Public Health Insurance and Medical Treatment: The Equalizing Impact of the Medicaid Expansions

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Abstract

We investigate the impact of expanding public health insurance on the medical treatment received by women at childbirth, using Vital Statistics data on every birth in the U.S. over the 1987-1992 period. The effects of insurance status on treatment are identified using the tremendous variation in eligibility for public insurance coverage under the Medicaid program over this period. Among low education mothers 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. 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, moving from insurance which reimburses medical care more generously to insurance with much less generous reimbursement. This movement was accompanied by reductions in procedure use. Thus, on net, the Medicaid expansions had an equalizing effect, increasing the treatment intensity of the previously uninsured while lowering it among the previously insured.

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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 has motivated calls for expanded public insurance as a means of guaranteeing access of the uninsured to high quality medical care. Past research suggests that expansions in public insurance can indeed increase contacts of the uninsured with the medical system. A large number of studies further suggest that those with *private* insurance coverage are treated more intensively than the uninsured. These studies, however, are able to control for differences in the underlying health of insured and uninsured patients in only a limited way. In studies of the effects of insurance coverage on the treatment of hospitalized patients, it is also difficult to control for selection into hospital on the basis of health status. Perhaps for these reasons, the available evidence regarding the effects of *public* insurance coverage on treatment intensity is mixed.

Moreover, no previous work has considered a potentially countervailing effect of public insurance expansions on treatment intensity. Expansions in public insurance can be associated with reduced private insurance coverage among the target population. This "crowdout" of private insurance coverage may lead to *reductions* in treatment intensity, as public coverage generally reimburses providers at a much lower level than does private coverage. Thus, overall, the impacts of public insurance expansions on treatment intensity may be ambiguous.

In this paper, we address both of these issues 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.[1] Among pregnant women, eligibility for Medicaid coverage has greatly expanded since 1987, and this expansion has occurred at a differential pace across the states. These eligibility changes can be used to identify the effect of insurance status on treatment, 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.

We do so using excellent national data on the treatment of childbirth that 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.

We find that recent expansions of the Medicaid program had significant effects on the medical treatment of child birth. We focus first on mothers who are teens, high school dropouts, or unmarried high school graduates, a group that was largely without insurance before becoming eligible for Medicaid. In this group, eligibility expansions increased the generosity of insurance coverage, and we find that increased eligibility was associated with an increase in the utilization of a variety of obstetric procedures.

But we find evidence of a countervailing effect on aggregate procedure utilization among mothers who were married high school graduates or who had some college education. These women

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[1]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.

were much more likely to have had private insurance coverage before becoming eligible for Medicaid, but a large share became eligible for the program. 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 are moving to insurance (Medicaid) which reimburses physicians much less generously 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. We confirm these findings by using data on relative Medicaid reimbursement of Cesarean section delivery to show that the effects are consistent with physician responses to reimbursement differentials. And we show that for college graduate women, for whom the expansions are largely irrelevant, there is little association between Medicaid eligibility and procedure utilization.

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. Conclusions are presented in Part IV.

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 affects patient well-being.[2] 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.[3] 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 is generally that the hospital provides uncompensated care.[4] Second, newly signed-up women may be treated more intensively for their childbirth 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

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

[3]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.

[4]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).

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.

Several 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 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.[5] These selection issues may be the reason that there is only mixed evidence on the implications of public coverage for treatment intensity.

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 over $1200 in 1987, while Medicaid is completely free.[6] It is also possible that employers of low wage employees will cease to

[5] A lack of primary care among the uninsured could also cause them to be in worse health upon admission. Evidence about observable differences in severity upon admission by insurance status is mixed.

