Less is More: How Family Size in Childhood Affects Long-Run Human Capital and Economic Opportunity*

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Abstract

This paper examines the impact of family size on children's long-term wellbeing. The number of siblings is a prominent aspect of childhood family environments that affects parental time and resource investments. Leveraging temporal and county-level variation in access to abortion in the United States during the 1970s, my research design contrasts adult outcomes of children born just before an abortion clinic opened with adult outcomes in counties in which abortion remained difficult to obtain. The results suggest that access to abortion decreases the completed number of younger siblings. As their parents avoided unplanned children and achieved smaller family sizes, the children experienced significant improvements in their long-run outcomes, including increased educational attainment, greater labor-force participation, and higher neighborhood quality. The effects are larger in areas with greater exposure to safety net programs. These findings imply large, persistent returns to reproductive health policies that promote smaller families.

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[T]he shift from couples having large families and making small investments in their children to having small families and making large investments in their children is one of the fundamental dimensions of economic development.

- David Lam, The Population Association of America Presidential Address, 2011

1 Introduction

From 1960 to 2017, the average number of children per U.S. woman fell from 3.7 to 1.8.[1] This rapid transition to smaller families associated strongly with improvements in children's education and economic outcomes. Theoretical work in economics suggests that family size influences parental investment and therefore "child quality" (Becker, 1960; Becker and Lewis, 1973; Willis, 1973), as well as economic growth (Becker and Barro, 1988). Consequently, policymakers in many countries have implemented policies to influence parents' decisions regarding the number of children they have. Besides the clearest example from China's One Child Policy, countries such as Mexico, India, Colombia, and Indonesia promote family planning publicly. In the United States, advocates of family planning programs emphasize family size as a crucial determinant of child development.

However, little empirical research has shown that family size has a causal effect on children. The major challenge is the endogeneity of family size, which is potentially correlated with omitted variables.[2] Recent studies rely on instrumental variables, using arguably exogenous sources of variation in family size due to twinning (Rosenzweig and Wolpin, 1980) or the sex composition of existing children (Angrist and Evans, 1998). Yet these pioneering strategies provides remarkably inconclusive evidence on the effects of family size on child outcomes (Angrist, Lavy and Schlosser, 2010).[3] A separate strand of literature evaluates the effects of reproductive health policies more generally, but such evaluations do not disentangle the family size effect from the selection effect. For instance, extensive literature documents improved cohort characteristics after abortion legalization, including education and economic outcomes, but attributes such gains largely to the fact that children are more likely to be born into families with more advantageous environments after abortion was legalized. Indeed, these estimates reflect the combined influence of the selection channel and an

[1] Total fertility rates according to OECD (2019), Fertility rates (indicator). doi: 10.1787/8272fb01-en (Accessed in June 2019)

[2] Numerous studies find that smaller families are associated with better child outcomes, but such descriptive studies are subject to potential omitted variable problems. See Schultz (2005) for a review of these studies.

[3] This approach is also applied to study the effects of fertility on women. Researchers have documented that smaller families increase labor supply for mothers (Angrist and Evans, 1998; Rosenzweig and Wolpin, 2000).

effect through family size.[4] More than 50 years after the onset of America's most recent demographic transition, the size *and* sign of the effects of family size on children remain poorly understood.

This paper provides new evidence on the long-run effects of family size by exploiting the staggered roll-out of U.S. abortion clinics. Using information on the number of abortion service providers between 1970 and 1979, I show that although legalization of abortion occurred at the state level, service availability varied considerably by county. This staggered introduction at the county level provided an exogenous shock to family size for cohorts born before the roll-out. Using an event-study empirical strategy, I show that children born just before abortion became accessible experienced significant reductions in their number of younger siblings relative to children born where abortion remained difficult to obtain. The effect is stronger when less time has elapsed between birth and abortion clinic's roll-out, and not driven by selection on family environments since these pre-abortion cohorts were not selected into birth. Compared to cohorts without access to abortion service providers, the cohort born immediately before abortion became available had a family size that was 0.277 smaller on average.

