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THE TECHNOLOGY OF BIRTH: HEALTH INSURANCE, MEDICAL INTERVENTIONS, AND INFANT HEALTH

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ABSTRACT

Two key issues for public insurance policy are the effect of insurance status on medical treatment, and the implications of insurance-induced treatment differentials for health outcomes. We address these issues in the context of the treatment of childbirth, using Vital Statistics data on every birth in the U.S. over the 1987-1992 period. The effects of insurance status on treatment and outcomes are identified using the tremendous variation in eligibility for public insurance coverage under the Medicaid program over this period. Among teen mothers and high school dropouts, who were largely uninsured before being made eligible for Medicaid, eligibility for this program was associated with significant increases in the use of a variety of obstetric procedures. On average, this more intensive treatment was associated with only marginal changes in the health of infants, as measured by neonatal mortality. But the effect of eligibility on neonatal mortality is sizeable among children born to mothers whose closest hospital had a Neonatal Intensive Care Unit, suggesting that insurance-induced increases in use of "high tech" treatments can have real effects on outcomes. Among women with more education, however, there is a countervailing effect on procedure use. Most of these women had private insurance before becoming Medicaid-eligible, and some may have been "crowded out" onto the public program. These women moved from more generous to less generous insurance coverage of pregnancy and neonatal care. This movement was accompanied by reductions in procedure use without any discernable change in neonatal mortality.

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The share of the U.S. population without health insurance coverage has grown by 15% over the past 8 years to 17.4% (Employee Benefits Research Institute, 1996). This decline in insurance coverage raises important questions about the role of health insurance in determining patterns of care and health outcomes. Many studies document the fact that the uninsured have fewer contacts with the medical system than their insured counterparts, and that in particular, they are less likely to see doctors for preventive care.[1] But less is known about disparities in the treatment of insured and uninsured patients, *conditional* on gaining access to the medical system. Are uninsured patients treated less intensively than their insured counterparts? And does any existing differential in treatment intensity have important implications for health outcomes?

A number of previous studies have addressed the first of these questions, examining differences in hospital treatment by insurance status. The findings have been mixed, which may reflect an inability to control for differences in the underlying health of insured and uninsured patients, or for differential selection into the hospital. Moreover, perhaps due to these underlying selection problems, there has been little attempt to map the effects of insurance-induced treatment differentials into health outcomes.

In this paper, we address both of these questions in the context of the treatment of childbirth. The main advantage to our approach is that we are able to exploit the tremendous variation in insurance status that arose from expansions of the Medicaid program, the public insurance program that covers low-income women and children.[2] Among pregnant women, eligibility for Medicaid coverage has greatly expanded since 1987, and this expansion has occurred at a differential pace

[1] See for example Kasper (1986); Short and Lefkowitz (1992); Mullahy (1994); Currie and Thomas (1995); Currie and Gruber (1996b).

[2] The Medicaid program also covers other low income groups, the elderly and the disabled; low income women and children represent the vast majority of enrollees, although they account for a minority of program spending.

across the states. As a result, these eligibility changes can be used to identify the effect of insurance status on treatment and outcomes, producing estimates that are not contaminated by unobserved individual heterogeneity. Moreover, since virtually every woman in the United States delivers her baby in a hospital, and hospitals are essentially required to treat women in labor, it is possible to obtain a picture of treatment patterns that is not contaminated by the selection of patients into the hospital.

An additional advantage of examining childbirth is that it is a diagnosis with well defined measures of both underlying health (birthweight and gestational age), and of the outcome after any intervention (mortality of the infant). Excellent national data on both the treatment of childbirth and birth outcomes is available from the National Center for Health Statistic's (NCHS) uniform birth certificate data. These data cover the full census of births in the U.S. in each year, and provide information on several common interventions used during childbirth. Moreover, the NCHS has linked information about infant deaths to these natality data, allowing us to examine the effect of insurance on mortality conditional on fetal health, thereby isolating the effect of the treatment of childbirth and neonatal care on survival probabilities. The locational detail available in these data also allows us to use distance to a hospital with a level-III Neonatal Intensive Care Unit (NICU), as a measure of access to "high tech" interventions that have been shown to save infant lives.

We find that recent expansions of the Medicaid program had significant effects on the medical treatment of child birth and on neonatal mortality. We focus first on mothers who are teens or high school dropouts, a group that was largely without insurance before becoming eligible for Medicaid. In this group, eligibility expansions increased the generosity of insurance coverage, and hence increased eligibility was associated with an increase in the utilization of a variety of obstetric procedures. On average, this increase in treatment intensity was not associated with significant reductions in infant mortality conditional on fetal health. But there were sizeable reductions in mortality among those women whose nearest hospital had a NICU. This finding suggests that insurance coverage increases utilization of both low tech and high tech interventions, and that the latter effect has real health benefits.

There is also evidence of a countervailing effect on aggregate procedure utilization among other (higher education) mothers. These women were much more likely to have had private insurance coverage before becoming eligible for Medicaid. Some of these women may have been "crowded out" of private insurance onto the public program in response to becoming eligible. To the extent that this movement occurs, these women may be thought of as moving from more generous to less generous coverage of childbirth, since Medicaid reimburses providers at lower rates than do private insurance plans. As a result, we find that in this group, increased eligibility is accompanied by reductions in procedure utilization, which in the aggregate largely offset the increases in procedure use among the (smaller) group of teens and dropouts. These reductions in procedure use do not have any discernable effect on mortality, however, suggesting that the care sacrificed by those moving from private insurance to public insurance coverage may not have been beneficial on the margin.

The paper proceeds as follows: Part I provides background information about the Medicaid expansions and prior evidence regarding the effects of insurance coverage on the utilization of hospital care. Part II describes the data sources and empirical strategy. Part III documents the effects of Medicaid eligibility on the treatment of childbirth. Part IV examines the effects of the expansions on mortality. Conclusions are presented in Part V.

Part I: Background

a) Insurance Status and Treatment Patterns

In the standard economic model of provider behavior, hospitals/physicians care both about profits/income (or about minimizing costs in the case of non-profits), and about quality/quantity of care as it effects patient well-being.[3] In such a model, uninsured patients, who are unlikely to pay much of their hospital bill, will either be shunned or treated less intensively than their insured counterparts. These incentives can be large; hospital uncompensated care amounted to $15 billion in 1989 (Gruber, 1994), and childbirth was the single largest component, accounting for 17.4% of these expenditures (Saywell *et al.*, 1989). By federal regulation, hospitals that accept any payments from Medicare (i.e. virtually all hospitals) must treat women who are in labor, reducing hospitals' ability to use patient avoidance to lower costs. However, hospitals may still prove more or less welcoming to poor patients through a variety of mechanisms.[4] And within hospitals in which uninsured women in labor show up to deliver their babies, there is an incentive to treat these women less intensively.

As a result, when pregnant women who would previously have been uninsured become eligible for Medicaid, we can predict three responses. First, hospitals that serve poor patients will make every effort to identify eligibles and make sure that they become covered, since the alternative

[3]For the case of hospitals, see for example Dranove (1988); for the case of physicians, see for example McGuire and Pauly (1991).

[4]For example, hospitals may not post information in Spanish or provide translators, if knowledge of English is strongly correlated with insurance coverage. Evidence on such hospital behavior is obviously difficult to document in a systematic fashion, however.

is generally that the hospital provides uncompensated care.[5] Second, newly signed-up women may be treated more intensively for their childbirth (and accompanying neonatal care) within hospitals, since the expected reimbursement to both the hospital and the physician has risen. Finally, some hospitals that previously encouraged such women to go elsewhere may become more welcoming; this could also cause an increase in treatment intensity, if these hospitals have better facilities or more intensive treatment styles.

Although the theory is clear, however, the size of any increase in procedure use is an empirical question. It will depend on the extent to which procedure use is supply rather than demand driven; on the marginal costs of supplying procedures whose fixed costs have already been absorbed by providers; on the marginal reimbursement for more intensive treatment under the Medicaid program; and on the extent to which a given procedure is viewed as "essential" rather than "discretionary", since providers are assumed to care about patient well-being. This last consideration raises the important additional question of whether changes in procedure use in response to differential reimbursement have significant effects on health outcomes.

A number of studies have examined the effect of insurance on treatment intensity. These studies have established that the uninsured have shorter hospital stays, and receive fewer procedures than the privately insured (Kelly, 1984; Sloan *et al.*, 1986; Weissman and Epstein, 1989; Wenneker, Weissman, and Epstein, 1990; Hadley, Steinberg, and Feder, 1991). The differences are particularly pronounced for procedures categorized as "discretionary". However, these studies find no consistent differences between the treatment of the uninsured and the treatment of patients covered by Medicaid. This latter comparison may be more salient if one wishes to consider the likely effects

[5]Indeed, the 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 towards claiming benefits (U.S. General Accounting Office, 1994).

of extending eligibility for public health insurance.

