

Is Medicaid Pronatalist? The Effect of Eligibility Expansions on Abortions and Births

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Context: *Income thresholds for Medicaid eligibility for pregnant women were raised in two phases between 1987 and 1991. During roughly the same period, the U.S. fertility rate rose and the abortion rate declined; changes were particularly marked among young women, raising the possibility that fertility increases were related to Medicaid expansions.*

Methods: *Pooled time-series cross-section regressions were used to examine the effects of the Medicaid eligibility expansions in 15 states on rates of abortions and births among unmarried women aged 19–27 with 12 or fewer years of schooling. Abortion data came from the National Center for Health Statistics or state health departments and were aggregated by women's age, race, marital status and schooling; data on births were from national natality tapes.*

Results: *The Medicaid expansions were associated with a 5% increase in the birthrate among white women, but did not influence the rate among black women. Overall, no effect on the abortion rate was evident, but in analyses restricted to a subsample of eight states with the most complete abortion data, the rate among white women showed a significant decline after the second phase of expansions.*

Conclusions: *Subsidized health care for low-income pregnant women in these 15 states may have encouraged white women to have more children than they would have without coverage.*

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Through the mid-1980s, Medicaid eligibility requirements included very low income thresholds established by the individual states. Between 1987 and 1991, eligibility standards for poor and near-poor pregnant women were expanded dramatically. The 1986 Omnibus Budget and Reconciliation Act (OBRA) permitted states to extend eligibility (and receive reimbursement for services provided) to individuals with an income up to 100% of the federal poverty level. The 1987 OBRA permitted states to raise the threshold to 185% of the poverty level, and the 1989 OBRA mandated increases to 133% of poverty.¹ As a result, the proportion of births financed by Medicaid rose from 15% in 1985 to 32% in 1991² and to 39% in 1994.³ Furthermore, the number of children enrolled in Medicaid increased by 47% between 1989 and 1992.⁴

Coincident with these eligibility expansions, the U.S. fertility rate rose by 7%, from a recent trough of 65.4 births per 1,000 women aged 15–44 in 1986 to 69.6 per 1,000 in 1991. Over roughly the same

period, the abortion rate fell from 28.0 abortions per 1,000 women aged 15–44 in 1985 to 25.9 per 1,000 in 1992.⁵ Among subgroups, changes were more dramatic. The fertility rate of unmarried women aged 20–24 rose from 46.5 to 68.0 births per 1,000 between 1985 and 1991,⁶ and the abortion rate for teenagers fell from 43.5 to 37.7 per 1,000 over the same period.⁷

In this article, we examine trends in the rates of births and abortions between 1986 and 1992, and investigate whether changes were related to increases in Medicaid eligibility thresholds for pregnant women. Specifically, we examine variations in the timing and magnitude of state expansions in Medicaid eligibility to assess whether publicly provided health insurance for pregnancy, delivery and postpartum care was associated with changes in rates of births and abortions in 15 states.

Related Literature

Medicaid eligibility lowers the costs to parents of prenatal, delivery and postpartum care. A simple economic model of

fertility suggests that the effect of such a change on fertility is ambiguous: Parents may have more children, or they may devote more resources to the health and well-being of the children they had already planned to have.⁸

This ambiguity is evident in results from several studies that have investigated effects on births of higher payments through Aid to Families with Dependent Children (AFDC). These studies are relevant because, as with Medicaid, higher AFDC benefits reduce the costs of having children and may affect a couple's choice of contraceptives. Several investigators have found no effects of AFDC on fertility.⁹ Others have found that higher AFDC benefits increase the probability of a premarital birth among women between the ages of 14 and 23.¹⁰ The size of the effect was relatively large: A 20% rise in AFDC benefits increased the probability of a birth by 33%.

The same investigators also found a relatively small effect of AFDC on abortion: A 20% increase in AFDC benefits reduced the probability of abortion by 9%.¹¹ Other researchers also have reported that AFDC benefits were negatively related to the probability of abortion.¹² In addition, higher AFDC benefits have been associated with a reduction in nonmarital fertility for women aged 15–19, but not for older women.¹³ Finally, one analysis suggested that a 10% increase in AFDC benefits re-

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duced fertility by approximately 2% among women aged 16–24. Unexpectedly, however, the effect was greater on marital than on nonmarital fertility.¹⁴

Only two studies that we are aware of have analyzed the effect of expansions in Medicaid eligibility on fertility. One, using individual-level data on births and abortions in three southern states, found that the expansions reduced the probability of abortion among pregnant, unmarried, white women with less than a high school education by 2–5 percentage points, or 13–24%.¹⁵ What was not assessed was whether the change resulted from a fall in abortions, a rise in births or both.