[6]Of course, there are some counterbalancing disadvantages of Medicaid: for adult women the only costs covered are those of pregancy; many physicians are reluctant to see Medicaid patients due to low reimbursement rates; and individuals may not be able to move freely back to private insurance if their

offer insurance coverage, given that many of their employees will be eligible for Medicaid coverage of a large portion of their potential medical bills, or that employers may raise employee premiums in an effort to encourage their low wage employees to choose the Medicaid option. Of course, on the other hand, there are a number of barriers to crowdout, not the least of which is the uncertainty of moving from a secure private option to a public option that is subject to political changes and to income changes which move the family out of the eligible income range.

These possibilities have generated a substantial empirical literature on the "crowdout" effects of Medicaid. A large number of studies document that there was some crowdout, although the estimated magnitude of the effect varies. Cutler and Gruber (1996) originally estimated 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%, and their basic conclusions are supported by Currie (1999), Shore-Sheppard (1998) and Rask and Rask (forthcoming). Other studies have estimated smaller effects, on the order of 10-20% (Dubay and Kenney, 1997; Blumberg et al., 2000).[7]

While there has been considerable discussion of the *magnitude* of crowdout, there has been no work on its *implications*. The key insight of early work by Peltzman (1973), which provides the theoretical basis for the work cited above, is that crowdout might conceivably *lower* the overall quality of the services purchased. That is, while public provision can raise the quality of services for those who

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Medicaid eligibility ends. See Cutler and Gruber (1996) for a more detailed discussion of this issue.

[7]Directly comparing these estimates of crowdout is difficult. For example, Cutler and Gruber measure crowdout as the ratio of the Medicaid expansion-induced fall in private coverage relative to the expansion-induced rise in Medicaid coverage; Dubay and Kenney take the ratio of the overall falls in private coverage and rise in Medicaid coverage for populations targeted by expansions.

were not receiving services beforehand, it can also lower the quality of services for those who were privately providing the service beforehand, and who move to the lower quality public service. In Peltzman's example of public education, the implication was that, without the seamless ability to "top off" the public education level, free (moderate quality) public schools could increase the educational investment of those who were poorly educated before, while reducing the educational investment of those who were previously paying for higher-quality private education from their own pockets. This latter group of individuals were "crowded out" into the lower-quality public program; if that group was large enough relative to those who were increasing educational quality, overall quality could fall.

Since Medicaid typically reimburses at about half the rate of private insurers (Currie, Gruber, and Fischer, 1995), women who are "crowded out" onto Medicaid 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. As in the case of education, it is even conceivable in the presence of crowdout that intensity of treatment could actually *fall* from the expansion of public insurance; if the group of women who move from private to public insurance is large enough relative to those moving from uninsured to public insurance, and if public insurance reimbursement is low enough, overall intensity could decline.

Of course, there may be a countervailing effect on the demand side. Since the crowded out 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.[8] Thus, the net impact of crowdout on treatment intensity depends on the relative strengths of supply and demand side incentives. 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, demand side incentives may be equally important.

Moreover, it is important to note that we use the term "quality" here interchangeably with "treatment intensity". If, in fact, reduced treatment intensity due to lower physician reimbursement among a crowded out population simply lowers excess medical care, the overall quality of treatment may be unchanged. We discuss the "quality" implications of the procedures that we examine below. But it is important to recognize that without actual data on health outcomes, the implications of our findings for true "quality" of care are ambiguous.

Three previous studies are related to ours. Currie and Gruber (1996a) examine the impact of the Medicaid expansions on health outcomes using aggregate state-level data on eligibility and outcomes. They find significant declines in infant mortality associated with the very targeted Medicaid expansions of the early 1980s, but no effect of the much larger post-1987 expansions that are the subject of the current paper. However, they do not examine the impact of the expansions on actual procedure utilization, and they suggest that much of the improvement in infant health found in the earlier period may have arisen from increased use of early pre-natal care.

The most closely related effort is Hass *et al*. (1993) who show that expansions of Medicaid in

[8]This may also be a positive supply side response to reduced sharing; if physicians know that women now do not face costs of the services, they may be more likely to recommend or perform them.

