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THE EFFECT OF MEDICAID FAMILY PLANNING EXPANSIONS ON UNPLANNED BIRTHS

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Background. **Medicaid covers nearly 50% of all family planning services nationally. Between 1994 and 2001, 11 states implemented demonstration programs that expand coverage of family planning beyond the federally mandated minimum coverage levels.**

Methods. **We estimate the effect of income- and postpartum-based eligibility expansions on birth rates using states that did not expand coverage as a control for states that did expand coverage. Our data span 1991–2001 and include all 50 states. We also estimate net expansion costs from societal and state perspectives for 5 expansions that published incremental expansion costs.**

Results. **We find that Medicaid eligibility expansions lowered average annual birth rates in all states. Birth rates were reduced on average by 1.95 points in income-based expansions and by 0.87 points in postpartum-based expansions. The cost offset of maternal and child health expenditures of the expansions exceed program costs in all states but California. This result is likely because the objectives and scope of the California program goes beyond just unplanned births, which makes the program cost higher relative to the reduction in births.**

Conclusions. **Both income- and postpartum-based family planning expansions either yield financial benefits or, at the very least, are cost neutral from the perspective of state governments. Income-based expansions are significantly more effective because eligibility is not limited to only postpartum women. The experience of these early family planning expansions should be a guide for other states considering family planning benefit expansions. From the national perspective, 4 out of 5 programs were cost neutral, although California had significantly higher costs. From the state's perspective, all of the expansions were either budget neutral or yielded a net cost savings.**

Medicaid has played an important role in the provision of contraceptive care since the passage of Medicaid family planning–related amendments in the early 1970s. Medicaid expenditures currently account for approximately half of all family planning spending within the United States. Coverage is mandated for virtually all FDA-approved family planning services; bars cost-sharing for Medicaid family planning benefits; and entitles states to receive 90% of the cost of services from the federal government rather than the usual 50–83% matching rate (Social Security Act, 1972a, 1972b, 1972c). Consequently, all Medicaid beneficiaries are guaranteed access to a wide range of family planning services at no financial cost.

The clinical and economic benefits of contraception are well documented (Centers for Disease Control and Prevention, 2000; Forrest & Samara, 2006; Hatcher, 2004; Paton, 2002; Trussell et al., 1995; Trussell, Wiebe, Shochet, & Guilbert, 2001). There is, however, less empirical research regarding the value of Medicaid family planning expansion benefits. Initial work on Medicaid family planning expansions was conducted by Edwards, Bronstein, and Adams (2003). They examined the budgetary implications of these demon-

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strations in conjunction with their impact on unintended pregnancies. Using detailed data on Medicaid beneficiaries and costs, they found that expansions were consistently budget neutral, although they did not always reduce the number of unintended pregnancies. Foster et al. (2004) examined California Family Planning, Access, Care, and Treatment (PACT) program during 1997 and 1998. They estimated a Markov model of unintended pregnancies using data from paid claims and medical record review. They found that California's program averted 108,000 unintended pregnancies that would otherwise have resulted in 50,000 births and 41,000 induced abortions.

Frost, Sonfield, and Gold (2006) extend this literature by simulating the effect of extending Medicaid family planning benefits nationally. They simulate the effect of a 200% and 250% of federal poverty level (FPL) limit income-based expansions on unintended pregnancies based on averages that appear in the 2002 National Survey of Family Growth and data on the failure rates of contraception care. Policy costs are then calculated using existing estimates and use these calculations to measure the impact of proposed policies on total program costs and medical cost offsets. They find that both the 200% FPL and 250% FPL income-based expansions would be cost effective if implemented nationally.

This paper is different from earlier work in several ways. First, we estimate the effect of the expansions econometrically, and in doing so, control for measured differences and unmeasured fixed differences across states. This is done using a difference-in-difference approach that allows us to control for contemporaneous trends that affect fertility by using states with stable programs as a control. This results in an accurate estimate of the average effect of the programs that is not confounded by contemporaneous trends. It also yields estimates that are net of substitution of purchases from other forms of public and private coverage. Second, we do not need to make assumptions regarding the take up rate or the effectiveness of contraception care, as is usually done in this literature. Third, we estimate the net program cost in 5 states and calculate the standard errors, confidence intervals (CI), and p-values associated with the estimates. To date, the estimates of the effect of these programs have been point estimates that do not take statistical error into account.

