Fifty Years of Family Planning: New Evidence on the Long-Run Effects of Increasing Access to Contraception

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Abstract

This paper assembles new evidence on some of the longer-term consequences of U.S. family planning policies, defined in this paper as those increasing legal or financial access to modern contraceptives. The analysis leverages two large policy changes that occurred during the 1960s and 1970s: first, the interaction of the birth control pill's introduction with Comstock-era restrictions on the sale of contraceptives and the repeal of these laws after *Griswold v. Connecticut* in 1965; and second, the expansion of federal funding for local family planning programs from 1964 to 1973. Building on previous research that demonstrates both policies' effects on fertility rates, I find suggestive evidence that individuals' access to contraceptives increased their children's college completion, labor force participation, wages, and family incomes decades later.

Family planning policies, defined in this paper as those increasing legal or financial access to modern contraceptives and related education and medical services, have grown increasingly controversial over the last decade.[1] In 2010 and 2011, congressional Republicans supported proposals to cut family planning funding through Title X of the Public Health Service Act, which funds U.S. family planning clinics serving over 4 million women (Cohen 2011). This represents a significant departure from the bipartisan support enjoyed by these programs over the last 40 years. The first legislation authorizing a national family planning program passed in 1970 with the strong support of Republican President Richard Nixon. In fact, public opinion surveys indicate that support for family planning programs was *stronger* at that time among Republicans than among Democrats.[2]

Much of the current debate surrounding family planning focuses on women's reproductive rights and health. In the 1960s, however, proponents of these programs often emphasized their links to the economy. Both President Lyndon Johnson and President Nixon stressed how family planning programs would promote the opportunities of children and families and thus drive economic growth. This reasoning is consistent with a long theoretical tradition in economics, including standard formulations of the quantity-quality models of investments in children (Becker and Lewis 1973, Willis 1973, Hotz, Klerman, and Willis 1997) and

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[1]In this paper I do not consider the effects of policies regarding abortion. I refer the interested reader to the large literature in economics on this topic. See, for instance, Levine and others (1999), Gruber, Levine, and Staiger (1999), Donahue and Levitt (2001), Charles and Stephens (2006), Foote and Goetz (2008), and Ananat and others (2009).

[2]Today the situation is reversed, with Democrats slightly more favorable.

standard formulations of the importance of family size and credit constraints in limiting children's human capital investment (Becker and Tomes 1979, 1986).[3] Through changes in fertility rates and these human capital channels, family planning policies could directly affect the long-run growth of the economy (Becker, Murphy, and Tamura 1990).

The empirical literature provides evidence consistent with causal links running from family planning to children's adult outcomes. It is well known that poorer families have more children than more affluent families. It has also known that children from poorer families receive fewer parental time and resource investments (Guryan, Hurst, and Kearney 2008), and that they are more likely to experience delayed academic development and health problems, live in more dangerous neighborhoods, and attend underperforming schools (Levine and Zimmerman 2010). Children from poorer households are less likely to graduate from high school and to complete college (Bailey and Dynarski 2011), which limits their earnings potential later in life. Ultimately, over 40 percent of children born to parents in the lowest quintile of family income remain in that income quintile as adults (Pew Charitable Trusts 2012, figure 3, p. 6).

However, the extent to which growing up in a larger family per se *causes* adult disadvantage is unclear. Poverty itself may directly affect adult outcomes through channels such as inadequate nutrition, poor health care, and limited access to quality education. That said, larger family size may have an independent and direct effect on adult outcomes, for instance by reducing the amount of time parents spend with each child or reducing resources available for each child's education. Further complicating the measurement of these relationships, poorer families tend to have more children. Consequently, the empirical literature provides little guidance regarding the long-run implications of current proposals to cut federal funding for family planning or to alter funding for family planning services for Medicaid recipients.

This paper provides new evidence on the relationship between family planning and long-term economic outcomes such as educational attainment, labor supply, and family income. The analysis exploits two large policy changes during the 1960s and 1970s: the first is the interaction of the birth control pill's introduction with Comstock-era laws banning the sale of contraceptives and the repeal of these laws after *Griswold v. Connecticut* in 1965 (Bailey 2010); the second is the expansion of federal funding for local family planning programs from 1964 to 1973 (Bailey 2012). Previous work has established the effects of both sets of policy changes on fertility rates, and this paper builds on this work to examine these policies' long-run implications for children's outcomes in adulthood.

The results suggest that increasing access to family planning reduced mothers' reports of child "unwantedness" but had no measurable effects on infants' weight at birth, infant mortality, or maternal mortality in the 1960s and 1970s. In the long run, increasing access to

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[3]The history of this idea is much older. Thomas Malthus popularized the link between childbearing and poverty in his Essay on the Principle of Population (1798). Malthus argued that this link was rooted in the fact that agricultural yields grow arithmetically whereas population grows exponentially. Left unchecked, population growth would thus outstrip growth in agricultural production and perpetuate a subsistence economy. According to Malthus, improving living standards beyond subsistence required "preventive checks," namely, a reduction in the number of births through "moral restraint" and delay in marriage.

family planning is associated with 2 percent higher family incomes among the affected cohorts as adults, largely due to increases in men's wage earnings and weeks and hours worked. Federal grants for family planning also increased children's educational attainment. College completion (proxied by 16 or more years of education attained) increased by 2 to 7 percent for children whose mothers had access to family planning, relative to children who were born in the same location just before family planning programs began.

These findings are suggestive of much larger and broader effects of family planning. Not only are potentially many more outcomes affected than considered in this analysis, but the direct effects on the families that gained access to contraception may be considerably larger than this paper's cohort-level estimates suggest. The within-family and cross-cohort spillovers and the effects of measurement error, both of which are expected to reduce the magnitudes of the estimates, may lead the analysis to understate the effects of family planning programs. The results, however, are consistent with the growing literature on the sizable and persistent effects of early childhood interventions (Heckman and others 2010, Almond and Currie 2011) and place family planning within the set of interventions that potentially increase early investments in children.

The paper begins by describing the history of family planning policies and their public support, starting with the early-20th-century birth control movement and extending to today with the rise of publically funded family planning programs (section I). The paper next describes the expected effects of changes in these family planning policies on fertility rates, children's resources, and their adult outcomes (section II) and discusses the empirical evidence linking family planning policies to these outcomes (section III). New empirical evidence describing the long-run effects of family planning programs on children's outcomes in adulthood is reported in sections IV and V. Section VI draws implications from the analysis and concludes.

I. From Salacious to Subsidized: A Brief History of Family Planning in the United States

Today, a variety of highly effective contraceptive methods, scientifically tested and U.S. Food and Drug Administration (FDA) approved, are widely available either by prescription or over the counter. Manufacturing and selling contraceptives is legal in all 50 states, and federal and state governments and nonprofit and private organizations subsidize family planning services.

Historically, however, contraceptives and information on contraception were considered obscene material and banned under federal and many state statutes. At the federal level, the 1873 Comstock Act outlawed the interstate mailing, shipping, or importation of articles, drugs, medicines, or printed materials considered "obscenities," a term that applied to anything used "for the prevention of conception" (18 U.S.C. §1461–1462).[4] After the

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[4]The Comstock Act banned any "book, pamphlet, paper, writing, advertisement, circular, print, picture, drawing or other representation, figure, or image on or of paper or other material, or any cast, instrument, or other article of an immoral nature, or any drug or medicine, or any article whatever for the prevention of conception" (Tone 1996, p. 488). The act takes its name from its zealous advocate, Anthony Comstock of New York.

Comstock Act passed, 45 states enacted or amended anti-obscenity statutes mentioning contraception (Bailey 2010). Doctors received little training relating to contraception.[5] Information (and misinformation) about contraception flowed through families and friends (and often charlatans) rather than through the medical community.

I. A. The Birth Control Movement

Margaret Sanger is typically credited with beginning the U.S. birth control movement (although there were many contributors to the cause), which gained traction in the 1920s. The movement is often dated to Sanger's arrest in 1914 for the publication of a pamphlet using the obscene words "birth control."[6] Consistent with the claim that this event catalyzed the movement, mentions of "birth control" in books increased sharply around this time, according to Google Ngrams (figure 1). The charges were eventually dropped, and Sanger's activism continued. Her strategy for making birth control more acceptable was to cast it as a means to improve women's health. The movement's success in increasing birth control's medical legitimacy led the U.S. Second Circuit Court of Appeals to strike down portions of the federal Comstock law in *U.S. v. One Package* (86 F.2d 737, 1936). The following year the American Medical Association reversed its longstanding opposition to birth control.

Despite the taboos surrounding birth control, early public opinion polls show strong support for the movement (see the online data appendix for details on surveys).[7] In 1936, when the Gallup Poll first asked respondents whether they "favor the birth control movement," 61 percent answered affirmatively (figure 2; 13 percent did not answer). Starting in 1938, Gallup fielded a new question about whether respondents "would like to see a government agency furnish birth control information to married people who want it." The share of affirmative answers varied over the next 10 years, but support appears to have increased from about 62 percent of the nation's adults in 1938 to 67 percent in 1947. Twenty years later, on the eve of the first birth control pill's approval by the FDA, support had continued to increase. And in 1959, 73 percent of Gallup respondents said that "birth control information should be available to anyone who wants it."[8] Thus, during the two decades leading up to the introduction of "the Pill"—an era noted for its large baby boom and pronatalist policies—public support for the free availability and government provision of birth control information remained high and even increased.

Public support for government-provided birth control information increased at the same time that the supply of condoms and diaphragms increased. But these contraceptives were expensive and often of low quality. Encouraged by Sanger's courtship of the medical community, physicians built lucrative practices around filling contraceptive prescriptions in

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[5]One large-scale survey of physicians about their attitudes regarding birth control revealed that only 10 percent of medical school graduates before 1920 had received any training regarding contraception (Guttmacher 1947).

[6]Sanger was indicted for nine violations of the New York state Comstock law for her use of the words "birth control" in her journal *The Woman Rebel*. After the charges were dropped, she launched a new journal in 1916 provocatively called *The Birth Control Review,* in conjunction with the opening of a "birth control clinic" in Brooklyn, New York. This clinic was shut down by the vice squad the next day, but Sanger managed to open her first "legal" birth control clinic in 1923, claiming to use birth control for "medical purposes."

[7]Online appendixes and replication files for the papers in this volume may be accessed on the *Brookings Papers* website, www.brookings.edu/about/projects/bpea, under "Past Editions."

[8]The full question reads, "In some places in the United States it is not legal to supply birth control information. How do you feel about this—do you think birth control information should be available to anyone who wants it, or not?"

house, and local pharmacists provided "legitimate" supplies at large markups. One study of the diaphragm industry in 1938 found the average physician markup to be substantial (Tone 2001, p. 132). A device for the typical patient would have cost at least half of an entire week's earnings at the 1938 minimum wage. In states prohibiting the sale of contraceptives under their Comstock statutes, black market distribution channels became well established. Couples could often obtain diaphragms and condoms through the mail, or from gas station clerks or truck stop vending machines (Tone 2000, 2001; Garrow 1994). Data from the Growth of American Families survey show that, in 1955, 47 percent of ever-married women aged 18 to 29 had at some time used a barrier method like the diaphragm or a condom, and rates of "ever use" (not current use) did not differ for women living in states with Comstock statutes (Bailey 2010, Freedman, Campbell, and Whelpton undated).

In short, Sanger's strategy of making the sale of birth control methods profitable cultivated the support of physicians and increased the social acceptance of these methods. Her strategy also increased the momentum of the family planning movement that would ultimately lead policymakers to subsidize contraceptives for families with fewer resources.[9]

I. B. The Introduction of the Pill and Restrictions on the Sale of Contraceptives

Enovid, what would become the first oral contraceptive, was initially introduced for the regulation of menses in 1957. Only in 1960 was it approved by the FDA for longer-term use as a contraceptive. The new medication, which soon became known as "the Pill," was met with "extraordinary immediate enthusiasm" (Weinberg 1968, p. 1). But enthusiasm turned into controversy as couples realized that state Comstock laws prohibited physicians from prescribing the Pill and pharmacists from selling it.

State obscenity statutes of the Comstock era varied in their language relating to obscenity and, consequently, in their implications for access to the Pill. Although the Comstock laws were outdated and had historically been difficult to enforce, their importance increased with the Pill's introduction. The Pill was available only from physicians and pharmacists, who tended to comply with state laws because violating them could jeopardize their licenses and livelihoods. Newly introduced and still under patent, Enovid would have been hard to obtain through the usual black market channels,[10] and women could not verify beforehand the effectiveness of illicitly obtained pills—much less their safety.

The popularity of the Pill collided with these statutes in the early 1960s. In 1964 and 1965, affirmative responses to Gallup's question (reworded to say, "In some places in the United States it is not legal to supply birth control information. How do you feel about this—do you think birth control information should be available to anyone who wants it, or not?") topped 80 percent (figure 2)—a figure almost identical to the percent of ever-married women who in 1965 reported ever using a contraceptive.[11] Popular support for and pervasive use of

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[9]In fact, Sanger and others used the revenue from the sales of condoms and diaphragms to subsidize free clinics for less advantaged women. Sanger calculated that the sale of 5,000 diaphragms at market rates by her affiliated company would yield enough profit to give away 15,000 diaphragms to her birth control clinics (Tone 2001, p. 131).

[10]A combination of the chemical compounds mestranol and norethynodrel, Enovid was hard to produce by laypersons—or for that matter by other pharmaceutical companies that might have tried to infringe on the patent. It took several years for competing birth control pills to come to market.

[11]The comparable figure was 84 percent in the 1965 National Fertility Study (Bailey 2010).

contraceptives likely helped birth control advocates win the 1965 U.S. Supreme Court case *Griswold v. Connecticut* (381 U.S. 479), which induced state legislatures to revise their obscenity statutes. By 1970 every state (and the federal government) had revised its statute to permit the sale of contraceptives to *married* individuals. Unmarried adults did not have legal access to contraceptives in every state until the 1972 *Eisenstadt v. Baird* decision (405 U.S. 438, 453; see Bailey and others 2011 for a description of legal changes that expanded access for unmarried minors).[12]

I. C. The Rise of Today's Publicly Funded Family Planning Programs

With legal questions settled, advocates next turned their attention to expanding financial access to reliable contraceptives through government-supported "family planning" programs. The argument for subsidizing family planning was based upon the premise that the high cost of contraceptives (and related information and services) tended to keep birth rates high among lower-income individuals. Just as legal restrictions had inhibited many from obtaining reliable contraceptives, advocates argued that the cost of modern contraceptives differentially inhibited lower-income individuals from using them.

