Effects of Medicaid Family Planning Waivers: The Arkansas Experience

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INTRODUCTION

Family planning and related services are essential not only for planning family size but also for maintaining reproductive health and obtaining related preventive care that women need regardless of pregnancy status. Access to, and effective use of contraceptives are important to the Healthy People 2010 goal that all pregnancies be intended (Healthy People 2010, Family Planning). Public funding for these services may be critical to reduce access barriers for low-income women (Sonfield, 2006; Frost, Frohwirth and Purcell, 2004) and to address long-standing disparities in reproductive health outcomes.

Approximately half of all pregnancies are reported to be either unwanted or earlier than desired (Finer and Henshaw, 2006). The adverse consequences of unintended pregnancy likely include later entry into prenatal care, maternal smoking during pregnancy and potentially, other poor outcomes (Kost, Landry, and Darroch 1998; Sable and Wilkinson 1998; McComb Hulsey 2001). A recent review indicates less evidence of effects on maternal risk behaviors, pregnancy outcomes and preventive (well-baby visits) or curative care for infants but consistent evidence of less breastfeeding, lower child nutrition and increased infant mortality among infants who are the result of unintended pregnancies (Gipson, Koenig and Hindin, 2008). While the U.S. has implemented a series of policies over the 1980s and 1990s in an effort to expand insurance coverage and access to prenatal care for low-income pregnant women, the U.S. continues to fall behind other industrialized nations in terms of sustaining or accelerating reductions in infant mortality rates or reducing disparities in birth outcomes (Hogue and Vasquez, 2002; Hessol and Fuentes-Afflick, 2006).

The Medicaid eligibility expansions in the mid 1980s provided insurance coverage to new groups of pregnant women, resulted in earlier Medicaid enrollment (Ellwood and Kenney, 1995) and improved prenatal care timing (Dubay et al., 2001) but did not reduce rates of low birth weight (Dubay et al., 2001; Howell, 2001). The limited effect of these policies may have been due in part to enrollment of women at delivery or late in pregnancy (Ellwood and Kenney, 1995). The effects of the implementation of managed care in Medicaid programs in the1990s appear to be mixed with respect to prenatal care timing and birth outcomes (Sommers, Kenney, and Dubay, 2005; Kenney et al. 2005; Tai-Seale et al., 2001; Howell et al., 2004; Duggan, 2004).

In addition, it appears that federal welfare reform may have led to reductions in insurance coverage among low-income women (Kaestner and Kaushal, 2003) and even among new mothers (Adams et al, 2005).

An important insight from the US experience with these past policies is that programs aimed at women only *after* they are pregnant may have only limited effects on birth outcomes, especially since such a large proportion of births appear to be unintended. A natural evolution in policy has now occurred as many states have expanded eligibility for Medicaid coverage of family planning services independent of pregnancy status and for women with higher income levels by obtaining 'waivers' from the Centers for Medicare and Medicaid Services (CMS). As of 2009, 27states had family planning waivers in place most of which made women (and in eight states, men) newly eligible for family planning services on the basis of income.

In this study we examine the effect of a family planning waiver implemented in Arkansas in 1997 which made women with incomes up to 133% of the FPL who were not previously eligible for Medicaid, eligible for family planning and related services such as STD or other clinical screening paid for by the Medicaid program. We use survey data from the Pregnancy Risk Assessment Monitoring System (PRAMS) to ask:

- Did the waiver change the use of family planning and contraceptive services such that there is a reduction in the percentage of births reported as unwanted or mistimed among women affected by the waiver versus those not affected?
- Are there differential changes in intrapartum intervals or contraceptive use in the pre versus post-partum period?
- Are teens, a group with a high rate of unintended pregnancy, affected by the expansion of subsidized family planning services under the waiver?

In addressing these questions we use the experience of a comparison group to assess the impacts of the expansion of family planning services in Arkansas.

Earlier Studies

Two recent studies (Lindrooth and McCulough, 2007; Kearney and Levine, 2007) used difference-in-difference technique to evaluate the effects of family planning waivers on

contraceptive use and birth rates by testing for a difference in the pre/post changes in states with versus without, family planning waivers. National natality files were used by Lindrooth and McCullough (2007) to show that between1994-2001birth rates among *all* women declined more in states with family planning waivers than in those without them. The rate of decline in population birth rates was greater in states using family planning expansions based on income than in those based on only postpartum status.