Estimating the value of family planning is important because it is unclear whether contraception subsidies are a wise policy. On the one hand, maternal and infant health cost offsets associated with family planning benefits may overshadow program costs. On the other hand, Medicaid family planning coverage may substitute for other types of purchases leading to smaller net effects. For example, in the absence of the expansion, individuals may take advantage of other public provisions for family planning products, such as Title X direct service subsidies. Some women might also substitute out-of-pocket or insured private contraception purchases for Medicaid-covered purchases after an expansion.

Table 1. Family Planning Expansions

State	Year Implemented
Income-based expansions	
Arizona	1996
Arkansas	1998
California	1997
Missouri	1999
New Mexico	1998
New York	1998
Oregon	1999
South Carolina	1997
Postpartum-based expansions	
Delaware	1996
Florida	1999
Maryland	1995–2000
Rhode Island	1994
South Carolina	1994–1997

Source: State Family Planning Administrators Informational Update on State Medicaid Family Planning Waivers (June, 2003)

In summary, we measure the effect of Medicaid family planning expansions on birth rates and maternal and infant health expenditures and examine whether health care–related cost offsets are greater than program costs. The estimates are incremental and thus net of any substitution between private purchases and Medicaid purchases. Our results consistently reveal that 1) the income-based expansions are effective at reducing births; 2) from the state's perspective, family planning expansions save money or at least are budget neutral; and 3) from the national perspective, with the exception of California's unique program, family planning expansions are always at least budget neutral.

Methods

Family Planning Expansions

Medicaid's role in family planning expanded with the introduction of Medicaid 1115 family planning demonstration waivers that allow states to expand eligibility. The first waiver was approved in 1993 for South Carolina. Subsequently, 11 additional states implemented family planning waivers by 2000. Early programs provided services to women who lost Medicaid eligibility postpartum, subsequent programs expanded eligibility based on income (Table 1). The programs were designed specifically to expand the range of eligible beneficiaries for Medicaid family planning services in the hopes of avoiding unplanned births. The waivers allow states to expand eligibility