Massachusetts in the mid-1980s were associated with increases in the rate of cesarean section delivery among the previously uninsured. While suggestive, the case study methodology does not allow the authors to control for any underlying time series trends in the rate of procedure use in the low income population that might confound the analysis. By considering differential changes in eligibility across states, controlling for time series trends, and using a broader variety of obstetric procedures, we are able to make more general statements about the evidence.

Finally, Gruber, Kim, and Mayzlin (forthcoming) examine the impact of Medicaid fee differentials on cesarean section delivery rates. They find that increases in the reimbursement for cesarean section delivery under Medicaid are associated with increased rates of cesarean delivery, and that the lower fees paid by Medicaid can explain over one-half of the lower cesarean delivery rate in that population. This motivates our contention that the "crowdout" effect of Medicaid may cause some low income mothers to see reductions in their treatment intensity. However, this article does not examine the impact of coverage on treatment intensity, nor the separate impact of fees on mothers who were moving from uninsured to Medicaid, as opposed to moving from private insurance to Medicaid. We take up both of those issues below.

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 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. 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.[9] 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

[9]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.

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 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.[10] Thus, changes in eligibility were largely exogenous from the point of view of state governments as well as individual mothers.

Part II: Data and Empirical Strategy

a) Data

Our primary data sources are the 1989 to 1992 Detail Natality 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.

We focus on four obstetric procedures that are available on the birth certificate: 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

[10]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.

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 Neonatal Intensive Care Unit (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). As a result, the “quality” implications of changes in these treatment are unclear.

Nevertheless, the procedures listed on birth certificates are of interest for two reasons. First, because child birth is a frequent 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

[11]Cesarean delivery is obviously done in the hospital at the point of birth, as are inducement and stimulation 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.

[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

general propensity to use both low tech and "higher tech" procedures during and after the birth.

Use of these procedures will obviously be highly correlated with risk factors present at birth. The birth certificate data that we use include data on a variety of risk factors.[13] We include in our regressions a variable for the number of risk factors present at delivery, on average for each cell in our data; the mean of this variable is 0.28, since most women have no risk factors.

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 a regressor of interest from a policy perspective, since this is a primary tool that is available to policy-makers. Thus, our empirical work will ask: How does the treatment of childbirth in a given population change when eligibility for public insurance expands?

Conditional on gaining eligibility, 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

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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).

[13]The risk factors included on the birth certificate are: Anemia, cardiac disease, acute or chronic lung disease, diabetes, genital herpes, hydramnios/oligohydramnios, hemoglobinopathy, chronic hypertension, pregnancy associated hypertension, eclampsia, incompetent cervix, previous infant 4000+ grams, pervious preterm or small-for-gestational age infant, renal disease, rh sensitization, uterine bleeding, and a category called "other maternal risk factors".

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. This indicator divides our sample into two groups: "coverage gain" mothers who are teens (less than 19 years old), high school dropouts, or unmarried high school graduate; and (potential) "crowdout" mothers who are married high school graduates or women with some college education (but not a college degree). 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 the 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. We examine the coverage of this group by Medicaid and by private insurance.[14]

It is clear that these two groups were quite distinct in terms of their probability of having private

[14]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. We define Medicaid and private insurance exclusively, excluding those who report being covered by both

health insurance coverage. Among the coverage gain group, fewer than 30% of women were covered by non-Medicaid insurance, so that there was little scope for crowdout. On the other hand, almost 80% of the crowdout group was covered by private 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 from those likely to be switching insurance coverage. Moreover, among this crowdout group, the Medicaid coverage rate rose from 5.5 to 13.6% by 1992, suggesting that the possibility of switching to Medicaid was a real one over this period.

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 20% lower in the coverage gain 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 crowdout 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.

c) Empirical Strategy

The goal of our empirical analysis is to examine the effect of public insurance eligibility on the treatment of childbirth. 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 the

Appendix 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.

More specifically, we proceed as follows. First, 66 age/race/education/marital status 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 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 66 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/marital status 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.

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[15]There are 22 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, 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). The upper three education groups are then subdivided further by marital status. 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.

