DOES ACCESS TO 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 the economic resources of their children. Using the county-level introduction of U.S. family planning programs between 1964 and 1973, we find that children born after programs began had 2.8% higher household incomes. They were also 7% less likely to live in poverty and 12% less likely to live in households receiving public assistance. A bounding exercise suggests that the direct effects of family planning programs on parents' resources account for roughly two thirds of these gains.

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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.

~President Lyndon Johnson, Special Message to the Congress on Domestic Health and Education, March 1, 1966.

…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 et al. 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 et al. 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 2015). 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 relatively less studied impact of family planning programs on childhood disadvantage. Although largely absent from today's policy discussions of childhood disadvantage, family planning programs have been considered a means to improve children's lives since at least the 1960s

(Johnson 1966, Nixon 1969). Economic theory predicts that family planning programs could directly impact children's opportunities by increasing the incomes of their parents. This direct "resource effect" would result if family planning programs reduce the cost of avoiding unintended pregnancies, which constrain parents' choices both when they occur and even when they do not occur (because they influence parents' expectations and, therefore, discourage their investments). By reducing the cost of avoiding unintended pregnancies, family planning programs may encourage parents' investments in their human capital, partnerships, and careers (Goldin and Katz 2002, Bailey 2006, Hock 2008, Christensen 2011, Bailey et al. 2012). With standard assumptions about the income elasticity of child quality, a rise in income should itself serve to increase investment in each child and should be reinforced by the substitution effect (Becker and Lewis 1973, Willis 1973). To this point, empirical evidence shows that wanted children tend to have better health at birth (Corman and Grossman 1985, Grossman and Joyce 1990, Gruber et al. 1999) and be treated better by their parents (David et al. 2003, David 2006).

This paper provides novel evidence on the resource effect of family planning programs on children's economic resources. Our research design exploits the roll-out of federally funded family planning programs between 1964 and 1973 within a dynamic, event-study framework (Bailey 2012). Using the restricted-use long-form 1970 and 1980 Census samples, we compare the outcomes of children in the same county born before and after federally funded family planning programs began. We find that cohorts born after programs began were significantly more economically advantaged. They lived in households with 2.8 percent higher annual incomes. They were 7 percent less likely to live in poverty and 12 percent less likely to live in households receiving public assistance. These changes were driven in part by older mothers reducing unwanted pregnancies. As a result, the average child was slightly more likely to live with two parent families, have a younger mother, and fewer older siblings.

Understanding the role of "selection" in driving these findings is crucial their interpretation. If these gains in the outcomes of the average child reflect reductions in childbearing among disadvantaged parents, this selection effect could raise the income of the average child (because the poorest children are missing) even though the resources available to any child that is born would not increase. The importance of selection may be quantitatively large, because the federally funded family planning programs affected contraceptive use among more economically disadvantaged women (Jaffe et al. 1973, Torres and Forrest 1985).[1] If, on the other hand, family planning programs directly increase the household incomes of the average child (e.g., by encouraging investments in human capital, careers, and partnerships), our estimates imply a sizable and understudied role of family planning programs as an anti-poverty program.[2]

A bounding exercise informs our understanding of the contribution of selection to our findings. If selection follows the same income distribution as the distribution of incomes of family planning *users*, the exercise suggests that selection accounts for around 36 percent of the total increase in the income of the average child. That is, family planning programs *directly* raised incomes available to children by around $1,200 per year in 2013 dollars (the remaining 64 percent of the rise in children's average incomes). Taking into account the uncertainty in these estimates, the lower confidence interval on the resource effect can explain *at least* 16 percent of the estimated gains, or $300 per year.

This finding underscores an important and understudied return to family planning programs: they appear to directly raise parents' income by allowing them to invest in their human capital, careers, and find stable partnerships. This implies that a large share of the substantial education and labor-market gains to the children of mothers with access to family planning programs (documented in Bailey 2013) may be explained by the program's direct impact on their parents. Accounting only for the effects on child poverty, the resource effect of family planning programs' may reduce child poverty in the U.S. at around half of the cost of the EITC and one quarter of the cost of TANF. Because higher household incomes also increase children's human capital investments, these estimates suggest that the long-term effects of family planning programs on children's economic opportunities may be even greater.

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[1] Family planning programs in the period we study were not means tested, but more affluent women tended to use other sources of care. A related literature suggests the potential importance of abortion legalization via the selection effect is large in both the short run (Gruber et al. 1999) and the longer run (Pop-Eleches 2006, Ananat et al. 2009).

[2] 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 Babiarz (2015) 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.

I. A History of U.S. Family Planning Programs and Their Expected Effects on Children's Outcomes

The architects of President Lyndon B. Johnson's War on Poverty viewed the price of reliable contraceptives as a barrier to reducing poverty and promoting children's economic well-being. Of particular concern was that the "Pill", the first oral contraceptive, was prohibitively costly in the 1960s. An annual supply of oral contraception sold for the equivalent of $812 in 2013 dollars (Bailey 2013)—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). Numerous studies documented at the time that poor women were less likely to use effective contraceptives. We calculate in the 1965 National Fertility Study (NFS), for instance, that poor women were 44 percent (0.082 percentage points / 0.185, s.e. 0.023) less likely to use the Pill than more affluent women. They also had roughly 0.60 (s.e. 0.16) more children on average than families with higher incomes.

To reduce disparities in access to contraception, the Office of Economic Opportunity (OEO) began funding family planning programs as early as 1964. OEO grants rose to roughly $427 million (2013) by 1970 and supported the opening of new clinics in disadvantaged areas and, to a lesser extent, the expansion of existing family planning programs. 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 1970 levels. (See Bailey 2012 and Bailey 2013 for a detailed history of these programs and federal funding.)

The aim of these programs was to bring education, counseling, and the provision of low-cost contraceptives and related medical services to disadvantaged women. Family planning grants did not fund abortion, which was illegal before 1970 except in special circumstances. And, even after abortion was legalized in several states and nationally in 1973, Title X explicitly prohibited the use of federal funds "in programs where abortion is a method of family planning" (§1008). During these early years, little is known about these programs' day-to-day operations. The federal government did not collect information on their services or patients, and officials talked very little about them—perhaps because topics like birth control were taboo.[3] This dearth of evidence limits detailed exploration of the mechanisms of our effects. To the extent that family planning programs varied in their implementation, the effects we estimate will represent the weighted combination of many services and types of programs, all of which provided reduced cost contraceptives and related services.

Between 1964 and 1973, family planning began in different counties around the U.S., providing 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 red; the programs established between 1968 and 1969, during the expansion of family planning as a national emphasis program, are orange; and programs established from 1970 to 1973 under Title X are green. 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.

A. The Impact of Federal Grants on Use of Family Planning Services and Fertility Rates

Between 1969 and 1984, the number of family planning patients at federally funded programs grew 400 percent (Torres and Forrest 1985: 284), but no previous study has documented how much of this increase in the use of services was caused by federal funds. To the extent that patients of federally funded family planning programs had received services elsewhere or paid themselves, this 400 percent increase will overstate the impact of federal grants on the use of family planning services.

