,000 be spent to save children through other channels, then the targeted expansions could be viewed as relatively cost-effective. Tengs et al. (1995) review several alternative government interventions aimed directly at children and find that most of them cost substantially more than $840,000; for example, child restraint systems in cars cost $73,000 per life/year saved, or almost $5.5 million for a child with a 74.8-year life expectancy (the average for children born in 1986).[26] By this metric, then, the Medicaid expansions were fairly cost-effective.

Finally, figures given in the Institute of Medicine (1985) report cited above suggest that a policy that saved lives by targeting improved prenatal care to high-risk women would cost $113,000 per life saved.[27] The belief that infant lives could be saved at a reasonable

[24] Adjusting for income would narrow the gap in cost effectiveness between the targeted and broad changes, because the broad changes affected a significantly higher income population.

[25] Alternatively, if children are viewed as consumption goods, one could claim that the value of a newborn was the cost of adoption or of hiring a surrogate mother, both of which are much lower than the cost to Medicaid of saving an infant (see "Morals Meet the Market" 1988).

[26] Other interventions included child-resistant cigarette lighters ($3.15 million), flammability standards for children's sleepwear (ranging from $0 for the smallest sizes to over $1 billion for the largest sizes of clothing), and school bus safety (a range of estimates that all exceeded $10 million). Estimates of the costs of medical interventions such as immunization were much lower, however.

[27] This figure is calculated as follows. To screen 1,000 pregnancies at a cost of $20 each would cost $20,000. One would expect 70 of these pregnancies to result in low-birth weight births in the absence of any intervention. Of these, 47 will be preterm and 28 (60 percent) will be detected by this screening. Prenatal care for these 28 women will then cost $14,000 and will reduce the incidence of low birth weight by 20

cost was one of the driving forces behind the adoption of the Medicaid expansions, as discussed above. However, our estimates of the cost of saving a life through even the targeted eligibility changes are considerably higher than this baseline.

It is possible that these high figures reflect an emphasis on saving lives through interventions during and after birth rather than through increased use of appropriate prenatal care. Alternatively, it may be the case that improved prenatal care under the Medicaid program has not been narrowly targeted to high-risk women. If prenatal care designed to reduce preterm delivery was delivered not only to women identified as high risk, but to all pregnant women, the Institute of Medicine report implies that the cost of saving a life would rise to $1.06 million, a figure that is in line with our estimate of the cost of the targeted expansions.[28] The next subsection tries to distinguish between these alternatives by examining the effect of the expansions on the utilization of prenatal care using individual-level data from the National Longitudinal Survey of Youth (NLSY).

C. *Prenatal Care Utilization*

While there is some debate in the literature over which elements of prenatal care are most effective, there is widespread agreement that it is critical that women receive some care in the first trimester of their pregnancy (Institute of Medicine 1985). Early initiation of prenatal care is important both for conducting initial screenings and for establishing a baseline for the monitoring of maternal and fetal health (U.S. Office of Technology Assessment 1987*b*). The NLSY records the month in which prenatal care began, so that we can examine the effect of targeted Medicaid eligibility on whether a pregnant woman delayed the initiation of prenatal care beyond the first trimester.

The NLSY began in 1979 with a sample of 6,283 women between the ages of 14 and 21. Since 1983, women have been asked biannual questions about the prenatal care that preceded each birth; retrospective information has also been collected for births before 1983. The NLSY contains enough information about income, family structure,

percent, or six babies. Using the *Vital Statistics* data below, we estimate that a decrease of one low–birth weight baby per 1,000 births lowers the infant mortality rate by 0.05 deaths per 1,000 births. Thus the $34,000 spent to reduce low–birth weight births by six babies will save 0.30 lives, for a cost of $113,333 per life saved. This figure understates the benefits of prenatal care since some of the babies who would not have died but now have a higher birth weight will be less impaired later in life.