This argument was especially relevant in the early 1960s, when the monopoly producer of Enovid sold it at a premium. Shortly after its release, an annual supply of Enovid cost the equivalent of about $760 in 2010 dollars (Tone 2001, p. 257), roughly twice today's annual cost and equivalent to more than 3 weeks of full-time work at the 1960 minimum wage. In 1961 Maurice Saugoff of Planned Parenthood asserted that even his clinic's discounted price (less than half the retail price) was "beyond the reach of many of our low-income inquirers" (Tone 2001, p. 257).

Widespread concern about population growth (Wilmoth and Ball 1992, 1995), together with studies showing that lower-income families were having more children than they desired (National Academy of Sciences 1963), galvanized support for federal intervention. In 1968, 77 percent of adults surveyed nationwide said that birth control information should be available to everyone (figure 2). The rise in public support tracks fairly closely Google Ngrams mentions of birth control, contraception, and family planning in books published over the same period (figure 1).

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.[13] The EOA did not explicitly mention "family planning," but family planning fit easily within the anti-poverty agenda. The Office of Economic Opportunity (OEO), the office in charge of administering EOA funding, supported the opening of new clinics in disadvantaged areas

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[12]Eliminating many of these formal restrictions did not result in full, unimpeded access to contraception. Other laws or regulations in some jurisdictions continued to make the purchase of contraceptives inconvenient or extremely difficult. Legalization was a necessary, but not a sufficient, condition for expanding access.

[13]Before 1965, U.S. federal involvement and investments in family planning had been modest. This reflected the view expressed by President Dwight Eisenhower in 1959, who said that he could not "imagine anything more emphatically a subject that is not a proper political or government activity or function or responsibility… The government will not, so long as I am here, have a positive political doctrine in its program that has to do with the problem of birth control. That's not our business" (Tone 2001, p. 214). According to 1967 estimates, expenditure for family planning through the Maternal and Child Health programs started in 1942 and the Maternal and Infant Care programs under the 1963 Social Security Amendments were small (U.S. DHEW 1974).

and, to a lesser extent, the expansion of existing family planning programs. Generally speaking, these programs aimed to bring birth control information and contraceptives to disadvantaged individuals. Federal family planning dollars funded education, counseling, and the provision of low-cost contraceptives and related medical services, but they did not fund abortion. However, less is known about these programs' day-to-day operations. During these early years, organizations ran programs with little oversight from the federal government. Not only did the federal government collect little information on their services and patients, but officials talked very little about them. In an evaluation of the War on Poverty, Sar Levitan (1969, p. 209) wrote that, "Contrary to the usual OEO tactic of trying to secure the maximum feasible visibility for all its activities, 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.'"[14] The implication is that the treatment effect of these grants can be understood as one of increasing federal funding for family planning, rather than the effect of a particular, homogeneous intervention.

During this early period, federal funding for family planning expanded in two large steps (figure 3). The first expansion came with the 1967 amendment to the EOA, which designated family planning as a "national emphasis" program along with better-known programs such as Head Start. In the same year, Title V of the Social Security Act was amended to mandate that at least 6 percent of funds appropriated to child and maternal health at the state level be earmarked for family planning services (P.L. 90-248, Title V, §§ 502, 505a, 508a; Title IV, § 201a). In addition, the Maternity and Infant Care projects under the Department of Health, Education and Welfare (DHEW) supplemented the EOA effort by funding family planning services through city health departments. From fiscal 1967 to fiscal 1970, federal funds allocated to family planning increased to roughly $600 million (in 2010 dollars), over 10 times their level in 1967.

In 1969 President Nixon initiated a second expansion of federal support with his endorsement of a national family planning program, saying, "no American woman should be denied access to family planning assistance because of her economic condition" (Nixon 1969). Nixon called upon 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 the effort to fund these programs culminated in the passage of Title X of the Public Health Service Act (also known as the Family Planning Services and Population Research Act, P.L. 91-572). This legislation not only guaranteed the survival of federal support of family planning during the phasing out of the EOA, but also increased that support by 50 percent in real terms by 1974. As with the earlier federal grants, federal family planning dollars paid for education, counseling, and the provision of low-cost contraceptives and related medical services. In addition, Title X explicitly prohibited the use of federal funds "in programs where abortion is a method of family planning" (§ 1008).

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[14]The fact that the OEO might fund birth control was contentious before the EOA passed. For instance, on April 18, 1964, the *Washington Post* (p. A4) reported the controversy on this topic between Representative Phil M. Landrum (D-Ga.), the House sponsor of the EOA, and Republican members of the special House Education and Labor subcommittee.

At the time of its enactment, Title X was popular and supported by both Democrats and Republicans. The year after it passed, a survey by the U.S. Commission on Population Growth and the American Future asked, "Do you think that information about birth control should or should not be made available by the government to all men and women who want it?" Eighty-four percent of surveyed adults responded yes—including 87 percent of Republicans and 82 percent of Democrats. Recent surveys have not asked a similar question, but in the May 2012 Gallup poll, 89 percent of respondents (including 90 percent of Democrats and 87 percent of Republicans) said they considered birth control "morally acceptable," suggesting that public support for birth control has changed little (figure 2).[15]

After the initial period of growth, federal appropriations for Title X fell to an average of roughly $400 million per year from 1975 to 1980. Federal appropriations continued to fall throughout the 1980s and reached a low of $231 million in 1991. Since the early 1990s, annual appropriations have averaged around $300 million (all amounts are in 2010 dollars). But as federal appropriations have fallen or stagnated, dollars from other sources have risen. Whereas the bulk of funds before 1977 were federal (Cutright and Jaffe 1977, p. 3), the Alan Guttmacher Institute (2000) estimates that around 50 percent of public support of family planning came from Title X by 1980. By 1994 that figure was only 20 percent (Alan Guttmacher Institute 2000, p. 13).

Public support of family planning programs has continued to grow even as Title X has changed little. Since 1980, real family planning expenditure through Medicaid has increased 500 percent, accounting for almost all of the increase in family planning funding. In fiscal 2010 over 75 percent of funds for family planning came from Medicaid and another 12 percent from state-only sources; Title X funding accounted for only 10 percent of all public funding (Sonfield and Gold 2012).

II. Expected Effects of Family Planning on Childbearing and Child Outcomes

How have these programs affected children? The potential effects of family planning policies on a variety of outcomes relate to their effects on fertility rates. By providing cheaper, more reliable contraception and more convenient services, family planning should reduce ill-timed and unwanted childbearing by decreasing contraceptive failures. Additionally, reductions in the price of averting births should increase the number of births that parents *choose* to avert or delay.[16] Standard economic models and related empirical work highlight the potential for family planning policies to affect children's outcomes as well.

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[15]This recent question differs from the early question about the government providing information. Answers to this question do not rule out an increase or a decrease in public support for family planning since the 1970s.

[16]Potentially offsetting this effect is the fact that cheaper and more reliable contraception should reduce precautionary undershooting as well (Michael and Willis 1976). Estimates presented later suggest that reductions in childbearing have dominated empirically, so that greater access to cheaper and more reliable contraceptives tends to reduce family size.

II. A. Family Size Channel

Fewer children in a household implies an increase in the availability of parental time and material resources per child. In addition, a reduction in the number of children in the household should decrease the shadow price of child "quality" and thus increase parental investment in each child (Becker and Lewis 1973; Willis 1973; Becker 1981, p. 109; Hotz and others 1997, p. 297). Many of these parental investments cannot be directly measured in the data available for this analysis. These theoretical predictions, however, suggest that any measured effects of family planning should be reinforced by unmeasured changes.

II. B. Household Income Channel

The availability of family planning may directly increase household income for several reasons. First, cheaper and more reliable contraception reduces the immediate and expected costs of delaying childbearing, freeing up resources for investment in the parents' human capital. Delaying parenthood for a year or two could allow soon-to-be parents to get more education, work experience, and job training, and thus increase their lifetime earnings. The results of empirical studies of the effects of teen motherhood and teen access to the Pill are consistent with the claim that delaying childbearing has value. Bailey, Brad Hershbein, and Amalia Miller (2012) show that earlier access to the Pill increased women's investment in their careers and, ultimately, their wages. Heinrich Hock (2008) shows that early access to the Pill increased men's educational attainment as well. Of course, delaying childbearing need not have economic benefits. Joseph Hotz, Susan McElroy, and Seth Sanders (2005) show that women who became mothers in their teens have *higher* subsequent levels of employment and earnings than women of the same age who miscarried as teens.

Second, family planning also reduces the price of delaying marriage (Goldin and Katz 2002) and could improve spousal matching, thereby reducing subsequent divorce rates (Christensen 2011, Rotz 2011). The presence of two adults in a household could lead to an increase in household income as well.

II. C. Selection Channel

Family planning policy may also affect selection into parenthood. This may be particularly true for the federal family planning programs of the 1960s and 1970s, as they disproportionately benefited poorer households. For instance, Aida Torres and Jacqueline Forrest (1985) document that in 1983 these programs served almost 5 million Americans annually, and roughly 83 percent of family planning patients had incomes below 150 percent of the poverty line; 13 percent were recipients of Aid to Families with Dependent Children (AFDC, the principal cash welfare program at the time). Frederick Jaffe, Joy Dryfoos, and Martha Corey (1973) report that 90 percent of all patients in organized family planning programs had household incomes of no more than 200 percent of the federal poverty line. If family planning programs induce some lower-than-average-income households to opt out of or delay childbearing, this would increase the average incomes of parents.

In summary, family planning programs may directly reduce fertility rates and family size, and increase parental investment in children, even holding household income constant. These consequences for children should be reinforced by any effects of family planning on

household income and selection of some lower-income individuals out of parenthood. Any increases in household income would tend to increase further parental investment in their children, especially if the income elasticity of child quality exceeds that of child quantity (Becker and Lewis 1973). To the extent that family planning increases parental investment in children, it may improve their lifetime opportunities and labor market outcomes as adults.

II. D. Cohort Size Channel

A final channel through which family planning might alter children's outcomes is by changing cohort size. Smaller cohorts could increase the public resources available per child and decrease competition for these limited resources (Easterlin 1978). In schools, for instance, a decrease in cohort size might decrease class sizes and increase the likelihood of getting attention from teachers. It may also reduce classroom disruptions if a teacher is more easily able to monitor smaller classes. Finally, because changes in cohort size are unlikely to be accommodated fully by universities, a larger share of these smaller cohorts may be admitted to and complete college (Bound and Turner 2007).

Cohort size may also affect the scale of markets for illicit drugs and other social "bads" and thereby affect the incidence of related crimes. The premise behind this argument is that decreases in cohort size increase the average cost of drug distribution, which increases prices and reduces use (Jacobson 2004). A similar logic extends to labor markets, as smaller cohorts reduce aggregate labor supply, decrease workers' competition for firms' resources, increase capital-labor ratios, and tend to raise wages.

Note that these labor market channels—in addition to the within-household spillovers in family income and reductions in the price of child quality—suggest that the effects of family planning may extend *beyond* the children immediately affected. Access to family planning may benefit children slightly older or younger in the affected households, children in unaffected households in the same cohort, and children in slightly older or younger cohorts in the same labor market.

III. Empirical Evidence Relating Family Planning to Children's Outcomes

The idea that higher rates of childbearing cause economic disadvantage is consistent with a large body of empirical research, but testing this claim rigorously has proved difficult. In the United States, family planning programs or policies have never been intentionally randomly assigned to a representative set of locations or group of participants. (I discuss small-scale randomized interventions on teens below.) This is problematic for empirical researchers, because compelling theoretical reasoning argues that causal effects run *both* from childbearing (through childhood disadvantage) to adult disadvantage *and* from childhood disadvantage to adult disadvantage directly.[17]

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[17]Theoretical models suggest that women who use family planning services are different in many ways from those who do not. Sah and Birchenall (2012) show why women who use family planning services may be expected to differ in terms of their unobserved preferences as well as in the price associated with a conception. Theory also suggests that cross-sectional associations in childbearing and family planning may reflect both greater local demand for services and the effects of those services.

Time-series evidence is not particularly helpful in sorting this out. The large changes in legal and financial access to family planning in the 1960s coincided with the end of the U.S. baby boom (figure 4). The fact that fertility rates fell rapidly over the 1960s is thus consistent both with reversion to the longer-term national trend *and* with an effect of family planning policies. Largely because fertility rates also declined sharply in the 1920s, long before the introduction of the Pill and the important changes in family planning policy discussed above, many scholars have concluded that these factors played an insignificant role. Gary Becker, for instance, concludes in his *Treatise on the Family* (1991, p. 143) that "the 'contraceptive revolution'…ushered in by the Pill has probably not been a major cause of the sharp drop in fertility in recent decades."

To address this concern, the empirical literature has used several different research strategies to isolate the causal role of family planning. The earliest studies used multivariate regressions to adjust estimates of the relationship between access to family planning (whether areas had a program or individuals used them) and fertility rates. These largely cross-sectional studies were limited by well-known omitted variables and endogeneity problems (see Rosenzweig and Wolpin 1986, Hotz and others 1997). The limitations of these studies led to mixed evidence on the effects of family planning (see Mellor 1998 for a review).

More recent studies use localized, randomized interventions that aim to reduce *teen* pregnancies. These studies overcome common threats to internal validity but generally find that family planning programs have had no effect on teen pregnancy in the United States. A. DiCenso and others (2002), in a review and meta-analysis of 22 randomized studies of family planning, sex education, and abstinence interventions conducted from 1981 to 2000, conclude that these interventions did not increase the use of birth control or reduce the number of pregnancies among teens. The failure of these studies to find program effects may reflect the trials' short horizons (treatment effects may take longer to manifest than the 1 to 2 years between baseline and follow-up) or their small sample sizes (even when pooled for meta-analysis).[18] Another difficulty is that the effects of family planning interventions for teens, many of whom already have access to contraception through providers like Planned Parenthood, may not capture the effects of public family planning initiatives that fund such programs. Moreover, the results for teens may not generalize to the broader population.

Another recent development has been the use of quasi-experimental methodologies, which are ideal for addressing both endogeneity and statistical imprecision in the observational and experimental literatures. This research design also allows an investigation of effects for older individuals. Several empirical strategies define this genre of studies.

The first exploits recent changes in funding for family planning to estimate its effects on contraceptive use and birth rates. Melissa Kearney and Phillip Levine's (2009) state-level, differences-in-differences study provides the most recent evidence that family planning funding reduces birth rates. Exploiting the state × year variation in Medicaid eligibility for

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

family planning among the near poor, they find 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.

