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REEXAMINING THE IMPACT OF FAMILY PLANNING PROGRAMS ON U.S. FERTILITY: EVIDENCE FROM THE WAR ON POVERTY AND THE EARLY YEARS OF TITLE X

Martha J. Bailey

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Reexamining the Impact of Family Planning Programs on U.S. Fertility: Evidence from the War on Poverty and the Early Years of Title X

Martha J. Bailey

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ABSTRACT

Almost 50 years after domestic U.S. family planning programs began, their effects on childbearing remain controversial. Using the county-level roll-out of these programs from 1964 to 1973, this paper reevaluates their shorter- and longer-term effects on U.S. fertility rates. I find that the introduction of family planning is associated with significant and persistent reductions in fertility driven both by falling completed childbearing and childbearing delay. Although federally-funded family planning accounted for a small portion of the post-baby boom U.S. fertility decline, the estimates imply that they reduced childbearing among poor women by 21 to 29 percent.

Martha J. Bailey
University of Michigan
Department of Economics
611 Tappan Street
207 Lorch Hall
Ann Arbor, MI 48109-1220
and NBER
baileymj@umich.edu

"[L]ess than five dollars invested in population control is worth a hundred dollars invested in economic growth." ~President Lyndon B. Johnson, 1965

"It is my view that no American woman should be denied access to family planning assistance because of her economic condition. I believe, therefore that we should 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. This we have the capacity to do." ~President Richard Nixon, 1969

The 1960s marked an important turning point in U.S. population policy. Motivated by concerns over the international "population explosion" and the high fertility rates of the U.S. baby boom, domestic family planning grants began under the 1964 Economic Opportunity Act, the centerpiece of President Johnson's War on Poverty, and continued under Title X of the 1970 Public Health Service Act, signed into law by President Nixon. The rationale for family planning programs was twofold. By subsidizing contraception, family planning programs would promote greater economic opportunities for disadvantaged women, who "do not want more children than do families with higher incomes" but "do not have the information or the resources to plan their families effectively according to their own desires" (National Academy of Sciences 1963). In addition, the architects of the War on Poverty viewed reducing unwanted and ill-timed childbearing as a means to promoting opportunities for children and, thus, achieving broader and longer-term economic prosperity.

More than forty years later, the achievements of the U.S. family planning program remain an open question. In fact, even the fertility effects of these programs remain controversial. Although many economic models predict that family planning programs could reduce childbearing (Becker and Lewis 1973, Michael and Willis 1976), incorporating marriage-market effects or moral hazard dimensions (Akerlof et al. 1996, Levine et al. 1996, Ananat et al. 2009) can generate predictions that family planning programs increase childbearing. This theoretical ambiguity has not been resolved in the empirical literature. The conclusions of observational studies on family planning have been limited by well-known omitted variables and endogeneity problems (Rosenzweig and Wolpin 1986, Hotz et al. 1997, Mellor 1998), and randomized trials have found almost no evidence that family planning affects teen childbearing in the U.S. (Kirby 1997, DiCenso et al. 2002).[1] Although the null results of these trials are inconclusive due to their

[1] A handful of quasi-experimental studies in developing countries examine the longer-term effects of family planning programs on completed fertility (Joshi and Schultz 2007, Bangladesh; Salehi-Isfahani et al. 2008, Iran; and Miller 2009, Colombia), and a

short time horizons (which neglect longer-term effects), small sample sizes (which limit precision), and focus on teens (who may respond differently than non-teens), they provide no support for the view that family planning reduces births.[2] Kearney and Levine's (2009) differences-in-differences study of changes in state-level Medicaid eligibility for family planning provides the best available evidence that domestic programs reduce births for women near the poverty line for at least two years. Because their identification strategy relies upon recent policy changes that affected women who were just eligible for Medicaid, it is unclear how their results generalize to women with higher earnings or whether their shorter-term reductions in childbearing persist in the *longer-term*—effects important for understanding how family planning programs have shaped family size, the intergenerational transmission of poverty, and longer-term economic development.

This paper exploits the roll-out of federally-funded family planning programs from 1964 to 1973 to evaluate both their shorter and longer-term effects on childbearing. The "wild sort of grant-making operation" (Gillette 1996: 193) during the period provides both a plausible, county-level identification strategy and allows for the effects of federally-funded family planning to be evaluated over a fifteen years horizon. Using a balanced panel of U.S. counties from 1959 to 1988, I find that the introduction of family planning programs is associated with a significant and sustained reduction in childbearing. Five years after these programs were established, the general fertility rate was 2 percent lower *net* of fertility reductions in similar communities that did not receive federal family planning dollars. This reduction was partially driven by a delay in childbearing, as teen births and births to women in their early 20s remained 2 and 1.4 percent lower, respectively, five to ten years after communities received federally-funded family planning programs. But the decline in the fertility rate also reflected a reduction in completed childbearing, evinced by a longer-term, 2-percent reduction in second births. Together, delayed childbearing and reductions in higher-order births generated a decrease in the general fertility rate of 1.4 percent up to fifteen years after communities received their first federal family planning grants.

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recent randomized-control trial in Zambia examines the effects of spousal disagreement about the desired number of children—and the potential role of female contraception—in reducing fertility (Ashraf, Field, and Lee 2010). Although these studies suggest that family planning services should reduce childbearing, the findings do not easily generalize to the U.S., where differences in women's rights, knowledge and resources should imply different treatment effects.

[2] Helmerhorst et al. (2006) note additional limitations of published randomized trials including intentional exclusion of participants after randomization, failure to use intention-to-treat analyses, and lack of treatment blinding.

Because federally-funded family planning programs served mostly lower-income women and operated in only one fifth of all U.S. counties, the program accounted for a small portion of the large decline in the general fertility rate from 1959 to 1974. Nevertheless, the program had a profound effect on the women it served. The estimates imply that family planning programs reduced childbearing among poor women by 21 to 29 percent within 10 years. This effect is large enough to account for roughly half to three-quarters of the 1965 gap in childbearing between poor and non-poor women and suggests that federally-funded family planning substantially diminished the income-based differences in childbearing that initially motivated the program.

I. A Brief History of U.S. Family Planning Programs, 1964 to 1973

Today, the most effective contraceptive methods are scientifically tested, U.S. Food and Drug Administration approved, and medically prescribed. But historically, contraception was deemed obscene material and banned under federal and most state statutes (Tone 2001). After *Enovid*, the first birth control pill, was approved by the U.S. Food and Drug Administration in 1960, a series of legal changes at the national and state level removed restrictions on the shipping, manufacturing, and sales of contraceptives to married and, later, unmarried women (Bailey et al. 2011).

Legal access, however, did not guarantee access in practice. Although the Pill was popular, it was prohibitively expensive. Shortly after its release, an annual supply of *Enovid* sold for the equivalent of about $760 in 2010 dollars (Tone 2001: 257)–roughly twice today's annual cost and more than three weeks of full-time work at the 1960 minimum wage. Both widespread concern about population growth (Wilmoth and Ball 1992, 1995) and studies showing that lower income women were having more children than they desired (National Academy of Sciences 1963) galvanized support for federal intervention.

A. The Early Expansion of U.S. Family Planning Programs

Federal grants for family planning began under the Economic Opportunity Act (EOA, 1964), the cornerstone legislation of President Johnson's War on Poverty.[3] Although explicit language about family planning was not included in the EOA, the program fit within its funding authority. Sargent Shriver, the head of the Office of Economic Opportunity (OEO), began

[3] 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).

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 substantial increase in funding 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 (2010) dollars.

Like other OEO programs, *any* local organization could apply for family planning funding.[4] Unfortunately, both the sensitivity of the family planning program (contraception was still a taboo topic and its sales were illegal in many states in 1964) and its small size resulted in few written records about funding decisions in the National Archives. Family planning funding comprised less than 0.4 percent of the 7.6 billion dollars in OEO outlays from 1965 to 1969 and less than 3 percent of the Community Action Program budget. Gillette's (1996) oral histories and interviews with former OEO officials provide one of the richest pictures of the agency's functioning. OEO administrators report receiving applications for all of their programs 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). 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 the agency's urgency "to move fast to shovel out the money, because a few precious, perfect projects won't do anybody any good" (199).

The second large increase in funding for family planning occurred under the administration of Richard Nixon. His 1969 Special Message to the Congress on the Problems of Population Growth advocated that Congress "establish as a national goal the provision of

[4] This feature of the OEO funding was intended to empower those who had been excluded from local politics to create positive changes in their community. It is also closely linked to Civil Rights. The direct-to-local-organization granting structure made the War on Poverty programs controversial, because they challenged traditional political structures and entrenched local interests.

adequate family planning services within the next five years to all those who want them but cannot afford them." In November 1970, Title X of the Public Health Service Act (also known as the Family Planning Services and Population Research Act, P.L. 91-572) was enacted and called for comprehensive family planning services for all who wanted them. Importantly, Title X prohibited the use of federal funds "in programs where abortion is a method of family planning" (§ 1008). Under Title X, family planning grants were made directly from Department of Health Education and Welfare (DHEW) to local organizations in response to grant applications. With the new agency came important changes in policies, priorities, and administrative responsibility as well as a good deal of confusion. 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).

By 1973, federally supported family planning programs existed in 656 U.S. communities and served 1.9 million patients annually. By 1983, the number of patients had grown to almost 5 million annually (Dryfoos 1988). In the same year, roughly 83 percent of family planning patients were below 150 percent of the poverty line (13 percent were AFDC recipients); 70 percent of patients were white and roughly one quarter were black (284). From 1969 to 1983, service use increased by four times (from 1.2 million to almost 5 million), in large part due to strong federal support and rising support from state and local governments. Although the bulk of family planning funds was federal in the first decade of the program (Cutright and Jaffe 1977: 3), the Alan Guttmacher Institute (2000) estimated that, by 1980, 50 percent of public support for family planning came from sources other than Title X. By 1994, 80 percent of public support came from sources other than Title X (13).

