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Abstract:

This paper examines the relationship between parents' access to family planning and the economic resources of the average child. Using the county-level introduction of U.S. family planning programs between 1964 and 1973, we find that children born after 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. Even with extreme assumptions about selection, these estimates are large enough to imply that family planning programs directly increased children's resources, 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) directly raised the household incomes of poor children at costs of around $10 and $63 billion in 2013, respectively. Medicaid and the State Children's Health Insurance Plan (S-CHIP) contributed roughly $444 billion for children's health insurance in 2013. Head Start provided $8 billion for pre-school programs for around 930,000 disadvantaged children in 2014.

This paper evaluates the impact of a less studied program that targets childhood disadvantage. Family planning programs, largely absent from today's policy discussions of childhood disadvantage, have been used as a means to improve children's lives since the 1960s (Johnson 1966,[1] Nixon 1969 quote above).

[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).

The programs' potential effects on children relate closely to economic theory. Holding parents' 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.

This paper provides novel evidence on the direct effect of family planning programs on the resources available to the average child. Using the restricted-use long-form 1970 and 1980 census samples and a research design exploiting the timing of program initiation between 1965 and 1975, we compare the outcomes of children in the same county born before and after federally funded family planning programs began.[3] Our event-study estimates show that cohorts born after programs began were significantly more advantaged in multiple dimensions. They lived in households with 2.5 percent higher annual incomes. They were 7 percent less likely to live in poverty and 11 percent less likely to live in households receiving public assistance. These children were also slightly more likely to live with two parent families, have younger mothers, and fewer older siblings.

The second part of the paper seeks to understand the mechanisms for improvements in child outcomes and characterize the value of family planning programs as investments in children.[4] Do

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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] 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 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.

[4] 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.

improvements corresponding to family planning program initiation result from reductions in childbearing among disadvantaged parents (often called the "selection effect") or, as suggested by economic theory, parents changing their investments in their own human capital (e.g., in their educations or careers), labor-force participation (especially of mothers), and partnerships (e.g., better and more stable unions)—something we call the "empowerment effect"? If only the selection effect operates, then the resources of the average child could rise even though the resources available to any one child do not. The role of selection is potentially large. Although 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).[5]

We use a bounding exercise to understand the contribution of the selection and empowerment effects of family planning programs. Using the 1960 census, we simulate the effect of family planning programs on children's household income under several scenarios. Under the extreme assumption that all births averted due to family planning programs would have been the *poorest* children, selection can explain at most 84 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. This simulation exercise implies that the empowerment effect explains at least 16 percent and plausibly 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 seeking to increase the opportunities of disadvantaged children. Excluding the effects of family planning through selection, our estimates imply that family planning programs likely reduced child poverty at around half of the cost of the EITC and one third the cost of TANF.

[5] A related literature suggest the potential importance of abortion legalization via the selection effect is large in both the short run (Gruber, Levine, and Staiger 1999) and the longer run (Ananat, Gruber, Levine, and Staiger 2009; Pop-Eleches 2006).

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 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.[6] 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

[6] 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.

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) 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, 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.[7] 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

[7] 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.

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 counties also had lower poverty rates. Our analysis accounts for these cross-sectional differences using county-fixed effects or by restricting the sample to funded counties 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).[8] 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.[9] Panel A of table 1 shows that federal family planning programs significantly increased the use of

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[8] 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).

[9] We estimate $Y_{j,t} = \theta_j + \gamma_{s(j),t} + \tau 1\left(t > T_j^*\right) + \boldsymbol{X}_{j,t}'\boldsymbol{\beta} + \varepsilon_{j,t}$ 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 $\boldsymbol{X}$ 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

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.

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.[10] 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 around 23 to 30 percent (13 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.[11]

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) limits strong conclusions.

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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.