Moreover, the interpretation of these findings is complicated by two selection issues. First, there may be underlying differences in the health of individuals who choose to become covered by private insurance or by Medicaid rather than remaining uninsured. Both types of coverage reflect individual choices to some extent, and these choices are likely to be correlated with health status or tastes for intensive treatment. Second, there may be differences in the prognosis of patients upon admission to a hospital that can affect their treatment. For example, since patients without insurance are more likely to be using hospital clinics and emergency rooms for their primary care, they may be more likely to be hospitalized with a given diagnosis than the insured. Hence, it is possible that they could be healthier upon admission to hospital, and less likely for this reason to be treated intensively.[6] A final limitation of the literature comparing insured and uninsured patients is that it has not documented the implications of insurance related differences in patient treatment for health outcomes, a critical issue for welfare analysis of insurance policy.

It is important to note that increased eligibility for public insurance, while potentially increasing treatment intensity among those who move from being uninsured to being covered by Medicaid, may also have a countervailing effect on treatment intensity through the movement of some women from private insurance to Medicaid coverage. Upon gaining eligibility for Medicaid coverage of pregnancy, some privately insured women could find it advantageous to switch to this public program, for two reasons. First, child birth is the single largest medical expense most young women are likely to face. Second, the average privately insured person pays roughly one-third of the cost of their medical care through copayments, deductibles, and premium sharing, amounting to

[6]Of course this pattern of utilization could bias in the opposite direction as well, if a lack of primary care among the uninsured causes them to be in worse health upon admission. Evidence about observable differences in severity upon admission by insurance status is mixed.

over $1200 in 1987, while Medicaid is completely free.[7] It is also possible that employers of low wage employees will cease to offer insurance coverage, given that many of their employees will be eligible for Medicaid coverage of a large portion of their potential medical bills. Cutler and Gruber (1996) estimate that for every two persons who enrolled in the Medicaid program as a result of the expansions, one person dropped private insurance, for a "crowdout" of 50%.

Since Medicaid typically reimburses at about half the rate of private insurers (Currie, Gruber, and Fischer, 1995), these "crowded-out" women can be thought of as moving from more generous to less generous insurance coverage. Consequently, providers may choose to decrease the supply of procedures offered to these women. This decrease will be a function of the generosity of reimbursement for more intensive treatments under Medicaid, relative to reimbursements in the private sector. There is little empirical work, however, on the response of treatment intensity to Medicaid reimbursement differentials.

b) The Medicaid Expansions

Our discussion of the previous literature noted two potential sources of bias in comparing the treatment of insured and uninsured patients: selection into a hospital setting and selection into insurance. The former source of bias is not a problem in the context of childbirth, since virtually every birth in the U.S. occurs in a hospital. In order to address the second selection problem, we need a source of variation in the insurance status of the target population that can plausibly be viewed as exogenous with respect to health and tastes for treatment intensity. The Medicaid expansions fit

[7] Of course, there are some counterbalancing disadvantages of Medicaid, such as the fact that many physicians are reluctant to see Medicaid patients due to low reimbursement rates, and the fact that individuals may not be able to move freely back to private insurance if their Medicaid eligibility ends; see Cutler and Gruber (1996) for a more detailed discussion of this issue.

this description.

Medicaid is operated as a joint federal/state program, with the federal government offering matching funds to states whose programs meet certain requirements. Historically, eligibility for Medicaid was generally restricted to very low income single mothers and children who received cash welfare payments under the Aid for Families With Dependent Children (AFDC) program. Since the generosity of the AFDC program varied a great deal from state to state, income thresholds for Medicaid eligibility also varied.

Beginning in the early 1980s, and particularly after 1987, eligibility for Medicaid coverage of the expenses of pregnancy and child birth was greatly expanded; and since 1986, any infant whose birth was covered has also been covered for 60 days afterwards. These Medicaid expansions were first introduced as options that states could take or leave. Later, the expansions were made mandatory in the sense that states that did not implement them would not receive matching funds for their Medicaid programs. By 1992, all states were required to cover the expenses of pregnancy and child birth for women in households with incomes up to 133% of the poverty line, and were permitted to extend eligibility up to 185% of the poverty line.[8] As a result, the share of women who were eligible for Medicaid coverage should they become pregnant rose from 20% in 1986 to almost 45% in 1992 (Currie and Gruber, 1996a).

More importantly for our purposes, there was tremendous heterogeneity across the states in the size and timing of these expansions. This heterogeneity provides the exogenous variation in the insurance eligibility of mothers that we use to identify our models of procedure use and outcomes. For example, from 1986 to 1992, eligibility for pregnancy and childbirth coverage among women

[8]States were also permitted to raise this income threshold above 185% of the federal poverty line, but could not receive federal matching funds to do so.

of child bearing age rose by 34% in Texas and 33% in Alabama, but by only 5% in Utah and 3% in Washington state. The fact that the federal government gradually imposed a uniform income threshold on a set of state programs that initially had widely varying thresholds accounts for most of the variation in Medicaid eligibility over our sample period.[9] Thus, changes in eligibility were largely exogenous from the point of view of state governments as well as individual mothers.

c) Related Work on the Expansions

Two previous studies are closely related to ours. Hass et al. (1993), show that expansions of Medicaid in Massachusetts in the mid-1980s were associated with increases in the rate of cesarean section delivery among the previously uninsured. While suggestive, in this case study the authors were unable to control for any underlying time series trends in the rate of procedure use in the low income population which might confound the analysis. By considering differential changes in eligibility across the states, controlling for time series trends, and using a broader variety of obstetric procedures, we are able to generalize this case study evidence.

Currie and Gruber (1996a) examine the impact of the Medicaid expansions on health outcomes and Medicaid program costs using aggregate state-level data on eligibility and outcomes. They find that in the early 80s, narrowly targeted increases in Medicaid eligibility to low income women who had been ineligible for reasons of family structure induced significant declines in infant mortality. However, the much larger post-1987 expansions of Medicaid to women of higher income had small and statistically insignificant effects on infant outcomes. Nevertheless, these later expansions substantially increased payments to hospitals under the Medicaid program, raising the

[9]See Currie and Gruber (1996a) for more details on these expansions. Cutler and Gruber (1996) estimate that 70% of the increased eligibility for pregnant women in this era was due to the federal mandates.

possibility that Medicaid affected treatment patterns even if it did not affect outcomes.

This study improves on our earlier work in two respects. First, we focus specifically on the effect of Medicaid on treatment at the point of birth (or shortly thereafter). Second, we use the natality microdata to exploit within-state differences in Medicaid eligibility across groups at a point in time.[10] Making use of this within state/year variation allows us to more precisely estimate the impact of Medicaid, as well as to control for any state-specific time trends that could otherwise confound our estimates. Most importantly, it allows us to disaggregate the sample of births into two groups: disadvantaged mothers who were likely to be moving from being uninsured to Medicaid; and other women who may have been moving from being privately insured to being covered by Medicaid.

Part II: Data and Empirical Strategy

a) Data

Our primary data sources are the 1987 to 1992 Detail Natality, and the 1987 to 1991 Linked Birth/Infant Death data released by the National Center for Health Statistics (NCHS, various years). The natality data is collected from birth certificates, and is a census of all births in the United States. There are approximately 4 million births per year. Birth certificates give the mother's state and county of residence, as well as information about her age, race, and education. In addition, there are data on several measures of fetal health including birthweight and gestational age.

Information about mortality rates can be computed using the linked files, which match information on all infant deaths to the natality files. Following the medical literature on infant

[10]Cole (1995) also follows this strategy in her examination of the effects of the expansions on the use of prenatal care.

outcomes, we focus on the neonatal mortality rate, which measures infant deaths up to one month of age. Note that over our sample period infants whose deliveries were paid for by Medicaid would also be covered by the program.

We focus on four obstetric procedures that are available on the birth certificate data: cesarean section delivery; use of a fetal monitor; receipt of an ultrasound; and induction/stimulation of labor. All of these technologies other than ultrasounds are used predominantly in a hospital setting, and generally close to or at the point of birth.[11] The prevalence of these procedures varies widely, as shown in the first panel of Table 1. Roughly three-quarters of all women use a fetal monitor, while only 20% of births have induced/stimulated labor. Slightly under one-quarter of births are by cesarean section, while about one-half involve an ultrasound.

All of these procedures are "low-tech" in the sense that they have been used for many years and involve relatively simple interventions. Newer, "high-tech" procedures, such as the treatment of the child in a NICU, are not yet reported on birth certificates. Access to interventions such as cesarean section undoubtably save some lives; however, there is little evidence that recent increases in the rate of cesarean section delivery improved birth outcomes (Gruber and Owings, 1996).