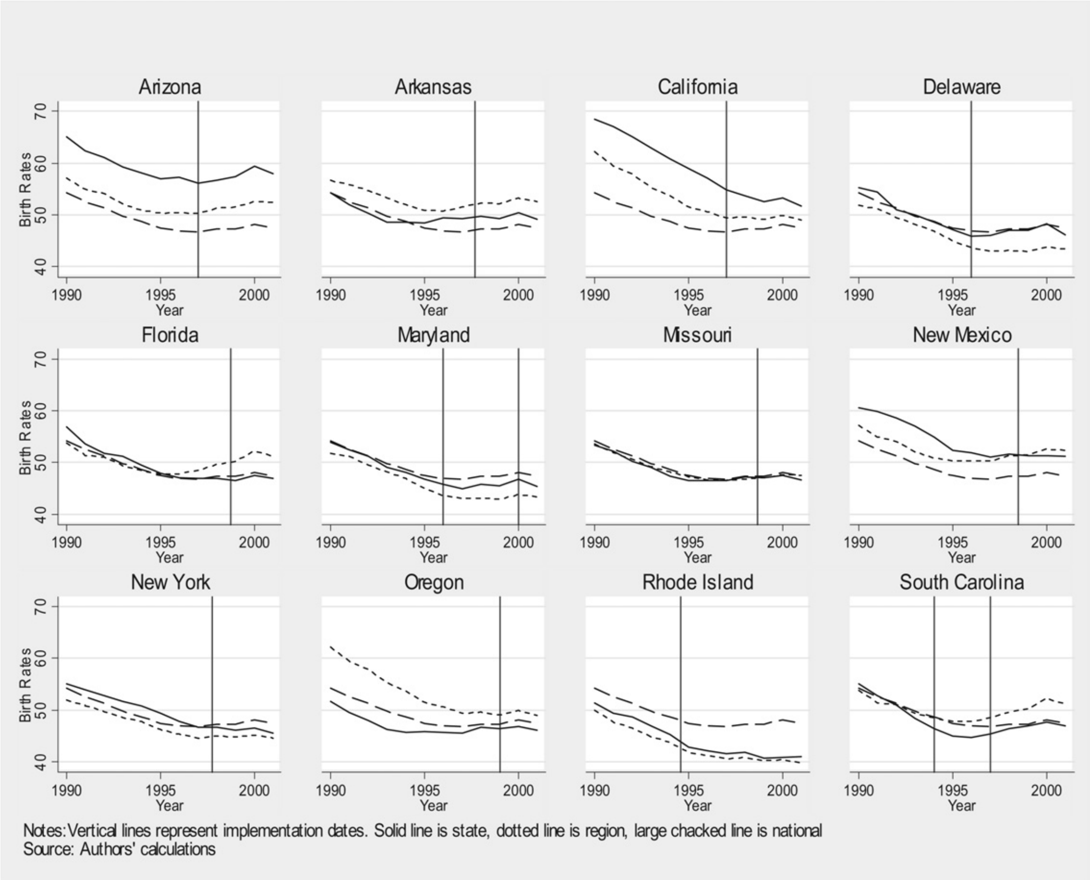

Figure 1. Birth rates by state, region, and year. Vertical lines represent implementation dates. Solid line is state, dotted line is region, and large checked line is national.

requirements specifically for Medicaid family planning services.

State demonstration program policies were compiled from a review of states' 1115 family planning demonstration fact sheets and confirmed with existing literature (Edwards et al., 2003; Gold, 2003). State policies are characterized by the first full year of implementation and whether they are postpartum- or income-based eligibility expansions. Our outcome variable is the birth rate; therefore, we do not expect programs to have an effect until at least 9 months after implementation. As a result, we measure the effect of expansions beginning the year after the first full year of implementation.

We study all programs implemented before 2000. There were 8 income-based expansions and 5 postpartum-based expansions. Maryland and South Carolina switched from a postpartum to an income-based expansion in 2000 and 1997, respectively. We did not include Maryland's income expansion in the measurement of the effect of income-based expansions because of its late start. We do, however, control for Maryland's transition between programs separately. As a result, South Carolina appears as a postpartum expansion until 1997 and thereafter as an income expansion. In contrast, Maryland appears as a postpartum expansion until 1999 but is never coded as an income expansion. States that did not expand coverage are used as a control for states that did expand coverage.

Data

We collected state-level data for all 50 states for 1991–2001. Our main outcome variable, births per 1,000 women of child-bearing age, is obtained from the National Vital Statistics Report (Hamilton, Sutton, & Ventura, 2003). Thus, for expansions that started in 1998 or 1999, we only have 2 or 3 years of postexpansion data. Figure 1 displays the birth rates for each state that experienced an expansion, the US Census Division (regional) average, as well as the national average. Expansion states are pooled with comparison states within their respective regions. The solid line reflects the state and lines with long and short dashes reflect the national and regional averages, respectively. There was a downward trend in all regions' birth rates until about 1996. After 1996, only the Middle Atlantic and New England regions continue the downward trend. Other regions, and the national average, experience flat or increasing fertility rates subsequent to 1996. Increases were especially large in the South Atlantic region.

In addition to summarizing how fertility rates have varied over time, these fertility rates provide an unadjusted measure of the effect of family planning expansions on fertility. The policy effect is found by comparing the vertical difference between the state and comparator rates both before and after policy implementation. The California experience, for example, can be measured by comparing the difference of the California rate and the Pacific region rate in 1997 (the year of initiation) with this same difference during postimplementation years. This difference, which narrows after implementation, is consistent with the policy reducing fertility.

The California example also illustrates the roles of national and regional trends. Before 1997, California's trend clearly mirrors the regional trend rather than the national trend. Were the national trend used as a comparison, the measured policy effect would be biased upward. Conversely, were the national, rather than regional, trend used in Rhode Island, the policy effect would have a sizable negative bias. Overall, the figures illustrate the importance of controlling for regional trends in the empirical work.

State demographic controls were constructed from 2 separate datasets. First, the Census Bureau's "State Characteristics Population Estimates" data are the source of the variables that measure population distribution by age and gender. Second, female labor force participation rates by age; unemployment rates by age and gender; and the share of African American, Hispanic, non-Hispanic White, population by age and gender were drawn from the Census Bureau's Current Population Survey.

When the Current Population Survey weights were adjusted in 2000 to reflect information collected in the 2000 census, there was a major change in the sampling weights for Hispanics. As a result, the state-specific race data exhibit significant discontinuous changes between in the late 1990s and early 2000. We employ a moving average smoother using the average of the current year data and the 2 subsequent years' data to smooth the ethnicity data.