Two new data sources uniquely allow this paper to describe the effect of federal funding on the *use* of family planning services and the birth control pill. The first 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, OEO

[3] Levitan (1969: 209) writes 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.

1971, OEO 1974).[4] 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.[5] Panel A of Table 1 shows that federal family planning programs significantly increased the use of family planning services among women counted as "medically indigent" (i.e., those who could not afford care). The share of family planning patients served at all locations who are deemed medically indigent by the provider 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. We also examine whether the expected increase in Pill use was larger among poorer women.[6] The estimates show

[4] 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. This term was defined by the OEO to mean those who could not afford care due to lack of income but they did not recommend a specific income threshold. The definition, therefore, likely varied across providers.

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

[6] We estimate $\Pr(\mathrm{Use}_{ij}) = \mathbf{F}(\mathbf{Z}_{ij}\boldsymbol{\delta} + \theta_1 1(\mathrm{T}_j^* < 1970) + \theta_2 1(\mathrm{Pov}_{ij}) + \theta_3 1(\mathrm{Pov}_{ij}) 1(\mathrm{T}_j^* < 1970))$, 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, 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. Panel B of Table 1 reports average partial effects associated with θ_1 and θ_3 from a probit and bootstrapped standard errors (1000 replications) and capture differences in the use of family planning services and the Pill in funded counties by 1970.

that poor women in areas that had federal family planning programs by 1970 were much more likely to have used the Pill relative to poor women in areas receiving programs after 1970, an increase of around 23 to 30 percent (13 to 17 percentage points) over the mean for poor women (Table 1, panel B). This effect of receiving a federal family planning grant is large enough to erase income-based differentials in Pill use.[7] The sparseness of these data limits strong conclusions, but both analyses suggest that federal grants increased the use of family planning services by a sizable amount.[8]

As a result, family planning also decreased childbearing.[9] 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. The idea behind using the program roll-out is that the timing of implementation approximates the conditional random assignment to greater access to family planning services. 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 up to 15 years later (See Bailey 2012, table 2). Supporting the validity of her 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 fixed effects, state-by-year fixed effects, and a variety of time-varying county-level covariates.[10]

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[7] 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.

[8] The OEO reports were collected at only four points in time, and the 1970 NFS do not have complete coverage of all counties or time periods and exclude never married women.

[9] Reductions in childbearing likely reflect both reductions the number of *wanted* and *unwanted* births and change the *desired* timing of childbirth (Michael and Willis 1976).

[10] 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.

Bailey (2012) provides four additional pieces of evidence to support the internal validity of this research design.[11] First, oral histories and interviews note that the OEO funding was not targeted or well organized. Robert Levine (1970) sums up the situation saying, "It was an era of great administrative confusion." Second, quantitative evidence supports the conditional random assignment assumption. 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. Moreover, 1964 fertility rates or 1960 to 1964 changes in fertility rates are uncorrelated 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.

Third, reproductive and contraceptive attitudes and behaviors in the 1965 NFS are uncorrelated with the initiation of federal family planning programs. These include pro-natalist attitudes, contraceptive use and behaviors, and family background characteristics correlated with the number of children. In addition to examining these outcomes individually, we also follow Kling et al. (2007) and create a summary index of equally weighted average z-scores for these outcomes—a technique that serves to improve the statistical power to detect correlations that move in a common direction. Neither approach finds evidence that federal family planning grants are related to these outcomes individually or when combined in an index (coefficient: 0.054, robust standard error: 0.047, observations: 2,857).[12]

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. 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

[11] For the interested reader, some of this evidence is summarized in Online Appendix A.

[12] This z-score index excludes age at first pregnancy and age at first Pill use from the index. Including the age at first 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 A1, col. 6) because it is often missing and inclusion severely limits our sample size.

short, the qualitative and quantitative evidence is consistent with the implementation timing of federal family planning grants being conditionally, randomly assigned to U.S. counties.

II. The Expected Effects of Family Planning Programs on the Economic Resources of Children

The increased availability of family planning services could affect the economic and living circumstances of children through two main channels: what we call the resource effect and selection.

The resource effect refers to the situation where family planning *directly* raises the incomes of parents. This would be the case if soon-to-be parents could use family planning services to delay childbearing in order to get more education, pursue different career paths, or obtain different amounts of work experience and job training. In addition, women should 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.[13]

Evidence regarding the existence of this resource effect comes from the literature on legal access to the birth control pill for younger women. Using variation in state laws regulating the age of consent for (mostly) unmarried teens (Bailey et al. 2011, 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 career investments. With earlier legal access to the Pill, women and men were more likely to enroll in and complete college (Hock 2008, Bailey et al. 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 et al. 2012). And, as women aged, these investments resulted in higher wages (Bailey et al. 2012). Ananat and Hungerman (2012) additionally show that access to contraception at younger ages improved the

[13] 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.

economic resources of children born to these women, although they provide no evidence on whether these gains accrued due to selection or the resource effect.[14]

Another channel for family planning's resource effect is through 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 2016). If family planning programs increase the likelihood that parents remain together after a child is born, this will also serve to increase the economic resources available to children.[15]

The alternative to the resource effect is the selection effect. Selection refers to the channel through which family planning programs alter who becomes a parent. This shift in the composition of parents could improve the living circumstances of the average child even in the absence of any direct effect of family planning programs on parental income. For instance, if fewer children are born to lower income parents, the resources of the average child could rise even though the incomes of any child that is born do not.

Selection may be a particularly important channel in explaining these programs' effects on children because programs in the 1960s disproportionately served poorer households. 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 et al. 1973, Torres and Forrest 1985: 284).

Separating the resource and selection effects in cross-sectional, retrospective data is impossible to do directly, because the two effects appear identical. Notably, although the literature quantifying the impact of legal abortion on children's outcomes in the U.S. attributes *all* of the gains to selection, this literature's findings could also partially reflect a resource effect. This literature uses 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

[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 et al. 2005).
[15] Akerlof et al. (1996) suggest an alternative model in which family planning programs reduce the cost of non-marital sex and, therefore, increase non-marital childbearing. If non-marital childbearing increases, this should tend to lower household incomes of the average child.

Roe v. Wade in 1973. Empirical studies show that birth rates fell by 4 to 8 percent (Levine et al. 1996) and that the outcomes for cohorts born after abortion was legalized improved. Gruber et al. (1999) shows that children born after abortion legalization were less likely to die as infants, live with single parents or in families receiving welfare, and live in poverty. Ananat et al. (2009) find that, later in life, these children were more likely to graduate from college, less likely to rely on welfare and less likely to be single parents. Donohue and Levitt (2001) show that cohorts born after abortion legalization were less likely to commit crime,[16] and Charles and Stephens (2006) show that these cohorts were less likely to use controlled substances in their late teens. One study of Romania explicitly considers the role of selection. When Romania experienced the reverse policy change—the prohibition of abortion, Pop-Eleches (2006) shows that birth rates among the most educated mothers (who had been using abortion and family planning services) increased by around 30 percent. After accounting for this positive selection of mothers, however, the paper shows that these unintended children fared worse in terms of their later socio-economic outcomes.