Although suggestive, these results leave open questions relating to the broader and longer-term effects of family planning. First, a global change in family planning policy—such as the repeal of state statutes banning the sale of contraceptives, or the introduction of federal subsidies for family planning programs—may affect women other than the near poor. (Kearney and Levine's identification strategy allows them to examine only the effects for women with incomes ranging from 133 to 200 percent of the poverty line.) Second, the scale effects of family planning resources may be highly nonlinear. With diminishing returns to program scale (Schultz 1973, 1992), Kearney and Levine's identification strategy may understate the marginal effects of the initial expansion of family planning programs. Third, their shorter-term estimates may differ from the program's longer-term effects. If family planning affects fertility by allowing couples to delay childbearing, then the immediate decline in the birth rate may overstate the effects of family planning on fertility over a longer period. This critique is not specific to Kearney and Levine. T. Paul Schultz (2008) argues that the difficulty of recovering longer-term effects is a general problem for studies of family planning. Although a handful of quasi-experimental studies in developing countries examine the longer-term effects of family planning programs on childbearing (Joshi and Schultz 2007, in Bangladesh; Salehi-Isfahani, Abbasi-Shavazi, and Hosseini-Chavoshi 2010, in Iran; and Miller 2009, in Colombia), these studies do not easily generalize to the United States, where women's rights, knowledge, and resources imply a different demand for children and thus different treatment effects.

A second empirical strategy exploits more historical policy variation. The research design uses state-level restrictions on contraceptive access for unmarried younger (typically 18- to 21-year-old) women. For this group, access to contraception was limited by law in many states until the mid-1970s. Using variation in these laws across states (see Bailey and others 2011), a body of studies shows that early legal access to the Pill affected the timing of marriages (Goldin and Katz 2002) and births (Bailey 2006, 2009, Guldi 2008) and the incidence of premarital cohabitation (Christensen 2011) and had broad effects on women's and men's education, labor force attachment, and lifetime wages. Women and men were more likely to enroll in and complete college (Goldin and Katz 2002, Hock 2008, Bailey and others 2012) in states where access to contraceptives was easier. Women were more likely to work for pay (Bailey 2006), invest in on-the-job training (Bailey and others 2012), and pursue non-traditionally female professions (Goldin and Katz 2002, Bailey and others 2012). And as women aged, these investments paid off. Bailey and others (2012) find that 30 percent of the reduction in the wage gap between men and women in the 1990s may be attributed to career investments made possible by the Pill. Elizabeth Ananat and Dan Hungerman (2012) additionally show that access to contraceptives at younger ages improved the economic resources available to these women's children before age 18. In short, this series of quasi-experimental studies shows that although family planning interventions for teens had small effects on teens' childbearing, they may have had larger, longer-term effects on the same teens at older ages. They may also have affected the material well-being of their children during childhood.

The long-term effects of family planning on these children as *adults*, however, remain an open question. Do the children of mothers with greater access to family planning get more college education, earn higher wages, or live in more affluent households as adults? The next sections summarize two historical policy changes that allow an investigation of these questions.

IV. The Long-Term Effects of Increasing Legal Access to Contraception

State-level anti-obscenity statutes (also called Comstock laws) had existed for almost three-quarters of a century by the time the Pill was introduced. Although 47 of the 48 coterminous states had enacted anti-obscenity laws (most before 1900), idiosyncratic differences in their language had an important impact on their relevance for contraceptive access decades later. For instance, only 31 states explicitly enumerated "contraception" among the regulated obscenities, and language in 24 states additionally banned "sales" of contraceptive supplies. These Comstock-era sales bans remained on states' books and significantly increased the price of obtaining or using the birth control pill after it became available in the early 1960s. [19] The 1965 *Griswold* decision that struck down Connecticut's ban on the use of contraceptives had the effect of reducing compliance with and the enforcement of bans on contraceptive sales nationwide—even in states where these bans remained in effect. Following this ruling, state legislatures also revised their obscenity statutes to delete mentions of "contraception" and began permitting the sale of contraceptives to married women.

The presence of sales bans in almost half the states, which reduced the availability of the birth control pill for 7 years after its introduction, together with the removal of these bans following the *Griswold* decision, facilitate a quasi-experimental strategy for testing the effects of increasing legal access to the Pill on fertility rates and children's outcomes. This section first describes my differences-in-differences methodology to examine the impacts of the Pill. Next it examines these policies' effects on child wantedness and birthweight. Finally, it examines the cumulative effects of mothers' legal access to the Pill on the affected cohorts' adult outcomes in the 2000 census and the 2005–11 American Community Surveys (ACS).

IV. A. The Effect of Increasing Legal Access to the Pill on Childbearing

My analysis is similar to that in Bailey (2010) and uses the following flexible linear specification:

$$Y_{st} = \sum_{t=1951}^{1980} \tau_t \text{PillSalesLegal}_s\, D_t + X'_{st}\delta + f_t + g_s + h_{r(s)t} + \varepsilon_{st}, \quad (1)$$

where Y_{st} is a measure of the fertility rate in state s observed in year $t = 1950, 1951, \ldots,$ 1980. PillSalesLegal is a binary variable equal to 1 if state s had *no* preexisting ban on the sale of contraceptives, and zero otherwise,[20] D_t is a dummy for each year of observation

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[19]My legal research with Allison Davido on anti-obscenity statutes is summarized in Bailey and Davido (2010, www-personal.umich.edu/~baileymj/Bailey_Griswold_Legal_Appendix.pdf). Scans of supporting statutes are posted at www-personal.umich.edu/~baileymj/Comstock_Statutes.
[20]Note that the key independent variable (PillSalesLegal) is reverse coded in equation 1 from what Bailey (2010) presents.

(1950 is omitted), X_{st} is a vector of time-varying covariates,[21] f_t is a set of year fixed effects, g_s is a set of state fixed effects, and $h_{r(s)t}$ is a set of region × year fixed effects. Of interest is whether, after the Pill was introduced in 1957, fertility rates fell faster in states where it could be sold legally relative to fertility rates in states in the same census region that banned the sale of contraceptives. This is captured by the time pattern of τ, which captures the differential changes in fertility rates in states permitting the sale of contraceptives, after adjusting for other model covariates.

In this framework a causal interpretation of τ requires that fertility rates in states permitting the sale of contraceptives would have changed similarly to those in states banning their sale, in the absence of the Pill (from 1957 to 1965) and in the absence of *Griswold* (from 1966 to 1970). That is, states banning the sale of contraceptives provide an appropriate counterfactual. In addition, the presence of sales bans and the *Griswold* decision need to have meaningfully changed access to the Pill after it was introduced. These assumptions would be violated if, for instance, states permitting the sale of contraceptives experienced rapid growth in the demand for women workers, which would reduce the demand for children, and thus decrease fertility rates independent of the Pill's effect. The latter assumption could be violated if sales bans were not effective constraints.

Bailey (2010) provides several pieces of empirical evidence to support these assumptions. First, in analyses using data from the 1955 Growth of American Families survey (Freedman, Campbell, and Whelpton undated) and the 1965 and 1970 National Fertility Studies (Westoff and Ryder undated-a, undated-b), both the use of barrier methods specifically and the use of any contraceptives from 1955 to 1970 are unrelated to whether a state permitted sales of the Pill. This is consistent with any relationship between sales bans and fertility rates being driven by differences in the type of technology available, rather than by the demand for contraceptives. Second, use of the Pill before 1965 was significantly higher in states permitting its sale, and after 1965, use of the Pill converged to national rates in states previously banning the sale of contraception. If the ability to purchase the Pill encouraged the diffusion of modern contraceptives, and this affected childbearing outcomes, one should observe fertility rates falling more quickly in permissive states in the early 1960s and, after the *Griswold* decision, falling more quickly in states that had banned sales (which would result in the difference reverting toward its pre-1958 level). Bailey (2010) finds that the general fertility rate did change in a pattern consistent with these predictions.

Figure 5 reproduces these findings and presents estimates for the total fertility rate, an age-adjusted summary measure of fertility.[22] (More details regarding the estimates presented in the figures in this paper can be found in the online appendix.) Estimates of τ are close to zero between 1951 and 1957, which implies that the difference in fertility rates in states with

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[21]These covariates are constructed by linearly interpolating the following variables between census years: proportion of the state population of 15- to 44-year-olds residing on a farm, proportion currently married, proportion nonwhite, proportion foreign born, proportion in poverty, mean total income, and mean educational attainment. Other covariates include binary indicators for whether a state mentioned "contraception" in its obscenity law and for whether a state excepted physicians from its ban; both these variables are interacted with each year in the analysis. Following Levine and others (1999), I also include an indicator for early abortion repeal states (Alaska, California, Hawaii, New York, and California) interacted with each year dummy.
[22]The total fertility rate is equal to the sum of 5-year-age-group birth rates (the ratio of births to women in the age group divided by the population of women in that age group) multiplied by 5.

sales bans and the model-based counterfactual was stable before the Pill was introduced. Between 1958 and 1965, however, estimates of τ become more negative, and statistically significant, indicating that the difference in fertility rates by either measure fell after the Pill was introduced.[23] Because fertility rates were declining overall after the baby boom peaked in 1957, this increasingly negative difference indicates that fertility rates were falling more *rapidly* in states where selling the Pill was legal, as the Pill diffused more quickly there before the *Griswold* decision. In states permitting the sale of the Pill, the total fertility rate was about 6 percent lower in 1963–65 (a decrease of 0.2 from a base of 3.5 children per woman). This trend reversed after the *Griswold* decision. After 1965 both the general and the total fertility rates dropped more sharply in states where the sale of the Pill was illegal, because these restrictions ceased being enforced. Accordingly, the difference in fertility rates rebounded toward its pre-1958 level, as fertility rates in states previously banning the sale of the Pill converged to those in states where it could be sold legally over the entire period. Removing restrictions on contraceptive sales after the 1965 *Griswold* decision decreased birth rates in those states by around 4 percent.[24]

If one takes the estimates in figure 5 as causal estimates of the effects of greater access to the Pill on fertility rates, counterfactual estimates imply that, without the sales bans, the marital fertility rate could have been 8 percent lower in states with sales bans and 4 percent lower in the nation as a whole. Approximately 124,600 more births in 1965 occurred in states with bans on sales of contraceptives than would have occurred without these restrictions. Finally, Bailey (2010) uses a back-of-the envelope calculation to show that as much as 40 percent of the decline in the marital fertility rate from 1955 to 1965 might be attributable to the Pill.

IV. B. The Effects of Increasing Legal Access to the Pill on the Next Generation in Childhood

The effects of legal access to the Pill for mothers may have direct or indirect effects on their children's lifetime opportunities. This paper cannot separate the importance of each of the channels discussed previously; instead it investigates the presence of direct associations—the cumulation of many channels—between increases in legal access to contraceptives and the outcomes of cohorts born in these states.

Child wantedness and the timing of births—The Integrated Fertility Survey Series (IFSS; Smock, Granda, and Hoelter 2012) allows a direct investigation of the effect of legal access to the Pill on mothers' reports of child wantedness and of birth timing, and of subgroup differences in these relationships. The 1955–76 surveys asked (mostly ever-married) female respondents about each of their pregnancies and live births, including whether the pregnancy was wanted and timed as desired. Because this data set is much smaller than the one employed in the analysis of fertility rates, I group children born from 1950 to 1988 into birth cohort categories: 1950–57, the period before the birth control pill was introduced; 1958–65, the period following the Pill's introduction when only some states

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[23]For the total fertility rate, the estimates are individually statistically different in years 1962 through 1965 relative to 1950 and jointly statistically significant for 1958 to 1965 ($F = 7.03$) relative to 1950.

[24]Bailey (2010) also shows that these results are robust to dropping one region at a time and are present for women across age groups.

permitted its sale; and 1966–76, the period after *Griswold* when state-level restrictions on the sales of contraception were lifted.[25] In practice, D_t in equation 1 becomes a dummy variable equal to 1 for each of the last two periods, so that the point estimates of interest capture the change in the difference between states permitting the sale of the Pill and others in the same census region relative to the difference in the pre-Pill era.[26]

Table 1 presents the results. Column 1-1 shows that the ability to buy the Pill was associated with a 7 percent ($0.027 \div 0.37$) decrease in unwanted or ill-timed births between 1958 and 1965. After the 1965 *Griswold* decision, the magnitude of this effect fell to less than 0.0001, indicating that the magnitude was more similar to its pre-1958 level. Neither effect is precisely estimated, however, and neither is statistically different from zero at conventional levels. Within the sample of second and higher-order births (column 1–2), legal access to the Pill is associated with a statistically significant 10 percent decrease in ill-timed or unwanted births, which then falls by 40 percent in the 1966–76 period, after the *Griswold* decision. Most of this relationship appears to be driven by decreases in ill-timed childbearing (column 1–4), although unwanted births are also lower (column 1–3). Consistent with Mark Rosenzweig and Kenneth Wolpin's (1993) findings that the prevalence of unwanted births is severely overreported, these estimates suggest that unwanted births fell by much less than 100 percent with legal access to the Pill.

The last four columns of table 1 provide additional evidence on the effects of the Pill on wantedness by estimating the regression in column 1–2 separately for various subsamples of second and higher-order births: whites, women with 12 or fewer years of education, women with 13 or more years of education (some college), and women with 16 or more years of education (likely college graduates). Because these effects are imprecisely estimated, they are not statistically different from one another. The pattern of results is, however, suggestive. The magnitude of the effect for whites only (column 1–5) is similar to that for the entire sample of second and higher-order births (column 1–6). Moreover, the effect appears to be concentrated in the middle of the education distribution: mothers with 12 or fewer years or 16 or more years of education in states permitting the sale of the Pill have similarly lower levels of unwanted or ill-timed childbearing between 1958 and 1965, and the magnitude of this effect reverts toward zero in the decade after *Griswold*. Women with some college in states permitting the sale of the Pill, however, have significantly fewer unwanted or ill-timed births between 1958 and 1965, and the magnitude of this effect weakens in the decade after *Griswold* (column 1–7). The effects appear weaker for the subgroup of women with 16 or more years of education (column 1–8). This evidence is consistent with the Pill having widespread effects on women across the education distribution and of both races, rather than only on women from much more advantaged or disadvantaged households.