[10] We estimate the specification, $\Pr(\mathrm{Use}_{ij}) = \mathbf{F}(\mathbf{Z}_{ij}\boldsymbol{\delta} + \theta_1 1(\mathrm{T}_j^* < 1970) + \theta_2 1(\mathrm{Pov}_{ij}) + \theta_3 1(\mathrm{Pov}_{ij}) 1(\mathrm{T}_j^* < 1970))$, where Use_{ij} is equal to 1 if an individual *i* in county *j* had ever used family planning or the birth control pill, $1(\mathrm{T}_j^* < 1970)$ is a binary variable equal to one if county *j* received a federal family planning grant before the survey date, and $1(\mathrm{Pov}_{ij})$ 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.

[11] 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.

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). 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.[12] 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

Understanding the mechanisms through which family planning programs affect the economic and living circumstances of children reflects two main complementary channels: selection and empowerment.

[12] 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.

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 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 after *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,[13] 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 1966 declaration that abortion and family planning were illegal 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).

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

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 in and complete college (Hock 2008; Bailey, Hershbein, and Miller 2012). Women were more 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.[14]

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. 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.[15] 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

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[14] Some studies, however, show that 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).
[15] 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.

divorce rates (Christensen 2011, Rotz 2011). On the other hand, family planning programs could increase 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 about the income distributions of family planning program users.

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).[16] Our analysis aggregates the economic resources and living circumstances of children under age 18 into birth-year/county cohorts.[17] These birth-year/county cohorts are then linked to information on when their county received a federally funded family planning program.[18]

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:[19]

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

[16] 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.
[17] Birth year is constructed using age and quarter of birth at the time of the census.
[18] 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.
[19] 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.

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*—our proxy for the date the program started.[20] 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.[21]

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

[20] 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 was 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.

[21] 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.

abortion providers, which accounts for within-state changes in the provision of abortion between 1970 and 1979 (cf. Bailey 2012).[22]

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

[22] 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.

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).[23]

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

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[23] 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 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.

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 planning programs began were significantly more likely to live with a parent who moved in the five years before the census.[24]

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,

$$\tau_c = \sum_{j=a}^{b} \tau_j^* \cdot p_{j,c}. \qquad (2)$$

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

[24] 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).

the full set of estimates, $\boldsymbol{\tau}^* = \boldsymbol{\tau}\boldsymbol{P}^{-\mathbf{1}}$.[25] 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 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

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[25] $\boldsymbol{\tau}$ 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.

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.[26] 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

[26] 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.

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 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 mechanically if family planning reduced 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).[27] 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 attributable to family planning, suggesting that subsequent births in the household reduced the estimated 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.

[27] 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.

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 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[28]) 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 births 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 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 third parity births for up to 15 years after they were established—a result consistent with family planning programs affecting stopping behavior. Her estimates for fourth and higher order births are negative but imprecise, largely because fourth-order and higher births occur infrequently.

[28] 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.

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 be associated with *lower* household incomes—so the increases we observe in figure 2 outweigh this effect.

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. We generate an upper bound on the role of selection (and therefore a lower bound on the empowerment effect) by assuming that only the children in the lowest tail of the household income distribution were averted using family planning services. Additionally, we characterize the more likely selection effect by assuming that the missing children would have had parents following the empirical distribution of incomes of family planning users. 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, $g(\quad)$, relating childbearing to a household characteristic, such as household income, *y*, among parents before the introduction of family planning programs, where $0 \leq g(y)$ for all *y* and $\sum g(y) = N$. 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 $\mu^{fp} = \frac{1}{M}\sum h(y)g^{fp}\big(h(y)\big)$.

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 $\mu^{fp} = \frac{1}{M}\sum h(y)g^{fp}\big(h(y)\big)$. The following difference between these means captures the effect of family planning programs on the household income of the average child:

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

Rewriting the equation by adding and subtracting a cross-term, this treatment effect can be decomposed into a selection and empowerment effect,[29]

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

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 $g^{fp}(\quad)$=$g(\quad)$), 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. To the extent that older children born before family planning programs also benefitted (e.g., through intra-household spillovers), the pre-family planning program mean, $\mu \equiv \frac{1}{N}\sum yg(y)$, will be overstated. The difference between the two means will, therefore, *understate* the treatment effect of family planning on children's outcomes. Because we cannot address this problem directly without information on

[29] 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.

children's consumption, we assume our findings understate the overall effect and also that this understatement is relatively proportioned between the selection and empowerment effects.