The other analysis, based on data from the Current Population Survey (CPS) for 1989–1992, revealed that among women aged 15–44, the expansions were associated with a statistically significant 0.3% increase in the probability of a birth, which is equivalent to a 5% increase in the birthrate.¹⁶ Since the CPS is a national data set containing information on the ages of each child in the family, the researcher was able to assess whether expansions in the provision of publicly provided health insurance for infants and children affected fertility.

However, the study had a number of drawbacks: The CPS provides lower estimates of births than vital statistics indicate, and although agreement is almost complete for births among women aged 25 and older, the degree of underreporting among women aged 18–24 (who are disproportionately affected by the Medicaid eligibility expansions) is unclear. Furthermore, the analyst made no attempt to define relevant “treatment” and “control” groups. Thus, the 5% increase in the birthrate was driven by the relatively small proportion of women affected by the expansion. Finally, the investigator found that the association between the expansions and fertility was related to the subsidy for children, not to the subsidy for infants; this result seems anomalous, given the comparative values of these subsidies (in 1991 dollars, \$743 and \$5,522, respectively).

In sum, evidence on the effects of AFDC payments on fertility is mixed, which is not inconsistent with an economic model of fertility. One problem with all of these studies may be the lack of sufficient “experimental” variation in AFDC funding. Most research has examined state variation in benefit levels, but these levels may be causally linked to state attitudes regarding nonmarital fertility. Only two studies have examined effects of the Medicaid eligibility expansions,¹⁷ which are more likely independent of state attitudes,

since the states must follow government mandates in setting eligibility thresholds. Despite their limitations, both offer evidence consistent with the interpretation that subsidies for infant and child health care are pronatalist.

Our study differs from previous work in a number of important ways. We use a larger sample of states, and we analyze abortion rates and birthrates separately. Thus, we are able to examine whether changes in fertility were the result of changes in the probability of carrying a pregnancy to term or changes in the pregnancy rate.

Additionally, we use vital statistics to measure births, and we stratify the sample by age, race, marital status and schooling. Most previous work on abortion has relied on aggregate data from The Alan Guttmacher Institute (AGI) provider survey. Although the AGI data are regarded as the most accurate count of total abortions, they include few covariates by which to stratify analyses. Stratification allows us to examine effects of the Medicaid expansions on groups most likely to be affected by the policy; this, in turn, increases our ability to detect statistically significant effects.

Methodology

Data

Between 1986 and 1992, the National Center for Health Statistics (NCHS) maintained a data file on induced terminations and pregnancies for 15 states,* including individual-level records on the date and place the abortion occurred, the length of gestation, and the woman’s state of residence, age, race, marital status and completed schooling. For our analyses, we eliminated Nebraska and New York from the NCHS sample because information on schooling and marital status was missing; we added Georgia and Mississippi, whose state offices of vital statistics were able to provide information that was comparable to the NCHS data.

Data on abortions by women’s state of residence were available, but abortions obtained outside a woman’s home state were poorly reported. Therefore, we used data

Table 1. Number of abortions, by state of occurrence, as reported by The Alan Guttmacher Institute (AGI) and the National Center for Health Statistics (NCHS), and percentage difference in numbers reported, 15 states, 1987 and 1992

State	1987			1992		
	AGI	NCHS	% difference	AGI	NCHS	% difference
Total	214,420	192,733	-10.1	212,690	183,792	-13.6
Colorado	18,850	12,232	-35.1	19,880	10,607	-46.6
Georgia	36,030	35,613 [†]	-1.2	39,680	38,052 [†]	-4.1
Indiana	14,750	13,876	-5.9	15,840	12,975	-18.1
Kansas	11,430	5,941	-48.0	12,570	10,385	-17.4
Maine	4,950	4,767	-3.7	4,200	3,210	-23.6
Missouri	20,190	17,574	-13.0	13,510	13,390	-0.9
Mississippi	5,430	5,469 [†]	0.7	7,550	7,555 [†]	0.1
Montana	3,280	3,175	-3.2	3,300	2,869	-13.1
Oregon	14,370	11,147	-22.4	16,060	12,684	-21.0
R.I.	7,390	7,529	1.9	6,990	6,667	-4.6
S.C.	12,770	12,992	1.7	12,190	11,008	-9.7
Tennessee	22,050	21,622	-1.9	19,060	18,029	-5.4
Utah	4,830	4,556	-5.7	3,940	3,942	0.1
Vermont	3,690	3,310	-10.3	2,900	2,778	-4.2
Virginia	34,410	32,930	-4.3	35,020	29,641	-15.4