Note that using a nationally representative population (rather than a state-specific sample) is the only feasible means of imputing eligibility to 66 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, 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, and we have a total of 12,525 cells. The fraction eligible in the median cell is .40, while in the coverage gain and crowdout groups the comparable figures are .67 and .33. All our regression models are weighted by cell size.

The models estimated are of the following form:

$$(1)\quad PROC_{jtraem} = a + ß_1 ELIG_{jtraem} + ß_2 RISK_{jtraem} + ß_3 s_{raem} + ß_4 d_j + ß_5 t_t + ß_6 d_j * p_r + ß_7 t_t * p_r + ß_8 d_j * ?_e + ß_9 t_t * ?_e + ß_{10} d_j * t_t + e_{jtraem}$$

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/marital status cell, RISK is the average number of risk factors in each cell, and s_{rae}, d_j, t_t, p_r, and $?_e$ are full sets of dummy variables for the 66 race/age/education/marital status 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 procedure use, so fixed effects are included for both states and years.

In addition, average procedure utilization rates differ substantially by race, and by education: for example, in 1991, the rate of cesarean section delivery was 17.5% among black teens and dropouts, 19.1% among white teens and dropouts, and 24% among other white women. Similarly, the incidence of labor being induced or stimulated was 15.5%, 17.7% and 24% among these three groups. 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 identification 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.

Part III: Effects on Procedure Use

Coverage Gain Group

The first column of Table 2 shows the coefficients on the fraction eligible from estimates of model (1), for the coverage gain group. This is the group likely to be uninsured before being made

[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).

Medicaid eligible, so we expect that there will be a positive impact on their procedure utilization. Each row of the Table shows the coefficient of interest ($ß_1$) 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 0.32 percentage point increase in the rate of cesarean section delivery, which is 1.6% of the baseline rate for this group. The percentage effects are fairly similar across these different procedures, ranging from 1.1% to 1.6%. The uniformity of these results suggest a shift to a generally more aggressive treatment regime as these teen/dropout mothers are made eligible for Medicaid. These findings provide strong confirmation that more generous insurance coverage for the uninsured will increase treatment intensity.

Crowdout Group

If we wish to examine the effects of Medicaid policy on the overall utilization of care, it is important to remember that the coverage gain mothers were not the only ones potentially affected by the expansions. As noted above, among the crowdout 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 the crowdout sample are presented in Table 3. In this sample the estimated effects of Medicaid are indeed negative for all four procedures, and significant in three of the four cases. For cesarean delivery, the negative effect is substantially larger than the positive effect on the coverage gain group discussed above; we discuss below why it might be natural in this case for the negative impact on the formerly privately insured to be larger than the positive impact on the formerly uninsured. For induction of labor, the negative effects here are comparable to the positive effects on the coverage gain group, and for fetal monitors and ultrasound, they are smaller. The insignificant impact on ultrasounds may reflect the potentially countervailing demand side effects of crowdout noted above: for this elective treatment that is occurring in advance of childbirth, reduced cost sharing for mothers may be offsetting reduced reimbursement levels for physicians. Overall, 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.

We conclude that insurance status matters for treatment, but not necessarily in the manner anticipated by advocates of expanding public insurance. Table 2 suggests that among the coverage gain group, increases in expected provider reimbursement as a result of Medicaid eligibility led to an increase in overall treatment intensity. Table 3, however, suggests that among the crowdout group, there is a reduction in the rate of procedure use. 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. We discuss this issue further in the conclusion.

Part IV: Confirmation

The evidence in Tables 2 and 3 strongly suggests that insurance coverage matters for the treatment of childbirth. This finding emerges from a careful empirical specification which attempts in a variety of ways to overcome the potential biases associated with an exercise of this nature. Moreover, the fact that we find opposite signed results for our two groups of women provides suggestive evidence that our results are real and not driven by underlying related trends in the data. Nevertheless, it is important to confirm that this finding reflects the changes in provider reimbursement associated with changes in insurance status among the newly eligible population, and not other confounding trends. In this section, we attempt to do so in two ways.