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 $g^{fp}(\quad)$ 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 $g^{fp}(\quad)$, 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

$$\tau^T \equiv \mu^T - \mu = \frac{1}{M}\sum\nolimits_{y>T} yg(y) - \frac{1}{N}\sum yg(y). \tag{4}$$

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 Bailey's (2012) estimates of the effect of family planning programs on fertility rates.[30] This results in truncation of children with household

[30] 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. We choose this as a baseline 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.

incomes on average below $286 in 2013 dollars annually in the 1959 distribution of children's household income.

B. Empirically Based (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,

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

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 empirical distribution.[31] 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 2 percent of children from the 1960 census sample of children ages 4 to 6, 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 simulated empowerment effect under different types of selection. 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 the simulated selection

[31] 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

effect is, therefore, zero, and the simulated empowerment effect accounts for the entirety of the estimated effects, the average of the estimated effects of family planning programs on household income in event years 4 to 6 (column 4 of table 2). The second and third rows simulate the empowerment effects under lower truncation (row 2) and empirically approximated selection (row 3). The 95-percent intervals for the estimates are generated using a non-parametric bootstrap procedure and presented in brackets (Johnston and DiNardo 1997).[32] The results show that even 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.[33]

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 the average child was more advantaged along a number of dimensions. Cohorts born four to six years after program initiation lived in households earning 2.5 percent more annually. The gains in household income were largest among 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. The results will also understate the broader effects of family planning programs if mobility induces classical measurement error.

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

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 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 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 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.[34] 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

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

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 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 of 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.

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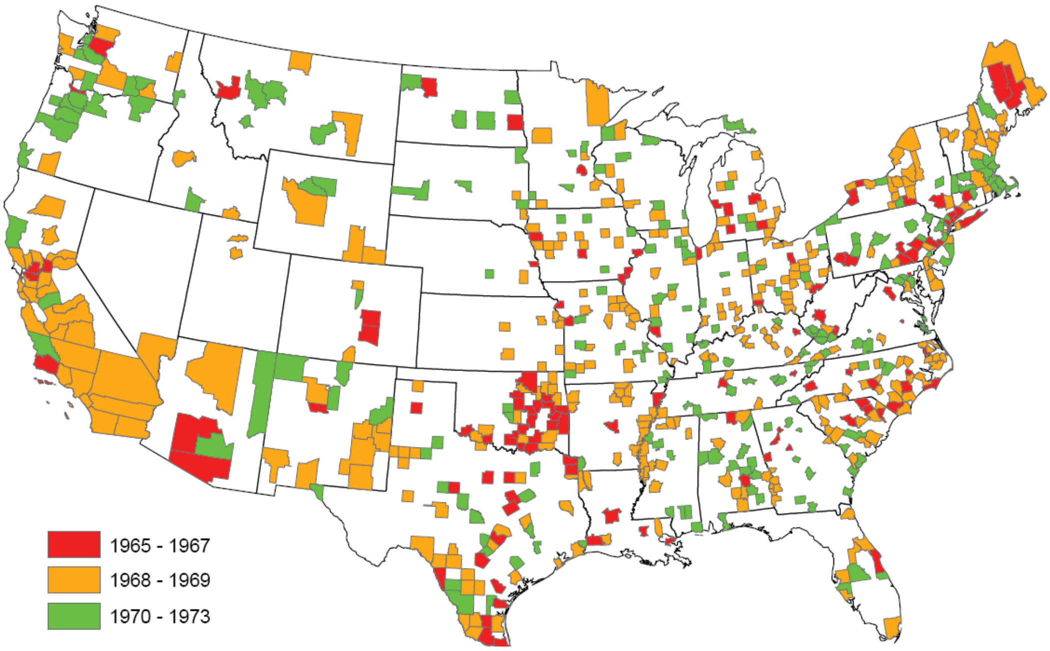
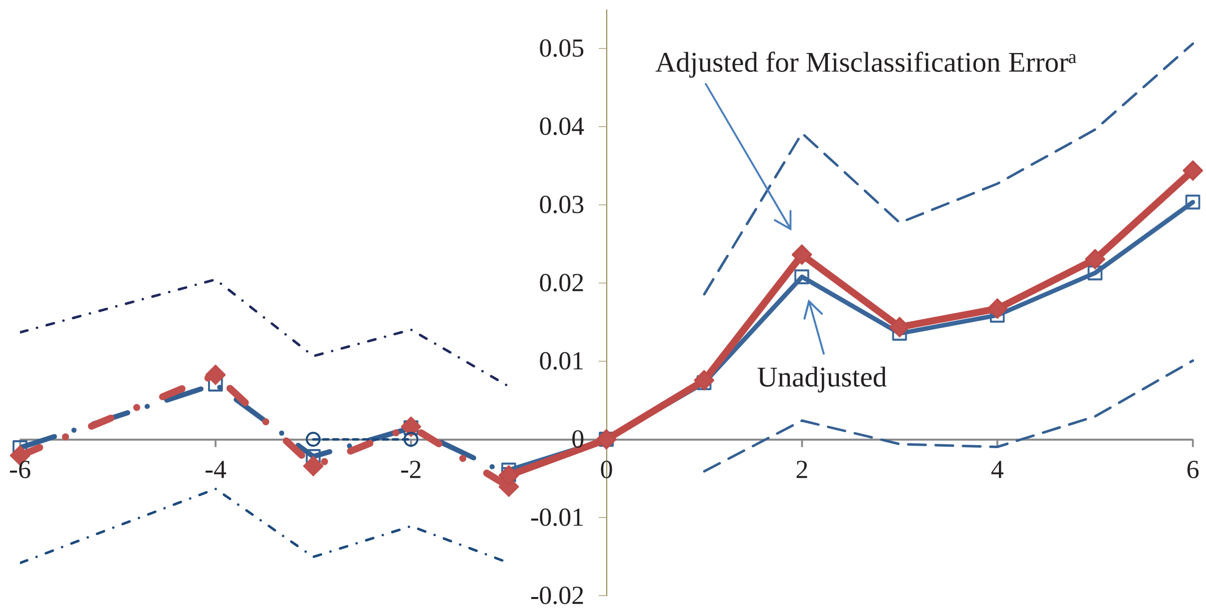
Figure 1. The Roll-Out of Federally Funded Family Planning Programs, 1965-1973



