DOES FAMILY PLANNING INCREASE CHILDREN'S OPPORTUNITIES?
EVIDENCE FROM THE WAR ON POVERTY AND THE EARLY YEARS OF TITLE X

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Abstract:

This paper examines the relationship between parents' access to family planning and their children's economic resources. Using the county-level introduction of U.S. family planning programs between 1964 and 1973, we find that children born after family planning programs began had 2.5% higher household incomes. They were also 7% less likely to live in poverty and 11% less likely to live in households receiving public assistance. A bounding exercise shows the most extreme type of selection fails to explain these effects. This finding implies an important role for empowerment effects, including increases in mothers' paid work and increased childbearing within marriage.

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...I called for a national commitment to provide a healthful and stimulating environment for all children during their first five years of life. One of the ways in which we can promote that goal is to provide assistance for more parents in effectively planning their families. ...Unwanted or untimely childbearing is one of several forces which are driving many families into poverty or keeping them in that condition.

~President Richard Nixon, Special Message to the Congress on Problems of Population Growth, July 18, 1969.

A growing body of literature shows that early childhood and family characteristics are important determinants of lifetime health, earnings, and well-being (Almond and Currie 2011). Household income in childhood, in particular, is one of the strongest correlates of completed education and adult health (Case 2002, Almond and Currie 2011). There are many potential reasons: Poor children receive fewer parental time and resource investments and are more likely to experience health and academic problems, live in more dangerous neighborhoods, attend underperforming schools, and be incarcerated (Guryan, Hurst, and Kearney 2008; Levine and Zimmerman 2010). Poor children ultimately have lower academic test scores (Reardon 2011), lower rates of high school and college completion (Bailey and Dynarski 2011), and ultimately lower earnings in adulthood (Pew Charitable Trusts 2012).

This cycle of disadvantage is the target of a variety of public policies and programs which aim to reduce gaps in early childhood resources and improve children's lifetime outcomes (Currie and Rossin-Slater 2014). Programs such as the Temporary Assistance for Needy Families (TANF) and the Earned Income Tax Credit (EITC) raise directly the household incomes of poor children at costs of around $10 and $63 billion in 2013, respectively. Head Start provides subsidized pre-school for around 930,000 disadvantaged children for around $8 billion in 2014. Medicaid and the State Children's Health Insurance Plan (S-CHIP) provided nearly 75 million disadvantaged children with health insurance at a cost of around $444 billion in 2013.

This paper evaluates the impact of a less studied program targeting the cycle of economic disadvantage. Family planning programs, largely absent from today's policy discussions of childhood disadvantage, has been used as a means to improve children's lives since the 1960s (Johnson 1966[1], Nixon 1969 quote above). The programs' potential effects relate closely to economic theory. Holding parents'

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[1] President Johnson said in a Special Message to the Congress on Domestic Health and Education, "We have a growing concern to foster the integrity of the family and the opportunity for each child. It is essential that all families have access to information and services that will allow freedom to choose the number and spacing of their children." (March 1, 1966).

income constant, a reduction in the number of children reduces the shadow price of child quality and promotes investments in each child (Becker and Lewis 1973; Willis 1973).[2] In addition to these price effects, family planning programs could increase parents' income directly by helping parents avoid unwanted or ill-timed births and invest in their own human capital, partnerships, and careers (Goldin and Katz 2002, Bailey 2006, Bailey et al. 2012). Standard quantity-quality models suggest that these increases in parent income further promote investments in children and can mitigate the importance of credit constraints.

The difficulty of evaluating these effects of family planning programs relates to the role of "selection."[3] If family planning programs cause more disadvantaged parents to opt out of childbearing, this mechanically causes the resources of the average child to rise *while failing to increase the resources of any child that is born*. The empirical importance of selection is potentially large. For instance, the literature on abortion legalization argues a central role for selection in explaining effects on child outcomes in the short run (Gruber, Levine and Staiger 1999) as well as in the longer run (Ananat, Gruber, Levine and Staiger 2009; Pop-Eleches 2006). Even though family planning programs are not means-tested, they tend to affect contraceptive use among more economically disadvantaged women (Jaffe, Dryfoos and Corey 1973; Torres and Forrest 1985) which makes evaluating their impact on children's resources especially difficult.

This paper provides new empirical evidence on the effect of family planning programs on the resources available to children *after accounting for selection*. It proceeds in two steps. First, using the roll-out of federally funded family planning programs in the 1960s and early 1970s, our research design compares the outcomes of children in the same county born before and after federally funded family planning programs began.[4] Using the restricted-use long-form 1970 and 1980 census samples, an event-

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[2] In addition, reductions in unwanted children may have an independent negative effect on child outcomes through mechanisms linked to health at birth (Corman and Grossman 1985; Grossman and Joyce 1990; Gruber, Levine and Staiger 1999) or parents' treatment of children (David et al. 2003, David 2006).

[3] A large literature studies the relationship of childbearing to child welfare. Schultz (2008) provides a thorough review of these studies in the context of developing countries. A much smaller literature studies the relationship of family planning programs and children's outcomes. See Miller and Babriaz (forthcoming) for a review of these studies for middle- and low-income countries. Section II of our paper also reviews related studies of abortion legalization in the U.S.

[4] This research design is well documented. As noted by Bailey (2012), the roll-out was first funded through the Office of Economic Opportunity (OEO) during what was described as a "wild sort of [grant making] operation" (Gillette 1996: 193) and then, later, through the Department of Health Education and Welfare (DHEW) under Title X of the 1970 Public Health Service Act. Supporting the internal validity of this design, the roll-out of federal family planning programs is uncorrelated with pre-existing differences and changes in county-level fertility rates and measures of economic disadvantage, childbearing, sexual behavior, birth control use, and attitudes about sex and family before family planning programs began from the 1965 National Fertility Study (NFS). It is also

study analysis shows that cohorts born after family planning programs began were significantly more advantaged along several dimensions. They lived in households with 2.5 percent higher annual incomes relative to children in the same counties born before family planning programs began. They were also 7 percent less likely to live in poverty and 11 percent less likely to live in households receiving public assistance. Finally, these children were slightly more likely to live with two parent families, have younger mothers, and fewer older siblings.

The second part of this paper develops a novel theoretical framework to account for the role of selection in explaining these results. Using the 1960 census, we simulate the effect of family planning programs on children's household income by modelling selection. Under the extreme assumption that all of the 2 percent of births averted due to family planning programs would have been the *poorest* children, selection can explain at most 86 percent of the increase in children's household incomes. Under the more plausible assumption that births averted due to family planning programs came from households with incomes following the distribution of users of family planning programs, selection would account for only 40 percent of the increases in children's household incomes. Our simulation exercise shows that the "empowerment effect"—our term for the effect of family planning on parents' human capital investments (e.g., in their educations or careers), labor-force participation (especially of mothers), and partnerships (e.g., better selection of and more stable unions)—should explain at least 15 percent and possibly as much of 60 percent of the income gains to children. We conclude with a discussion of how family planning programs' empowerment effects compare to other public programs targeting disadvantaged children. Netting out the likely selection effects, our estimates imply that family planning programs may directly reduce child poverty at around half of the cost of the EITC and one third the cost of TANF.

I.   A HISTORY OF U.S. FAMILY PLANNING PROGRAMS, RELATED LITERATURE, AND THEIR EXPECTED EFFECTS ON CHILDREN'S OUTCOMES

Enovid, the first birth control pill, was approved for use as a contraceptive by the U.S. Food and Drug Administration in 1960 and was immediately in high demand. But *Enovid* was under patent and

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unrelated to the roll-out of other OEO programs that could have similar effects on children. As documented in national reports on family planning use and the 1970 NFS, however, the use of family planning services and the birth control pill rose significantly among disadvantaged women after these programs began.

prohibitively expensive. In the early 1960s, an annual supply of "the Pill" sold for the equivalent of $812 in 2013 dollars—roughly twice today's annual cost and equivalent to more than three weeks of full-time work at the 1960 minimum wage (without factoring in the cost of visiting a physician).

The implications of the Pill's costs raised concern among policy makers. Social scientists noted the strong *negative* relationship between household income and the number of children and the strong *positive* relationship of household income with birth control pill use. In 1960, 54 percent of women with less than a high school education had two or fewer children versus 77 percent among more educated women; 30 percent of women with less than a high school education had four or more children versus only 13 percent among more educated women.[5] The 1965 NFS also showed that poor women were significantly less likely to have ever used the Pill.

Widespread concern about disparities in access to the Pill, higher rates of childbearing among lower income women (National Research Council 1965), population growth (Wilmoth and Ball 1992, 1995), and the cycle of poverty galvanized support for federal intervention. The architects of President Johnson's War on Poverty viewed reducing income-based disparities in access to contraception as a means of promoting children's opportunities and increasing well-being in the long run.

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

The first U.S. family planning programs were quietly funded under the 1964 Economic Opportunity Act (EOA), a centerpiece of President Johnson's War on Poverty. The Office of Economic Opportunity (OEO), the office in charge of administering EOA funding, supported the opening of new clinics in disadvantaged areas and, to a lesser extent, the expansion of existing family planning programs. With the designation of family planning as a "national emphasis" program under the 1967 EOA amendments, federal funding for family planning rose to roughly 427 million (2013) dollars by 1970.

The aim of these programs was to bring education, counseling, and the provision of low-cost contraceptives and related medical services to disadvantaged women. (Programs did not provide abortion,

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[5] These figures are from the 1960 decennial census using a sample of ever-married women ages 41 to 50 years (Ruggles et al. 2010). In the 1965 NFS, women in households with incomes below the poverty line for a family of four had 0.60 more children on average (s.e. 0.16) than families earning at least four times as much. Poor women were over 44 percent (0.082 percentage points / 0.185, s.e. 0.023) less likely to use the Pill than the same group of more affluent women.

which was still illegal except in special circumstances before 1970). But little else is known about these programs' day-to-day operations. During these early years, organizations ran programs with little oversight from the federal government. The federal government did not collect information on their services or patients, and officials talked very little about them.[6] The varied implementation of this program implies that its treatment effect represents a combination of many services and types of programs, all of which provided reduced cost contraceptives and related services.

The second large increase in federal funding for family planning occurred under President Nixon. In 1969, Nixon asked Congress to "establish as a national goal the provision of adequate family planning services within the next five years to all those who want them but cannot afford them." In November 1970, Congress passed Title X of the Public Health Service Act (also known as the Family Planning Services and Population Research Act, P.L. 91-572). By 1974, this legislation had increased federal support by 50 percent in real terms over EOA levels. As with family planning under the EOA, little is known about how federal family planning dollars were spent in this early period. Most are believed to have paid for education, counseling, and the provision of low-cost contraceptives and related medical services. Abortion had been legalized in several states, but Title X explicitly prohibited the use of federal funds "in programs where abortion is a method of family planning" (§1008).

These two large increases in funding expanded the availability of family planning unevenly across counties and years which provides the identifying variation for this study. Figure 1 shows the roll-out of these family planning programs at the county level. The earliest programs, established between 1964 and 1967, are shaded in the lightest gray; the programs established between 1968 and 1969, during the expansion of family planning as a national emphasis program, are shaded in the next darkest gray; and programs established from 1970 to 1973 under Title X are in black. As shown in appendix table A1, funded counties (what we call those receiving a federally funded family planning program) differed from unfunded counties. Funded counties were more urban, had more elderly residents, and were more educated and affluent. Funded

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[6] Sar Levitan (1969: 209) wrote that, "Contrary to the usual OEO tactic of trying to secure the maximum feasible visibility for all its activities, the OEO prohibited [family planning] grantees from using program funds to 'announce or promote through mass media the availability of the family planning program funded by this grant.'" Before 1965, U.S. federal involvement and investments in family planning had been modest.

counties also had lower poverty rates. Our analysis accounts for these cross-sectional differences using county-fixed effects or by examining a sample of funded counties only and relies upon within county changes in family planning services for identification.

B. Did Federal Grants Increase the Use of Family Planning Services or Reduce Fertility Rates?

By 1973, federal funding had initiated or substantially expanded over 660 family planning programs in each of the lower 48 states. These programs funded services in locations where roughly 56 percent of the U.S. population of women ages 15 to 44 lived. Previous studies have noted the national, four-fold increase in family planning patients at federally funded programs from 1969 to 1983, but none has been able to quantify the extent to which this reflected the crowd out of non-federal family planning programs or changes in the use of contraceptives.

Two new data sources uniquely allow this paper to describe these effects. The first data source is a series of OEO reports from four different years. These reports supply information on the use of family planning services by county as reported by *all known* providers (hospitals, health departments, and clinics operated by other agencies) (OEO 1969, 1971, 1974).[7] We entered these data and estimate a differences-in-differences specification, where we code the first year a county receives a federal grant as the "treatment" dummy.[8] Panel A of table 1 shows that federal family planning programs significantly increased the use of family planning services among "medically indigent" women. The share of medically indigent women using family planning services increased by around 2.7 percentage points *after* the federal family planning programs began. These estimates are robust to the inclusion of additional covariates (cols. 1 to 3) and about half the magnitude of the national increase in family planning program use over the same period.

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[7] Completion rates of the survey were high. In 1968, for example, 97 percent of hospitals and 100 percent of all other agencies responded (OEO 1969, table 3: 244). The purpose of the survey was to approximate the universe of potential family planning providers for our period of interest and document the number of "medically indigent" patients (but not other patients).

[8] We estimate $Y_{j,t} = \theta_j + \gamma_{s(j),t} + \tau 1(t > T_j^*) + X_{j,t}'\beta + \varepsilon_{j,t}$ where $Y_{j,t}$ is the share of medically indigent patients in county j using family planning services from any provider (federally funded or not) in time t (FY 1968, CY 1969, and FY 1971), θ_j is a set of county fixed effects, $\gamma_{s(j),t}$ is a set of state-time fixed effects, and X is a set of covariates including REIS controls and 1960 county covariates interacted with a linear trend. The binary indicator, $1(t > T_j^*)$, is equal to 1 for observations in years after the date county j received its first federal family planning grant, T_j^*. The point estimate of interest, τ, captures the differential change in share of medically indigent women using family planning services after federal family planning programs were established. With the inclusion of county fixed effects, only counties receiving federal programs between 1968 and 1971 identify τ.

The second data source is the 1970 NFS. This survey sampled ever-married women between the ages of 18 and 44 and provides an alternative perspective from the point of view of individuals. We estimate a probit model to examine whether respondents living in counties that had received a federal family planning grant before 1970 were more likely to have used the Pill by the time of the survey (table 1, panel B). We also examine whether the expected increase in Pill use was larger among poorer women.[9] The estimates show that poor women in areas funded before 1970 were much more likely to have used the Pill relative to poor women in areas funded after 1970, an increase of approximately 25 percent (15 to 17 percentage points) over the mean among poor women. In fact, the effect of receiving a federal family planning grant is large enough to erase income-based differentials in Pill use.[10]

Both analyses provide consistent evidence that federal family planning grants increased the use of family planning services by a sizable amount. While this evidence is suggestive and helps understand the magnitude of changes in children's outcomes quantified later in the paper, the sparseness of data (the OEO reports were collected at only four points in time; the 1970 NFS do not have complete coverage of all counties or time periods or include never married women) limit strong conclusions.

Indirect evidence on the effect of family planning programs on contraceptive use and method choice comes from the relationship of these programs to fertility rates. Family planning programs reduce the cost of using more reliable, medical contraceptives by lowering the direct cost of using these methods and increasing the number of clinics (reducing the time cost of obtaining contraceptives). Economic theory suggests that these reductions in the price of medical contraception reduces the price of averting births, which could decrease the number of *wanted* births and change the *desired* timing of childbirth (Michael and Willis 1976).

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[9] We estimate the specification, $\Pr(\text{Use}_{ij}) = \mathbf{F}(\mathbf{Z}_{ij}\boldsymbol{\delta} + \theta_1 1(T_j^* < 1970) + \theta_2 1(\text{Pov}_{ij}) + \theta_3 1(\text{Pov}_{ij})1(T_j^* < 1970))$, where Use_{ij} is equal to 1 if an individual i in county j had ever used family planning or the birth control pill, $1(T_j^* < 1970)$ is a binary variable equal to one if county j received a federal family planning grant before the survey date, and $1(\text{Pov}_{ij})$ is a binary variable equal to 1 if the annual household income in 1970 fell below the poverty line. County fixed effects cannot be included with this single cross-section, but we include a rich set of covariates, $\mathbf{Z}$, including state fixed effects, dummy variables for age, educational achievement, population size of the county, and Catholic religion. In addition, one specification includes dummy variables for the "number of children most desirable" to capture residual, unaccounted-for differences in the demand for children. Panels B and C of table 2 report average partial effects associated with θ_1 and θ_3 from probits and bootstrapped standard errors (1000 replications) and capture differences in the use of family planning services and the Pill in funded counties by 1970.

[10] Another interesting finding is that Pill use increased by about 4 percentage points for women *above* the poverty line in funded locations. This estimate is not statistically significant but suggests family planning grants may have reduced the price of the Pill among women using other sources of family planning.

In addition, reductions in failure rates arising from using more reliable methods may reduce *unwanted* or *ill-timed* births as well. Provided the research design is sound, any effects of family planning programs on fertility rates should result only through their effects on method use and method choice.