[†]Data are from the state health department. Note: The percentage difference is the NCHS number minus the AGI number divided by the AGI number and multiplied by 100. Sources: AGI—for 1987, Henshaw SK and Van Vort J, 1992, reference 5; for 1992, Henshaw SK and Van Vort J, 1994, reference 5; NCHS—special tabulations of NCHS tapes.

on abortions by state of occurrence. This is not a serious limitation, because Medicaid income eligibility thresholds do not affect the relative price of in-state and out-of-state abortions, and thus women have no incentive to seek an abortion in another state as a result of a change in eligibility thresholds. (By contrast, women may travel out of state for abortions to circumvent mandatory delay laws or parental notification statutes for minors.¹⁸)

Nevertheless, policies in neighboring states could have affected the number of abortions occurring in the states in our sample. For example, if a neighboring state expanded Medicaid eligibility and, as a result, resident women who might otherwise have traveled out of state for an abortion gave birth instead, the number of abortions occurring in the sample state would have declined. The bias should not be large, however: It is the product of the marginal effect of the Medicaid expansions on abortion rates and the proportion of abortions in sample states that were obtained by non-residents, which exceeded 20% in only two states (Kansas and Rhode Island).¹⁹

The total number of abortions by state of occurrence as reported by NCHS and state vital statistics compare favorably with totals provided by AGI. Overall, the NCHS total was 10% lower than the AGI total for 1987 and 14% lower for 1992 (Table 1). The difference was unusually high (more than 25%) in only two states

*Colorado, Indiana, Kansas, Maine, Missouri, Montana, Nebraska, New York, Oregon, Rhode Island, South Carolina, Tennessee, Utah, Virginia and Vermont.

in 1987 and one state in 1992.

The proportion of abortion records missing information on race,* marital status or education varied by state and year. In general, for each year, fewer than 4% of records were missing data on race (except in Colorado and Montana, where the proportion was 7–32%) or marital status (except in Maine, Montana and Virginia, where it ranged from 2% to 9%). Schooling was less well reported. In most states, the proportion of records that were missing information rarely exceeded 10% in any year, but in Colorado, Mississippi and Rhode Island before 1989, it was 25–60%.

Clearly, if the proportion of records missing data varied over the study period, this is a potential source of bias. Our strategy was to preserve the largest sample possible, and thus we used all states in the basic analyses. We also conducted analyses using only states for which fewer than 10% of records were missing information on race, marital status and schooling. For whites, this subsample consisted of Georgia, Kansas, Montana, South Carolina, Tennessee, Utah, Vermont and Virginia; for blacks, it was Georgia, Kansas, South Carolina, Tennessee and Virginia.

Data on births came from the 1986–1992 National Natality Files, which contain the same obstetric and demographic information as the abortion files. Race, marital status and educational attainment were well reported for the natality data in all 15 states. Fewer than 1% of birth records were missing data on race or marital status, and fewer than 3% were missing data on mother's educational attainment.

We calculated state-specific rates of abortions and births using population data from the 5% Public Use Micro Sample of the 1990 census. We calculated separate population estimates by each demographic characteristic. For years other than 1990, we used the appropriate birth cohort and the 1990 race- and age-specific distribution of women by marital status and education. For example, to calculate the number of unmarried 19-year-old black women with 12 years of education in 1986, we multiplied the total number of 23-year-old black women in 1990 by the proportion of unmarried 19-year-old black women with

12 years of education in 1990.[†] To align births and abortions to the same cross section of women, we lagged population figures one quarter of a year for abortions and three quarters of a year for births.