The Lindrooth study did not explore the causal pathway from the waivers through increased receipt of Medicaid family planning services to increased contraceptive use. To address this gap, Kearney and Levine 2007 examined the impact of family planning waivers between 1990 and 2003 using Medicaid Statistical Information Systems (MSIS) data, natality files and data on contraceptive use from the National Survey of Family Growth (NSFG). They found a 5 to 10 percentage point increase in the share of women ages 15-44 receiving Medicaid family planning services based on a difference in difference type of estimator and reductions in *births* for non-teens of 2% and among teens, of over 4%, in states with family planning waivers versus those without. Using income data in the NSFG to define 'newly eligible' treatment and non-eligible control groups in states with income-based waivers versus non-waiver states, they found that 5% fewer adult women failed to use contraception at their last intercourse and around 3% fewer adult women had unprotected sex in the last three months.

These national studies using natality files assert that the reductions in *total* birth rates are indicative of reduced unintended births associated specifically with waivers. Yet, the natality data cannot be used to identify those who are eligible for services under a family planning waiver. Hence, the identification of the effect measured from data on *total* births appears to rely on the assumption that the birth rate among those not affected by the waiver does not change over time nor vary across states with and without family planning waivers.

One study (Edwards et al 2003) used the Pregnancy Risk Assessment Monitoring System (PRAMS) to look specifically at unintended pregnancy. These authors examined differences in the change in unintended births and intrapartum intervals in Florida and South Carolina, states with family planning waivers, relative to two states without such waivers (Washington and West

Virginia) over the pre/post waiver periods. They identified women who would be eligible under pregnancy expansions in each of the study states for their analysis. This empirical work indicated that while the differences in changes between the waiver and control states' samples were generally in the expected direction they were largely insignificant with the exception of a lower percentage unwanted births (p =.001) in FL versus WA.

The Edwards et al results were not published and were based on only two waiver states. The published results from the natality data cannot test directly the outcomes of unintended pregnancy, intrapartum intervals or use of contraceptives postpartum. Given the potential adverse outcomes associated with intendedness of pregnancy, it is important to analyze this broader set of outcomes among the women who are made newly eligible by family planning waivers and in turn, to examine individual states' experiences. It is quite possible that the national results are driven by California's very large family planning waiver.

Budget Neutrality. A key expectation of these waivers is that they would be budget neutral—i.e., that states save public dollars by 'averting' births that would otherwise be covered by Medicaid (Edwards, Bronstein and Adams, 2003). However, as states estimate budget neutrality they calculate an 'expected birth rate' for those newly eligible based on historically observed birth rates for *all* Medicaid enrollees or for expansion-eligible women, neither of which necessarily match the income range of the newly eligible. This rate is multiplied times the current number of women in the newly eligible population to estimate 'expected births' and 'averted births' are then the difference between the 'expected' and actual births occurring among *participants* in the waiver. Since participants are women who not only enroll in the waiver but also use services they may be women who would have used contraceptives effectively in the absence of the waiver and hence, the estimate of 'averted births' due to the waiver may be overstated. Moreover, other factors, such as economic conditions affect the year-to-year number of Medicaid births making historic rates highly variable; a reduced number of participants/births may simply reflect lower need/take-up of Medicaid.

Instead of taking the retrospective view of Edwards et al, Frost et al (2006) used data from the National Survey of Family Growth (NCFG) on changes in the contraceptive *use rate*

and *mix* of contraceptives for two populations of women representing 'before' and 'after' a family planning waiver expansion. The 'before' group included: uninsured, sexually active income and age- eligible women, able to get pregnant but not currently pregnant, postpartum or seeking pregnancy, while the 'after' group included women using publicly funded providers. Using a 'take-up' rate from eight states' waiver data and method-specific failure rates, they simulate future contraceptive behavior and hence, 'averted' pregnancies due to the waiver. They estimate a reduction of 144.3 unintended *pregnancies* of every 1,000 women participating in the program and roughly 48% of these are counted as 'averted births'. Despite the differences in methods, their estimated rate of 'averted births' among participants is comparable to that of Edwards et al, ranging from 6-8% for Arkansas specifically (Frost et al, 2006). The actual number of 'averted births' estimated by Edwards et al (2003) for Arkansas ranged from 2,748 to 4,486 in the first years of the waiver.

Arkansas Background

The Arkansas Medicaid Family Planning Demonstration Waiver which was implemented September 1, 1997, expanded family planning services to women with children with incomes between the welfare/TANF eligibility level and 133 percent of the FPL and to all childless women below 133 percent of the FPL (see Table 1). At the time of the waiver, the 133 percent FPL cut off was consistent with the eligibility level for pregnant women. Subsequently in 2003, the income eligibility level for both the family planning and pregnancy group has been raised to 200% FPL.