In summary, the availability of family planning programs may increase family resources of both younger and older parents. However, family planning programs could also affect children's measured outcomes by altering selection into parenthood. Bailey's (2013) findings that family planning programs improved the adult outcomes is consistent with both channels. Increases in parents' incomes or changes in selection (or a combination of both) could raise college completion rates and raise incomes by 2 percent for the average adult child. Understanding the quantitative importance of the resource and selection channels is, therefore, crucial to interpreting these estimates. The next section describes this paper's empirical strategy to estimate the effects through the combined resource and selection channels. After quantifying these combined effects, we bound the role of selection to recover an estimate of the direct effect of family planning programs on parents' incomes.

[16] Claims that abortion reduced crime are disputed (Donohue and Levitt 2004, Joyce 2004, Dills and Miron 2006, Foote and Goetz 2008).

III. RESEARCH DESIGN AND DATA

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 of residence (this information is suppressed in public samples).[17] Our analysis aggregates the economic resources and living circumstances of children under age 18 into birth-year/county cohorts.[18] These birth-year/county cohorts are then linked to information on when their county received a federally funded family planning program.[19]

Our primary specification exploits variation in the *timing* of the first federal family planning grants to identify the program's effects. In practice, our event-study framework contrasts the evolution of outcomes for cohorts born after family planning programs began to those born before they were available and allows adjustments for a variety of potentially confounding factors:

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

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* —the year the program started.[20] We run separate regressions using the 1970 and 1980 Censuses in order to

[17] Public Census samples identify county groups (which change between 1970 and 1980) and are much smaller. Restricted data are available after a formal application to Census and can be used only in Research Data Centers.

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

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

[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.

recover the regression-adjusted evolution of children's outcomes for cohorts born from six years before and up to six years after each county received its first federal family planning grant for a balanced set of counties. In our analysis, event time, *c*, runs from *a* years before up to *b* years after the date of the first family planning grant, where the 1970 Census covers $a=-7$ through $b=0$, and the 1980 Census covers $a=-3$ through $b=7$. We do not pool the two Censuses because the 1970 data identifies τ with only the 461 counties funded prior to 1970, whereas in the 1980 data the full set of funded counties identify τ. Both Censuses, therefore, independently estimate event years -3 through 0, and using both Censuses allows us to estimate τ using a balanced set of counties for the greatest range of event time.[21]

Our baseline specification includes θ, a set of county fixed effects which capture time-invariant county-level differences, a set of birth-year fixed effects, and γ, and 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. We present unweighted results (Solon et al. 2015). Standard errors are robust to heteroskedasticity and corrected for serial correlation within state (Arellano 1987, Bertrand et al. 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) (Almond et al. 2011). In some specifications, we also include the number of

[21] The 1980 Census only covers 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. Using the 1970 Census, we set $a=-7$ when $c\leq-7$ and $b=1$ when $c\geq1$, and event years -6 through 0 are estimated using all funded counties. $c=0$ 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 (Bailey 2012).[22]

An important challenge to our analysis is that the Censuses contain information on a child's residence in (or five years before) the Census year—not residence at the time of the child's birth. If mothers move between the time of birth and observation in the Census, this could lead us to misclassify mothers' access to federal family planning around the time of conception. We take several steps to limit this mobility induced misclassification. First, we use county of residence five years before the Census, because 1975 is more temporally proximate to the year of birth for cohorts of children identifying our parameters of interest (and, therefore, more strongly correlated with mother's exposure to family planning) than 1980; we use 1965 for the 1970 Census for consistency. Second, we exclude unfunded areas from our estimation sample, because of their differential mobility relative to funded areas after family planning programs started. Finally, we follow Card and Krueger (1992) to adjust for mobility using a post-estimation correction.[23] (See Online Appendix B for a discussion and additional results related to corrections for mobility bias.)

IV. THE ESTIMATED EFFECTS OF FAMILY PLANNING PROGRAMS ON THE ECONOMIC RESOURCES OF CHILDREN

We begin by examining the effects of family planning programs on the log household income of the average child born. Figure 2 presents results from our baseline model, which includes county and state-by-birth-year fixed effects. Estimates to the left of the vertical axis represent cohorts born in event years

[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.

[23] Card and Krueger note that the estimated coefficients affected by mobility, τ_c, are a reweighting of the true coefficients, τ_j^*. In our case, $\tau_c = \sum_{j=a}^{b} \tau_j^* \cdot p_{j,c}$, where $p_{j,c}$ 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. We use the matrix form of the equation to recover the full set of estimates, $\boldsymbol{\tau}^* = \boldsymbol{\tau}\boldsymbol{P}^{-\mathbf{1}}$. $\boldsymbol{\tau}$ 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 P is a (b-a+1) × (b-a+1) matrix with elements of the transition probabilities, $p_{j,c}$, such that $\boldsymbol{\tau} = \boldsymbol{\tau}^*\boldsymbol{P}$. 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. 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.

before family planning programs began (1970 Census estimates plotted in dashed lines), 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 plot 95-percent, point-wise confidence intervals for this model.[24]

Consistent with the resource and the selection effects operating as expected, Figure 2 shows that cohorts born after federal family planning programs began had significantly higher household incomes. Although the household income of birth cohorts was stable before family planning programs began, the introduction of family planning programs corresponds to a notable trend break. Table 2 demonstrates the robustness of these estimates to different control variables and mobility bias adjustments for the 1980 Census. Column 1 includes only county and year fixed effects, column 2 (our baseline model and preferred specification) adds 850 state-by-year fixed effects (50 states *17 birth years), column 3 adds county-level covariates, column 4 adds controls for abortion providers, and column 5 adjusts our baseline model for mobility bias using Card and Krueger's (1992) post-estimation correction. Across specifications, cohorts born five years after the program began had household incomes that were very similar—2.7 percent higher in our baseline specification (col. 2). Consistent with mobility attenuating our estimates, the Card and Krueger (1992) mobility correction raises the estimate modestly to 3.0 percent (col. 5). In contrast, the coefficients on household incomes among cohorts 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.[25] Our baseline specification implies that the income of the average child was around $1,840 higher ($68,000 pre-program mean family income × 0.027) among children born after family planning programs began.[26]

[24] For the interested reader, estimates for 1970 and additional robustness checks are presented in Online Appendix C.

[25] Online appendices present all point estimates for the 1970 and 1980 Censuses, robustness checks, and the Card and Krueger (1992) correction for mobility. These sensitivity checks do not alter the conclusions presented here.

[26] This intention-to-treat estimate is not easy to rescale into a LATE (Imbens and Angrist 1994), because we do not have an estimate of the first stage. If we could measure a first stage, it would include use of new methods like the Pill (as in Table 1, panel B) as well as the more effective use of methods. Lapses in use or errors in use are an important cause of unintended pregnancy even today. Forty-one percent of unintended pregnancies occur among women who

Because the restricted Census data are not top-coded, we also investigate the importance of outliers. Trimming the top and bottom 1 percent of children's household incomes, however, has little effect on these estimates.[27] Another check 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 in cohorts born five years after the program began had a statistically significant 3.5 percent higher per-capita household income (appendix table A5, col 2).