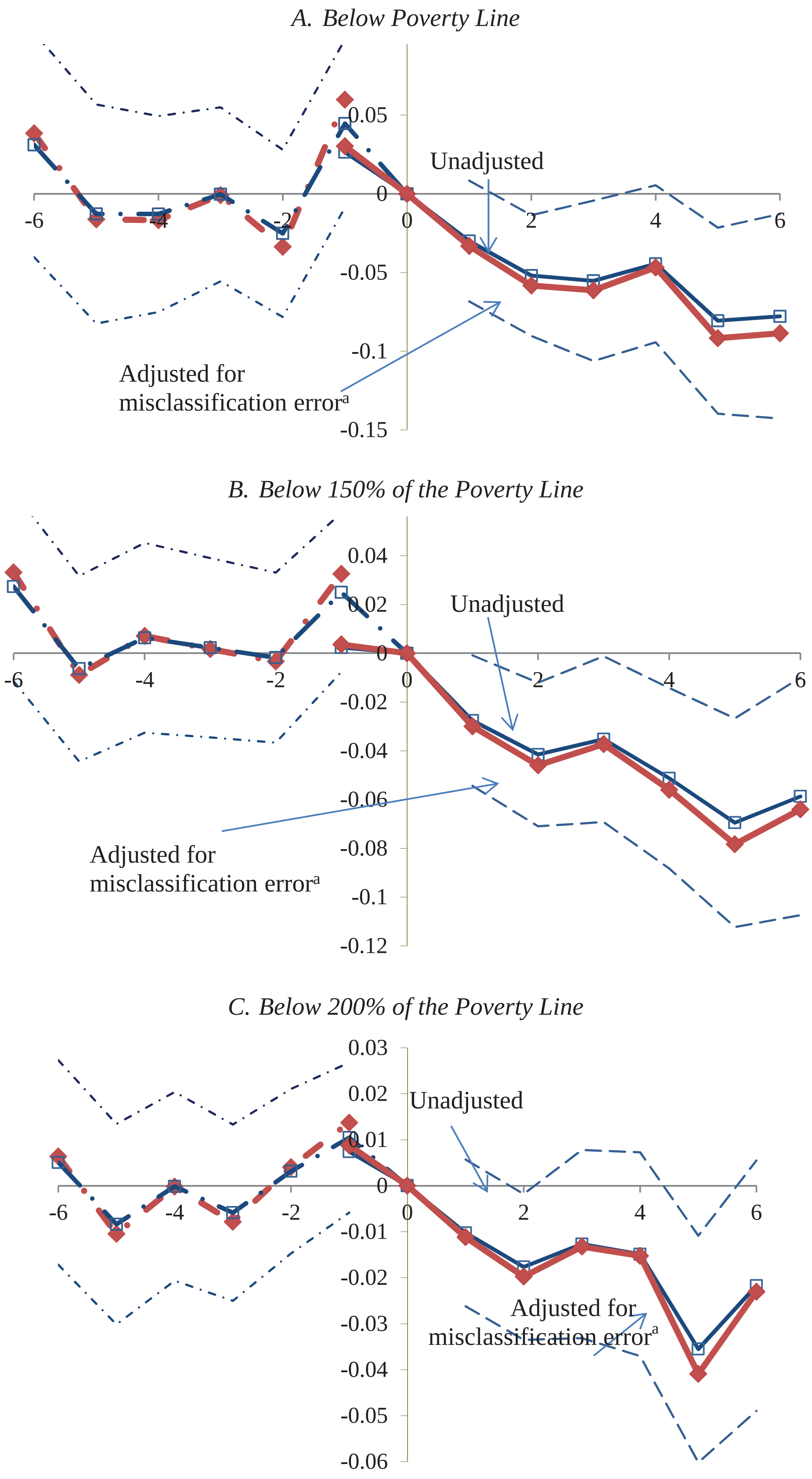
Dates are the year that the county first received a federal family planning grant. Counties not receiving a family planning grant between 1965 and 1973 are not shaded. Sources: NACAP, NAFO, and OEO (1969, 1971, and 1974).