Nevertheless, the procedures listed on birth certificates are of interest for two reasons. First, much previous discussion of the effects of insurance on the use of technology focuses on very expensive high-tech procedures that are used relatively infrequently. Because child birth is a frequent

[11]Cesarean delivery is obviously done in the hospital at the point of birth, as are inducement and stimuation of labor. Fetal monitoring is generally done in a hospital inpatient or outpatient setting close to the point of birth, although it may be done somewhat earlier as a diagnostic (but rarely earlier than the 25th week because the infant's heartbeat cannot be accurately detected). Ultrasounds are generally done outside of the hospital in the second trimester, although for high risk or post-date deliveries they may be done closer to the point of delivery. Moreover, for women who first come to a provider close to the delivery, the decision to perform an ultrasound near the delivery to obtain some baseline data on the fetus could be sensitive to reimbursement incentives.

event, even low cost procedures could increase health care costs if they were widely adopted. For example, at 1989 prices, a 30% increase in the rate of Cesarean section would have increased the total costs of child birth in the United States by almost a billion dollars (Gruber and Owings, 1996).[12] Second, use of these obstetric procedures may serve as indicators of a general propensity to use both low tech and "higher tech" procedures during and after the birth.

While utilization of these procedures is therefore worthy of study, it is important to emphasize that our analysis *does not* propose to establish any direct link between these specific low-tech procedures and mortality. Rather, our interpretation will be that Medicaid coverage of the previously uninsured is associated with an overall shift towards more aggressive treatment, and that this more aggressive regime may have effects on outcomes. We also look below for indirect evidence about the use of "higher tech" procedures not listed on birth certificates by examining interactions between the probability that a woman is eligible for Medicaid and a measure of access to hospitals that have a NICU.

b) Heterogeneity

The focus in this paper is on the effect of Medicaid eligibility on treatment patterns. We focus on eligibility for two reasons. First, we have no data on insurance coverage in the uniform birth certificates database. Second, eligibility is the regressor of interest from a policy perspective, since this is the tool that is available to policy-makers. Thus, our empirical work will ask: How does

[12]This figure is calculated using the differential hospital and physician charges for cesarean section delivery relative to normal childbirth. Overall, there were $5.6 billion in hospital charges for cesarean delivery in 1992; this was, for example, 41% as large as total hospital spending on cardiac bypass surgery, a representative "high tech" procedure (based on unpublished data provided by Mark McClellan). Even low cost procedures such as fetal monitors (roughly $150 per use) can add up when used on many births; in 1992 approximately $454 million was spent on this procedure (authors' tabulations).

the treatment of childbirth in a given population change when their eligibility for public insurance expands?

Conditional on gaining eligibility, however, there are two routes to Medicaid coverage. The first is moving from being uninsured to Medicaid, thereby gaining insurance coverage and presumably increasing the incentives for more intensive treatment by providers. The second is moving from private insurance to Medicaid, thereby lowering the generosity of insurance coverage, and decreasing the incentives for more intensive treatment. Ideally, our data would measure the mother's insurance status before pregnancy, which would allow us to separate these two routes onto the Medicaid program, but such information is not available.

Therefore, we stratify our sample using an indicator that is strongly correlated with the ex-ante availability of private insurance coverage: whether the birth is to a teen mother (less than 19 years old) or high school dropout, versus other mothers. Women in the former group are unlikely to have had private insurance coverage before being made eligible for Medicaid, so that they are likely to be gaining insurance coverage when they join the program; for other women, however, the crowdout mechanism may be operative.

Evidence about the insurance status of these two groups is presented in the second panel of Table 1. This panel shows information from the March 1988 CPS, which asks about insurance coverage in 1987. Since the CPS does not ask whether women are pregnant, we include in our sample women who have a child less than one year old at the time of the survey.[13] We examine

[13]Since women are asked about insurance coverage in the previous year, which is 1987 (when the expansions were just beginning), and since the survey is carried out in March, three quarters of the women with a child less than one year old will have had that child in the previous year.

the coverage of this group by Medicaid and by other insurance (predominantly private insurance).[14]

It is clear that these two groups were quite distinct in terms of their probability of having private health insurance coverage. Among teens and high school dropouts, fewer than one-third of women were covered by non-Medicaid insurance, so that there was little scope for crowdout. On the other hand, almost 80% of the other women were covered by non-Medicaid insurance. This suggests that this is a useful split of the data from the point of view of separating mothers likely to be gaining insurance coverage (teens/dropouts) from those likely to be switching insurance coverage (other mothers).

In fact, as the second and third columns of the first panel of Table 1 show, there are substantial differences in procedure utilization between the two groups. For example, the rate of cesarean section is 30% lower in the teen/dropout group, as is the incidence of induced labor. These women are also less likely to receive an ultrasound during the pregnancy or to use a fetal monitor. Although the mothers in the "other" group are somewhat older on average, it is unlikely that these differences in treatment are solely a reflection of differences in the underlying health of the fetus; in fact, as Table 1 shows, these other woman have healthier babies, as measured by birth weight and gestation. These differential treatment patterns are therefore suggestive of a role for insurance coverage. Finally, neonatal death rates are much higher among the teens/dropouts, although it is impossible to determine from these tabulations whether this is due to poorer underlying fetal health, or to less intensive treatment of childbirth.

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[14]We include Medicare coverage (for the disabled) and CHAMPUS coverage (for military dependents) along with private insurance, since our main concern (as described below) is with payment differentials between Medicaid and other payers, and these payers pay roughly the private sector rate for services. Our results are similar if we just use private insurance. There is a small group that reports coverage by both Medicaid and some other form of insurance during the year, which we exclude; the results are very similar if we instead count them in both categories.

We provide further evidence about the effects of Medicaid eligiblity on the type of insurance held by our two groups in Appendix 1. It provides a regression analysis of the effect of Medicaid eligibility on coverage using the Current Population Survey. The results are imprecise, but they support the idea that among teens and dropouts, eligibility was associated with large increases in Medicaid coverage with little reduction in private insurance; while among all other mothers, the increase in Medicaid coverage is smaller and the reduction in private insurance coverage more sizeable. We therefore use this sample split in the empirical work below.

c) Empirical Strategy

The goal of our empirical analysis is to examine the effect of public insurance eligibility on the treatment of childbirth, and on child survival probabilities. We do not have information in the Vital Statistics data, however, on a number of key determinants of eligibility (such as income). We therefore incorporate outside information on eligibility, in two steps. First, we use data from the March Current Population Survey (CPS) to impute the fraction eligible for Medicaid in each of several demographic groups, states, and years. We do so using a detailed simulation program that summarizes each state's Medicaid policy in each year; see Appendix 2 for details. Second, data on the fraction eligible in each demographic group/state/year is matched to the Vital Statistics data in order to estimate reduced form effects of eligibility on procedure use and outcomes.

More specifically, we proceed as follows. First, 48 age/race/education cells are defined using data from the March CPS's for each year, which have sufficient information on income, family structure, and location to determine eligibility for Medicaid.[15] Second, a nationally representative

[15]There are 16 categories for each racial group, where the race categories are white, black, and "other race". We categorize births on the basis of the mother's race. There is one category for teen mothers within each racial group. Mothers 19 or over are divided into 3 age categories (19 to 24,

random sample of women of child-bearing age (15 to 44) for each cell is drawn from each year's CPS.[16] Then, this *same sample* is used to calculate the fraction of women in each of these 48 cells who would be eligible for Medicaid if they lived in each state, using our simulation program. That is, we ask how many white women aged 19-24 who were high school dropouts would have been eligible had they lived in California, how many would have been eligible had they lived in Texas, etc. After computing the percent eligible for each cell in the CPS data, these "simulated" eligibility measures are matched to each birth in the Vital Statistics data using the mother's age/race/education category, state, and year. In effect, a probability of being eligible is assigned to each woman in the Vital Statistics data, using information about similar women from the CPS.

Note that using a nationally representative population (rather than a state-specific sample) is the only feasible means of imputing eligibility to 48 groups in each state and year, given the size of the CPS. In addition, this measure provides a convenient index of the generosity of state Medicaid rules that utilizes only two sources of variation: differences in eligibility rules across states and over time, and within-state differences in the effects of the rules on nationally representative samples of women from different groups. For example, an increase in eligibility from 75% of the poverty line to 133% of the poverty line (as was mandated by the Omnibus Reconciliation Act of 1989) would be expected to have little impact on highly educated older women since these women generally have incomes higher than 133% of poverty. But it would be expected to have a large impact on younger,

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25 to 34, and 35 and over), and are further subdivided into 4 education groups (less than 12 years, 12 years, between 12 and 16 years, greater than or equal to 16 years). When we refer to controls for education in the regression specification below, teen mothers are controlled for as a fifth education category.