Control Variables

We created the following demographic variables to use as controls in the analysis: percent of females by age group (10–14, 15–19, 20–24, 25–29, 30–34, 35–39, 40–44, and 45–49); the ratio female to male population; male and female unemployment rates by age group (16–19, 20–24, 25–34, 35–44); and female labor force participation rates by age group (16–19, 20–24, 25–34, 35–44). We controlled for both the percent of males and females who are Hispanic and non–Hispanic Whites by age group (16–20, 21–44, 45–64), and overall and Hispanic annual population growth rates. The growth rates control for differences in the change in the composition of the population over time. The choice of age groups for ethnicity and unemployment rates reflect data availability. Unfortunately, data on Medicaid eligibility are not consistently available during this time period. However, variation in unemployment rates, labor forces participation, age, and regional quadratic trends likely controls for any unmeasured changes in the number of people eligible for Medicaid.

We performed subanalyses using data from Arkansas, California, New Mexico, Oregon, and South Carolina using state-reported total program costs and Medicaid cost estimate of maternal and infant health expenditures (Arkansas Department of Human Services, 2004; California Department of Health Services, 2004; Edwards et al., 2003; Oregon Department of Human Services, 2003). Unfortunately, state-level data from a common source do not exist. CMS does collect state-level data through the Medicaid Statistical Information System (MSIS), but these data only cover fee-for-service (FFS) beneficiaries. The expenditures and number of users reported in the MSIS vary dramatically over time and across states, making these data unsuitable for a panel data analysis. In the end, our selection of 5 states was driven entirely by the availability of valid, comparable data because most other states did not report expansion-related costs separately from the cost of the entire program. Expansion-related costs for our purposes include direct medical expenditures, administrative costs, and additional costs due to the demonstration status of the expansions. All of these costs are incremental and thus would not have been incurred had the expansion not taken place.

Maternal and infant health care related costs are not included in the program costs, but are used to estimate cost offsets. These costs include: prenatal services, delivery costs, and infant medical expenses. Costs range from $5,349 for Oregon to $8,837 for Arkansas. Cost data were based on states *ex post* reviews of actual Medicaid maternal and infant health costs. These data were reported annually by the state Department of Health Services for Arkansas, California, and Oregon. Whereas New Mexico and South Carolina did not publish these results directly, the states Medicaid and maternal and infant health costs were documented and published by Edwards et al. (2003); they also provided data on Arkansas. In each case, these data were documented for the purpose of demonstration program reauthorization. Cost data were not, however, available for Arkansas in 2000 and 2001 or Oregon in 2001. Consequently, we constructed these data points from previous Arkansas and Oregon data as well as state- and year-specific Medicaid delivery and child health cost growth measures (Frost et al., 2006).

Statistical Methods

We perform a difference-in-difference regression analysis to measure the effect of the expansions on birth rates. The model is called a difference-in-difference estimator because we measure the difference between the pre- and postexpansion birth rates in states with eligibility expansions and compare this to the difference between pre- and postexpansion birth rates in states that do not expand eligibility; in other words, we estimate the difference between expansion and nonexpansion state pre–post expansion differences.

Intuitively, this approach is similar to controlled experiments where there is a treatment group and a control group. The change in the treatment group after an intervention is compared with the control group that did not receive the intervention. In this manner, states that do not expand coverage serve as a control group and thus we control for regional secular trends as well as other contemporaneous variation in factors that are correlated with birth rates but unrelated to the expansion.

The statistical specification is as follows:

$$\begin{aligned} Birthrate_{st} = \gamma_s + \tau_t + \beta_1 Inc\_expansion_{st} \\ + \beta_2 PP\_expansion_{st} + \beta_3 X_{st} + \varepsilon_{st} \end{aligned} \quad (1)$$

where the subscript s denotes state and t denotes time; *Inc_expansion* equals 1 if an income-based expansion is in effect and 0 otherwise; *PP_expansion* equals 1 if a postpartum-based expansion is in effect and 0 otherwise. The coefficients associated with the expansion variables (β_1, β_2) measure the average annual changes in birth rates due to the expansion holding all other covariates constant. Variation in demographic characteristics (X_{st}) is controlled for with the variables described; β_3 is a vector of coefficients. Fixed, systematic differences across states are controlled for using state fixed effects (γ_s) and contemporaneous unmeasured trends are controlled for with functions of time (τ_t). Finally, ϵ_{st} is an identically and independently distributed error term.