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



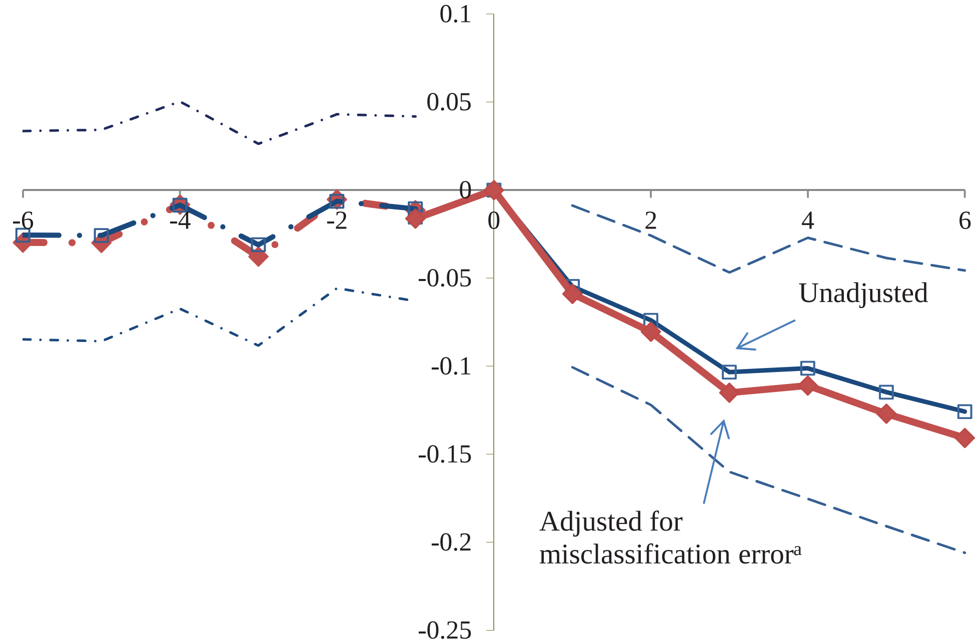
Panels plot of τ from equation 1 for model 2, both unadjusted and adjusted for misclassification error, divided by the average dependent variable for cohorts in the same county born before family planning programs began in the 1980 census. [a] Adjustment for misclassification error indicates that eq. 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