Table 1. Eligibility Levels for Medicaid and Family Planning Waiver In Arkansas

	Pre-Family Planning Waiver	**Post-Family Planning Waiver (September 1997)**	**Policy Change/Implementation Dates and Information**
Parents	18.9% FPL[i] (welfare level)	133% FPL in 1997; raised to 200% FPL in 2003 and currently at 200%FPL[ii]	Eligibility for the waiver was raised to 200% FPL on August 1, 2003[iii] and is currently at 200% FPL
Childless Adults	0	133% FPL in 1997; raised to 200% FPL in 2003 and currently 200% FPL	
Pregnant Women	133% FPL[iv]	133% FPL[v]	Eligibility for pregnant women increased to 200% FPL effective November 1, 2001[vi]

The family planning waiver was implemented due to concerns about high rates of teen pregnancy in Arkansas and while the state had already expanded coverage to teens (through age

18) up to 200% FPL, the waiver effectively made all teens above 200 percent FPL eligible since determination is made on the basis of the teen's own income (Arkansas site visit and interviews). Arkansas staff report: "if services are requested for a minor who is living with her parents, the income of the minor's parents will be disregarded. The minor will be budgeted on a separate application with only her children, if any, and the father of her children, if living in the home.

The Arkansas waiver (now called Women's Health Waiver) uses predominantly local health departments and Title X providers to serve women and most outreach was by word of mouth (Arkansas site visit and interviews). As part of our broader study, we conducted focus groups and these findings confirmed that many women learned about the waiver through word of mouth and that many did not understand the renewal process. This is consistent with the Edwards et al (2003) evaluation which indicated that the availability of private providers increased immediately after the waiver but that the flow of clients/dollars to private providers did not dramatically increase.

Arkansas Department of Health (DOH) has also conducted assessments of the waiver using primarily a pre/post analysis of key measures (Arkansas DOH, 2003) and also report a decline in active private providers back to levels seen in the initial waiver year. Using Medicaid Management Information System (MMIS) and the Arkansas Networked System for Welfare Eligibility and Reporting (ANSWER) the DOH reported a decline in the percentage of Medicaid births that are first births, consistent with the expansion to childless adults; they also found some evidence that the age at first birth increased.

Data and Methods

The Pregnancy Risk Assessment Monitoring System (PRAMS) is a state-level, population-based surveillance system that assesses maternal behaviors, experiences, and insurance coverage before and during a woman's pregnancy and during the early infancy of her child (http://www.cdc.gov/prams/). A sample of women with a recent birth is drawn from states' birth certificate records with women at higher risk of poor birth outcomes sampled at a higher rate. Selected women are contacted with a mailed-in questionnaire, and in some cases of non-

response, followed up with by phone. Sample weights are provided for researchers but data are made available only if the state achieves a 70 percent response rate.

Data from Arkansas PRAMS Phase 3 and Phase 4 covering births from 1997-2003 were consistently recoded to carry out our empirical analysis. The outcome measures we focused on included: 1) intra-partum period - number of months from birth to birth or birth to conception-expressed as a continuous measure and a set of indicator variables < 12 months or < 18 months; 2) age at first birth; 3) unintended pregnancy, including respondents that did report not wanting to get pregnant as well as those that would have preferred to be pregnant later (mistimed); 4) contraceptive use pre-pregnancy and post partum, excluding the small percentage who were trying to get pregnant again.

Treatment and Comparison Groups. Our analytic approach is to estimate the impacts of the waiver by contrasting changes in the outcome variables for women in the treatment group (i.e., those made newly eligible for family planning services under the waiver) to those in a comparison group not affected by the policy change. This allows us to examine if there were improvements in outcomes among women in the treatment group relative to the comparison group(s)—the so-called difference-in-differences method.

The categorical nature of the income data in PRAMS means, however, that we could not identify the women targeted by the family planning waiver (between 19% and 133% FPL) with certainty. Likewise, we found that using data within Arkansas, women above 133% FPL were so different from those < 133% FPL in terms of outcomes and underlying characteristics that they did not appear to be a valid comparison group. Finally, given that the treatment group of teens spans all incomes, it was not possible to construct a higher income group within-state comparison group for them.