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[25]Very few births are reported in 1976, because this is the year in which the last IFSS survey that I use was conducted.
[26]One limitation of this analysis is its use of an imperfect measure of states where the births occurred, with which to link children to the legal environment in which they were born. Three of the surveys harmonized in the IFSS, the 1955 Growth of American Families and 1965 and 1970 National Fertility Studies, contain information on residence at the time of interview. Two others, the 1973 and 1976 National Surveys of Family Growth, contain information on state of residence of the respondent at ages 6 to 16 (not at the time of the interview). I group this information into a single measure of "state" for purposes of the analysis. All regressions are weighted by the IFSS-provided sampling weight, which is normalized to sum to 1 within each of the IFSS surveys and multiplied by the number of respondents sampled in the survey. This preserves the within-survey weights and gives each respondent a weight in the analysis proportional to the information contained in the survey.

Weight at birth—Differences in wantedness may translate into different prenatal investments in children. Douglas Almond and Janet Currie (2011) argue that these investments have large and lifelong effects on children's well-being as adults. Moreover, a number of studies have shown that the availability of abortion improves infant outcomes by reducing the number of low-birthweight babies (Grossman and Jacobowitz 1981, Joyce 1987, Grossman and Joyce 1990). Using a specification identical to the one described above, table 2 examines whether the faster diffusion of the Pill in certain states affected the share of infants with low birthweights. The dependent variable is the logarithm of the share of low-birthweight infants in total births; the data come from the universe of reported births from the Vital Statistics database.[27] Even with this very large data set, the analysis finds little evidence that birthweight changed differentially in states where selling the Pill was legal and in states where it was not, whether the sample is all births, white births, or nonwhite births (first three columns). An additional specification examines changes in nonwhite births in the South and again finds no statistically significant relationship (column 2–4). In all cases the changes are small in magnitude as well as statistically insignificant.

In summary, mothers in states permitting the sale of the Pill were less likely to report that their children were unwanted or ill-timed—an outcome strongly associated with subsequent developmental issues and diminished lifetime human capital and earnings. Because these effects appear concentrated among second or higher-order births, one should expect slightly older cohorts (older siblings) to be affected. However, infants born in states where their mothers could purchase the Pill appear no more likely to have had low birthweight.

These findings suggest that the selection and household income channels may be much less important in the context of this policy change: before 1965, the diffusion of the Pill affected older, married households in states where it could be legally sold; beginning in 1965, *Griswold* extended legal access to older, married women in states previously banning the sale of contraceptives. The household income channel effect may be much less important because most of the affected couples would have already completed their human capital investments and selected their occupations and partners. (This may be one reason why effects on wantedness are weaker for first births than for higher-order births.) The selection effect may have been much less important because the sales of contraceptives and bans on these sales affected most married women across the socioeconomic and education distributions. Although the imprecision of the IFSS-based estimates does not permit firm conclusions, the results in table 1 are suggestive. The Pill affected unwanted births similarly among families of different racial groups and in the *middle* of the education distribution. The absence of effects on birthweight is also consistent with this. Put another way, the marginal mother in states where the sale of the Pill was legal may have been very similar to the average mother in the population. In terms of interpreting the channels driving the effects, a stronger case can be made for the family size and cohort size channels: a reduction in the number of children tends to increase parental investment in each child, increase the public

[27]Vital Statistics reports annually by racial group the number of births that are classified as low birthweight (below 2,500 grams). These data have been hand entered by Tara Watson from 1954 to 1968 and paired with information from the Natality Detail files microdata of the National Center for Health Statistics (NCHS 2003) from 1968 to 1976. Together with information entered on the number of births each year, these data allow me to construct a panel of the share of infants born with low birthweight from 1954 to 1980 by race.

resources available to each child, and reduce the thickness of markets for illegal drugs. Many changes in parental investment—in time spent with children and the share of household resources spent on children—may have shifted but are unobserved in the IFSS and Vital Statistics data.

IV. C. The Effects of Increasing Legal Access to the Pill on the Next Generation in Adulthood

An important and open question is whether differences in parents' investments in their children due to differences in access to contraception affect the long-run outcomes of their children. A final set of analyses tests this idea using data from the 5 percent Integrated Public Use Microdata Samples (IPUMS) from the 2000 decennial census and the 2005–11 ACS (Ruggles and others 2010). An ideal feature of these data is that they include the state where each individual was born and the year of birth, which together tell me whether the individual's mother lived in a state permitting the sale of contraceptives. In addition, these data contain information on labor force outcomes, education, marital status, and childbearing in the individual's adult prime. I restrict the sample to individuals born from 1946 to 1980, and I exclude Alaska, Hawaii, and the District of Columbia.[28] I also restrict the sample to individuals aged 20 to 59, to capture labor market effects on workers before they begin retiring. The data are collapsed to birth year × state of birth × year of observation cells and weighted by the relevant cell population.

The fertility and wantedness analyses show how differences in the availability of the Pill may have affected individuals directly (by being more wanted or better timed as children), but indirect effects within the family or across cohorts may operate as well—these are the family size and cohort size channels discussed previously. This logic implies that differences in access to birth control between 1958 and 1965 may have had an effect on slightly older or younger children in the affected households—children born before 1958 or after 1965 who have a sibling that arrived in the 1958–65 period—or on cohorts slightly older or younger than the 1958–65 cohorts. These within-household or cross-cohort spillovers cannot be examined directly, because the census does not contain information on the siblings of individuals who are not living in the same household or on the relevant education or labor market cohorts of an individual. That changes in the law to permit the sale of the Pill affected cohorts who were born just before 1958 or just after 1965, however, is consistent with importance of the family size and cohort size channels.

Figure 6 summarizes the long-run, differences-in-differences effects of one's mother having lived in a state permitting the sale of the Pill on one's own total family income, income from wages (for men), and weeks or hours worked (for men). For descriptive purposes I group cohorts into 4-year categories: 1946–49, 1950–53, 1954–57, 1958–61, 1962–65, 1966–69, 1970–73, and 1974–80. In practice, D_t in equation 1 becomes a dummy variable equal to 1 for each category, with 1950–53 omitted. The empirical specification is otherwise identical

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[28]Restricting cohorts to those born before 1980 means that individuals in the sample will be of working age by 2000, the first year of data in the analysis. Restricting cohorts to those born in 1946 or later is also appropriate, because many born earlier in the 1940s would have begun retiring from 2000 to 2011, which complicates the interpretation of the labor force outcomes. For all of these reasons, the 1950–53 cohorts may be a more appropriate comparison group for subsequent cohorts than the 1940s cohorts.

to equation 1 except that it adds a quadratic in age to increase precision. The point estimates of interest capture the *change* in within-cohort category differences between children born in states permitting the sale of contraceptives and those born in states in the same census region banning their sale, with the 1950–53 difference normalized to zero.

The top left panel of figure 6 presents the estimates for log family income. Because the dependent variable is in logs, the point estimates can be interpreted as percent changes in the difference relative to the difference between these two groups for the omitted 1950–53 cohort category. Children born from 1958 to 1965 in states permitting contraceptive sales had roughly 1.5 percent higher family incomes as adults. Cohorts born in these same states just before the Pill was introduced (from 1954 to 1957) also appear to have been affected, perhaps because of the indirect household or cohort size effects described previously. This increase in cohort family income departs from the relative stability of cohort-category differences for the 1946–53 cohort categories. Moreover, the relative increase in family incomes is temporary. Consistent with the convergence in access to the Pill between states permitting the sale of contraceptives over the entire period and those prohibiting their sale until *Griswold*, the difference in family incomes for the post-1965 cohorts is not statistically different from that for the 1950–53 cohort category.

Much of this effect was driven by changes in men's wage incomes. The top right panel of figure 6 shows that the gap in income from wages is around 2 percent larger for men born from 1962 to 1965 in states permitting the sale of the Pill. This relative rise in wage earnings gap is largely due to greater labor force involvement among affected men: the bottom two panels (hours and weeks worked, counting no hours or weeks worked as zeros) show a relative increase in labor force effort, especially for men in the 1962–65 cohorts. Most of this is driven by changes on the extensive margin. In results not reported here, I do not find this pattern for average hourly wages (income from wages divided by usual hours times weeks worked last year) of full-time, full-year male workers. Differences in health and disability may play a role in these findings, but an investigation of these additional outcomes is beyond the scope of this study.

Figure 7 investigates the role of mothers' access to contraception on children's higher education. The results are also suggestive and concentrated among men. The relative share of men with 16 or more years of education grows by around 1 to 2 percent for cohorts born from 1958 to 1969 in states permitting contraceptive sales (top left panel). The positive effect on 16 or more years of education is small and statistically insignificant for the 1954–57 cohorts (who would have been affected only indirectly), and negative and statistically insignificant among cohorts born in the 1970s, whose mothers did not differ in their legal ability to buy the Pill. None of these effects is individually statistically different from that for the 1950–53 cohorts at conventional levels, nor does a joint test change this conclusion. Interestingly, these patterns are not present across the education distribution. Using some college or more (13 years or more of education, top right panel) or high school or more (12 or more years, not reported) as the dependent variable results in much smaller and statistically insignificant effects for men born after 1957. The effects for women are even more muted and also statistically insignificant.

In summary, differences in mothers' access to birth control predict differences in the extent and intensity of their offspring's labor force participation, wage earnings, and household income well into the most recent decade. Despite the multitude of experiences, labor market shocks, and events that shape labor force outcomes over a lifetime, the evidence suggests that the differential diffusion of the Pill, induced by preexisting state Comstock laws, had sizable and persistent effects on individuals and labor markets. These long-lasting effects are the possible result of four channels: family size, household income, selection, and cohort size. Based on the evidence presented here, the most plausible channels for the effects are family and cohort size. *Griswold*'s effective repeal of Comstock bans on the sale of contraceptives likely represents an improvement in families' ability to invest in each child and, perhaps, a relaxation of the financial constraints on sending children to college. Significant reductions in cohort sizes may have also altered children's resources and opportunities. These within-family and cross-cohort spillovers are consistent with the effects of contraceptive access extending beyond the immediately affected cohorts.

V. The Long-Term Effects of Subsidizing Access to Contraception

Legal barriers limited the use of modern medical contraception, but so did its cost. As already noted, when the Pill was introduced, an annual prescription cost roughly twice what it does today. Over 650 federal grants for family planning between 1964 and 1973 increased financial access to contraception by subsidizing expensive medical contraceptives (like the Pill) and related medical services, education, and counseling. These grants expanded existing programs and established new programs in underserved areas. From 1969 to 1983, users of family planning services increased from 1.2 million to almost 5 million, owing in large part to increases in federal support and rising support from state and local governments. Roughly 83 percent of family planning patients in this period had incomes below 150 percent of the poverty line (13 percent were AFDC recipients); 70 percent of patients were white and 25 percent were black (Torres and Forrest 1985).

The quiet and disorganized beginning of this program under the EOA and, later, the DHEW facilitates a quasi-experimental strategy to evaluate its longer-term effects. This section first describes my differences-in-differences methodology (Bailey 2012) for examining the fertility effects of funding family planning programs. It next summarizes how subsidized access to medical contraceptives affected the material and living circumstances of the average child. Finally, it uses a similar research design to examine the effects of family planning programs on the educational attainment, income, and employment of these children as adults in the 2000 census and the 2005–11 ACS.

V. A. The Effect of Subsidizing Contraception on Childbearing

The research design in Bailey (2012) relies upon the county-level rollout of over 650 federal family planning program grants, using the following differences-in-differences framework (Jacobson, LaLonde, and Sullivan 1993):

$$Y_{jt} = \theta_j + \gamma_{s(j)t} + \sum_y \tau_y D_j D_y + X'_{jt}\beta + \varepsilon_{jt}, \quad (2)$$

where $Y_{j,t}$ is the fertility rate in county j in year t = 1959, 1960, ..., 1988 ; θ_j is a set of county fixed effects, which allow consistent estimation of τ even in the presence of preexisting *unobserved* differences between funded and unfunded counties; $\gamma_{s(j)t}$ is a set of either year fixed effects or state × year fixed effects, which captures time-varying, state-level changes in the legal availability of abortion in the late 1960s and early 1970s, changes in Medicaid policy, and changes in family planning funds under Title V of the 1967 Amendment to the Social Security Act; and X is a vector including a constant and covariates.[29] The idea behind the inclusion of these covariates is to account for potentially confounding changes in population demographics and policies.

The coefficients of interest, τ_y, measure how outcomes differed over time between counties that received a family planning grant from 1964 to 1973 (D_j= 1) and counties that did not, both before and after the grant began, T_j^*. Because family planning grants occurred in different years, time is normalized to be relative to the date of the grant, using an indicator variable for the event year, $D_y=1(t-T_j^*=y)$. For instance, τ_5 corresponds to the regression-adjusted difference in outcomes 5 years after the program began. The date of the grant, $y = 0$, is omitted, and event years greater than 14 and less than −6 are grouped into two separate indicators to ensure that all parameters are well estimated.

Using the general fertility rate as the dependent variable,[30] figure 8 plots weighted estimates of τ: model 1 includes county and year effects (assuming $\gamma_{s(j)t}=\gamma_t$); model 2 adds state × year fixed effects to model 1; and model 3 adds the time-varying county-level covariates to model 2 (model 3's 95 percent confidence interval is also shown). Across models, the estimates are consistent with family planning grants reducing childbearing. Before the family planning program began, the trend in the general fertility rate was similar in counties that would eventually receive them and in those that would not (the pretreatment differences are close to zero and individually and jointly statistically insignificant), but it fell sharply in the funded counties after the family planning grants began. Within 3 years of the grant, the general fertility rate had fallen by roughly 1 birth per 1,000 women of childbearing age in these counties on average. By years 6 to 10 it had fallen by an average of 1.5 births per 1,000 women. Fifteen years after an organization received its first federal family planning grant, the fertility rate remained 1.4 to 2 percent lower in the county than in the year the program started, net of declines in fertility in other counties in the same state and after adjusting for observable county-level characteristics. These findings are robust to variations in the specification: omitting unfunded counties, not weighting the regressions, and including county-level linear time trends. In addition, the effects are similar for programs funded before and after Title X began in 1970 (Bailey 2012).

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[29]These covariates are the interaction of 1960 census characteristics (from Haines 2005; these include the shares of the population who are in urban areas, nonwhite, under age 5, and over age 64; the share of households with annual income under $3,000, and the share over $10,000, and the share of the county's land area that is rural or a farm) with linear time trends. In addition, information on the number of abortion providers in each county accounts for within-state changes in the availability of abortion from 1970 to 1988 (zero before 1970). I also use annual, county-level per capita measures of government transfers (from the Bureau of Economic Analysis' Regional Economic Information System, REIS; Bureau of Economic Analysis undated); these transfers include cash public assistance such as AFDC, Supplemental Security Income, and General Assistance; medical spending such as Medicare and military health care; and cash retirement and disability.