[28] That is, there would be $500,000 spent in delivering prenatal care to all 1,000 women in the sample, and 9.4 preterm low–birth weight births would be prevented (since 100 percent of at-risk cases would now be detected). This implies a cost per life saved of $1.06 million.

and state of residence to allow us to determine whether the woman was eligible for Medicaid coverage in the first trimester of the pregnancy, using a program similar to that developed using the CPS. After the exclusion of missing values, we are left with 4,997 observations on births that occurred between 1979 and 1990.[29]

The data indicate that throughout our sample period, women eligible for Medicaid coverage of their pregnancies are poorer, less educated, and more likely to be African-American or Hispanic relative to the sample as a whole.[30] They also have much lower scores on the Armed Forces Qualification Test (AFQT), a standardized test of ability.[31] Hence, it is not surprising that Medicaid-eligible women are more likely to delay obtaining prenatal care: 26 percent of women eligible under targeted programs delay care compared to 19 percent of noneligibles.

The disparity in the fractions who delay care highlights the possibility that estimates of the effects of individual Medicaid eligibility on the usage of prenatal care will be biased by omitted variables correlated with both eligibility and the propensity to seek care. In order to address this problem, we instrument individual eligibility using both the actual and the simulated fraction eligible in the state and year calculated using the CPS. We examine only the impact of the targeted eligibility changes because the post-1987 sample size is quite small. Also, since only the targeted changes affected Medicaid coverage, it seems reasonable to assume that the broad changes had little impact on the use of prenatal care.[32]

[29] It is important to note that the NLSY is not a representative sample of U.S. women in the relevant age range because African-Americans, Hispanics, and the poor were oversampled. Almost half of the infants are African-American or Hispanic, and 73 percent of the African-American infants, 78 percent of the Hispanic infants, and 32 percent of the other infants were born to women from the supplemental "poverty" sample. We control for race, ethnicity, and membership in the poverty sample in our analysis.

[30] In order to attenuate the effects of random measurement error and minimize the amount of missing data, we use the average income in the two years preceding the birth as our measure of income. If the woman was living with her parents, then we use the parents' income less the need standard for a family of that size (following the procedure used by the AFDC program to impute family resources to minors living at home). Otherwise, we use the sum of the woman's own income, the spouse or partner's income, and "other" income. The use of this measure also avoids the imputation of eligibility for prenatal care based on temporary income losses suffered after the birth.

[31] Since the AFQT was administered to all the women at the same point in time, scores were normalized using the mother's age. Some readers may prefer to regard the AFQT as a summary measure of background and education rather than as a measure of native ability.

[32] Indeed, reestimating these models using overall eligibility yields somewhat weaker results than if targeted eligibility only is used.

We estimate linear probability models that include exogenous characteristics of the mother and child, in addition to a full set of year and state dummies.[33] As discussed above, the inclusion of state fixed effects controls for time-invariant characteristics of states that may be correlated with state Medicaid policy.[34]

The instrumental variables results are presented in table 7. Column 1 indicates that even conditional on observable characteristics, Medicaid-eligible women are more likely to delay prenatal care than other women. However, when we instrument using the actual fraction eligible, individual eligibility is associated with decreases in delay; this coefficient is significant at the 9 percent level when the simulated instrument is used. The estimated effect is quite large: targeted Medicaid eligibility is found to decrease the probability of delay by almost half when the simulated instrument is used. Thus these NLSY data suggest that women eligible for Medicaid under the targeted programs did increase their usage of prenatal care in the direction recommended by the medical literature.[35]

VI. Discussion and Conclusions

A key question for health care reform is whether covering the uninsured will actually lead to improvements in health. While a number of studies have shown that the uninsured are in worse health, the issue of causality is clouded by the fact that the uninsured may be fundamentally less healthy than the insured, independent of insurance status. Our approach to this problem is to examine exogenous changes in the Medicaid eligibility of pregnant women in the 1980s and early 1990s. Judged by the most frequently used indicator of birth outcomes, the infant mortality rate, the Medicaid eligibility changes were a great success: the 30-percentage-point increase in the fraction of women eligible for Medicaid in the event of pregnancy was associated with a decrease in infant mortality of 8.5 percent.

However, a closer look suggests important heterogeneity in the effects of the two different types of policies we examine. The broad

[33] The results are similar if logit models are used instead.

[34] However, models with state fixed effects demand a lot of our data: although there are over 600 observations for the largest state (California), nine states represented in our sample have fewer than 15 observations. We do not include fixed effects for these nine states, so together they form the omitted "state."

[35] In a sample of this size, there are very few deaths, but it is possible to ask whether individual eligibility was associated with decreases in the incidence of low birth weight. We have estimated models similar to those shown in table 7 using low birth weight as the dependent variable, with disappointing results: the standard errors are simply too large for us to be able to draw any inferences. It may be that larger sample sizes are necessary before an effect can be detected in individual-level data. For further results on birth weight using the NLSY, see Currie and Cole (1993).