[16]We use all women of child-bearing age, rather than just women with children less than one year of age, due to sample size constraints; we are unable to obtain the requisite demographic variation, even using a national sample, if we restrict ourselves to women with young children.

less educated women whose incomes more often lie between 75% and 133% of poverty.

Since variation in eligibility is only identified at the state/age/race/education level, and since there are no other exogenous individual-level covariates in the Vital Statistics data, we aggregate our data up to the cell level. Our analysis is conducted using these cells as the unit of observation, which yields 2448 observations per year. The fraction eligible in the median cell is .40, while in the teen/dropout and all other groups the comparable figures are .62 and .31. All our regression models are weighted by cell size.

The models estimated are of the following form:

$$\text{(1)} \qquad PROC_{jtrae} = \alpha + \beta_1 ELIG_{jtrae} + \beta_2\sigma_{rae} + \beta_3\delta_j + \beta_4\tau_t + \beta_5\delta_j{}^*\pi_r + \beta_6\tau_t{}^*\pi_r + \beta_7\delta_j{}^*\eta_e + \beta_8\tau_t{}^*\eta_e + \beta_9\delta_j{}^*\tau_t + \epsilon_{jtrae},$$

where j indexes states; t indexes years; r indexes races; a indexes ages; e indexes education groups, PROC is the average rate of utilization of a given procedure in that cell, ELIG is the simulated fraction of women eligible for Medicaid coverage of their pregnancies in each state/year/race/age/education cell, and σ_{rae}, δ_j, τ_t, π_r, and η_e are full sets of dummy variables for the 48 race/age/education cells, state, year, race, and education groups, respectively.

This model relates the average rate of utilization of a particular procedure in a cell to the probability that a woman in that cell is eligible for Medicaid. Our detailed set of controls are included to account for possible spurious correlation between Medicaid eligibility and underlying variation in utilization. Such a correlation could arise across demographic groups, which might have systematic correlated differences (for example) in income and tastes for medical intervention; we therefore include dummy variables for each demographic group. There may also be state and time-specific variables that are correlated with both Medicaid eligibility and outcomes, so fixed effects are included for both states and years. In addition, average outcomes differ dramatically by race, and by education: for example, the infant mortality rate is over 2 times higher for blacks than for whites and is three times higher for teen mothers than for college graduates. It is possible that time trends or state effects could vary along these dimensions as well, so interactions of state and race, time and race, state and education, and time and education are all included.

Finally, the underlying processes determining treatment patterns could vary within states across years, due for example, to other changes in state policy (such as hospital reimbursement practices). In order to control for possible biases due to omitted variables of this kind, interactions of state and year effects are included in our models. Note that even after state/year interactions have been included, our model is identified by variation in legislation over states and years as it affects different groups. Thus state-specific changes in the circumstances of particular groups that affected both our measure of eligibility and procedure use would violate our identication assumptions.[17] However, since our eligibility measure is constructed using a national rather than a state-specific sample, we remove such correlation from the model. That is, we rely only on differences in the effects of *rules* across places, time, and groups, and not on differences in the *characteristics* of the population in each place and time. Thus, we have isolated the component of variation in eligibility that is most likely to be exogenous with respect to procedure use and outcomes.

These regression models are estimated using grouped logits. Since a number of cells have zero means, we use Cox's (1970) specification of the dependent variable: $\log[(P + (2*N)^{-1}) / (1 - P + (2*N)^{-1})]$, where N is the number of observations, and P is the probability that the outcome is observed in the sample data. The estimates are similar if the zero observations are simply excluded.

[17]For example, if there was a recession in a given state which caused members of a particular group to lose jobs that offered health insurance, this might raise the eligibility of this group (through lower income) and decrease procedure use (through less insurance coverage).

Part III: Effects on Procedure Use

a) Basic Results

The first column of Table 2 shows the coefficients on the fraction eligible from estimates of model (1), for the teen/dropout group. Each row of the Table shows the coefficient of interest (β_1) from a separate procedure-specific regression. Our findings provide a striking confirmation of the proposition that insurance status matters for treatment intensity. We find that eligibility has positive and highly statistically significant effects on the use of all four procedures examined. This result suggests that being made eligible for Medicaid moves women in this group, who were more likely to be uninsured before becoming eligible, to a generally more aggressive treatment regime.

Since the coefficients in these grouped logits are somewhat difficult to interpret, the second column of Table 2 shows the implied percentage point (and percentage) effects of a 10 percentage point increase in Medicaid eligibility. The estimates imply that a 10 percentage point increase in eligibility would be associated with a 0.45 percentage point increase in the rate of cesarean section delivery, which is 2.4% of the baseline rate for this group. The percentage effects are fairly similar across these different procedures, ranging from 1.7% to 2.4%.

If we wish to examine the effects of Medicaid policy on the overall utilization of care, it is important to remember that teens and dropouts were not the only ones potentially affected by the expansions. As noted above, among other mothers, eligibility may affect procedure use negatively, to the extent that it induces a shift from private insurance to the Medicaid program.

The results for all others are presented in the first panel of Table 3. In this sample the estimated effects of Medicaid are indeed negative and statistically significant for three of the four procedures (they are insignificant for ultrasounds). For cesarean delivery, the negative effect is larger in absolute terms than the positive effect on the teen/dropout group discussed above, *and the* size of the effect is roughly comparable in percentage terms. These findings suggest that reductions in private insurance coverage that were coincident with the eligibility expansions may be having real effects on the treatment of women at childbirth.[18]

The second panel of Table 3 shows that there is little overall effect on procedure use when the teens/dropouts are pooled with all others. Use of two procedures rises and use of two procedures falls, but in all cases the effects are quite small, relative to the large effects observed for the teens/dropouts only.

We conclude that insurance status matters for treatment. Table 2 suggests that among teens and dropouts, increases in expected provider reimbursement as a result of Medicaid eligiblility led to an increase in overall treatment intensity. Table 3 suggests that among other mothers, who are more likely to be experiencing "crowdout", there is a reduction in the rate of procedure use. Thus the Medicaid expansions had little impact on the overall treatment of childbirth, although inequities in procedure utilization were reduced. These estimates imply that Currie and Gruber's (1996a) estimate of the effect of the expansions on Medicaid program costs overstate the social costs of the expansions, since increases in costs borne by Medicaid may have been partially offset by reductions in claims to private insurers.

[18]There is a potentially countervailing effect of crowdout for the higher education group, through demand side cost-sharing. Since these individuals are now paying less on the margin for their medical care (since private insurance often has copayments and Medicaid does not), they may demand more intensive treatment once they move to the Medicaid program. For invasive treatments at the point of childbirth, such as cesarean section delivery, it seems likely that the supply side incentives would dominate; but for earlier and more discretionary procedures such as ultrasounds, the demand side incentives may be equally important. This may explain the positive effect on ultrasounds, relative to the negative effect on other procedures. We are grateful to Richard Frank for making this point to us.

b) Reimbursement Differentials

The evidence in Tables 2 and 3 strongly suggests that insurance coverage matters for the treatment of childbirth. One means of confirming this finding is to ask whether the estimated effects of Medicaid eligibility are greatest where the Medicaid program offers the largest financial incentives for intensive treatment. Ideally, we would carry out this test using information on differential reimbursements of both hospitals and physicians for a variety of obstetrical procedures. Unfortunately, the available fee data is more limited. For physicians only, we were able to obtain cross-state data for 1989 to 1992 on Medicaid reimbursement for vaginal and for cesarean delivery.[19] The difference between the fee for a cesarean and the fee for vaginal delivery provides a measure of the physician's financial incentive to substitute towards cesarean delivery under Medicaid.

In 1989, Medicaid paid physicians a (birth-weighted) average of $635 for a vaginal delivery and $127 more for a cesarean. In contrast, private insurers paid an average of $1476 for a vaginal delivery and $561 more for a cesarean (Health Insurance Association of America, 1989). The fact that the Medicaid differential is only 23% as large as the private sector differential (in dollar terms) is consistent with our finding that for cesarean delivery, the negative effect on the other mother sample is larger than the positive effect on the teens and dropouts; on average, moving from private insurance to Medicaid induces a larger change in differential reimbursement to physicians than moving from being uninsured to Medicaid.

The difference between Medicaid reimbursements for vaginal and cesarean deliveries varies

[19]This data comes from American College of Obstetricians and Gynecologists (1988,1990,1992), Holahan(1991), and PPRC(1991). These data are for the global fee differential, which includes pre and post-natal care. These fee data are not available for 1991; we interpolate data for that year as an average of 1990 and 1992.

widely across the states, and within states over time. The differential ranged from 0 to $653 dollars in 1989. Over the 1989 to 1992 period, this differential rose only slightly ($39) on average, but the changes across states varied from an increase of $403 to a decrease of $203. This extensive variation allows us to test for interactions between Medicaid eligibility and reimbursement generosity in a model of treatment intensity.