[30]Accurate age-county population estimates are not annually available in the early years to allow construction of the total fertility rate.

Because these programs served mostly lower-income women and operated in only one fifth of all U.S. counties in this period, federally funded family planning programs account for a small portion of the overall decline in fertility rates over the 1960s. These programs, nevertheless, had large effects on the poor women they served: the magnitudes are large enough to account for half of the 1965 gap in childbearing between poor and nonpoor women. Like the expansion of access to the Pill earlier in the 1960s, federally funded family planning programs had large effects on childbearing. They reduced overall fertility rates in the counties they served by around 2 percent, and among poorer patients (on the presumption that they were their only beneficiaries) by 20 to 30 percent within a decade.

V. B. The Effects of Subsidizing Contraception on Child Outcomes

A second set of analyses builds on this empirical strategy to investigate the link between federally funded family planning programs in the late 1960s and early 1970s and child outcomes. The analysis makes use of two large data sets: the universe of infant and maternal deaths from Vital Statistics from 1959 to 1988 and the restricted long-form samples of the 1970 and 1980 censuses.

Infant and maternal mortality—The analysis of infant and maternal mortality uses these outcomes as the dependent variable in a specification identical to model 3 in the fertility analysis (figure 8). Because the denominators of these outcomes are births, I use the number of births as weights. Figure 9 plots the estimates using infant or maternal mortality rates (left- and right-hand panels, respectively) as the dependent variable. Although fertility rates declined rapidly following the introduction of family planning programs, both the infant mortality rate and the maternal mortality rate changed negligibly after the programs began. I omit reporting of additional specification checks, because adding additional covariates, county trends, or omitting weights does not alter this conclusion.

Infant mortality rates are defined as the ratio of infant deaths to births, and figure 8 makes clear that family planning programs affected births. The absence of changes in infant mortality may therefore reflect important shifts in who becomes a mother (the selection channel). For instance, if the more advantaged of poor households used family planning to delay or prevent births, this could *increase* the share of infants living in the most disadvantaged of poor households. This, in turn, could increase post-neonatal infant mortality rates. In results not reported here, I examine this possibility by separating neonatal and post-neonatal infant mortality. The results are consistent with compositional factors playing a role. Neonatal mortality declines slightly following the introduction of a family planning program, but post-neonatal mortality appears to *increase*. Although the inclusion of covariates reduces the size of these estimates, the resulting magnitudes imply a sizable effect: an increase of approximately 0.10 death per 1,000 live births, or roughly 1.5 percent, over the 1965 post-neonatal infant mortality rate.

The absence of effects on maternal mortality is less surprising, because of the "population control" focus of many family planning programs in the 1960s. Whereas today's programs provide a menu of reproductive, gynecological, and prenatal health services, many programs

in the 1960s provided no health services at all and only handed out birth control pills (Bailey 1999).

For both infant and maternal mortality rates, the imprecision of the estimates is also important to consider when interpreting them. For the model shown in figure 9 (model 3), a 95 percent confidence interval ranges from –0.37 to 0.33 at year 5, encompassing both a reduction in the infant mortality rate of 1.9 percent and an increase of 1.7 percent over the 1970 mean. A 95 percent confidence interval for the maternal mortality rate arranges from –0.30 to 0.31 at year 5, encompassing a reduction of 13 percent and an increase of 13 percent over the 1970 mean.

In summary, children born just after federal family planning programs began operating were not measurably healthier, nor were their mothers. However, the measures I use capture only the extreme (and rare) events of infant or maternal death and may miss improvements in other dimensions of health. In addition, the imprecision of the estimates—despite using the universe of all infant and maternal deaths in the United States during the years in question— does not allow me to rule out meaningful improvements in either measure.

Children's material resources and living circumstances—Bailey, Olga Malkova, and Zoë McLaren (2013) also investigate the role of family planning programs in altering children's material resources and living circumstances. Our analysis draws on the half of all respondents on the census long form in 1970 and 1980 (10 percent and 8 percent of the total population, respectively) who also provided information on their residence in 1965 and 1975. These large samples are available in the Michigan Research Data Center and contain information on exact county of residence, rather than county group.[31] Using this information, we link children's year of birth and county of residence in 1965 and 1975 to the availability of federal family planning programs at their time of birth.

Estimating a version of equation 2 separately for both the 1970 and 1980 censuses,[32] Bailey and others (2013) find that children born just after a county received its first federal family planning grant experienced substantial improvements in their material resources. These children lived in households with higher mean annual incomes and were 5 percent less likely to live in poverty. Family planning grants also appear to have reduced the share of children living in households receiving welfare payments: that share fell significantly, by 15 percent, among children born after these grants were made. Finally, family planning programs reduced the share of children living in single-parent households. This suggests that greater access to contraception did not appear to influence less committed couples to have premarital sex or to undertake marriages that were less durable. In short, Bailey and others

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[31]Public use census samples contain only information on county groups, which are typically contiguous agglomerations of counties. In some cases counties are split between different county groups. Moreover, the county groupings changed from 1970 to 1980. Access to these geocoded, large long-form samples is restricted, and the samples are available only in the Census Research Data Centers.

[32]Note that children born between 1964 and 1970 will be aged zero to 6 in the 1970 census, and those born between 1964 and 1973 will be aged 6 to 16 in the 1980 census. To avoid the selection problem of children leaving their parents' household, we limit the sample to children under age 18. The regressions are not identical to previously presented specifications of equation 2, because they are unweighted and exclude unfunded counties to minimize the importance of measurement error from migration. In the 1970 census we set the lowest lead equal to –7 for all leads less than –7, and the highest lag equal to 1, to ensure that the coefficients can be estimated. The 1980 census allows us to examine the evolution of outcomes 6 years after the establishment of the family planning program. For this census, we set all leads less than –3 to be equal to –3, and the highest lag equal to 7.

(2013) show that one reason to expect family planning programs to have improved longer-term outcomes is that they improved children's economic resources and living circumstances in the short run.

V. C. The Effects of Subsidizing Contraception on Adult Outcomes

A final set of analyses investigates the long-run relationship between a mother's access to family planning services and the adult outcomes of her children. These analyses are based on the 5 percent 2000 decennial census sample and the 2005–11 ACS. The data do not contain information on the county in which individuals were born. Instead, I proxy for county of birth using the Public Use Microdata Areas (PUMAs), the locations where individuals were living at the time of the interview.[33] In the absence of systematic changes in migration patterns for individuals observed in the same PUMA but born before and after the family planning program began, misclassification error introduced by using PUMAs to proxy for county of birth should tend to attenuate the results. On the other hand, using PUMAs rather than counties for longer-term outcomes may reduce misclassification error if, for instance, using a slightly larger area improves the assignment of mothers' access to family planning (that is, more of the individuals remain in the PUMA of birth than lived in their county of birth). Both scenarios are possible, so the impact of misclassification error for this analysis is difficult to assess without more information on lifetime migration. Readers should keep both scenarios in mind when interpreting the estimates.

As in the previous analysis of long-run outcomes, I restrict the sample to include individuals born from 1946 to 1980. I also restrict the sample to individuals aged 20 to 59, to capture the labor market outcomes of workers before they begin to retire. The data are collapsed to birth year $\times$ PUMA $\times$ year of observation cells. The analysis uses a specification very similar to the model 2 version of equation 2. First, I limit the PUMAs used to those that ever received a family planning grant from 1964 to 1973 (Bailey and others 2013). Second, I group cohorts into the following categories:[34] D_y in equation 2 becomes a dummy variable equal to 1 for each of nine birth cohort categories in event time: –32 to –20 (cohorts born 33 to 20 years before the family planning program began), –19 to –15, –14 to –10, –9 to –5, –4 to zero, 1 to 5, 6 to 10, 11 to 15, and 16 and more. I omit cohorts born 4 to zero years before the family planning program started, so that the point estimates reflect the changes in cohort differences relative to the cohort differences for those born in the 5 years leading up to the introduction of the family planning program. Estimates for the first and last categories are suppressed in the presentation, because they are estimated using only a subset of cohorts. The point estimates of interest capture the *change* in the average difference in cohort outcomes between adults whose mothers would have had access to a family planning in the adult's year of birth and adults born in the same PUMA to mothers without access to family planning. The fact that the policy variation occurred between 1965 and 1973 allows a long

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[33]There are 2,069 distinct PUMAs, each with a population of 100,000 or more. PUMAs do not cross state borders, and they often follow county boundaries. Each of 1,269 PUMAs is matched to at least one family planning grant. See the online appendix for more information.

[34]Previous estimates use model 3, which uses all available covariates. Figure 8 shows that the addition of these covariates matters little. I omit these covariates here because of the difficulty of mapping them onto PUMAs.

preperiod to be examined for differences in trends before the family planning program began.

Figure 10 summarizes the long-run effects of mothers' increased access to family planning services on their affected offspring as adults, including the effects on total family income, men's income from wages, and men's weeks or hours worked. The top left panel shows that children born just after family planning programs began (years 1 to 10) had family incomes that were approximately 1 percent higher than residents of the same PUMA born in the immediately preceding years (years –4 to zero). To the extent that individuals born just before the programs' introduction may have also been affected (for example, because they lived in the same family or went to the same schools), the more appropriate comparison may be with individuals born 5 to 9 years before the family planning program began. This comparison suggests that greater access to family planning programs results in a statistically significant 2 percent increase in family income.

These estimates provide a pattern similar to those using Comstock-era bans and the diffusion of the Pill and are, in many cases, similar in magnitude. This correspondence in magnitude is surprising given that the two policy changes likely affected individuals at different income levels, and given that the analyses are based on different identifying assumptions.

The top right panel of figure 10 shows that much of this increase is driven by increases in men's earnings. The estimates are imprecise, but the pattern is suggestive of around a 2 percent increase for men born after family planning programs began. If some of the effects operated within families or schools or labor markets on cohorts just older than the affected cohorts (as in the case of the diffusion of the Pill), this may understate the effect of family planning programs. When instead the comparison is with men born 9 to 5 years before the family planning grant, the change in the difference reaches almost 3 percent, and the estimates for categories –9 to –5 years and 1 to 5 years are statistically different at the 5 percent level. As in the case of Comstock-era sales bans, some of the long-run effects on income appear to be driven by work decisions, but the estimates are too imprecise to allow firm conclusions. In contrast to the results in section IV. C, these effects appear to be driven by changes on the intensive margin (hours and weeks worked exclude zeros).

Figure 11 investigates the relationship between mothers' access to family planning and their children's educational attainment. These results suggest a striking relationship between family planning programs and children's human capital. Children born just after family planning programs began were more likely to complete at least 12, 13, and 16 years of education. These relationships are largely driven by increases in 16 or more years of educational attainment. Children born 1 to 5 years after a family planning program began were 2 percent more likely to complete 16 or more years of education than children conceived in the decade before family planning programs began. This number topped 5 percent for those born 6 to 10 years after family planning programs began and reached over 7 percent for those born 11 to 15 years after. These results contrast with the more modest pattern of educational attainment effects in the analysis of Comstock-era sales bans.

The differences between these results and those based on changes in states' contraceptive laws likely relate to the role of selection, household income, and family size. The selection effect may have been much more important for family planning programs, because they disproportionately served lower-income women. Moreover, increases in household income could complement the selection channel by reducing the cost to women of delaying their childbearing enough to complete school, finish their job training, or get a promotion—all of which should increase the resources available to their children once they are born. Finally, the family size channel implies that the children of these more affluent parents would have received more parental time and material resources. It is harder to make the case for the cohort size channel, because overall changes in fertility rates were much smaller than those induced by changes in the Comstock laws. As a result of all these factors, children of parents with greater access to family planning appear to have achieved higher lifetime incomes.

VI. Implications and Conclusions

The rationale for funding the first domestic family planning programs in the 1960s was closely intertwined with the War on Poverty era's notion of expanding economic opportunities for the poor. Subsidizing contraception through family planning programs, it was argued, would promote opportunities for disadvantaged women, who "do not want more children than do families with higher incomes" but "do not have the information or the resources to plan their families effectively according to their own desires" (National Research Council 1965, p. 10). It was also argued that these programs would promote the opportunities of the next generation and thus advance broader and longer-term economic prosperity.

A long literature estimates the costs and benefits of family planning policies. One (in)famous estimate was cited by President Johnson in 1965: "less than five dollars invested in population control is worth a hundred dollars invested in economic growth." Johnson's claim rests upon some dubious calculations, as does much of the empirical literature estimating the costs and benefits of family planning programs (Lam 2012). Following some early work by S. Enke (1960, 1966, 1971), the heart of many of these arguments is that it is easier to increase income per capita by reducing the denominator than by increasing the numerator.

This paper explores a different set of potential consequences of family planning policies. It has presented indirect evidence that, as envisioned by some of the programs' early advocates, family planning programs may influence national income (the numerator) directly over the longer term. The introduction of the Pill, the Griswold decision, subsequent state repeals of Comstock-era bans on contraceptive sales, and increases in federal funding for family planning programs are associated with large and persistent improvements in the material living circumstances of the affected children as adults. Analyzing two different policy experiments during the 1960s and 1970s, I find that children conceived in areas with greater legal or financial access to family planning went on to live in higher-earning households as adults than did children conceived in the same areas whose mothers had less access to family planning. Both increasing legal access and increasing financial access to the Pill are associated with a 2 to 3 percent increase in family income over *all adults* in the

affected cohorts. Scaling these estimates by a guess at the share of children benefiting from them implies much larger effects, perhaps around a 20 to 30 percent gain in family incomes for the children of directly benefiting families.[35] An important component of these income gains reflects increases in children's educational attainment. Children conceived in areas with greater financial access to contraception were 2 to 7 percent more likely to attain 16 or more years of education.

At first glance, these estimates may seem large. However, the magnitudes are not inconsistent with other recent findings on the effects of early-life policy interventions to improve the human capital of disadvantaged children. For instance, James Heckman and others (2010) show that the 2-year Perry Preschool program that provided home visits and prescheduled education to disadvantaged children significantly improved education, employment, and earnings. Raj Chetty and others (2011) document that children randomly assigned to smaller classes from kindergarten to third grade and to higher-quality classrooms were more likely to attend college and had higher earnings at age 27. Finally, Paul Gertler and others (2013) show in a recent working paper that 1-hour weekly visits to parents of stunted toddlers over 2 years from community health workers in Jamaica raised the average earnings of participants' children by over 40 percent. These earnings gains reflect a tremendous increase in educational attainment, as the treatment group was three times as likely to have some college education relative to the control group.