A drawback to our population projections is that if marriage rates fell between 1986 and 1990, the result could be an overestimate of the number of unmarried women prior to 1990 and an underestimate after 1990. Census data strongly suggest that marriage rates were falling during the study period: The proportion of ever-married women 20–24 years of age fell from 52% to 41% among whites and from 33% to 24% among blacks between 1980 and 1990.²⁰ We employed several strategies to adjust for what is likely an inflated rate of growth in our rates of births and abortions among unmarried women between 1986 and 1990.

We limited the analysis to unmarried women aged 19–27 with 12 or fewer years of completed schooling. The objective was to define a sample of women who were most likely to be affected by expansions in Medicaid eligibility up to 185% of the federal poverty level. Clearly, eligibility approximated by demographic characteristics is a crude proxy for eligibility based on individual information on family income. Nevertheless, according to tabulations from the 1990 census data, such stratification appears to capture a large portion of eligible women. For example, among black women in our sample, 47% with at least one child had a family income that was less than 76% of the federal poverty level, and 26% had a family income that was 76–185% of the federal poverty level; 47% were on public assistance. For white women, the proportions were 32%, 30% and 28%, respectively.

Statistical Techniques

We used pooled time-series cross-section regressions to assess the association between rates of births and abortions and the expansions in Medicaid eligibility. The dependent variable was the natural logarithm of the state-specific annualized quarterly birthrate or abortion rate (i.e., the rate for one-quarter of a year multiplied by four). Since the dependent variable was an aggregation of a binary outcome, we used minimum chi-square methods with appropriate weights.²¹ All regressions were run separately by race; analyses of the black sample excluded Maine, Montana, Utah and Vermont, because the black populations of these states are too small to permit reliable estimates of race-specific rates. Therefore, the analyses were based on 420

observations for whites (multiplying seven years by four quarters by 15 states) and 308 observations for blacks.

We used two indicator variables to assess the effect of the expansions. The first was coded one if the state raised the threshold in the first phase of the expansions, when the 1986 OBRA extended federal reimbursement for services provided to Medicaid enrollees with a family income equal to the poverty level. The second was coded one if the state expanded eligibility to 101–185% of the poverty level in the second phase, following the 1987 and 1989 OBRAs. In the regressions, we lagged the expansion indicators by one quarter for abortions and two quarters for births, because we did not expect an expansion in Medicaid eligibility to affect abortions or deliveries immediately.

Regressions also included indicator variables for each state and each year and a set of quarterly dummies to control for seasonality. State indicators controlled for differences across states that were time-invariant; year indicators controlled for general trends in births and abortions that were the same across all states. This is a standard specification in a “fixed-effects” regression.²²

The estimated effects of the Medicaid expansions on rates of births and abortions represent a weighted average of changes *within* each state after each phase of the expansions, relative to the period before any expansion.²³ Put differently, we used rates of births and abortions among women in each state before the expansion as “controls” for rates within each state after the expansion. Even though we included controls for national trends in rates of births and abortions, our research strategy was vulnerable to confounding by state-specific trends that were unrelated to the expansions. As a further control, therefore, we added to each regression a linear trend term that we interacted with each state indicator; these controls adjust for state-specific trends in births and abortions.

We also performed analyses using the natural logarithm of the abortion *ratio* (the number of abortions divided by the sum of live births and abortions) as the dependent variable. The advantage of these analyses was the absence of a population measure. They also replicated previous work,²⁴ but with a larger sample of states. In addition, we used the natural logarithm of the *number* of births and abortions as dependent variables in calculations including state-specific linear trends. Again, the purpose was to carry out analyses that did not use population estimates. As long as the natural log-

*We use two racial categories, black and nonblack. Since most women in the latter category were white, we refer to this group as white women.

[†]We ignored the effects of mortality and migration because we had no means of estimating such changes for the subpopulations in our analysis. We performed several calculations without an explicit estimate of the population to assess how sensitive the results might be to our measure of the population.