We do so by estimating models of the form:

$$(2) \qquad CS_{jtrae} = \alpha + \beta_1 ELIG_{jtrae} + \beta_2 ELIG_{jtrae}{}^{*}DIFF_{jt} + \beta_3 \sigma_{rae} + \beta_4 \delta_j + \beta_5 \tau_t + \beta_6 \delta_j{}^{*}\pi_r + \beta_7 \tau_t{}^{*}\pi_r + \beta_8 \delta_j{}^{*}\eta_e + \beta_9 \tau_t{}^{*}\eta_e + \beta_{10} \delta_j{}^{*}\tau_t + \epsilon_{jtrae},$$

where CS is the cesarean section delivery rate in a cell, and DIFF is the Medicaid reimbursement differential in a state and year. In order to control for state-specific medical price levels we normalize the Medicaid differential by private reimbursement for vaginal delivery, as calculated in Currie, Gruber, and Fischer (1995)[20]; unfortunately, we do not have state-by-state data on the private reimbursement differential between cesarean and vaginal delivery.

We expect that the main effect on Medicaid eligibility, β_1, will remain positive/negative for the low education/other mother samples, since we are measuring differences in physician incentives only and have no data on hospital incentives. But we expect that in both samples the interaction β_3 will be positive: where the fee differential is the greatest, there should be the largest increase (or smallest decrease) in cesarean delivery. That is, for teen/dropout mothers, higher fees increase the positive incentive to perform cesareans on women who have become eligible for the Medicaid program. For other mothers, higher physician reimbursement reduces the negative effect of

[20]The private fee data are for 1989 only, and are inflated forward using year-year increases in state-specific hospital costs. Data are missing for four states; in those cases, we use the average private fee information for that division, where there are 9 divisions in the U.S. The regression results are very similar if the dollar fee differential itself is used, or if the Medicaid fee differential is normalized instead by the Medicaid fee for vaginal delivery.

crowdout on treatment, since it reduces the disparity in payments that can be expected from different reimbursement sources. There is no main effect for fee differentials since these vary only by state and year, so that the direct effect of fee differences is not identified in a model with state*year interactions.

Estimates of these interactions are shown in Table 4; we show only the coefficients of interest, β_1 and β_3. Among teens and dropouts, the main effect remains positive as expected. There is also a sizeable and significant interaction between Medicaid eligibility and the fee differential, showing that more generous (relative) reimbursement of c-sections increases their use. The estimates indicate that if there were no Medicaid fee differential for cesarean delivery, a 10% increase in Medicaid eligibility would raise the cesarean delivery rate by only 0.35 percentage points (as compared to the 0.45 percentage point effect in Table 2). But if instead the existing differential were raised to the level paid (on average) by the private sector (38% of the private vaginal delivery fee), this same increase in eligibility would raise the cesarean delivery rate by 0.65 percentage points, or 3.5% of the baseline rate.

For the other mothers, we once again see sizeable negative main effects on Medicaid eligibility, suggesting that physician fee differentials alone do not drive the reduction in procedure use as these women move from private insurance to Medicaid. But there is a significant positive interaction with the fee differential. These results imply that if Medicaid paid no fee differential for cesarean delivery, a 10% increase in Medicaid eligibility would lower the cesarean delivery rate by 0.70 percentage points. But if the Medicaid differential rose to the average level in the private sector, this eligibility increase would lower the cesarean delivery rate by only 0.27 percentage points, or 1.2% of the baseline rate in this population.[21] These findings suggest a powerful role for provider financial incentives in determining the treatment of childbirth.

Part IV: Effects on Mortality

a) Effects of Eligibility on Mortality

The results above suggest that the Medicaid expansions had a substantial impact on procedure use. In order to assess the welfare implications of these changes, however, we must ask whether they were associated with changes in infant outcomes. Did more intensive treatment of the babies born to teens and dropouts improve their outcomes? Did the newborns in the sample of other mothers who were treated less intensively suffer worse outcomes as a result?

Changes in treatment intensity may not translate into significant effects on outcomes for two reasons. First, as we have emphasized, there is no direct evidence that marginal changes in the utilization of procedures recorded in the birth certificate data have real effects on birth outcomes; although if increases in the use of these procedures indicate a general increase in the utilization of procedures not yet recorded on birth certificates, then there could be real effects. Second, the *marginal* procedure use that is induced or denied as a result of changes in insurance coverage may not have important effects on health outcomes if doctors and hospitals provide essential services without regard for insurance status.

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[21]The positive fee interaction for the sample of other mothers also rules out an alternative explanation for our findings on treatment intensity for this population: physician income effects. In a standard "demand inducement model", treatment intensity varies inversely with physician incomes (Gruber and Owings, 1996). It is therefore possible that the increase in physician incomes resulting from increased procedure utilization among teens and dropouts lowered procedure utilization among other mothers through the income effect. If this were true, however, then there would be a negative coefficient on the fee differential interaction: where fees were highest, the income effect would operate most strongly, and we would see the largest drop in demand inducement among other mothers.

To examine the implications of changing treatment, we model the effect of Medicaid eligibility on neonatal mortality. Our evaluation of Medicaid's effects is complicated by the fact that insurance policy can affect mortality through two channels: by improving the underlying distribution of fetal health through better prenatal care, or by influencing the way that mothers and newborns are treated conditional on the level of fetal health. Fortunately, the excellent measures of fetal health available in the natality data allow us to condition on fetal health in order to assess the direct effect on outcomes through interventions at childbirth or later. That is, we can estimate models similar to (1), but including measures of fetal health. These controls will capture any effect of Medicaid through improvements in prenatal care, and the remaining influence of Medicaid will reflect the effect of insurance on treatment, the channel of interest for our analysis.

Models of mortality similar to (1), but using the neonatal mortality rate in each cell as a dependent variable, are shown in the first row of Table 5. The mortality data are available only up to 1991, but the sample can be extended back to 1987 in order to take advantage of more of the variation in eligibility that accompanied the Medicaid expansions. Once again, the sample is divided into teens and high school dropouts, and all others. The second columns of each panel show the absolute and percentage (in parentheses) effects of a 10 percentage point increase in the fraction eligible for Medicaid.

Among the teens and dropouts, there is a marginally significant (at the 7% level) negative effect of Medicaid eligibility on mortality when fetal health controls are not included. The point estimate suggests that a 10% increase in eligibility lowers neonatal mortality by 2.3%. When the fetal health controls are included, however, this effect falls by roughly one-quarter, and is no longer statistically significant. Thus, while these results suggest that treatment intensity matters for outcomes, the estimates are very imprecise.

We saw above that increases in eligibility among all other women were associated with decreases in insurance coverage, and reductions in treatment intensity. It is natural to ask whether there were corresponding increases in mortality among this group. The second panel of Table 5 shows that changes in the fraction eligible are not associated with changes in mortality in this group, either unconditional or conditional on fetal health.[22]

This finding may simply reflect the imprecision of our estimates, since only a small share of these other women will be moving from private insurance to Medicaid and mortality is a relatively rare outcome. Alternatively, it is possible that women drop private insurance coverage in order to take up Medicaid coverage only if they feel that the risk of a negative birth outcome is low. If women understand that less generous coverage lowers treatment intensity that is beneficial for high risk births, then the set of mothers selected into the crowdout group will be drawn from the lowest end of the distribution of expected risk. This finding raises the possibility that the crowdout of private insurance by less generous public insurance can reduce procedure use without any adverse effect on health.

b) Interactions With Distance to a Hospital With a NICU

The discussion above is based on the assumption that any effects of eligibility on mortality that are not accounted for by fetal health are due to changes in the treatment of childbirth and newborns. In this section, we take a somewhat more direct approach, by asking whether the effects of eligibility are larger if one has relatively more access to "high tech" treatments. If the effects of Medicaid operate through increasing treatment intensity at birth, then the effects should be greatest

[22]Note that, overall, the finding of little net mortality effect over the post-1987 period is consistent with the results in Currie and Gruber (1996a).

where there are potentially important technologies available that can be differentially applied as the mother's insurance status changes.

More specifically, we carry out this test by measuring whether the effects of Medicaid on mortality are larger for women who have more access to a hospital with a Neonatal Intensive Care Unit (NICU). A number of studies have documented dramatic effects of NICUs on fetal health. For example, Paneth *et al.* (1982) find that after adjusting for observables, one month mortality rates for low birth weight infants were 27 to 39% lower in NICUs than in other hospital units, despite the fact that the infants in NICUs are expected to be the sickest babies. And Phibbs (1995) finds that babies born in hospitals with level-III NICUs have significantly higher survival probabilities conditional on fetal health. Moreover, there is limited access to hospitals with a NICU for a large share of the U.S. population. This provides the variation in access necessary for our identification strategy.