TABLE 7

EFFECTS OF MEDICAID ELIGIBILITY ON DELAYING PRENATAL CARE, NLSY (N = 4,997)

		Instrumental Variables	
	OLS	Actual	Simulated
Eligible under targeted changes	.044	−.280	−.466
	(.019)	(.252)	(.275)
Child characteristics:			
Firstborn	−.002	−.007	−.010
	(.012)	(.013)	(.014)
Multiple birth	−.117	−.098	−.087
	(.056)	(.059)	(.062)
Male	−.011	−.011	−.011
	(.011)	(.012)	(.012)
Maternal characteristics:			
African-American	.017	.028	.034
	(.017)	(.020)	(.021)
Hispanic	.056	.077	.083
	(.019)	(.022)	(.023)
Highest grade	−.013	−.014	−.015
	(.003)	(.004)	(.004)
AFQT score	.025	.016	.010
	(.012)	(.015)	(.016)
Income below poverty	.087	.164	.209
	(.014)	(.062)	(.067)
Member of poverty sample	−.009	−.007	−.006
	(.013)	(.014)	(.014)
Urban resident at age 14	−.016	−.017	−.018
	(.014)	(.015)	(.015)
R^2	.042	.039	.037

NOTE.—All regressions include state and year dummies. Standard errors are in parentheses. The dependent variable is a dummy indicating that the woman waited until after the third month to initiate prenatal care. Individual eligibility is instrumented using the actual and simulated fractions eligible in the state and year, calculated from the CPS.

expansions of Medicaid eligibility to all low-income women appear to have had little effect on birth outcomes, primarily because they were not effectively translated into increased Medicaid coverage, even among needy (otherwise uninsured) women. However, these eligibility changes did increase Medicaid payments for inpatient hospitalization, perhaps as a result of hospitals' efforts to enroll eligible women upon delivery. As a result, the broad eligibility changes have so far been both costly and quite ineffective.

On the other hand, targeted increases in insurance eligibility have been an effective means of improving infant health. But these improvements have come at a high cost relative to previous estimates of the cost of saving lives through improvements in prenatal care. We are unable to distinguish among three possible explanations for this finding. First, although we show that eligibility was associated with earlier initiation of prenatal care, it is possible that the utilization of care, or the type of care received, may still have fallen short of optimal levels. Second, prenatal care may be less cost-effective than has been supposed, perhaps because it is not as precisely targeted to high-risk births as clinical studies presume. Finally, changes in Medicaid eligibility may have led to increased use of expensive hospital services as well as prenatal care services. A more detailed investigation of the effects of the eligibility changes on the utilization of different types of prenatal and hospital care would be a fruitful direction for future research.

It is difficult to draw a firm conclusion on the cost effectiveness of Medicaid policy, but the cost of targeted policies does appear to be low relative to either compensating differentials estimates of the value of adult lives or the cost of other government regulations designed to save the lives of children. In any case, the targeted expansions were clearly more cost-effective than the broad eligibility changes.

Thus our research offers an important insight into the design of public insurance policy. The cost of extending public insurance will be lowered if patients can be induced to consume an efficient bundle of services. In the case of pregnant women, this can happen only if the newly eligible take up their benefits and receive appropriate prenatal care. Groups that have not previously qualified for government social insurance programs may be particularly hard to reach. Fortunately, public policy at the state level is moving to address this problem. Several states have adopted public relations campaigns with themes such as "Baby Your Baby" (Utah) or "Baby Love" (North Carolina) to accompany expansions in Medicaid eligibility. Buescher et al. (1991) found that the North Carolina program had significant positive effects on the utilization of prenatal care and on birth outcomes. To the extent that informational problems are to blame for the inefficient utilization of resources under the broad eligibility changes, the effect of these changes may grow over time as information about the program diffuses through the eligible population.

A question that our research cannot resolve is whether insurance policy can be effective in improving the health of other populations. Birth outcomes may be particularly responsive to interventions. It would be useful to extend the methodology developed here to explore the effect of exogenous changes in insurance coverage on health at other points in the life cycle.[36]

[36] For two recent attempts to do so for children, see Currie (1995) and Currie and Gruber (1996).

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