Table 2. Family income, as a percentage of the federal poverty level, qualifying a pregnant woman for Medicaid before eligibility expansions and after two phases of expansions, by state, 1987–1991

State	Before expansion [†]	First phase		Second phase	
		%	Year	%	Year
Colorado	55	75	1989	133	1990
Georgia	45	100	1989	133	1990
Indiana	34	50, 1988,		133, 1990,	
		100, 1989,		150, 1991	
Kansas	61	100	1988	150	1989
Maine	71	‡		185, 1988	
Missouri	37	100	1988	133	1990
Mississippi	48	100	1987	133	1990
Montana	53	100	1989	133	1990
Oregon	70	85, 1988,		85, 1989,	
		100, 1988,		133, 1990	
R.I.	83	100	1987	185	1988
S.C.	50	100	1988	185	1989
Tennessee	45	100	1987	150, 1990,	
				185, 1991	
Utah	91	100	1989	133	1990
Vermont	81	100	1987	185	1988
Virginia	47	100	1988	133	1990

[†]AFDC or medically needy income threshold (whichever is higher). [‡]Unchanged. Source: Hill IT, 1992, reference 25.

arithm of the population of unmarried women grew linearly between 1986 and 1990, state-specific trends were a good proxy for the number of unmarried women.*

Two other issues are of concern: the underreporting of abortions, as well as missing data on race, marital status or education in some states; and the use of abortion data by place of occurrence as opposed to residence. To address these issues, we redid the analyses using only data from the eight states for which fewer than 10% of records were missing information on race, marital status or education. In addition, we performed the calculations using abortion data both by state of occurrence and by state of residence.

Since the dependent variables in the regression analyses are in logarithms, the coefficients represent proportionate changes; multiplying them by 100 yields percentage changes. As a point of reference, we also report the means of the dependent variables.

Results

Summary Statistics

Except in Rhode Island, Utah and Vermont, the income threshold for Medicaid eligibility as of January 1987 was below 75% of the federal poverty level in each sample state (Table 2).²⁵ After the expansions, all 15 states raised their eligibility thresholds, but to varying extents and at different times.

In 1986, white women in our sample had an abortion rate of 49.1 procedures per 1,000 (Table 3); in the states with the most complete reporting, the rate was 59.9 per 1,000 (not shown). The rate for white

women was almost double the nationwide rate for women 15–44 years of age. It was, however, very similar to national estimates of the abortion rate for unmarried white women in 1987—55 per 1,000 among those aged 20–24 and 46 per 1,000 among those aged 25–29.²⁶

Among the black women in our sample, the 1986 abortion rate was 49.7 per 1,000; it was 52.9 per 1,000 in the states with good reporting. By contrast, other researchers have estimated abortion rates of 109 per 1,000 unmarried black women aged 20–24 and 86 per 1,000 among those aged 25–29 in 1987.²⁷ One reason for the discrepancy between our estimates and those previously reported is that our sample excludes states with large black urban populations that have high abortion rates (California, Illinois, Michigan, New York, Ohio and Pennsylvania, as well as Washington, D.C.).

Birthrates in our sample were reasonably close to national estimates. In 1990–1991, 102.8 births occurred per 1,000 white women in the sample states. According to national estimates, in 1994, the birthrate was 98.6 per 1,000 unmarried white women aged 18–24 years with 12 years of schooling and 118.9 per 1,000 for similar women with 9–11 years of schooling.²⁸

The black women in our sample had a substantially higher birthrate: 208.6 per 1,000 in 1990–1991. Nationally, the birthrate in 1994 was 217.3 per 1,000 unmarried black women with 12 years of education and 152.0 per 1,000 among those with 9–11 years of schooling.²⁹

The change in the natural logarithm of the abortion rate after Medicaid eligibility expansions shows that the rate rose 6.2% among white women and 18.8% among black women. The rise in birthrates between 1989 and 1991 was substantially greater: 39.5% for white women and 23.5% for black women.

These changes, however, are only suggestive. The interval between the period before and after the Medicaid expansions used in these calculations (3.5 years) was relatively long and subject to confounding by trends in rates of births and abortions unrelated to the expansions. To estimate changes while taking such trends into account, we turn to the regression analyses.

Regression Estimates

Relative to birthrates before the Medicaid eligibility expansions, the rate among whites increased after each expansion, and the effects were large (Table 4, page 112). After the first phase of expansions, the birthrate among white women in our sample was 5.2% higher than it was when the

Table 3. Numbers and rates of abortions and births among unmarried women aged 19–27 with 12 or fewer years' education, before and after Medicaid eligibility expansions, and changes in the logarithms of the rates, by women's race

Measure	Abortion		Birth	
	Black	White	Black	White
Number				
Before	12,277	28,820	40,706	40,724
After	14,555	29,507	50,595	58,140
Rate				
Before	49.71	49.05	164.81	69.31
After	60.00	52.20	208.57	102.84
Difference				
in logst	0.188	0.062	0.235	0.395
S.E.	0.012	0.008	0.007	0.006
Z score	15.465	7.558	36.211	61.722