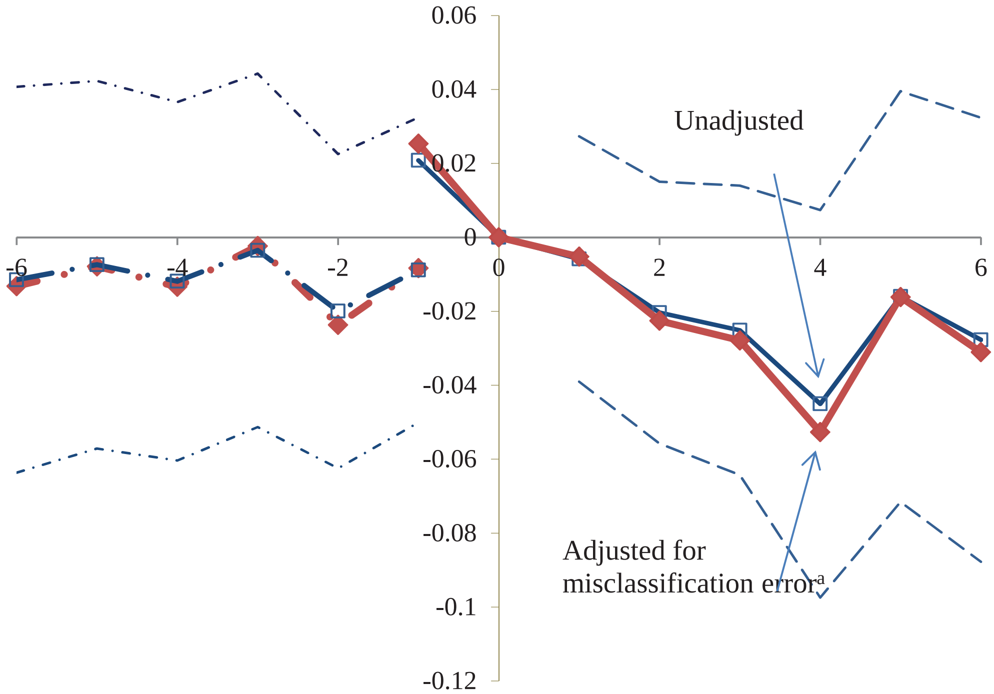
See notes for figure 2 and estimates in table 3.