Increases in the income of the average child should be driven by households in the lower part of the income distribution. As noted previously, eighty-three percent of family planning patients had incomes below 150 percent of the poverty line. Consistent with this prediction, Figure 3 and Table 3 present evidence of sharp reductions in measures of children's economic disadvantage. To simplify the interpretation of the coefficients, Figure 3 plots 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. Table 3 presents estimates in levels and also summarizes the relevant pre-treatment means.

The results show that cohorts born 5 years after federal family planning programs began were 7.4 percent less likely to live in poverty (Figure 3A; Table 3, col. 1), 6.4 percent less likely to live below 1.5 times the poverty line (Table 3, col. 2), and 4.3 percent less likely to live below 2 times the poverty line (Table 3, col. 3). As was the case with household income, Figure 3A shows no evidence that these reductions reflect a pre-trend. These results are consistent with family planning programs having the largest effects on lower income women.

Administrative statistics also suggest that nonwhite women were overrepresented among family planning patients (30 percent of family planning patients versus only 17 percent of women of childbearing

are *using* contraception in the month they become pregnant (Sonfield et al. 2014). Effective use includes the continuity of use which is not well measured in the NFS (or other modern fertility surveys).

[27] 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.

age). This suggests that the intention-to-treat effects of family planning might be larger among nonwhite children. Consistent with this prediction, Table 3 shows that the absolute reductions in poverty rates were two times larger for the average nonwhite child born 5 years after the program began (-1.5, s.e. 1.6) than the average white child (-0.8, s.e. 0.6). Owing to smaller samples, the event study estimates for subgroups are imprecise. To increase precision, we summarize the event-study estimates in a standard differences-in-differences framework. Using a single dummy variable for the affected cohorts, we find that poverty rates among non-white children fell by a statistically significant 5.5 percent (-2.0/34.8, s.e. 0.81, not reported in tables for brevity) over the first six years after family planning programs began. For whites, poverty rates fell by an insignificant 2 percent (-0.28/14, s.e. 0.29). For nonwhite children, the effects of family planning programs are also generally larger for lower poverty thresholds, although imprecision limits strong conclusions about comparisons across thresholds.

Of course, reductions in child poverty could occur mechanically if family planning reduced the number of children in the household (which could lower the poverty threshold), even if household incomes did not change (i.e., the resource effect is zero).[28] 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 evidence foreshadows one of the paper's main conclusions that the resource effect plays an important role in explaining the improvement in children's living circumstances.

The introduction of family planning programs is also associated with a reduction in another measure of disadvantage: the share of children living in families receiving public assistance. Children born 5 years after family planning programs began were on average 12 percent less likely to live in households receiving public assistance (Figure 3B, Table 4, col. 1) relative to those born just before family planning

[28] 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.

programs began. While the magnitude of the coefficients suggests that the reductions mainly occurred among whites, the confidence intervals are too large to conclude that the effect of family planning on receiving public assistance differs by race. White cohorts born 5 years after family planning programs began were 10 percent less likely to live in households receiving public assistance (col. 2). Although a greater share of non-white children lived in households receiving public assistance, the event-study estimates for nonwhite children are much less precise and noisier, exhibiting both positive to negative-signed coefficients (col. 3).

A final measure of children's disadvantage is living in a household with a single head. Non-marital childbearing and single headship rose dramatically in the 1960s and 1970s (McLanahan and Watson 2011, Bailey et al. 2014), and both trends have served to increase child poverty over the longer-term. If family planning programs encouraged these trends by reducing the cost of non-marital sex, they could have increased non-marital childbearing (Akerlof et al. 1996). On the other hand, family planning programs could have dampened the longer-term increase in non-marital childbearing due to the reduced unwanted pregnancies among less committed couples.

Figure 3C shows evidence of the latter. Following the introduction of family planning programs, the share of children born to single heads decreased. Although the 95-percent confidence intervals in figure 3C (Table 4, col. 4) include zero, a one-sided test rejects the hypothesis that the introduction of family planning programs increased the share of children living with single parents. As in the case of public assistance, these patterns are less noisy and more precisely estimated for white relative to nonwhite children (Table 4, cols. 5 and 6). To investigate this result further, we conduct a complementary analysis of county-level Vital Statistics for marriage and divorce. Using marriages and divorces per woman ages 15 to 44 as the dependent variables in the same specification (and replacing cohort with year), we fail to reject that marriage and divorce rates remained the same when family planning programs began (appendix figure A4).

Two final analyses help understand how family planning programs affected these outcomes. 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[29]) as dependent variables, we find that age of the average child's mother fell by around 0.3 years by year 5, a reduction of 3.6 months (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 births to older women contribute relatively less to the overall birth rate than births to younger women, the potential reduction in age by avoiding an unwanted pregnancy in one's thirties is much larger (e.g., 5 to 10 years on average) than the increase in age by delaying a pregnancy (e.g., 1 to 3 years on average). On net, these opposing changes average out to a small negative number, suggesting that the share-weighted reduction in age was slightly larger than the share-weighted delay in birth.

The reduction in older siblings is also consistent with family planning programs helping older women avoid unplanned births. Children born 5 years after family planning programs began had significantly fewer older siblings, implying that they were more likely to be lower-order births (first or second children rather than third or higher) (Table 5, col. 4). Evidence using changes in parity-specific birth rates 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. Changes in the living circumstances of the average child, therefore, appear to be driven by older mothers having fewer children. Because wages tend to increase with age, this makes the income and poverty results even more striking.

V. A FRAMEWORK FOR BOUNDING THE IMPORTANCE OF SELECTION

To what extent do improved living circumstances for children born after family planning programs began reflect the selection effect (i.e. reductions in childbearing among disadvantaged parents) or the resource effect (i.e. increased investments by parents in their own human capital, careers, and partnerships)?

[29] 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.

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. This section develops a novel framework for bounding the importance of selection, which allows us to approximate the resource effect. First, we show that empirical estimates of the overall effect of family planning programs can be decomposed into the sum of the selection and resource effects. Second, we use this decomposition to quantify the role of selection in raising the income of the average child.

Let $g(y) \geq 0$ represent the empirical distribution of the number of children across household income, y, before family planning programs began. We model income in discrete bins rather than as a continuous variable to match how it is reported in Census data and for simplicity of exposition. Summing over all households, $\sum_{y=0}^{\infty} g(y) = N$ is the total number of children and the household income of the average child can be written as $\mu \equiv \frac{1}{N}\sum_{y=0}^{\infty} yg(y)$. The introduction of a family planning program may change this average in two ways. First, selection may lower the number of children that lower income families have. Let $g^{fp}(y) \geq 0$ capture the empirical distribution of the number of children by household income in the context of a family planning program. Note, $\sum_{y=0}^{\infty} g^{fp}(y) = M$, where M<N and represents the number of children in all households after family planning programs. The inequality is strict because family planning programs strictly lower the number of children born. If family planning operated exclusively via the selection effect, then ${\mu_0}^{fp} \equiv \frac{1}{M}\sum_{y=0}^{\infty} yg^{fp}(y) > \frac{1}{N}\sum_{y=0}^{\infty} yg(y) \equiv \mu$, as fewer children were born to lower income families. Second, family planning programs may directly raise the incomes of parents through the resource effect which we capture as $h(y) \geq y$. This resource effect would further increase the income of the average child, such that ${\mu_1}^{fp} = \frac{1}{M}\sum_{y=0}^{\infty} h(y)g^{fp}\big(h(y)\big) > {\mu_0}^{fp} > \mu$.

