REEXAMINING THE IMPACT OF U.S. FAMILY PLANNING PROGRAMS: EVIDENCE FROM THE WAR ON POVERTY, 1964-1973

Martha J. Bailey

October 10, 2010

Abstract:
This paper examines the impact of U.S. family planning programs begun from 1964 to 1973. Using variation in the timing of *first* federal grants to U.S. communities, the analysis shows that (1) fertility, infant and maternal mortality rates evolved similarly in funded and unfunded communities before the award and that (2) fertility (but not infant or maternal mortality) fell more rapidly in funded counties following the award. Although federal family planning grants explain only 8 percent of the 1959 to 1974 decline in fertility, they reduced the 3-year likelihood of having a child among family planning patients by 30 percent.

Contact Information:
Department of Economics, University of Michigan, 611 Tappan Street, Ann Arbor, Michigan 48109; baileymj@umich.edu; Website: www-personal.umich.edu/~baileymj.

Acknowledgements:
This project was supported by the National Science Foundation (Grant 6334659), the National Bureau of Economic Research's (NBER) Dissertation Grant for the Study of the Nonprofit Sector (2003-2005); the University of Michigan's National Poverty Center (NPC) and Robert Wood Johnson Health and Society Programs; the University of Michigan Population Research Center's Eva Mueller Award; and the National Institute of Health (Grant HD058065-01A1). I also received grant support from the University of Kentucky Center for Poverty Research (UKCPR) through the U.S. Department of Health and Human Services, Office of the Assistant Secretary for Planning and Evaluation (Grant 5 UO1 PE000002-05). The opinions and conclusions expressed herein are solely those of the author and should not be construed as representing the opinions or policy of the NBER, NPC or UKCPR or any agency of the federal government. I am grateful to Doug Almond and Hillary Hoynes for sharing the Regional Economic Information System (REIS) data for the 1959 to 1978 period and to the Guttmacher Institute and Ted Joyce for sharing information on abortion providers from 1973 to 1979. I also thank John Bound, Nzinga Broussard, Charlie Brown, John DiNardo, Daniel Eisenberg, Hilary Hoynes, David Lam, Ron Lee, Daniel Leeds, Paul Rhode, Mel Stephens, and Jeff Smith for helpful comments and suggestions. Exceptional research assistance was provided by Andrew Goodman-Bacon, Allie Davido, Nic Duquette, Emily Collins, Brad Hershbein, and Jessica Williams.

In 1964, the world population growth rate reached an unprecedented 2.2 percent—a rate implying that population would double faster than at any time in 20th century and, probably, human history. This "population explosion" was driven both by rapidly declining mortality and increasing fertility during the Baby Boom. In the U.S. alone, the total fertility rate reversed its longer-term downward trend to rise by over 50 percent from 1940 to 1957. Fearing increases in poverty and mass starvation, social scientists and government officials turned to new contraceptive technology and family planning policies to curb birth rates. Since 1960, birth rates around the world have fallen to historic lows and governments continue to fund family planning as part of broader development and anti-poverty strategies.

But did family planning policies really cause the reduction in birth rates? Social scientists have fiercely debated this question since at least the 1960s. Provocative articles in *Science* with titles like "Population Policy for Americans: Is the Government Being Misled?" (Blake 1969) or "Family Planning and Public Policy: Who is Misleading Whom?" (Harkavy, Jaffe and Wishik 1969) featured this early controversy. A more recent example debates the effects of international family planning policy using cross-country comparisons (Lant Pritchett 1994a, 1994b; John Bongaarts 1994; and James Knowles, John Akin, and David Guilkey 1994).

For both theoretical and empirical reasons, economics has focused on the demand-side of this debate and downplayed the importance of family planning policy. This focus is implicit in the discipline's workhorse theoretical models, which limit the role of family planning policy through two simplifying assumptions: (1) households choose the number of children *with certainty* and that (2) *preventing births is costless*.[1] The discipline's demand-side focus also reflects the absence of definitive empirical evidence of family planning policies' effects. Small randomized-controlled trials often lack the power to distinguish even large effects and the money to follow participants over the longer term. Moreover, observational estimates are difficult to interpret as causal effects in the absence of random assignment. In their *Handbook of Population Economics* chapter on fertility in developed countries, Hotz, Klerman and Willis (1997) conclude that "the crucial challenge is to find plausibly exogenous variation in proxies for price and income concepts appearing in the theories [of childbearing]." Joshi and Schultz (2007) echo this sentiment in their call for better evidence on family planning policy saying that "few studies have identified the impact of these policies on the fertility and health of women" due to the rarity

[1]Two notable exceptions include Michael and Willis (1976) and Rosenzweig and Schultz (1985).

of estimates "based on sources of variation in fertility induced by policies that are independent of parent preferences and preconditions" (1). Recent quasi-experimental studies have advanced the literature on the longer-term effects of family planning programs in developing countries (Bangladesh: Joshi and Schultz 2007, Iran: Salehi-Isfahani et al. 2008, Colombia: Miller 2005), but the effects of these programs in developed countries like the U.S. remains an open question.

This paper reevaluates the effects of family planning programs in United States begun in the 1960s and 1970s. Section I begins with a simple model that relaxes the assumption that preventing births is costless. In this framework, opposing income and substitution effects challenge the conventional wisdom that family planning policies necessarily reduce births. Crucially, the impact of family planning policies depends upon the pre-existing equilibrium number of children born and, therefore, is likely to interact with the level of development of the community in question.

The paper, then, examines empirically the impact of U.S. family planning programs first funded under the 1964 Economic Opportunity Act, the cornerstone legislation of President Johnson's War on Poverty. Section II presents a brief history of the Office of Economic Opportunity's (OEO) "wild" grant-making operation, which sent funds from the federal government directly to local organizations (Gillette 1996), and changes in federal policy under Title X of the 1970 Public Health Services Act, which led to delays in program funding and growth (Gould 1979). Because communities receiving federal family planning programs differed from communities that did not, section III outlines an event-study framework (Jacobson et al. 1993) exploiting variation in the timing of first grants dictated by the "wild" OEO operation and administrative idiosyncrasies under Title X.

Section IV builds an empirical case for the importance of U.S. family planning programs using three types of evidence. First, newly-compiled OEO administrative data show that use of family planning services among medically indigent women increased two times faster after communities first received a federal grant. Second, I show that family planning programs did not coincide with six other federal programs including Head Start, Community Health Centers, Jobs Programs, Maternal and Infant Care Projects, Maternity and Child Health Projects, and Legal Services. This supports claims that the estimated effects arose through family planning programs themselves rather than other, contemporaneous federal initiatives.

The final piece of evidence relies upon newly compiled county-level Vital Statistics data on fertility and mortality from 1959 to 1988. For a balanced panel of counties, an event-study framework shows that (1) fertility, infant and maternal mortality rates evolved similarly in funded and unfunded communities before funded locations received their first federal family planning grant. In light of the dramatic changes in fertility and mortality rates during the 1960s and 1970s, this finding underscores the strength of econometric model. Fertility rates, however, exhibit a sharp trend break after the first family planning grant. Consistent with family planning grants initiating *and* expanding programs (including building or renovating facilities, hiring doctors, and hiring additional staff), the estimates grow nonlinearly over the first five years. Six to ten years after the grant, annual fertility rates in funded counties had fallen by 2 percent. These patterns are virtually unchanged with the inclusion of state-by-year effects (which account for shifts in state policies, such as the expansion of Medicaid and the legalization of abortion), linear county trends, and a rich set of county-level covariates (including controls for local providers of abortion). Contrary to claims by the Center for Disease Control (1999a, 1999b), I find no evidence that family planning grants reduced maternal or infant mortality.

Although the evidence shows that federal family planning grants had a significant and lasting effect on U.S. childbearing, these programs can account for only 8 percent of the total decline in the general fertility rate from 1959 to 1974. On the other hand, they had a profound effect on poor women, who were the bulk of new patients (Dryfoos 1988: 284). Roughly 30 percent of federal-grant induced new users of family planning had one less child within the first three years of the program. The small aggregate effects of family planning, therefore, translate into substantial effects for lower-income women and children.

I. Extending the Neoclassical Paradigm: How Family Planning Affects Fertility

The primary stated objective of federally-funded family planning programs was to provide low-income individuals with greater choice over the number and spacing of their children (OEO 1969: 3). Improving maternal and infant health was a second objective. As highlighted in the introduction, whether these programs achieved these objectives remains an open empirical question. To formalize the hypotheses to be tested, this section extends the neoclassical childbearing model to incorporate the effects of family planning subsidies.

As a starting point consider a standard, neoclassical utility specification, $U(N, Z)$, where N is lifetime childbearing and Z is a composite, non-child commodity purchased on the market.

Constrained optimization involves couples choosing the number of children and amount of the composite commodity to maximize their utility subject to the expected, discounted shadow price of rearing a child, p, and full income, M (Z is the numeraire good), $pN + Z \leq M$. This formulation implicitly assumes that averting births is costless. In reality, the cost of averting births may consist of utility costs (e.g. abstinence) as well as pecuniary and effort costs of using a particular contraceptive method. Although having few children has always been possible, important changes in contraceptive technology and the price of modern methods have reduced price of averting births.

Incorporating a positive, pecuniary price of averting births into the standard framework is straightforward. By redefining lifetime childbearing as the difference between natural fertility (childbearing in the absence of any contraception,N_n, which is fixed by nature) and the number of births averted, $A \geq 0$, through contraceptive effort, $N = N_n - A$, couples maximize utility, $U(N_n - A, Z)$, by choosing the number of births averted and Z. As before, the budget constraint includes the expected, discounted shadow price of rearing a child, p, and full income, M, but it also includes a constant marginal cost of averting each birth, π.[2] The new budget constraint is $p(N_n - A) + \pi A + Z \leq M$, and the new optimization problem is,

(1) $$max_{A,Z}\,\{U(N_n - A, Z): pN_n + (\pi - p)A + Z \leq M\}$$

Notice that this problem differs from the standard problem only in the budget constraint's inclusion of a price for averting each birth. $\pi = 0$ returns us to the standard formulation. Making standard assumptions about the utility function, $U_N \equiv \frac{\partial U(N_n - A, Z)}{\partial N} > 0$, $U_z \equiv \frac{\partial U(N_n - A, Z)}{\partial Z} > 0$, $U_{NN} \equiv \frac{\partial^2 U(N_n - A, Z)}{\partial N^2} < 0$, and $U_{ZZ} \equiv \frac{\partial^2 U(N_n - A, Z)}{\partial Z^2} < 0$, optimality implies

(2) $$U_N(N_n - A, Z) = U_Z(N_n - A, Z)(p - \pi).$$

Family planning programs decrease the cost of averting births by reducing search for contraceptive information, increasing the number of locations providing supplies, and reducing the costs of supplies (like the Pill or the Intra-Uterine Device, IUD) and doctors' visits. The following proposition describes the effect of family planning on completed childbearing and births averted.

[2] See Michael and Willis (1976) for a generalization of the cost function to incorporate varying marginal costs of averting births.

PROPOSITION: With additively-separable utility, $U_{NZ} = U_{ZN} = 0$, (1) the number of children falls as the price of averting a birth falls, $\frac{dN}{d\pi} > 0$, and, (2) the number of births averted increases as the price of averting a birth falls, $\frac{dA}{d\pi} < 0$, if and only if $[U_{ZZ}(p-\pi)A + U_Z] > 0$.

PROOF: Totally differentiating (2) and the budget constraint with respect to π, and combining the resulting equations yields

$$\frac{dN}{d\pi} = \frac{-U_Z}{U_{NN} + U_{ZZ}(p-\pi)^2} + \frac{-AU_{ZZ}(p-\pi)}{U_{NN} + U_{ZZ}(p-\pi)^2}. \tag{3}$$

Under standard assumptions about the utility function, the first term, $\frac{-U_Z}{[U_{NN}+U_{ZZ}(p-\pi)^2]} > 0$, is positive, and the second term, $\frac{-U_{ZZ}(p-\pi)A}{[U_{NN}+U_{ZZ}(p-\pi)^2]} < 0$, is negative. When the second term is larger in absolute value, $[-U_Z - U_{ZZ}(p-\pi)A] < 0$ and $\frac{dN}{d\pi} > 0$. Because $\frac{dN}{d\pi} = \frac{-dA}{d\pi}$, the same condition implies $\frac{dA}{d\pi} < 0$. Showing the converse is trivial.

Contrary to conventional wisdom, the theoretical effect of a reduction in the price of averting births on the number of children is ambiguous due to the opposing income and substitution effects. With a reduction in the price of averting births, the substitution effect (the first term in equation 3) implies that households will substitute toward averting more births (and having fewer children). On the other hand, a reduction in the price of averting births saves households money on every birth they want to avert. The second term of equation 3 captures this as the product of the income effect $\frac{dN}{dM} = \frac{U_{ZZ}(p-\pi)}{[U_{NN}+U_{ZZ}(p-\pi)^2]} > 0$ with the number of births averted. Because children are a normal good, the income effect increases both non-child consumption as well as children born. These countervailing income and substitution effects of family planning may be one reason why the literature evaluating family planning programs has found such different effects for different countries and contexts. For the standard intuition to hold (childbearing falls with family planning), the substitution effect must dominate.

Another interesting implication of this framework is that the income effect will be quantitatively more important for couples with fewer children who choose to avert more births. All else equal, couples with fewer children are more likely to increase the number of children born with family planning subsidies. On the other hand, the income effect will be quantitatively less important for higher fertility couples. In the extreme, when the number of children equals natural fertility, $A = 0$, the income effect is zero and the number of children born falls

unambiguously with family planning programs. The effect of U.S. family planning programs on completed childbearing is, therefore, theoretically ambiguous.

By relaxing only the standard assumption that *preventing births is costless,* this framework demonstrates the ambiguous effect of family planning programs. Relaxing a second assumption that *childbearing can be chosen with certainty* does not change these results. Although the literature often claims that family planning reduces *unwanted* or *unintended* births, this framework makes clear that changes in prices have direct, albeit ambiguous effects, on *intended* or *wanted* births.