Of course, hospitals with NICUs are also potentially more likely to provide other interventions that improve the mortality prospects of newborns. Thus, NICU access may be proxying for access to a range of neonatal services. So, more generally, this test assesses whether Medicaid has more significant effects on mortality when the mother has access to high tech care. If we find that this is so, it suggests that becoming eligible for Medicaid induces increased use of high tech services as well as of the low tech procedures listed on birth certificates.

We construct a measure of distance to a NICU in several steps. First, for each zip code in the U.S., we measure distance from that zip code to the nearest hospital with a NICU.[23] The NCHS data does not report zip code of residence for mothers; the finest locational detail available is county of residence. Thus, for each mother in our sample, we calculate the probability that she

[23]This is based on hospital zip code. The hospital data are from the American Hospital Association annual survey, and were provided by both the AHA and Health Economics Research, Inc.

lives in each zip within her county, based on her race, age, and education. These probabilities were calculated using a matching algorithm, and zip code characteristics from the 1990 U.S. Census. Third, we take a weighted average of distances to NICUs in the person's county, where the weights are the probabilities that an individual lives in each zip code in the county. This average is then assigned to the person as their distance measure.[24]

The absolute distance to a NICU may not be the relevant concept, however, since women will go to a hospital to deliver their baby in any case. Rather, it may be more relevant to ask whether the closest hospital has a NICU: i.e. to use the distance to a NICU relative to the distance to the nearest hospital that delivers babies.[25] Since hospital choice is strongly negatively correlated with distance, then this "relative distance" will be closely related to the probability that a woman delivers in a hospital with a level-III NICU.[26] Moreover, absolute distances to NICUs could be correlated with a host of other geographic variables that determine the effectiveness of Medicaid policy; for example, distances are likely to be shorter in cities than in rural areas. McClellan and Newhouse (1996) suggest using relative distances as a way of controlling for general differences in the availability of medical care.

Therefore, as our measure of access we use the share of women in each cell for whom the

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[24]In other words, if a county has no segregation by age, race, or education, we are simply assigning the average distance to a NICU in the county. But to the extent that there is some segregation along these dimensions, this provides us with some additional precision in assigning location to the mothers in our data; in the limit, with perfect segregation (i.e. all black high school dropouts in one zip code), we could assign zip code of residence with certainty.

[25]This is defined as a hospital which either reports an obstetrics department, or which reports having any births during the year.

[26]The literature on hospital choice demonstrates that distance is in general the key determinant of the site of care (Luft et al., 1990). Phibbs *et al.* (1993) shows that distance is an important determinant of choice of hospital for child birth, and Howell *et al.* (1993) show that distance affects the probability that a low-birthweight infant is born in a hospital with a NICU.

closest hospital has a NICU.[27] The distribution of this variable is displayed in the final panel of Table 1. In the average cell, 37% of births are to women who have a NICU in their nearest hospital. In the 10th percentile cell, this share is only 17%; but in the 90th percentile cell, the majority of women have a NICU in their nearest hospital. This distribution is fairly similar across our two groups, although there is slightly more proximity for the teens/dropouts.

We use this distance measure to estimate models of the form:

$$(3) \qquad MORT_{jtrae} = \alpha + \beta_1 ELIG_{jtrae} + \beta_2 NICU_{jtrae} + \beta_3 ELIG_{jtrae}{*}NICU_{jtrae} + \beta_4\sigma_{rae} + \beta_5\delta_j + \beta_6\tau_t + \beta_7\delta_j{*}\pi_r + \beta_8\tau_t{*}\pi_r + \beta_9\delta_j{*}\eta_e + \beta_{10}\tau_t{*}\eta_e + \beta_{11}\delta_j{*}\tau_t + \gamma FH_{jtrae} + \epsilon_{jtrae},$$

where NICU is the fraction of women in the cell whose closest hospital has a NICU, and FH is our set of fetal health controls. If Medicaid affects outcomes primarily by increasing utilization of NICUs among previously uninsured patients, then for the low education sample, β_1 should be negative and small (or possibly zero) and β_3 should be negative and large; that is, Medicaid should only be found to reduce mortality where a NICU is available. β_2 measures the average effect of NICU access on births in the sample.

Models including this distance measure are shown in Table 6 for the teen/dropout group. When the interaction between eligibility and distance is added to the model for teens and dropouts, the main effect on Medicaid eligibility goes to zero, while the interaction itself is sizeable, negative, and statistically significant. These findings suggest that Medicaid eligibility has no effect on (fetal health-conditional) outcomes among mothers whose nearest hospital does not have a NICU, but it has large effects among mothers whose nearest hospital does have a NICU. More specifically, we find that raising eligibility by 10% among teen/dropout mothers whose nearest hospital has a NICU

[27]That is, these are women for whom either the nearest hospital has a NICU, or for whom the nearest zip code with any hospital has a hospital both with and without a NICU.

would lower mortality by roughly 4.5%, but would have no effect for other teen/dropout mothers. On the other hand, for this sample of teens and dropouts, there is no effect of NICU proximity per se.

These findings have two implications. First, the fact that NICU proximity determines the effectiveness of Medicaid eligibility, along with the absence of a main effect for NICUs, suggests that Medicaid eligibility is expanding utilization of higher tech services among some teens and dropouts. Second, the results suggest that the marginal increase in the use of higher tech services brought about by the Medicaid expansions had real effects on outcomes. These effects are large; we estimate that the 24% rise in eligibility for our low education group over the 1987-1992 period lowered the mortality among those with access to a NICU by almost 11%.

Of course, we cannot tell whether changes in insurance status affect the hospital that women choose to arrive at for delivery or whether patients within the same hospital are being treated differently. That is, it may be that hospitals with NICUs make themselves relatively unattractive to the poor, and that they become more welcoming with increases in the fraction of these women eligible for Medicaid; alternatively, a given hospital with a NICU may increase the likelihood of NICU admission when women gain insurance. While it would be of great interest to separate these channels, no information about hospital choice is available on the birth certificates, and little research has been done into the mechanisms governing the selection of patients into different kinds of hospitals. In either case the implication of our findings is the same: Medicaid increases access to NICUs and/or related technologies, and this has real health effects.

One explanation for this finding is that we have not controlled sufficiently for the characteristics of areas that do and do not have NICUs. In order to investigate this alternative hypothesis, we constructed several other measures of conditions in the mother's zip code. These measures were constructed in the same way as the NICU distance measure. That is, we assign a probability that a mother lives in each zip code in her county using Census data and the mother's demographic characteristics. We then take a weighted average of the characteristics of each zip code where the weights are given by these probabilities. These characteristics are only measured once, in the 1990 census. But there is still some variation in our measure over time as the composition of births in each cell changes. The following set of zip code characteristics are included: percent black; percent of families that are female headed; percent of household heads that are high school dropouts; percent high school graduates; percent with some college; the male unemployment rate; median family income; and percent urban.

The second row of the first panel of Table 6 shows a model similar to that in the first row, except that it includes these zip code controls. The estimates are similar to those discussed above: the main effects on Medicaid and NICUs are not statistically significant, but the interaction is large and negative, although statistically significant only at the 90% level of confidence. Overall, these results are supportive of the findings in the first row: Medicaid has a bigger effect if the mother has access to a NICU.

The next panel of Table 6 shows the estimated effects for all others. As might be predicted given Table 5, there is little effect of Medicaid eligibility in this sample, either through the main effect or through the interaction. There is, however, a negative and (in the second row) marginally significant main effect of NICU access on mortality; this estimate indicates that raising the share of the sample whose closest hospital has a NICU by 10% lowers mortality by 2.7%. It is striking that access to hospitals with a NICU per se has no effect for the teen/dropout group, while it has a large negative effect for other mothers. This finding supports our contention that NICUs (or other correlated hospital resources) may be applied differentially across populations with different underlying levels of insurance coverage, with real health consequences. It also suggests that Medicaid is equalizing the distribution of treatment intensity across less and more advantaged mothers.

Part V: Conclusions

This study offers evidence that insurance affects the way patients are treated, and that these treatment differentials affect outcomes. Among teen mothers and high school dropouts who would be largely uninsured in the absence of Medicaid, we find that expansions of Medicaid eligibility were associated with the increased use of a variety of procedures, suggesting that increasing the generosity of insurance coverage causes a general increase in treatment intensity. We also show that physician financial incentives played an important role in this move to increased treatment intensity, as the effect on cesarean-section delivery was largest where differential Medicaid reimbursement of cesarean delivery was most generous.

Overall, this increased intensity of procedure use had positive but imprecisely estimated impacts on mortality, conditional on fetal health. But Medicaid eligibility had significant effects on the subset of teen/dropout mothers whose closest hospital had a NICU, and little effect on other teen/dropout mothers. This finding suggests that insured and uninsured populations have differential access to NICUs and related interventions, and that this difference has real implications for health outcomes.