Our estimates, therefore, capture the treatment effects of family planning programs through both the resource and selection channels, which we can write as follows:

(3) $$\tau^{fp} \equiv {\mu_1}^{fp} - \mu = \sum_{y=0}^{\infty}\left(\frac{1}{M}h(y)g^{fp}\big(h(y)\big) - \frac{1}{N}yg(y)\right)$$

By adding and subtracting a cross-term, we can decompose this effect into these two components,[30]

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

On one hand, if we assume family planning programs do not have a direct resource effect (i.e. $h(y) = y$), the second part of the expression (the resource effect) becomes zero and the treatment effect represents selection alone. On the other hand, if we assume the selection effect is zero (i.e. *M*=*N* and $g^{fp}(\quad)$=$g(\quad)$), the treatment effect reflects only the resource effect.

We cannot estimate the selection effect directly. Instead, we use this equation to simulate the role of selection and then bound the resource effect. Our approach uses the 1960 Census data and family planning user characteristics from the 1970 NFS. We simulate two types of selection: (1) “lower truncation” which assumes that missing births were at the bottom of the income distribution, and (2) “likely selection” which assumes missing births followed the observed empirical distribution of the users of family planning services.

Table 6 presents the results from the bounding exercise. The first row of the table assumes the selection effect is zero; by construction the resource effect explains the entirety of the estimated 2.75 percent increase in children’s household income (unweighted average of effects for cohorts born 4 to 6 years after family planning, col. 2 of Table 2).To simulate lower truncation, we drop the poorest 2 percent of children from the 1960 Census which corresponds to Bailey’s (2012) estimates of the effect of family planning programs on fertility rates.[31] This approach assumes that only the *poorest* parents use family planning

[30] We obtain this result by adding and subtracting $\frac{1}{N}g(y)h(y)$ and $\frac{1}{N}g(h(y))h(y)$. This approach is standard in decompositions, the weighting of the selection and resource effects will vary with the choice of this cross-term.

[31] We restrict our sample to the average age of children born 4 to 6 years after family planning programs started in our 1960 sample (ages 5 to 7) of funded counties. This results in truncation of children with household incomes on average below $3442 in 2013 dollars annually in the 1959 distribution of children’s household income. These estimates correspond to Bailey (2012)’s weighted estimates averaged over event years 4 to 6 in Figure 6A and unweighted estimates, model 3, in Figure 6B.

programs to avert births.[32] We simulate likely selection by assuming that missing births followed the empirical distribution of household income for the users of family planning services. We fit a log normal distribution to the household incomes reported by family planning patients in the 1970 NFS which results in a 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. We then remove a random 2 percent of children from the 1960 Census sample of children ages 5 to 7 from this distribution 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 matches the poverty rate of family planning program users in administrative data (83 percent).

The second and third rows of Table 6 simulate the resource effects under lower truncation and empirically likely selection. The 95-percent confidence intervals for the estimates are generated using a non-parametric bootstrap procedure (Johnston and DiNardo 1997).[33] Selection by truncation generates a 1.84 percent increase in household income, accounting for around 67 percent (1.84/2.75) of the increase in income of the average child, while the resource effect accounts for around 33 percent. The largest effect of selection (implied by its upper confidence interval of 2.0) suggests that selection can explain at most 72 percent of the gains. If we also account for the uncertainty in the estimate of the increase in income, however, we fail to reject that the resource effect could be zero and that the most extreme form of selection explains the entire increase in household income.

Likely selection can explain around 36 percent (1 percentage point) of the estimated increase in income of the average child. Our best estimate of the magnitude of the resource effect is that it accounts for

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[32] The general equation for selection can be written as, $\tau^S \equiv \mu^S - \mu = \frac{1}{M}\sum y\left[g^{fp}(y) - \frac{M}{N}g(y)\right]$. For the case of lower truncation, $\mu^s = \frac{1}{M}\sum_{y>T} yg(y)$ where the mean captures the household income of the average child after removing all children below income T, the truncation point.

[33] 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 remaining 64 percent (1.75/2.75) of the increase in child income. Taking account of the uncertainty in the estimate of the increase in income as well as the simulated effect of selection, the lower 95-percent confidence interval on the resource effect implies that it accounts for *at least* 16 percent of the income gains to the average child. In short, the simulation rejects that the resource effect is zero under likely selection. This finding underscores an important and understudied return to family planning programs: they may *directly* raise parents' income by encouraging parents to invest in their human capital, careers, and partnerships.

VI. FUNDING FAMILY PLANNING AS AN ANTI-POVERTY PROGRAM

More than 50 years after the first family planning programs began in the United States, this paper provides new evidence that they increased children's economic resources and reduced child poverty. Using a validated research design and large, restricted-use Census samples, we find that children born after a federal family planning program began were 7 percent less likely to live in poverty and 12 percent less likely to live in households receiving public assistance. A bounding exercise suggests that these increases in the income of the average child were achieved through the sizable *increases* in the incomes of disadvantaged parents—net of changes in selection. Our bounding exercise using the distribution of incomes of family planning users shows that the direct effect of family planning programs on the incomes of parents accounts for around 64 percent of the income gains for the average child. Although our data do not allow us to establish the exact mechanisms for this effect, they are consistent with family planning programs relaxing the constraints of unintended pregnancy and encouraging greater investments by parents in their human capital, careers, and partnership decisions. To the extent there were spill-overs in services to nearby counties, older siblings (who are part of our comparison group) benefitted, or parents' geographic mobility obscures our ability to measure the true effects, our results may understate the resource effects of family planning programs on children's economic resources.

The magnitudes of the estimated effects of family planning on income square well with others in the literature. For instance, our bounding exercise suggests that family planning programs directly increased the income of the average child by \$1,200 (0.0275 × the average pre-family planning mean family income of \$68,000 in 2013 dollars × share due to the resource effect of family planning net of selection, or 1.75/2.75). Using Bastian and Michelmore's (forthcoming) estimate that college completion should rise by 4.2 percent for every \$1,000 increase in EITC income, our findings imply that college completion of children born to parents with access to family planning should increase by around 5 percent. This estimate is similar to the findings in Bailey (2013), who shows that the income and selection effects of family planning programs together raised children's likelihood of college graduation by 2 to 7 percent.[34]

The magnitudes of these estimates also imply that the returns to family planning programs compare favorably 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). Using our estimates of likely selection, family planning programs directly reduced child poverty by 1 percent for just under \$1 billion (\$4.4 billion/(7*0.64), where 7 is the percent reduction in child poverty (Table 3) and 0.64 is the share of each point attributable to the resource effect (Table 6, col. 3, likely selection).