The empirical analysis additionally considers the impact of family planning grants on maternal and infant mortality (related to the second objective of the program). This is not explicitly modeled here, but the logic is straightforward. As more women used lower cost contraceptives, this increased contact with primary caregivers (both birth control pill and the IUD required visits to a physician for refills and check-ups). Therefore, more health conditions could be identified and treated before becoming critical. Family planning services reduced the cost of preventing risky pregnancies for women with pre-existing health conditions, which lowered health risks to the mother and also to the infant. Finally, frequent contact with a physician may have also encouraged women to seek prenatal care if they became pregnant.[3] With these predictions in mind, the remainder of this paper evaluates the actual childbearing and mortality effects of U.S. family planning programs.

II. A Brief History and Literature Review of U.S. Domestic Family Planning Policy

Today, the most effective contraceptive methods are scientifically tested, U.S. Food and Drug Administration approved, and medically prescribed. A variety of nonprofit and public organizations make family planning information, services and supplies available to women without means. But historically, contraception was deemed obscene material and banned under federal and most state statutes (Tone 2001).

After a series of legal decisions curtailed states' rights to restrict the distribution of contraception (Bailey 2010), widespread concern about the "population bomb" and reports that lower income women were having more children than they desired (National Academy of Sciences 1963) fueled controversy about further government intervention. The Pill was

[3] Jaffe et al (1973) estimated that among patients in all organized family planning programs (not just federally-funded programs), 60 percent received a medical examination (not pelvic or breast) Hemminki et al. (1987) show a 50 percent increase in the proportion of low-income women seeing a physician in the first trimester of pregnancy from 1960 to 1980.

prohibitively expensive. Shortly after its release, *Enovid* sold for roughly $750 per year (in 2008 dollars, Tone 2001: 257)—roughly three times the cost of birth control pills today (ignoring the cost of doctor visits) and more than three weeks of full-time work at the 1960 minimum wage. Family planning proponents argued that subsidies would increase information (Becker 1960 picks up on this argument) and reduce costs, thereby promoting greater use, decreasing unwanted childbearing, and improving maternal and infant health among poorer women.[4]

A. The Expansion of Federally-Funded Family Planning Programs, 1964 to 1973

Federal grants for family planning began under the Economic Opportunity Act (EOA, 1964), the cornerstone legislation of President Johnson's War on Poverty.[5] Although explicit language about family planning was not included in the EOA, the program fit easily within its agenda. With the quiet endorsement of Sargent Shriver, the head of the Office of Economic Opportunity (OEO), the OEO began funding family planning programs through the Community Action Program as early as 1964 (Levitan 1969).

Figure I shows gradual increases in federal outlays for family planning between 1965 and 1967 and two large increases in funding corresponding to two important policy changes. The first change came with the 1967 "Green Amendment" to the EOA (Public Law 90-222, Title II, Section 222a), which designated family planning as a "national emphasis" program. From fiscal year 1967 to 1970, federal allocations to family planning increased by over 13 times their 1967 level to roughly 400 million dollars (2008 dollars). The second change in outlays occurred under the administration of President Richard Nixon. His 1969 State of the Union Address advocated that Congress "establish as a national goal the provision of adequate family planning services within the next five years to all those who want them but cannot afford them." In November 1970, his effort culminated in the passage of Title X of the Public Health Services Act (also known as the Family Planning Services and Population Research Act, P.L. 91-572). The associated family planning grants were made through the Department of Health Education and

[4] This logic still permeates the policy discussion today. In February 2009, the Congressional Budget Office estimated that a proposed family planning provision of the "Stimulus Package" would save over $200 million over five years. This calculation, of course, relied upon the presumption that funds for family planning reduce births.

[5] According to 1967 estimates, expenditures for family planning through the Maternal and Child Health programs (started in 1942) and the Maternal and Infant Care programs under the 1963 Social Security Amendment were small (DHEW 1974: 3).

Welfare (DHEW).[6] Both the OEO and DHEW grants were made directly from the federal government to local organizations in response to grant applications.

Little qualitative or printed information documents how decisions about family planning grants under the OEO or Title X were made. While the lack of information on the motives of the applicants is not surprising, it is surprising that the National Archives contain almost no memoranda or correspondence about how, when and why OEO administrators chose to fund any of the War on Poverty programs—much less family planning, which comprised less than 0.4 percent of the 7.634 billion dollars in OEO outlays from 1965 to 1969. Gillette's compilation of oral interviews with former Johnson administration officials (1996) explains why. Donald Baker, chief counsel of the OEO, recalls: "It was a wild sort of operation in those early days, making the first grants. We didn't have any guidelines and didn't have the time really to draft them to start out…As a practical matter, Sarge[nt Shriver, director of the OEO,] and [Jack] Conway[, head of the Community Action Program,] and many others in the Congress were pressing the program people to get the money out and to go, go, go and make the grants and make the contracts" (193). Edgar Cahn, an attorney who worked closely with the OEO, described OEO administrators' attempt "to move fast to shovel out the money, because a few precious, perfect projects won't do anybody any good" (199). In light of these accounts, the dearth of federal records and funding guidelines makes sense, as OEO administrators had little time to develop or write them down

Under Title X, important changes in the administration of family planning grants affected the availability of funds throughout the 1970s. Not only did the DHEW delegate grant decisions to its regional offices, but policy regarding applications and funding shifted as well. At one point, for instance, DHEW's regional offices "refused to entertain local grant applications for family-planning services, insisting instead that state health departments be the only vehicle for such applications" (Gould 1979). This reflected conflicts within the DHEW about how to implement Nixon's "new federalism" in order to shift control of funds from federal to state officials (Dryfoos 1976)."

[6] The political and popular support for funding family planning waned with two events in 1973. First, *Roe v. Wade* put family planning providers at the center of a national debate about restrictions on federal funds for this purpose. Second, the involuntary sterilization of two girls, Minnie Lee and Mary Alice Relf (ages 12 and 14), called to national attention the abuses of local "family planning" programs. As the OEO was phased out under the Nixon and Ford administrations, the total federal appropriations fell to an average of roughly 400 million per year (in 2008 dollars) from 1974 to 1981. In fiscal year 1981, appropriations fell again to an average of 300 million per year (in 2008 dollars) and have remained close to this level for the last 25 years. State and local dollars were increasing over this period. Before 1977, the bulk of funds were federal (Cutright and Jaffe 1977: 3). By 1980, the Alan Guttmacher Institute (2000) estimated that 50 percent of public support of family planning came from Title X. By 1994, however, Title X provided only twenty percent of public support (13).

As is detailed in subsequent sections, the idiosyncratic component of grant timing is useful for the purposes of this paper, as both the "wild operation" at the OEO and these internal conflicts at the DHEW induced substantial, unanticipated delays in the funding of family planning applications. Because there is less information on how funding decisions were made under Title X, the analysis will compare the estimates for *first* grants made before and after Title X. The idea is to test whether the results hold when the historical basis for timing exogeneity is strongest.

B. What is Known about the Effects of U.S. Family Planning Programs

Over 45 years after the first federal grants, the literature has not reached consensus on the effects of family planning programs on birth rates and health. A large cross-sectional literature on U.S. family planning programs during the 1960s and 1970s comes to strikingly different conclusions (cross-sectional comparisons of states, see Weingarden 1974, Moore and Caldwell 1977, and Brann 1979; cross-sectional comparisons of counties, see Damey 1975; Udry, Bauman, and Morris 1976; Cutright and Jaffe 1977; Forrest, Hermalin, and Henshaw 1981).[7] These differing conclusions reflect differences in the unit of analysis (county or state), the measure of family planning access/use, and the timeframe of the analysis. The most formidable challenges to a causal interpretation of these studies' estimates reflect (1) the joint determination of supply of and demand for family planning services at the local level and (2) the dynamic evolution of effects, both of which are hard to address in cross-sectional analyses.

An example clarifies the first problem. Ideally, causal inference would be conducted in a framework holding the demand for family planning (or demand for children) fixed and randomly assigning federal grants affecting the supply of services. This would be similar to the price shock to averting births modeled in section I. In practice, however, communities receiving family planning grants differed along both demand and supply dimensions. For instance, educated communities may have demanded fewer children *and* been more effective at writing grants for federal funding. "Controlling" for education levels, therefore, would account for both differences

[7] See Mellor (1998) for a review. For instance, Damey and Moore-Caldwell find that measures of family planning use and availability, respectively, are associated with *reduced* birth rates among black women. While Moore- Caldwell do not find a significant correlation between family planning use and fertility rates for white teens, Damey reports that the use of family planning *increases* birth rates among white women. On the other hand, Forrest et al. find no relationship between program enrollment in family planning programs and birth rates among black teens, but conclude that enrollment *reduces* birth rates among white teens. County-level studies of roughly the same period come to contradictory conclusions: Udry et al. find no significant relationship between average dollars spent per recipient of family planning services and births, whereas Cutright and Jaffe report that enrollment in family planning programs is associated with reductions in birth rates among blacks and whites.

in demand as well as the supply of family planning and lead the estimates to misstate the effect of family planning. This problem relates directly to different ways of measuring of family planning access used in past analyses (for instance, program enrollment, use of services, and dollars spent on family planning). These measures may similarly conflate differences in supply and demand for services. As a result, cross-sectional partial correlations—even with ample controls—cannot be interpreted as the effect of the greater supply of family planning services.

The second limitation relates to the dynamic evolution of program effects. Especially during the period between 1965 and 1973, averaging the effects of more established programs (with potentially larger effects) with those just beginning to operate (with potentially smaller effects) may bias the programs' estimated effects toward zero. The next section outlines an empirical strategy for dealing with both problems and tests this strategy's identifying assumptions.

III. Data and Empirical Strategy: Using The Roll-Out of Family Planning Grants as a Supply Shock

A. New Data Documenting the Roll-Out of Federal Family Planning Grants, 1965 to 1973

To quantify the impact of federal grants, I compiled information on family grants to 656 U.S. communities through the OEO and DHEW from 1965 to 1973 to describe funding outcomes.[8] Information on family planning grants funded under the OEO is drawn from the National Archives Community Action Program (NACAP) files, and information on family planning grants funded under Title X is culled from the National Archives Federal Outlay (NAFO) files. These files provide two crucial pieces of information: (1) information about where services were delivered under the grant (county and state), which allows each grant to be matched to the annual, county-level fertility and mortality outcomes (the lowest level of geographic aggregation in the Vital Statistics records); and (2) the date of the *first* federal family planning grant to each funded location. This information allows the analysis to relate changes in county-level fertility and health outcomes to the precise *timing* of changes in funding for family planning communities nationwide.[9]

[8] An earlier version of this paper also included first grants from 1974 to 1980, but these grants were administered under a different statutory authority and are omitted in this draft.

[9] The main limitation of the data is that they provide little documentation explaining the purpose of each grant or the many dates associated with each record. To identify family planning awards, I used string searches on grant titles. In addition, fiscal year 1969 is missing from the electronic records. To minimize measurement error in the date of the first federal award and the location of service delivery, I compared the NACAP and NAFO data to printed, county-level OEO reports covering the universe of family planning programs in fiscal year 1968, calendar year 1969, and fiscal year 1971 (OEO 1969, 1971, 1974). This comparison

The analysis uses only the *first federal family planning grant* to a county. This makes intuitive sense, because first grants presumably *shocked* the local provision of family planning services by making new services available, expanding existing services, and lowering the costs of services—both directly and by competing with non-federal providers. Subsequent funding may have only served to continue existing services. Using the timing of *first* grants has several analytical advantages as well.

First grants abstract from potentially endogenous refunding decisions. If, for instance, strong programs were refunded and weak programs were not, refunding decisions would be positively correlated with measures of performance independent of a direct effect. Equally problematic would be a decision rule that increased funding to failing family planning programs and asked successful programs to seek funding from alternative sources. In this case of compensatory refunding, correlations between grant amounts and outcomes would show little correlation even if federal funds affected outcomes. Because there is no information on program effects at the initial proposal stage, using the *timing* of *first* grants avoids both problems. Using the timing of first grants also ameliorates concerns about potentially endogenous decisions about the amount of the grant. Although there is little reason to believe administrators could predict program effects at the application stage, using the incidence of first grants circumvents potential problems with funding amounts reflecting, for instance, a stronger proposal, greater local resources, or greater need for family planning services. Finally, the use of first grants implies that roughly 80 percent of my funded communities first got grants before Title X, which further mitigates concerns about endogenous grant placement under Title X.

Figure II presents the rollout of first family planning grants. The lightest gray shades counties first receiving grants between 1965 and 1967; the next darkest gray shades counties first receiving grants between 1968 and 1969 during the expansion of family planning as a national emphasis program; the black counties first received grants from 1970 to 1973 with the initiation

---

allowed me to add 285 communities that received their first funding in 1969. Only 23 of the remaining 422 grants were revised to reflect the earlier date in the OEO reports. Overall, the electronic records and printed reports agreed in 95 percent of cases where both are present. The analysis may miss some communities receiving their first grant before 1968. By necessity, these communities are grouped with unfunded communities in the analysis. Under the assumption that these early, unobserved grants had similar effects to the ones I observe, this measurement error should lead the analysis to understate the effects of family planning programs. As a final additional check on my data, the dates and grant amounts in my database are compared to Title X appropriations published by the Office of Population Affairs (OPA, 2009). Because the OPA figures omit allocations through the Community Action Program, figure I shows that my series—which includes both OEO and Title X grants—is larger than the OPA Title X series before 1974, when both Title X and the OEO funded family planning. The similarity of my data to the OPA figures after 1973, when the OEO was disbanded, suggests I capture the bulk of federal family planning awards.

of Title X. Although counties in each of the lower 48 states received grants, grant timing differed considerably within states. For instance, the first U.S. community to receive a grant was Corpus Christi, Texas, in 1964; Ector and Gonzales, Texas, received first grants in 1973. In 43 of the lower 48 states, programs were first funded in at least two different years; counties in 41 states first received funding in at least four different years. In more than half of states, counties were first funded in at least five different years of the nine-year period.