[†]The natural logarithm of the rate after the eligibility expansion minus the natural logarithm of the rate before the expansion. Notes: For abortions, the pre-expansion period is 1986; for births, it is the 12 months from July 1986 through June 1987. The six-month difference in these periods aligns births and abortions to the same cohort of pregnancies. It also permits a full year before the first expansion in any sample state. The postexpansion periods are July 1990 through June 1991 for abortions and 1991 for births; these were chosen for similar reasons. S.E.=standard error.

income threshold was less than 75% of the poverty level; after the second expansion, the change relative to the original level was about the same—a 4.8% increase. This indicates, however, that the birthrate did not increase between the two phases of expansions, since we cannot reject the null hypothesis that the coefficients on the two eligibility expansion terms are different. Thus, the change in birthrates was associated with women's having become eligible for Medicaid as a result of the initial expansion.

Although the increase in the birthrate among white women made eligible by the first expansion was 5.2%, it was not equally distributed among all women, since some women were not affected by the expansion. For example, according to the 1990 census, 28% of white women in our sample were receiving public assistance. Assuming, therefore, that roughly a third of the women in the sample became eligible because of the initial expansion, the effect of the expansion on those women would have been triple the overall effect, or about 15%. More generally, the larger the proportion of women in our sample made eligible by the expansions, the smaller the effect attributable to the expansions.

The expansions had no statistically significant effect on the birthrate among black women, although the sign of the coefficient

*To see this, let $\ln(A_{jt}/P_{jt}) = X_{jt}\beta + u_{jt}$ be a regression of abortion rates in state j in quarter t on our set of state, year and quarter controls (X_{jt}); let A be abortions and P the population. Rewrite the regression as $\ln A_{jt} = X_{jt}\beta + \alpha \ln P_{jt} + u_{jt}$. We estimate this model with the state-trend interactions as our proxy for $\ln P_{jt}$.

Table 4. Regression coefficients (and standard errors) showing the estimated effects of Medicaid eligibility expansions on abortion rates, birthrates and abortion ratios among unmarried women aged 19–27 with 12 or fewer years' education, by phase of expansion, according to race, 15 states, 1986–1992

Measure and expansion phase	White (N=420)		Black (N=308)	
	Model 1†	Model 2‡	Model 1†	Model 2‡
Abortion rate				
First phase	.005 (.025)	-.001 (.024)	.005 (.036)	.021 (.030)
Second phase	.003 (.039)	.0003 (.034)	-.054 (.055)	-.021 (.054)
Mean	44.0		31.2	
R ²	.86	.90	.90	.94
Birthrate				
First phase	.071*** (.012)	.052*** (.010)	.016 (.013)	.010 (.012)
Second phase	.066*** (.018)	.048*** (.015)	.005 (.019)	.006 (.017)
Mean	83.2		186.6	
R ²	.93	.96	.89	.91
Abortion ratio				
First phase	-.038** (.016)	-.017 (.016)	-.002 (.030)	.016 (.005)
Second phase	-.040 (.026)	-.018 (.023)	-.039 (.045)	-.009 (.037)
Mean	.331		.147	
R ²	.89	.93	.87	.94

p<.05. *p<.01. †Includes dummies for year, quarter and state as explanatory variables. ‡In addition to year, quarter and state dummies, includes a state-specific linear trend term. Notes: Ns represent the number of state-specific quarters that contributed data. The N for black women is smaller than that for nonblack women because four states with small black populations (Maine, Montana, Utah and Vermont) are excluded from the analysis.

was always positive. One explanation for this finding is that black women in these age and educational strata are less affected than their white counterparts, given that almost half receive public assistance.

No statistically significant change occurred in abortion rates as a result of the expansions. However, for black women, the coefficients associated with the second expansion were negative and suggested declines of 5.4% without controls for state trends and 2.1% when state trends were taken into account.

Sensitivity Analysis

We were concerned that the population data on which we relied in the above analyses overestimated the unmarried population in 1986, and therefore underestimated the birthrates and abortion rates for the years prior to 1990. Thus, we analyzed changes in the abortion ratio, which has no population component. Results showed negative and marginally significant coefficients for white women when state trends were not taken into account. This finding is consistent with results for rates of births and abortion, since the abortion ratio is dominated by the rise in births relative to abortions. Among black women, coefficients on the abortion ratio were negative in three of four cases, but were statistically insignificant.