Control Group

One approach to controlling for confounding trends is to find a control group which is unlikely to be either gaining or losing insurance coverage as a result of the Medicaid expansions. College graduates, who have not been used in the analysis thus far, provide such a control. There is a small eligibility increase for college graduates over this time period, but there is essentially no change in Medicaid coverage; even by 1992, only 2% of those women with children less than one with college degrees in the CPS had Medicaid coverage. This suggests that, if our story is valid, we should see no impact at all of Medicaid expansions on college graduates in our data.

As Table 4 shows, this is indeed the case. For each variable studied, there is an insignificant (or marginally significant, in the case of induction of labor) relationship between Medicaid eligibility and treatment intensity for this college graduate population. The magnitudes in this table are all much smaller

than in Table 3 or 4. This suggests the absence of any underlying confounding factors in our empirical design.

Reimbursement Differentials

The most direct means of assessing our explanation is to assess whether the effects of Medicaid eligibility that we estimate vary with the financial incentives offered by the Medicaid program for intensive treatment. Contrast two states, one which has very low Medicaid reimbursement, and one which reimburses at private insurance levels. In the first of these states, we should see relatively small positive impact of Medicaid eligibility on the treatment of the formerly uninsured, since there is little reimbursement to be gained from their movement to Medicaid coverage; but there should be a large negative impact on the treatment of the formerly privately insured, since reimbursement is falling substantially. Likewise, in the second of these states, we should see large positive impacts on the treatment of the formerly uninsured, and relatively small negative impacts on the formerly privately insured (since there is little change in net reimbursement). On the other hand, if our results do not reflect the dynamics of reimbursement differentials, then this pattern across states may not emerge. Thus, in this section, we use data on relative Medicaid reimbursement across states to carry out a comparison of this nature, as a means of confirming our conclusions.

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.[18] 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 an average of $635 for a vaginal delivery and $127 more for a cesarean.[19] 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 (e.g. this is more like the first of the states in the example above).

The difference between Medicaid reimbursements for vaginal and cesarean deliveries varies 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:

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[18]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.

[19]All national averages in this discussion are weighted by number of births in that state and year.

$$(2) \quad CS_{jtrae} = a + ß_1 ELIG_{jtraem} + ß_2 ELIG_{jtraem} * DIFF_{jt} + ß_3 RISK_{jtraem} + ß_4 s_{rae,m} + ß_5 d_j + ß_6 t_t +$$

$$ß_7 d_j * p_r + ß_8 t_t * p_r + ß_9 d_j * ?_e + ß_{10} t_t * ?_e + ß_{11} d_j * t_t + e_{jtraem}$$

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, $ß_1$, will remain positive/negative for the coverage gain/crowdout 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 $ß_3$ 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 coverage gain mothers, higher fees increase the positive incentive to perform cesareans on women who have become eligible for the Medicaid program. For crowdout mothers, higher physician reimbursement reduces the negative effect of 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.

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[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.

Estimates of these interactions are shown in Table 5; we show only the coefficients of interest, $ß_1$ and $ß_3$. Among the coverage gain group, the main effect remains positive as expected, although it is now insignificant. 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 0.13 percentage points (as compared to the 0.32 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 0.69 percentage points, or 3.5% of the baseline rate. That is, moving to a higher level of Medicaid reimbursement deepens the intensity-increasing impact of Medicaid eligibility on the formerly uninsured, as per the discussion above.

For crowdout 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 0.6 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 0.22 percentage points, or 0.9% of the baseline rate in this population That is, a higher level of Medicaid reimbursement weakens the intensity-decreasing impact of Medicaid eligibility on the formerly privately insured, confirming that our negative findings for this group in Table 3 were driven by reimbursement differentials between Medicaid and the private

sector.