Figure II also suggests that counties receiving grants differed from those that did not. Columns (2) and (4) of table I quantify these differences using the 1960 Census. Roughly 60 percent of the U.S. population of women ages 15 to 44 lived in the shaded counties that received federal money between 1964 and 1973. Funded counties were more urban, had more elderly residents, and were more educated and affluent. Funded counties also had more of their population *above* the 1960 poverty line for a family of four ($3000 in 1960). Interestingly, the proportion of nonwhite residents and residents under the age of five do not differ.

Column (3) of table I also show that 1960 characteristics of communities first funded in different periods appear quite similar with two exceptions: more populous counties received first grants sooner and 1960 characteristics differ for some communities first funded between 1970 and 1973. The first is not surprising, because more urban areas may have been more likely to apply or be funded first for a variety of reasons. Because county fixed effects will be included in regression specifications, this correlation need not be a threat to the internal validity of the analysis (external validity will be considered later). More concerning would be funding patterns that relate to past fertility rates (proportion of residents under age 5), because they could indicate that funding patterns are related to the demand for children. Linear regressions (available upon request) examine whether these 1960 county characteristics predict the timing of the receipt of family planning awards after accounting for population size (see Hoynes and Schanzenbach 2009 for a similar exercise). In both weighted and unweighted specifications with state fixed effects, only the proportion of residents above age 64 significantly predicts receiving a grant sooner aside from population size. In limiting the sample to only grants received before Title X, the partial correlation of the proportion of residents above age 64 loses predictive power (unweighted: 0.003, s.e. 0.036, weighted: -0.048, s.e. 0.065). Notably, differences in lagged fertility measures have no predictive power. The failure of these characteristics to predict the timing of adoption differs from results in Hoynes and Schanzenbach (2009), who report statistically significant

relationships between these characteristics and the timing of food stamps program initiation. Consistent with oral history accounts of a haphazard application for and allocation of federal grants, 1960 characteristics are poor predictors of when communities received their first grant.

B. Data on Birth and Mortality Outcomes

Data on birth and death outcomes come from published and electronic Vital Statistics data for 1959 to 1988. The main fertility outcome, the general fertility rate (GFR), is defined as the number of births by county of mother's residence per 1000 women of childbearing age (15 to 44). The numerator for the 1959 to 1968 GFR was entered from published Natality Volumes; the numerator for the 1968 to 1988 GFR was aggregated from NCHS microdata to mother's county of residence. GFR denominators were constructed for 1959 to 1968 by linearly interpolating information between the 1950, 1960, and 1970 censuses (Haines 2005); GFR denominators for the 1969 to 1988 period use the Surveillance Epidemiology and End Results (SEER) data. Although county-level birth rates (births per 1000 in the relevant population) by age group, parity, and race group are not available before 1968, I have constructed them from NHCS microdata for 1968 to 1988. The main mortality outcomes are the maternal mortality rate (MMR), defined as deaths from birth complications per 10,000 live births, and the infant mortality rate (IMR), defined as deaths before one year of age per 1,000 live births. The numerators for both rates are constructed from publicly available micro-data of all registered deaths from 1959 to 1980 (NCHS 2008) by the county of residence of the decedent.

Rather than comparing counties that received family planning grants to those that did not, this analysis uses this county-level panel dataset of birth and mortality outcomes and relies upon the variation in the timing of *when* communities received their *first* federal grants for family planning. To illustrate this strategy, panel A of figure III describes the evolution of the GFR for funded counties by period of first funding 1965-1967, 1968 to 1969, and 1970 to 1973 and unfunded counties (left figure) from 1946 to 1988 and plots the differences between the GFR for funded counties relative to unfunded counties (right figure). These descriptive results provide suggestive evidence for the identification strategy: a comparison to unfunded counties account for the nonlinear evolution of the GFR from 1959 to 1974 including the fertility notch of the late 1960s and the dramatic post-1970 fall in fertility. Relatively flat and stable lines differences indicate that fertility declined similarly from 1959 to 1966 in funded and unfunded counties. After 1966, however, the GFR diverged systematically by date of first federal grant. After 1967,

the GFR fell more rapidly in counties funded between 1965 and 1967 than in unfunded counties. After 1969, the GFR more fell rapidly in counties funded between 1968 and 1969. After 1973, the general fertility rate fell more rapidly in counties funded between 1970 and 1973. In short, the GFR fell more rapidly in funded counties following receipt of a federal family planning grant.

Panels B and C of figure III create these plots for IMR and MMR. Both rates fell by over 50 percent between 1965 and 1980; the fact that the MMR is considerably noisier is also evident. Like the pre-period comparisons for the GFR, the IMR and MMR in funded counties show no evidence of a pre-trend from 1959 to 1966. Unlike the GFR, however, neither the IMR nor the MMR diverged systematically in funded counties during the period following a family planning grant. For neither outcome does figure III provide suggestive evidence that either mortality rate responded to federal family planning grants.

C. Beyond Cross-Sectional Comparisons: The Value of an Event Study Framework

The following event-study specification (Jacobson, LaLonde and Sullivan 1993) formalizes and improves upon this descriptive analysis by mapping the *exact timing* of declines in outcomes to the *exact* fiscal year of grant receipt (rather than using three periods). This specification also permits the inclusion of a rich set of fixed effects and time-varying covariates. I estimate:

$$(4) \qquad Y_{j,t} = \theta_j t + \gamma_{s(j),t} + \sum_{y=-8}^{-1} \pi_y D_j 1(t - T_j^* = y) + \sum_{y=1}^{16} \tau_y D_j 1(t - T_j^* = y) + \boldsymbol{X}_{jt}^{'}\boldsymbol{\beta} + \varepsilon_{jt} ,$$

where $Y_{j,t}$ is a fertility or mortality outcome in county *j* in year *t* = 1959,…,1988 (for age estimates, *t* = 1968,…,1988); θ_j is a set of county fixed effects; $\gamma_{s(j),t}$ is either a set of year fixed effects or state-by-year fixed effects, which captures time-varying, state-level changes in the legality of abortion in the late 1960s and early 1970s, changes in Medicaid policy, and changes in family planning funds in Title V of the 1967 Amendment to the Social Security Act;[10] and $\boldsymbol{X}_{jt}$ is a column vector including a constant and a set of control variables that exhaust the information available to OEO administrators. These include the interaction of 1960 census characteristics in table I (share of population in urban area, nonwhite, under age five, over 64 years of age; share of households with income under $3000, over $10,000, and the share of the county's land that is

[10] In 1967, Title V of the Social Security Act mandated that at least 6 percent of funds for child and maternal health at the state level be earmarked for family planning services (Public Law 90-248, Title V, Secs. 502, 505a, 508a; Title IV, Sec. 201a).

rural or a farm) with linear time trends; annual and county-level per capita measures of government transfers using data from the Bureau of Economic Analysis Regional Information System (REIS) (cash public assistance benefits such as Aid to Families with Dependent Children, Supplemental Security Income, and General Assistance; medical spending such as Medicare and military health care; and cash retirement and disability payments). In addition, county-level information on the number of abortion providers in each county accounts for *within-state changes in the provision of abortion* from 1970 to 1988 (zero before 1970).[11]

Of interest are the coefficients on the interaction of D_j, a dummy variable equal to one if the county ever received a family planning grant, and an indicator function (this is excluded in specifications that exclude unfunded counties), with $1(\,)$, which is equal to one when the year of observation is y = -8,-7,…,15, 16, years from the date, T_j^*, the year that county *j first* received a family planning grant.[12] Because the indicator for $y = 0$ is omitted, π describe the differential evolution of outcomes in *funded* counties before their first family planning grant. τ describes the divergence in outcomes y years *after* the first family planning grant. Key to isolating the shock to the supply of family planning services is the inclusion of county effects (not possible in cross-sectional studies), which allows consistent estimation of π and τ even in the presence of pre-existing *unobserved* differences between funded and unfunded locations.

After evaluating the timing of changes in outcomes relative to the first family planning grant, the main results are *summarized* in the following specification, in which the individual indicators in equation 4 are replaced with indicators for five-year groups,

$$(5) \qquad Y_{jt} = \theta_j t + \gamma_{s(j),t} + \sum_g \tilde{\tau}_g D_j 1\left(t - T_j^* \in g\right) + \boldsymbol{X}_{jt}'\boldsymbol{\beta} + \varepsilon_{jt}\,.$$

The equation is identical to that in (4) (all notation remains as defined) save the measure of exposure to family planning: g indexes each five-year period (-7 to -6, -5 to -1, 1 to 5, 6 to 10, 11 to 14, and 15 or more years past the date of the first grant). Even though all of the dummies in equations 4 and 5 are included in all specifications, figures and tables present only estimates for a *balanced* sample of counties (years -6 to +14 for equation 4, -5 to +14 for the DiD).

[11] The first two sets of county characteristics in $\boldsymbol{X}$ are comparable to specifications in Almond et al. (2008: 15). The third set were generously provided by the Guttmacher Institute and Ted Joyce. Note that changes in the distance to states providing legal abortion before 1970 is accounted for in state-by-year fixed effects.

[12] To ensure the parameters are well estimated, values of $y < -7$ are grouped to be equal to -8 and all values greater than 15 are grouped into the category 16.

Keep in mind, however, that the event-study framework in equation 4 confers important advantages over the more standard difference-in-differences (DiD) in (5). One is that the non-parametric specification of τ relaxes the standard DiD assumption the treatment with a family planning grant is associated with a one-time, level shift in outcomes. A federal family planning grant cannot be spent instantaneously and may be used to build new facilities and hire employees. As a result, the effects in newly funded programs may be considerably smaller than the effects several years later. Moreover, if family planning programs allow women to delay childbearing several years, then federal grants could initially depress birth rates but raise them later—that is, there might be *no effects on average* over a five-year period, although there are meaningful inter-temporal changes. The flexible, event-study specification allows the effects to grow over time and also allows the detection of inter-temporal substitution.

A second advantage is that estimates of π allow a visual and statistical evaluation of the *evolution* of pre-treatment unobservables in funded communities (rather than assuming that $\pi_y = 0$ for $y<0$). Specifically, plots of π show whether a different, potentially non-linear, preexisting trend may confound the estimates of τ. Furthermore, they show whether the effects preceded the treatment even by a few years–an important falsification test. The event study, therefore, allows a direct evaluation of an important threat to identification in DiD and requires a tighter correspondence in the timing between the federal grant and changes in outcomes.

D. Did Federal Family Planning Grants Coincide with Other Local of Federal Initiatives?

Threats to identification of any program effects in the 1960s and 1970s relate to the potentially confounding political, social and economic changes. The event-study specification in (4), however, goes a long way to narrow the potential bias resulting from these factors. State-by-year effects capture time-varying national or state level unobservables. Moreover, *unobserved* political, social and economic changes that occurred in funded counties would be evident in the pre-grant estimates of π. Crucial to identification is (1) the absence of a pre-trend, which is consistent with the econometric model adequately accounting for pre-grant changes (federal spending response to greater turmoil, differential evolution in funded counties of fertility norms or the demand for children, etc.) and (2) a trend-break in outcomes in the year following the *first* family planning award. The presence of pre-trends could indicate a problem with the identification strategy, and they will be closely examined in the results section. This section examines the possibility of potentially confounding shocks *concentrated among the funded*

counties that are *coincident to* or *occur just after the first family planning grant*. The possible culprits are (a) packaged federal spending and (b) coincident local initiatives, which could generate both the absence of a pre-trend and a trend-break with appropriate timing.

The most likely is targeted federal spending. Although the oral histories provide no indication that OEO administrators deliberately packaged OEO grants, the coincidence of family planning grants with other federal program grants may have happened inadvertently or because certain communities were more effective at writing proposals. This is an important concern, because other OEO programs also impacted fertility and health. For instance, Ludwig and Miller (2007) show that Head Start programs reduced child mortality, while Almond et al. (2008) report that food stamps programs increased birth weight and decreased neonatal infant mortality. Other programs like Community or Neighborhood Health Centers (CHCs) or Maternal and Infant Health projects often provided services for family planning and aimed to improve infant and maternal health (DHEW 1974: 5). If first family planning grants tended to co-occur with these federal grants, then estimates of τ may fail to isolate the effect of a family planning grant.

My analysis is unique in its ability to test this concern. Using newly compiled information on *all* grants for other OEO programs from the NACAP and the NAFO data, I estimate equation 4 (excluding covariates) with a dependent variable grant receipt (=1) for each of eight OEO programs as the dependent variable.[13] Event-study estimates presented in figure IV are relative to the year the county received its first federal family planning grant. Panel A of figure IV is presented to fix ideas: almost all counties receiving first family planning grants receive their first grant at time zero (this figure is less than 100 percent due to the handful of first grants which do not appear in the grant data and come from OEO reports). There is no pre-trend in family planning funds, and subsequent coverage rates indicate that federal refunding occurred in many but not all cases.[14]

In contrast, panels B through E show almost no pattern. In no case is there evidence of an abrupt shift in funding for these programs at the time of the first family planning award. Funding for Community Health Centers (panel B), Head Start (panel C), jobs programs (panel D), and

[13] Because grants information for other federal programs are missing 1969, I omit this year from the sample (failing to do so would favor finding no correlation in timing between a first family planning grant and other programs).

[14] This is not an indication that these programs were discontinued. Federal funds subsidized setting up these programs, which is one reason why using the incidence of a first federal grant is important. Unobserved, state and local dollars, however, were also increasing over this period. Before 1977, the bulk of funds were federal (Cutright and Jaffe 1977: 3). By 1980, the Alan Guttmacher Institute (2000) estimated that fifty percent of public support of family planning came from Title X. By 1994, however, Title X provided only twenty percent of public support (13).

legal services (panel E) show very small or no increases predating family planning grants; funding did not increase over the years when family planning funding was expanding. The next two panels of figure IV examine the coincidence of first family planning grants with maternal and infant care projects (panel F) and maternity and infant health projects (panel G). These plots are flat lines and provide no evidence of a trend break in the year of the first family planning grant. These programs are not exhaustive, but this evidence does alleviate concerns of packaged federal spending on six of the most likely, potentially-confounding federal programs.