In work closely related to this study, Bailey (2012) exploits the county-level roll-out of federal family planning programs between 1964 and 1973 as a natural experiment to quantify their fertility effects. Using an unweighted, event-study specification and county-level Vital Statistics data on births from 1959 to 1988, she shows that fertility rates fell by 2 percent within 5 years after federal family planning programs began and remained approximately 1.7 percent lower for up to 15 years (see appendix figure A3). Supporting the validity of the research design, differences between eventually funded counties and never funded counties were not statistically different from zero in the pre-period and post-funding effects are robust to the inclusion of county and year effects, state-by-year fixed effects, and a variety of time-varying county-level covariates.[11] In summary, a body of evidence supports the conclusion that federally funded family planning programs affected contraceptive use and, therefore, childbearing outcomes.

C. Related Literature and Expected Effects

The more difficult question is understanding how family planning programs affect the economic and living circumstances of children. The related literature suggests two complementary ways one might interpret resulting changes in children's outcomes: selection and empowerment.

Selection, or changes in the composition of parents, could play a key role in explaining improvements in cohort outcomes. For instance, if family planning programs cause more disadvantaged parents to opt out of childbearing or stop sooner, the resources of the average child would rise *while failing to increase the resources of any given child that is born*. Even though family planning programs were not means-tested, selection may be a particularly important channel in explaining these programs' effects. Programs in the 1960s disproportionately served poorer households and would tend to affect contraceptive use the most among these women. Roughly 83 percent of these family planning patients had incomes below

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[11] A related study by Kearney and Levine (2009) of the more recent period exploits state-level variation in Medicaid eligibility for family planning among the near poor within a differences-in-differences framework. This study finds that greater eligibility for services in 17 states significantly reduced birth rates among teens (by 4 percent) and among older women (by 2 percent) within a few years.

150 percent of the poverty line, and 13 percent were recipients of Aid to Families with Dependent Children (AFDC, the principal cash welfare program at the time) (Jaffe, Dryfoos and Corey 1973; Torres and Forrest 1985: 284).

The selection mechanism is the literature's main explanation for improvements in children's outcomes that followed the legalization of abortion. Using the staggered legalization of first-trimester abortion in the U.S., initially in five states around 1970 and then in the remainder of states with *Roe v. Wade* in 1973, studies show not only that birth rates fell by 4 to 8 percent (Levine et al. 1996) but they also argue that improvements in child outcomes reflect changes in selection. Gruber, Levine and Staiger (1999) show that children born after abortion legalization were less likely to die as infants, live with single parents or with families receiving welfare, and less likely to live in poverty. Donohue and Levitt (2001) show that cohorts born after abortion legalization were less likely to commit crime,[12] and Charles and Stephens (2006) show that these cohorts were less likely to use controlled substances in their late teens. The country of Romania experienced the reverse policy change. Pop-Eleches (2006) shows that the dictator's 1967 declaration of abortion and family planning increased birth rates by around 30 percent and worsened children's socio-economic outcomes (after accounting for the positive selection of mothers). In both cases, studies of the longer-run effects of abortion legalization argue that *selection* played an important role (Ananat, Gruber, Levine and Staiger 2009; Donohue and Levitt 2001; Charles and Stephens 2006; Pop-Eleches 2006).

A second mechanism is empowerment. If family planning altered parents' decisions by relaxing the biological constraints of fecundity, these programs could *directly* affect the economic resources and living circumstances of children. Evidence supporting empowerment comes from the literature on access to the birth control pill. Using variation in state laws regulating access to contraception for (mostly) unmarried teens (see Bailey, Guldi, Davido, and Buzuviz 2012; Guldi 2011), this literature shows that access to the birth control pill affected marital and birth timing and had lasting effects on women's and men's education, career investments, and lifetime wage earnings. With earlier access to the Pill, women and men were more likely to enroll and complete college (Hock 2008; Bailey, Hershbein, and Miller 2012). Women were more

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[12] Claims that abortion reduced crime is disputed (Foote and Goetz 2008, Donohue and Levitt 2004; Joyce 2004).

likely to work for pay, invest in on-the-job training, and pursue non-traditional professional occupations (Goldin and Katz 2002; Bailey 2006; Bailey, Hershbein, and Miller 2012). And as women aged, these investments paid off in terms of higher wages (Bailey, Hershbein, and Miller 2012). Ananat and Hungerman (2012) additionally show that access to contraception at younger ages improved the economic resources of children born to these women, although they provide no evidence on whether these gains accrued due to selection or empowerment channels.

By similar logic, access to family planning programs may affect young adults' life-courses. Soon-to-be parents could use family planning services to delay childbearing in order to get more education, select different career paths, or obtain different amounts of work experience and job training. Women might make different investments in their careers or stay attached to a job if they *expect* to be able to control future childbearing. These changes would tend to increase their economic resources as well as those of their future children.

Alternative channels are plausible as well. Access to family planning programs could also have sizable effects on women's labor-force participation and couples' decisions to remain married, if parents opt to have children at times when the impact on family income or marital stress is lower. If family planning programs allow older women to time births better, they could raise the labor-force participation of mothers, which could have large effects on household incomes of children once they are born. On the other hand, parents may decide to have children earlier in their careers if subsequent births are more easily avoided, which could reduce the incomes of children in the short run but not necessarily in the long run.[13]

Another way in which family planning could affect children's household income is by altering partnership decisions. For instance, family planning programs could reduce the price of delaying marriage (Goldin and Katz 2002) and improve spouse matching and reduce marital stress, thereby reducing subsequent divorce rates (Christensen 2011, Rotz 2011). On the other hand, family planning programs could increase

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[13] Recent empirical work informs these hypotheses. Studies evaluating the effects of early legal access to the Pill show that these law changes increased women's investments in their careers and, ultimately, their wages (Bailey, Hershbein, and Miller 2012). Early access to the Pill also appears to have increased men's educational attainment (Hock 2008), perhaps through intra-household bargaining. On the other hand, studies examining women who became mothers in their teens (relative to teens who miscarried) had *higher* subsequent levels of employment and earnings (Hotz, McElroy and Sanders 2005).

non-marital childbearing (Akerlof, Yellen and Katz 1996), which should lower household incomes available to children.

In summary, the availability of family planning programs may empower both younger and older parents by affecting completed childbearing and spacing, marital decisions, and career investments and opportunities. These programs could also affect cohort outcomes by altering selection into parenthood. The next section describes this paper's empirical strategy to estimate the combined effects through the selection and empowerment channels of family planning programs on cohort outcomes. After presenting the results, a final section quantifies and bounds the role of selection under different assumptions of income distributions of family planning program beneficiaries.

II.   DATA, RESEARCH DESIGN, AND MEASUREMENT ERROR CORRECTIONS

The restricted, long-form samples of the 1970 and 1980 censuses provide information on children's economic resources and living circumstances. In addition to their large sample sizes (20-percent and 16-percent samples of the U.S. population for 1970 and 1980, respectively), these samples identify counties (this information is suppressed in public samples).[14] Our analysis aggregates the economic resources and living circumstances of children under age 18 into birth-year/county cohorts.[15] These birth-year/county cohorts are then linked to information on when their county received a federally funded family planning program.[16]

A.   Empirical Specification

Our primary specification describes the evolution of outcomes for cohorts born before versus after family planning programs began in their county of residence within the following event-study framework:[17]

$$(1) \qquad Y_{j,t} = \theta_j + \gamma_{s(j),t} + \sum_{c=a}^{b} \tau_c 1(t - T_j^* = c) + X_{jt}'\boldsymbol{\beta} + \varepsilon_{j,t} \,,$$

where Y is a measure of the outcomes for children residing in county j within state s and born in calendar year t. $1(\ )$ indexes birth cohorts relative to the year of the first federal family planning grant, T_j^*

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[14] Public census samples identify county groups (which change between 1970 and 1980) and are much smaller. We gained access to the restricted data after a formal application process through the University of Michigan Research Data Center.

[15] Birth year is constructed using age and quarter of birth at the time of the census.

[16] These data come from the National Archives Records about Community Action Program Grants and Grantees (NACAP), which contain information on family planning programs funded under the EOA, and the National Archives Federal Outlay (NAFO) files, which contain information on family planning programs funded under Title X. Bailey (2012) describes these data in greater detail.

[17] We run separate regressions by census years. This is because in the 1970 census data only the 461 counties that received their first family planning grant before 1970 identify τ, whereas for 1980 the full set of funded counties identify τ.

—our proxy for the date the program started.[18] Thus, event time, c, runs from a years before up to b years after the date of the first family planning grant, which varies by census year.[19]

Our baseline specification includes θ, a set of county fixed effects which capture time-invariant county-level differences, and γ, a set of state-by-birth year fixed effects that capture time-varying changes in state policies, including abortion legalization and the roll-out of Medicaid. In addition, we estimate a differences-in-differences model that restricts τ_c to equal 0 for $c<=0$ and restricts all post-effects to be equal, $\tau_c = \tau$ for $c>=1$. This analysis, thus, recovers the regression-adjusted evolution of children's outcomes for cohorts born from six years before (in the 1970 census) and up to six years after (1980 census) each county received its first federal family planning grant. Standard errors are robust to heteroskedasticity and corrected for serial correlation within state (Arellano 1987, Bertrand, Duflo, and Mullainathan 2004).

The robustness of these baseline results are examined by sequentially including covariates used in other studies of the War on Poverty. Covariates include annual information on per capita measures of government transfers 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) (cf. Almond, Hoynes and Schanzenbach 2011). We also include the number of abortion providers, which accounts for within-state changes in the provision of abortion between 1970 and 1979 (cf. Bailey 2012).[20]

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[18] Data limitations (federal family planning grant information is missing for some years) make it impossible to use federal grant dollars as the independent variable of interest. But even if this were feasible, we prefer a binary measure of family planning access for several reasons. Using variation in federal funding could also be related to program performance, which could induce reverse causality and threaten the interpretation of our estimates. Second, as described in section I, federal dollars paid for infrastructure when needed and also many programs were heavily supported by other public and non-profit funds by the end of the period of interest. Thus, federal dollars are poor proxies of program size or intensity. Our specification captures the fact that federal dollars *created* or significantly expanded family planning programs.

[19] The 1980 census allows us to examine the evolution of outcomes for cohorts born up to six years after the establishment of the family planning program for a balanced set of counties. The 1980 census, however, only observes a two-year cohort pre-trend for a balanced set of counties because many of the individuals in cohorts born before 1963 had begun leaving home (and the earliest family planning programs began in 1965). Therefore, we set $a=-3$ when $c\leq-3$ and $b=7$ when $c\geq7$, and event-years −2 through 6 are estimated using all funded counties for the 1980 census. We estimate separate regressions with the 1970 census and set $a=-7$ when $c\leq-7$ and $b=1$ when $c\geq1$. $c=0$ is omitted in both cases to facilitate easy comparisons across census years.

[20] The interactions of county covariates are identical to those in Almond, Hoynes and Schanzenbach (2011). Because information on abortion providers is not available at the county level before 1973, we follow Joyce, Tan, and Zhang (2013) in assuming the number of providers in 1970 to 1972 in states that legalized before Roe v. Wade is identical to the number observed in 1973.

B.   The Internal Validity of Using Program Roll-Out as a Natural Experiment

The idea behind using the program roll-out is that the timing of implementation approximates the

conditional random assignment of parents to greater access to family planning. Both historical and

quantitative evidence supports the internal validity of this research design. Oral histories and interviews note

that the OEO funding was not targeted or well organized. 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" (Gillette 1996: 193). Robert Levine (1970) sums up

the situation saying, "It was an era of great administrative confusion."

Quantitative evidence shores up this narrative as well. Aside from urbanicity, 1960 county

characteristics found to predict the roll-out of other War on Poverty programs fail to predict the initiation of

federal family planning programs (see appendix table A2; cf. Hoynes and Schanzenbach 2009; Almond,

Hoynes, and Schanzenbach 2011). Second, 1964 fertility rates or 1960 to 1964 changes in fertility rates

(appendix figure A1) are not correlated with the initiation of federal family planning programs. This means

that applicants were not more likely to apply for programs *and* that administrators were not more likely to

prioritize funding programs based on differentially high (or low) fertility rates.

A third piece of evidence is that reproductive and contraceptive attitudes and behaviors in the 1965

NFS are uncorrelated with the initiation of federal family planning programs (appendix table A3). In

addition to running individual level regressions, we also pooled outcomes to improve the statistical power to

detect correlations that move in a common direction. Following Kling, Liebman and Katz (2007), we created

a summary index of equally weighted average z-scores of pro-natalist responses to questions regarding

contraceptive attitudes, behaviors, and other correlates of the number of children. Including the variables in

appendix table A3 in a common index (normalized by the mean and standard deviation of the unfunded

distribution), we find no evidence that the year of the first federal grant for family planning is related to this

index (coefficient: 0.054, robust standard error: 0.047, observations: 2,857).[21]

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[21] This z-score index excludes age at first pregnancy and age at first Pill use from the index. Including the age at fist pregnancy
(which implicitly omits women who never became pregnant), we find that the year of the first federal grant to family planning is

A final piece of quantitative evidence shows that the initiation of federal family planning programs is uncorrelated with the initiation of other War on Poverty programs (appendix figure A2). The likelihood of receiving a family planning grant does not appear to be correlated with the likelihood of receiving a community health center, a Head Start grant, a jobs program grant, a legal services grant, or a grant for maternal and infant care. In short, the qualitative and quantitative evidence is consistent with the implementation timing of federal family planning grants being conditionally, randomly assigned.

C. Strategies to Minimize Misclassification Error in Treatment Status

An important challenge to our analysis is that the censuses only contain information on a child's residence in (or five years before) the census year, not at the time of the child's birth. This implies that we may misclassify mothers' access to federal family planning around the time of conception. If misclassification is random, this should generally lead our analysis to understate of program's effects on children's outcomes. We diagnose the severity of misclassification attenuation by comparing estimates of specification (1) for the Vital Statistics birth rates (which contain county of birth) and 1980 census (which uses county of residence in 1980 and year of birth). We find that misclassification error is large enough so as to completely obscure the fertility effects of family planning programs in the census (appendix figure A3, panel A). Whereas Vital Statistics (using county of mother's residence at birth) show a large and precisely estimated 2 percent reduction in fertility rates following the introduction of family planning programs (Bailey 2012), the census yields imprecise zeros for the same specification and cohort sample.

Differential mobility in areas with family planning programs and mobility that differentially increases after the programs begin is consistent with theoretical predictions. The empowerment model suggests that family planning programs allow women to make different location choices because they would be less likely to be constrained by a child after family planning programs began. Without an ill-timed birth, women should be more likely to move to attend school, pursue a better job, or follow a partner. They would also be less geographically constrained by the location of grandparents, who may help provide childcare. These predictions are born out empirically: appendix figure A4 shows that children born after family

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positively but insignificantly related to the index (coefficient: 0.043, robust standard error: 0.054, observations: 2,607). We exclude when a woman first used the Pill (appendix table A3, col. 6) because its inclusion severely limits our sample size.

planning programs began were significantly more likely to live with a parent who moved in the five years

before the census.[22]

To limit attenuation due to misclassification error in this analysis, we make several deliberate

specification choices. First, we use county of residence five years before the census, because 1965/1975 is

more temporally proximate to the treatment. Second, although Bailey (2012) presents weighted and

unweighted estimates (recommended best practice, Solon et al. 2015) and finds similar results, our regression

analysis omits weights. This is because mobility (and hence mobility-induced misclassification error) is

much greater in more populous places, significantly attenuating the weighted estimates. These two decisions

reduce misclassification error substantially, so that the census estimates of the effects of family planning

programs on fertility rates are similar to those from the Vital Statistics (appendix figure A3, panel B).

A final correction for measurement error adapts the approach of Card and Krueger (1992) to

characterize the sign and magnitude of any remaining bias due to mobility. Their insight is that mobility

induced measurement error leads the estimated coefficients, τ_c, to be a reweighting of the true

coefficients, τ_j^*. In our case,

$$\tau_c = \sum_{j=a}^{b} \tau_j^* \cdot p_{j,c}.$$

(2)

Here $p_{j,c}$ is the probability of being born in a county treated with family planning j years before birth

conditional on living in a county at the time of the census that was treated c years before birth. To

characterize how mobility could bias our estimates, we use the matrix form of equation (2) above to recover

the full set of estimates, $\boldsymbol{\tau}^* = \boldsymbol{\tau} \boldsymbol{P^{-1}}$.[23] Even after imposing other corrections, this ex post adjustment for

misclassification modestly increases the magnitude of the estimates, which is consistent with mobility

induced misclassification error attenuating the results. It is also reassuring that the implementation of these

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[22] The selection model also predicts a smaller relationship in the census than Vital Statistics. For instance, if cohorts born after family planning programs began were more likely to be wanted, they may have also been less likely to die in infancy or childhood (Gruber et al. 1999). Although an increase in the number of children surviving to the census due to family planning programs' effects on wantedness would tend to attenuate census estimates of fertility reductions, these programs had no measurable effects on infant mortality (Bailey 2013).

[23] $\boldsymbol{\tau}$ is a (b-a+1) × 1 column vector containing each of the event-study coefficients, τ_c for c=a, a+1, a+2…, b, and $\boldsymbol{P}$ is a (b-a+1) × (b-a+1) matrix with elements of the transition probabilities, $p_{j,c}$, such that $\boldsymbol{\tau} = \boldsymbol{\tau}^* \boldsymbol{P}$. Note that this inversion also assumes that migration is uncorrelated with treatment, which holds in this context in our analysis. We estimate $p_{j,c}$, as the probability of living in a county in 1975/1965 receiving a family planning program j years before a birth conditional on living in a county in 1980/1970 that received a family planning program c years before birth. Implicitly, this assumes that county-to-county misclassification of treatment status between 1975 and 1980 (or 1965 and 1970) is correlated with mobility induced misclassification that occurred before 1965/1975.

corrections yields fertility estimates in the census that are virtually identical to the Vital Statistics estimates in event years 4 through 5 (see appendix figure A3, panel B).