Because of these concerns, the analysis we present here uses more broadly defined treatment groups in Arkansas and similarly defined comparison groups drawn from three other states (Oklahoma, North Carolina, and West Virginia) that were fairly similar to Arkansas in terms of both the outcomes and the characteristics of the treatment group but which did not adopt

a family planning waiver over our analysis period. We present results for two adult treatment groups: 1) births to all women with a Medicaid-covered delivery; 2) nulliparous women who were 20 or older. Since the Arkansas waiver potentially expanded family planning services to all teens through age 19 and because teens who already qualified for Medicaid might be more likely to get family planning services with a waiver because of the greater confidentiality afforded to them, our third treatment group is all teens.

Appendix Table 1 shows the characteristics for women in Arkansas and the three comparison states for each of the three groups examined (Adults 20 and over with a Medicaid delivery, Nulliparous adults 20 and over, and teens.**)**. As these data show, the patterns are fairly consistent for Arkansas and the three comparison states with respect to maternal race, education, employment status, income risk factors, and marital status though Arkansas has proportionally fewer women who are Hispanic, more with a high school diploma and a higher proportion who reported smoking.

Statistical Methods

The effect of the Waiver is assessed by evaluating a series of models where the probability of an outcome is captured as a function of a "Treatment" indicator variable (whether the woman is in the treatment or the comparison group), a "Post" waiver dummy (whether a record is drawn from pre or post waiver period), the interaction of the two terms (whether the outcomes changed for the treatment group differently than for the comparison group, pre and post the implementation of the waiver), and a set of socio-economic and health related controls. The coefficient on the interaction term provides the difference–in-differences impact estimate which has been used extensively in the literature to estimate the effects of public policy changes (Kaestner and Kaushal, 2003; Kenney et al., 2005; Dubay et al., 2001; Adams et al., 2005). It is important to note that for all outcomes except for post partum birth control the waiver pre-period includes data from January 1997 through -July 1998; this incorporates a 10 month lag from the initiation of the waiver since pregnancies already under way could not have been affected by the waiver. The post waiver period then, is August 1998 through December 2003. For post partum birth control the pre and post periods span January through November 1997 and December 1997 through December 2003, respectively.

Given the non-linear nature of the dependent variables, we estimated multivariate logit models for each outcome and reported our findings in terms of marginal effects. The estimated models took the following form:

$$P_{it} = \alpha_t + \xi X_{it} + \lambda P_t + \sigma T_i + g P*T + \mu M_{it}$$

Where:

P_i = probability of outcome for i^{th} woman with birth in t^{th} month;
X_i = vector of individual characteristics (e.g., parity, age, race, education, etc.);
M_{it} = dummy variable to indicate month t for i^{th} woman/birth;
P_t = dummy variable to denote pre/post Waiver based on month of birth;
T_i = dummy variable to denote i^{th} woman is in treatment or comparison group; and
P_t*T_i is the interaction term.

The vector of independent variables include: 1) maternal age [< 20, 25-34, 34+], 2) race/ethnicity [black non-Hispanic, white non-Hispanic, other], 3) maternal education [< high school, high school graduate, some college, college graduate,] 4) worker [yes/no] 5) urban status [urban metro, urban non-metro, rural] 6) smoker [yes/no] and an indicator for missing smoking data (specifications evaluating effect of the waiver on birth control use post-partum control for smoking status post-partum, while all in other specifications smoking indicator if defined as smoking pre-pregnancy) 7) prior low birth weight 8) prior pre-mature birth 9) abuse [pre pregnancy only, during pregnancy, pre and during pregnancy] and 10) number of 'stressors' during the 12 months before the birth.. Examples of stressors include job loss, death in family, separation or divorce, unable to pay bills, etc. We also use data on income, income sources, insurance status and family size in defining some of the treatment and comparison groups analyzed. We tested morbidity measures including hypertension, and diabetes as risk factors and found our results to be robust; the latter are not reported here due to concerns with endogeneity.

In addition, we use a propensity-score method to bring the distributions of the control variables closer across the treatment and comparison groups, because of concerns that characteristics of women that are correlated with the outcome may differ between the treatment and comparison groups, (Rubin, 1997). Specifically, we estimate a first-stage model of whether a woman is in the treatment group and use the predicted probabilities from this stage to create

appropriate factors to use in re-weighting the observations for women in the comparison states. We then re-estimate the difference-in-differences estimator using re-weighted samples, following recent research (Long et al., forthcoming; Shen and Zuckerman, 2006; and Garrett and Zuckerman, 2005).