Indeed, a growing literature on the returns to early life interventions generally supports their importance for human capital and health investments early in life, but the mechanisms for these effects remain largely elusive.[36] Similarly, the mechanisms underlying the relationship between family planning and long-run outcomes remain unclear. Unlike educational or home-visit interventions, family planning programs do not provide educational resources directly, nor do they teach parenting. Family planning policies are, however, similar insomuch as they increase parents' economic resources and time available per child, both of which may facilitate children's development and complement subsequent educational and health investments in a dynamic manner (Cunha and Heckman 2007).

One simple way to assess the costs and benefits of investments in family planning programs is to compare them with those of other national programs and policies aimed at increasing college attendance and completion. Family planning programs in the 1960s cost an average of around $260 million per year in 2010 dollars, and today the federal government spends around $300 million per year on Title X family planning programs. One can use the lower confidence interval of the year 1 to 5 post-effects in figure 11 to make a conservative estimate for the impact of these programs on the number of individuals completing 16 or more years of education: for the 1973 birth cohort, such a calculation suggests that approximately 9,300 ($0.003 \times 3,098,683$) more individuals *completed* college than would

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[35]Bailey and others (2013) show an increase in the share of women using the Pill of around 5 percentage points in areas gaining family planning programs. Assuming that the only beneficiaries from family planning programs were the women switching onto the Pill (an assumption that likely understates actual program benefits) and that each of these women had two children, this implies that the reported intention-to-treat effects might be scaled up by around 10. This is a very rough calculation and intended only as a benchmark.

[36]See Heckman, Pinto, and Savelyev (forthcoming) for new evidence that the Perry program affected cognitive and personality traits.

have otherwise. Using today's higher annual family planning expenditures together with this conservative estimate of program benefits implies a cost of no more than $32,271 per individual induced to complete college. This estimate may be too high due to the use of recent costs and the lower confidence interval to compute benefits. Nevertheless, it implies family planning may be much cheaper than many other interventions to increase educational attainment. Head Start, for example, costs around $133,333, and Upward Bound $93,667, per student induced to *attend* college. (On the other hand, family planning could be more expensive than other interventions such as the FAFSA application assistance program, which costs $1,257 per additional student enrolled; Dynarski and others 2011). Of course, using only college completion ignores many of the other potential returns to family planning programs, which may extend beyond increasing higher education. Overall, the results suggest that family planning programs provide a cost-effective strategy for promoting opportunities and the longer-term prosperity envisioned by their early proponents.

Acknowledgments

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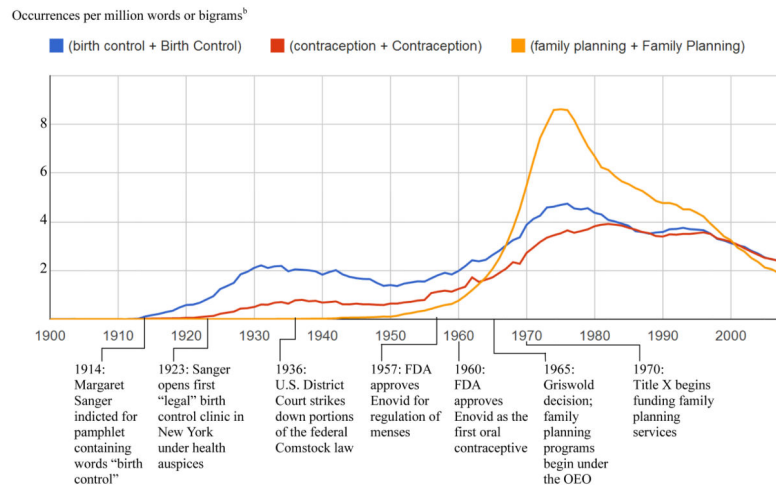
Figure 1. Incidence of Terms Related to Contraception in Google Books, 1900–2008[a]

Source: Author's tabulations using http://books.google.com/ngrams

a. FDA = Food and Drug Administration; OEO = Office of Economic Opportunity.

b. Words when only "contraception is used; bigrams when more than two words are used. A bigram is two consecutive words. Counts include both capitalized and lowercase occurrences.

Percent answering "yes"
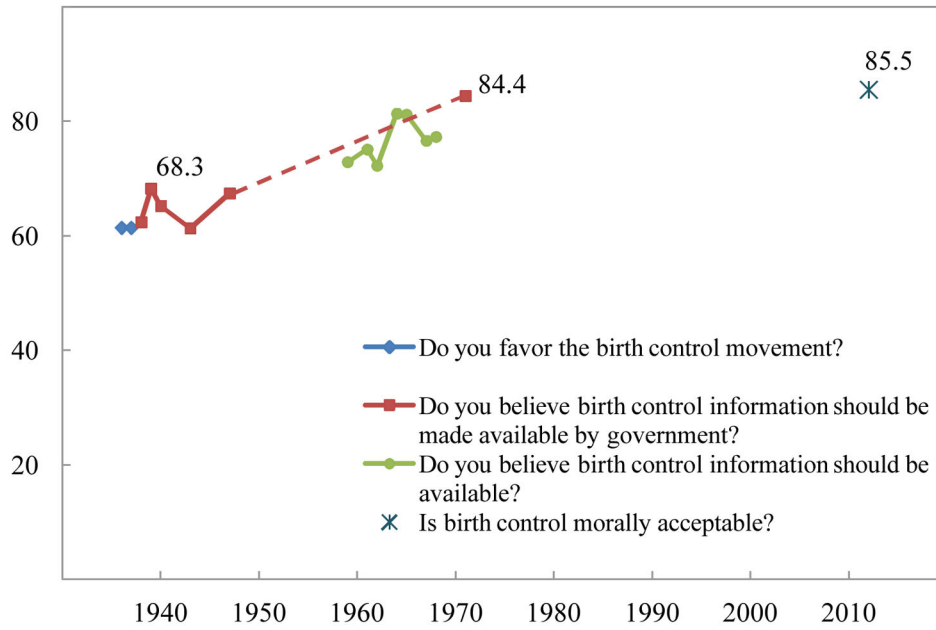
Figure 2. Survey Responses Regarding Support for the Birth Control Movement and Family Planning Programs, 1936–2012
Source: Author's tabulations using Roper Center data. See the online appendix for further details on the questions and the surveys.

Millions of 2010 Dollars
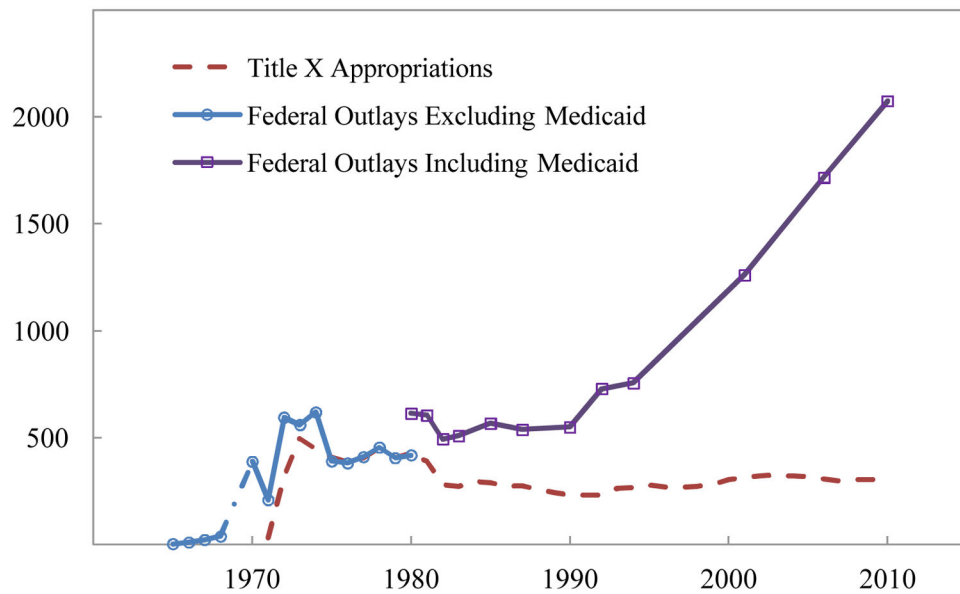
Figure 3. Federal Spending on Family Planning, 1965–2008[a]
Sources: Office of Population Affairs budget data from www.hhs.gov/opa/about/budget,
accessed February 7, 2009, Sonfield and Gold (2012), and author's calculations using data
from the National Archives Community Action Program and National Archives Federal
Outlays Data (Bailey 2012).

a. Title X appropriations differ from those in the inflation-adjusted table 14 in Alan
Guttmacher Institute (2000), because data in that table are deflated using the CPI for medical
care whereas here the CPI-U is used. Title X data for 1969 are unavailable.

b. Includes Title X and OEO appropriations.
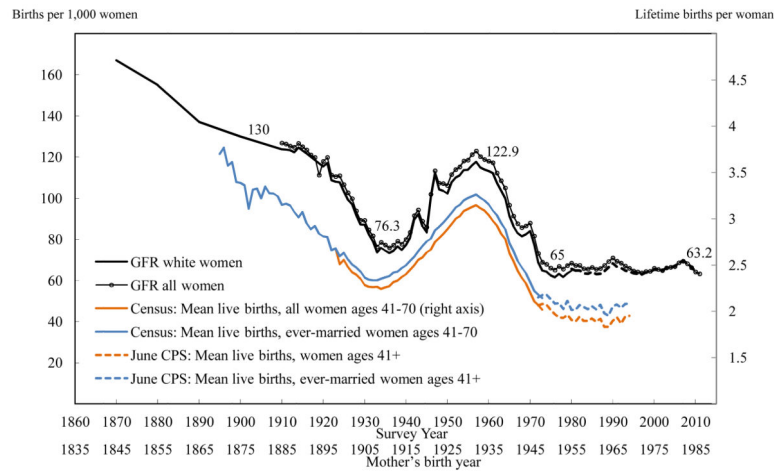
Figure 4. General Fertility Rates and Completed Childbearing over the Last Century
Sources: National Center for Health statistics, "Live Births, Birth Rates, and Fertility Rates, by Race: United States, 1909–2000," available at www.cdc.gov/nchs/data/statab/ t001x01.pdf, and Bailey, Hershbein, and Guldi (forthcoming) using data from the 1940–90 IPUMS of the decennial censuses and the 1995–2010 June Current Population Surveys.
a. Rates are from surveys undertaken in the top horizontal scale.
b. Mean lifetime births is the mean self-reported number of children ever born for each birth cohort (bottom horizontal scale), measured between the ages of 41 and 70. Dashed lines are extensions of the series using the June Current Population Surveys for all women aged 41 and over.