II. Previous Research on U.S. Family Planning Programs

Although it has been almost 50 years since the first federally-funded family planning programs began, the empirical evidence that they reduce birth rates is remarkably thin. The earliest literature evaluating U.S. programs provides mixed evidence of its effects on fertility (see Mellor 1998 for a review).[5] Although these studies are closest to the period and programs

[5] See Mellor's comprehensive, interdisciplinary literature review. For instance, Damey (1975) and Moore-Caldwell (1977) find that measures of family planning use and availability, respectively, are associated with *reduced* birth rates among black women.

considered in this analysis, they are primarily cross-sectional and limited by well-known omitted variables and endogeneity problems (Rosenzweig and Wolpin 1986, Hotz et al. 1997).

More recent studies using randomized trials of interventions to reduce teen pregnancies overcome these methodological limitations, but generally find that family planning programs had no effect on teen pregnancy in the U.S.[6] Indeed, DiCenso et al.'s (2002) review and meta-analyses of 22 randomized studies of family planning, sex education, and abstinence interventions conducted from 1981 to 2000 concludes that the interventions did not increase the use of birth control among teens or reduce the number of teen pregnancies. Although these findings may not generalize to the broader population of U.S. women, they are hardly an endorsement of family planning's fertility effects. The absence of program effects may reflect the trials' short time horizons (treatment effects may take longer to manifest than the one to two years between baseline and follow-up time) or small sample sizes (even when pooled for the meta-analysis).[7]

Quasi-experimental methodologies, ideal for addressing both endogeneity and power problems in the observational and experimental literatures, have been difficult to implement in the U.S. context, because federal funds for family planning have changed little since 1980 (figure I). Kearney and Levine's (2009) state-level, differences-in-differences study provides the most compelling and direct evidence to date that U.S. family planning programs reduce birth rates. Using the state-by-year variation in Medicaid eligibility for family planning, they find that greater eligibility for family planning services in 17 states significantly reduced birth rates among teens (by 4 percent) and among older women (by 2 percent).

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. (1981) 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. (1976) find no significant relationship between average dollars spent per recipient of family planning services and births, whereas Cutright and Jaffe (1977) report that enrollment in family planning programs is associated with reductions in birth rates among blacks and whites. Weingarden (1974), Moore and Caldwell (1977), and Brann (1979) use cross-sectional comparisons at the state level. Damey (1975), Udry, Bauman, and Morris (1976), Cutright and Jaffe (1977), and Forrest, Hermalin, and Henshaw (1981) use cross-sectional comparisons of counties.

[6] Ashraf, Field and Lee (2010) find that the presence of a spouse when presenting wives with information about family planning has a substantial effect on their contraceptive use and unwanted childbearing in Zambia. The importance of spousal disagreement about the desired number of children may also play a role in the U.S. and affect the use of the Pill (because neither spousal knowledge nor consent is required to use it) as well as the use of free or reduced cost family planning clinics (as they eliminate the need to bargain with a spouse over spending money on an expensive contraceptive).

[7] Helmerhorst et al. (2006) note additional limitations of published randomized control trials including intentional exclusion of participants after randomization, the failure to use intention-to-treat analysis, and the lack of treatment blinding.

While suggestive, it is unclear how Kearney and Levine's local average treatment effects relate to the broader and longer-term effects of U.S. family planning policy for several reasons. First, the effects of a global change in family planning policy considered in this paper may affect women in different income ranges (Kearney and Levine were able to examine effects for women whose incomes were about 133 to 200 percent of the poverty line) or affect treated women differently. Second, the scale effects of family planning resources may be highly nonlinear. With diminishing returns to program scale (Schultz 1973, 1992), Kearney and Levine's marginal changes in program eligibility may *under*state Title X's overall effects on birth rates. Third, their analysis only recovers the two-year effects of changes in Medicaid eligibility for a balanced panel of states. To the extent that changes in shorter-term eligibility affect fertility due to better timing and increased birth intervals, their estimates may *over*state the effects of family planning on completed fertility as well as on period fertility over a five to ten year period. The ability to recover only shorter-term effects is by no means a limitation specific to Kearney and Levine's analysis. Schultz (2008) argues it is a general problem for studies of family planning. Although a handful of quasi-experimental studies in developing countries examine the longer-term effects of family planning programs on fertility, these studies do not easily generalize to the U.S., where women's rights, knowledge and resources imply considerably different treatment effects (Joshi and Schultz 2007, Bangladesh; Salehi-Isfahani et al. 2008, Iran; and Miller 2009, Colombia).[8]

In summary, the shorter and longer-term effects on U.S. childbearing remain an open question. The dearth of evidence reflects the difficulty of recovering causal effects with observational data or small-scale trials as well as the limited amount of variation in family planning policy since the 1980s. This paper contributes to this literature by exploiting county-level variation in the roll-out of federally-funded family planning programs between 1964 and 1973 to estimate the program's shorter and longer-term effects on U.S. childbearing. The next

[8] A closely-related quasi-experimental literature examines the effects of changes in *legal* access to abortion and the birth control pill on birth rates during the 1960s and the 1970s (Levine et al. 1996, Guldi 2008, Bailey 2010). Using the staggered legalization of first trimester abortion in five states in 1970 and in the remainder with *Roe v. Wade* in 1973, Levine et al. (1996a) show that birth rates fell more rapidly after 1970 in states with legal abortion. To examine the impact of the birth control pill on marital fertility, Bailey (2010) uses state-level variation in the language of Comstock statutes that banned the sales of contraceptives until 1965. Her analysis shows that fertility rates fell more rapidly in states without laws banning contraceptive sales. Guldi (2009) exploits state-level differences in the age of consent to show that the birth control pill reduced birth rates among younger, unmarried women as well. Although none of these studies considers the effects of greater *financial* access to family planning services on shorter and longer-term birth rates, several consider the impact of changes in Medicaid income eligibility on abortions and births (Levine et al. 1996b, Joyce et al. 1998).

section describes the newly collected data that facilitate the analysis and motivates the event-study identification strategy.

III. The Roll-Out of Family Planning Programs, 1964 to 1973

Newly compiled information on family planning grants allows the documentation of the establishment of federal programs in 656 U.S. communities from 1964 to 1973. The National Archives Community Action Program (NACAP) and National Archives Federal Outlay (NAFO) files provide information on (1) the county and state where services were delivered, which allows each program to be matched to the annual, county-level fertility statistics (the lowest level of geographic aggregation in the Vital Statistics records) and (2) the date of the *first* family planning grant, which provides a consistent proxy for when the program began operating.[9]

Figure II illustrates the roll-out of federally-funded family planning programs, showing their introduction by time period and by county of service provision (in most cases, county or local governments were not grantees; county is used here because family planning programs are matched to counties in the Vital Statistics Natality Files). From 1965 to 1973, family planning programs began in each of the lower 48 states. Figure II categorizes counties receiving family planning programs (also called "funded counties") during these years into three periods: the initial programs established between 1964 and 1967; the programs established between 1968 and 1969, during the expansion of family planning as a national emphasis program; and programs established from 1970 to 1973, with the initiation of Title X.

Immediately evident in figure II is that funded counties tended to encompass more urban areas, an impression confirmed by the 1960 census data presented in table I. Counties receiving family planning programs had much larger populations: roughly 60 percent of the U.S. population of women ages 15 to 44 lived in the 656 funded counties. Also consistent with family

[9] The main limitation of the data is that they provide little documentation explaining the purpose of grants. 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 allowed me to add the establishment of family planning programs in 278 communities. Only 23 of the remaining 378 dates 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. Under the assumption that these early, unobserved grants had similar effects to those 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, "Federal Outlays from All Sources" 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 that my method captures the vast majority of federal family planning awards.

planning programs being in more urban areas, funded counties had fewer elderly residents and had more of their population *above* the 1960 poverty line for a family of four ($3,000); they were also more educated and affluent. Importantly, counties with federal programs did not have higher fertility rates (as proxied by the proportion of the 1960s population under 5 years of age) or more nonwhite residents.[10] These observed differences and their suggestion of unobserved differences motivate the inclusion of county fixed effects in the analysis. With county fixed effects, cross-sectional differences in characteristics are not a threat to the internal validity of this study, but they are important to keep in mind when evaluating the study's external validity.

Figure II also illustrates significant within-state variation in the establishment of federally-funded family planning programs. For instance, the first U.S. community to receive a family planning program was Corpus Christi, Texas, in 1964, but Ector and Gonzales, Texas did not receive a program until 1973. In 43 of the lower 48 states, programs were established in at least two different years; counties in 41 states established family planning programs in at least four different years. In more than half of states, programs were established in at least five different years. This within-state variation is useful for this analysis as it allows the inclusion of state-by-year fixed effects in the analysis, which absorb time-varying changes in state policy like abortion legalization, Medicaid expansion, and changes in laws governing the age of access to contraception.

Rather than cross-sectional differences, the event-study analysis (described below) uses variation in *when* family planning programs were established to estimate their effects. If the establishment of family planning programs was as disorganized as the oral history indicates, then county characteristics should not predict this date. The unadjusted statistics in table I show that this appears to be the case with one exception: family planning programs were established earlier in areas with greater urban populations. (This is not surprising given that urban areas have more organizations that may apply for funding and more infrastructure for programs to build upon.) No evidence, however, indicates correlations between program establishment date and other 1960 population characteristics—correlations that may indicate a more systematic selection process than administrators claimed. After accounting for proportion of the population in urban areas in both weighted and unweighted regression specifications, none of the 1960 population characteristics except the proportion of residents above age 64 significantly predicts receiving a

[10] The latter is inconsistent with claims that federally-funded family planning programs were motivated by racism at the OEO.

family planning program sooner.[11] For programs initiated under the OEO, 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 (proportion of the population under 5 years of age) and in poverty rates have no predictive power in any specification,[12] despite the stated intent of the program to help reduce poverty.

Another possibility is that establishment may be correlated with more current fertility levels or recent fertility changes. If proposals were more likely to originate or be funded in locations with high fertility rates, then the event study may pick up reversion to the mean or the later end of the baby boom. Alternatively, OEO officials, eager to claim results for the program, may have used *past changes* in fertility to evaluate proposals, and given higher priority to funding for locations where fertility rates were already falling. If this were the case, then the program establishment date could be negatively correlated with fertility changes from 1960 to 1964. Figure III, however, provides no evidence of systematic application or selection on the basis of observable fertility patterns.