Using large-scale Census/ACS data that contain a rich set of adult outcomes linked to administrative data that contain date and place of birth, I then examine the long-run wellbeing of children born into smaller families. I find that on average, decreasing family size by one causes 0.146 more years of schooling, reflecting a 1-percent increase in high school graduation and an 8-percent (2.7 percentage points) increase in college completion. The gains are driven primarily by men, who experienced a 0.242-year gain in schooling and a 13.4-percent (4.4 percentage points) increase in college completion. Gains in human capital also persist as gains in economic self-sufficiency, since men particularly experienced a 1.4-percent increase in labor supply on the extensive margin and a 4.5-percent increase in wages. Further, smaller families led to significantly improved living circumstances, measured by a neighborhood quality index that consisted of family income, home ownership, and share of children in poverty in the census tract of residence. In contrast, abortion access had no effect on children born more than eight years before access, since their chances of having younger siblings were unlikely to be affected.

[4] Extensive evidence documents such selection and characteristics of the affected 'marginal child.' See Gruber, Levine and Staiger (1999) and Ananat et al. (2009). Studies on other policies also experience similar challenges. Bailey, Malkova and McLaren (2018) simulate the role of selection in family planning and use a bounding exercise to estimate the quantitative importance of selection and resource effects.

This paper is first to exploit and validate within-state variation in abortion access as an exogenous source of variation in family size. A long-standing concern in the family size literature is that instrumental variables used so far are potentially subject to violations of the exclusion restriction. Twinning is more common in older mothers. Having twins also heightens health risks for the babies *and* mothers, and brings extremely close spacing between two children difficult for parents to cope with. The same-sex instrument affects sex-specific cost savings, since existing same-gender children might benefit from *hand-me-down* economies which offset the family size effect (Rosenzweig and Wolpin, 2000). Such omitted factors have unobserved and unclear effects on later outcomes, contributing to the imprecision of existing estimates. My findings suggest that abortion availability has a substantial effect on children's family size. Importantly, no detectable changes in family background variables were found for these children. Contrary to previous research that suggests lack of a detectable family size effect, I find that reduction of family size induced by abortion access improves individuals' wellbeing over the lifecycle.

My research design is novel in that it focuses on cohorts born just *before* abortion service roll-out, instead of on those born after. A primary identification threat to the abortion literature has been that abortion not only changes family size and resources, but also induces selection into parenthood and thus influences the composition of children born after the abortion roll-out (Levine et al., 1996; Gruber, Levine and Staiger, 1999; Malamud, Pop-Eleches and Urquiola, 2016; Bailey, Malkova and McLaren, 2018). Even if the resources available to a family did not change, selection alone would lead to better outcomes for the average child. To address this problem, I focus on the cohorts born just before abortion became available. I find that these existing children were not different in terms of the composition of their family backgrounds. The roll-out of abortion providers just after they were born, however, affected their childhood family environment through the number of younger siblings they had. This isolation allows me to provide direct evidence on the sign and magnitude of the family size effect. In addition, focusing on pre-abortion cohorts also allows me to exclude the mechanism of *cohort* size and birth timing because the size of the cohort should remain largely unchanged across regions before abortion roll-out, as was parents' ability to intentionally time pregnancies.

Large-scale administrative data not only provide the precision to detect potentially small effects, but also contain crucial geo-code information at birth and thus allow within-state analyses.

Discussions of existing cross-state studies on abortion legalization have raised a concern of confounding state-level factors during the 1970s, the same time abortion was legalized in the United States. Besides various state characteristics that might be trending differently, the Vietnam War brought significant variation in the hardship deferment for paternity (Bailey and Chyn, 2018). Establishment and subsequent elimination of deferment eligibility for paternity during the late 1960s and early 1970s changed the incentives for married couples to have children. In this paper, any changes at the state level are accounted for by the state-by-cohort fixed effects in my empirical model. The effects of family size on long-run wellbeing are identified solely by within-state comparisons.

Besides highlighting the importance of family size in its own right, another contribution of this paper is that I find interactions between family size and public programs. Investigating heterogeneity in the effect, I show that a smaller family size during childhood increased long-run human capital returns to Head Start, a large-scale public preschool program aimed at reducing poverty. A decrease in family size also improved the effect of Food Stamps on economic self-sufficiency.