III. ESTIMATION RESULTS: CHANGES IN ECONOMIC RESOURCES AND LIVING CONDITIONS AMONG CHILDREN BORN AFTER FAMILY PLANNING PROGRAMS BEGAN

Our main paper results and discussion focus on the results from our baseline specification, which includes county and state-by-birth-year fixed effects. Online appendix tables contain all point estimates for the 1970 and 1980 censuses, robustness checks, and adjustments for misclassification using equation 2, none of which alter the conclusions presented here. Figures simplify the interpretation of the coefficients by plotting estimates of τ divided by the pre-treatment mean dependent variable for both the 1970 and 1980 estimates. The series, therefore, denote changes in *percent* for each birth cohort indexed relative to the year the family planning program began. Estimates to the left of the vertical axis represent cohorts born in event years before family planning programs began (1970 census; plotted in dashed lines with markers), and estimates to the right of the vertical axis capture cohorts born after family planning programs began (1980 census, solid lines with markers). Dashed lines present 95-percent, point-wise confidence intervals for the baseline model.

Figure 2 begins by summarizing the effects of family planning programs on the household income of the average child. Consistent with both the selection and empowerment effects, panel A makes clear that cohorts born after federal family planning programs began had significantly higher household incomes. Although the household income of the average child was stable before family planning programs began, the introduction of family planning programs corresponds to a notable trend break. Table 2 summarizes the differences-in-differences weighted average of these estimates for the 1980 census. Cohorts born four to six years after the program began had household incomes that were on average 2.2 (col. 2) to 2.5 percent higher (col. 4). In contrast, the coefficients on household incomes among children born up to six years before family planning programs began exhibit little trend and are not statistically different from the year the county received its first family planning grant (see 1970 results in appendix table A4.2).

Table 2 also demonstrates the robustness of these results. Column 1 includes only county and year fixed effects, column 2 adds 850 state-by-year fixed effects (50 states *17 birth years), and column 3 adds

county-level covariates and controls for abortion providers. Column 4 further adjusts for misclassification error in our baseline model. As expected, applying our adjustment in column 4 for misclassification using 5-year migration patterns raises the post-period estimates. Because the restricted census data is not top-coded, we also investigate the importance of outliers by trimming. Another robustness check shows that trimming the top and bottom 1 percent of children's household incomes, to account for possible outliers, has little effect on these estimates.[24] Finally, using per-capita household income as a dependent variable shows that the effects are even stronger after taking account of the number of people in the household in 1980: the average child born four to six years after the program began had a per-capita household income that was sixty percent larger at 3.5 percent (appendix table A4.3, col 2), although this estimate is imprecise.

Administrative statistics suggest that these results should be driven by families at the lower end of the income distribution. Eighty-three percent of family planning patients had incomes below 150 percent of the poverty line. Figure 3A shows that increases in household income led to reductions in poverty that were larger at the lower end of the income distribution. Averaging event years 4 to 6, children born after federal family planning programs began were 6.8 percent less likely to live in poverty (figure 3A; table 3, col. 1), 6.0 percent less likely to live below 1.5 times the poverty line (figure 3B; table 3, col. 2), and 3.1 percent less likely to live below 2 times the poverty line (figure 3C; table 3, col. 3). As with household income, we find no evidence that these reductions reflect a pre-trend.

Administrative statistics also suggest that 30 percent of family planning patients were nonwhite (whereas only 17 percent of women ages 15 to 44 in the population were nonwhite). This suggests that the intention to treat effects of family planning should be larger among nonwhites. Consistent with this finding, the absolute and relative reductions in poverty rates were two to four times larger in the post-period among nonwhite children than for white children (table 3). Owing to smaller samples for nonwhites, event study estimates for this group are imprecise. The differences-in-differences estimates suggest the share of nonwhite children in poverty was a significant 4.5 percent lower in the post-period (col. 7), whereas poverty

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[24] Trimming the top and bottom 1 percent of incomes somewhat reduces the magnitude of the estimate at year +6 but has a negligible effect on event years 4 and 5. One exception is event year −5 in 1970, where one child in a very small county came from an extremely affluent family. This single observation inflated the estimate at event year −5 by 30 percent in the full sample over the trimmed sample. Figure 2 suppresses this outlier from the presentation. See appendix tables A4 for the full set of estimates.

rates for white children born in the post-period were an imprecise 1.8 percent lower (col. 4). For nonwhites, the effects of family planning programs are also generally stronger at the lower end in the income distribution. The relative reduction in the share of children in poverty and below 150 percent of the poverty line is also larger than the reduction in the share of children below twice the poverty line, although these estimates are not statistically different.

The selection effect suggests that this reduction in child poverty could occur mechanistically by family planning reducing the number of children in the household (which would lower the poverty threshold), even if household incomes did not change (i.e., the empowerment effect is zero).[25] We, therefore, repeat our analysis by subtracting out the younger siblings that arrived after each child was born and then reconstruct poverty thresholds for each child at the time of birth. Notably, this lower threshold generates even larger reductions in poverty rates, suggesting that the eventual arrival of younger siblings worked to mitigate the effects of family planning. This is suggestive evidence that both the selection (the reduction in childbearing) and empowerment effects (the delay of childbearing and increase in household incomes) play an important role in explaining these results. We develop a direct approach to bounding the role of selection in the next section.

The introduction of family planning programs was also associated with reductions in the share of children living with public assistance recipients. Figure 4 shows that cohorts born 4 to 6 years after family planning programs began were on average 11.4 percent less likely to live in households receiving public assistance (table 4, col. 1) relative to those born just before family planning programs began. This effect appears largely driven by reductions among whites. White children born 4 to 6 years after family planning programs began were 10.9 percent less likely to live in households receiving public assistance (table 4, col. 2). The event-study estimates for nonwhite children are noisier, and the differences-in-differences estimate is relatively smaller in magnitude and imprecisely estimated (table 4, col. 3).

Another hypothesis of interest is whether family planning programs affected the share of children living with single household heads. Non-marital childbearing and single headship rose dramatically in the

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[25] Consider a family of two earning $18,000 in 2013: they would *not* fall below the federal poverty line of $15,500. But, having a child at the same annual income would put the same family below the federal poverty threshold of $19,530 for a family of three.

1960s and 1970s and both trends have served to increase child poverty over the longer-term. Some theoretical arguments suggest that family planning programs may have encouraged these trends by reducing the cost of sex between less committed individuals and, therefore, increased non-marital childbearing (Akerlof et al. 1996). Figure 5, however, shows no evidence that the share of children born to single heads increased in the short term following the introduction of a family planning program. Although the 95-percent confidence intervals in figure 5 (table 4, col. 4) include zero, a one-sided test rejects the hypothesis that the share of children living with single parents increased following the introduction of family planning programs—patterns more precisely estimated for white children than nonwhite children (table 4, cols. 5 and 6). A complementary piece of evidence comes from county-level marriage and divorce counts from the Vital Records. Event-study estimates using marriages and divorces per woman ages 15 to 44 fail to reject that marriage and divorce rates remained the same before and after family planning programs began (appendix figure A5).

Two final analyses help understand which children were most affected by the introduction of family planning programs. Using both the age of mothers at the time of each birth and the number of each child's older siblings (a measure of each child's birth order[26]) as dependent variables, we find that the mother of the average child born 4 to 6 years after family planning programs began was 0.25 years younger, a reduction of 3 months (figure 6 and table 5, col. 1). This finding is a weighted average of the effects of family planning programs on childbearing delay (which should increase the age of the average child's mother) and women ending their childbearing careers sooner (often called stopping, which should decrease the age of the average child). Although these two changes average out to a small negative number, additional analyses suggest that both channels were quantitatively important—albeit in opposing directions.

Although we cannot observe these delays directly, evidence from birth rates suggests they were quantitatively important. Bailey (2012: table 4) reports that after the introduction of family planning programs, birth rates fell by 1.3 per 1000 teens and 2.4 births per thousand women in their early twenties, or 2 percent and 1.4 percent, respectively. In contrast, births per thousand women in their early and late thirties

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[26] We calculate the number of older siblings in the household at the time of each child's birth by subtracting out younger siblings from those present in the household.

fell by 1.1 and 0.6 births per 1000 women. Although these numbers are *relatively* large, comprising

reductions of 1.5 and 1.7 percent for the respective groups, they contribute substantially less to overall birth

rates. Birth rates for women under 25 are more than twice as high as those for women in their thirties.

But stopping childbearing was also important. Children born 4 to 6 years after family planning

programs began had 0.07 fewer older siblings (off of the pre-treatment average of 1.8), suggesting post-

family planning births were lower-birth order (first or second children rather than third or higher) (figure 7,

table 5, col. 4). Evidence using changes in parity-specific birth rates also reinforces this impression. Bailey

(2012: table 4B) shows that family planning programs did not affect first birth rates in the longer term (years

11 to 15), but they did reduce 3rd parity births for up to 15 years after they were established—a result

consistent with family planning programs affecting stopping behavior. Her estimates for 4th and higher order

births are negative but imprecise, largely because fourth-order and higher births occur infrequently.

Changes in the age of the average child's mother, therefore, reflect the average of higher frequency

childbearing delays (e.g., changes in one to two years) averaged together with infrequent but much larger

age reductions due to stopping (e.g., a reduction in the age at last birth from 38 to 30). The net fall in the age

of the average mother means that the latter dominates empirically. An interesting implication of these

changes is that falling average age of mothers should lead to *decreases* in household income—not the

increases we observe in figure 2.

The results thus far say little about how family planning programs affected children's opportunities.

In theory, the entirety of the increase in children's household income may be due to selection, the result of

disadvantaged parents opting out of childbearing rather than any given child experiencing an increase in

income. This section lays out a theoretical framework to bound the role of selection using estimates of family

planning's effects on fertility rates as well as the characteristics of the users of family planning from the 1970

NFS. Assuming only that the children in the lowest tail of the household income distribution selected out of

childbearing allows us to generate an upper bound on the role of selection and, therefore, a lower bound on

the empowerment effect of family planning programs on the resources of children. Alternatively, assuming

that the missing children would have had parents following the empirical distribution of incomes of family

planning users, we characterize "likely" selection. This approach produces our best estimate of the empowerment effects of family planning.

IV. A THEORETICAL FRAMEWORK FOR BOUNDING THE IMPORTANCE OF SELECTION AND EMPOWERMENT

Consider the function, $g(\ )$, relating childbearing to a household characteristic, y, among parents before the introduction of family planning programs, where $0 \leq g(y)$ for all y and $\sum g(y) = N$. For concreteness, we call this y household income in this discussion. The mean of children's household income before family planning programs begin can be written as $\mu \equiv \frac{1}{N}\sum yg(y)$. Let $g^{fp}(\ )$ represent the function of childbearing after the introduction of a family planning program, where $0 \leq g^{fp}(\ )$ and $\sum g^{fp}(\ ) = M$. For instance, if household income was unaffected by family planning, $g^{fp}(y)$ would differ from $g(y)$ only to the extent that parents with lower incomes opt to have fewer children, $M < N$. To capture the effects of family planning through empowerment, let $h(y)$ represent the transformation of household income with the introduction of a family planning program. Mean household income among children born after family planning programs begin can, therefore, be written as $\mu^{fp} = \frac{1}{M}\sum h(y)g^{fp}(h(y))$.

Our analysis implicitly assumes that cohorts born just before family planning programs allow us to estimate the pre-family planning mean, $\mu \equiv \frac{1}{N}\sum yg(y)$, whereas cohorts born just after family planning programs began allow us to estimate $\mu^{fp} = \frac{1}{M}\sum h(y)g^{fp}(h(y))$. The following difference between these means captures the effect of family planning programs on the household income of the average child:

$$(3) \qquad \tau^{fp} \equiv \mu^{fp} - \mu = \sum\left(\frac{1}{M}h(y)g^{fp}(h(y)) - \frac{1}{N}yg(y)\right).$$

Rewriting equation by adding and subtracting a cross-term, this treatment effect can be decomposed into a selection and empowerment effect,[27]

$$(4) \quad \tau^{fp} = \underbrace{\frac{1}{M}\sum h(y)\left[(g^{fp}(h(y)) - \frac{M}{N}g(h(y))\right]}_{selection\ effect} + \underbrace{\frac{1}{N}\sum g(y)[h(y) - y] + h(y)[g(h(y)) - g(y)]}_{empowerment\ effect}.$$

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[27] The second line of the equation is obtained by adding and subtracting $\frac{1}{N}g(y)h(y)$ and $\frac{1}{N}g(h(y))h(y)$. As is standard in decompositions, the weighting of the selection and empowerment effects will vary with the choice of cross-terms.

Holding household income fixed ($h(y) = y$), the second part of the expression (the empowerment effect) and drops out and the treatment effect is due solely to selection: the change in the relationship between childbearing and household income following the introduction of family planning. Alternatively, holding constant the relationship between childbearing and income (M=N and $g^{fp}(\ )=g(\ )$), the selection term drops out and the treatment effect is solely due to changes in household income.

Our event-study analysis quantifies the treatment effect of family planning by taking the difference between the outcomes of children born just before and just after family planning programs began using retrospective data. The implicit assumption is that cohorts born just before family planning programs began allow us to estimate the pre-family planning mean, $\mu \equiv \frac{1}{N} \sum y g(y)$, whereas cohorts born just after family planning programs began allow us to estimate the post-family planning mean, $\mu^{fp} = \frac{1}{M} \sum h(y) g^{fp}(h(y))$. To the extent that older children were also treated and, therefore, are used to estimate the pre-family planning program mean, $\mu \equiv \frac{1}{N} \sum y g(y)$, the difference between the two means will *understate* the treatment effect of family planning on children's outcomes if children born before family planning programs began also benefitted (e.g., through intra-household spillovers). We cannot address this problem directly. Instead, we assume this understatement is relatively proportioned between the selection and empowerment effect and focus on bounding the role of selection in the sections that follow.

The quantity of interest is the empowerment effect, the effect of family planning on children's opportunities. Empirically isolating the effect of empowerment, however, requires either longitudinal data or further assumptions on $g^{fp}(\ )$ and $h(\ )$ for which little empirical evidence exists. Because available longitudinal datasets are too small to be used with our research design, we impose structure on selection, or $g^{fp}(\ )$, and quantify the role of empowerment as the difference between the total effect, τ^{fp}, and the simulated effects of selection.

A. Selection by Lower Truncation

The most extreme case of negative selection is generated by lower truncation. We first focus on the case in which only the *poorest* parents use family planning programs to avert births—a form of selection

which should have the largest effect on the household income of the average child. The effect of family planning under lower truncation can be written as

$$\text{(4)} \qquad \tau^T \equiv \mu^T - \mu = \frac{1}{M}\Sigma_{y>T}\, yg(y) - \frac{1}{N}\Sigma\, yg(y).$$

The first term in the equation, μ^T, is the household income of the average child after removing all children below income T, the truncation point.

To simulate the effects of lower truncation, we use the empirical distribution of children's household incomes from the 1960 census. In order to parallel our discussion of event-study estimates at 4 to 6 years, we restrict our sample to the average age of children born 4 to 6 years after family planning programs started in our 1980 sample (ages 5 to 7). We then remove the poorest 2 percent of children from this distribution so that the implied percent decrease in the share of missing children corresponds to with Bailey's (2012) estimates of the effect of family planning programs on fertility rates.[28] This results in truncation of children with household incomes on average below $5,049 in 2013 dollars annually in the 1959 distribution of children's household income.[29]

B. Likely Selection

An alternative approach is to model selection based on the observed household income of family planning patients in the 1970 NFS. In this case treatment effect arises only through changes in the distribution of childbearing by household income which can be written,

$$\text{(5)} \qquad \tau^E \equiv \mu^E - \mu = \frac{1}{M}\sum y \left[g^{fp}(y) - \frac{M}{N}g(y) \right].$$

Because family planning programs were disproportionately used by lower income women during this time, we expect the treatment effect under "likely" selection to be positive though not as large as predicted under the extreme case of selection by truncation.

To simulate the likely effects of selection on the outcomes of the average child, we use the household income categories reported by family planning users in the 1970 NFS and approximate their

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[28] These estimates correspond to Bailey (2012)'s weighted estimates averaged over event years 4 to 6 in figure 6A or unweighted estimates, model 3, in figure 6B.
[29] This average 2.6th centile value is $7,447 for the analysis where we only look at observations with household incomes above zero. Each 2.6th percentile household income value was different for every county.

empirical distribution.[30] The empirical distribution of family planning users is well approximated by a normal distribution centered at the poverty line for a family of four ($23,636 annually in 2013 dollars in the 1960 census) and a standard deviation corresponding to one quarter of the standard deviation in the household incomes of all children ages 5 to 7. We, then, remove children from the 1960 census so that the household income of the missing children corresponds to the household income distribution of users of family planning programs. The result is that 81.4 percent of averted children come from households with incomes below 150 percent of the poverty line, which is also similar to the poverty rate of family planning program users in administrative data (83 percent).