Yet another hypothesis is that applications for federal family planning dollars are much more correlated with local sexual behavior and attitudes about contraceptive and childbearing than 1960 Census characteristics and fertility outcomes. If, for instance, proposals originated sooner in locations with more permissive views about contraception and sex out-of-wedlock (even though they had similar childbearing outcomes) *and* these views predict future fertility declines, then the roll-out of family planning programs could be spuriously correlated with *future* changes in fertility. The 1965 National Fertility Study (NFS), designed to illuminate the determinants of childbearing among ever-married women, allows a direct test this hypothesis. The results presented in table II show no relationship between the date of federally-funded family planning program establishment and 16 measures of attitudes and outcomes related to contraception and childbearing.[13] These 16 measures include attitudes about the dangers of world population growth, abortion, and the ideal number of children, as well as coital frequency

[11] I regress the date federally-funded family planning programs were established on 1960 county characteristics in table I. These regressions include all the characteristics in table 1 except population size, because it is highly collinear with proportion of residents in urban areas. I also include state fixed effects and, therefore, omit regional dummies. Regression coefficients are available upon request but omitted from this draft for brevity.

[12] 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 many of these characteristics and the initiation of food stamps programs.

[13] I regress each characteristic on the date federally-funded family planning program was established the respondent's county of residence. To account for the stratified sample design of the 1965 NFS, I include dummy variables for the size of the sampling unit, decade of birth, and race.

in the last four weeks, the use of the birth control pill, and whether the respondent or her spouse had operations "making it impossible to have another child." The date of program establishment is also uncorrelated with women's education, Catholicism, age at first marriage and remarriage, age at first pregnancy, the total children born, and husband's income in 1965; it is uncorrelated to childhood characteristics such as whether the respondent lived with both parents at age 14 and the number of the respondent's mother's children.

In summary, this quantitative evidence is consistent with oral history accounts of a "wild sort of operation" of federal grant making. Neither 1960 Census characteristics, 1964 fertility levels, 1960 to 1964 fertility changes, nor a rich set of measures related to sexual behavior, birth control use, and childbearing predict when federally-funded family planning programs were established during the 1964 to 1973 period. As I will show, the date a family planning program began strongly predicts *when* county-level fertility rates began declining more rapidly than those in comparable communities without programs. The next section describes changes in fertility over the 1960s and 1970s, introduces the event-study regression framework, and tests key identifying assumptions.

IV. Empirical Strategy: Event-Study Analysis

Data on family planning programs are linked to birth records from Vital Statistics from 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 1,000 women of childbearing age (15 to 44 years). Although county-level birth rates by age group and parity are not available before 1968, I use county-of-residence-level aggregates from NHCS microdata for 1968 to 1988 to create age- and parity-specific birth rates. [14]

Panel A of figure IV shows changes in the GFR for the estimation sample for counties with family planning programs established in three periods, and for unfunded counties. For instance, counties in the "First funded 1965-67" group got programs between 1965 and 1967. The "Unfunded" group corresponds to unshaded counties in figure II and to column (2) of table I. For each of these groups, the GFR evolves nonlinearly over the 1959 to 1988 period, as the baby boom ends. Panel B plots the raw difference in the GFR between the funded counties and

[14] The numerator for the 1959 to 1968 GFR was hand-entered from published Natality Volumes; the numerator for the 1968 to 1988 GFR was aggregated from NCHS microdata to mother's county of residence. Denominators for all birth rates 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.

unfunded counties. From 1959 to 1965, the relatively flat lines for each of the series in panel B indicate that, before family planning programs began, the difference in the GFRs in funded and unfunded locations did not change – that is, fertility declined similarly in locations eventually getting *programs* and locations that did not. Simply subtracting the GFR in unfunded counties completely accounts 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. After 1966, however, the GFR in funded counties diverges systematically by the date that the family planning program began operating. After 1967, the GFR fell more rapidly in counties receiving family planning programs between 1965 and 1967 than in unfunded locations. Similarly, after 1969, the GFR fell more rapidly in counties receiving family planning programs between 1968 and 1969 than in unfunded locations. Finally, after 1973, the GFR fell more rapidly in counties receiving family planning programs between 1970 and 1973 than in unfunded locations. Also consistent with family planning having a treatment effect, the GFR in counties funded from 1965 to 1967 fell *before* the GFR in counties first receiving a program from 1968 to 1969. In short, both comparisons of funded to unfunded locations and comparisons by date of establishment among funded locations suggest that family planning programs reduced fertility.

A. Event-Study Regression Framework

An event-study specification (Jacobson, LaLonde and Sullivan 1993) formalizes this analysis by mapping the *exact year* of fertility declines to the *exact* fiscal year when family planning programs began; it also permits the inclusion of a rich set of fixed effects and time-varying covariates to adjust the raw differences shown in figure IV for potentially confounding factors. I estimate the following linear model,

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

where $Y_{j,t}$ is a fertility outcome in county *j* in year *t* = 1959,…,1988 (for age group and parity 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;[15] and $\boldsymbol{X}_{jt}$

[15] 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).

is a column vector including a constant and a set of control variables that exhaust the information available to OEO administrators. These control variables include 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) and one of the following: (1) 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 rural or a farm) with linear time trends or (2) county-specific linear trends. In addition, information on the number of abortion providers in each county accounts for *within-state changes in the availability of abortion* from 1970 to 1988 (zero before 1970).[16] Federal funds for family planning did not go to programs providing abortion, so the estimated effects of family planning should reflect sex education, counseling (and possible referral for abortion), and the provision of contraceptive supplies.

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, $1(\,)$, 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 a federally-funded family planning program was established in county j.[17] Because the indicator for $y = 0$ is omitted, the set of π describes the differential evolution of outcomes in *funded* counties before the family planning program began. The set of τ describes the divergence in outcomes y years *after* the family planning program began. 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.

One advantage of the non-parametric specification of τ relaxes the standard differences-in-differences (DiD) assumption that treatment with a family planning grant is associated with a one-time, level shift in outcomes. A family planning program cannot be set up instantaneously,

[16] 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.

[17] 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.

implying that the initial program effects may be smaller due to a smaller clientele. Moreover, if family planning programs allow women to delay childbearing by several years, then federal grants could initially depress birth rates but raise them later. The flexible specification in (1) allows the analysis to characterize changes in the program effect over 15 years and sheds light on how much of the effect found by Kearney and Levine reflects shorter-term adjustments in birth timing versus longer-term changes in completed fertility. Another advantage of this specification 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 $y<0$, plots of π show whether non-linear, preexisting trends confound estimates of τ. They also show whether the "effects" preceded the program even by a few years – an important falsification test.

After presenting the timing of changes in outcomes in the event-study framework, the main results are *summarized* in the following DiD specification, in which the individual indicators in equation 1 are replaced with dummies for five-year groups for the periods -6 and before, -5 to -1, 1 to 5, 6 to 10, 11 to 15, and 16 or more years after the establishment of a federal family planning program. Although all lead and lag dummies are included in all specifications, figures and tables only present estimates for years that include a *balanced* set of counties (years -6 to +15 for the event study/ -5 to +15 for the DiD).

B. Testing for Threats to Internal Validity

Potential threats to the internal validity of the analysis are shocks that are (1) *concentrated among the funded counties*, (2) *coincident to* or *occurring just after the family planning program began,* and (3) *correlated with fertility*. It seems very unlikely that "various and sundry" organizations in the 656 counties would begin unrelated initiatives to reduce fertility at the same time as the OEO independently decided to fund a family planning program. (If a complementary community family planning initiative begins *because* of the increase in federal family planning funds, the event-study would *appropriately* incorporate these indirect, "crowd-in" effects.) And, *even if* most applicants also began local programs after submitting a grant proposal, it is unlikely that these initiatives would correspond closely *in timing* to OEO/DHEW decisions to fund a local program (because of application rejections and administrative delays).

More plausible is the coincidence of other federal spending. Although the oral histories provide no indication that OEO administrators deliberately packaged OEO programs, it may have happened inadvertently or because certain communities were more effective at writing

proposals. This is an important concern, because other War on Poverty 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 or Maternal and Infant Health projects often provided services for family planning and aimed to improve infant and maternal health (DHEW 1974: 5, Dryfoos 1988). If the establishment of family planning programs co-occurred with the start-up of other federal programs, then estimates of τ may fail to isolate the effects of the family planning program.

Using newly compiled information on *all* grants for other OEO programs, I estimate equation 1 with a dependent variable grant receipt (=1) for each of eight OEO programs as the dependent variable.[18] Event-study estimates presented in figure V are relative to the year the county received its family planning program. Panel A of figure V is presented to fix ideas: counties receiving family planning programs got their first grant at time zero.[19] There is no pre-trend in family planning funds, and subsequent coverage rates indicate that federal refunding occurred in many but not all cases.[20] 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 legal services (panel E) show very small or no increases predating the establishment of family planning programs; funding did not increase over the years when family planning funding was expanding. The next two panels of figure V 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.

[18] 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).
[19] 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.
[20] This is not an indication that these programs were discontinued, because state and local funds for these programs were increasing over the same period (AGI 2000). Funds from nonprofit sources were also increasing.

In summary, much evidence supports the internal validity of this study's research strategy. Consistent with oral history accounts of federal grant making, section III (above) shows that neither 1960 Census characteristics, nor 1964 fertility levels, nor 1960 to 1964 fertility changes predict when federal family planning programs began. This section further narrows the scope for omitted variables bias by showing—using the same specification as used for the main results—that the initiation of other federal programs does not coincide with the roll-out of family planning. The next section uses this plausibly-exogenous shock to a county's family planning resources to examine the program's impact on childbearing.