As an additional check on the sensitivity of our results to our population estimates, we regressed the natural logarithm of the number of births and abortions on the in-

dicator variables. (Interactions between state and trend serve as a state-specific measure for log-linear growth in the unmarried population.) For white women, the coefficients on the Medicaid expansion terms in a log-linear regression of births (not shown) were almost identical to those for birthrates shown in Table 4. Coefficients on the Medicaid expansion terms in the log-linear regression of abortions were negative, but statistically insignificant.

For black women, use of the natural logarithm of births instead of birthrates as the dependent variable did not change our findings.

The coefficients on the Medicaid expansion terms were negative, but very small and statistically insignificant. For abortions, the coefficients on the expansion indicators were negative and in one case statistically significant. In short, our key finding—a large and statistically significant increase in white women's birthrates associated with the Medicaid eligibility expansions—remained unchanged when we used the log of births instead of the log of birthrates.

When we redid the analyses, focusing on abortion and including only the eight states with the most complete data on race, marital status and education, the results indicated a negative association between abortion rates among white women and the Medicaid expansions (Table 5). Although the association was statistically significant only for the second phase of expansions, it existed for abortion rates by state of occurrence and state of residence, and it was large: 9.1–10.0%. Results for the effects on the number of abortions and the abortion ratio were quite similar to

those for abortion rates, although the coefficients for the number of abortions were only marginally significant. For black women, effects of the Medicaid expansions on all six abortion outcomes were positive, but they generally reached no more than a marginal level of statistical significance.

Finally, we found that the initial expansion was associated with a 3.6% rise in the birthrate among white women (p<.05), but with no change in the rate among black women (not shown). In sum, results for the subsample of eight states and the total sample of 15 states were in general agreement for births but not abortions. This suggests that abortion underreporting may have obscured an association between the Medicaid eligibility expansions and reductions in abortion rates among white women in the larger sample. The fall in abortions associated with the second Medicaid expansion, however, was unaccompanied by a rise in births. The increase in birthrates, a consistent finding across the large and smaller samples, was limited to the first phase of the Medicaid expansions.

Discussion

The Medicaid eligibility expansions for pregnant women initiated in the mid-1980s represent arguably the largest expansion in health care coverage for the poor since the establishment of Medicare and Medicaid more than 30 years ago. By 1994, 39% of all births in the country were

Table 5. Regression coefficients (and standard errors) showing the estimated effects of Medicaid eligibility expansions on abortion measures among unmarried women aged 19–27 with 12 or fewer years' education, by phase of expansion, according to race and state of occurrence vs. state of residence, 1986–1992

Measure and expansion phase	White (N=224)		Black (N=140)	
	Occurrence	Residence	Occurrence	Residence
Abortion rate				
First phase	-.014 (.030)	-.005 (.029)	.047 (.035)	.061* (.037)
Second phase	-.100** (.045)	-.091** (.043)	.036 (.053)	.066 (.055)
Mean	63.3	57.6	67.9	63.7
R ²	.78	.88	.93	.91
No. of abortions				
First phase	-.016 (.030)	-.019 (.030)	.066* (.037)	.076* (.039)
Second phase	-.077* (.046)	-.090* (.046)	.113* (.058)	.128** (.061)
Mean	779.0	707.0	709.0	678.0
R ²	.98	.99	.98	.98
Abortion ratio				
First phase	-.011 (.019)	-.015 (.020)	.037 (.029)	.050 (.032)
Second phase	-.064** (.029)	-.077** (.031)	.034 (.044)	.060 (.048)
Mean	.440	.407	.260	.247
R ²	.91	.94	.89	.87

*p<.10. **p<.05. Notes: Only states for which fewer than 10% of records are missing data on race, marital status and education are included: for blacks, Georgia, Kansas, South Carolina, Tennessee and Virginia; for nonblacks, these plus Montana, Utah and Vermont. Calculations include year, quarter and state dummies, as well as a linear trend term interacted with each of the state dummies. Ns represent the number of state-specific quarters that contributed data.

covered by Medicaid.³⁰ Until now, evaluation of the expansions has focused almost exclusively on whether increased eligibility has increased prenatal care utilization and improved infant health.