C. The Empowerment Effects of Family Planning on Children's Household Income

Table 6 presents the results of these simulations. Column 1 presents the simulated effects of selection under different assumptions; column 2 presents the resulting simulated effect on empowerment under different assumptions; and column 3 presents the share of the total effect due to empowerment under different assumptions. The first row of the table assumes the selection effect is zero and, therefore, that empowerment explains the entirety of the estimated effect of family planning on children's household income. The value of column (2) in this case, is the average of the estimated effects of family planning programs on household income in event years 4 to 6 (column 4 of table 2), which is an increase of 2.5 percent. We choose this value because it is more directly comparable with the fertility effects in Bailey (2012), which does not suffer from the same attenuation due to mobility-induced measurement error. The second and third rows simulate the effects under lower truncation, where the empowerment effect represents the difference between the overall treatment effect (column 2, row 1) and the simulated selection effect. The 95-percent intervals for these estimates are generated using a non-parametric bootstrap procedure and presented in brackets (Johnston and DiNardo 1997).[31]

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[30] Income is reported categorically in the 1970 NFS. To smooth this distribution, we fit a normal distribution to match the moments similar to the categorical distribution

[31] We generate 10,000,000 bootstrap draws of the pre- and simulated post-program household incomes of the average child who is age 5 to 7 using the 1960 census and take their difference. The resulting 2.5th and 97.5th percentiles of the 10,000,000 values of the difference represent the 95-percent confidence interval for the selection effect.

The results show that the most extreme form of selection fails to explain the entire estimated increase in household income. Selection by truncation generates a 2.09 (column 1, row 2) percent increase in household income, which explains around 84.6 percent (column 2, row 1) of the increase estimated using our event-study regressions.. The more likely form of empirical selection explains around 1 point of the effect, or 40 percent of the increase in household income. In short, 15 to 60 percent of the overall increase in children's household income can be directly attributed to the empowerment channel, the resulting changes in parents' human capital, work, and partnership decisions.[32]

V.   DISCUSSION AND CONCLUSIONS

Using a new research design and large, restricted-use census samples, this paper quantifies the effects of the earliest family planning programs on children's economic resources and living circumstances. Our comparison of children in the same county born before and after the introduction of family planning programs suggests that affected cohorts were economically better off. Cohorts born four to six years after family planning programs began lived in households earning 2.5 percent more annually. The largest gains in household income accrued to the most disadvantaged families. Children born four to six years after family planning programs began were 7 percent less likely to live in poverty and 11 percent less likely to live in households receiving public assistance. These results may understate the broader effects of family planning programs on children's economic resources to the extent that older siblings (born before family planning programs began and part of our comparison group) also benefitted or measurement error due to mobility attenuates these estimates.

A second key finding is that much of this income effect was likely achieved through the *direct* impact of family planning on disadvantaged parents, who delayed or constrained childbearing to make investments in their own human capital and partnerships. Our bounding exercise shows that the most extreme form of selection—truncating the poorest 2.0 percent of children—implies an increase of 2.09 percent of the estimated 2.5 percent increase in children's mean household income. This upper bound on the role of selection implies a lower bound on the role of the empowerment channel of around 15 percent of the

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[32] The results are very similar if we restrict household incomes to be greater than zero.

gains in children's household income. In the more likely and interesting case of empirically based selection, the direct effect of family planning on parents' human capital and partnership decisions accounts for around 60 percent of these gains.

Simple cost-benefit calculations permit comparisons of family planning programs with other public policies aiming to increase the resources of disadvantaged children. In the 1960s the federal government spent an average of around $278 million per year (2013 dollars) on family planning, or $4.4 billion cumulating over 1964 to 1980 (the period considered in this analysis). Combined with our most conservative estimates of the empowerment effect, this implies that family planning *directly* reduced child poverty by 1 percent lower for every $4.19 billion spent on family planning ($4.4 billion/7*0.15, where 7 is the percent reduction in child poverty and 0.15 is the share of each point attributable to the empowerment effect). Using our estimates of empirically based selection, our estimates imply that family planning programs directly reduced child poverty by 1 percent lower for every $1.05 billion spent on that family planning ($4.4 billion/7*0.60, where 7 is the percent reduction in child poverty and 0.60 is the share of each point attributable to the empowerment effect).

Comparisons of the empowerment effects of family planning to other programs targeting child poverty place these estimates in perspective.[33] According to the supplemental poverty measure (SPM) for 2012, TANF cost $10.24 billion in 2013 dollars and reduced child poverty rates by 2.7 percent (from 18.5 to 18.0 percent). Ignoring offsetting behavioral changes and deadweight loss implies that TANF reduced child poverty by 1 percent for every $3.8 billion spent. Another useful comparison is the EITC. In 2012, the EITC cost around $63 billion, and the 2012 SPM suggests that EITC and the refundable portion of the child tax credit reduced child poverty rates by 27 percent (from 24.7 to 18.0 percent). A similar calculation implies that EITC reduced child poverty by 1 percent for every $2.3 billion spent. In short, ignoring the effect of family planning programs on public assistance outlays and through the selection channel, the program's empowerment effects via parents' human capital investments (e.g., in their educations or careers), labor-force

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[33] Accounting for behavioral changes, the effects of TANF may have been smaller and the effects of EITC larger (Hoynes, Miller and Simon 2015).

participation (especially of mothers), and partnerships (e.g., better selection of and more stable unions) likely reduce child poverty at around half of the cost of the EITC and one third the cost of TANF.

Family planning programs may have longer-run implications as well (Cunha and Heckman 2007, Almond and Currie 2011). For instance, Dahl and Lochner (2012) use variation in EITC eligibility over time and find a 4 to 6 percent of a standard deviation improvement in children's test scores for each $1,000 of additional income. Milligan and Stabile's (2011) study of Canada's child benefit programs and Chetty, Friedman and Rockoff's (2011) study of U.S. tax credits find comparable estimates. Aizer, Eli, Ferrie, and Lleras-Muney (2014) show that children receiving a 12 to 25 percent increase in household income through the mother's pension program in the early twentieth century went on to attain about 0.4 years more schooling, had healthier weights in adulthood, earned about 14 percent more as adults, and lived about one year longer. Consistent with this, Bailey (2013) provides suggestive evidence from public census data that cohorts born after family planning programs began were 2 percent more likely to attain 16 or more years of education and had 1 percent higher family incomes as adults (see also Schultz 2008 and 2009 for evidence from developing countries). Future work should investigate these longer-run linkages as well as the intergenerational impact of family planning programs on the economy.

VI.   REFERENCES

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Figure 1. The Roll-Out of Federally Funded Family Planning Programs, 1965-1973

1965 - 1967
1968 - 1969
1970 - 1973
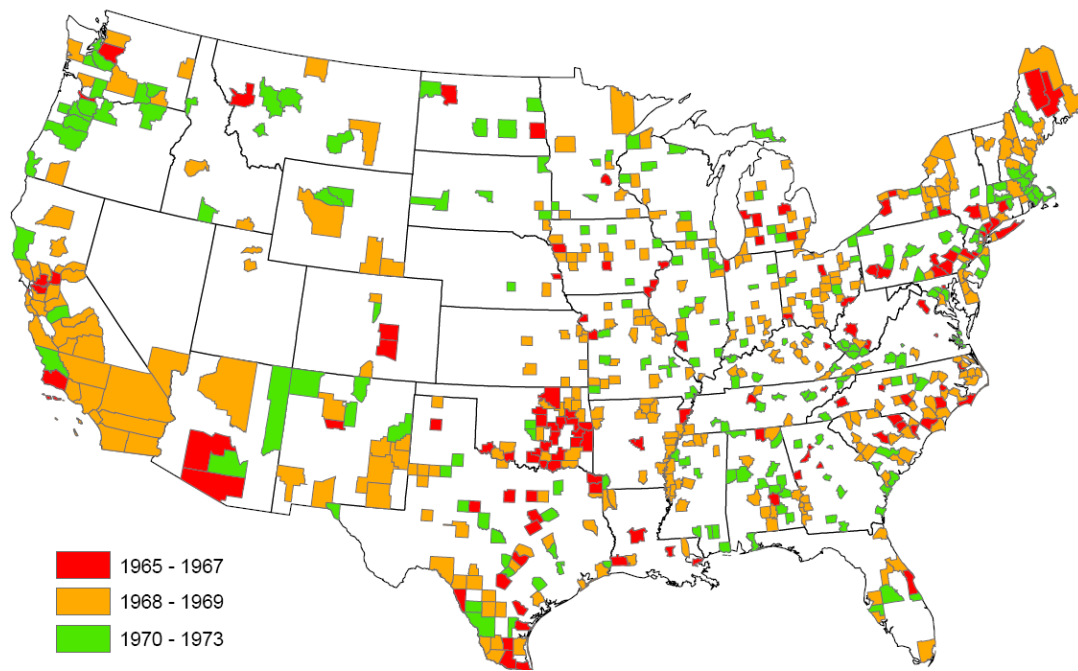
Dates are the year that the county first received a federal family planning grant. Counties not receiving a family planning grant between 1965 and 1973 are not shaded. Sources: NACAP, NAFO and OEO (1969, 1971 and 1974).

Figure 2. Percent Change in Children's Household Income for Cohorts Born Before and After Family Planning Programs Began

Adjusted for Misclassification Error[a]
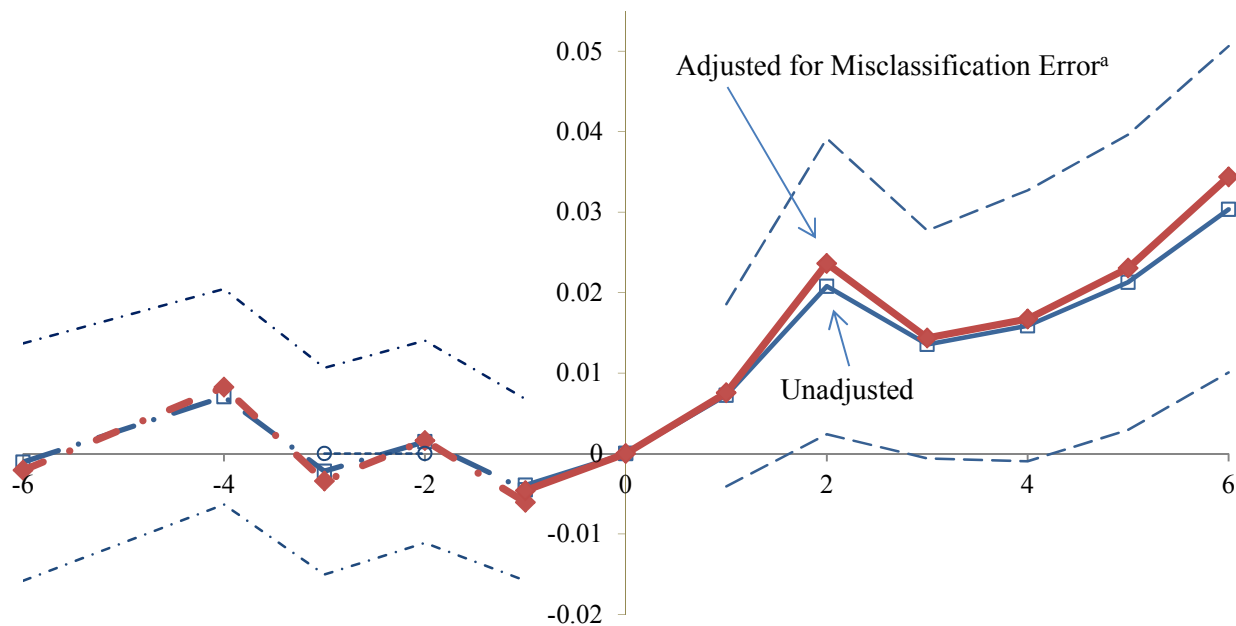
Unadjusted

Panels plot of τ from equation 1 for model 2, both unadjusted and adjusted for misclassification error, divided by the average dependent variable for cohorts in the same county born before family planning programs began in the 1980 census. [a] Adjustment for misclassification error indicates that eq. 2 has been used to adjust the estimates as described in text. Standard errors have been clustered by county and used to construct 95-percent, point-wise confidence intervals for the baseline model (dashed lines). Sources: 1970 (dashed lines with markers) and 1980 (solid lines with markers) restricted-use censuses. See table 2 for estimates.

**Figure 3. Percent Change in Children Living in Poverty
for Cohorts Born Before and After Family Planning Programs Began**

A. Below Poverty Line

B. Below 150% of the Poverty Line

C. Below 200% of the Poverty Line
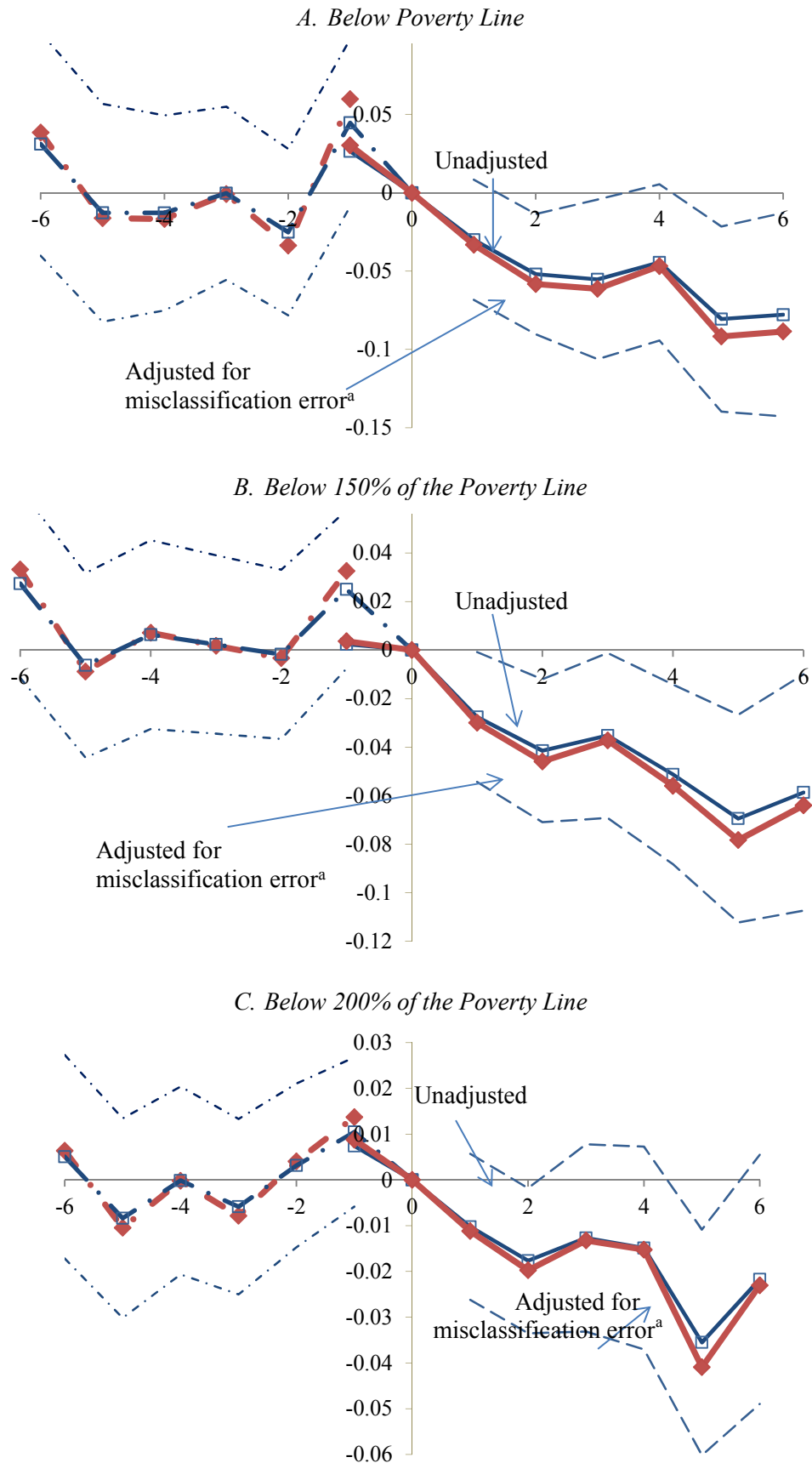
See notes for figure 2 and estimates in table 3.

Figure 4. Percent Change in Children Living in Households Receiving Public Assistance for Cohorts Born Before and After Family Planning Programs Began
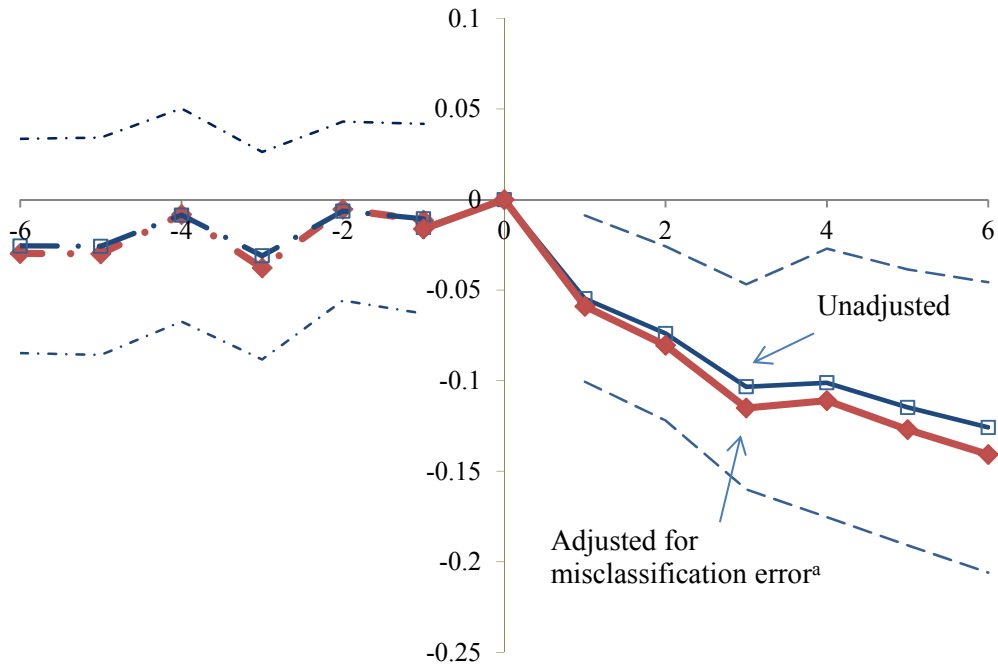
See notes for figure 2 and estimates in table 4.