Comparisons of the resource effects of family planning to other programs targeting child poverty place these estimates in perspective. 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, this 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, even when we ignore the effect of family planning programs on public assistance outlays and through the selection channel, the

[34] Note that Bailey's (2013) findings may be affected by mobility from birth to adulthood.

effects of family planning programs on parents' incomes reduced child poverty at less than half of the cost of the EITC and roughly one quarter of the cost of TANF.[35]

These resource effects may translate into large and longer-term gains for children. 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 et al. (2014) study of U.S. tax credits find comparable estimates. Aizer and Currie (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 Schultz 2008 for a discussion of developing countries). Future work should investigate these longer-run linkages as well as the intergenerational impact of family planning programs on the economy.

VII. References

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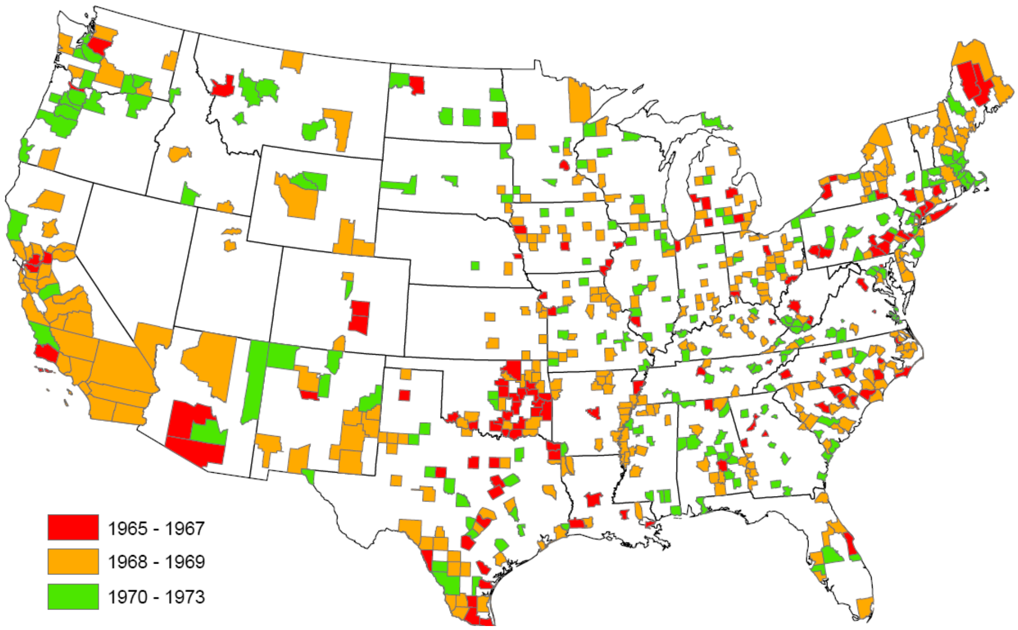
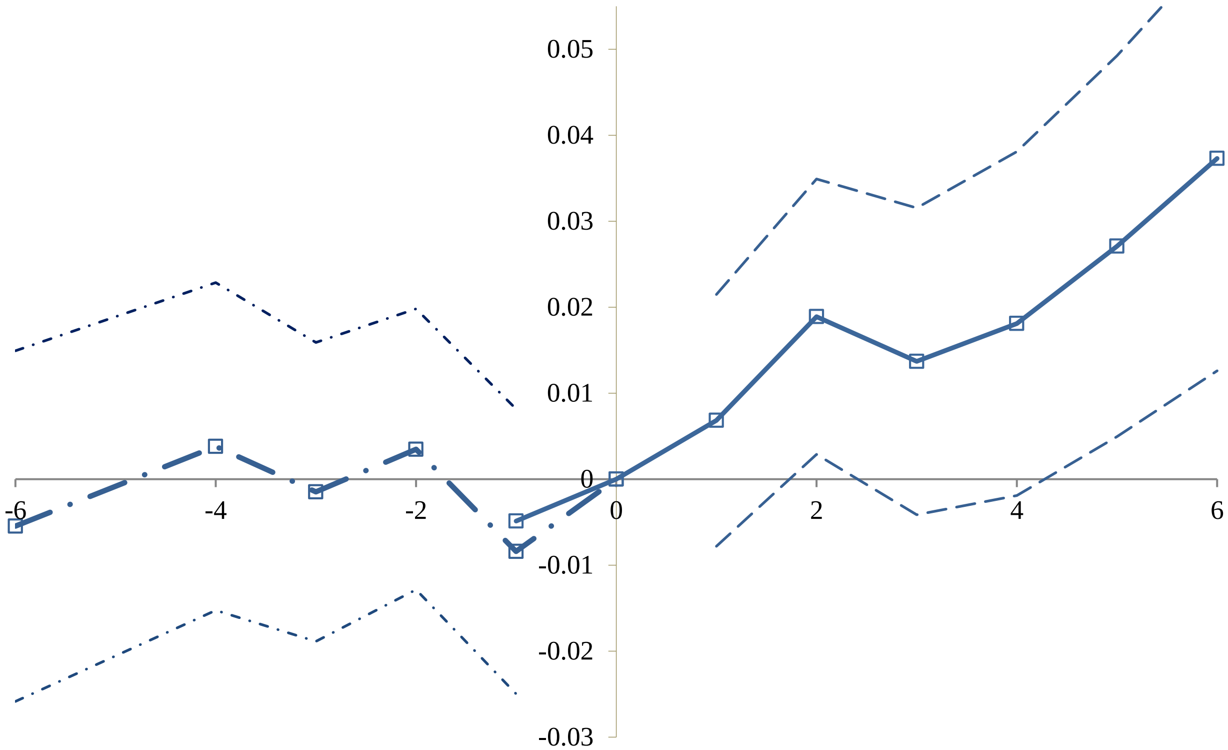
Figure 1. The Roll-Out of Federally Funded Family Planning Programs, 1965-1973



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. Source: Bailey (2012).