The models were evaluated using Stata 9 SE taking into account complex survey design of the PRAMS data. Standard errors for the interacted indicators were adjusted following recommendations proposed in Ai and Norton (2003**).** Using this software and the above modeling approach we conducted **e**xtensive sensitivity analysis. For example, all regressions shown here were also run on 'exact' adult treatment groups (adults and in turn, just nulliparous adults between 19% and 133% FPL, Medicaid delivery and non-welfare). In these runs, the majority of the DID effects were insignificant but where significant, similar to those presented here; the smaller sample sizes of these 'exact' groups limited the power of this analysis. Finally, given that there may be pre-existing differences in our treatment and control groups across states that the DID method does not account for we also tested DDD models. These models test, for example, whether the differences between Medicaid and private insured in Arkansas change differently than the differences between these groups observed in other states as we moved from the pre to the post period? While the direction and magnitude of the effects seen for nulliparous were similar to those presented here, they were not significant at $p < .05$.

Results

Descriptive Analyses. One factor that can affect outcomes of a waiver is the percentage of the eligible population who 'take up' the benefit by enrolling**.** Based on our interviews with Arkansas staff, initial enrollment in the waiver was a little over 35,000 and increased to almost 60,000 in 1998; enrollment peaked in 2003 at 92,000 and current enrollment is about 62,000. Using enrollee counts and Current Population Survey (CPS) data on women income eligible Edwards et al (2003) estimated a take -up rate under the Arkansas Waiver of 20% in 1997, growing to 54% by1999. The percent of eligible women enrolled *and using* services in the waiver, however, was estimated at only 12% in the first and 22%, in the third year of the waiver; this reduction reflects both the reduction in take-up of benefits and percent of enrollees actually using services in the waiver (Edwards et al, 2003). One reason women may not use waiver

services is the availability of free or reduced price services through Title X. However, this does not appear to explain the low use of services under the waiver in Arkansas. The data in Table 2 summarize spending through Medicaid and Title X over the pre/post waiver period. As these data show, there is a four-fold increase in total Medicaid spending on family planning beneficiaries (waiver and non-waiver) as the waiver begins between 1997 and 1998. Medicaid family planning spending per women < 150% FPL also increases (from $3.17 to $14.18) over these years with a corresponding increase in Medicaid revenues flowing to Title X clinics (from $2.39 to $11.50). While there is a slight decline in Title X spending at first, Title X spending stabilizes and increases; there was also an initial shift away from Title X providers for women < 150% (and a corresponding increase in higher income women) but this too, reverses to pre-waiver levels. Thus, it appears that there was little displacement of Title X funding or service use.

Table 2. Birth & Abortion Rates, Poverty Status (% Users of Title X/FPL), & Medicaid FP Spending by Year

AR	MEDICAID FAMILY PLANNING PROGRAM Family Planning Waiver**		Birth Rate*	Teen Birth Rate***	Abortion Rate^	USERS OF TITLE X POVERTY STATUS @			Total Medicaid FP Paid $$$/ Women < 150% Poor Woman	Medicaid Revenue at Title X Clinics $$$++/ Women < 150% Poor Woman	Total Title X $$$++/ Women < 150% Poor Woman
	Unique Beneficiary Count	Total Family Planning Medicaid Paid Amount				Percent Users < 150% FPL	Percent Users > 150% FPL but < 200% FPL	Percent Users > 200% FPL			
1995	18,204	$1,912,200	13.9	73.2	9	--	--	--	$3.36	$2.04	$4.18
1996	19,667	$2,062,953	14.1	75.1	11	88%	7%	4%	$3.55	$1.82	$5.35
1997	16,832	$1,853,557	14.0	72.5	11	85%	7%	8%	$3.17	$2.39	$4.61
1998	50,089	$8,389,414	14.0	70.6	10	78%	7%	15%	$14.18	$11.50	$4.56
1999	62,477	$11,872,808	13.9	68.1	11	85%	9%	6%	$20.87	$14.20	$5.27
2000	64,678	$13,154,753	14.1	66.1	10	85%	9%	6%	$24.51	$18.19	$6.32
2001	67,830	$13,904,900	13.7	63.4	11	85%	9%	6%	$25.28	$18.59	$6.53
2002	72,465	$15,306,219	13.8	61.3	9	86%	8%	6%	$27.16	$18.85	$6.64
2003	78,627	$17,163,086	13.9	60.4	10	86%	8%	6%	$29.60	$17.51	$6.43
2004	86,664	$19,281,175	14.0	61.6	8	86%	8%	6%	$33.60	$18.49	$7.26
2005	91,508	$18,291,298	14.1	60.1	8	93%	--	7%	--	--	--
2006	87,395	$19,939,631	14.6	62.3	--	93%	--	7%	--	--	--