Figure 5. Percent Change in Children Living in Single -Parent Households for Cohorts Born Before and After Family Planning Programs Began
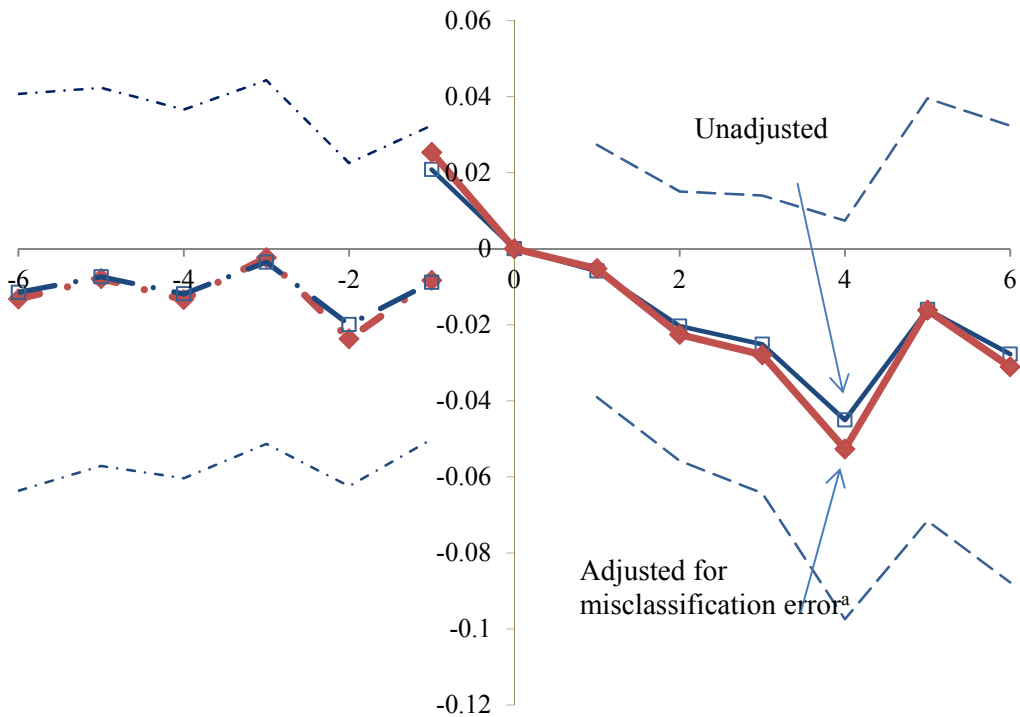
See notes for figure 2 and estimates in table 4.

**Figure 6. Average Age of Mother for Cohorts Born
before and after Family Planning Programs Began**
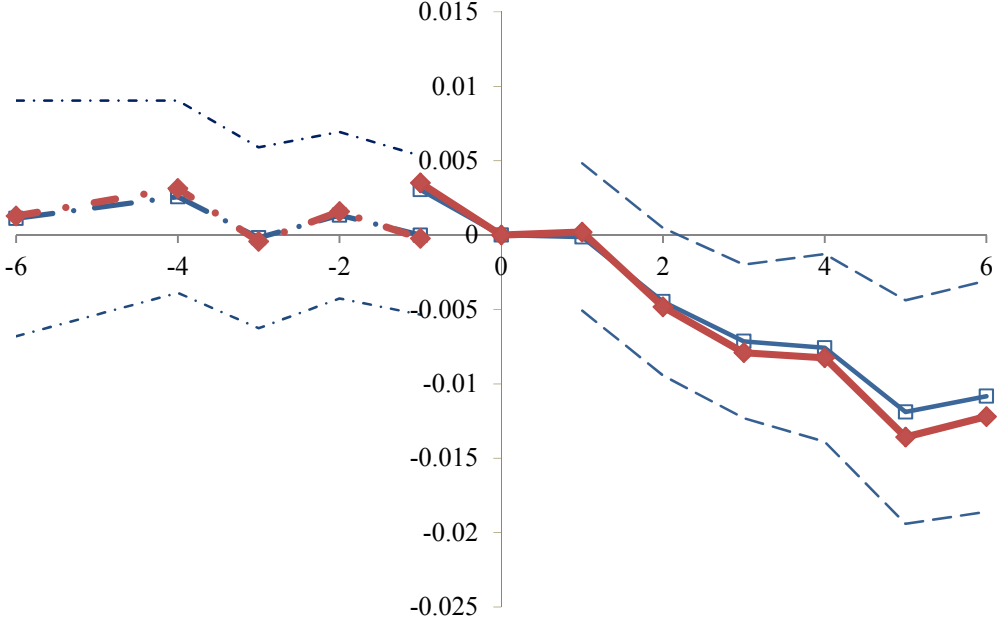
**Figure 7. Average Number of Older Siblings for Cohorts Born
before and after Family Planning Programs Began**
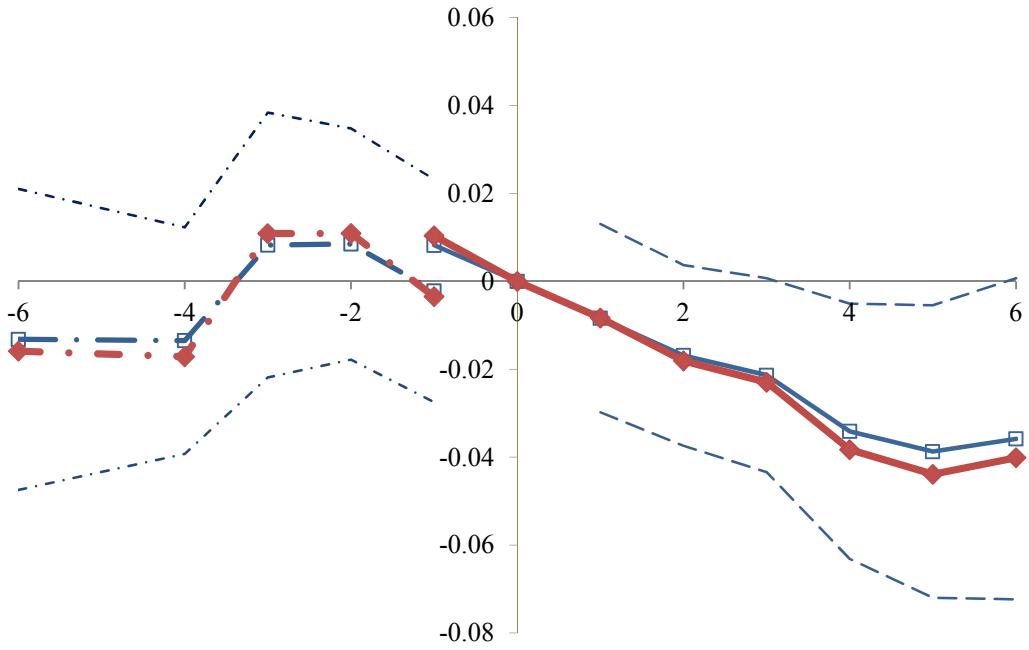
See notes for figure 2 and estimates in table 5.

Table 1. The Use of Family Planning Services Before and After Federal Planning Programs Began

	(1)	(2)	(3)
	A. Dependent Variable: Share of Medically Indigent Patients Using Family Planning Services (1968 Mean=0.046)		
After family planning began	0.027	0.028	0.027
	[0.011]	[0.011]	[0.012]
R-squared	0.71	0.75	0.75
Counties	666	666	666
Observations	1998	1998	1998
Covariates	C,Y	C,S-Y	C,S-Y,R,X
	B. Dependent Variable: 1=Ever Used the Pill (Pre-treatment Mean=0.56)		
After family planning began	0.040	-0.005	-0.007
	[0.024]	[0.023]	[0.020]
In Poverty	-0.165	-0.160	-0.179
(Mean DV=0.58)	[0.084]	[0.075]	[0.073]
After family planning began ×	0.166	0.132	0.146
In Poverty *(Mean DV=0.65)*	[0.076]	[0.070]	[0.076]
Pseudo R-squared	0.026	0.157	0.165
Observations	3699	3699	3681
State fixed effects	X	X	X
Other covariates		A,C,E,P	A,C,E,P,K

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

Table 2. Changes in Children's Household Income for Cohorts Born After Family Planning Programs Began

	(1)	(2)	(3)	(4)
	Dependent Variable: Household Income			
	Pre-Treatment Mean in 1980[a]: $68,417			
A. Difference-in-differences estimates [b]				
Pooled event years 1 - 6	570.4	767.2	716.4	851.6
	[343.7]	[408.3]	[410.9]	[451.7]
R-squared	0.34	0.41	0.41	0.41
B. Event-study estimates [c]				
-2	224.5	136.9	175.1	189.0
	[484.5]	[549.7]	[547.0]	[583.4]
-1	-121.6	-269.0	-266.8	-315.4
	[401.0]	[417.5]	[415.7]	[434.9]
0 (omitted)				
1	414.6	495.3	454.1	516.5
	[352.8]	[395.3]	[400.6]	[406.7]
2	1263.0	1422.0	1353.0	1615.8
	[588.0]	[641.9]	[648.7]	[685.2]
3	779.4	927.5	833.8	983.0
	[469.0]	[494.3]	[506.4]	[511.5]
4	1095.0	1086.0	979.8	1144.0
	[546.6]	[587.4]	[618.9]	[620.5]
5	1479.0	1456.0	1321.0	1576.4
	[588.1]	[640.1]	[671.8]	[685.0]
6	2193.0	2076.0	1935.0	2352.5
	[714.4]	[707.8]	[743.7]	[769.7]
R-squared	0.339	0.405	0.407	0.405
Model [d]	1	2	3	2M
Covariates [e]	C, Y	C, Y, S-Y	C, S-Y, R, A	C, Y, S-Y, adjusted using eqn. 5
County-year cells	11313	11313	11313	11313
Counties	666	666	666	666

The table presents point estimates of the change in household income of children for cohorts born before and after family planning programs began (event year 0). See appendix table A4 for 1970 point estimates. Heteroskedasticity-robust standard errors clustered by county are presented in brackets. See appendix tables for 1970 point estimates.

[a] Pre-treatment mean in 1980 is calculated as the mean of the dependent variable in event years t=0, t=−1 and t=−2 in 2013 dollars.

[b] Coefficients are least-squares estimates of τ in equation 1 using the 1980 restricted-use census data.

[c] Our baseline model is model 2. Event study estimates from models 2 and 2M are plotted in figure 2.

[d] Covariate abbreviations are as follows: C and Y denote county and year fixed effects. S-Y denotes state-by-year fixed effects. X, R and A indicate county-level covariates interacted with linear time trends, REIS variables and abortion access measures (see text).

Source: Authors' calculations using the 1980 restricted-use census data.

Table 3. Percent of Children In or Near Poverty among Cohorts Born After Family Planning Programs Began

Dependent Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Percent of Children below			Percent White Children below			Percent Non-white Children below		
	100% Poverty	150% Poverty	200% Poverty	100% Poverty	150% Poverty	200% Poverty	100% Poverty	150% Poverty	200% Poverty
Pre-treatment Mean[a]	*17.4*	*28.5*	*40.4*	*15.8*	*30.8*	*48.1*	*44.0*	*63.4*	*76.7*
A. Difference-in-differences estimates[b]									
Event years 1 to 6	-0.846	-0.872	-0.701	-0.284	-0.439	-0.251	-1.994	-1.307	-0.986
	[0.258]	[0.323]	[0.346]	[0.285]	[0.369]	[0.407]	[0.809]	[0.860]	[0.905]
R-squared	0.09	0.13	0.19	0.08	0.11	0.16	0.11	0.11	0.13
B. Event-study estimates[c]									
-2	0.737	0.175	0.139	0.540	0.034	-0.025	0.946	0.020	0.073
	[0.434]	[0.389]	[0.410]	[0.485]	[0.435]	[0.521]	[1.093]	[1.083]	[1.182]
-1	0.459	0.067	0.381	0.545	0.219	0.679	0.779	-0.305	-0.005
	[0.366]	[0.391]	[0.406]	[0.406]	[0.476]	[0.513]	[1.208]	[1.264]	[1.256]
0 omitted									
1	-0.522	-0.786	-0.523	0.177	-0.236	-0.070	-0.993	-1.325	-0.242
	[0.340]	[0.389]	[0.437]	[0.360]	[0.506]	[0.567]	[1.194]	[1.058]	[0.963]
2	-0.903	-1.182	-0.918	-0.391	-0.826	-0.256	-2.148	-0.795	-1.111
	[0.340]	[0.428]	[0.432]	[0.334]	[0.488]	[0.522]	[1.288]	[1.164]	[1.171]
3	-0.961	-1.003	-0.651	-0.382	-0.399	0.088	-2.704	-2.133	-2.026
	[0.452]	[0.494]	[0.558]	[0.453]	[0.543]	[0.634]	[1.168]	[1.236]	[1.242]
4	-0.773	-1.460	-0.755	0.015	-0.721	0.071	-3.007	-2.585	-0.619
	[0.442]	[0.538]	[0.603]	[0.455]	[0.624]	[0.733]	[1.385]	[1.549]	[1.624]
5	-1.401	-1.979	-1.860	-0.819	-1.468	-0.950	-1.582	-1.272	-1.509
	[0.524]	[0.622]	[0.668]	[0.544]	[0.679]	[0.735]	[1.566]	[1.633]	[1.816]
6	-1.353	-1.672	-1.124	-0.227	-0.855	-0.288	-2.195	-0.694	-0.999
	[0.576]	[0.708]	[0.732]	[0.603]	[0.736]	[0.798]	[1.664]	[1.810]	[1.992]
R-squared	0.092	0.130	0.191	0.080	0.110	0.163	0.111	0.114	0.126

See notes for table 2. All estimates are for model 2. In columns 1-6, the number of observations and counties are identical to table 2. For columns 7-9, only 529 counties have sufficient numbers of nonwhites for inclusion. These estimates are based on 8855 county-year observations.

Table 4. Percent of Children in Households Receiving Public Assistance or Headed by Single Parents for Cohorts Born After Family Planning Programs Began

Dependent Variables	(1)	(2)	(3)	(4)	(5)	(6)
	Percent Children in Households Receiving Public Assistance			Percent Children in Single Parent Households		
Sample	All	White	Non-white	All	White	Non-white
Pre-treatment mean [a]	*11.7*	*7.9*	*25.7*	*17.4*	*7.94*	*25.7*
A. Difference-in-differences estimates [b]						
Event years 1 to 6	-0.643	-0.446	-0.426	-0.290	-0.044	0.108
	[0.212]	[0.189]	[0.740]	[0.269]	[0.252]	[0.935]
R-squared	0.10	0.08	0.12	0.10	0.12	0.11
B. Event-study estimates [c]						
-2	-0.052	-0.046	-0.048	-0.438	-0.586	1.322
	[0.287]	[0.283]	[1.020]	[0.340]	[0.389]	[1.198]
-1	-0.180	-0.016	0.343	0.363	-0.109	1.588
	[0.269]	[0.241]	[1.218]	[0.358]	[0.385]	[1.261]
0 omitted						
1	-0.638	-0.455	0.496	-0.102	-0.092	1.763
	[0.273]	[0.241]	[1.008]	[0.295]	[0.275]	[1.310]
2	-0.862	-0.554	-0.648	-0.355	-0.282	0.457
	[0.286]	[0.264]	[1.005]	[0.315]	[0.345]	[1.338]
3	-1.206	-0.738	-1.731	-0.438	-0.009	-1.300
	[0.337]	[0.309]	[1.077]	[0.348]	[0.376]	[1.405]
4	-1.180	-0.687	0.274	-0.785	-0.492	-0.270
	[0.441]	[0.343]	[1.384]	[0.466]	[0.449]	[1.546]
5	-1.338	-0.805	-0.344	-0.279	-0.172	1.696
	[0.453]	[0.407]	[1.503]	[0.494]	[0.513]	[1.724]
6	-1.467	-1.087	0.776	-0.483	-0.143	0.753
	[0.477]	[0.473]	[1.666]	[0.534]	[0.563]	[1.841]
R-squared	0.102	0.084	0.122	0.095	0.123	0.115

See notes for table 2 and 3 and the text for variable definitions.

Table 5. Mother's Age at Birth and Number of Older Siblings among Cohorts Born After Family Planning Programs Began

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variables	Mother's Age at Birth			Mean Number of Older Siblings		
Sample	All	White	Non-white	All	White	Non-white
Pre-treatment mean [a]	*25.5*	*25.4*	*25.3*	*1.8*	*1.6*	*2.4*
A. Difference-in-differences estimates [b]						
Event years 1 to 6	-0.066	-0.041	0.056	-0.021	0.001	-0.003
	[0.045]	[0.049]	[0.144]	[0.014]	[0.014]	[0.045]
R-squared	0.20	0.14	0.16	0.54	0.45	0.32
B. Event-study estimates [c]						
-2	0.045	0.083	0.206	-0.019	-0.015	0.049
	[0.0602]	[0.0684]	[0.181]	[0.0234]	[0.0233]	[0.0635]
-1	0.078	0.070	0.526	0.015	0.009	0.088
	[0.0615]	[0.0678]	[0.177]	[0.0215]	[0.0208]	[0.0580]
0 omitted						
1	-0.003	0.010	0.335	-0.015	0.005	0.042
	[0.0644]	[0.0676]	[0.202]	[0.0197]	[0.0205]	[0.0560]
2	-0.114	-0.047	0.000	-0.030	-0.001	-0.015
	[0.0645]	[0.0646]	[0.191]	[0.0189]	[0.0181]	[0.0480]
3	-0.182	-0.132	-0.099	-0.039	-0.008	-0.038
	[0.0671]	[0.0750]	[0.221]	[0.0203]	[0.0184]	[0.0559]
4	-0.193	-0.140	0.010	-0.062	-0.012	-0.016
	[0.0820]	[0.0825]	[0.263]	[0.0267]	[0.0236]	[0.0783]
5	-0.303	-0.220	-0.252	-0.070	-0.015	-0.126
	[0.0977]	[0.0949]	[0.285]	[0.0306]	[0.0267]	[0.0775]
6	-0.276	-0.173	-0.497	-0.065	-0.011	-0.120
	[0.101]	[0.101]	[0.285]	[0.0336]	[0.0277]	[0.0834]
R-squared	0.203	0.140	0.159	0.541	0.455	0.326

See notes for table 2 and 3 and the text for variable definitions.