The other possible confounder of estimates of τ is roughly *coincident* local initiatives or programs that *would have the same effects on outcomes* as family planning and are *unrelated to the federal family planning grant*. (Note: If a complementary community initiative family planning initiative begins *because* of the increase in federal family planning funds, the estimates would *appropriately* incorporate these indirect, "crowd-in" effects.) Given the evidence presented thus far and the history at the OEO and DHEW, the importance of *unrelated* local initiatives seems remote. On the one hand, applications came from "various and sundry groups" often having little to do with the spirit of the legislation (Gillette 1996: 196 quoting Theodore M. Berry, assistant director of the OEO). *Even if* most applicants also began local programs after submitting a grant proposal, the openness of the proposal process and administrative delays at the OEO and DHEW meant that applications were routinely rejected and deferred. These rejections would put a number of these potentially-confounding, local initiatives in the *unfunded* group—one reason why including unfunded counties in regressions helps generate a more appropriate counterfactual. On the other hand, *even if* all applicants were eventually funded, administrative delays meant that the effects of these potentially-confounding, independent local initiatives would be unlikely to coincide closely in timing with the first federal grant—one reason why finding a trend break in outcomes corresponding to the timing of the first federal award is crucial to identification. In short, strong evidence of (1) the absence of a pre-trend and (2) a distinct trend-break in outcomes immediately after the *first* family planning award makes it unlikely that the bulk of the 656 communities receiving federal grants *randomly* experienced an independent, coincident local initiative. For this reason, the subsequent discussion will rely heavily on (1) and (2) in discussing internal validity.

IV. Event-Study Estimates of The Relationship between Federal Family Planning Grants, Family Planning Use, Fertility and Mortality

The case for the effects of family planning programs on fertility and mortality outcomes rests upon three types of evidence. The first section uses OEO administrative data to show that poor women increased their use of family planning services two-times faster *after* their community received its first federal grant—a finding that is corroborated in the 1970 *National Fertility Study*. The second section uses county-level Vital Statistics births and mortality data between 1959 and 1988 to show (1) no evidence of diverging or differently-trending birth rates, infant mortality or maternal mortality in the six years before a federal grant was received and (2) a striking trend break in birth rates in the year of the first family planning grant. A third section finds no evidence that family planning grants reduced infant or maternal mortality. Although this finding runs contrary to claims by the Center for Disease Control, it ameliorates concerns that the fertility effects are spuriously driven by coincident War on Poverty programs (Ludwig and Miller 2007, and Almond et al. 2008) or independent local initiatives.

A. Did Federal Family Planning Grants Increase Use of Family Planning Programs?

Before receiving a federal grant, over 40 percent of funded communities had family planning programs, and more than 50 percent of unfunded counties had a family planning program that was not federally funded in the 1968 to 1972 period. Crucial to the analysis is whether federal grants increased access or crowded out funds from other sources. Newly compiled data from the OEO and survey data address this question. The first data source consists of newly entered, county-level OEO reports covering the *universe* of family planning programs in fiscal year (FY) 1968, calendar year (CY) 1969, and FY 1971 (OEO 1969, 1971, 1974). These reports summarized surveys sent to *all known* providers of family planning (hospitals, health departments, and clinics operated by other agencies) and documented medically indigent patients receiving family planning services.[15] These data permit estimation of a restricted version of (4),

$$(6) \qquad Y_{j,t} = \theta_j + \gamma_{s(j),t} + \tau 1(t - T_j^* > 0) + \boldsymbol{X}_{jt}^{'}\boldsymbol{\beta} + \varepsilon_{jt} \, ,$$

where $Y_{j,t}$ is the share of medically indigent patients in county *j* using family planning services from *any* provider (federally-funded or not) in time *t*: FY 1968, CY 1969, and FY 1971, and the remaining notation remains as defined. The point estimate of interest, τ, is associated with

[15] Completion rates of the survey were high. In 1968, for example, 97 percent of hospitals and 100 percent of all other agencies responded (OEO 1969, table 3: 244). Jaffe et al. (1993) report that 90 percent of all patients in organized family planning programs had household incomes no greater than 200 percent of the federal poverty line.

$1(t - T_j^* > 0)$, which is equal to 1 after county *j* received its first federal family planning grant. With the inclusion of county fixed effects, this estimate can be interpreted as the *differential change* in share of medically indigent women using family planning services *after* first funding.

Panel A of table II presents the results adding covariates sequentially. Column 1 includes county and year fixed effects, column 2 adds state-by-year fixed effects, and column 3 adds all other covariates. From 1968 to 1971, the share of medically indigent women who used family planning services increased by 1.6 percentage points in CY 1969 and by another 2.88 percentage points by FY 1971 in the average U.S. county. The increase was an average of 4.5 to 4.7 percentage points in counties *after* receiving federal family planning grants—more than twice the rate of increase in unfunded counties over the 1968-71 period. The estimate changes negligibly with the inclusion of increasingly more covariates in columns 1 to 3 and should understate the overall effects of family planning grants if they also affected women with higher incomes.

A second data source, the 1970 *National Fertility Study* (*NFS*), corroborates these findings for low income women and provides more detail on use by household income. The *NFS* sampled ever-married women between the ages of 18 and 44 about whether they had ever used a family planning clinic (regardless of source) and also whether they had ever used the Pill, the most popular and expensive contraceptive method in 1970. Unlike administrative data, this dataset contains responses from women regardless of medical indigency but is limited to the population of married women. These data allow me to estimate the following:

$$(7) \qquad P(Use_{ij}) = F(\boldsymbol{Z}_{ij}\boldsymbol{\delta} + \theta_1 1(T_j^* < 1970) + \theta_2 1\big(Pov_{ij}\big) + \theta_3 1\big(Pov_{ij}\big)1(T_j^* < 1970),$$

where Use_{ij} is a measure of whether individual *i* in county *j* had ever used a family planning clinic or the birth control pill, $1(T_j^* < 1970)$ is a binary variable equal to one if county *j* received a federal family planning grant before the survey date (1970), and $1\big(Pov_{ij}\big)$ is a binary variable equal to 1 if the annual household income was below the poverty line. Because county fixed effects cannot be included with this single cross-section, my estimation sample includes a rich set of covariates in the row vector, $\boldsymbol{Z}_{ij}$, including state fixed effects, dummy variables for age, educational achievement, population size of the county, and Catholic religion. In addition, one specification includes dummy variables for the "number of children most desirable" to capture residual, unaccounted for differences in the demand for children (see table II notes for

details on variable coding). The effects associated with θ_1 and $\theta_3$capture differences in family planning service and Pill use in funded counties by 1970.

Panels B of table II reports average partial effects from probits and bootstrapped standard errors (1000 replications) using "Ever used a family planning clinic" as the dependent variable. Notice the mean of this variable is much higher in the *NFS* than the OEO data, because *cumulative* use is measured (OEO data measure use in the survey year). Covariates are added sequentially to examine robustness. Column 1 includes state fixed effects, and columns 2 and column 3 add all other covariates. In none of the three specifications does receiving a federal grant increase use of family planning clinics for respondents above the poverty line. For women below the poverty line, however, any use of family planning services increases by roughly 4 percentage points. Perhaps owing to the fact that self-reports are not as reliable as administrative data and because the 1970 *NFS* was not a census of use in all counties, this relationship is imprecise (t=1.48, p=0.14). The comparability of the magnitudes supports the external validity of the OEO data and that grants impacted clinic use primarily among married women.

The bigger question is how federal family planning grants altered contraceptive use. Although the 1970 *NFS* contains almost no information on the use of medical services or illegal abortions, respondents were asked if they "ever used the Pill." Panel C shows that married women in locations receiving family planning grants before 1970 were more likely to have ever used the Pill. Including additional covariates in columns 1 to 3 does not alter this conclusion. Interestingly, use of the Pill also increased for women *above* the poverty line (4 percent increase) as well as below (~20 percent increase). The effect on women above the poverty line may be direct (more affluent women went to public clinics to get cheaper Pills) or operate through the competition-induced price reductions of the Pill for private providers. For women below the poverty line, the effect on Pill use is large enough to offset the poverty penalty.

In summary, the data show that use of family planning services among the poor and use of the Pill for women above and below the poverty line increased faster in locations after receiving a federal family planning grant. They are also robust to the inclusion of a rich set of covariates and across datasets. After presenting the main results, these estimates will be used to evaluate the magnitudes of the effects of federal family planning grants on the women served.

B. Did Federal Family Planning Grants Reduce Births?

Using equation 4, figure V presents event-study estimates of the effect of the family planning grants on the general fertility rate (GFR); table III summarizes the estimates using equation 5. Panel A weights by the 1970 population of women ages 15 to 44 and, therefore, captures the effect of the first family planning grant for the average woman of childbearing age. Three specifications are presented: model 1 includes county and year effects (assumes $\gamma_{s(j),t} = \gamma_t$); model 2 adds state-by-year fixed effects to model 1; and model 3 adds the timing-varying county-level covariates to model 2. Heteroskedasticity-robust standard errors clustered by county are used to construct 95-percent, point-wise confidence intervals.[16] While omitted from figure V, table III shows that the addition of linear, county trends (column 4) yields larger, though statistically indistinguishable, estimates (compare to model 3, column 3).

In the decade before the first family planning grant, none of the three models shows a pre-trend in eventually funded counties. As in the descriptive analysis (figure III), the GFR evolved similarly in funded and unfunded counties: the pre-treatment estimates are close to zero and individually, statistically insignificant. Table III also shows that the estimate for years −5 to −1 is indistinguishable from zero in four models. A sharp trend break, however, is evident in the year after the first family planning grant in every specification. Consistent with first grants initiating or expanding family planning programs, the effects grow over time. Within three years of the award, the GFR had fallen by roughly one birth per 1000 women of childbearing age (model 3, panel A) and two to 2.5 births per 1000 women in the average county (panel B). By years 5 to 7, the GFR had fallen by 1.6 to 1.9 births per 1000 women of childbearing age (model 3, panel A) and over 2 births per 1000 women in the average county (panel B)—roughly where they remain in years 8 to 14. Interestingly, the decline in fertility was stronger in *less populous* counties. Not only is the trend break more striking in unweighted models, but also the estimates are absolutely larger. This may be due to measurement (treated women may be a larger proportion of the county population in smaller populations) or reflect heterogeneity in the effect across locations (family planning options in larger cities might have been better). Table III places

[16] Although the discussion will emphasize the results in model 2 or 3, it is unclear whether model 1, 2 or 3 better captures the effects of family planning grants. On the one hand, it accounts for year-to-year changes in states' support for family planning, maternal and infant health policies, Medicaid, and abortion access in the form of state-by-year fixed effects and a rich set of time-varying covariates. But, it may understate of impact of federal grants, if women in unfunded counties benefitted from family planning grants to neighboring counties. It is, therefore, reassuring that differences in the magnitudes of the estimates in years 1 to 8 between models are modest and that the equivalence of the estimates cannot be rejected.

the differential reduction in years 6 to 10 at an average of 1.5 for the average woman (column 3, panel A) and 2.1 in the average county (column 3, panel B). One final note is that county-level controls for abortion alter the estimates for model 3 less than 0.001 in all cases (model estimates omitted for brevity but available upon request). This suggests that very little of the estimated effect operates through abortion referrals or counseling. I interpret this as saying that the provision of preventative contraception provides the primary mechanism for these fertility reductions. The fact that the estimates do not rebound is consistent with the dominance of the substitution effect in section I, implying that lifetime fertility falls with a family planning grant.

Three final specification checks underscore the robustness of these findings. Under the assumption that funded counties evolved differently than unfunded counties (an assumption at odds with the absence of a pre-trend in panels A and B), one might opt to include only funded counties in the regressions (i.e. estimate specification 4 without D_j). Across specifications, the pattern of results is similar for years 1 to 9. Consistent with panels A and B, estimates using funded counties only show (1) no pre-trend and (2) a sharp trend break in the year after the grant, and (3) are not statistically distinguishable from models in panels A and B in years 1 to 8. Estimates from this restricted sample are appreciably larger after year eight but also increasingly imprecise (the estimates are neither statistically different across models nor distinguishable from zero). Column 5 of table III places the differential reduction in the GFR in years 6 to 10 at 1.7 for the average woman. The exclusion of unfunded counties, therefore, does not change the patterns of results but does limit precision.

Panel D of figure V examines heterogeneity in the magnitudes of the estimates for pre-Title X and post-Title X grants by omitting counties funded in the other period from the analysis (i.e. pre-Title X regressions omit all counties first receiving a grant after 1970 and vice versa). Although the magnitudes differ modestly, grants in both periods reduced the GFR by an average of 1.5 to 1.6 births per 1000 women in years 6 to 10 (columns 6 and 7, table III). Consistent with panels A through C, the estimates show (1) no pre-trend and (2) a sharp trend break in the year after the grant, and (3) similar magnitudes. The estimates for pre- and post-Title X grants are statistically distinguishable from one another only in year 1, because the post-grant trend-break is sharper for grants first occurring in 1971 or later.

Finally, figure VI examines heterogeneity by age group, parity, and race for model 3 (education is not available for all states for 1968-1988). Because this county-level information is

not available before 1968, I construct a short panel from the natality microdata for 1968 to 1988. (Aggregate estimates for this short panel show that omitting counties funded before 1968 has a negligible impact, panel B, figure V; column 8, table III). Estimates for different age groups are presented in panels A and summarized in table IV. In the first five years following a family planning grant, birth rates to each age group decreased. Six to ten years from the first grant, the largest absolute drop in birth rate levels was approximately 4 births per 1000 women, or 2 percent, among 25 to 29 year olds (table IV, panel A, column 3); 2.4 births among 20 to 24 year olds (column 2) and 1.1 births among 30 to 34 year olds (column 4) for a drop of 1.5 percent; and 1.3 births, or 1.9 percent, among teens (column 1). Changes in birth rates among women in their late 30s and 40s are negative but statistically insignificant. Consistent with these effects are reductions in parity specific rates presented in panel B of figure and table IV, where the largest absolute reductions in births were for first through third births. Changes at parity four and above are statistically insignificant. Panel C of figure and table IV shows that the reductions in childbearing were slightly larger, but not statistically different, among nonwhites.