V. Results: Did Family Planning Programs Reduce U.S. Fertility Rates?

Using equation 4, figure VI presents event-study estimates of the effects of the family planning programs on the GFR; table III summarizes the magnitudes of the estimates and their joint significance in a DiD specification. Panel A of table III weights the results 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 time-varying county-level covariates to model 2. Table III also shows that the addition of linear, county trends (column 4) yields larger, though statistically indistinguishable, estimates (compare to model 3, column 3).

Across models, the estimates show that family planning programs reduced childbearing. Consistent with figure IV, the GFR evolved similarly in funded and unfunded counties before family planning programs began. In each case, the pre-treatment differences are close to zero and individually, statistically insignificant. Table III also shows that the average leads for years –5 to –1 are also indistinguishable from zero in each model. At the time family planning programs began operating, a sharp trend break is visually evident and statistically significant. In the first five years of program operation, the GFR fell more quickly in funded than in unfunded communities (plots of the estimates and confidence intervals decrease and fall below zero). Within three years of the program establishment, the GFR had fallen by roughly 1 birth per 1,000 women of childbearing age (model 3, panel A) and 2.5 births per 1,000 women in the average county (model 3, panel B). By years 6 to 10, the GFR had fallen by an average of 1.5 (weighted; model 3, panel A) and to 2.1 (unweighted; model 3, panel B) births per 1,000 women of childbearing age. The growth in the effects is consistent with the earliest federal grants

establishing or significantly expanding family planning programs and these programs reaching their full capacity in around 4 to 5 years. After the initial 5-year period, these effects hardly changed for the next decade. 15 years after family planning programs began, the GFR remained 1.4 to 2 percent lower than in the year the program started, net of declines in fertility in other counties in the same state and after adjusting for observable county-level covariates.

Two 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 figure IV), one might opt to include only funded counties in the regressions. Using funded counties only, column (5) of table III shows (1) no evidence of a pre-trend and (2) a sharp trend break in the year after the grant. Omitting the unfunded counties results in less precise estimates. For neither the weighted nor the unweighted specifications are these estimates statistically distinguishable from those in column 2. The exclusion of unfunded counties, therefore, does not change the patterns of results but does affect precision. A second check considers the difference in the effects for pre-Title X and post-Title X family planning programs. 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 IIIA). The event studies (omitted for brevity) also show that for grants in both periods the estimates exhibit no pre-trend and a sharp trend break in the year after the family planning program began.

Taken together, these results provide robust evidence that family planning programs reduced childbearing. Given the extensive controls included in the analysis, it is remarkable that the pattern of estimates varies so little across specifications. Interestingly, the trend break and the absolute magnitude of the effects tend to be larger in unweighted specifications, which implies that the effects were stronger in *less populous* counties. This may be due to measurement error (treated women may have been a larger proportion of the population in smaller counties) or heterogeneity in the effect across locations (larger cities may have had more alternatives to federally-funded family planning providers).

The longer-term reduction in the GFR may reflect changes in the timing of childbirth as well as reductions in completed fertility. I examine both channels by considering age-specific birth rates as well as birth rates by parity, which I construct using the natality microdata from 1968 to 1988 (these regressions omit counties funded before 1968 from the analysis to ensures a

balanced panel of treated counties in the post-period).[21] I cannot observe fertility delay directly, but delay should result in reductions in childbearing among younger women. Permanent reductions in childbearing manifest as decreases in childbearing at older ages (earlier stopping) and as reductions in higher parity births. Figure VII presents event-study estimates consistent with both channels; table IV presents DiD summary estimates. Panel A of figure VII and table IV show that, following the establishment of a federally-funded family planning program, birth rates for all age groups tended to decrease. Six to ten years after the program began, the largest absolute drop in levels was approximately 4 births per 1,000 women, or 2.7 percent, among 25 to 29 year olds (table IV, column 3). These reductions among teens reached approximately 2 percent (column 1), while the effects for women in their early twenties and early thirties reached 1.4 percent (column 2) and 1.5 percent (column 4), respectively. Changes in birth rates among women in their late 30s and 40s are negative, but they are statistically insignificant. Consistent with larger effects among younger women, panel B of figure VII and table IV show that the introduction of a family planning program also reduced the number of first, second, and third births by 2.0, 2.8 and 2.1 percent, respectively, 6 to 10 years after the program started. Although family planning programs did not affect first birth rates in the longer term (years 11 to 15), they reduced 2nd and 3rd parity births for up to 15 years after they were established—a result consistent with family planning programs reducing completed childbearing. The roughly 35 percent reduction of the magnitude of these effects in the last 5-year period, however, is consistent with family planning programs causing women to delay 2nd and 3rd births. Estimates for 4th and higher order births are negative, but statistically insignificant.

Together, these results imply that family planning programs achieved a significant reduction in completed fertility and also induced delays in childbearing for 15 to 34 year olds. In fact, the absence of effects among women 35 and older may reflect the fact that fertility delays offset reductions in childbearing for this group.

VI. How Large Are the Effects of Federally-Funded Family Planning Programs?

Thus far, the paper has examined changes in fertility rates for the average woman in counties receiving federally-funded family planning programs (or the average treated county in the unweighted specifications). A number of factors build a case for a causal interpretation of

[21] Parity measures (panel B) are the number of births of the indicated parity divided by the population of women ages 15 to 44. Panels A of figure VI and column (8) of table III show that the aggregate estimates for this shorter panel are similar in magnitude to the estimates for the longer panel.

the estimates: the (1) absence of a pre-trend in funded counties, (2) the sharp trend-break in fertility following the establishment of the family planning program, (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), and (4) the robustness of these estimates across a variety of demanding specifications (figure IV, table II). Given this robustness, a natural question is the plausibility of the estimated magnitudes.

The lack of data on childbearing, family planning, and birth control use by county and year prevents a direct examination of this question. As an alternative, I use a combination of independent data sources to explore the magnitudes of the estimates from two perspectives. First, I use a back-of-the-envelope calculation to examine whether the estimated effects of family planning programs make sense given a plausible reduction in contraceptive failure rates. Second, I approximate the treatment effect of family planning programs on users and compare this effect to fertility differences by income in 1965. The idea of this second exercise is to evaluate whether the implied effects of family planning programs on the treated are reasonable given differences in birth rates by income before federally-funded family planning programs began.

A. A Back-of-the-Envelope Calculation of the Expected Effects of Family Planning on Fertility

Family planning programs may both affect the share of women choosing to prevent pregnancies (by reducing the marginal cost of doing so) and reduce the contraceptive failures among those who do (by subsidizing more reliable methods like the Pill). The rate of contraceptive failure is difficult to estimate directly, because failures reflect both method failures and selection. To circumvent this problem, I begin by backing out an estimate of the average failure rate in the population using information on the share of women who are fecund and trying to get pregnant in the 1965 *National Fertility Study* (*NFS*).

This estimate of the failure rate relies upon the following tautology. The GFR in year t can be written as the sum of the planned birth rate, P_t, and the unplanned birth rate due to contraceptive failures, F_t, or $GFR_t = F_t + P_t$. Planned births and failures reflect decisions in the previous period about whether or not to get pregnant as well as the respective success and failure rates. If Q denotes the share of women who are fecund and trying to get pregnant, S the share of women who are not fecund, s the time-invariant average rate of success among women trying to get pregnant, and f the average failure rate among fecund women, then the GFR can be written as

$$GFR_t = F_t + P_t = Q_{t-1}\, s + (1-Q_{t-1}-S_{t-1})\, f. \tag{2}$$

In 1966, the U.S. marital GFR was 123.6 per 1,000 married women ages 15 to 44. (I compute the marital GFR, because the 1965 *NFS* contains information only on ever-married women.) The 1965 *NFS* indicates that Q and S are 0.023 and 0.26 respectively. Trussell (2004) places the likelihood of pregnancy occurring within a year among women who are trying at 0.85. For the U.S. marital fertility rate to have reached 123.6 per 1,000 married women in 1966, these numbers imply that, before the U.S. family planning program began, roughly 11.7 percent (f = (0.1236 – 0.023 ×0.85) / (1 - 0.023- 0.26) = 0.117) of married U.S. women who were *not* trying to get pregnant in 1965, conceived children. (This is much lower than the rate obtained using estimates of method failures.)

This inferred failure rate then allows the computation of a differences-in-differences (DiD) estimate of how family planning affected the GFR by rewriting equation (2) as $GFR_t = Q_{t-1}\, s + (1-Q_{t-1} -S_{t-1}) [q_{t-1} f^F + (1- q_{t-1}) f]$, where q_{t-1} denotes the share of the fecund women using family planning services, and f^F the average failure rate for family planning program users. The change in the difference in GFR between funded, B_t^F, and unfunded communities, B_t^0, between period T and the date of the family planning program's initiation, 0, can be written, $\Delta D \equiv (GFR_T^F - GFR_T^0) - (GFR_0^F - GFR_0^0) = [(Q_T^F - Q_T^0) - (Q_0^F - Q_0^0)]\, s + (1-Q_T-S_T)^F [q_T^F f^F + (1-q_T^F) f] - (1-Q_T-S_T)^0 [q_T^0 f^F + (1- q_T^0) f] - [(1-Q_0-S_0)^F - (1-Q_0-S_0)^0] f$. Assuming that Q and S did not change from the date the program began to T, and that there were no behavioral differences in locations with family planning programs between when programs began and at time T (this assumes that family planning does not affect choice to try to get pregnant or sterilization), or $(1-Q_0-S_0)^F = (1-Q_0-S_0)^0 = (1-Q_T-S_T)^F = (1-Q_T-S_T)^0$), the DiD estimate of the effect of family planning depends upon only three quantities: the fraction of women who are fecund and not trying to get pregnant, the difference in the fraction of these women using family planning services between funded and unfunded locations, and the difference in failure rates conferred by family planning services, or

$$\Delta D = (1-Q_T-S_T)(q_T^F - q_T^0)(f^F - f). \quad (3)$$

The 1965 NFS implies that $(1-Q_T-S_T) = 0.717$. Assuming that the failure rates of services supplied by family planning programs (i.e. the Pill) are around those implied by "typical use" (2 to 8 percent), $(f^F - f)$ ranges from -0.037 to -0.097.