* CDC estimate: http://205.207.175.93/VitalStats/TableViewer/tableView.aspx

** Medicaid Statistical Information System (MSIS) State Summary Datamart: Tables by FP Program

*** NCHS; Birth Rate for 15-19 year olds:

^ Abortions per 1,000 women (15-44 yrs). # women in this age group obtained from U.S. Commerce Dept., Census Bureau (special unpub. tabulations)

^^ Centers for Disease Control and Prevention. National Center for Health Statistics. VitalStats. http://www.cdc.gov/nchs/vitalstats.htm

[@] Annual FPAR Demographic Profile Reports (1996-2005) Unknown FPL users omitted from computation.

++ data from prior FP OPA Regional Directors: FPAR Revenue Data (1)_July_01_03_95-02.xls: http://www.hhs.gov/opa/familyplanning/rcontacts/index.html

Note. "--" in cell indicated data unreported

These descriptive data on Arkansas also indicate fairly stable birthrates over the period shown in Table 2 (1995-2005). We note that over our pre-post period the teen birth rate in Arkansas fell from a high of around 73 to 61 births per 1,000; the percent of Medicaid births that were to teens also went down. Although the abortion rate was lower in the 2002 to 2006 period than it was between 1995 and 1998 before the waiver was implemented, we cannot attribute this change to the waiver.

Table 3 shows PRAMS data on the changes in Arkansas and the comparison states for six key outcomes before and after the implementation of Arkansas' waiver for the three treatment groups noted above. For all three groups in Arkansas, the rates of unintended births were high both before and after the implementation of the waiver, with the highest rates reported for teens. Even after the implementation of the family planning waiver, 74 percent of births to teens and 63 percent of Medicaid-covered births to women over 20 were reported to be unintended in Arkansas. High rates of unintended birth were found in the three comparison states as well. In both Arkansas and the comparison states, most of the unintended births were pregnancies that were wanted later, but close to one in every five Medicaid-covered births to adult women were reported to be unwanted altogether.

Consistent with the relatively high rates of unintended birth are low rates of birth control use at the time of pregnancy in both Arkansas and the comparison states (not shown). In both Arkansas and the comparison states, less than one third of adults and only around one third of teens giving birth were using birth control pre-pregnancy. The reported rates of birth control use post-partum appeared to have risen for all three treatment groups in Arkansas, although the increases do not appear to be statistically significant, due perhaps to the small sample sizes in the baseline period. After the waiver, close to 85 percent of Arkansas women, including teens, were reporting that they were using birth control post-partum. Post-partum birth control use in West Virginia (the only comparison state with post-partum data) was also similarly high, but did not show the increase that was found for Arkansas teens following the implementation of the

Waiver. Likewise, the length of the intra partum intervals appeared to increase in Arkansas but the increases were not statistically significant.

Multivariate Results. The results in Tables 4, 5, and 6 show the difference-in-difference impact estimates with and without propensity score reweighting for each of the three treatment groups. While we defined these treatment groups without an income constraint (other than a Medicaid delivery for all adult births) we used income as a key independent control variable in the regressions.[1] The estimates in Tables 4-6 represent the estimated change in the probability of each outcome holding all other characteristics at the mean; in other words, these represent the Waiver attributable change in outcomes examined for an "average woman" affected by the expansion in Arkansas. These are derived as predicted probabilities for each woman and the marginal effect presented represents the change for an "average woman". As indicated earlier, the impact estimates were robust with respect to alternative specifications (including a DDD specification). Overall, very few of the impact estimates are different from zero. In particular, while signs on the intrapartum birth intervals and use of birth control in the post-partum period are in the expected direction, the signs on unintended pregnancy are not always negative and no effects were statistically significant for adult women delivering on Medicaid. As shown in Table 4 the DID estimates indicate intrapartum intervals increased by around 3 months for all but are insignificant ($p > .20$). In our sensitivity analysis of adult women under 133 percent of the FPL, Medicaid delivery and not on welfare (not shown), our 'exact' treatment group, the odds of having an unintended birth was negative but only at $p = .13$.