Given the robustness of the estimates, a natural question is their plausibility. Using estimates of how family planning grants increased use (table II, panels A and B), figure V's intention-to-treat (ITT) effects can be scaled to approximate treatment effects on the treated (TOT). If use of family planning grants increased the use of clinics only among women in poverty by roughly 4 percentage points and roughly 20 percent of women ages 15 to 44 in 1970 were in poverty, the ITT estimates for one to five years in table III (panel A, columns 3 and 4) imply a reduction of roughly 2.3 to 2.8 births per 1000 women ages 15 to 44 over three years, the average time since receiving a federal family planning award in the administrative data. Together these estimates imply that a federal grant induced a delayed or prevented birth among 28 to 34 percent of the grant-induced, new users of family planning services. If one, instead, takes account of the broader effects of federal grants on use of the Pill, the implied TOT estimates fall. Scaling instead by the fraction of women ever using the Pill (panel C, column 3 implies a 0.043= 0.8*0.026+0.2*0.10 percent increase in Pill use) implies a TOT to one child for 5.4 to 6.6 percent of the women served. In short, the estimated effects are reasonable and consistent with estimated changes in use of family planning and the Pill.

In summary, the (1) absence of a pre-trend, (2) sharp trend-break in the year following the first grant, (3) evidence that no other federal program changed abruptly at the same time or in

the years immediately following the first federal grant (figure III), (4) the robustness of these estimates across specifications in the first eight years after the grant, and (5) the consistency of the magnitudes with data on service use builds a strong case for a causal interpretation.

C. Did Federal Family Planning Grants Reduce Infant or Maternal Mortality?

Figure VII plots event-study estimates using infant and maternal mortality as dependent variables using equation (4), and table VI summarizes these estimates using (5). Similar to figure III, panels A and B show that neither the infant mortality rate (IMR) nor the maternal mortality rate (MMR) exhibit trend breaks following the first family planning grant. Adding additional covariates, county trends or omitting weights (see table V) does not alter this conclusion.

One explanation for the absence of changes may reflect important shifts in the composition of mothers, because family planning grants affected births, the denominator in the IMR and MMR. In unreported results, I examine this possibility by separating neonatal and post-neonatal infant mortality. Although neonatal mortality is slightly lower following a family planning grant, this appears to be the continuation of a pre-trend dating to four years before the first grant was awarded. On the other hand, post-neonatal mortality appears to *increase* following the receipt of a family planning grant. Although the inclusion of control variables reduces the size of these estimates, their magnitudes imply an important effect: an increase of approximately 0.1 deaths per 1000 live births, or roughly 1.5 percent, over the 1965 level. This is consistent with more advantaged of disadvantaged households using family planning programs to delay or prevent births and, consequently, an increase in the proportion of infants living in the most disadvantaged households.

These effects may not be surprising given the “population control” focus of many family planning programs of the 1960s. Whereas today’s family planning programs provide a menu of reproductive and gynecological health services and may have an appreciable effect on the health of infants and women, many programs in the 1960s provided no health services at all and only handed out Pills (Bailey 1999). Nevertheless, I cannot rule out imprecision. For event-study estimates of model 3, a 95-percent confidence interval for the IMR includes estimates of a –0.40 to 0.33 at year 5. That is, these estimates are not inconsistent with family planning grants reducing the IMR by 2 percent or increasing it by 1.7 percent over the mean 1970 IMR. A 95-percent confidence interval for the IMR includes estimates of a –0.23 to 0.36 at year 5, a range that includes a reduction of 10 percent and an increase of 18 percent over the mean 1970 MMR.

Estimates of similar sizes cannot be ruled out in the pre-grant period. Despite relying upon the *universe* of deaths from 1959 to 1988, these estimates do not rule out meaningful effects on infant and especially maternal mortality.

V. Reevaluating the Effects of Family Planning

The effects of U.S. family planning programs have been debated by social scientists and policy makers since they began over forty years ago. Despite the importance of this debate for models of economic and human development and the intergenerational transmission of poverty, the dearth of plausibly-exogenous variation in the availability of family planning services has limited the ability of social scientists to evaluate the effects of these programs.

Using variation in the timing of first federal grants to U.S. communities, I provide new robust evidence that federal family planning grants from 1964 to 1973 reduced annual fertility rates by roughly 2 percent within six years. Consistent with fertility delay, the largest relative reductions in period birth rates occurred among women under age 30 for the first, second and third births. In contrast, the analysis finds no evidence that family planning reduced infant or maternal mortality.

What do these results imply about the importance of family planning policies in reducing birth rates? Over the course of ten years, women in communities with federally-funded family planning programs had roughly 1,806,346 fewer births. This reduction in birth rates cost of roughly $2,700 per birth averted (2008 dollars) in the program's first 10 years.[17] Nevertheless, aggregating the event-study estimates to each year from 1965 to 1974 implies that U.S. family planning grants explain a modest 8 percent of the total decline in the general fertility rate from 1959 to 1974. The small aggregate effects of family planning, however, translate into substantial effects for the families they served. Together with new estimates on the use of family planning, they imply that a family planning grants from 1965 to 1973 prevented births among roughly 30 percent of new patients within the first three years of operation, 90 percent of whom were below the federal poverty line (Dryfoos 1988: 284). Future work should consider how these changes in shorter-term period fertility rates contributed to longer-term changes in poverty, economic growth, and the resources available to children.

[17] Births averted estimates are obtained by multiplying the mean population of women ages 15 to 44 in funded counties in 1970 (145,193) by the event-study estimates for model 3 and summing over years 1 to 10. Federal outlays from 1965 to 1980 for family planning in 2008 dollars are estimated at $5,003,834,508. This is less than half the cost of estimated by Kearney and Levine (2008)'s estimate of $6800 per birth in the 1990s.

VI. REFERENCES

Alan Guttmacher Institute (AGI). (2000). *Fulfilling the Promise: Public Policy and U.S. Family Planning Clinics*. (New York: AGI).

Almond, Douglas, Hilary W. Hoynes, and Diane Whitmore Schanzenbach. (2008). "Inside the War on Poverty: The Impact of Food Stamps on Birth Outcomes." *National Bureau of Economic Research Working Paper 14306,* September.

Bailey, Beth. (1999). Sex in the Heartland: Politics, Culture, and the Sexual Revolution. Cambridge, MA: Harvard University Press.

Bailey, Martha J. (2010). "'Momma's Got the Pill': How Anthony Comstock and *Griswold v. Connecticut* Shaped U.S. Childbearing." *American Economic Review* 100 (1): 98-129.

Becker, Gary. (1960). "An Economic Analysis of Fertility," in *Demographic and Economic Change in Developed Countries. Conference of the Universities-National Bureau Committee for Economic Research*, a Report of the National Bureau of Economic Research. Princeton, N.J.: Princeton University Press: 209-40.

Blake, Judith. (1969). "Population Policy for Americans: Is the Government Being Misled?" *Science*, New Series, Vol. 164, No. 3879, May 2: 522-529.

Bongaarts, John. (1994). "The Impact of Population Policies: Comment." *Population and Development Review* 20 (3): 616-620.

Brann, Edward. (1979). "A Multivariate Analysis of Interstate Variation in Fertility of Teenage Girls." *American Journal of Public Health* 69 (7): 661-66.

Center for Disease Control and Prevention. (1999a). "Ten Great Public Health Achievements—United States, 1900-1999." *Morbidity and Mortality Weekly Report* (04/02/99) 48 (12): 241.

-----. (1999b). "Achievements in Public Health, 1900-1999: Family Planning." *Morbidity and Mortality Weekly Report* (12/03/99) 48 (47): 1073-1080.

Cutright, Phillips and Frederick S. Jaffe. (1977). *The Impact of Family Planning Programs on Fertility: The U.S. Experience*. (New York, NY: Praeger Publishers).

Damey, Philip D. (1975). "Fertility Decline and Participation in Georgia's Family Planning Program: Temporal and Area Associations." *Studies in Family Planning* 6 (6): 156-65.

Department of Health and Economic Welfare (DHEW), Center for Family Planning Program Development (Planned Parenthood-World Population). (1974). *Family Planning, Contraception, and Voluntary Sterilization: An Analysis of Laws and Policies in the United States, each State and Jurisdiction (as of September 1971): A Report of the National Center for Family Planning Services, Health Services and Mental Health Administration*. (Rockville, MD: U.S. DHEW, publication no. 74-16001).

Dryfoos, Joy G. 1988. "Family Planning Clinics--A Story of Growth and Conflict." *Family Planning Perspectives* 20 (6): 282-287.

Forrest, Jacqueline Darroch, Albert I. Hermalin, and Stanley K. Henshaw. (1981). "The Impact of Family Planning Clinic Programs on Adolescent Pregnancy." *Family Planning Perspectives* 13 (3): 109-16.

Gillette, Michael L. (1996). *Launching the War on Poverty: An Oral History*. (New York, N.Y.: Twayne Publishers).

Gould, Ketayun H. (1979). "Family Planning and Abortion Policy in the United States." *Social Science Review* 53 (3): 452-463.

Haines, Michael R. (2005), and the Inter-university Consortium for Political and Social Research. "Historical, Demographic, Economic, and Social Data: The United States, 1790-2000" [Computer file]. ICPSR02896-v2. (Hamilton, NY: Colgate University/Ann Arbor: MI:

Inter-university Consortium for Political and Social Research [producers], 2004; Ann Arbor, MI: Inter-university Consortium for Political and Social Research [distributor], 2005-04-29).
Harkavy, Oscar, Frederick S. Jaffe, Samuel M. Wishik. (1969). "Family Planning and Public Policy: Who Is Misleading Whom?" *Science*, New Series, Vol. 165, No. 3891, Jul. 25: 367-373.
Hemminki, Elina, Donald McNellis, and Howard J. Hoffman. (1987). "Patterns of Prenatal Care in the United States." *Journal of Public Health Policy* 8: 330-50.
Hotz, Joseph V., Jacob A. Klerman and Robert J. Willis. (1997). "The Economics of Fertility in Developed Countries," *Handbook of Population Economics*, volume 1A: 275-348.
Hoynes, Hilary and Diane Schanzenbach. (2009). "Consumption Reponses to In-Kind Transfers: Evidence from the Introduction of the Food Stamp Program." *American Economic Journal: Applied Economics* 1(4): 109-39.
Jacobson, Louis S., Robert J. LaLonde, and Daniel G. Sullivan. (1993). "Earnings Losses of Displaced Workers." *American Economic Review* 83 (4): 685-709.
Jaffe, Frederick S., Joy G. Dryfoos, and Marsha Corey. (1973). "Organized Family Planning Programs in the United States: 1968-1972." *Family Planning Perspectives* 5 (2): 73-79.
Joshi, Shareen and T. Paul Schultz. (2007). "Family Planning as an Investment in Development: Evaluation of a Program's Consequences in Matlab, Bangladesh." *Yale Economic Growth Center Discussion Paper No. 951*.
Kearney, Melissa S. and Phillip B. Levine. (2007). "Subsidized Contraception, Fertility, and Sexual Behavior." *NBER Working Paper 13045*, April.
Knowles, James, John S. Akin, David K. Guilkey. (1994). "The Impact of Population Policies: Comment." *Population and Development Review* 20 (3): 611-615.
Levitan, Sar. (1969). The Great Society's Poor Law: A New Approach to Poverty. (Baltimore: Johns Hopkins).
Ludwig, Jens and Douglas L. Miller. (2007). "Does Head Start Improve Children's Life Chances? Evidence from a Regression Discontinuity Design." *Quarterly Journal of Economics* 122 (1): 159–208.
Mellor, Jennifer M. (1998). "The Effect of Family Planning Programs on the Fertility of Welfare Recipients: Evidence from Medicaid Claims." *Journal of Human Resources* 33: 866-895.
Michael, Robert T. and Robert J. Willis. (1976). "Contraception and Fertility: Household Production under Uncertainty," in *Demographic Behavior of the Household*. (Cambridge, MA: National Bureau of Economic Research).
Miller, Grant. (2005). "Contraception as Development? New Evidence from Family Planning in Colombia." *National Bureau of Economic Research Working Paper No. 11704*, October.
Moore, Kristen A. and Steven B. Caldwell. (1977). "The Effect of Government Policies on Out-of-Wedlock Sex and Pregnancy." *Family Planning Perspectives* 9 (4): 164-69.
National Academy of Sciences. (1963). "The Growth of World Population, Analysis of the Problems and Recommendations for Research and Training." April 11, Publication No. 1091, Washington, D.C.
National Center for Health Statistics (NCHS). (2003). Natality Detail File, 1968-1988: [UNITED STATES] [Computer file]. Hyattsville, MD: U.S. Dept. of Health and Human Services, National Center for Health Statistics [producer], 1981. Ann Arbor, MI: Inter-university Consortium for Political and Social Research [distributor].
-----. (2008). Multiple Cause of Death Files, 1959-1988 [Computer file]. (Ann Arbor, MI: Inter-university Consortium for Political and Social Research [distributor]).

National Fertility Study, 1970 [machine readable data file]. Survey conducted by Norman B. Ryder and Charles F. Westoff for the Office of Population Research; data file edition was prepared under the direction of Larry Bumpass, Center for Demography and Ecology, University of Wisconsin, Madison (Princeton, NJ: Office of Population Research [producer], 1970; Madison, WI: University of Wisconsin Data and Program Library Service [distributor], 1976).

Office of Economic Opportunity. (1969). *The Need for Subsidized Family Planning Services: United States, Each State and County, 1968*. (Washington, DC: Government Printing Office).

-----. (1971). The Need for Subsidized Family Planning Services: United States, Each State and County, 1969. (Washington, D.C: Government Printing Office).

-----. (1974). *The Need for Subsidized Family Planning Services: United States, Each State and County, 1971*. (Washington, D.C: Government Printing Office).

Office of Population Affairs. (2009). http://www.hhs.gov/opa/about/budget (accessed February 7, 2009).

Pritchett, Lant H. (1994a). "Desired Fertility and the Impact of Population Policies." *Population and Development Review* 20 (1): 1-55.

----. (1994b). "Impact of Population Policies: Reply." *Population and Development Review* 20 (3): 621-630.