These findings suggest a powerful role for provider financial incentives in determining the treatment of childbirth. More importantly for our purposes, they confirm the implications of our overall finding on treatment intensity. Among those coverage gain mothers likely moving from uninsured to Medicaid, there is a fee increase, so that there is more intensive treatment; as this fee increase becomes larger, the intensity increment from Medicaid rises. Among those crowdout mothers likely to be moving from private insurance to Medicaid, there is a fee decrease, so that treatment intensity falls; as Medicaid reimbursement rises and the fee differential is reduced, treatment intensity falls less.

The results in Table 5 also rule out two alternative explanations for our findings on treatment intensity for the advantaged group. The first is that our findings are due to 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 crowdout 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 crowdout mothers.

The second is that our findings reflect short-run capacity constraints. If, for example, there were somehow constraints on the capacity of hospitals and/or physicians to provide intensive treatment (e.g. a limited number of fetal monitors), then we could find that when care expands to the coverage gain group, care to other mothers fall through supply-side constraints. This is unlikely to be a very

powerful effect for the procedures which we examine, since they are not very capital intensive. Moreover, this alternative is inconsistent with our fee interaction pattern as well. In states where the fees are the highest, there is the largest increases in treatment of the previously uninsured; as a result, if capacity constraints were driving our findings, those states would be the ones where care to the previously insured would fall the most as well. In fact, we see the opposite: high fee states see the smallest reduction in care to the crowdout mothers, not the largest.

Part V: Conclusions

This study offers evidence that insurance affects the way patients are treated, in both expected and unexpected ways. Among teen mothers, high school dropouts, and unmarried high school graduates 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 an 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. Thus, we extend the results of previous studies that suggested that insurance increases treatment intensity relative to no insurance, but in a more statistically convincing framework.

As we have highlighted, however, there is a countervailing effect on procedure use among higher education 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. These reductions are mitigated as Medicaid reimbursement for more intensive treatment rises, confirming that our findings are driven by the reimbursement change induced by moving from private to public coverage.

These results have important implications for measuring the social costs of expanding eligibility for public health insurance. The key issue in assessing these implications is the value of the marginal treatments that are consumed or foregone as a result of changes in eligibility for public insurance. At one extreme, it is possible that marginal changes in treatment are irrelevant for both populations, since the truly medically necessary treatment is provided by responsible providers in any case. In terms of the medical effectiveness curve which plots health production against spending, in this case we would be on the proverbial "flat of the curve", and these results would suggest that there is little aggregate impact of the change in treatment intensity associated with the expansions, since overall treatment intensity is not much changing.

Another possibility is that the changes in marginal treatment are important for health outcomes for both populations. In this case, there is a tradeoff associated with the public health insurance expansions; more valuable health care for the uninsured, but less valuable health care for the previously insured. Such a tradeoff could nevertheless be welfare improving, under either of two conditions. First, if there is some concavity to the relationship between medical spending and health production, then there are larger marginal gains to increases in treatment for the previously uninsured than for decreases in treatment for the previously insured. Second, if the social welfare function is concave, this policy would amount to redistributing resources from higher income to lower income groups.

Finally, the "best case" scenario would be that the marginal increases in treatment for the

previously uninsured are valuable in terms of health, but that the reductions in treatment for the previously uninsured are not; that is, that the previously uninsured are on an upward sloping portion of the medical effectiveness curve, and the previously insured are on a flat portion.[21] In this case, there are two types of welfare gains from the policy: improvements in the health of the uninsured, and reductions in wasteful treatment of the insured.[22] Of course, there is a corresponding "worst case" scenario that is the opposite, but this seems unlikely to be relevant; that is, it seems unlikely that the medical effectiveness curve is downward sloping over the relevant range.