Births per 1,000 women per year

Age-adjusted births per woman[b]
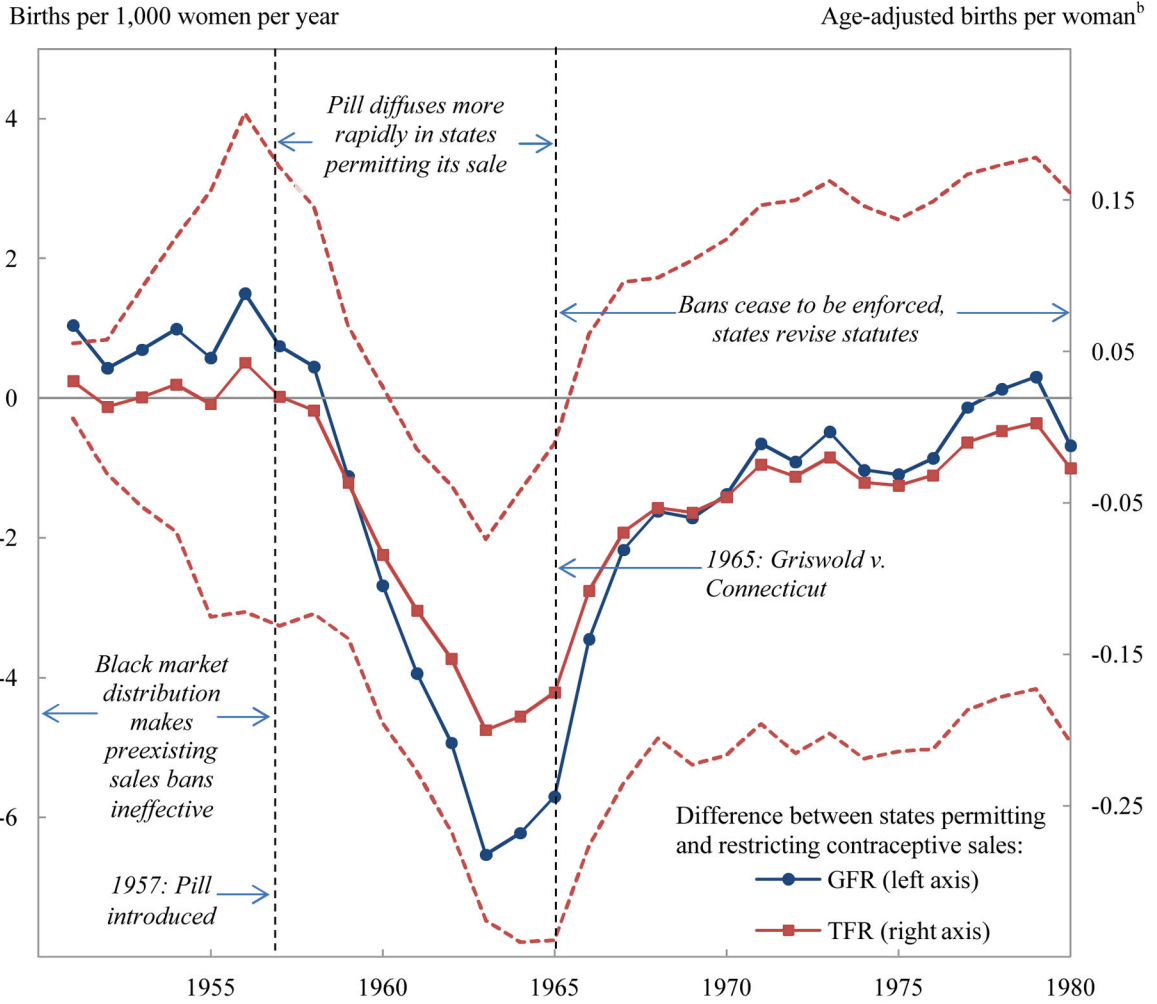
Figure 5. Differences-in-Differences Estimates of Fertility Effects of the Pill and the *Griswold* Decision[a]
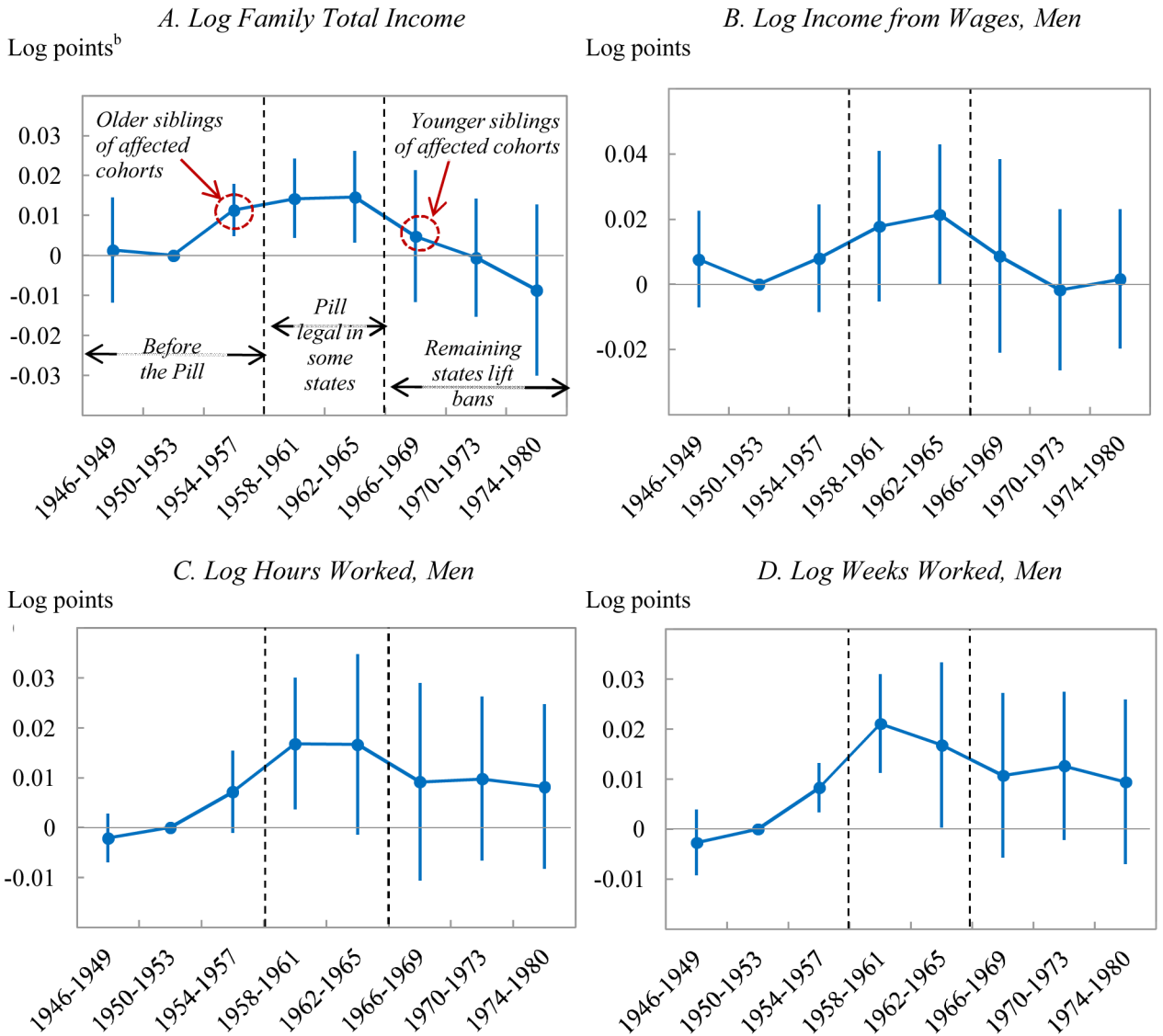
Source: Author's calculations using data from the 1950–67 Vital Statistics volumes and NCHS (2003). See the online appendix for details of the data sources and the regressions.

a. Each series plots weighted least-squares estimates of τ_1 from equation 1 using either the general fertility rate (GFR) or the total fertility rate (TFR) as the dependent variable. Robustness checks are omitted and can be found in Bailey (2010) for the GFR.

b. This scale is in TFR units as defined in footnote 22

c. Dashed lines indicate pointwise 95 percent upper and lower confidence intervals for the TFR estimates based on heteroskedasticity robust standard errors corrected for an arbitrary covariance structure within states.

Figure 6. Estimates of the Effects of the Pill and *Griswold* on Next-Generation Family Income, Wages and Labor-Force Participation[a]
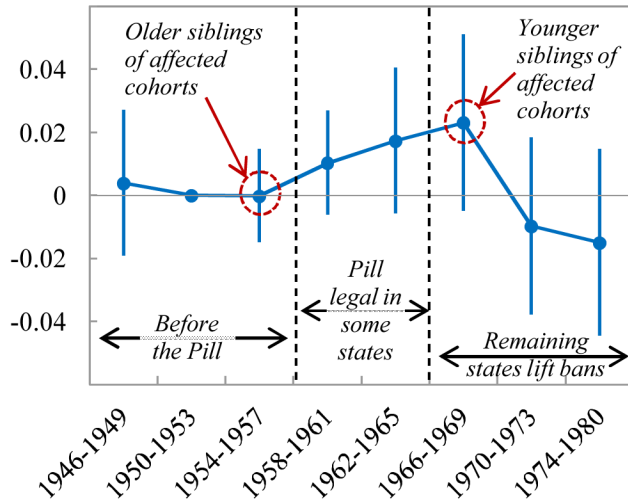
Source: Author's calculations using data from the 5 percent sample of the 2000 decennial census and the 2005–11 ACS (Ruggles and others 2010). See the online appendix for details of the data sources and the regressions.

a. Estimates are of the effects in adulthood of being born in a state with a ban on contraceptive sales, from the specification of equation 2 described in the text. The 1950–53 birth cohort category is omitted, and error bars represent 90 percent confidence intervals based on heteroskedasticity-robust standard errors corrected for an arbitrary covariance structure within birth state. The sample consists of individuals born in the United States from 1946 to 1980 who are aged 20 to 60. Data are collapsed to birth cohort category × birth state × year of observation cells and weighted by the population of each cell. In the 2000 census, income is measured for calendar 1999. In the ACS, income is measured for the 12 months before the survey. The ACS surveys are conducted throughout the year, and, to

protect confidentiality, the month of the survey is not released. Each income observation is inflated to real 2012 dollars using the consumer price index. Income in the ACS is treated as earned entirely in the year before the survey (see usa.ipums.org/usa/acsincadj.shtml). Weeks of work in the previous year are recorded in intervals in the 2008–11 ACS, so interval means are constructed here using the 2000–07 period when individual weeks worked are reported. The cell means used in the estimation include zero hours or weeks worked when applicable. b. Differences in log outcomes between states permitting and states restricting contraceptive sales. Normalized to equal zero in 1950–53.
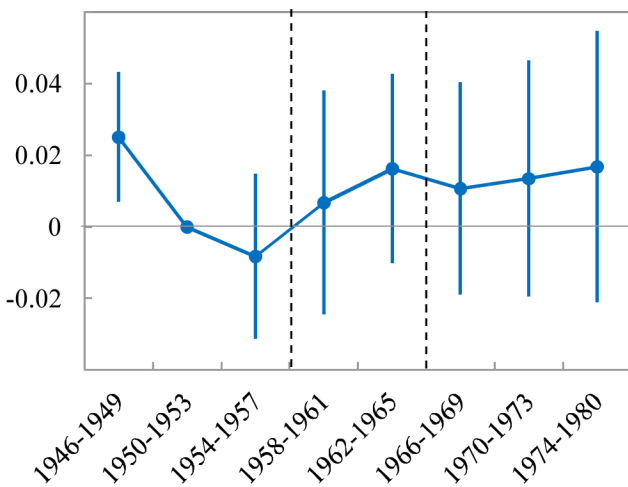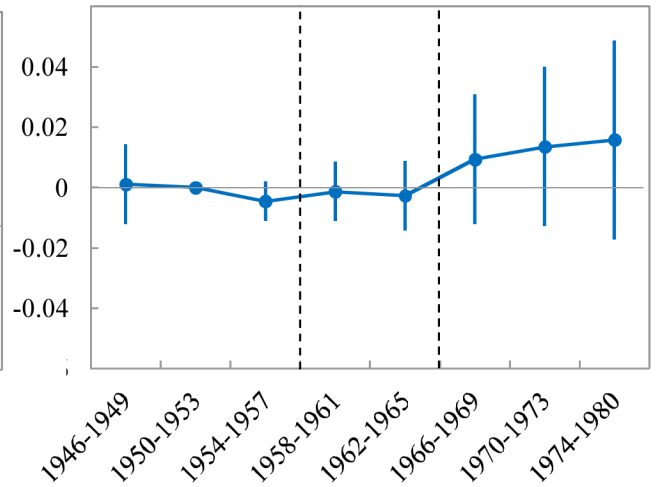
A. Log(Share with ≥16 years education), Men

B. Log(Share with ≥13 years education), Men
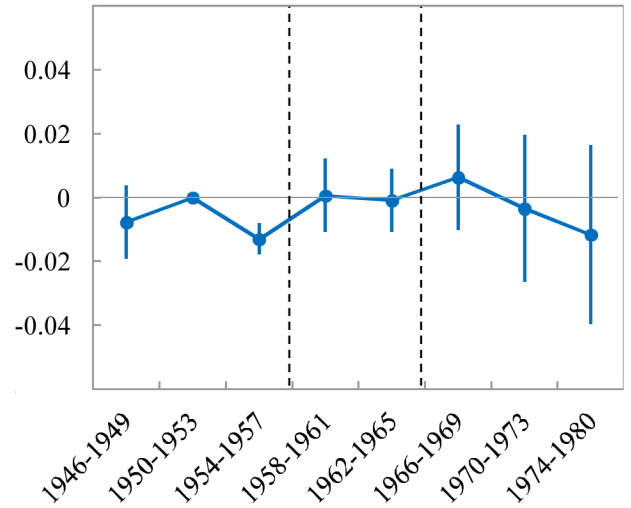
C. Log(Share with ≥16 years education), Women

D. Log(Share with ≥13 years education), Women

Figure 7. Estimates of the Effects of the Pill and *Griswold* on Children's Higher Educational Attainment[a]

Source: Author's calculations using data from the 5 percent sample of the 2000 decennial census and the 2005–11 ACS. See the online appendix for details of the data sources and the regressions.

a. See figure 6 for details of the estimation.

b. Differences in log outcomes between states permitting and states restricting contraceptive sales. Normalized to equal zero in 1950–53.

Births per 1,000 women per year
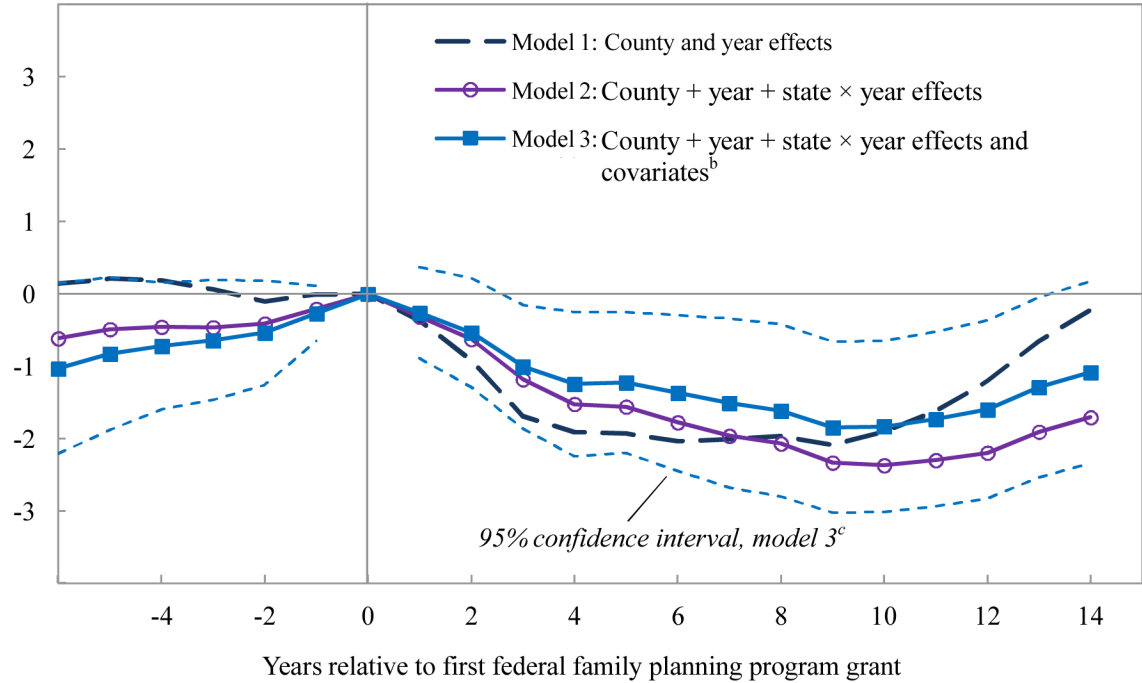
Figure 8. Estimates of the Effects of Subsidizing Family Planning Services on the General Fertility Rate[a]
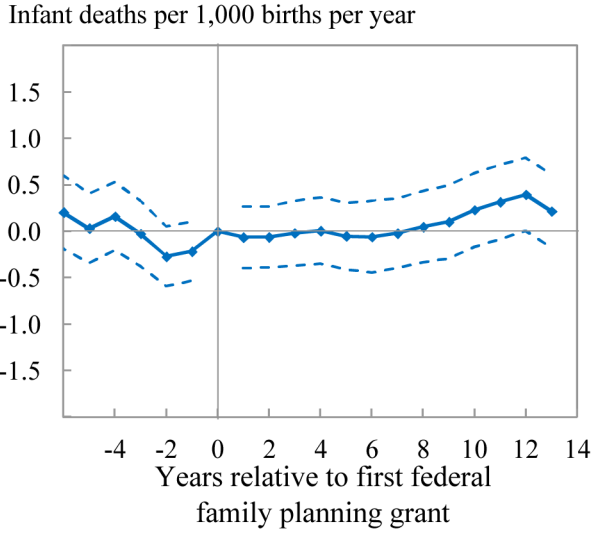
Source: Author's calculations using data from the National Archives, the Office of Economic Opportunity (1969, 1971, and 1974), and hand-entered data by county from Vital Statistics; Natality Detail microdata from NCHS (2003); and Surveillance Epidemiology and End Results (SEER) data (Surveillance Research Program, National Cancer Institute 2009). See the online appendix for details of the data sources and the regressions. (See Bailey 2012.)

a. The figure plots weighted least-squares estimates of the change in the difference in general fertility rates between counties with and counties without federal family planning grants relative to time zero (y in equation 2). The weights are the 1970 population of women aged 15 to 44. Denominators for 1959–68 were constructed by linearly interpolating information between the 1950, 1960, and 1970 censuses; denominators for 1969–88 use the SEER data. Dashed lines plot 95 percent, pointwise confidence intervals for model 3 based on heteroskedasticity-robust standard errors that account for an arbitrary covariance structure within county.

b. The model adds 1960 county covariates interacted with a linear trend and controls from the REIS data to model 2. See the text for details.

c. Pointwise confidence intervals based on heteroskedasticity-robust standard errors that account for an arbitrary covariance structure within county.