Regional Economic Information System (REIS), 1969-1989. Bureau of Economic Analysis, U.S. Department of Commerce, CIESIN (http://www.ciesin.org/datasets/reis/reis-home.html, accessed 2/2009).

Salehi-Isfahani, Djavad, M. Jalal Abbasi-Shavazi, and Meimanat Hosseini-Chavoshi. (2008). "Family Planning and Rural Fertility Decline in Iran: A Study in Program Evaluation." http://www.internationalpolicy.umich.edu/edts/pdfs/isfahani-10-30-08.pdf (accessed 11/3/ 2008).

Rosenzweig, Mark R. and T. Paul Schultz. (1985). "The Demand for and Supply of Births: Fertility and its Life Cycle Consequences." *American Economic Review* 75 (5): 992-1015.

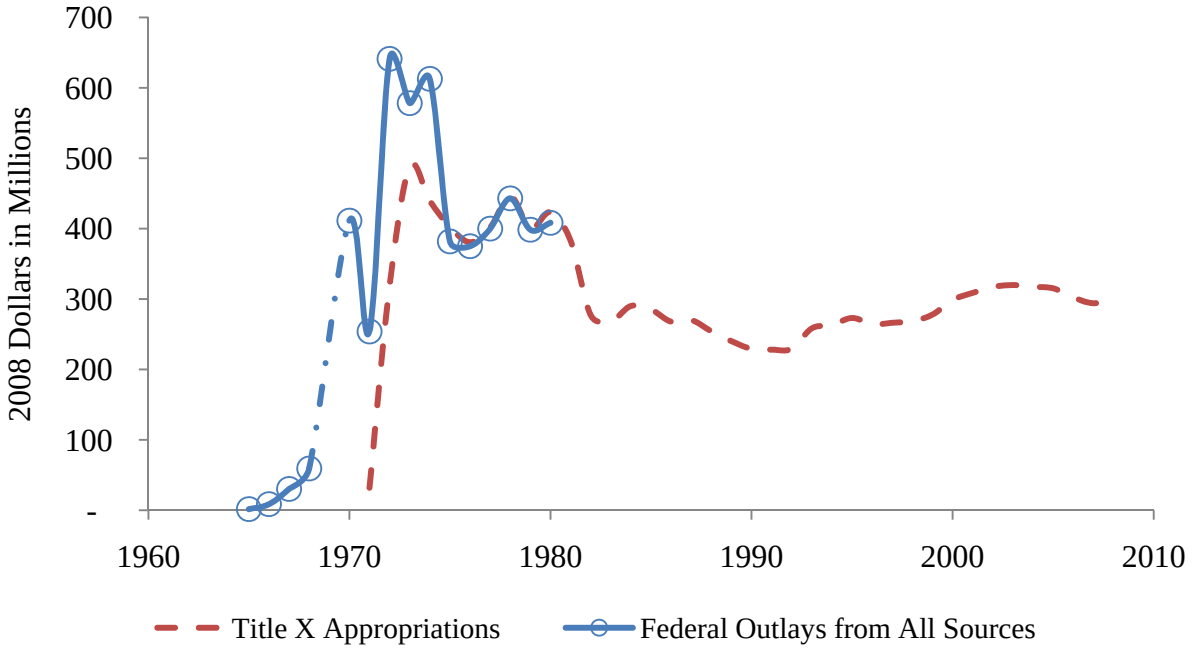
Surveillance, Epidemiology, and End Results (SEER) Program Populations (1969-1988). National Cancer Institute, DCCPS, Surveillance Research Program, Cancer Statistics Branch, released February 2009. (www.seer.cancer.gov/popdata, accessed 3/01/2009),

Tone, Andrea. (2001). *Devices and Desires: A History of Contraceptives in America*. (New York, NY: Hill and Wang).

Udry, J. Richard, Karl E. Bauman, and Naomi M. Morris. (1976). "The Effect of Subsidized Family Planning Services on Reproductive Behavior in the United States." *Demography* 13 (4): 463-78.

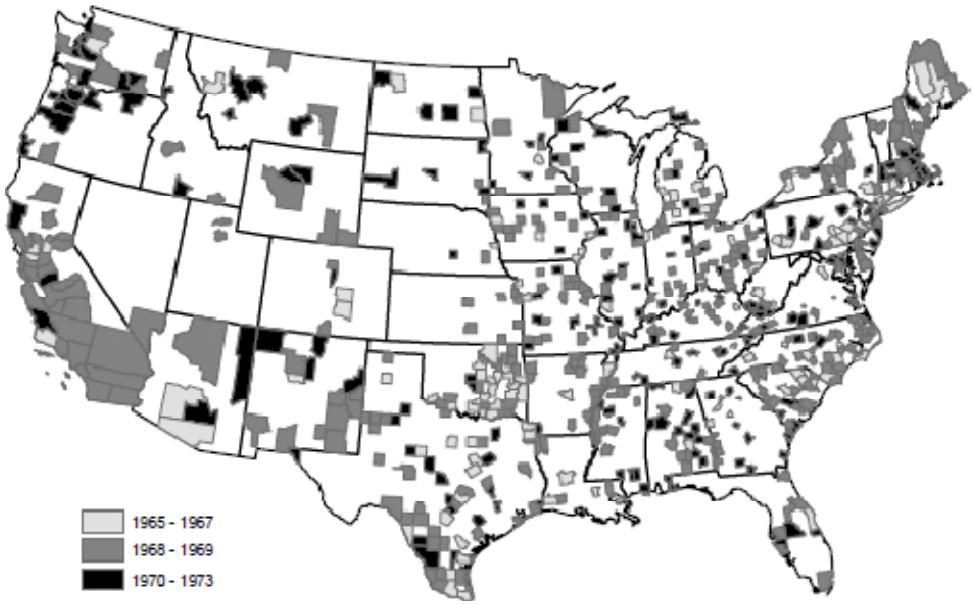
Weingarden, C.R. (1974). "The Fertility of AFDC Women: An Econometric Analysis." *Journal of Economics and Business* 26 (3): 159-66.

Figure I. Federal Spending on Family Planning, 1965-2008



Notes: No information is available for 1969, so a dashed line connects the 1968 and 1970 points. Title X appropriations series differs from the inflation adjusted table 14 (p. 47) in Alan Guttmacher Institute (AGI) (2000), because I use the CPI-U and AGI (2000) uses the CPI for medical care. Source: Title X appropriations are taken from the Office of Population Affairs (2009). *Federal Outlays from All Sources* from 1965 to 1980 are computed by the author using the National Archives Community Action Program Data (NACAP) and the National Archives Federal Outlays Data (NAFO).

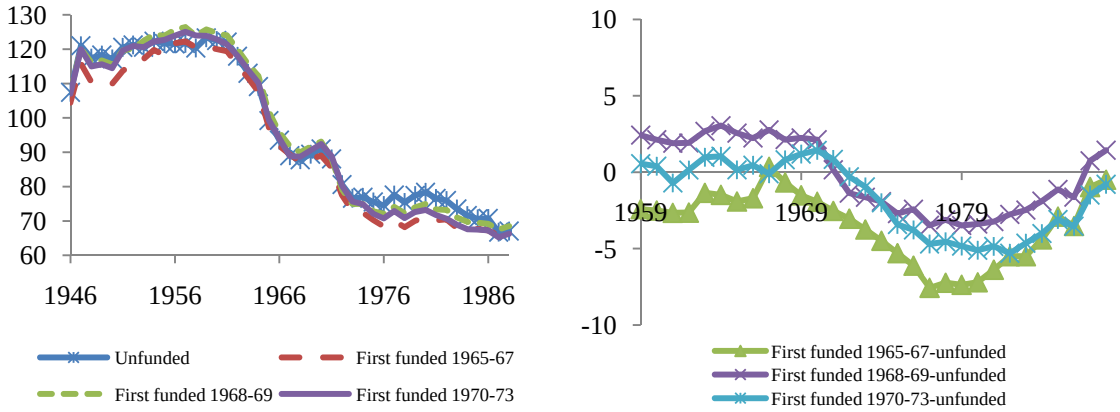
Figure II. The Date of the First Federal Family Planning Grant, 1965-1973



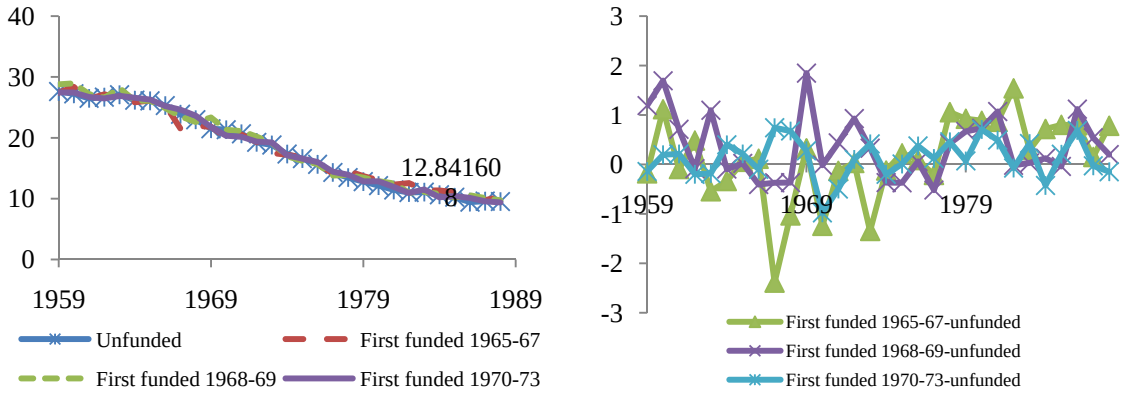
Note: Dates are the year that the county first received a federal grant. Counties not receiving a family planning grant between 1965 and 1973, including communities that received funding but with an unknown starting date, are not shaded. Source: NACAP, NAFO and OEO (1969, 1971 and 1974).

Figure III. Descriptive Evidence of the Effects of Family Planning on Fertility and Mortality

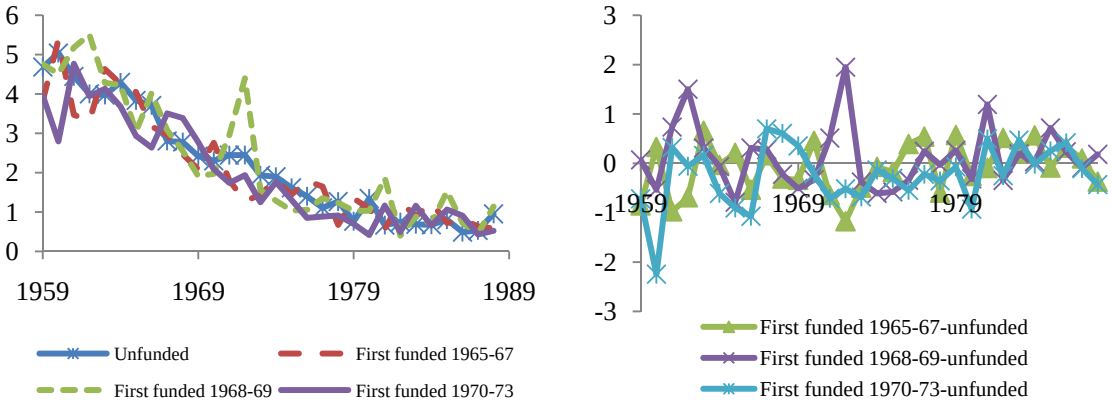
A. General Fertility Rate (GFR)



B. Infant Mortality Rate (IMR)



C. Maternal Mortality Rate (MMR)

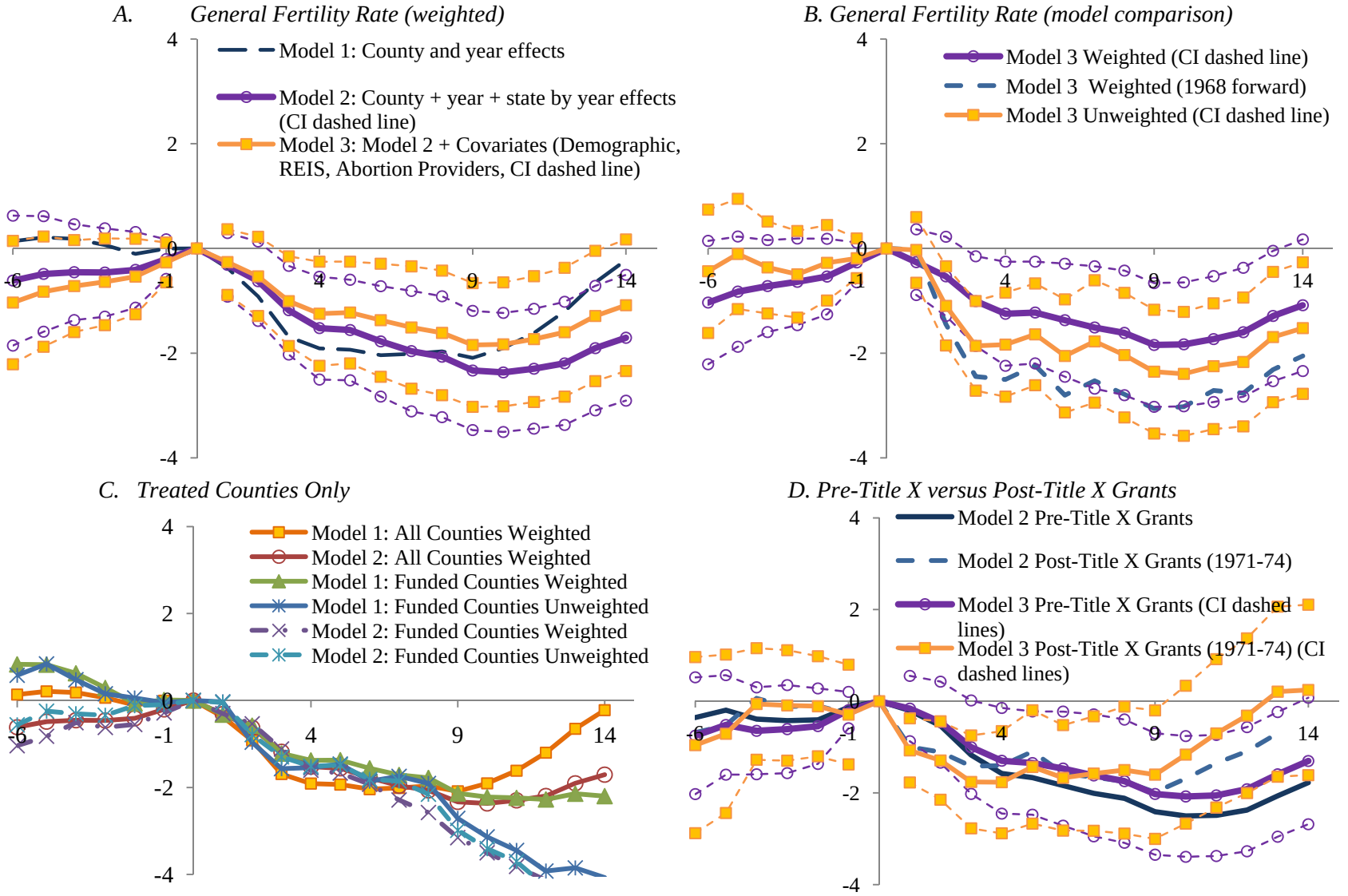


Notes: Left figures plot the evolution of the general fertility, infant mortality rate, and maternal mortality rate as indicated. Right figures plot the annual difference in outcomes between funded and unfunded counties by year of first funding. All estimates are unweighted. Sources: Grant timing: NACAP, NAFO and OEO (1969, 1971 and 1974). GFR: Hand-entered 1959 to 1967 county vital statistics and 1968 to 1988 natality detail microdata files (NCHS 2003). Denominators were constructed for 1959 to 1968 by linearly interpolating information between the 1950, 1960, and 1970 censuses; denominators for the 1969 to 1988 period use the Surveillance Epidemiology and End Results (SEER) data. IMR/MMR: Numerators are computed from the multiple-cause-of-death mortality microdata, 1959-1988. Denominators obtained as described for the GFR (NCHS 2003).