I estimate the final unknown, $(q_T^F - q_T^0)$, using the 1970 NFS. The 1970 NFS asked ever-married women between the ages of 18 and 44 whether they had ever used the Pill—the most

prescribed method by family planning clinics. Using a linear probability model, I find that federally-funded family planning programs raised cumulative use of the Pill by 5.2 percentage points.[22] Consistent with these programs having their largest effect on contraception use among women below the poverty line, the increase in Pill use was only 4 percentage points (or 6.5 percent) among women above the poverty line and 23 percentage points (or 38 percent) higher among women below the poverty line.

Altogether, the back-of-the-envelope calculation implies a DiD estimate ($\Delta D = (1-Q_T - S_T)(q_T^F - q_T^0)(f^F - f)$) ranging from -1.3 (0.717*0.05*-0.037) to -3.5 (0.717*0.05*-0.097) births per 1,000 married women. Given that this estimate should be larger if the share of women trying to avoid pregnancy increases with family planning programs ($1-Q_T-S_T$ increases), my estimates of -1.4 to -2 births per 1,000 women presented in section V are quite reasonable.[23]

B. Treatment Effects on the Treated: Family Planning's Effects on New Users

Because not all residents of a county would have benefited from the new family planning program (many women would have obtained services anyway or from other providers), the intention-to-treat (ITT) effects in section V understate the effects of family planning on the women they served ("treated women"). To approximate the effects of federally-funded family planning on the treated (TOT), I rescale these ITT effects by the induced changes in users of the Pill. I use changes in use of the Pill (rather than changes in use of family planning services), because it has the advantage of capturing substitution toward more reliable methods, which should drive fertility rates down by reducing contraception failures. Using the estimate derived from the 1970 NFS that shows federally-funded family planning programs increased use of the Pill by 5.2 percentage points,[24] a reduction in the period birth rate between 1.4 and 1.9 implies a TOT of 27 (1.4/0.052) to 37 (1.9/0.052) births per 1,000 women.

Is such a reduction among treated women reasonable? Because women below 150 percent of the poverty line were the vast majority of program users (and Vital Statistics Natality

[22] Specifically, I regress a binary variable for Pill use on a binary variable equal to one if the PSU of residence had a federal family planning program before 1970. I limit my sample to PSUs that received federally-funded family planning programs between 1964 and 1973. The regression coefficient on family planning before 1970 is 0.052 (s.e. 0.0256, p-value=0.044). Interacting the binary variable for family planning program establishment before 1970 with Poverty status yields coefficients of 0.04 (s.e. 0.0257, p-value 0.116) on the direct effect of family planning before 1970 and a coefficient of 0.188 (s.e. 0.093, p-value 0.043) on the interaction of family planning before 1970 and Poverty.

[23] Although I have no way to evaluate this directly (the NFS did not sample never-married wom1en), the effect may be larger or smaller depending upon how family planning affected unmarried women.

[24] This calculation uses the specification described in footnote 22 and assumes that roughly 20 percent of women fall below the poverty line over the period in question and that the estimated changes in use apply to never-married women as well (the 1970 *NFS* does not contain information on never married women).

data do not contain information on income), I use the 1964 to 1966 *Natality Followback Survey* (US DHHS and ICPSR 2008) to generate a pre-program estimate of differences in childbearing by income. This survey shows that the 1965 GFR among women below 150 percent of the poverty line was 128, but the GFR among women above that threshold was 77. The implied TOTs, therefore, indicate a 21 to 30 percent reduction in childbearing for women below 150 percent of the poverty line and can account for 53 (27/51) to 73 (37/51) percent of the fertility disparities between poor and non-poor women before the program began. In short, the effects are a reasonable magnitude and consistent with federally-funded family planning programs diminishing the income-based differences in childbearing that initially motivated the program.

VII. REEVALUATING THE EFFECTS OF FAMILY PLANNING

For almost 50 years the federal government has invested in domestic family planning programs with mixed evidence of their short-run effectiveness and no credible evidence that these programs reduced U.S. childbearing in the longer-term. The impact of these programs on childbearing in the U.S. was contentious in the 1960s and it remains so today.[25] Although recent research has argued that the birth control pill played an important role in reducing U.S. fertility rates (Guldi 2008, Bailey 2010), these studies' reliance on legal variation only provides evidence on the importance of modern contraception for women who could afford it.

This paper adds to the literature by considering how family planning programs' subsidization of contraception affected childbearing in both the shorter and longer run. Using county-level variation in when family planning programs were established between 1964 and 1973, I find that domestic family planning programs reduce shorter- and longer-term fertility rates by ameliorating income-based differences in access to more reliable contraceptives. My estimates imply that U.S. family planning programs prevented roughly 1.8 million births in the first ten years they existed at a cost of roughly 2,700 federal dollars per birth averted.[26]

Because federally-funded family planning programs served mostly lower-income women and operated in only one fifth of all U.S. counties in 1974, the program accounts for only a

[25] See, for example, the heated exchange in *Science* featuring the provocative articles, "Population Policy for Americans: Is the Government Being Misled?" (Blake 1969) and "Family Planning and Public Policy: Who is Misleading Whom?" (Harkavy, Jaffe and Wishik 1969).

[26] 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 are estimated at $5,067 million (2010 dollars). Thus, the reduction in births cost roughly 2,734 in federal dollars per birth averted in the program's first 10 years. Due to the large (unknown) contributions from state and local governments and non-profits, it is unclear how much each birth averted cost overall.

modest portion of the large decline in the general fertility rate from 1959 to 1974. Nevertheless, these programs had a substantial effect on the women they served. This paper's estimates imply that federal family planning dollars reduced childbearing among poor women by 21 to 29 percent within ten years—magnitudes large enough to account for half to three-quarters of the 1965 gap in childbearing between poor and non-poor women. Future work should consider how family planning programs affected a host of longer-term outcomes including the age-structure of poverty, children's resources and life chances, and economic growth.

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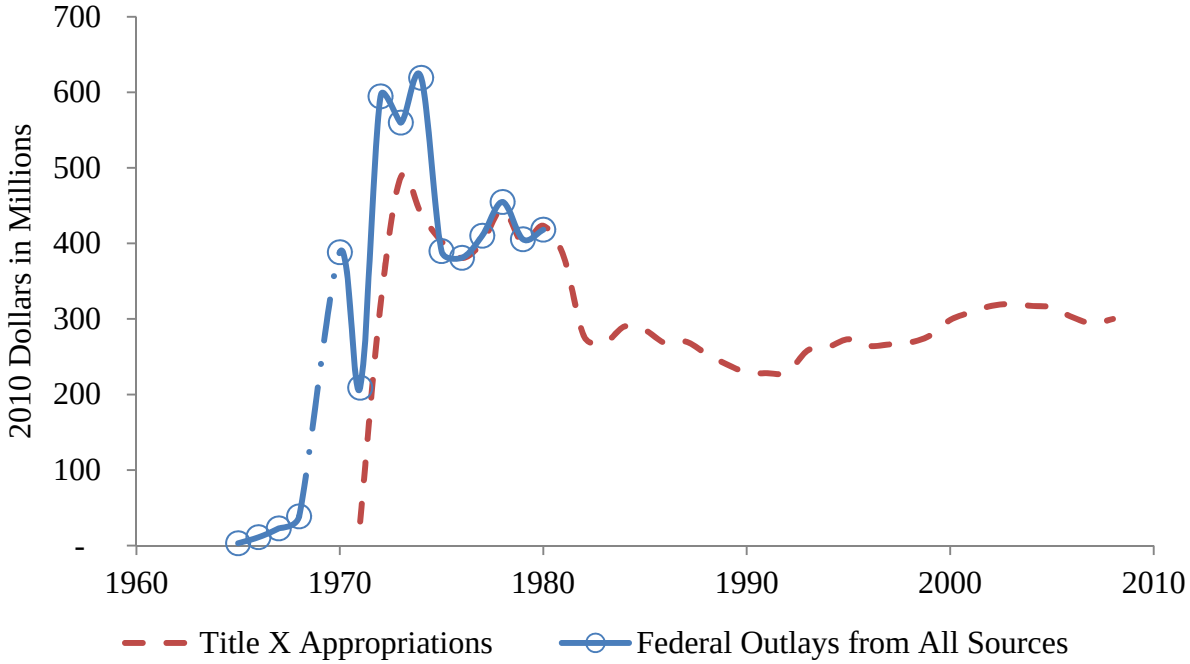
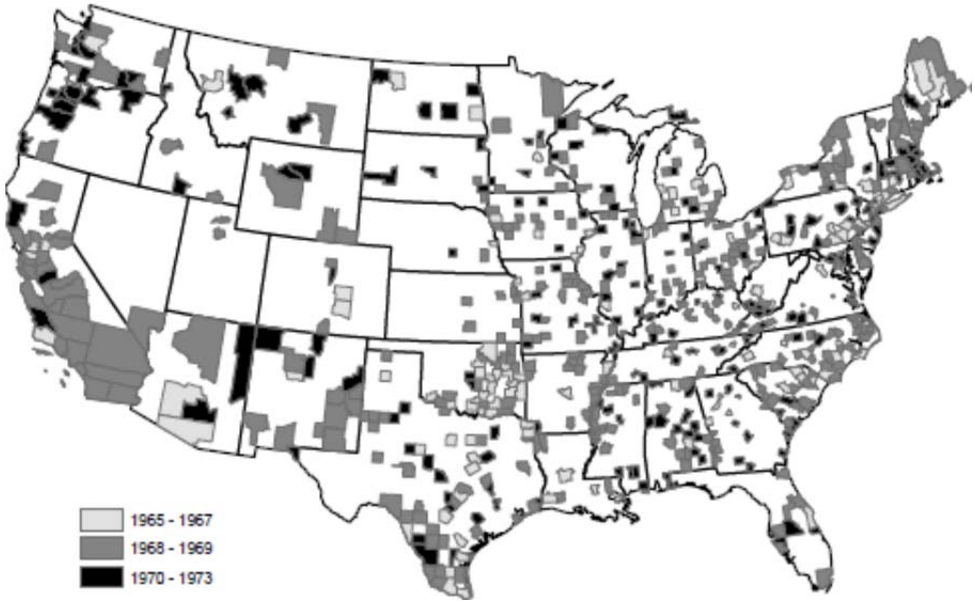
Figure I. Federal Spending on Family Planning, 1965-2008