The specification controls for unobserved national trends when τ_t is defined as a vector of year-specific binary variables. However, such a specification does not control for region- or state-specific trends. If region- or state-specific trends are favorable for an expansion in some states but not others, our results could be biased. To test whether this is the case, we estimate several alternative specifications of τ_t. The first type of time trend is quadratic—this allows for the type of nonlinear trend displayed in Figure 1, where birth rates initially fall, but then plateau or even rise. We control for national, region-specific, and state-specific quadratic time trends in separate specifications.

The regional specification adds a regional trend to the national controls. Region-specific trends are controlled for by interacting a region dummy with the quadratic time trend. This specification completely controls for any unobserved national trend as well as any unobserved region-specific quadratic trend. The state specification replaces the region quadratic time trend interaction with a state quadratic time trend interaction. However, this specification has an important limitation in that it is not possible to separately identify state time trends from the effect of the program because we only have 1 observation per state in each year. Thus, part of the effect of the expansion is embodied in the state-specific trends. Consequently, the specification using regional quadratic time trends is preferred and is the focus of our discussion.

Finally, we estimate a specification that controls for regional contemporaneous shocks by interacting the region dummy variable with the year dummy variables. This approach allows for both national and regional unobserved contemporaneous trends. Further, by using year dummy variables, this approach relaxes the assumption that unobserved trends are quadratic, allowing such trends to be completely nonparametric. We present the results from all of the specifications below. Standard errors are calculated assuming state-level clustering in all specifications. Note that under clustering the p-values are inflated by .05 to .10 in each specification. This is likely due to the fact that standard errors that do not control for clustering are biased downward owing to serial correlation (Bertrand, Duflo, & Mullainathan, 2004).

Policy Simulations

We use the estimates of β_1 and β_2 from the region-specific quadratic trends specification of Equation 1 to calculate the fertility and financial effects of family planning demonstrations. We first convert the parameter estimates to an average annual change in unplanned births by year for each state that implements a family planning policy. We then multiply each state's estimated change in unplanned births by Medicaid maternal and infant costs per birth. Nearly all averted births would otherwise have been covered by Medicaid as Medicaid maternal and infant health eligibility ceilings are at least as high as Medicaid family planning eligibility ceilings within our study states (Frost et al., 2006).

Next, we calculated the program's net financial benefit by calculating the difference between each state's average annual cost offset and average annual expansion costs. We then calculated the net program costs from the states perspective by estimating the difference between each state's share of Medicaid maternal and infant health care costs and the 10% state share of expansion program costs. Finally, we calculate the standard errors of our estimates using a statistical bootstrap with 1,000 repetitions controlling for state-level clustering. The CIs and p-values are

Table 2. Annual Effect of Family Planning Expansions on Birth Rate

Quadratic Time Trends†	Change in Birth Rate		
	Estimate	95% CI	*p* Value
National			
Income-based expansions	–1.97**	–3.64, –0.30	.02
Postpartum-based expansions	–1.16	–2.83, 0.50	.17
Regional			
Income-based expansions	–1.95***	–3.06, –0.84	<.01
Postpartum-based expansions	–0.87	–2.17, 0.43	.18
State			
Income-based expansions	–0.66***	–1.22, –0.11	.02
Postpartum-based expansions	–0.09	–0.52, 0.33	.65
Year Indicators‡: Regional			
Income-based expansions	–1.86***	–3.13, –0.60	<.01
Postpartum-based expansions	–0.84	–2.24, 0.56	.24

Notes: Standard errors are calculated assuming clustering at the state level.
Difference in difference estimates adjusted for female population by age group; ratio of females to males; share of male and female population Hispanic, African American, and non-Hispanic White; male and female unemployment rates; and female labor force participation by age group and total and Hispanic population growth rates.
***$p < .01$; **$p < .05$; *$p < .10$.
†Quadratic time trends either national, region specific, or state specific.
‡Year dummy variables interacted regional dummy variables.

based on a test with a null hypothesis that the estimate is equal to zero versus an alternative hypothesis that the estimate is not equal to zero.

Results

Table 2 presents estimated changes in unplanned births in each specification. The 95% CI is in column 3 and the *p*-value is displayed in column 4. The first set of results is based on the specification that controls for only national time trends. The second set of results (rows 4 and 5) is based on the specification that control for region-specific quadratic time trends. This is the favored specification. The third set of results controls for state-specific quadratic time trends and the fourth set of results include region-year dummy variable interactions. We only report the coefficients on the policy variables; complete results that include all coefficients are available from the corresponding author.