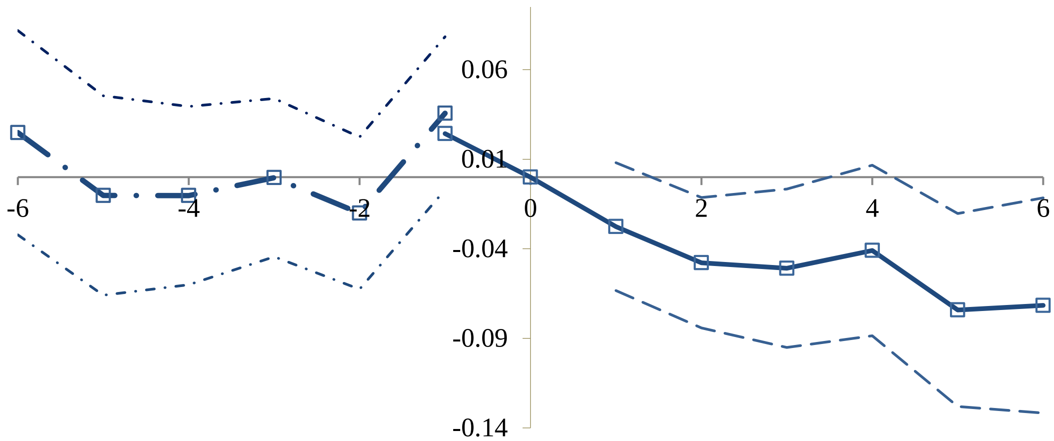
Figure 2. The Effect of Family Planning Programs on Log Household Income of the Average Child



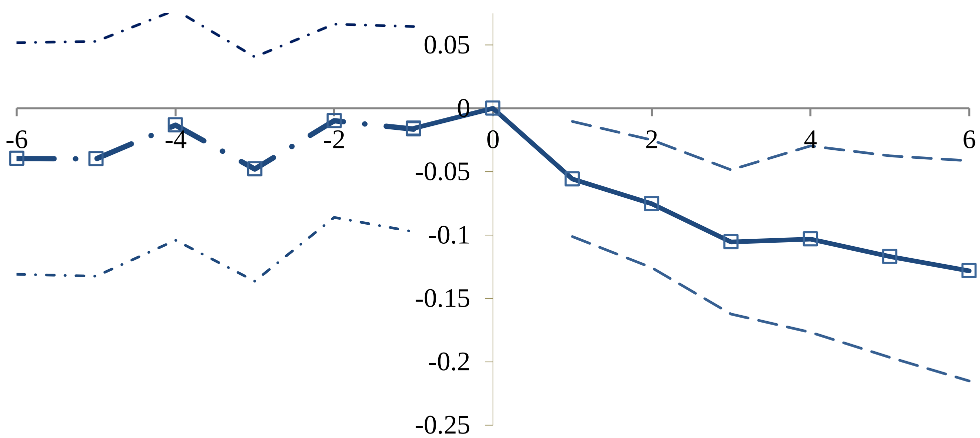
The figure plots the percent change in household income (dependent variable) for children born up to 6 years before and up to 6 years after family planning programs began using the baseline model from estimating equation 1 (see also Table 2 and Table A4). The x-axis plots children's birth year relative to the year the county's family planning program started. As covariates, the baseline model includes county, birth year, and state by birth year fixed effects. 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.

Figure 3. The Effect of Family Planning Programs on the Economic Disadvantage of the Average Child

A. Percent Change in Children in Poverty



B. Percent Change in Children in Households Receiving Public Assistance



C. Percent Change in Children Living in Single-Parent Households

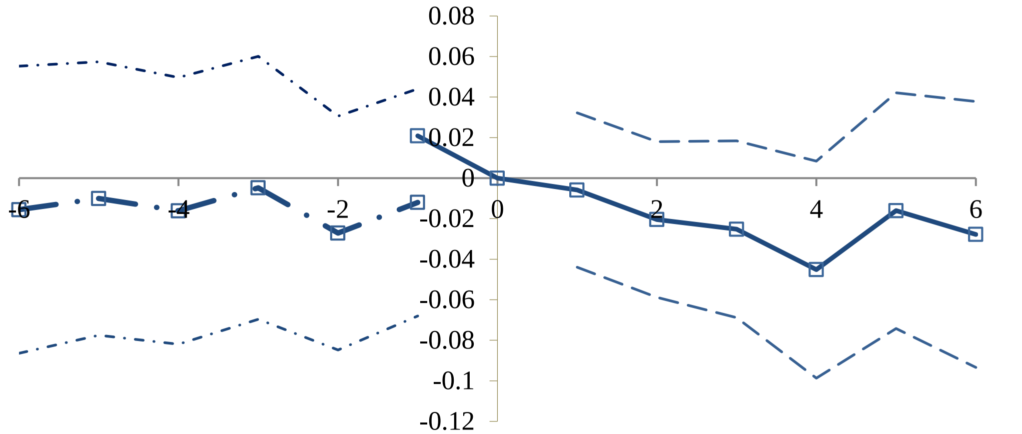


Figure plots estimates of τ from estimating equation 1 divided by the pre-treatment mean dependent variable for both the 1970 and 1980 estimates. The series, therefore, denote changes in *percent* of children in a birth cohort with a given characteristics relative to the cohort born in the year the family planning program began. See also notes for Figure 2 and estimates in Table 3 and Tables A6, A9, A10.

Table 1. The Effect of Family Planning Programs on the Use of Contraception by Poor Women

	(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		G,C,E,P	G,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 (G), Catholic (C), educational achievement (E), and the size of the primary sampling unit (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). Medically indigent: who could not afford care due to lack of income or insurance eligibility. Source: 1970 NFS.

Table 2. The Effect of Family Planning Programs on Log Household Income of the Average Child

	(1)	(2)	(3)	(4)	(5)
	Dependent Variable: Log Household Income Pre-treatment mean of household income[a]: $68,000				
Event time: Cohort year of birth – year family planning program began					
-2	0.004	0.001	0.001	0.001	0.002
	[0.00680]	[0.00747]	[0.00746]	[0.00747]	[0.00741]
-1	-0.001	-0.005	-0.005	-0.005	-0.006
	[0.00616]	[0.00665]	[0.00662]	[0.00663]	[0.00626]
0 (omitted)					
1	0.006	0.007	0.007	0.007	0.007
	[0.00691]	[0.00747]	[0.00748]	[0.00749]	[0.00732]
2	0.016	0.019	0.019	0.019	0.021
	[0.00754]	[0.00817]	[0.00817]	[0.00818]	[0.00835]
3	0.011	0.014	0.014	0.013	0.014
	[0.00807]	[0.00910]	[0.00911]	[0.00915]	[0.00946]
4	0.015	0.018	0.018	0.018	0.019
	[0.00911]	[0.0102]	[0.0103]	[0.0103]	[0.0106]
5	0.024	0.027	0.027	0.026	0.030
	[0.00996]	[0.0113]	[0.0115]	[0.0115]	[0.0118]
6	0.033	0.037	0.037	0.036	0.043
	[0.0114]	[0.0126]	[0.0127]	[0.0128]	[0.0131]
R-squared	0.345	0.413	0.414	0.414	0.413
Covariates [b]	C, Y	C, Y, S-Y	C, S-Y, R	C, S-Y, R, A	C, Y, S-Y, mobility adjusted
County × birth year cells [c]	11313	11313	11313	11313	11313
Counties	666	666	666	666	666

The table presents point estimates of the change in log household income of children for cohorts born 2 years before and 6 years after family planning programs began. Coefficients are least-squares estimates of τ in equation 1 using the 1980 restricted-use Census data. See Online Appendix C for 1970 point estimates. Figure 2 plots event study estimates from the baseline model. Heteroskedasticity-robust standard errors clustered by county are in brackets. [a] Pre-treatment mean in 1980 is the mean of the dependent variable in event years t=0, t=−1 and t=−2 in 2013 dollars. [b] Covariate abbreviations are as follows: C and Y denote county and birth year fixed effects. S-Y denotes state-by-birth year fixed effects. R and A indicate REIS variables and abortion access measures. Column 5 includes estimates using baseline model (column 2) with Card and Krueger's (1992) post-estimation correction for mobility bias. [c] The sample sizes for 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.