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" 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 Establishment of Federal Family Planning Programs by Period, 1965-1973

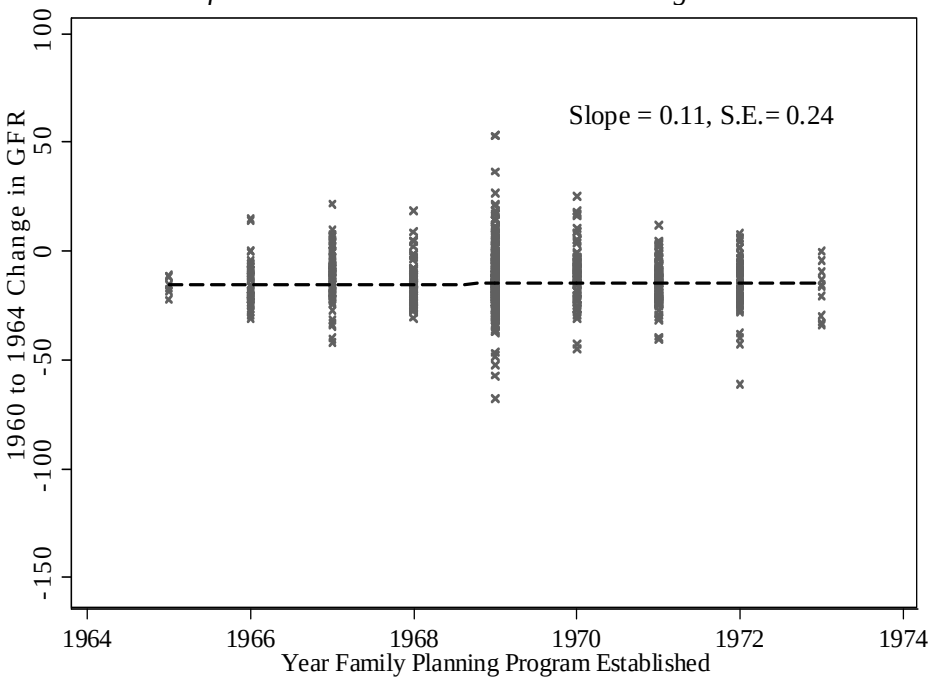


Notes: Dates are when the first federal family planning program was established in the county. Counties not receiving family planning programs grant between 1965 and 1973, including a handful of communities that received funding but with an unknown starting date, are not shaded. Source: NACAP, NAFO and OEO (1969, 1971 and 1974).

Figure III. Fertility Rates and the Roll-Out of Federal Family Planning Programs

A. Year of Establishment and 1964 General Fertility Rate (GFR)

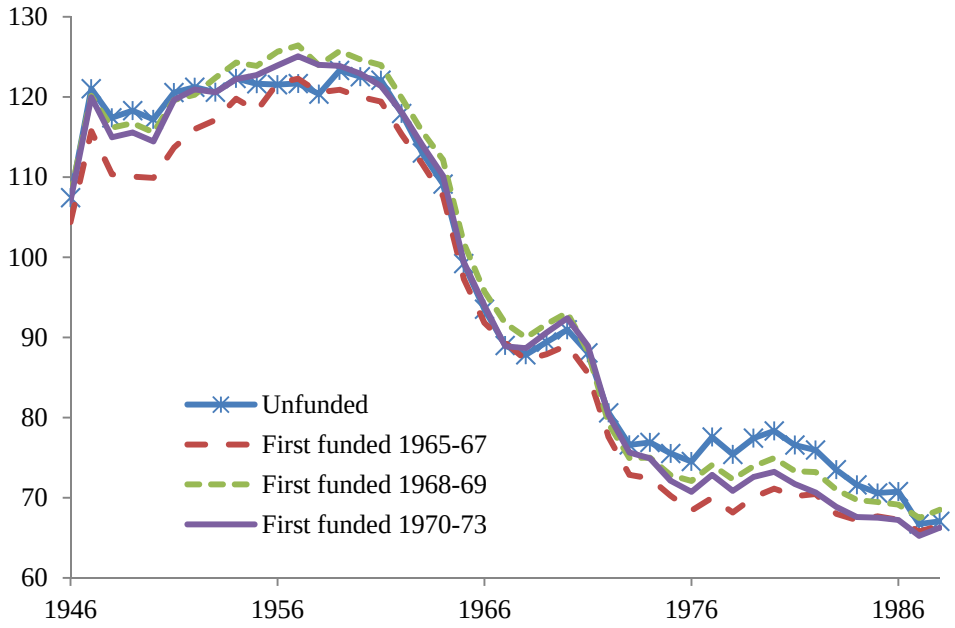
B. Year of Establishment and 1960 to 1964 Change in the GFR



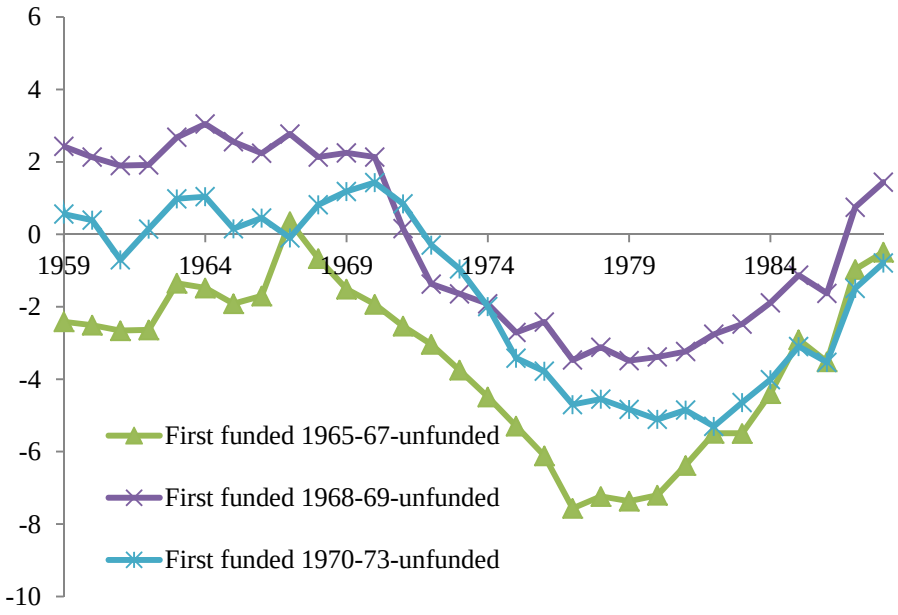
Notes: The x-axis plots when the first federal family planning program was established in the county. The y-axis in panel A plots the 1964 GFR, the y-axis in panel B plots the change in the GFR from 1960 to 1964. The dashed lines indicate the estimated relationship between the x and y variables using linear regression; text indicates the estimated slope and standard error for each panel. Sources: Family planning data: 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.

Figure IV. Descriptive Evidence of the Effects of Family Planning on Fertility Rates

A. Changes in the General Fertility Rate (GFR), 1946 to 1988



B. GFR in Funded Counties (By Date of Establishment) – GFR in Unfunded Counties

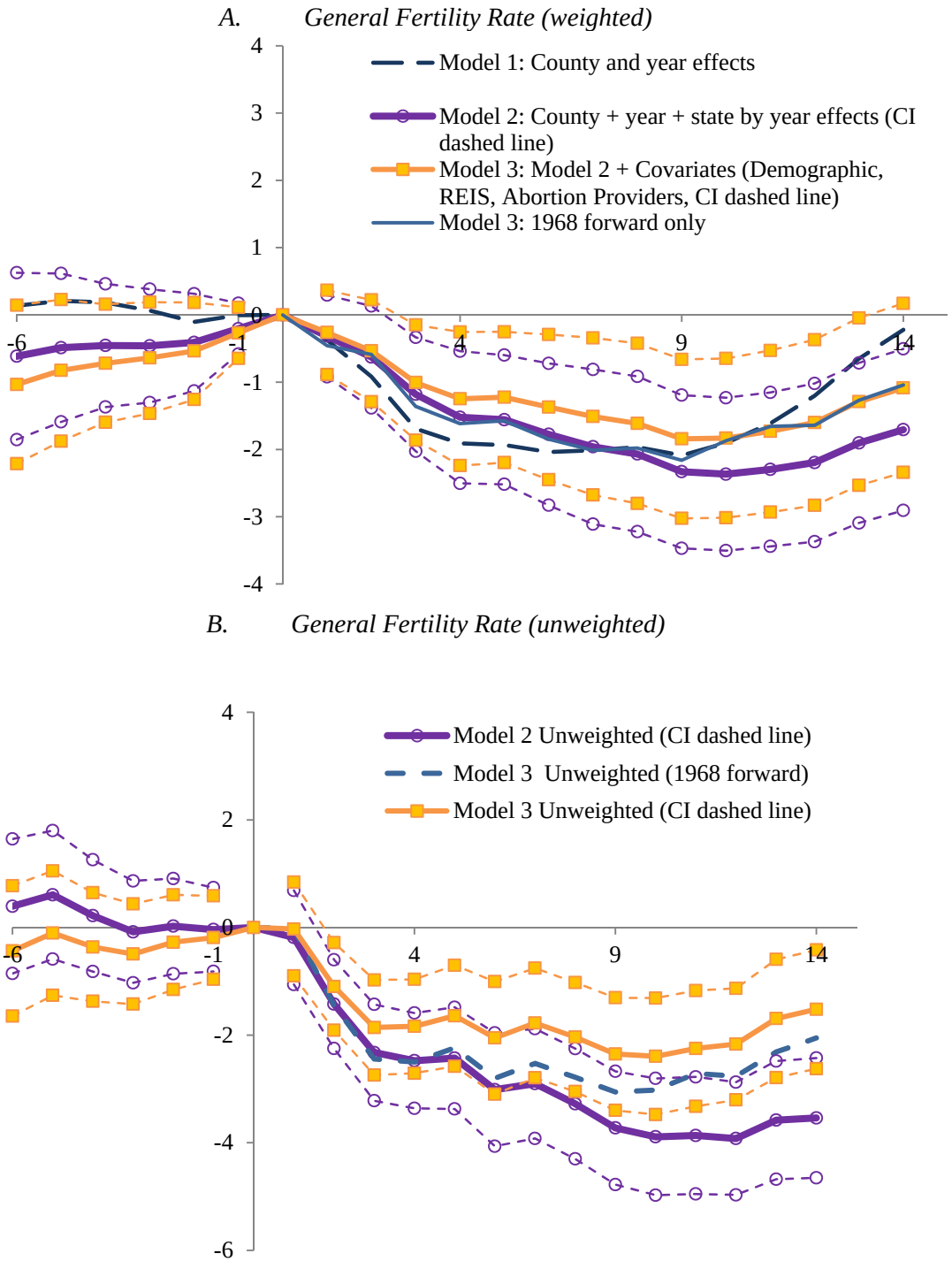


Notes: Panel A plots the evolution of the general fertility. Panel B plots the annual difference in the GFR between funded and unfunded counties for counties funded in the three indicated periods. All estimates are unweighted.
Sources: See figure III.