The results show that average annual birth rates were significantly lowered by income-based expansions, regardless of the specification. In all specifications, the parameter estimates are larger for income-based than for postpartum-based expansions. The estimates are statistically significant in all income-based specifications. The strongest effects are measured when we control only for national trends. The effect is slightly smaller when we control for region-specific trends. The estimate falls dramatically when we control for state-specific trends, although it remains statistically significant. As discussed, this is not surprising because the state-specific trend captures part of the effect of the expansion.

We do not find a statistically significant effect of postpartum-based expansions. However, this estimate is an average of all programs, and if we were to exclude the Arizona expansion, the average postpartum-based expansion leads to a reduction in birth rates. It is unclear why inclusion of Arizona's program leads to an insignificant average postpartum-based expansion effect. In addition, we may be underpowered in this analysis because the effect size of a postpartum-based expansion may be too small to detect a significant difference with only 50 states.

Table 3 reports our subanalyses of cost offsets and net program costs. All figures are reported in $1,000s. The first column displays the program cost for each state. The California program was by far the largest, with an average annual cost of >$270 million. This is due to several reasons. First, California has a much larger population than the other states. Second, California's program has the highest income threshold of any state, offering family planning benefits to those with incomes ≤200% FPL; other states typically cap enrollment at 185% FPL. Third, California Family Planning, Access, Care and Treatment also invested

Table 3. Cost Offsets and Program Costs, Selected States

	Years	Program Cost	Maternal and Infant Costs Averted	Net Program Costs	Net Program Cost to State
Arkansas	1999–2001	$7,706	–12,581*** (–21,304, –3858)	–4876 (–13,599, 3847)	–2530** (–4818, –242)
California	1999–2001	$270,862	–135,550*** (–234,096, –37,005)	135,311*** (36,766, 233,857)	–43,000 (–89,962, 8484)
New Mexico	2000–2001	$3,918	–7,910*** (–13,316, –2505)	–3993 (–9399, 1413)	–1890** (–3450, –331)
Oregon	2000–2001	$12,421	–11,404*** (–19,512.63, –3295.474)	1017 (–7092, 9126)	–3141** (–6257, –24)
South Carolina	1995–1997	$2,064	–6,936 (–18,958, 5086)	–4230 (–16,252, 7792)	–1857 (–5546, 1831)

Notes: All values expressed in $1,000s. 95% Confidence intervals in parentheses. Standard errors are calculated using a statistical bootstrap with clustering at the state level.
Data from the Arkansas Department of Health Services (2004); California Department of Health Services (2004); Oregon Department of Health Services (2003); and Edwards, Bronstein, and Adams (2003) for New Mexico and South Carolina.
÷***$p < .01$; **$p < .05$; *$p < .10$.

heavily in outreach and access. All of these features increased California's demonstration program costs. South Carolina's postpartum-based expansion is the least expensive, reflecting South Carolina's relatively sparse population and the fact that postpartum-based expansions tend to be smaller in scope.

The next column displays maternal and infant costs averted and the 95% CI. Arkansas's expansion saved almost \$13 million in maternal and infant health costs (95% CI, −\$21.304 million to −\$3.858 million). California's expansion yielded a savings of about \$135 million, and the programs in New Mexico and Oregon also yielded statistically significant offsets ($p < .01$). The cost offset in South Carolina is barely insignificant.

The third column of results displays the net program costs. We find that with the exception of California, the savings in maternal and infant health care are neutral. The net change in costs from these programs range from −\$4.876 million in Arkansas (95% CI, −\$13.599 million to \$3.847 million) to −\$3.993 million in New Mexico (95% CI, −\$9.399 million to \$1.413 million). The Oregon estimate is >0, but not statistically significant. Only California is statistically significant, which likely reflects the size (and thus expense) of the California program.

The last column reflects the net cost of the expansion from the state's perspective. On average, all programs reduce state expenditures. Arkansas's expansion saves the state an estimated \$2.5 million annually ($p < .05$); New Mexico's expansion yields an estimated \$1.89 million annually ($p < .05$); Oregon's expansion saves an estimated \$3.14 million annually ($p < .01$); and South Carolina's postpartum-based expansion saved the state an estimated \$1.857 million annually. However, although the estimated savings in California is large, it is not statistically significant.