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

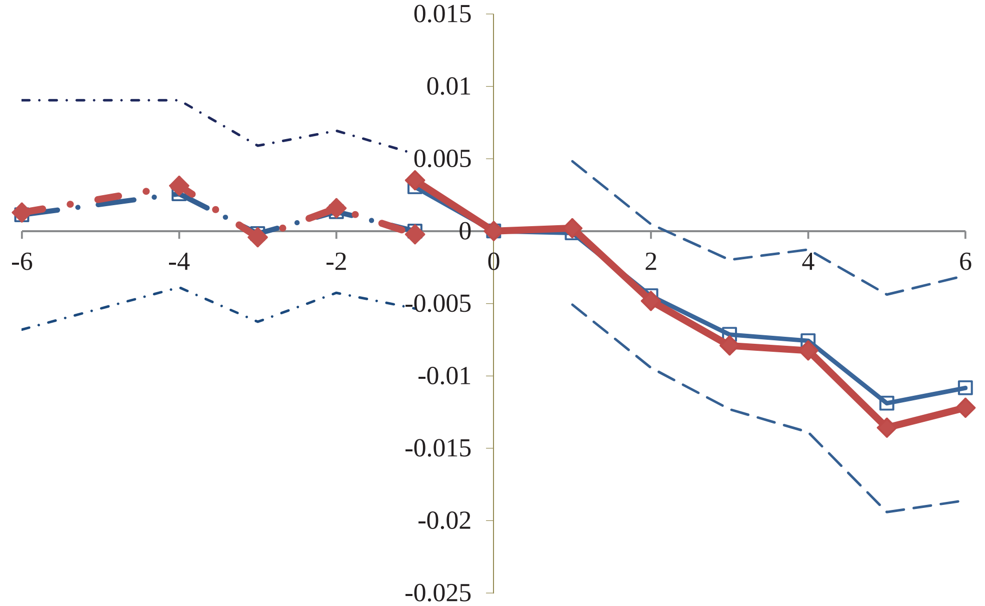
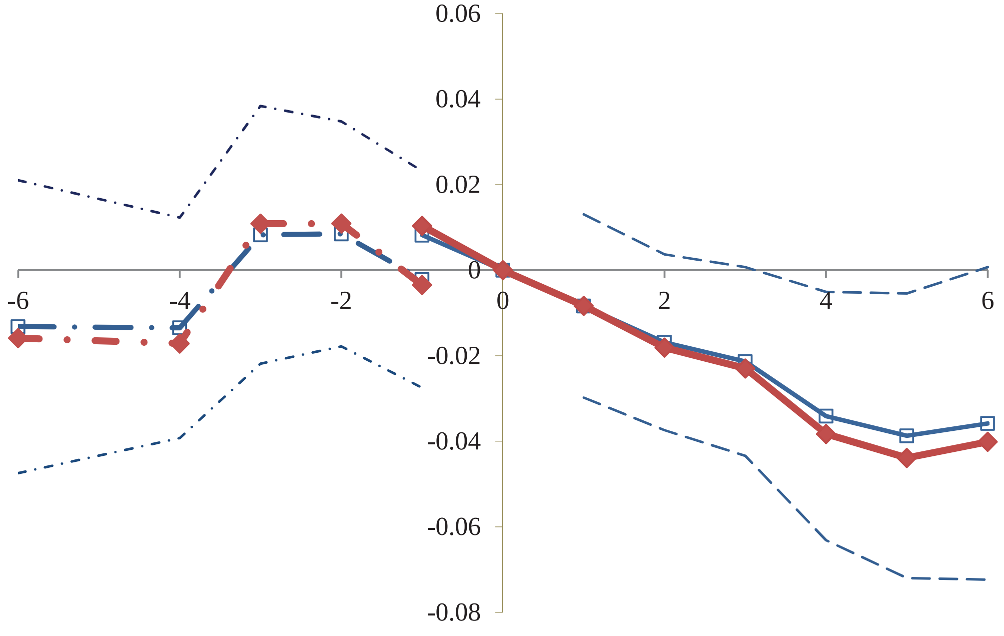


Figure 7. Average Number of Older Siblings for Cohorts Born before and after Family Planning Programs Began



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. The samples of children in event-years -2, -1, 0, 1, 2, 3, 4, 5, and 6 are 1,862,388; 1,816,232; 1,799,900; 1,796,536; 1,735,989; 1,686,895; 1,636,621; 1,588,435 and 1,560,401, respectively. [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

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Percent of Children below			Percent White Children below			Percent Non-white Children below		
Dependent Variables	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. The samples of children in event years -2, -1, 0, 1, 2, 3, 4, 5, and 6 for whites are 1,489,588; 1,448,089; 1,433,763 ; 1,428,864, 1,370,352, 1,325,507, 1276760, 1237436, 1214314, respectively. These sample sizes for nonwhites are 370,019; 365,304; 363,280; 365,009; 362, 757; 358,642; 356,818; 347,608; and 342,863, respectively.

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

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variables	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	100%
Selection by lower truncation (lower bound on empowerment)	2.09 [2.0, 2.29]	0.38	15.4%
Empirically likely selection	0.99 [0.9, 1.29]	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 empirically 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.

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