Figure V. The Relationship of the Roll-Out of Family Planning Programs with 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. Sources: NACAP and NAFO.

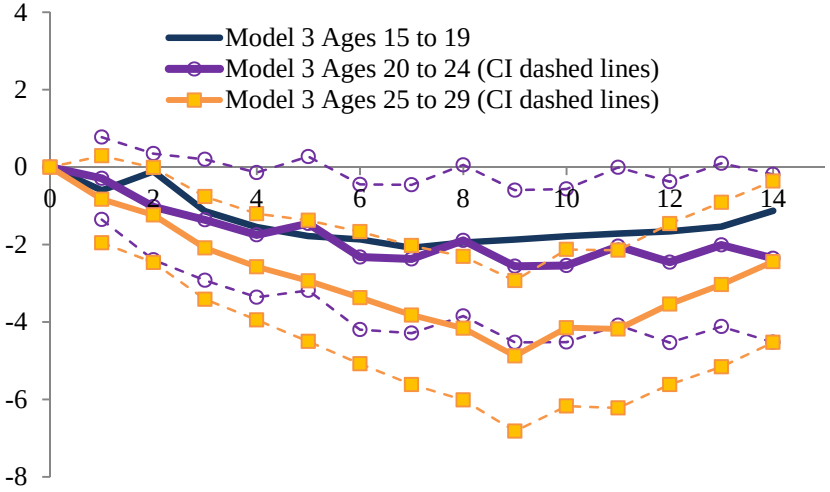
Figure VI. The Effects of Federal Family Planning Programs on Fertility Rates

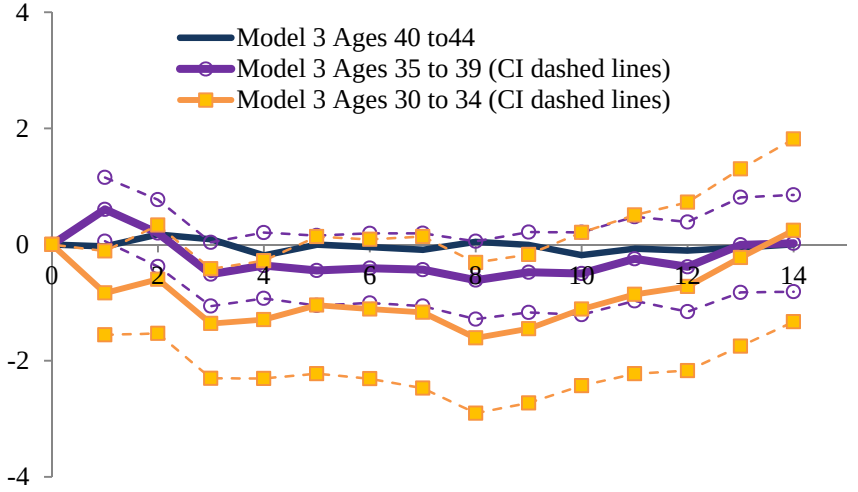


Notes: Panels plot either weighted or unweighted estimates of π and τ from equation 1. Weights are the county population of women ages 15 to 44 in 1970. Heteroskedasticity-robust standard errors clustered by county construct 95-percent, point-wise confidence intervals (dashed lines). Sources: See figure III.

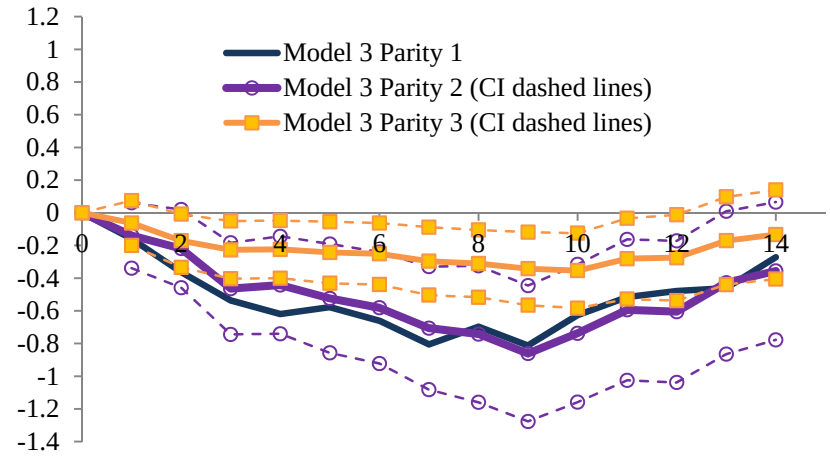
Figure VII. The Effects of Federal Family Planning Programs on Fertility Rates, by Age and Parity

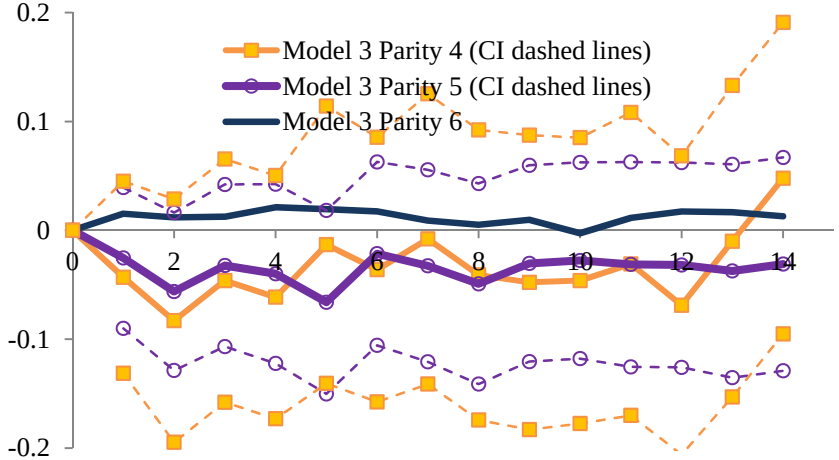
A. Birth Rate by Age Group, 1968-1988





B. Birth Rate by Parity, 1968-1988





Notes: Panels present weighted, least-squares estimates of τ from equation 1 for the indicated group for the 1968 to 1988 data. Sources: Outcome data taken from 1968 to 1988 Natality 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.. Information on family planning programs drawn from NACAP, NAFO and OEO (1969, 1971 and 1974) as described in text.

Table I. Characteristics of Counties Receiving Federal Family Planning Programs from 1965 to 1973

	(1) All Counties	(2) Not Funded	(3) First Funded in years 1965-7	1968-9	1970-3	(4) All Funded	(5) Col 4- Col 2 (std. error)
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	16.6	31.8	16.9	38.9	26.0	9.4
							(4.96)
in Midwest	24.0	38.6	20.9	27.4	23.1	24.5	-14.1
							(6.23)
in South	26.9	37.3	29.0	24.4	28.4	26.6	-10.7
							(4.85)
in West	22.8	7.4	18.4	31.2	9.6	22.8	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	11.4	11.3	11.4	11.6	11.5	11.5	0.227
							(0.12)
65 or older	9.2	9.8	8.9	8.6	9.1	8.8	-1.00
							(0.20)
Nonwhite	10.9	11.3	12.8	10.4	7.8	10.6	-0.694
							(1.05)
with 12 years of education	42.7	41.0	43.2	45.0	42.9	44.0	3.06
							(3.88)
with fewer than 4 years of education	8.4	9.6	7.9	7.1	7.8	7.5	-2.14
							(0.40)
of households with income under $3,000	21.9	27.1	17.4	17.9	18.9	18.0	-9.13
							(1.45)
of households with income over $10,000	14.8	11.7	17.4	17.7	15.4	17.1	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 family planning programs drawn from NACAP, NAFO and OEO (1969, 1971 and 1974) as described in text.