In this article, we have presented preliminary evidence that birthrates among unmarried white women aged 19–27 who have no more than a high school education increased as a result of the expansions. We did not find a similar association among black women. We also found a fall in abortions among whites, but the result was limited to the second phase of the Medicaid expansions and was sensitive to the sample of states.

Our results are consistent with and extend those of previous work showing that the Medicaid expansions were associated with a decrease in the abortion ratio among white women in South Carolina, Tennessee and Virginia.³¹ They are also consistent qualitatively with a previous analysis showing a 5% increase in birthrates associated with the expansions.³² However, since that analysis included all women aged 15–44, rather than just those most likely to have been affected by broadened eligibility, the 5% increase implies a much larger rise in birthrates among women made eligible by the expansions. If, for example, only 10% of women 15–44 qualified because of the expansions, then a 5% increase in the birthrate would imply a 50% increase in births within this group (assuming no change among the 90% unaffected by the expansions).

The findings of an increase in birthrates among white women after the initial expansion in Medicaid eligibility and no additional increase after the second expansion have several implications. First, they suggest that the fertility of women with family incomes less than 100% of the federal poverty level was more responsive to changes in eligibility than the fertility of those with incomes at 101–185% of poverty. Second, since abortion rates fell only after the second expansion and only among women in a subsample of states, the finding that birthrates rose only after the first expansion implies that poor white women with a strong preference for children may have been encouraged to become pregnant earlier or more frequently than they would have if the expansions had not occurred.

The decline in abortion rates associated with the second phase of the Medicaid

expansions in the subsample of states is a less robust finding than the increase in birthrates and is more difficult to interpret. Nevertheless, the fall in abortion rates and rise in birthrates among women in these states indicate a smaller change in pregnancy rates than appears to have taken place after the first expansion. Moreover, these findings suggest, and we emphasize, that women affected by the second phase of expansions may have carried to term pregnancies that they would have terminated in the absence of the expansions.

We can only speculate as to the reason for the differential response to the expansions by race. As noted previously, black women were probably less affected by the increase in income eligibility thresholds, given that 47% of those in our sample who had at least one child were on public assistance. In addition, our sample of states was small and was dominated by southern states. A national analysis might alter this finding.

Is it plausible that insurance coverage of prenatal and postpartum health care is a sufficient subsidy to alter the fertility behavior of poor white women? Maternity and infant health care from conception through the first year of life cost an estimated \$6,850 in 1989.³³ This figure, however, overestimates the value of Medicaid coverage for the uninsured, since many pregnant women who lack insurance are eligible for subsidized care through Title V (the Maternal and Child Health Block Grant), as well as free care paid from state uncompensated care pools.*

Evidence as to the importance of relatively small subsidies for births comes from an experiment in which women in a free-care plan had significantly more births over the course of the study than did women in a coinsurance plan.³⁴ Notably, the financial risk to women in the coinsurance plan was limited to \$1,000 per year. A study of a cutoff in Medicaid-financed abortions makes a similar point.³⁵ Although the cost for a first-trimester abortion in North Carolina was only \$200, the cutoff of state funds for abortion was associated with an 11% decline in abortions and a 5% increase in births among poor black women 18–29 years of age.

In the context of an economic model of the demand for children, our findings indicate that subsidized health care for pregnant women is associated with increases in the number of children among unmarried white women.³⁶ Our finding is broadly consistent with those of other studies that report changes in fertility associated with restrictions on Medicaid-financed abor-

tions, in that the price associated with the demand for children—in this case, the price of fertility control—affects childbearing.³⁷

This finding has important implications for policies designed to improve infant and child health. As economic theory predicts, subsidized care may induce some parents to choose to have more children, which may have the unintended consequence of diminishing average investments per child and may thereby attenuate the policy's impact on infant and child health.

Our findings should be viewed as preliminary, and replication is needed at the national level. Unfortunately, only the Centers for Disease Control and Prevention collect national data on abortion that are stratified by age, race or marital status. These data, however, are available only at the aggregate level, and stratification by more than one characteristic is therefore not possible. On the other hand, data on births are available nationally at the individual level, and we hope future research will continue our line of inquiry.

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*The focus on outlays, however, ignores the time costs, inferior quality and accumulation of bad debt for uninsured women who were unable to pay for services not covered by other public sources.

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