As seen in Table 5 for the nulliparous women age 20 and above, the DD estimates with and without propensity score reweighting indicate that the implementation of the family planning waiver in Arkansas reduced the share reporting that their birth was not wanted by about 4 ($p = .06$) and 5 percentage points ($p = .045$), respectively. For teens (Table 6) the effect on unwanted pregnancy is from 6 ($p = .09$) to almost 8 percentage points ($p = .02$). However, for teens, the

[1] We note that many other independent variables were significant and largely in expected directions. For example, married women were less likely to have unintended (unwanted or mistimed) births while greater stress was positively associated with a greater likelihood of unintended births. Black non-Hispanic women were more likely than white non-Hispanic women to report unintended or unwanted [but not mistimed] births while women with lower income were more likely to report unwanted and mistimed births. Older age and more education were associated with lower rates of unintended births. What about covariate patterns on other outcomes?

impact estimates also indicate a counterintuitive effect of an increase of 15 to 17 percentage points in the share of teens reporting that they wanted the birth later at highly significant levels ($p = .001$). Yet, the impact estimates also indicate the expected result that the share of teens reporting post-partum birth control use increased by 8 percentage points ($p = .05$) due to the Arkansas family planning waiver.

Limitations

While this analysis is the first to use a direct measure of unintended pregnancy as an outcome in analysis of the impacts of the implementation of a family planning waiver, there are numerous limitations to our study. As noted in our methods section, the inability to identify within state control groups led us to use comparison states to provide an appropriate counterfactual for what would have happened in Arkansas without the waiver. This means we are comparing women across states whose populations may have very different views on birth control, contraception and unintended pregnancies that are not captured by observed characteristics we explicitly control for (for instance, the Native American women in Oklahoma). In creating the comparison group, we do as much as we can to find 'similar women' by restricting to a quite narrow income range and on Medicaid at delivery as well as by using other control variables and propensity scoring to reweight the sample. However, we do recognize that despite these adjustments, there are innate cultural differences that affect these important decisions for which we cannot control.

In addition, while the three comparison states did not institute any major Medicaid expansions over the time frame we are analyzing, all three did expand coverage to teens under the Children's Health Insurance Program up to 200 percent of the FPL (Kaiser Family Foundation.) As indicated above, Arkansas was already covering teens at that income level in 1997. The expansion of public coverage to teens in the comparison states would likely bias our teen impact estimates downward. While we would like to test the models for only teens above the 200% FPL cut-off now common to these states, our sample size was not sufficient. We note that there were not significant changes in the.pre-pregnancy coverage status of teens giving birth in Arkansas to those in the comparison states pre and post the waiver period.

We are limited by the categorical nature of the PRAMS income data and further mis-measurement in that it reflects income over the past year and it is income at the time of application that is used to qualify women for Medicaid services. Our analysis is also constrained by the data availability for Arkansas in PRAMS leaving us with smaller than desired sample size at baseline. This likely affects our power to detect overall effects and certainly limits our ability to look at sub-populations of interest such as racial/ethnic groups. We also note that there is inevitably a measurement error in the treatment group which could bias down our estimates if the 'treated' includes non-treated women.

We were also not able to control for some important characteristics, such as citizenship, which would affect a woman's eligibility for and tendency to seek, Medicaid benefits; our use of the 'all adult Medicaid deliveries' as one treatment group does partially get around this problem. Finally, our analysis, as well as all others to date, does not address the potential for Medicaid family planning waivers to reduce unintended pregnancies as opposed to unintended births. While the descriptive data indicate that abortion rates declined following the implementation of the waiver, which suggests that the rate of unintended pregnancy may have been reduced, we cannot attribute this to the waiver policy. Additional research is needed to assess the impacts of Medicaid family planning waivers on these other outcomes.

Discussion

Our findings indicate that the Medicaid family planning waiver introduced in Arkansas decreased the percentage of births that were unwanted among the nulliparous and teen treatment groups. There also appeared to be a positive effect on teens' use of birth control post partum related to the family planning waiver. This suggests that there might be a subsequent, longer-run reduction of repeat teen pregnancies or increases in inter-partum birth intervals due to this changed behavior among teens that we are unable to observe. While these findings suggest positive effects of the waiver, we did not find consistent impacts across all three treatment groups and we found increases in reports of mistimed pregnancies among teens associated with the waiver, which we cannot fully explain.