Table II. The Relationship of the Roll-Out of Federal Family Planning Programs and 1965 Determinants of Childbearing

	(1) Population Growth a Problem	(2) Ideal Number of Children	(3) Approve of Abortion	(4) Coital Frequency	(5) Ever Used the Pill	(6) When 1st Used Pill | Ever Used	(7) Surgically Sterilized	(8) Children Ever Born to Mother
Mean Dependent Variable	*0.804*	*3.290*	*0.389*	*6.036*	*0.221*	*772.0*	*0.198*	*5.062*
Year Family Planning	-0.00495	0.00989	-0.000901	0.0363	-0.00444	0.198	-0.00436	-0.0543
Program Established	[0.00711]	[0.0221]	[0.00537]	[0.0708]	[0.00971]	[0.384]	[0.00747]	[0.0656]
Observations	3,106	3,069	3,106	2,967	3,106	742	3,106	3,101
R-squared	0.022	0.038	0.023	0.136	0.154	0.022	0.095	0.075

	(9) Married Once	(10) Age at 1st Marriage	(11) Age at 1st Pregnancy	(12) Children Ever Born	(13) Husband's Income	(14) Catholic	(15) Highest Grade	(16) 2 Parents at 14
Mean Dependent Variable	*0.868*	*20.83*	*22.35*	*2.651*	*7620*	*0.289*	*11.33*	*0.782*
Year Family Planning	0.00624	0.0541	0.0631	0.0171	50.61	0.0234	0.0359	0.00411
Program Established	[0.00492]	[0.0593]	[0.0663]	[0.0312]	[157.7]	[0.0157]	[0.104]	[0.00600]
Observations	3,106	3,103	2,815	3,106	3,006	3,106	3,105	3,106
R-squared	0.040	0.111	0.160	0.141	0.170	0.061	0.092	0.015

Notes: Dependent variables are coded as follows by column: (1) Do you consider the growth of world population a serious problem? Yes=1, (2) What is the ideal number of children for average American family? (3) Index from three questions about whether the respondent approves of abortion if a woman is not married, for health concerns, or in the case of financial hardship. 1=approve in all three cases; (4) Coital frequency in the last four weeks? (5) Have you ever used the Pill? Yes=1, (6) When did you first use the Pill? (month and year), (7) Have you or your husband had an operation making it impossible to have (another) child? 1=Yes; (8) How many children did your mother have? (9) Is this your first marriage? 1=Yes; (10-11) Age in months constructed from month and year of birth and month and year of first marriage and month and year of first pregnancy end date; (12) How many live births have you had? (13) Husband's income in nominal dollars. (14) Respondent identifies as "Roman Catholic." (15) Highest grade attained by the respondent. (16) Did you live with both parents at age 14? 1=Yes. Estimates are obtained from weighted regressions of the indicated dependent variable on the year the family planning program was established. To account for sampling design, the regressions control for size of sampled PSU, decade of respondent's birth, and race (1=Nonwhite). Source: *1965 National Fertility Study*.

Table III. The Effects of Federal Family Planning Programs on Fertility Rates

	(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 2 +R,A, 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 DiD estimates as described in text 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, R, and A to model 2. Column 5 estimates model 2 for funded counties only. Column 6 omits counties receiving first grants after 1970. Column 7 omits counties receiving first grants before 1971. 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 VI notes.

Table IV. The Effects of Federal Family Planning Programs on Fertility Rates, by Age Group and Parity

	(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

Notes: The table presents weighted least-squares estimates obtained from estimating the DiD specification separately for indicated groups as described in text. All specifications are model 3. Heteroskedasticity-robust standard errors clustered by county are beneath each estimate in brackets. Source: See figure VII.

DATA APPENDIX

A. Description of County-Level Natality Data

1. Natality Data for 1959 to 1967

The data for 1959 to 1967 were double-entered from published volumes of the Vital Statistics Division of the National Center for Health Statistics (NCHS, formerly the National Office of Vital Statistics). These records were compiled by Vital Statistics using microfilm copies of transcripts or state data files of all original birth certificates sent by states to the NCHS. All births are classified according to the place of residence of the mother. Data for the years 1959-1966 are based on a 50 percent sample consisting of only the even-numbered live birth records. The data for 1967 are based on a 20 percent to 50 percent sample (depending upon the state). The NCHS generated the published counts by multiplying the samples by two in the case of a 50 percent sample and five in the case of a 20 percent sample.

2. Completeness of Birth Registration (1959 to 1967)

The data do provide a representative sample of all registered births. Although there were state laws requiring that all births and deaths be registered, the degree of compliance and enforcement varied by state and over the course of the sample.

The NCHS conducted two tests of birth registration completeness. The first test was conducted based on records for infants born between December 1, 1939, and March 31, 1940, that were alive on April 1, 1940. The second test was conducted based on infants born between January 1, 1950, and March 31, 1950, and is fully described in chapter 6 of the 1950 Vital Statistics Volume. Both of these tests were implemented in a similar manner. During both testing periods Census enumerators filled out infant cards for children born during the months of the test. State Vital Statistics offices then provided NCHS with copies of all infant birth and death records from the test period. These two sets of records were matched by NCHS. A follow-up mail survey was sent to the institutions where any unmatched births occurred as well as to the parents' places of residence. More records were matched after additional information was obtained from the follow-up mail survey. Any remaining records were checked against other sources of information from the states.[27] The completeness of birth registration in each area was then determined by comparing the number of matched records to the total number of infant cards and death records for the area.

The percent completeness was higher in 1950 than in 1940. In 1950, the NCHS estimated that 98.1 percent of all births in the nation were registered: 98.8 percent for whites and 94.1 percent for the nonwhite (1950, p. XXI). In both tests of birth registration completeness, births that occurred in hospitals had higher completion rates than births occurring outside hospitals. By 1960, NCHS estimated that birth registration was 98.9 percent complete (1965, pp. 4-9). The counts of birth-related variables given by county were not adjusted for the completeness of registration.

3. Data Limitations

For all years before 1964, birth certificates for many intrastate nonresidents of Massachusetts were received from the place of occurrence of the birth as well as from the place of the mother's residence. This produced some duplication and the Technical Appendix from 1964 gives an estimate of a four percent overstatement of the number of births (1964, pp. 4-10).

[27] An example would be the use of adoption records to match birth records.

Also in 1964, 1,800 birth records for Massachusetts were not received by the NCHS and so live birth figures are understated (1946, pp. 4-10). Therefore, the sample of consistently identified counties for 1946 to 1988 excludes all counties in Massachusetts.

4. Natality Data for 1968 to 1988

The data for 1968 to 1988 were taken from NCHS Natality Detail Files (ICPSR Study Numbers 3241, 3242, 3244, 3243, 3245, 3246, 3247, 3248, 3249, 3250, 3251, 3304, 3305, 3307, 3327, 3308, 3309, 3310, 3311, 3312, and 6651). The files contain information entered from individual birth certificates during each calendar year. Data for the years 1968 to 1971 are based on a 50 percent sample. All births are classified according to the place of residence of the mother or according to the place of occurrence of the birth. Births to Americans outside of the United States are not recorded in these data, although births to foreigners in the United States are recorded and assigned to the county of occurrence. Race information was aggregated into county of residence categories to match the pre-1968 data.

5. Natality Variable Definitions

Most of the variables have been calculated based on the usual place of residence of the mother as reported on the birth certificate. Until 1964, births to nonresident aliens were classified with the place of residence being the exact place of occurrence of the birth within the county. From 1964 until the end of the sample, these births were allocated to the "Balance of County" of the county of occurrence rather than to the exact place of birth within the county (1964, pp. 4-5).

B. Description of County-Level Population Data, 1959 to 1988

County-level population data for the number of women per county between the ages of 15 and 44 is taken from the Census and from the Surveillance Epidemiology and End Result (SEER) published by the National Cancer Institute. County-level information from the 1940, 1950, and 1960 censuses were compiled by Michael Haines for the Inter-university Consortium for Political and Social Research (study 2896) and linear interpolation was used to generate population denominators between 1959 and 1969. The Surveillance Epidemiology and End Result's data cover the period from 1969 to 1988 in single years, and dictionary files were adapted from those written by Jean Roth of the National Bureau of Economic Research (NBER, Retrieved on Sept 1, 2008 from http://www.nber.org/data/vital-statistics-mortality-data-mulitiple-cause-of-death.html). Several counties are missing population data in at least one year. Counties missing this data are listed below:

Arizona:	Yuma
Colorado:	Adams, Boulder, Jefferson, Weld
Florida:	Dade
New Mexico:	Los Alamos, Valencia
New York:	New York
South Dakota:	Washabaugh
Virginia:	Alleghany, Arlington, Henry, James City, Rockingham, Spotsylvania, Stafford, York, Alexandria City, Clifton Forge City, Covington City, Falls Church City, Fredericksburg City, Galax City, Harrisonburg City, Martinsville City, Norton City, Williamsburg City

C. Anomalies in the Data

1. Changes in County Boundaries

Several counties combined or split during the sample period. I account for those boundary changes by aggregating the affected counties for the entire sample period. Only boundary changes that resulted in the creation or elimination of counties are reflected. I account for boundary revisions only if they resulted in a name or county federal information processing standard code change. These changes were verified by comparison with listed county changes from the Census or against table footnotes from the Vital Statistics Volumes. The boundary changes are as follows:

South Boston City, VA was completely absorbed into Halifax County, VA in 1960.
Fairfax City, VA became independent of Fairfax County, VA in 1961.
Franklin City, VA became independent of Southampton, VA in 1961.
Menominee County, WI was formed from the Menominee Indian Reservation, Oconto and Shawano counties, WI in 1961.
Norfolk County, VA and South Norfolk City, VA combined to form Chesapeake City in 1963.
Princess Anne County, VA was completely absorbed into Virginia Beach City, VA in 1963.
Lexington City, VA became independent of Rockbridge County, VA in 1966.
Emporia City, VA became independent of Greenville County, VA in 1967.
Bedford City, VA became independent of Bedford County, VA in 1968.
Roanoke City, VA and Salem City, VA became independent of Roanoke County, VA in 1968.
Ormsby County, NV merged with Carson City, NV in 1969.
Nansemond County, VA was incorporated into Suffolk City, VA in 1972.
Manassas City, VA and Manassas Park City, VA became independent of Prince William County, VA in 1975.

2. Natality Data Anomalies for 1959 to 1967

1961: Alabama Nonwhite and White Totals for Dallas County did not sum to published total for number of live births by place of occurrence.
1962: Ohio Nonwhite Total and White Total did not sum to published total for number of live births by place of occurrence; Ohio Nonwhite and White Totals for Cuyahoga, Hamilton, Montgomery, Stark, and Summit Counties did not sum to published total for number of live births by place of occurrence.
1963: Wisconsin Nonwhite Total and White Total did not sum to published total for number of live births by place of occurrence.
1964: Indiana Totals for DeKalb and Lagrange Counties were illegible for number of live births by place of occurrence.
1965: California Total for Lake County was illegible for number of births by attendant (other) by place of residence; North Carolina Total for Mitchell County was illegible for number of births by attendant (other) by place of residence.

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