Figure IV. Correspondence in Timing of First Family Planning Grants and Other Federal Grants

Notes: Each panel plots weighted least-squares estimates of π and τ from equation 4 excluding covariates in X (equivalent to model 2). The weights are the 1970 population of women ages 15 to 44. The dependent variable is equal to 1 if the county received *any* federal grant for the indicated program. Heteroskedasticity-robust standard errors clustered by county are used to construct point-wise, 95-percent confidence intervals, which are presented in dashed lines in each panel. Numerical estimates appear in online appendix. Source: NACAP and NAFO.

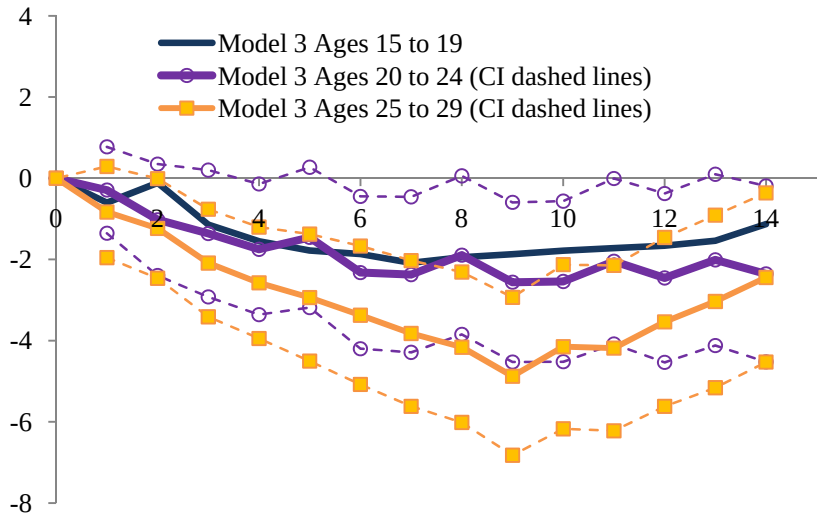
Figure V. Estimates of the Effects of Family Planning Grants on Fertility

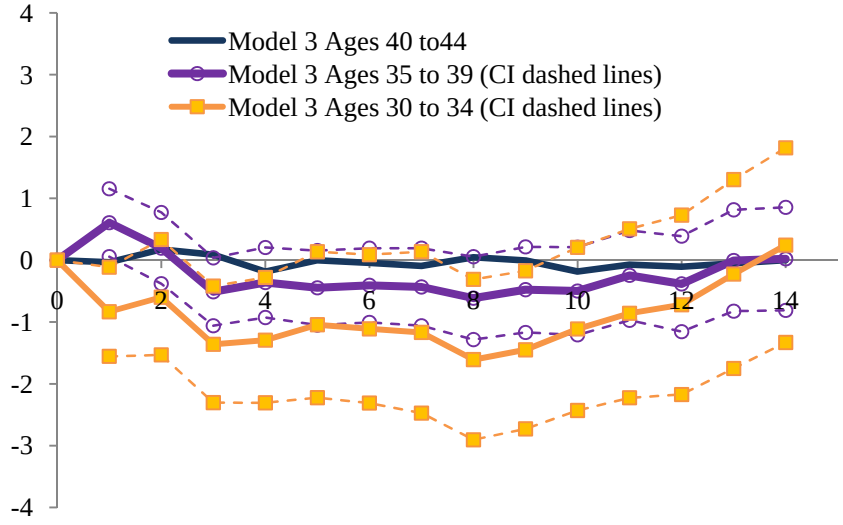


Notes: Panels plot either weighted or unweighted estimates of π and τ from equation 4. Weights are the county population of women ages 15 to 44 in 1970. Heteroskedasticity-robust standard errors clustered by county construct 95-percent, point-wise confidence intervals (dashed lines).Sources: See figure III notes.

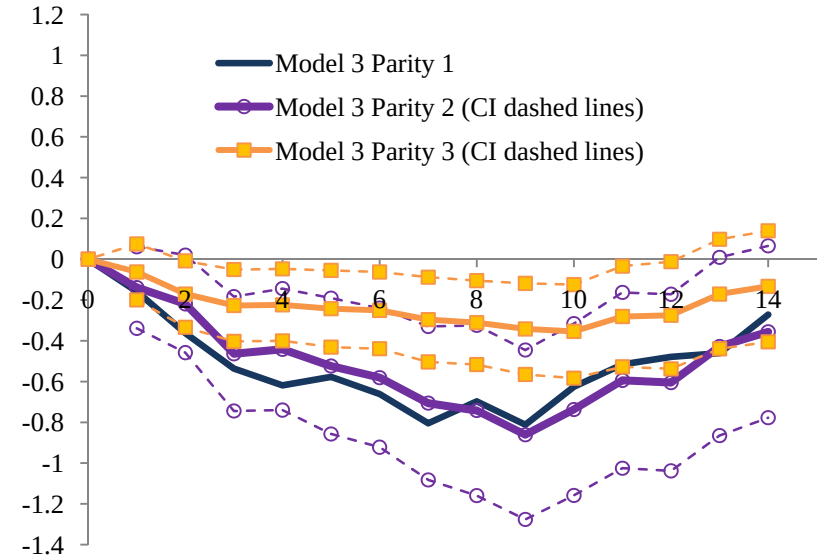
Figure VI. Estimates of the Effects of Family Planning Grants on Fertility by Age Group, Parity and Race

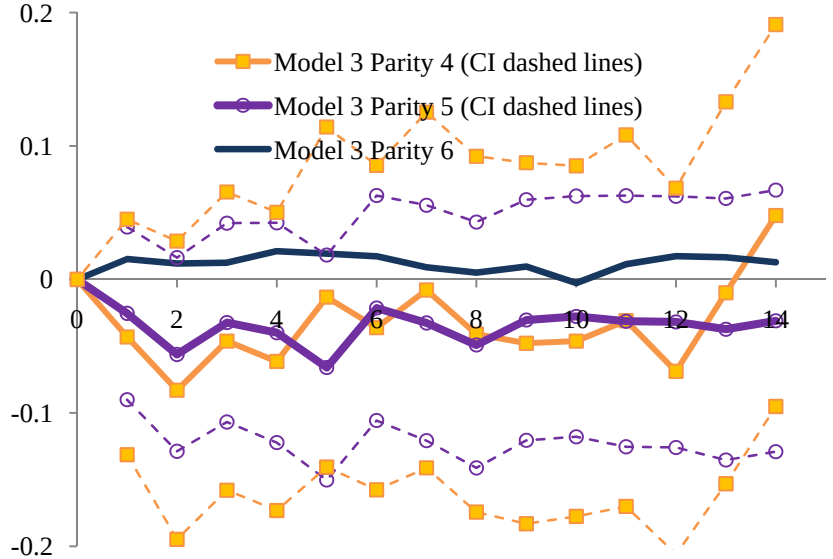
A. Birth Rate by Age Group, 1968-1988



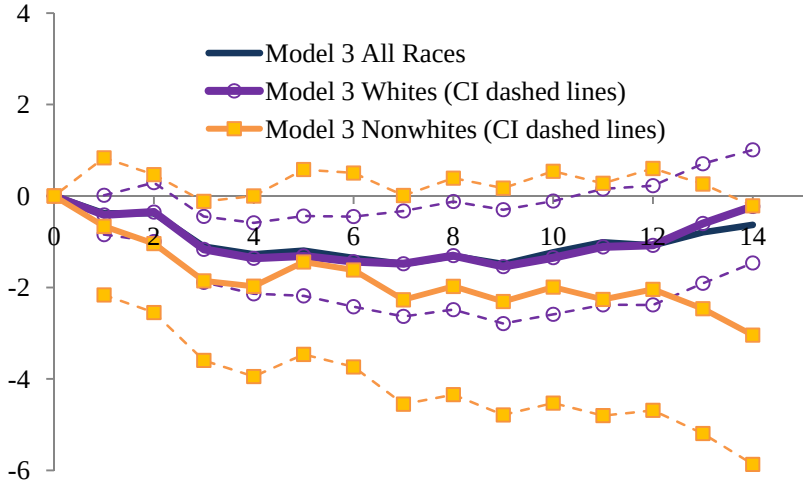


B. By Birth Parity, 1968-1988





C. By Race, 1968-1988

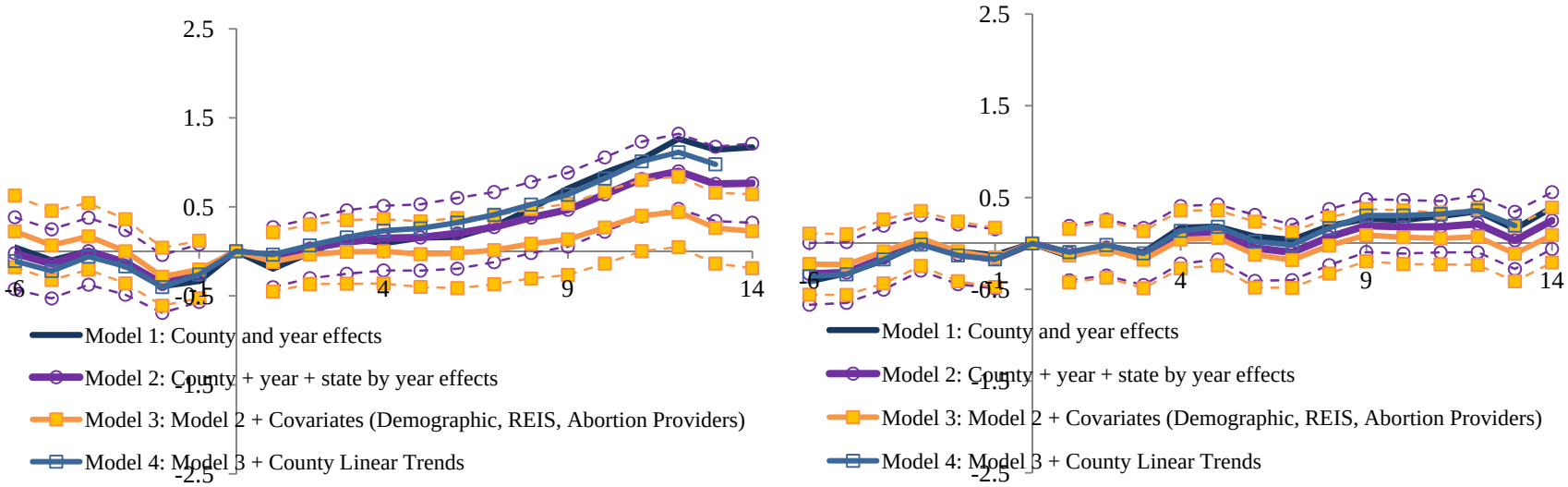


Notes: Panels present weighted, least-squares estimates of τ from equation 4 for the indicated group for the 1968 to 1988 data. Sources: See table IV notes.

Figure VII. Estimates of the Effects of First Family Planning Grants on Mortality

A. Infant Mortality Rate

B. Maternal Mortality Rate



Notes: Panels present weighted, least-squares estimates of τ from separate regressions for IMR and MMR. Sources: See figure III notes.