Discussion

Overall, we find that Medicaid family planning expansions led to lower birth rates. This finding is especially true for income-based Medicaid family planning expansions. Our statistical approach allows us to measure the effect of the expansions allowing for unobserved trends. As a result, our estimates can be interpreted as the difference between the birth rate after the expansion to what it would have been had the expansion not taken place. This method nets out any substitution from other types family planning coverage and purchases and controls for important (and unobservable) regional fertility trends. This may explain why our estimates are slightly lower than others have reported (see e.g., Frost et al., 2006).

Consistent with our finding that birth rates are lowered by the expansions, we identify significant maternal and infant health care cost offsets in all income-expansion states for which data are available. These health care cost offsets exceed total program costs in most cases. Thus, our analyses suggest that family planning expansions either reduce overall Medicaid expenditures, or at the very least are cost neutral. From the perspective of state governments, the financial benefit of family planning expansions is quite large; the federal government covers 90% of family planning costs but only 50–83% of Medicaid child and maternal health costs.

We do not find a statistically significant effect of postpartum-based expansions. However, this is an average estimate; it does not imply that there are not significant reductions in all programs. In fact, if we exclude the Arizona program the results become statistically significant. It is not clear why the Arizona program was ineffective, although we suspect that unobserved changes in the foreign-born population led us to underestimate the effect of postpartum-based expansions. In addition, the Arizona expansion is relatively small—only postpartum women between 100% and 133% FPL are newly eligible.

Medicaid family planning programs may also have benefits and costs beyond what we have measured in this study. For example, condom utilization (covered by some expansion programs) reduces the rate of sexually transmitted infections (Feldblum, Morrison, Roddy, & Cates, 1995). Levonorgestrel-releasing intrauterine system (Mirena) utilization may reduce rates of ectopic pregnancies and endometriosis as well as the risk of pelvic inflammatory disease (Luukkainen & Toivonen, 1995). Likewise, oral contraceptives may prevent pelvic inflammatory disease while protecting against a wide range of conditions including cancers of the ovary and endometrium (Harlap, Kost, & Forrest, 1991). Furthermore, it is likely that access to family planning benefits will reduce abortions. An estimated 44.7% of unplanned pregnancies are terminated (Henshaw, 1998).

Unplanned births are associated with delayed initiation of prenatal care and substance abuse during pregnancy (Brown & Eisenburg, 1995; Cartwright, 1988; Centers for Disease Control and Prevention, 1992; Kost, Landry, & Darroch, 1991; Pamuk & Mosher, 1988). These factors may lead to adverse birth outcomes such as low birth weight. Consequently, children of unplanned pregnancies are less likely to survive their first year of life. In addition to the private cost of unplanned birth (to both the children and their families), the government may face a substantial financial burden.

Nevertheless, the provision of family planning services may also produce social costs. The provision of oral contraceptives may, for example, lead to decreased condom use. Although oral contraceptives are more effective at preventing pregnancies, they provide little protection from sexually transmitted infections. Thus, family planning expansions may have the unintended effect of increasing sexually transmitted infection transmission rates.

Our study has several limitations. First, we are limited to analyzing program costs in 5 states owing to data availability. Data from other states are not available for the expansions separately from the entire program costs. Second, we are unable to adequately control for state-specific preimplementation trends because these trends cannot be identified separately from the effect of the expansion, although we do control for secular quadratic regional trends. Third, our results reflect an annual average effect that in a few cases reflects only 2 or 3 years of postexpansion data. Finally, it may be that the expansions are undertaken for reasons that are unobserved but correlated with birth rates. For example, if expansions occur only in states where there is likely to be the most benefit, our results would not be generalizable to other states. We do not think such bias is significant given the substantial heterogeneity of the states that have implemented family planning expansions.

These findings are particularly important given recent trends in public funding for family planning. South Carolina, for example, has cut its state family planning budget by $4 million dollars since 2001 ("2 Steps Forward," 2006). Similarly, Missouri, Minnesota, and Texas have made multimillion dollar cuts in the past 3 years ("A Backward Step," 2006; "More Support," 2006; "Women Struggling With Cuts," 2006). Overall, our results suggest that both types of Medicaid family planning expansions either yield financial benefits to states or, at the very least, are cost neutral. The financial benefit to state Medicaid budgets is generally quite large. However, the effect of income-based expansions is much larger that postpartum-based expansions. This is likely due to the fact income-based expansions expand eligibility to all women, rather than only those who are postpartum. The experience of these early family planning expansions should be a guide for other states considering family planning benefit expansions.

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