A. Infant Mortality Rate

Infant deaths per 1,000 births per year

B. Maternal Mortality Rate

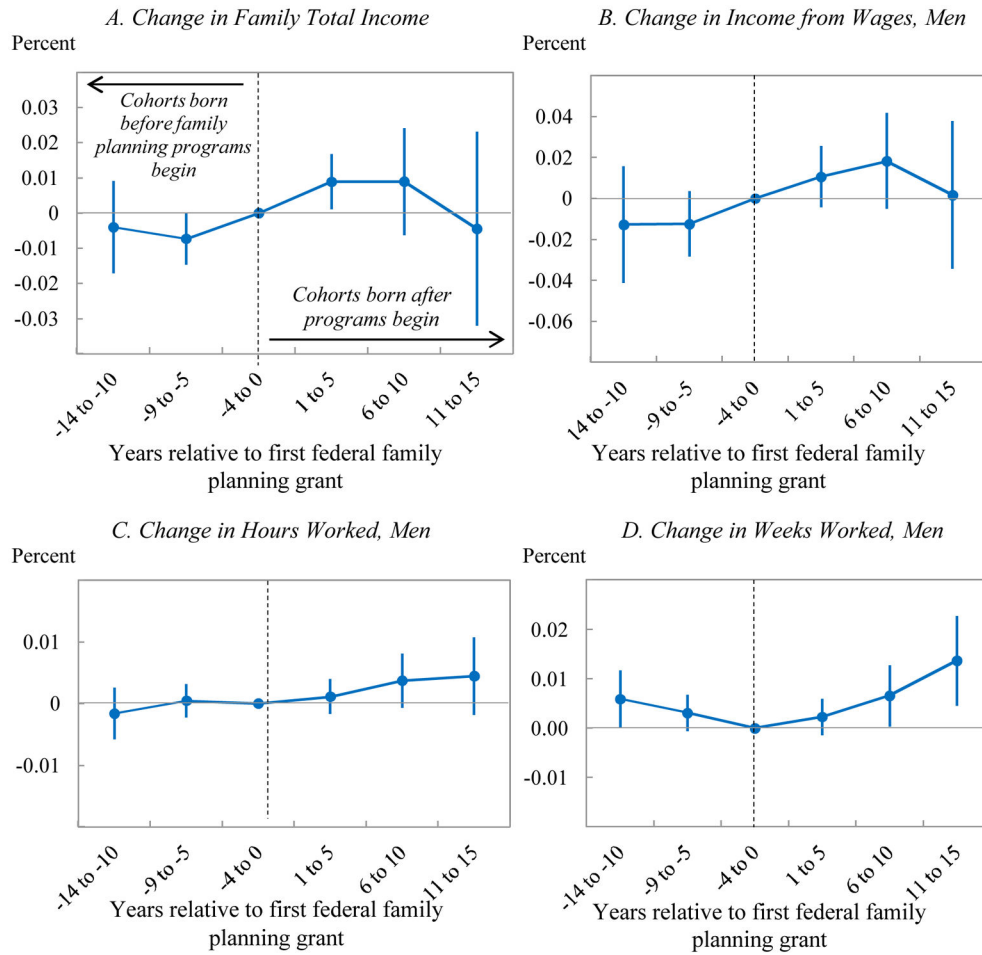Maternal deaths per 1,000 births per year
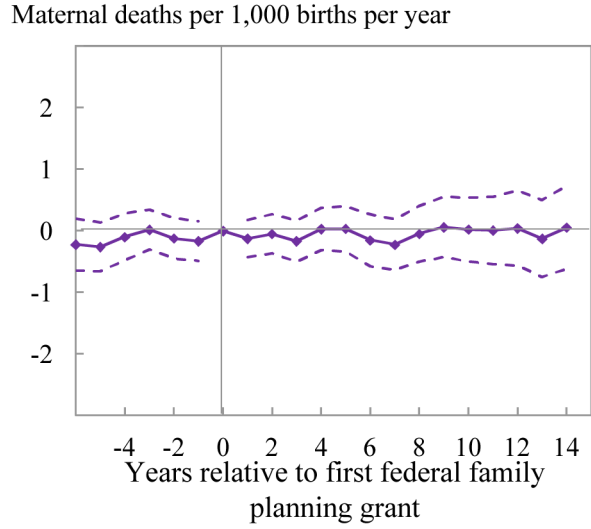
Figure 9. Estimates of the Effects of Subsidizing Family Planning Services on Infant and Maternal Mortality[a]

Source: Author's calculations using Multiple Cause of Death microdata, 1959–88, from NCHS (2008) for the numerators, and hand-entered 1959–67 birth records from Vital Statistics and 1968–88 Natality Detail microdata from NCHS (2003) for the denominators. See the online appendix for details of the data sources and the regressions.

a. Effects are measured as changes in the differences in the indicated outcome between areas receiving and areas not receiving federal family pla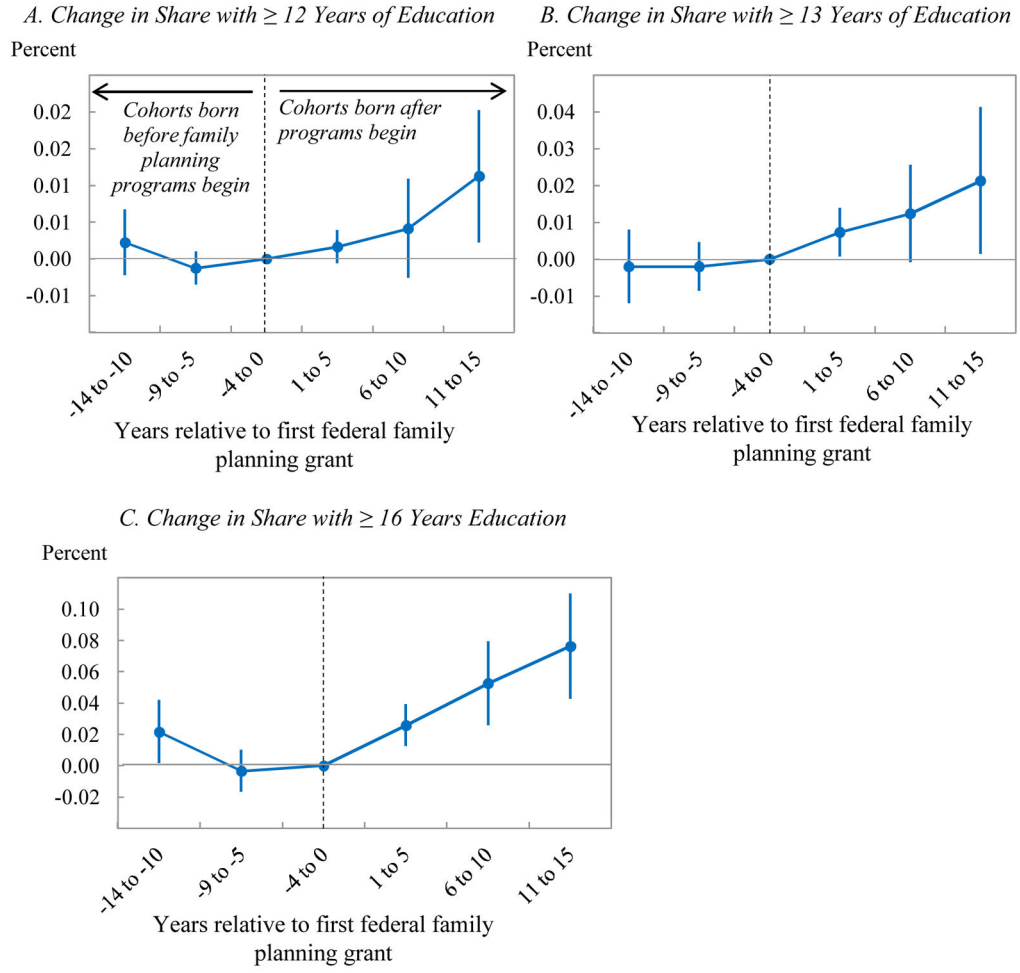nning grants, relative to time zero. Dashed lines indicate pointwise 95 percent confidence intervals. Estimates are for model 3; see the text and notes to figure 8 for more details on the estimation; see the online appendix for details of the data sources and regression output.

A. Change in Family Total Income

B. Change in Income from Wages, Men

C. Change in Hours Worked, Men

D. Change in Weeks Worked, Men

Figure 10. Estimates of the Effects of Family Planning Programs on Next-Generation Family Income, Wages and Labor-Force Participation

Source: Author's calculations using data from the 5 percent sample of the 2000 decennial census and the 2005–11 ACS. See the online appendix for details of the data sources and the regressions.

a. Estimates are of the effects in adulthood of being born in a Public Use Microdata Area (PUMA) that had a federally funded family planning program, from a specification of equation 2. Event time −4 to zero is omitted, and error bars represent 95 percent confidence intervals based on heteroskedasticity-robust standard errors corrected for an arbitrary covariance structure within PUMA. The sample consists of individuals born in the United States from 1946 to 1980 who are aged 20 to 59. Data are collapsed to birth cohort category × PUMA × year of observation cells. To minimize measurement error, estimates are unweighted and exclude Chicago, Los Angeles, and New York (see Bailey and others 2013). The cell means used in the estimation include observations of zero hours or weeks worked when applicable, so regressions are estimated in levels. For ease of interpretation, the results are rescaled by dividing by the mean dependent variable in event years zero to 4. See the notes to figure 6 for details on income and employment coding and the text for more information on the specification.

A. Change in Share with ≥ 12 Years of Education

B. Change in Share with ≥ 13 Years of Education

C. Change in Share with ≥ 16 Years Education

Figure 11. Estimates of the Effects of Family Planning Programs on Next-Generation Educational Attainment[a]

Source: Author's calculations using data from the 5 percent sample of the 2000 decennial census and the 2005–11 ACS. See the online appendix for details of the data sources and the regressions.

a. See the notes to figure 10 for details of the estimation..

Estimates of the Effects of the Pill and *Griswold* on Child Unwantedness and Birth Timing[a]

Table 1

		Dependent variable[b]						
	Birth unwanted or ill-timed		Birth unwanted	Birth ill-timed	Birth unwanted or ill-timed			
Independent variable[c]	1-1	1-2	1-3	1-4	1-5	1-6	1-7	1-8
PillSalesLegal × 1958–65[d]	−0.027 (0.018)	−0.045 (0.023)	−0.014 (0.020)	−0.031 (0.016)	−0.045 (0.026)	−0.030 (0.023)	−0.10 (0.055)	−0.029 (0.080)
PillSalesLegal × 1966–76[e]	−0.0001 (0.018)	−0.026 (0.022)	−0.0086 (0.022)	−0.017 (0.020)	−0.015 (0.025)	−0.022 (0.024)	−0.037 (0.038)	−0.11 (0.077)
Mean of dependent variable	0.37	0.43	0.24	0.19	0.40	0.45	0.35	0.31
No. of observations	65,851	43,676	43,676	43,676	27,175	36,627	7,035	2,312
R^2	0.104	0.097	0.033	0.103	0.098	0.095	0.131	0.175
Sample of mothers	All	All	All	All	Whites	<13 years of education	13 years of education	16 years of education
Sample of births	All	Second and higher-order	Second and higher-order	Second and higher-order	Second and higher-order	Second and higher-order	Second and higher-order	Second and higher-order

Source: Author's regressions using data from the Integrated Fertility Survey Series (Smock, Granda, and Hoelter 2012). See the online appendix for details on the data sources and variable construction.

[a]Coefficients are least-squares estimates of τ using a restricted specification of equation 1 as described in the text. Heteroskedasticity-robust standard errors corrected for arbitrary within-state covariance are presented in parentheses.

[b]The dependent variable in each regression is a dummy variable equal to 1 when the indicated condition is met, and zero otherwise.

[c]Each independent variable reported is a dummy variable equal to 1 when a ban on contraceptive sales was in place in the state where the mother resided and the birth occurred in the indicated period, and zero otherwise. The period 1950–57 is omitted.

[d]Period after introduction of the Pill.

[e]Period after *Griswold* when states with sales bans lifted these restrictions.

Table 2

Estimates the Effects of the Pill and *Griswold* on Weight at Birth[a]

Independent variable[b]	Dependent variable = logarithm of the share of births at low birthweight			
	2-1	2-2	2–3	2–4
PillSalesLegal × 1958–65[c]	−0.007 (0.007)	−0.005 (0.007)	0.025 (0.017)	−0.009 (0.038)
PillSalesLegal × 1966–76[d]	0.004 (0.016)	−0.004 (0.012)	0.051 (0.031)	−0.001 (0.059)
Mean of dependent variable (not in logarithms)	0.0776	0.0684	0.127	0.123
No. of observations	1,104	1,102	1,095	368
R^2	0.964	0.936	0.914	0.899
Sample	All births	White births	Nonwhite births	Nonwhite births, South

Source: Author's regressions using data from the 1954–67 volumes of Vital Statistics and NCHS (2003).

[a] Coefficients are least-squares estimates of τ using a restricted specification of equation 1 as described in the text. Heteroskedasticity-robust standard errors corrected for an arbitrary within-state covariance are in parentheses.

[b] Each dummy variables equal to 1 when a sales of contraceptives were legal in the state where the mother resided and the birth occurred in the indicated period, and zero otherwise. The period 1950–57 is omitted.

[c] Period after introduction of the Pill.

[d] Period after *Griswold* when states with sales bans lifted these restrictions.