One explanation for the lack of large, consistent impacts on the extent to which women targeted by the family planning waiver reported that their births were either unwanted or wanted later could be that take up in the family planning waiver, as noted earlier, was relatively low in the early period following implementation. Moreover, use of family planning services among those enrolled in the waiver also appeared to be low (Edwards et al, 2003) suggesting that many women who sign up for services do not actually access them through the waiver. This does not appear to be a provider capacity issue as access was improved with the waiver (Edwards et al, 2003) but there may be other barriers such as confusion regarding recertification, service coverage or other personal barriers preventing utilization. Unless the waiver reaches more eligible women with services in Arkansas on a continuous basis, it will not be able to have a substantial, sustained effect on the pregnancies and ultimately, on births.

While the percentage reductions in unwanted births were only found among the nulliparous and the teen treatment groups, the implied number of births 'averted' may be within the range of program impacts estimated by others (Kearney and Levine; Edwards et al, 2003). For example, Kearney and Levine's results suggest that waivers reduce (unwanted) births by 2%, which in Arkansas would translate into about 670 births (2% of Arkansas's 33,551 total births pre-waiver, in 1997). Our finding that the waiver reduced unwanted births by 5% among nulliparous women, suggests that the waiver averted 727 'first births' in 1997, quite similar to the number implied by Kearney's analysis. We note this is a lower bound estimate since we have not included the effect on unwanted births we found for teens. It also appears to be within the range of budget neutrality, albeit at the low end of the range that Edwards et al note as budget neutral. They conclude that 20 to 25 'averted births' per 1,000 waiver enrollees are needed for budget neutrality [Tables 1.3.1a and b] and given that the state reports enrollee levels in the waiver of between-35,000 to 58,000 enrollees (Edwards et al, 2003) 700 to 1,450 'averted births' would be consistent with budget neutrality for AR.

While states may anticipate the effect of waivers to be on adult women made newly eligible, the larger effect may be among teens, if national studies are correct (Kearney and Levine,). Our results for teens are both positive and negative. A reduction in unwanted births

accompanied by an increase in post-partum birth control use could help the state to lower its teen pregnancy rate. However, we also find the waiver was associated with a larger percentage of teens reporting mistimed births. Whether this reflects changed expectations and mores as more teens were able to access contraceptive services and were in contact with providers that perhaps discouraged teen child bearing is hard to say. It also raises the issue of the definition and measurement of 'unintended' pregnancy. As Trussell et al (1999) note there are contradictions in women's reporting; for example among women reporting their pregnancy was due to contraceptive failure, only 68% reported it as unintended and of the 32% reporting it as intended, 90% were happy or very happy. These issues may be particularly germain to teens who may in general be more inclined to report a birth as mistimed rather than unwanted.

Finally, our findings indicate that the rates of unintended births remain high among low-income women in Arkansas and the comparison states that are examined. We found rates are particularly high for teens, unmarried women, and for women with low levels of education (not shown). These findings suggest that other interventions may be needed beyond Medicaid family planning waivers to achieve significant reductions in unintended childbearing. As the national debate continues regarding health care reform it will be important to monitor the implications for not only access to affordable family planning services but also for the take-up and effective usage of these services by women at risk of unintended pregnancies.

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[i] National Governors Association, Center for Best Practices, “MCH Update September 1996: Medicaid Coverage of Pregnant Women
and Children,” Issue Brief, September 1996.

[ii] CMS documents. See specifically the letter from the Arkansas Department of Human Services, Division of Medical Assistance, to CMS dated June 2002, the Implementation Schedule for Family Planning Waiver Extension, and letter from the Arkansas Department of Human Services, Division of Medical Assistance, to CMS dated February 27, 2003 accepting the Terms and Conditions.

[iii] See Evaluation of the Women’s Health Waiver, A Final Report to the Division of Medical Services of the Arkansas Department of Health and Human Services
Prepared by the Fay W. Boozman College of Public Health at the University of Arkansas for Medical Sciences, Submitted October 31, 2006, Revised August 24, 2007, Little Rock, Arkansas; see also informant interview notes

[iv] National Governors Association, Center for Best Practices, “MCH Update September 1996: Medicaid Coverage of Pregnant Women
and Children,” Issue Brief, September 1996.

[v] National Governors Association, Center for Best Practices, “MCH Update 1997: State Medicaid Coverage of Pregnant Women and Children,” September 1997.

[vi] See Report to the Arkansas General Assembly and Governor Tobacco Settlement Proceeds Act of 2000, Program Performance and ATSC Recommendations
Submitted by the Arkansas Tobacco Settlement Commission, October 1, 2004;
http://www.atsc.arkansas.gov/pdf/biennial_report2004.pdf