Table 6. Simulated Effects of Family Planning on Children's Household Income under Different Assumptions about Selection

	(1) Simulated Selection Effect	(2) Simulated Empowerment Effect	(3) Share due to Empowerment
No selection	--	2.47 [1.32, 3.62]	100%
Selection by lower truncation (lower bound on empowerment)	2.09 [1.53, 2.66]	0.38	15.4%
Empirically approximated selection	0.99 [0.42, 1.55]	1.48	60.0%

The simulated empowerment effect under no selection corresponds to the magnitude of the average estimate for event-years 4 to 6 from table 2, column 4. The simulation analysis uses household income observed in the 1960 census for the sample of children who are ages 5 to 7. For selection by truncation, we drop the poorest 2.0 percent of children using Bailey's (2012) estimates of the percent of births averted due to family planning (see figure 6A, models 1-3 or figure 6B, model 3). For likely selection, we drop 2.0 percent of children assuming the household incomes of their parents are normally distributed with the mean at $3000 (the poverty line for a family of four) and a standard deviation equal to one quarter of the standard deviation in the household incomes of all children ages 5 to 7 in 1960. The decomposition uses the cross-term in equation 4. The upper and lower 95 percent confidence intervals are below each effect in brackets. In row 1 the confidence interval is calculated using the standard error of the estimate. In rows 2 and 3 the confidence interval is calculated using the parametric bootstrap (Johnston and DiNardo 1997). Source: Authors calculations using 1960 restricted-use census data.

ONLINE APPENDIX:
SUPPLEMENTARY ANALYSES AND ADDITIONAL ESTIMATES FOR THE PAPER

DOES FAMILY PLANNING INCREASE CHILDREN'S OPPORTUNITIES?
EVIDENCE FROM THE WAR ON POVERTY AND THE EARLY YEARS OF TITLE X

by
Martha J. Bailey, Olga Malkova, and Zoë M. McLaren

December 30, 2015

Table A1. Characteristics of Counties Receiving Federal Family Planning Programs from 1965 to 1973

	(1) All Counties	(2) Not Funded	(3) First Funded in years			(4) All Funded	(5) Col 4-Col 2 (std. error)
			1965-7	1968-9	1970-3		
Number of counties	3073	2407	124	337	205	666	
Mean population	55,385	30,735	239,319	138,895	96,271	144,473	113,738
							(12,721)
Percent of 1960 population in counties							
in Northeast	21.7	16.4	31.5	16.7	38.5	25.7	9.4
							(4.91)
in Midwest	30.3	38.2	20.8	27.1	22.9	24.3	-13.9
							(6.18)
in South	31.8	37.7	29.5	25.2	29.1	27.3	-10.4
							(4.84)
in West	16.0	7.4	18.2	30.8	9.5	22.6	15.2
							(5.62)
in urban areas	68.4	53.7	85.7	79.5	71.6	79.8	26.1
							(4.13)
in rural/farm areas	7.9	13.3	2.2	4.2	5.1	3.7	-9.5
							(1.19)
Percent of 1960 county residents							
Under 5 years of age	11.4	11.3	11.4	11.6	11.5	11.5	0.22
							(0.12)
65 or older	9.2	9.8	8.9	8.6	9.0	8.8	-0.97
							(0.20)
Nonwhite	11.0	11.4	13.0	10.6	7.9	10.8	-0.55
							(1.04)
with 12 years of education	42.7	41.0	43.2	44.9	42.8	43.9	2.9
							(3.83)
with fewer than 4 years of education	8.4	9.6	7.9	7.1	7.9	7.5	-2.1
							(0.40)
of households with income under $3,000	22.0	27.0	17.4	18.1	19.0	18.1	-8.9
							(1.43)
of households with income over $10,000	14.7	11.7	17.4	17.6	15.3	17.1	5.3
							(1.25)

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 A2. Correlates of the Timing of Federal Family Planning Program Establishment

	(1)	(2)	(3)	(4)
	Dependent Variable: *Year of first federal family planning grant*			
1(25 to 49 percent of population in urban areas)	0.55	0.69	0.74	0.86
	[0.29]	[0.32]	[0.43]	[0.46]
1(50 to 74 percent of population in urban areas)	0.78	0.89	1.1	0.81
	[0.53]	[0.59]	[0.76]	[0.76]
1(75 to 100 percent of population in urban areas)	0.48	0.57	1.28	0.65
	[0.70]	[0.77]	[1.00]	[1.03]
Proportion of residents				
in urban areas	-0.02	-0.02	-0.04	-0.03
	[0.01]	[0.01]	[0.02]	[0.01]
in rural or farm areas	0.01	-0.00	0.00	0.00
	[0.01]	[0.01]	[0.02]	[0.02]
under 5 years of age	0.06	-0.03	-0.03	-0.07
	[0.09]	[0.07]	[0.13]	[0.16]
65 or older	-0.03	-0.07	-0.07	-0.16
	[0.06]	[0.04]	[0.12]	[0.08]
Nonwhite	-0.01	-0.01	-0.02	-0.02
	[0.01]	[0.01]	[0.01]	[0.02]
with 12 years of education	-0.00	0.02	0.02	0.03
	[0.02]	[0.02]	[0.03]	[0.03]
with less than 4 years of education	-0.01	-0.00	0.02	0.01
	[0.02]	[0.02]	[0.08]	[0.06]
of households with income <$3,000	-0.02	-0.00	-0.02	0.01
	[0.01]	[0.01]	[0.05]	[0.04]
of households with income >$10,000	-0.01	-0.03	-0.00	0.01
	[0.03]	[0.03]	[0.04]	[0.04]
Weighted by population of women 15 to 44			X	X
State fixed effects		X		X
Observations	666	666	666	666
R-squared	0.07	0.26	0.13	0.38

Each column reports estimates from a separate linear regression. Heteroskedasticity-robust standard errors are corrected for correlation within state and presented in brackets beneath each estimate. Sources: See table A1.

Table A3. The Relationship between 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	*0.80*	*3.3*	*0.39*	*6.04*	*0.22*	*772*	*0.198*	*5.1*
Year Family Planning	-0.005	0.010	-0.001	0.036	-0.004	0.198	-0.004	-0.054
Program Established	[0.007]	[0.022]	[0.005]	[0.071]	[0.010]	[0.384]	[0.008]	[0.066]
Observations	3,106	3,069	3,106	2,967	3,106	742	3,106	3,101
R-squared	0.021	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 age 14
Mean Dependent Variable	*0.87*	*20.8*	*22.3*	*2.7*	*7620*	*0.29*	*11.3*	*0.78*
Year Family Planning	0.006	0.054	0.063	0.017	50.6	0.023	0.036	0.004
Program Established	[0.005]	[0.059]	[0.066]	[0.031]	[157]	[0.016]	[0.104]	[0.006]
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	0.016

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, 772 = March 1964), (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 A4.1. General Fertility Rate Before and After Family Planning Programs Began

	(1)	(2)	(3)	(4)	(5)
	Dependent Variable: General Fertility Rate				
A. 1970 Census: Mean DV			96.830		
-6	-0.629	0.436	0.434	-0.144	0.508
	[1.618]	[1.654]	[1.653]	[1.649]	[1.784]
-5	-0.302	0.520	0.510	-0.039	0.617
	[1.529]	[1.481]	[1.478]	[1.485]	[1.585]
-4	-1.164	-0.033	-0.052	-0.556	-0.066
	[1.328]	[1.345]	[1.333]	[1.325]	[1.438]
-3	-1.307	0.065	0.011	-0.423	0.089
	[1.188]	[1.150]	[1.135]	[1.109]	[1.218]
-2	0.067	1.156	1.077	0.805	1.585
	[1.075]	[1.117]	[1.105]	[1.101]	[1.203]
-1	-1.085	-0.815	-0.908	-1.008	-1.016
	[1.046]	[1.034]	[1.027]	[1.031]	[1.121]
	Event year zero omitted (Year family planning program began)				
Observations	7324	7324	7324	7324	7324
R-squared	0.336	0.394	0.397	0.418	0.394
B. 1980 Census: Mean DV			85.990		
-2	0.528	0.677	0.496	0.184	0.817
	[0.918]	[0.960]	[0.957]	[0.954]	[0.934]
-1	-0.747	-1.080	-1.179	-1.335	-1.199
	[0.927]	[0.937]	[0.940]	[0.935]	[0.925]
	Event year zero omitted (Year family planning program began)				
1	-1.615	-1.689	-1.577	-1.445	-1.837
	[0.805]	[0.834]	[0.836]	[0.833]	[0.878]
2	-1.709	-2.268	-2.187	-1.878	-2.499
	[0.817]	[0.901]	[0.900]	[0.897]	[0.912]
3	-2.818	-3.187	-3.122	-2.702	-3.612
	[0.968]	[1.020]	[1.004]	[1.002]	[0.963]
4	-1.890	-2.237	-2.198	-1.663	-2.426
	[1.054]	[1.081]	[1.063]	[1.063]	[0.986]
5	-1.917	-2.799	-2.864	-2.213	-3.132
	[1.218]	[1.272]	[1.250]	[1.253]	[1.053]
6	-2.568	-3.058	-3.215	-2.473	-3.497
	[1.312]	[1.351]	[1.312]	[1.302]	[1.075]
Observations	11313	11313	11313	11313	11313
R-squared	0.223	0.300	0.304	0.313	0.300
For Panels A and B:					
Model	1	2	3	4	2M
Covariates	C, Y	C, Y, S-Y	C, Y, S-Y, R, A	C, Y, S-Y, R, A, Ctrend	C, Y, S-Y, Adjusted for Misclassification
Counties	666	666	666	666	666

See table 2 notes and figure 2 notes in main paper. Point estimates of τ from our various specifications of equation 1. The dependent variable is the general fertility rate (GFR) calculated using the 1970 or 1980 censuses. The census estimates use the GFR implied by the county of residence in 1965/1975 and age of the child in the census from funded counties and do not weight.

Table A4.2 Average Household Income for Children Born Before
Family Planning Programs Began

	(1)	(2)	(3)	(4)	(5)	(6)
			Dependent Variable: Household Income			
A. 1970 Census: Mean DV			*53809*			*53420*
-6	-427.6	-73	-223.3	-234	-141.6	-363.3
	[518.6]	[515.1]	[518.7]	[510.0]	[518.5]	[468.4]
-5	1375	1978	1828	1759	2434.3	544.6
	[1,113]	[1,374]	[1,355]	[1,379]	[1518.5]	[399.9]
-4	102.1	482.6	336.5	218.9	564.1	44.57
	[485.1]	[467.5]	[464.8]	[464.5]	[452.4]	[416.8]
-3	-388.8	-150.6	-284	-425.5	-234	-148.5
	[406.0]	[448.4]	[452.5]	[445.3]	[448.7]	[396.1]
-2	-167.8	100.5	-7	-121.9	110.7	-127.9
	[381.3]	[438.8]	[443.2]	[433.0]	[450.9]	[322.8]
-1	-258.9	-309	-400.8	-449.4	-414.8	-668.7
	[362.8]	[393.9]	[390.1]	[395.2]	[420.9]	[358.2]
	Event year zero omitted (Year family planning program began)					
Observations	7324	7324	7324	7324	7324	7324
R-squared	0.122	0.185	0.188	0.198	0.185	0.429
Model	1	2	3	4	2M	2T
Covariates	C, Y	C, Y, S-Y	C, Y, S-Y, R, A	C, Y, S-Y, R, A, Ctrend	C, Y, S-Y, Adjusted for Misclassification	C, Y, S-Y, Trimmed sample
Counties	666	666	666	666	666	666

See notes in table 3 in main paper and figure 2 for estimates using the 1980 census.

Table A4.3. Average Household Income per Capita for Children

	(1)	(2)	(3)	(4)	(5)
	Dependent Variable: Household Income per Capita				
A. 1970 Census: Mean DV			10977		
-6	5.2	55.5	33.8	24.8	53.7
	[108.9]	[116.5]	[116.7]	[117.3]	[114.9]
-5	388.3	495.0	473.6	455.0	607.2
	[276.4]	[346.1]	[340.6]	[347.4]	[382.8]
-4	71.8	121.5	100.9	74.7	140.7
	[104.8]	[107.8]	[108.0]	[108.7]	[101.7]
-3	-38.8	-24.7	-43.6	-72.9	-42.9
	[93.44]	[107.3]	[109.0]	[108.0]	[106.8]
-2	-45.4	-20.2	-35.9	-58.7	-34.1
	[88.91]	[104.9]	[105.3]	[103.7]	[107.1]
-1	-27.7	-59.6	-72.3	-81.8	-81.3
	[81.02]	[86.39]	[85.79]	[86.18]	[90.7]
Event year zero omitted (Year family planning program began)					
Observations	7324	7324	7324	7324	7324
R-squared	0.007	0.065	0.066	0.072	0.065
B. 1980 Census: Mean DV			13792		
-2	-32.7	-60.1	-54.3	-19.7	-58.5
	[97.62]	[112.5]	[112.3]	[112.4]	[119.2]
-1	-32.7	-77.6	-77.9	-58.7	-88.2
	[84.83]	[92.67]	[92.54]	[92.24]	[96.8]
Event year zero omitted (Year family planning program began)					
1	110.7	122.1	114.5	101.2	125.8
	[97.02]	[102.8]	[103.4]	[103.8]	[107.5]
2	291.9	326.2	313.0	285.4	363.9
	[115.2]	[126.8]	[127.8]	[126.9]	[133.5]
3	240.2	286.1	267.9	227.5	304.9
	[113.2]	[124.1]	[125.7]	[124.5]	[129.4]
4	343.9	367.5	345.8	295.9	394.5
	[129.5]	[141.8]	[144.5]	[142.2]	[149.5]
5	472.2	483.7	455.1	398.0	530.8
	[145.0]	[158.5]	[163.6]	[160.2]	[169.7]
6	563.8	589.6	558.5	494.8	659.4
	[175.4]	[185.9]	[190.5]	[185.2]	[202.9]
Observations	11313	11313	11313	11313	11313
R-squared	0.218	0.285	0.287	0.303	0.285
For Panels A and B:					
Model	1	2	3	4	2M
Covariates	C, Y	C, Y, S-Y	C, Y, S-Y, R, A	C, Y, S-Y, R, A, Ctrend	C, Y, S-Y, Adjusted for Misclassification
Counties	666	666	666	666	666

See notes in table 3 of main paper.

Table A4.4. Share of Children Living Below 100 Percent of Poverty Line

	(1)	(2)	(3)	(4)	(5)
	Dependent Variable: Share Below 100% Poverty Line				
A. 1970 Census: Mean DV			*21.730*		
-6	1.006	0.540	0.550	0.467	0.668
	[0.602]	[0.632]	[0.639]	[0.640]	[0.679]
-5	0.230	-0.223	-0.208	-0.250	-0.281
	[0.569]	[0.617]	[0.614]	[0.617]	[0.662]
-4	0.212	-0.224	-0.204	-0.210	-0.291
	[0.499]	[0.552]	[0.551]	[0.550]	[0.590]
-3	0.268	-0.006	0.027	0.048	-0.015
	[0.480]	[0.490]	[0.489]	[0.491]	[0.521]
-2	-0.094	-0.437	-0.398	-0.382	-0.585
	[0.449]	[0.471]	[0.471]	[0.473]	[0.508]
-1	0.851	0.775	0.822	0.815	1.039
	[0.434]	[0.474]	[0.472]	[0.475]	[0.520]
	Event year zero omitted (Year family planning program began)				
Observations	7324	7324	7324	7324	7324
R-squared	0.017	0.105	0.107	0.130	0.105
B. 1980 Census: Mean DV			*18.750*		
-2	0.547	0.737	0.720	0.664	0.819
	[0.383]	[0.434]	[0.426]	[0.417]	[0.412]
-1	0.358	0.459	0.455	0.423	0.526
	[0.338]	[0.366]	[0.364]	[0.362]	[0.351]
	Event year zero omitted (Year family planning program began)				
1	-0.368	-0.522	-0.543	-0.520	-0.576
	[0.303]	[0.340]	[0.344]	[0.345]	[0.331]
2	-0.707	-0.903	-0.937	-0.895	-1.012
	[0.297]	[0.340]	[0.341]	[0.343]	[0.333]
3	-0.681	-0.961	-0.994	-0.934	-1.065
	[0.384]	[0.452]	[0.451]	[0.448]	[0.429]
4	-0.696	-0.773	-0.845	-0.771	-0.812
	[0.461]	[0.442]	[0.446]	[0.438]	[0.421]
5	-1.280	-1.401	-1.507	-1.426	-1.594
	[0.466]	[0.524]	[0.522]	[0.512]	[0.488]
6	-1.253	-1.353	-1.485	-1.396	-1.540
	[0.579]	[0.576]	[0.584]	[0.562]	[0.529]
Observations	11313	11313	11313	11313	11313
R-squared	0.021	0.092	0.095	0.100	0.092
For Panels A and B:					
Model	1	2	3	4	2M
Covariates	C, Y	C, Y, S-Y	C, Y, S-Y, R, A	C, Y, S-Y, R, A, Ctrend	C, Y, S-Y, Adjusted for Misclassification
Counties	666	666	666	666	666

See notes in table 3 of main paper.