Table 3. The Effect of Family Planning Programs on the Poverty of the Average Child

	(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]	*18.89*	*30.77*	*43.16*	*14.04*	*24.47*	*36.81*	*34.83*	*51.3*	*64.86*
Event time: Cohort year of birth – year family planning program began									
-2	0.737	0.175	0.139	0.540	0.034	-0.025	0.946	0.020	0.073
	[0.414]	[0.383]	[0.415]	[0.428]	[0.413]	[0.481]	[1.113]	[1.177]	[1.204]
-1	0.459	0.067	0.381	0.545	0.219	0.679	0.779	-0.305	-0.005
	[0.370]	[0.400]	[0.404]	[0.382]	[0.448]	[0.482]	[1.156]	[1.202]	[1.151]
0 omitted									
1	-0.522	-0.786	-0.523	0.177	-0.236	-0.070	-0.993	-1.325	-0.242
	[0.344]	[0.408]	[0.438]	[0.380]	[0.503]	[0.554]	[1.063]	[1.070]	[1.074]
2	-0.903	-1.182	-0.918	-0.391	-0.826	-0.256	-2.148	-0.795	-1.111
	[0.351]	[0.437]	[0.440]	[0.360]	[0.475]	[0.511]	[1.223]	[1.248]	[1.243]
3	-0.961	-1.003	-0.651	-0.382	-0.399	0.088	-2.704	-2.133	-2.026
	[0.426]	[0.491]	[0.525]	[0.467]	[0.554]	[0.632]	[1.179]	[1.277]	[1.355]
4	-0.773	-1.460	-0.755	0.015	-0.721	0.071	-3.007	-2.585	-0.619
	[0.459]	[0.571]	[0.593]	[0.492]	[0.647]	[0.731]	[1.407]	[1.565]	[1.635]
5	-1.401	-1.979	-1.860	-0.819	-1.468	-0.950	-1.582	-1.272	-1.509
	[0.519]	[0.652]	[0.677]	[0.572]	[0.729]	[0.825]	[1.636]	[1.790]	[1.888]
6	-1.353	-1.672	-1.124	-0.227	-0.855	-0.288	-2.195	-0.694	0.999
	[0.579]	[0.738]	[0.768]	[0.616]	[0.813]	[0.897]	[1.793]	[1.951]	[1.973]
R-squared	0.092	0.130	0.191	0.080	0.110	0.163	0.111	0.114	0.126

All estimates are for the baseline model. In columns 1-6, the number of observations and counties are identical to those reported Table 2. For columns 7-9, only 529 counties have sufficient numbers of nonwhites for inclusion. These estimates are based on 8,855 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; 1,276,760; 1,237,436; and 1,214,314, 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. See notes for Table 2.

Table 4. The Effect of Family Planning Programs on the Likelihood the Average Child Lives in a Household Receiving Public Assistance or Has a Single Parent

	(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]	*11.4*	*7.7*	*24.8*	*17.4*	*13.4*	*35.0*
Event time: Cohort year of birth – year family planning program began						
-2	-0.052	-0.046	-0.048	-0.438	-0.586	1.322
	[0.299]	[0.272]	[0.986]	[0.335]	[0.353]	[1.243]
-1	-0.180	-0.016	0.343	0.363	-0.109	1.588
	[0.282]	[0.251]	[1.099]	[0.349]	[0.362]	[1.139]
0 omitted						
1	-0.638	-0.455	0.496	-0.102	-0.092	1.763
	[0.265]	[0.249]	[1.055]	[0.338]	[0.316]	[1.210]
2	-0.862	-0.554	-0.648	-0.355	-0.282	0.457
	[0.294]	[0.256]	[1.013]	[0.341]	[0.360]	[1.223]
3	-1.206	-0.738	-1.731	-0.438	-0.009	-1.300
	[0.332]	[0.307]	[1.161]	[0.387]	[0.392]	[1.332]
4	-1.180	-0.687	0.274	-0.785	-0.492	-0.270
	[0.429]	[0.364]	[1.316]	[0.475]	[0.453]	[1.515]
5	-1.338	-0.805	-0.344	-0.279	-0.172	1.696
	[0.464]	[0.433]	[1.566]	[0.516]	[0.519]	[1.689]
6	-1.467	-1.087	0.776	-0.483	-0.143	0.753
	[0.507]	[0.482]	[1.665]	[0.582]	[0.576]	[1.890]
R-squared	0.102	0.084	0.122	0.095	0.123	0.115

See notes for Table 2 and 3.

Table 5. The Effect of Family Planning on the Age of Mother and the Number of Older Siblings of the Average Child

	(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]	25.04	25.03	24.75	1.535	1.355	2.044
Event time: Cohort year of birth – year family planning program began						
-2	0.045	0.083	0.206	-0.020	-0.016	0.047
	[0.0638]	[0.0675]	[0.190]	[0.0226]	[0.0225]	[0.0650]
-1	0.078	0.070	0.526	0.013	0.007	0.085
	[0.0603]	[0.0649]	[0.173]	[0.0198]	[0.0192]	[0.0595]
0 omitted						
1	-0.003	0.010	0.335	-0.017	0.004	0.037
	[0.0570]	[0.0612]	[0.170]	[0.0183]	[0.0187]	[0.0569]
2	-0.114	-0.047	0.000	-0.031	-0.003	-0.013
	[0.0652]	[0.0692]	[0.198]	[0.0191]	[0.0186]	[0.0570]
3	-0.182	-0.132	-0.099	-0.038	-0.009	-0.035
	[0.0700]	[0.0767]	[0.220]	[0.0210]	[0.0208]	[0.0616]
4	-0.193	-0.140	0.010	-0.061	-0.013	-0.013
	[0.0859]	[0.0892]	[0.266]	[0.0271]	[0.0253]	[0.0818]
5	-0.303	-0.220	-0.252	-0.069	-0.015	-0.122
	[0.0987]	[0.101]	[0.288]	[0.0297]	[0.0279]	[0.0842]
6	-0.276	-0.173	-0.497	-0.064	-0.012	-0.115
	[0.104]	[0.111]	[0.293]	[0.0336]	[0.0303]	[0.0875]
R-squared	0.203	0.140	0.159	0.541	0.454	0.325

See notes for Table 2 and Table 3.

Table 6. The Resource Effect of Family Planning under Different Assumptions about Selection

	(1) Simulated Selection Effect	(2) Resulting Resource Effect	(3) Share due to the Resource Effect
No selection	--	2.75	100%
		[1.46, 4.04]	
Selection by lower truncation	1.84	0.91	33.1%
	[1.69, 2.00]	[-0.39, 2.21]	
Empirically likely selection	1.00	1.75	63.6%
	[0.86, 1.15]	[0.45, 3.05]	

The results are very similar if we restrict the distribution of children's household incomes to be positive. 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 the restricted 1960 Census.

[Click here for link to Online Appendices]