Table I. 1960 Characteristics of Counties Receiving Family Planning Grants from 1965 to 1973

	(1) All Counties	(2) Not Funded	(3) First Funded in years			(4) All Funded	T-test of (4)-(2) (std. error)
			1965-7	1968-9	1970-3		
Number of counties	3037	2381	123	332	201	656	
Mean population	55,400	30,660	239,474	139,331	97,176	145,193	114,532
Proportion of 1960 population in counties							(12,903)
in Northeast	26.0	26.0	31.8	16.9	38.9	16.6	9.4
							(4.96)
in Midwest	24.0	24.0	20.9	27.4	23.1	38.6	-14.1
							(6.23)
in South	26.9	26.9	29.0	24.4	28.4	37.3	-10.7
							(4.85)
in West	22.8	22.8	18.4	31.2	9.6	7.4	15.32
							(5.67)
in urban areas	68.4	53.7	85.6	79.4	71.8	79.7	26.0
							(4.18)
in rural/farm areas	7.9	13.3	2.2	4.2	5.0	3.7	-9.60
							(1.21)
Proportion of 1960 county residents							
Under 5 years of age	11.5	11.5	11.4	11.6	11.5	11.3	0.227
							(0.12)
65 or older	8.8	8.8	8.9	8.6	9.1	9.8	-1.00
							(0.20)
Nonwhite	10.6	10.6	12.8	10.4	7.8	11.3	-0.694
							(1.05)
with 12 years of education	44.0	44.0	43.2	45.0	42.9	40.9	3.06
							(3.88)
with fewer than 4 years of education	7.5	7.5	7.9	7.1	7.8	9.6	-2.14
							(0.40)
of households with income under $3,000	18.0	18.0	17.4	17.9	18.9	27.1	-9.13
							(1.45)
of households with income over $10,000	17.1	17.1	17.4	17.7	15.4	11.7	5.45
							(1.26)

Notes: Characteristics except for population are weighted by 1960 county population. Source: 1960 County and City Databooks (Haines 2005). Information on funding drawn from NACAP, NAFO and OEO (1969, 1971 and 1974) as described in text.

Table II. The Effects of First Family Planning Grants on Use of Family Planning Services

	(1)	(2)	(3)
	A. DV: Share of Medically-Indigent Patients Using Family Planning Services (1968 Mean=0.0458)		
1(Year>First Federal Grant)	0.045	0.047	0.045
	[0.008]	[0.008]	[0.008]
R-squared	0.75	0.78	0.78
Counties	3051	3051	3051
Observations	9153	9153	9153
Covariates	C,Y	C,S-Y	C,S-Y,R,X
	B. DV: 1=Ever Used Family Planning Clinic		
Federal Grant before 1970	0.003	-0.007	-0.007
(Mean DV=0.070)	[0.008]	[0.008]	[0.008]
In Poverty	0.068	0.045	0.045
(Mean DV=0.150)	[0.021]	[0.018]	[0.019]
Federal Grant x In Poverty	0.052	0.037	0.041
(Mean DV=0.211)	[0.030]	[0.026]	[0.028]
Pseudo R-squared	0.079	0.141	0.146
Observations	5946	5946	5874
State fixed effects	X	X	X
Other covariates		A,C,E,P	A,C,E,P,K
	C. DV: 1=Ever Used the Pill		
Federal Grant before 1970	0.038	0.027	0.026
(Mean DV=0.63)	[0.015]	[0.015]	[0.015]
In Poverty	-0.096	-0.104	-0.104
(Mean DV=0.56)	[0.033]	[0.029]	[0.029]
Federal Grant x In Poverty	0.106	0.084	0.084
(Mean DV=0.65)	[0.049]	[0.045]	[0.044]
Pseudo R-squared	0.026	0.158	0.167
Observations	5946	5946	5874
State fixed effects	X	X	X
Other covariates		A,C,E,P	A,C,E,P,K

Notes: Panel A. The unit of observation is a county-year in FY1968, CY1969 and FY1971), and estimates are of τ from equation 6. Column 1 includes county, C, and year, Y, fixed effects. Column 2 adds state-by-year, S-Y, fixed effects. Column 3 adds 1960 county covariates interacted with a linear trend, X, and REIS controls, R. Heteroskedasticity-robust standard errors clustered by county are presented beneath each estimate in brackets. Sources: OEO 1969, 1971 and 1974. Panels B and C: The unit of observation is a married woman ages 18 to 44 in 1970. The estimates are average partial effects associated with θ_1, θ_2, and θ_3 from equation 7. Bootstrapped standard errors (1000 replications) are reported in brackets beneath. Columns 1-3 include state fixed effect, column 2 adds dummy variables for age categories (A), Catholic (C), educational achievement (E), and PSU size (P); and column 3 adds a set of dummy variables for the "ideal number of children" to proxy for other differences in the demand for children (K). Source: 1970 *National Fertility Survey*.

Table III. Summary of the Effects of First Family Planning Grants on the General Fertility Rate

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	----------All Counties (1959-88)----------				Funded Counties Only	Pre-Title X Grants Only	Post-Title X Grants Only	All Counties (1968-88)
A. Weighted Mean DV in 1970	87.6				86.7	87.9	88.2	87.6
Years –5 to –1	0.0830	-0.327	-0.525	-0.273	-0.494	-0.408	-0.221	
	[0.425]	[0.321]	[0.310]	[0.312]	[0.376]	[0.326]	[0.510]	
Years 1 to 5	-1.268	-0.959	-0.776	-0.945	-1.047	-0.754	-1.411	-0.990
	[0.425]	[0.380]	[0.383]	[0.402]	[0.689]	[0.438]	[0.448]	[0.267]
Years 6 to 10	-1.805	-1.901	-1.446	-1.832	-1.687	-1.558	-1.445	-1.762
	[0.672]	[0.529]	[0.541]	[0.592]	[0.907]	[0.603]	[0.637]	[0.462]
Years 11 to 15	-0.787	-1.829	-1.248	-1.803	-1.738	-1.484	-0.115	-1.225
	[0.842]	[0.567]	[0.586]	[0.692]	[1.086]	[0.636]	[0.867]	[0.508]
R-squared	0.874	0.909	0.925	0.945	0.938	0.923	0.910	0.793
B. Unweighted Mean DV in 1970	91.1				86.7	90.1	90.9	91.1
Years –5 to –1	0.126	0.169	-0.262	-0.127	-0.378	-0.311	-0.0403	
	[0.399]	[0.408]	[0.398]	[0.419]	[0.475]	[0.479]	[0.662]	
Years 1 to 5	-2.158	-1.728	-1.251	-1.441	-0.588	-1.194	-1.589	-1.649
	[0.375]	[0.376]	[0.370]	[0.391]	[0.837]	[0.447]	[0.589]	[0.409]
Years 6 to 10	-4.846	-3.285	-2.051	-2.540	-1.322	-2.115	-2.006	-2.732
	[0.526]	[0.490]	[0.485]	[0.560]	[1.138]	[0.570]	[0.854]	[0.528]
Years 11 to 15	-5.187	-3.649	-1.834	-2.613	-1.799	-2.082	-1.070	-2.355
	[0.577]	[0.525]	[0.518]	[0.691]	[1.394]	[0.607]	[0.876]	[0.569]
R-squared	0.693	0.753	0.774	0.812	0.846	0.770	0.759	0.481
For panels A and B:								
Observations	90949	90949	90949	90949	19638	86833	75427	61167
Counties	3037	3037	3037	3037	656	2899	2519	2914
Covariates	Model 1: C,Y	Model 1 +S-Y	Model 2 +R,A,D	Model 3 +Ctrend	Model 2	Model 3	Model 3	Model 3

Notes: Panels A (weighted by 1970 population of women ages 15 to 44) and B (unweighted) display least-squares estimates of equation 5 using the GFR as the dependent variable. Column 1 corresponds to model 1 and includes county, C, and year, Y, effects. Column 2 corresponds to model 2 and adds state-by-year, S-Y, effects to model 1. Column 3 corresponds to model 3 and adds 1960 county covariates including Demographic, D, REIS, R, and abortion, A, controls to model 2. Column 4 adds linear county trends, Ctrend, to model 3. Column 5 estimates model 2 for funded counties only and omits all unfunded counties. Column 6 omits counties receiving first grants after 1970 and includes unfunded counties. Column 7 omits counties receiving first grants before 1971 and includes unfunded counties. Column 8 uses all counties receiving grants from 1968 to 1973 and unfunded counties for the short 1968 to 1988 panel (for comparison with tables V). Heteroskedasticity-robust standard errors clustered by county are presented beneath each estimate in brackets. Source: See figure III notes.

Table IV. Summary of the Effects of First Family Planning Grants by Age Group, Parity and Race

	(1)	(2)	(3)	(4)	(5)	(6)
			A. DV: Age-Specific Birth Rate			
Age Group	15-19	20-24	25-29	30-34	35-39	40-44
1970 Mean DV	67.8	167.5	144.5	72.7	31.7	8.2
Year 1-5	-0.387	-1.863	-2.252	-1.273	-0.582	-0.184
	[0.390]	[0.694]	[0.558]	[0.426]	[0.362]	[0.132]
Years 6-10	-1.286	-2.411	-3.871	-1.110	-0.550	-0.154
	[0.547]	[0.891]	[0.798]	[0.579]	[0.396]	[0.144]
Years 11-15	-1.123	-2.635	-3.125	-0.412	-0.331	-0.147
	[0.638]	[1.077]	[0.923]	[0.689]	[0.450]	[0.156]
R-squared	0.496	0.791	0.602	0.552	0.595	0.454
Observations	60845	61156	61124	60825	59675	56319
Counties	2914	2914	2914	2914	2914	2912
			B. DV: Parity-Specific Birth Rate			
Parity	One	Two	Three	Four	Five	Six
1970 Mean DV	33.7	23.8	13.5	7.12	3.77	2.04
Years 1-5	-0.426	-0.333	-0.171	-0.0492	-0.0417	0.0154
	[0.187]	[0.112]	[0.0719]	[0.0461]	[0.0323]	[0.0237]
Years 6-10	-0.677	-0.673	-0.286	-0.0364	-0.0277	0.00748
	[0.280]	[0.181]	[0.0973]	[0.0607]	[0.0417]	[0.0311]
Years 11-15	-0.399	-0.445	-0.190	-0.0151	-0.0290	0.0151
	[0.311]	[0.204]	[0.126]	[0.0674]	[0.0462]	[0.0339]
R-squared	0.502	0.346	0.555	0.714	0.726	0.670
Observations	61109	61109	61109	61109	61109	61109
Counties	2911	2911	2911	2911	2911	2911

C. *DV*: *Race-Specific GFR* (Only 1479 Counties with Both Race Groups)

Race Group	All	White	Nonwhite
1970 Mean DV	87.3	82.8	115.2
Years 1-5	-0.749	-0.791	-1.293
	[0.283]	[0.291]	[0.712]
Years 6-10	-1.196	-1.210	-1.895
	[0.497]	[0.527]	[1.059]
Years 11-15	-0.740	-0.595	-2.322
	[0.556]	[0.593]	[1.252]
R-squared	0.834	0.821	0.659
Observations	31059	31059	31059
Counties	1479	1479	1479

Notes: The table presents weighted least-squares estimates obtained from estimating equation 5 separately for indicated groups. All specifications are model 3. H eteroskedasticity-robust standard errors clustered by county are beneath each estimate in brackets. Source: County-age group, county-parity, and county-race group are only available from 1968-88, so the regressions use 1968 to 1988 natality microdata aggregated to county (NCHS 2003).

Table V. Summary of the Effects of First Family Planning Grants on Mortality

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. Infant Mortality	----------Weighted----------				----------Unweighted----------			
1970 Mean DV	19.8				21.2			
Years –5 to –1	-0.225	-0.199	-0.0789	-0.162	-0.242	-0.0946	-0.125	0.0728
	[0.154]	[0.142]	[0.141]	[0.148]	[0.401]	[0.415]	[0.414]	[0.435]
Years 1 to 5	0.0338	0.0639	-0.0508	0.0363	-0.116	0.0198	0.0522	-0.140
	[0.153]	[0.141]	[0.140]	[0.151]	[0.411]	[0.429]	[0.428]	[0.450]
Years 6 to 10	0.449	0.346	0.0528	0.262	-0.198	-0.0650	-0.0432	-0.557
	[0.212]	[0.178]	[0.172]	[0.218]	[0.437]	[0.454]	[0.456]	[0.544]
Years 11 to 15	1.071	0.755	0.284	0.604	0.181	0.234	0.238	-0.572
	[0.230]	[0.193]	[0.180]	[0.257]	[0.448]	[0.461]	[0.465]	[0.659]
R-squared	0.608	0.636	0.646	0.672	0.254	0.278	0.283	0.315
Observations	91110	91110	91110	91110	91110	91110	91110	91110
Counties	3037	3037	3037	3037	3037	3037	3037	3037
B. Maternal Mortality	----------Weighted----------				----------Unweighted----------			
1970 Mean DV	2.01				2.27			
Years –5 to –1	-0.143	-0.154	-0.104	-0.0813	0.436	0.309	0.320	0.556
	[0.140]	[0.127]	[0.127]	[0.132]	[0.323]	[0.328]	[0.327]	[0.350]
Years 1 to 5	0.0101	-0.0161	-0.0580	-0.0743	0.281	0.230	0.219	-0.00512
	[0.120]	[0.123]	[0.123]	[0.133]	[0.332]	[0.344]	[0.343]	[0.370]
Years 6 to 10	0.137	0.0335	-0.0637	-0.114	0.225	0.185	0.138	-0.422
	[0.129]	[0.134]	[0.133]	[0.165]	[0.291]	[0.301]	[0.302]	[0.443]
Years 11 to 15	0.272	0.140	-0.00664	-0.0890	0.602	0.593	0.508	-0.347
	[0.131]	[0.137]	[0.135]	[0.200]	[0.305]	[0.317]	[0.317]	[0.617]
R-squared	0.053	0.073	0.081	0.116	0.015	0.031	0.033	0.066
Observations	91110	91110	91110	91110	91110	91110	91110	91110
Counties	3037	3037	3037	3037	3037	3037	3037	3037
Covariates	C,Y	Model 1 +S-Y	Model 2 +R,A,D	Model 3 +Ctrend	C,Y	Model 1 +S-Y	Model 2 +R,A,D	Model 3 +Ctrend

Notes: The dependent variable is either the IMR or MMR as indicated. Each panel plots weighted (columns 1-4) and unweighted (columns 5-8) least-squares estimates of equation 5. Columns 1 and 5 corresponds to model 1 and includes county, C, and year, Y, effects. Columns 2 and 6 correspond to model 2 and add state-by-year, S-Y, effects to model 1. Columns 3 and 7correspond to model 3 and adds 1960 county covariates including Demographic, D, REIS, R, and abortion, A, controls to model 2. Columns 4 and 8 add linear county trends, Ctrends, to model 3. Heteroskedasticity-robust standard errors clustered by county are presented beneath each estimate in brackets. Source: See figure III notes.