Table A4.5 Share of Children Living Below 150 Percent of Poverty Line

	(1)	(2)	(3)	(4)	(5)
	Dependent Variable: Share Below 150% Poverty Line				
A. 1970 Census: Mean DV			37.170		
-6	0.942	0.780	0.822	0.783	0.946
	[0.561]	[0.564]	[0.569]	[0.592]	[0.603]
-5	0.084	-0.180	-0.136	-0.123	-0.252
	[0.535]	[0.553]	[0.548]	[0.557]	[0.588]
-4	0.319	0.181	0.227	0.283	0.202
	[0.519]	[0.566]	[0.565]	[0.579]	[0.607]
-3	0.130	0.065	0.117	0.197	0.050
	[0.519]	[0.534]	[0.528]	[0.536]	[0.574]
-2	0.111	-0.050	0.000	0.065	-0.095
	[0.484]	[0.507]	[0.506]	[0.507]	[0.550]
-1	0.627	0.712	0.762	0.785	0.929
	[0.459]	[0.474]	[0.474]	[0.477]	[0.516]
	Event year zero omitted (Year family planning program began)				
Observations	7324	7324	7324	7324	7324
R-squared	0.017	0.094	0.097	0.113	0.094
B. 1980 Census: Mean DV			30.950		
-2	0.018	0.175	0.168	0.128	0.207
	[0.347]	[0.389]	[0.385]	[0.382]	[0.372]
-1	-0.068	0.067	0.065	0.038	0.103
	[0.369]	[0.391]	[0.390]	[0.388]	[0.375]
	Event year zero omitted (Year family planning program began)				
1	-0.815	-0.786	-0.804	-0.792	-0.853
	[0.357]	[0.389]	[0.391]	[0.393]	[0.378]
2	-1.191	-1.182	-1.209	-1.194	-1.309
	[0.404]	[0.428]	[0.433]	[0.432]	[0.415]
3	-0.881	-1.003	-1.031	-0.998	-1.062
	[0.436]	[0.494]	[0.494]	[0.484]	[0.473]
4	-1.353	-1.460	-1.512	-1.473	-1.595
	[0.505]	[0.538]	[0.542]	[0.522]	[0.511]
5	-1.782	-1.979	-2.052	-2.018	-2.231
	[0.523]	[0.622]	[0.625]	[0.594]	[0.580]
6	-1.469	-1.672	-1.761	-1.723	-1.824
	[0.671]	[0.708]	[0.722]	[0.690]	[0.650]
Observations	11313	11313	11313	11313	11313
R-squared	0.053	0.130	0.131	0.140	0.130
For Panels A and B:					
Model	1	2	3	4	2M
Covariates	C, Y	C, Y, S-Y	C, Y, S-Y, R, A	C, Y, S-Y, R, A, Ctrend	C, Y, S-Y, Adjusted for Misclassification
Counties	666	666	666	666	666

See notes in table 3 of main paper.

Table A4.6. Share of Children below 200 Percent of Poverty Line

	(1)	(2)	(3)	(4)	(5)
	Dependent Variable: Share Below 200% Poverty Line
A. 1970 Census: Mean DV			53.580		
-6	0.224	0.173	0.272	0.281	0.229
	[0.586]	[0.616]	[0.608]	[0.649]	[0.660]
-5	-0.169	-0.548	-0.448	-0.390	-0.674
	[0.543]	[0.604]	[0.596]	[0.618]	[0.646]
-4	0.141	-0.105	-0.007	0.092	-0.124
	[0.552]	[0.568]	[0.561]	[0.591]	[0.608]
-3	-0.222	-0.411	-0.314	-0.198	-0.534
	[0.509]	[0.532]	[0.524]	[0.538]	[0.572]
-2	0.179	0.083	0.169	0.263	0.113
	[0.455]	[0.490]	[0.489]	[0.491]	[0.531]
-1	0.488	0.483	0.561	0.601	0.639
	[0.440]	[0.446]	[0.446]	[0.450]	[0.485]
	*Event year zero omitted (Year family planning program began)*
Observations	7324	7324	7324	7324	7324
R-squared	0.010	0.078	0.084	0.100	0.078
B. 1980 Census: Mean DV			44.080		
-2	0.174	0.139	0.163	0.118	0.149
	[0.378]	[0.410]	[0.406]	[0.404]	[0.393]
-1	0.300	0.381	0.397	0.368	0.453
	[0.395]	[0.406]	[0.404]	[0.402]	[0.391]
	*Event year zero omitted (Year family planning program began)*
1	-0.699	-0.523	-0.549	-0.525	-0.570
	[0.397]	[0.437]	[0.436]	[0.432]	[0.423]
2	-1.045	-0.918	-0.946	-0.924	-1.027
	[0.393]	[0.432]	[0.434]	[0.427]	[0.422]
3	-0.725	-0.651	-0.679	-0.640	-0.678
	[0.472]	[0.558]	[0.559]	[0.545]	[0.532]
4	-0.898	-0.755	-0.797	-0.753	-0.769
	[0.575]	[0.603]	[0.606]	[0.579]	[0.570]
5	-2.112	-1.860	-1.902	-1.867	-2.145
	[0.585]	[0.668]	[0.674]	[0.642]	[0.622]
6	-1.383	-1.124	-1.164	-1.134	-1.189
	[0.681]	[0.732]	[0.744]	[0.703]	[0.673]
Observations	11313	11313	11313	11313	11313
R-squared	0.111	0.191	0.192	0.200	0.191
For Panels A and B:					
Model	1	2	3	4	2M
Covariates	C, Y	C, Y, S-Y	C, Y, S-Y, R, A	C, Y, S-Y, R, A, Ctrend	C, Y, S-Y, Adjusted for Misclassification
Counties	666	666	666	666	666

See notes in table 3 of main paper.

Table A4.7. Share of Children Living in Households Receiving Any Public Assistance

	(1)	(2)	(3)	(4)	(5)
	Dependent Variable: Share Receiving any Welfare					
A. 1970 Census: Mean DV			7.555		
-6	-0.148	-0.299	-0.367	-0.312	-0.347
	[0.330]	[0.352]	[0.374]	[0.374]	[0.376]
-5	-0.112	-0.301	-0.360	-0.288	-0.349
	[0.341]	[0.357]	[0.378]	[0.376]	[0.381]
-4	-0.031	-0.100	-0.150	-0.065	-0.096
	[0.320]	[0.350]	[0.367]	[0.367]	[0.375]
-3	-0.148	-0.362	-0.388	-0.297	-0.440
	[0.307]	[0.341]	[0.349]	[0.348]	[0.368]
-2	0.013	-0.074	-0.077	-0.003	-0.062
	[0.248]	[0.294]	[0.298]	[0.293]	[0.316]
-1	0.034	-0.124	-0.106	-0.068	-0.133
	[0.299]	[0.312]	[0.314]	[0.315]	[0.342]
	Event year zero omitted (Year family planning program began)				
Observations	7324	7324	7324	7324	7324
R-squared	0.005	0.079	0.083	0.097	0.079
B. 1980 Census: Mean DV			11.560		
-2	-0.022	-0.052	-0.069	-0.159	-0.056
	[0.266]	[0.287]	[0.288]	[0.292]	[0.274]
-1	-0.136	-0.180	-0.187	-0.234	-0.189
	[0.265]	[0.269]	[0.268]	[0.271]	[0.259]
	Event year zero omitted (Year family planning program began)				
1	-0.636	-0.638	-0.644	-0.604	-0.687
	[0.255]	[0.273]	[0.274]	[0.273]	[0.266]
2	-0.957	-0.862	-0.878	-0.797	-0.938
	[0.252]	[0.286]	[0.289]	[0.287]	[0.280]
3	-1.177	-1.206	-1.224	-1.111	-1.342
	[0.331]	[0.337]	[0.341]	[0.339]	[0.325]
4	-1.269	-1.180	-1.221	-1.079	-1.294
	[0.440]	[0.441]	[0.443]	[0.445]	[0.414]
5	-1.516	-1.338	-1.403	-1.238	-1.481
	[0.441]	[0.453]	[0.457]	[0.451]	[0.422]
6	-1.623	-1.467	-1.553	-1.369	-1.642
	[0.500]	[0.477]	[0.488]	[0.481]	[0.440]
Observations	11313	11313	11313	11313	11313
R-squared	0.010	0.102	0.103	0.114	0.102
For Panels A and B:					
Model	1	2	3	4	2M
Covariates	C, Y	C, Y, S-Y	C, Y, S-Y, R, A	C, Y, S-Y, R, A, Ctrend	C, Y, S-Y, Adjusted for Misclassification
Counties	666	666	666	666	666

See notes in table 3 of main paper.

Table A4.8. Share of Children Living with Single Parents

|	(1)	(2)	(3)	(4)	(5)
	Dependent Variable: Share in Single Headed Households				
A. 1970 Census: Mean DV			*12.840*		
-6	-0.013	-0.200	-0.225	-0.077	-0.230
	[0.438]	[0.464]	[0.492]	[0.488]	[0.498]
-5	0.064	-0.129	-0.154	-0.022	-0.137
	[0.422]	[0.442]	[0.468]	[0.464]	[0.471]
-4	-0.006	-0.207	-0.231	-0.119	-0.233
	[0.406]	[0.431]	[0.452]	[0.453]	[0.461]
-3	0.081	-0.061	-0.080	0.013	-0.041
	[0.407]	[0.425]	[0.437]	[0.436]	[0.459]
-2	-0.214	-0.348	-0.360	-0.296	-0.413
	[0.362]	[0.378]	[0.386]	[0.387]	[0.409]
-1	-0.043	-0.154	-0.163	-0.130	-0.145
	[0.339]	[0.367]	[0.369]	[0.369]	[0.402]
	Event year zero omitted (Year family planning program began)				
Observations	7324	7324	7324	7324	7324
R-squared	0.007	0.062	0.062	0.068	0.062
B. 1980 Census: Mean DV			*16.940*		
-2	-0.399	-0.438	-0.420	-0.381	-0.497
	[0.322]	[0.340]	[0.339]	[0.341]	[0.324]
-1	0.336	0.363	0.365	0.387	0.442
	[0.334]	[0.358]	[0.355]	[0.358]	[0.341]
	Event year zero omitted (Year family planning program began)				
1	0.002	-0.102	-0.143	-0.164	-0.091
	[0.298]	[0.295]	[0.296]	[0.294]	[0.288]
2	-0.080	-0.355	-0.424	-0.449	-0.393
	[0.290]	[0.315]	[0.312]	[0.310]	[0.308]
3	-0.086	-0.438	-0.528	-0.563	-0.486
	[0.326]	[0.348]	[0.351]	[0.348]	[0.338]
4	-0.392	-0.785	-0.913	-0.954	-0.918
	[0.434]	[0.466]	[0.460]	[0.459]	[0.438]
5	0.190	-0.279	-0.451	-0.489	-0.281
	[0.462]	[0.494]	[0.480]	[0.482]	[0.460]
6	0.057	-0.483	-0.681	-0.719	-0.541
	[0.495]	[0.534]	[0.524]	[0.526]	[0.491]
Observations	11313	11313	11313	11313	11313
R-squared	0.026	0.095	0.098	0.105	0.095
For Panels A and B:					
Model	1	2	3	4	2M
Covariates	C, Y	C, Y, S-Y	C, Y, S-Y, R, A	C, Y, S-Y, R, A, Ctrend	C, Y, S-Y, Adjusted for Misclassification
Counties	666	666	666	666	666

See notes in table 3 of main paper.

Table A4.9. Mother's Age at the Time of Child's Birth

	(1)	(2)	(3)	(4)	(5)
	Dependent Variable: Average Mother's Age at Birth				
A. 1970 Census: Mean DV			25.790		
-6	0.062	0.029	0.034	0.042	0.033
	[0.0966]	[0.103]	[0.0990]	[0.0957]	[0.111]
-5	0.083	0.044	0.050	0.062	0.052
	[0.0887]	[0.0915]	[0.0887]	[0.0869]	[0.097]
-4	0.094	0.066	0.072	0.086	0.080
	[0.0797]	[0.0840]	[0.0816]	[0.0810]	[0.090]
-3	0.003	-0.005	0.003	0.017	-0.011
	[0.0744]	[0.0791]	[0.0764]	[0.0760]	[0.085]
-2	0.020	0.034	0.043	0.051	0.040
	[0.0750]	[0.0728]	[0.0709]	[0.0702]	[0.077]
-1	0.001	0.000	0.009	0.008	-0.006
	[0.0628]	[0.0694]	[0.0686]	[0.0687]	[0.075]
	Event year zero omitted (Year family planning program began)				
Observations	7324	7324	7324	7324	7324
R-squared	0.104	0.177	0.182	0.203	0.177
B. 1980 Census: Mean DV			24.940		
-2	0.028	0.045	0.047	0.029	0.047
	[0.0545]	[0.0602]	[0.0599]	[0.0584]	[0.063]
-1	0.065	0.078	0.082	0.072	0.089
	[0.0589]	[0.0615]	[0.0614]	[0.0609]	[0.064]
	Event year zero omitted (Year family planning program began)				
1	-0.013	-0.003	0.002	0.010	0.005
	[0.0613]	[0.0644]	[0.0642]	[0.0639]	[0.067]
2	-0.116	-0.114	-0.106	-0.092	-0.123
	[0.0572]	[0.0645]	[0.0654]	[0.0642]	[0.066]
3	-0.156	-0.182	-0.172	-0.154	-0.201
	[0.0623]	[0.0671]	[0.0680]	[0.0664]	[0.069]
4	-0.170	-0.193	-0.181	-0.158	-0.210
	[0.0753]	[0.0820]	[0.0827]	[0.0814]	[0.087]
5	-0.269	-0.303	-0.284	-0.260	-0.346
	[0.0861]	[0.0977]	[0.0988]	[0.0961]	[0.105]
6	-0.236	-0.276	-0.254	-0.228	-0.311
	[0.0921]	[0.101]	[0.0999]	[0.0975]	[0.110]
Observations	11313	11313	11313	11313	11313
R-squared	0.133	0.203	0.208	0.224	0.203
For Panels A and B:					
Model	1	2	3	4	2M
Covariates	C, Y	C, Y, S-Y	C, Y, S-Y, R, A	C, Y, S-Y, R, A, Ctrend	C, Y, S-Y, Adjusted for Misclassification
Counties	666	666	666	666	666

See notes in table 3 of main paper.

Table A4.10. Average Number of Older Siblings for Cohorts

	(1)	(2)	(3)	(4)	(5)
		Dependent Variable: Average Number of Older Siblings			
A. 1970 Census: Mean DV			*1.894*		
-6	0.000	-0.024	-0.024	-0.015	-0.029
	[0.0300]	[0.0315]	[0.0313]	[0.0311]	[0.033]
-5	0.022	0.001	0.001	0.011	0.001
	[0.0270]	[0.0272]	[0.0269]	[0.0268]	[0.029]
-4	-0.009	-0.024	-0.023	-0.013	-0.031
	[0.0228]	[0.0237]	[0.0235]	[0.0237]	[0.024]
-3	0.016	0.015	0.017	0.027	0.020
	[0.0268]	[0.0277]	[0.0273]	[0.0277]	[0.029]
-2	0.016	0.015	0.018	0.025	0.020
	[0.0245]	[0.0242]	[0.0241]	[0.0240]	[0.026]
-1	0.001	-0.004	-0.001	0.002	-0.006
	[0.0212]	[0.0233]	[0.0234]	[0.0235]	[0.025]
	Event year zero omitted (Year family planning program began)				
Observations	7324	7324	7324	7324	7324
R-squared	0.234	0.295	0.297	0.313	0.295
B. 1980 Census: Mean DV			*1.378*		
-2	-0.016	-0.019	-0.018	-0.024	-0.021
	[0.0210]	[0.0234]	[0.0231]	[0.0226]	[0.024]
-1	0.008	0.015	0.016	0.013	0.019
	[0.0202]	[0.0215]	[0.0214]	[0.0215]	[0.022]
	Event year zero omitted (Year family planning program began)				
1	-0.023	-0.015	-0.014	-0.012	-0.015
	[0.0183]	[0.0197]	[0.0197]	[0.0196]	[0.020]
2	-0.043	-0.030	-0.028	-0.025	-0.033
	[0.0176]	[0.0189]	[0.0190]	[0.0187]	[0.018]
3	-0.044	-0.039	-0.035	-0.031	-0.041
	[0.0175]	[0.0203]	[0.0204]	[0.0197]	[0.021]
4	-0.072	-0.062	-0.057	-0.051	-0.069
	[0.0229]	[0.0267]	[0.0267]	[0.0259]	[0.028]
5	-0.086	-0.070	-0.062	-0.057	-0.079
	[0.0237]	[0.0306]	[0.0305]	[0.0293]	[0.032]
6	-0.081	-0.065	-0.055	-0.050	-0.072
	[0.0270]	[0.0336]	[0.0331]	[0.0316]	[0.036]
Observations	11313	11313	11313	11313	11313
R-squared	0.492	0.541	0.543	0.557	0.541
For Panels A and B:					
Model	1	2	3	4	2M
Covariates	C, Y	C, Y, S-Y	C, Y, S-Y, R, A	C, Y, S-Y, R, A, Ctrend	C, Y, S-Y, Adjusted for Misclassification
Counties	666	666	666	666	666

See notes in table 3 of main paper.