Ultimately, which of these scenarios is most relevant is an empirical question. We investigate this question in Currie and Gruber (1997), with mixed results. We examine the impacts of the Medicaid expansions on the infant mortality outcomes of these two groups of mothers. We find some suggestive evidence for the "best case" of mortality reductions for the teen/dropout mothers and no effect on the higher education mothers, but the results are statistically imprecise. We do find, however, that those teen/dropout mothers who live near a Neonatal Intensive Care Unit see more significant reductions in mortality associated with the expansions. This suggests that the expansions may have increased access to this "higher tech" source of treatment of childbirth, as well as to the "lower tech" treatments that we study. At the same time, we continue to find little impact of the expansions on the

[21]This would be consistent with health economics research which both finds important effects of insurance coverage on health (e.g. Lurie et al., 1984; Currie and Gruber (1996a) , and which finds little impact of exogenous reductions in the insurance generosity of the insured (Manning et. al., 1987).

[22]Of course, there is a question of why the insured were purchasing this unproductive health care to start with. But a large literature in health economics suggests that employed individuals (who, and whose dependents, represent over 90% of the privately insured) may be "overinsured" because of the tax subsidization of employer-provided health insurance (e.g. Pauly, 1986). One element of this overinsurance may be reimbursement for excessively intensive medical treatment.

mortality of children born to mothers with more education, regardless of proximity to a NICU.

These results suggest 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: 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 modeled 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.[23] We compute eligibility for the Medically Needy program by simply comparing income to this somewhat higher threshold 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,[24] to the expansion income limit to determine eligibility.

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

[24]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	Low Education	Crowdout
Procedure Utilization - Natality Data			
Cesarean Section Delivery	0.226	0.196	0.246
Fetal Monitor	0.736	0.702	0.752
Induced/Stimulated Labor	0.209	0.171	0.224
Ultrasound	0.534	0.478	0.559
Insurance Coverage - CPS (1987)			
Non-Medicaid Coverage	0.665	0.284	0.792
Medicaid Coverage	0.151	0.406	0.055

Notes: Means are from data sets and years described in header rows.

Table 2: Medicaid Eligibility and the Treatment of Childbirth: Coverage Gain Group

Procedure	Coefficient	Effect of 10% Eligibility Rise
Cesarean Section Delivery (N=3998)	0.200 (0.055)	0.32 (1.61%)
Fetal Monitor (N=4028)	0.441 (0.070)	0.92 (1.31%)
Induction of Labor (N=4067)	0.136 (0.062)	0.19 (1.12%)
Ultrasound (N=3965)	0.221 (0.065)	0.55 (1.15%)

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: Crowdout Group

Procedure	Coefficient	Effect of 10% Eligibility Rise
Cesarean Section Delivery (N=5215)	-0.500 (0.040)	-0.79 (-3.19%)
Fetal Monitor (N=5228)	-0.284 (0.053)	-0.53 (-0.70%)
Induction of Labor (N=5228)	-0.172 (0.042)	-0.30 (-1.33%)
Ultrasound (N=5147)	-0.084 (0.050)	-0.21 (-0.37%)

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 the Treatment of Childbirth: Control Group

Procedure	Coefficient	Effect of 10% Eligibility Rise
Cesarean Section Delivery (N=3252)	-0.072 (0.047)	-0.13 (-0.54%)
Fetal Monitor (N=3265)	-0.009 (0.062)	0.002 (0.002%)
Induction of Labor (N=3265)	-0.095 (0.051)	-0.17 (-0.72%)
Ultrasound (N=3211)	0.015 (0.056)	0.04 (0.07%)

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 5: Medicaid Eligibility and Cesarean Delivery: Interaction with Medicaid Fee Differentials

	Coefficients		Effect Size	
Group	**Eligibility**	**Eligiliblity* % Fee Diff**	**Eligibility (up 10%)**	**Eligibility* Fee Diff (up 10%)**
Coverage Gain (N=3664)	0.085 (0.060)	0.942 (0.158)	0.13 (0.68%)	1.48 (7.57%)
Crowdout (N=4687)	-0.313 (0.050)	0.502 (0.077)	-0.58 (-2.36%)	0.93 (3.78%)

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.