As we have highlighted, however, there is a countervailing effect on procedure use among other mothers, some of whom may be "crowded out" of their private insurance by increased Medicaid eligibility. These women can be thought of as moving from more generous to less generous coverage of their pregnancies. In this group we find reductions in procedure use without any accompanying change in infant mortality. This last result is provocative because it suggests that the social costs of expanding eligibility for health insurance to the needy could be offset to some extent by reductions in the number of procedures obtained by the more affluent, without causing any harm. Indeed, although the Medicaid expansions increased public expenditures, they may have had little effect on the net social costs of paying for child birth and neonatal care, while equalizing the treatment of more advantaged and less advantaged groups of mothers.

Our findings raise an important priority for future work: assessing the *process* by which hospital resources are differentially applied to women with insurance coverage of differing levels of generosity. Does differential procedure use arise largely through hospital choice, or through changes in treatment intensity within hospitals? How do hospital and physician financial incentives interact to determine the treatment of differentially insured patients? Answers to these questions will help provide a richer understanding of the health production process, as well as providing insights into efficient reimbursement and insurance eligibility strategies for the public sector.

Appendix 1: Medicaid Eligibility and Insurance Coverage

We wish to identify those women most likely to move from being uninsured to Medicaid coverage when they are made eligible, and those who are more likely to move from private insurance to Medicaid if they are affected by the eligibility expansions at all. The facts in the second panel of Table 1 suggest that the split into teen mothers and high school dropouts versus all other mothers may accomplish this goal. In this appendix, we provide more direct evidence on this question using regression analysis of the relationship between eligibility and insurance coverage in the CPS.

The effect of eligibility on coverage is modelled using CPS data over the 1987 to 1992 period. For the regression analysis, we use the full sample of women of child-bearing age (15 to 44), since we were unable to obtain usefully precise estimates using the sub-sample of women with small children. Our dependent variable is an indicator equal to one if the individual reports that she was covered by Medicaid at any time in the past year, or, alternatively, a dummy variable for whether the individual has some other type of insurance. In addition, as noted earlier, a small share of the sample reports that they were covered both by Medicaid and by some other form of insurance during the year. Individuals who dropped or lost private insurance coverage and took up Medicaid within the last year will be in this group. Hence, they are treated as an additional "crowdout" category.[28] The model specification is the same as equation (1) except that the dependent variables are measured at the individual level and linear probability models are estimated.

In order to estimate the extent of takeup and crowdout, we must take account of the fact that Medicaid only covers pregnancy, while our sample includes all women between 15 and 44, most of whom are not pregnant at a given point in time. Given average fertility rates, only 12 out of 100 women potentially eligible for the Medicaid expansions would be covered at any point in time, even if there were full takeup of Medicaid.[29] At the same time, women who drop private insurance to join the Medicaid program might be uninsured at times other than during their pregnancy. Thus there is a timing problem involved in trying to use the Medicaid and private insurance coefficients to calculate crowdout. As a result, the measure of crowdout may be greater than 100%, as in Cutler and Gruber (1996). Our results are therefore more useful for documenting a pattern of responses across groups, than for precisely estimating the extent of crowdout.

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[28]While we do not know whether women in this category are moving from private insurance to Medicaid or *vice-versa*, there is no reason to think that increased Medicaid eligibility should be associated with flows from Medicaid to private insurance. Thus, any association between Medicaid eligibility and being in this overlap category should be associated with movement from private insurance to Medicaid. Note that the Medicaid and other insurance variables are defined exclusively, in order to avoid double-counting.

[29]This number is calculated as follows: All women who gave birth during a year must have been pregnant at some point during that year. In addition, 3/4 of women whose pregnancies begin in one year will have their births in the next. The fertility rate over our sample period is 6.8%. Hence, the fraction pregnant at any point in a given year is roughly $(1+.75)*.068$, or .12. This is to some extent an underestimate, since some of the categories of women covered by Medicaid (such as women covered through the AFDC program) receive coverage for all of their costs.

The results of our investigation are reported in Table A1, which reports the coefficient on the fraction eligible. Using the full sample of women, we find that a 10% increase in the fraction eligible in a woman's demographic group is associated with a .65% increase in the probability of Medicaid coverage. This estimate implies a takeup rate of about 55% (relative to the 12% pregnancy rate described above). However, this increase in Medicaid coverage is accompanied by a reduction in other insurance coverage of 1.12%, as well as an increase in combined coverage of 0.13%, so that there appears to be a greater than 100% crowdout of other health insurance coverage. As noted above, this pattern is consistent with the fact that Medicaid coverage is available only for pregnancy, while private insurance may be dropped (or lost) for a longer period of time.

In the next two rows, the sample is divided into two groups: teen mothers and high school dropouts, and all others. We find that among the teen/dropout group, the expansion of Medicaid eligibility did increase insurance coverage. The Medicaid coefficient of 0.109 implies a takeup rates of 80% -- close to full takeup.[30] There is some crowdout, but the estimated coefficient is not statistically significant. Thus, among teens and dropouts it appears that most of the change in eligibility is translated into increases in coverage.

Among all others, however, the estimated Medicaid takeup rate is only 50%, and the reduction in private insurance coverage is twice as large as for the low education group.[31] In addition, the fraction eligible has some effect on the probability of being in the overlap category for these other mothers, suggesting even further crowdout. Unfortunately, as discussed above, we cannot estimate the precise extent of crowdout, but much of the increase in Medicaid appears to be offset by reductions in other forms of insurance coverage. For this group, then, there may be little net change in insurance coverage, but rather a shift in the type and generosity of coverage.

In summary, these estimates support the view that in response to becoming eligible for Medicaid, many teens and dropouts moved from being uninsured to being covered by Medicaid. In contrast, to the extent that other mothers were affected, they were moving primarily from private insurance to Medicaid coverage of their pregnancies.

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[30]This calculation uses the group-specific fertility rate (7.9%), rather than the overall average. Similarly, for other mothers we use their group-specific rate of 6.5% for the calculation.

[31]Although the estimates are not statistically significantly different across the two samples, however.

Appendix 2: Simulating Medicaid Eligibility

In this appendix, we describe the simulation program that we used to compute Medicaid eligibility. Eligibility arises from one of three sources:

1) *AFDC eligibility*. In order to qualify for Aid to Families with Dependent Children (AFDC), a family must pass three tests: their gross income must be below a 185% of the state's needs standard; their gross income less certain disregards for work expenses and child care must be below the state's needs standard; and their gross income less certain disregards less a portion of their earnings must be below the state's payment standard.

The exact definition of a family unit is the first source of difficulty in making this calculation. If a minor (which we define as less than age 19) is living with her parents, then a portion of the parents' income is deemed to that individual in making the eligibility calculation. This fraction is calculated by subtracting from family income the needs standard for a family of that size. If the individual is age 19 or above, then the treatment of family resources is less clear, and varies across states; see Hutchens *et al.* (1989) for a description of these differing treatments. We assume, following the practice of the majority of the states, that the parent's resources are ignored if the individual is not a minor.

For the first four months that they are enrolled in the program, individuals on AFDC can keep $30 per month plus one-third of their earnings. In addition, since 1985, individuals who would have lost Medicaid due to the end of the $30 and 1/3 rule after 4 months were allowed to remain on Medicaid for an additional 9 to 15 months (the length was at state discretion). We modelled this as amounting to a full 30 and 1/3 exclusion for the entire year.

Finally, a key restriction on the receipt of AFDC is family structure. In all states, single women with at least one child are eligible. In addition, in some states, married women with an unemployed spouse are eligible under the "AFDC-UP" program. Eligibility for AFDC-UP conditions on both current employment status and work history. Lacking longitudinal data on work histories, we assume that families are eligible if the state has a program and the spouse had worked less than 40 weeks in the previous year. Since families eligible for AFDC-UP make up only a small fraction of the overall AFDC population, this should not greatly affect our estimates

2) *Medically Needy*. One state option of potential importance is the Medically Needy program, which is designed to cover individuals who meet the family structure requirements for AFDC and whose gross resources are above AFDC levels, but whose high medical expenditures bring their net resources below some certain minimal level. States who take up this option may establish Medically Needy thresholds that are no more than 133% of the state's AFDC needs standard. Individuals can then "spend down" to these thresholds by subtracting their medical expenditures from their gross income; if they do, Medicaid will pay the remainder of their expenditures.[32] We compute eligibilty for the Medically Needy program by simply comparing income to this somewhat higher threshold

[32]The time frame over which such spend-down occurs varies across the states, and we do not model it.

in states that have the program.

3) *Expansions.* Beginning with the Deficit Reduction Act of 1984 (DEFRA '84), the Federal government began a series of mandates which extended the Medicaid coverage of pregnant women. DEFRA '84 included two features: mandatory coverage of first-time pregnant women under AFDC, if they would be eligible for the program upon the birth of their child, and mandatory coverage of pregnant women in AFDC-UP type families, even if the state did not have an AFDC-UP program. The Consolidated Budget Reconciliation Act of 1985 (COBRA '85) then mandated that pregnant women who met the AFDC resource standards were eligible regardless of family structure (similar to the state programs described above). This law was effective in July, 1986.