Figure A1. Fertility Rates and the Roll-Out of Federal Family Planning Programs

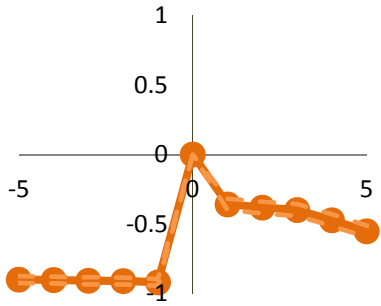*A.   Year of Establishment and 1964 General Fertility Rate (GFR)*

Slope = 0.13, S.E.= 0.42
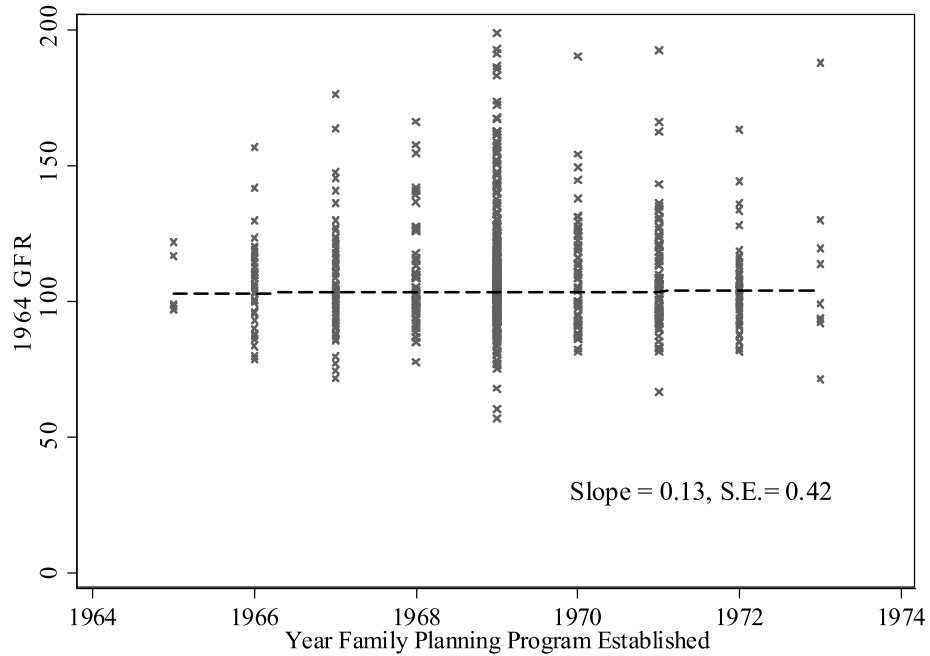
B.   Year of Establishment and 1960 to 1964 Change in the GFR
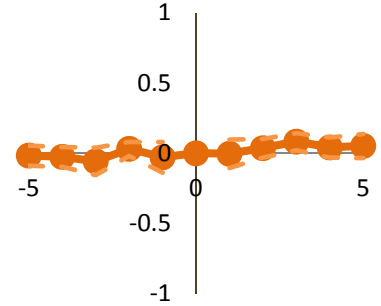
Slope = 0.11, S.E.= 0.24

The x-axis plots the year 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. The estimated slope and standard error for each relationship are indicated. Sources: Family planning data: NACAP, NAFO and OEO (1969, 1971 and 1974); GFR: Bailey (2012).

Figure A2. Funding for Other War on Poverty Programs Relative to the First Federal Family Planning Grant

A. Family Planning Spending *B. Community Health Centers* *C. Head Start Spending* *D. Jobs Programs Spending*
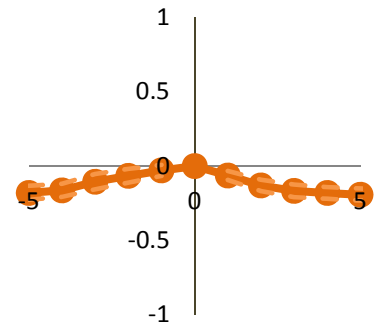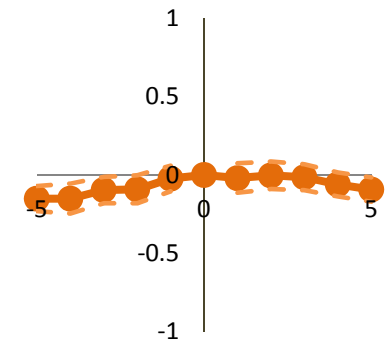
*E. Legal Services Spending* *F. Maternal and Infant Care* *G. Maternity and Child Health*
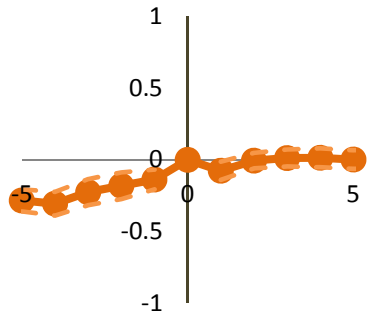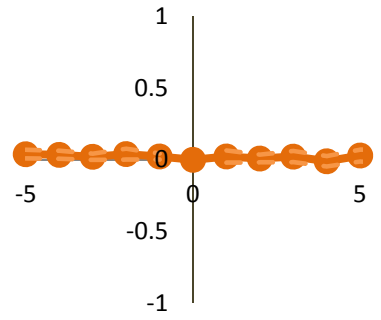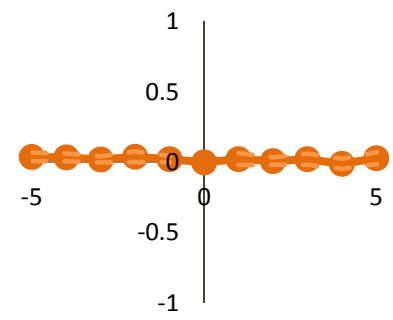
Each panel plots weighted least-squares estimates of τ from equation 1 including funded and unfunded counties and state-year fixed effects. The dependent variable is equal to 1 if the county received any federal funding for the indicated program. The weights are the 1970 population of women ages 15 to 44. The dependent variable is equal to 1 if the county received *any* federal grant for the indicated program. Sources: NACAP and NAFO.

Figure A3. General Fertility Rates Before and After Family Planning Programs Began

A. Comparison of Vital Statistics and Census Estimates before Misclassification Error Adjustments
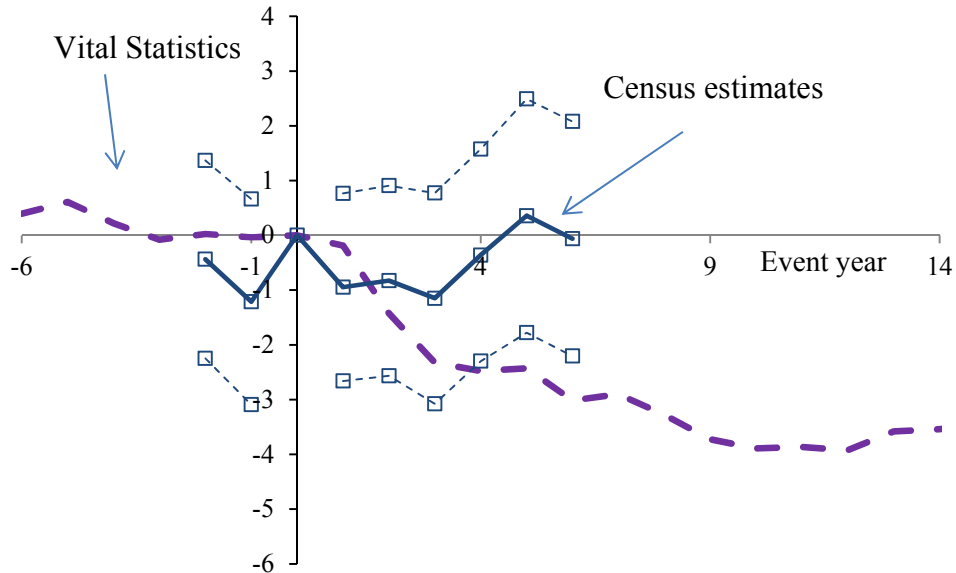

B. Comparison of Vital Statistics and Census Estimates after Misclassification Error Adjustments
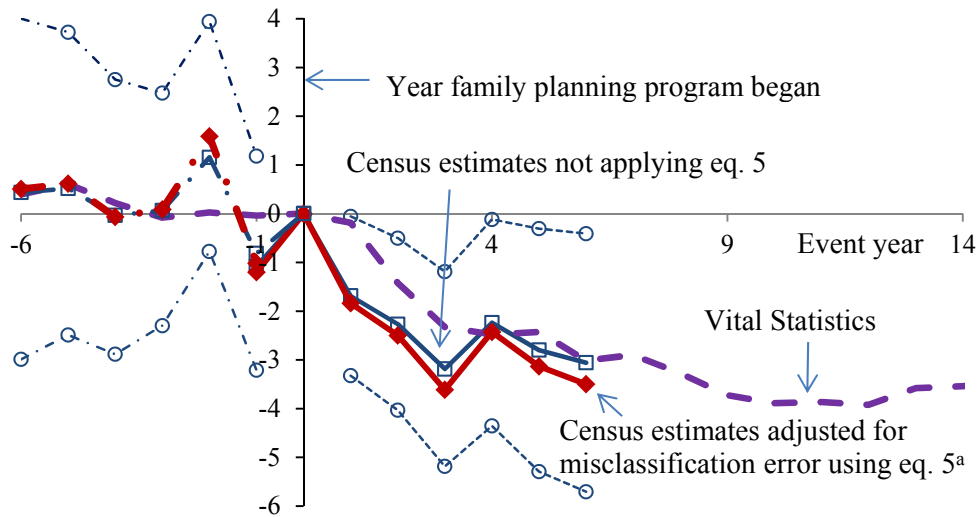
Series plot estimates of τ from our baseline model of equation 1. The x-axis plots the event year, equal to year of observation minus year of first family planning grant, the year the federal family planning began. The dependent variable is the general fertility rate (GFR) calculated using either Vital Statistics or the 1980 census (1970 estimates in appendix tables A4). The Vital Statistics have little measurement error in county of birth, as county represents county of mother's residence at the time of the birth. The census estimates use the GFR implied by the county of residence and age of the child in the census. Census estimates in panel A use county or residence at the time of the census and weight by population. Census estimates in panel B use place of residence in 1965/1975 and omit weights. [a] Adjustment for misclassification error indicates that eq. 5 has been used to alter the estimates after estimation as described in text. Sources: 1970 and 1980 restricted long-form census samples for both numerator and denominator estimates. Vital Statistics estimates use, for GFR numerators, hand-entered, county-level birth aggregates published in Vital Statistics from 1959 to 1967 and the Natality Detail Files from 1968 to 1988 (NCHS 2002). For GFR denominators, SEER county-level estimates of women ages 15 to 44 from 1969-1988 are augmented with interpolated, county-level estimates of between the 1960 census and the 1969 SEER.

Figure A4. Relationship of Misclassification of County of Birth and Parents' Treatment with Federally Funded Family Planning Programs
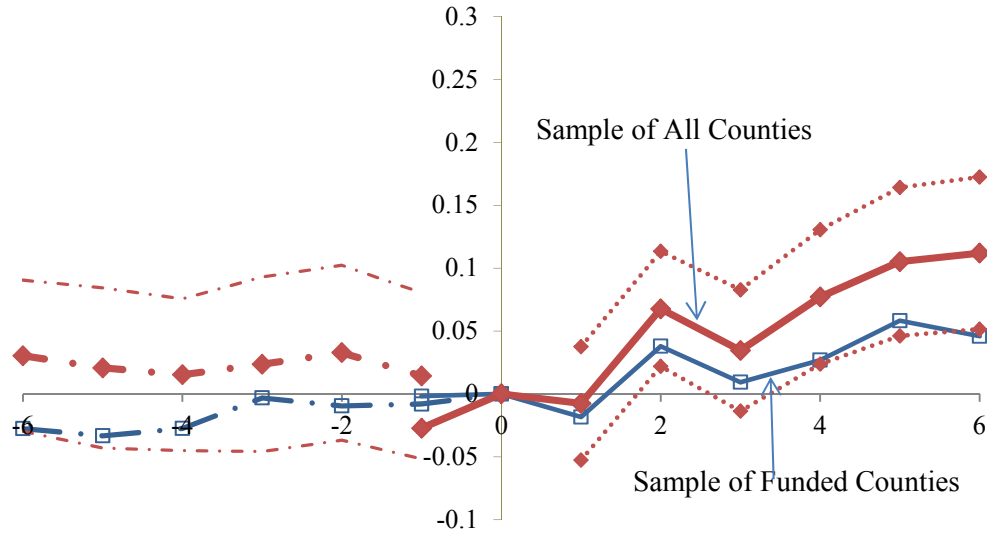

Series plot estimates of τ from our baseline model of equation 1 for a sample of all counties (red diamonds) and a sample of only funded counties (blue squares). The x-axis plots the event year, equal to year of observation minus year of first family planning grant, the year the federal family planning began. The dependent variable is the share of children who have changed their treatment status between 1975 and 1980 (between 1965 and 1970 when using the 1970 census). Heteroskedasticity-robust standard errors clustered by county are used to construct 95 percent confidence intervals for a sample of all counties and are presented as dashed lines. Sources: 1970 (dashed lines with markers) and 1980 (solid lines with markers) restricted-use censuses.

Figure A5. Effects of Federally Funded Family Planning on Marriage and Divorce

A. Marriages per 1000 Women Ages 15 to 44
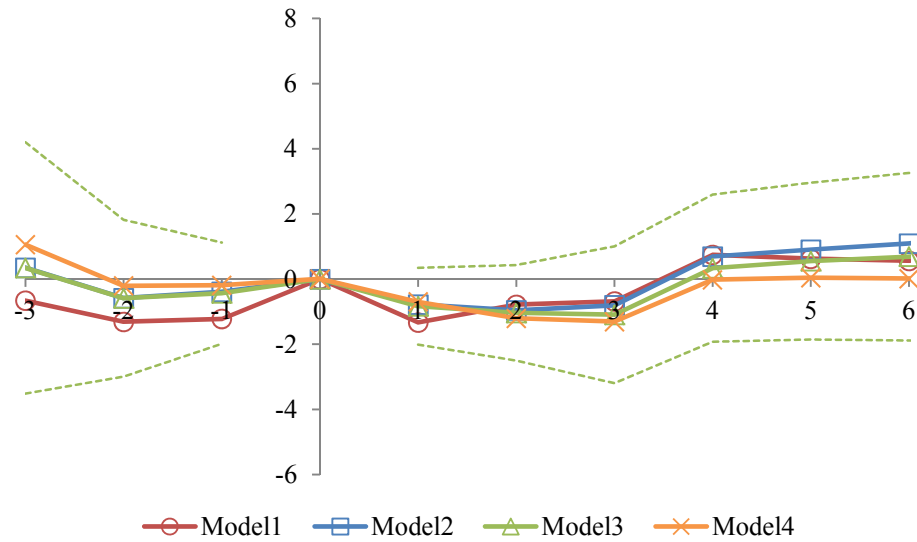
B. Divorces per 1000 Women Ages 15 to 44
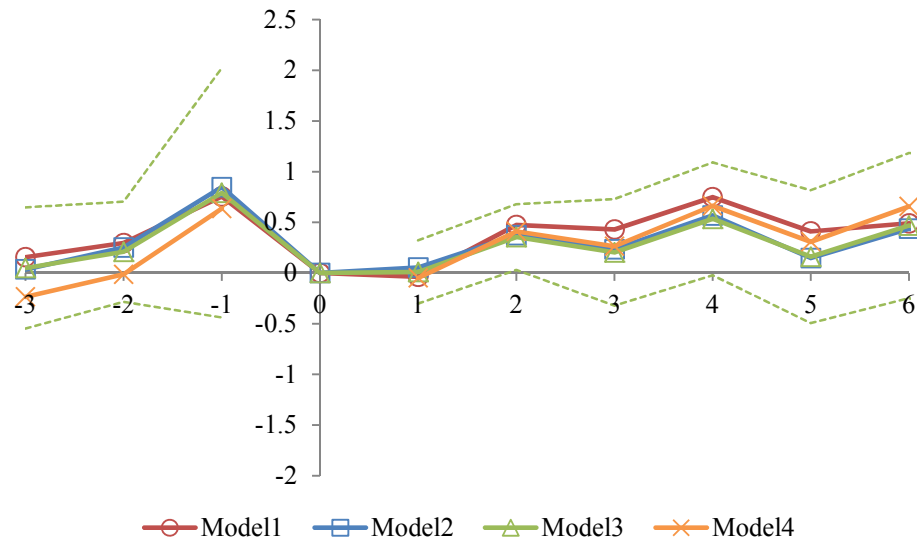
Series plot point estimates of τ from specifications of equation 1 using both funded and unfunded counties. See table 2 notes in main paper for information on specifications and models. The dependent variable in panel A is the number of marriages per 1000 women ages 15 to 44; the dependent variable in panel B is the number of divorces per 1000 women ages 15 to 44. Sources: Numerators are hand entered from published county-level tabulations from Vital Statistics, 1962 to 1988. Denominators rely on SEER population data from 1969 forward and data interpolated between the 1960 census and 1969 SEER data for the rest of the 1960s.