Beginning with the Omnibus Budget Reconciliation Act of 1986 (OBRA '86), states were first given the option, and then mandated to, increase the income thresholds for Medicaid eligibility, regardless of family structure. OBRA '86 gave states the option of covering pregnant women up to 100% of the poverty threshold, beginning in April, 1987. OBRA '87 increased that optional level to 185% of poverty. Under the Medicare Catastrophic Care Act states were mandated to cover pregnant women up to 75% of poverty by July 1. Then under OBRA '89, they were required to cover women up to 133% of poverty by April, 1990. We use data from the Intergovernmental Health Policy Project (various years) to model state adoption of eligibilty rules under the expansions of this era. We then compare the woman's gross income, less AFDC disregards,[33] to the expansion income limit to determine eligibility.

[33]Medicaid officials in Massachusetts report that income is considered net of disregards for the expansions.

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Table 1: Variable Means

	All Births	Teens/ Dropouts	Higher Ed
Procedure Utilization - Natality Data (1989-1992)			
Cesarean Section Delivery	0.226	0.185	0.241
Fetal Monitor	0.736	0.700	0.750
Induced/Stimulated Labor	0.209	0.167	0.224
Ultrasound	0.534	0.468	0.552
Insurance Coverage - CPS (1987)			
Non-Medicaid Coverage	0.698	0.313	0.803
Medicaid Coverage	0.145	0.353	0.087
Fetal Health - Natality Data (1987-1992)			
Low Birthweight	0.071	0.094	0.063
Very Low Birthweight	0.013	0.017	0.011
Short Gestation	0.106	0.140	0.095
Very Short Gestation	0.020	0.028	0.017
Neonatal Mortality Rate - Mortality Data (1987-1991)			
Death in 1st Month	0.0059	0.0075	0.0053
Fraction whose Closest Hospital has a NICU - Natality Data (1987-1991)			
Mean	0.336	0.351	0.331
10th Percentile	0.168	0.188	0.164
50th Percentile	0.314	0.341	0.310
90th Percentile	0.520	0.536	0.509

Notes: Means are from data sets and years described in header rows. Low birthweight refers to birthweight less than 2500 grams, while very low birthweight indicates birthweight less than 1500 grams. Short gestation is less than 36 weeks and very short gestation is less than 32 weeks.

Table 2: Medicaid Eligibility and the Treatment of Childbirth: Teens and High School Dropouts

Procedure	Coefficient	Effect of 10% Eligibility Rise
Cesarean Section Delivery	0.296 (0.068)	0.45 (2.40%)
Fetal Monitor	0.745 (0.088)	1.56 (2.23%)
Induction of Labor	0.202 (0.076)	0.28 (1.68%)
Ultrasound	0.349 (0.081)	0.87 (1.85%)

Notes: Each figure in column 1 is from a separate regression of the form (1); The coefficient shown is that on the percent eligible in the state/year/group cell from grouped logits. Standard errors in parentheses. The second column presents the implied percentage point and percentage (in parentheses) effects of a 10 percentage point increase in the fraction eligibile.

Table 3: Medicaid Eligibility and the Treatment of Childbirth: All Others and Total

Procedure	Coefficient	Effect of 10% Eligibility Rise
	All Others	
Cesarean Section Delivery	-0.336 (0.038)	-0.62 (-2.57%)
Fetal Monitor	-0.149 (0.056)	-0.28 (-0.37%)
Induction of Labor	-0.097 (0.044)	-0.17 (-0.75%)
Ultrasound	0.081 (0.052)	0.20 (0.36%)
	All	
Cesarean Section Delivery	-0.115 (0.033)	-0.20 (-0.9%)
Fetal Monitor	0.111 (0.048)	0.22 (0.29%)
Induction of Labor	-0.020 (0.038)	-0.03 (-0.16%)
Ultrasound	0.133 (0.045)	0.33 (0.62%)

Notes: Each row is from a separate regression of the form (1); The coefficient shown is that on percent eligible in the state/year/group cell from grouped logits. Standard errors in parentheses. The second column presents the implied percentage point and percentage (in parentheses) effect of a 10 percentage point increase in the fraction eligible.

Table 4: Medicaid Eligibility and Cesarean Delivery: Interaction with Medicaid Fee Differentials

Group	Coefficients		Effect Size	
	Eligibility	**Eligiliblity* % Fee Diff**	**Eligibility (up 10%)**	**Eligibility* Fee Diff (up 10%)**
Teens/Dropouts	0.230 (0.069)	0.521 (0.124)	0.35 (1.87%)	0.79 (4.25%)
All Others	-0.385 (0.038)	0.611 (0.052)	-0.70 (-2.92%)	1.12 (4.64%)

Note: Each row is from a grouped logit of the form (2) which uses data from the specified group. The coefficients shown in columns 1 and 2 are those on the eligibility main effect, and the interaction between eligibility and fee differential. Standard errors in parentheses. Columns 3 and 4 show the implied percentage point and percentage effects for a 10 percentage point increase in the fraction eligible.

Table 5: Medicaid Eligibility and Neonatal Mortality

Teens and Dropouts

Specification	**Coefficient**	**Effect of 10% Elibility Rise**
No Fetal Health	-0.228 (0.134)	-0.017 (-2.26%)
Fetal Health	-0.168 (0.127)	-0.013 (-1.67%)

All Others

Specification	**Coefficient**	**Effect of 10% Elibility Rise**
No Fetal Health	-0.014 (0.096)	-0.001 (-0.14%)
Fetal Health	-0.031 (0.091)	-0.002 (-0.31%)

Notes: Each row is from a separate grouped logit of the form (1). The first row in each panel shows models that do not control for fetal health, while the second row shows estimates from models that control for the fraction of infants in each cell who were in each of 3 birthweight categories, and for the fraction in each of three gestation categories. The coefficient shown in column 1 is that on the percent eligible in the state/year/group cell. Standard errors in parentheses. The second column presents the implied percentage point and percentage (in parentheses) effects of a 10 percentage point increase in eligibility.

Table 6: Mortality and NICU Access: Teens and Dropouts
All Models are Conditional on Fetal Health

Specification	Eligibility	Closest Hosp. has NICU	Interact.	Eligibility (up 10%)	Distance (share up 10%)	Interact (up 10%)
	Coefficients			**Effect Size**		
Basic Model	-0.014	0.259	-0.522	-0.001	0.019	-0.039
	(0.143)	(0.208)	(0.222)	(-0.14%)	(2.57%)	(-5.19%)
with Zip Controls	-0.087	0.117	-0.418	-0.006	0.009	-0.031
	(0.148)	(0.224)	(0.232)	(-0.86%)	(1.16%)	(-4.15%)

All Others

Specification	Eligibility	Closest Hosp. has NICU	Interact.	Eligibility (up 10%)	Distance (share up 10%)	Interact (up 10%)
	Coefficients			**Effect Size**		
Basic Model	-0.061	-0.187	0.102	-0.003	-0.010	-0.000
	(0.103)	(0.129)	(0.143)	(-0.57%)	(-1.87%)	(-0.03%)
with Zip Controls	-0.085	-0.267	0.144	-0.005	-0.014	0.008
	(0.103)	(0.141)	(0.145)	(-0.85%)	(-2.67%)	(1.51%)

Note: Each row is from a separate regression. The first three columns show estimates of the main effect of Medicaid eligibility, the coefficient on a dummy variable equal to 1 if the closest hospital has a NICU, and the coefficient on the interaction between these two variables. The estimates are from grouped logit models of the form (3); standard errors are in parentheses. The last two columns (labeled "Effect Size") show the implied percentage point and percentage effects for a 10 percentage point increase in the fraction eligible (in the case of eligibility and the interaction between eligibility and distance), and for a 10 percentage point rise in the share of women whose closest hospital has a NICU (for the distance main effect). The second row of each panel shows estimates from models that include controls for other zip code characteristics, as described in the text.

Table A1: Medicaid Eligibility and Insurance Coverage

Group	Medicaid	Other Insurance	Medicaid & Other
All (N=215955)	0.065 (0.013)	-0.112 (0.020)	0.013 (0.020)
Teens/Dropouts (N=51498)	0.109 (0.039)	-0.066 (0.050)	-0.010 (0.020)
All Others (N=164457)	0.056 (0.012)	-0.125 (0.022)	0.016 (0.007)

Notes: These models are estimated using March CPS data from 1987 to 1992. Each cell is from a separate regression. Estimates are from linear probability models of the form (1), and coefficient is that on percent eligible in state/year/group cell. Standard errors in parentheses.