Family size has received broad attention from researchers and policymakers. This paper is first to assess a causal link running from abortion access to family size, and eventually to individuals' lifecycle outcomes. Overall, my estimates suggest a strong, sustained family size effect on a wide array of long-run outcomes. These findings elucidate the role of reproductive health policies in promoting human capital and reducing poverty.

The rest of the paper proceeds as follows. Section 2 introduces a conceptual framework for identification of the family size effect. Section 3 summarizes prior research and provides background on access to abortion in the United States. Section 4 describes the Census/ACS and administrative data and presents the empirical model. Section 5 presents the results. Section 6 discusses mechanisms and interaction with safety net programs. Section 7 concludes.

2 Identifying the Family Size Effect: A Conceptual Framework

The legalization of abortion had broad influences. In this section, I present a conceptual framework that 1) formalizes the distinction between *family size* and *selection* effects through which abortion legalization might affect cohort characteristics, and 2) motivates an identification strategy aiming to isolate the family size effect. This parameterization illustrates the mechanism and is not imposed during empirical estimation.[5]

2.1 Model Overview

The seminal model from Becker (1960), Becker and Lewis (1973), and Willis (1973) highlights parents' endogenous fertility choices and the interrelationship between child quantity and quality. Following the parametrization of this model described by Mogstad and Wiswall (2016), consider a unitary household that chooses its number of children, N, and how much to invest in them to achieve child quality Q. Parents divide their resources between private consumption, C, and investment in their children to maximize their utility, or:

$$U(N, Q, C) = [(\alpha N^{\sigma} + (1 - \alpha)Q^{\sigma})^{1/\sigma}]^{\nu} C^{1-\nu}$$

The elasticity of substitution between child quantity and quality is $\frac{1}{1-\sigma}$. The price of child quality, p, which specifies a linear child quality production function, is assumed known to the parents.[6] The budget constraint of a household with income I is represented by $I = C + pQ \cdot N$, and is known to the parents. The quality of all siblings within a family is assumed to be equal, despite the well-documented birth order effects (Black, Devereux and Salvanes, 2005; Lin, Pantano and Sun, 2019), in order to highlight the key mechanism.

Both Q and N are choices, but with any exogenously given N, the locus gives the optimally chosen child quality $Q^*(N)$. The family size effect is then thought of as the change in the quality of a child when an exogenous shock increases quantity from N to $N + 1$.

Consider the possibility of a pregnancy in excess of the desired family size due to contraception failure. A household with its ideal family size thus has another pregnancy and makes subsequent decisions. For household i with ideal family size N_i^*, it may end up in one of the following three scenarios:

1) No extra pregnancy, in which case the utility is:

$$U(N_i^*, Q(N_i^*))$$

[5] A review of the classic quantity-quality model and its extensions appears in the Appendix.

[6] See Cunha, Elo and Culhane (2013), who relax this assumption.

2) An extra pregnancy occurs and results in birth, in which case the utility is:

$$U(N_i^* + 1, Q(N_i^* + 1))$$

3) An extra pregnancy occurs followed by abortion, in which case the unwanted child is avoided and the parents have utility:

$$U(N_i^*, Q(N_i^*)) - A$$

where A represents the cost of abortion, monetary and non-monetary, the parents incur. This cost is known after the additional pregnancy is realized [7].

Note that an implicit assumption of this model is that any contraceptive failure when a household has not yet reached its ideal family size is simply treated as mistimed. It represents a planned pregnancy that will be kept, and does not lead to a permanent, suboptimal excess birth.[8] It also assumes, for simplicity, that there can only be a maximum of one excess birth. Intuitively, couples become more careful and act to eliminate the possibility of future contraceptive failures once they have experienced their first unwanted pregnancy.[9]

Parents choose abortion when:

$$U(N_i^*, Q(N_i^*)) - U(N_i^* + 1, Q(N_i^* + 1)) > A \tag{1}$$

that is, when the utility loss due to an extra child is large enough and parents' willingness to avoid it outweighs the cost of abortion.

By definition, 'willingness to avoid' equals the decrease of utility when exceeding the optimal family size by one, which is tied to the parameters in the household utility function. For example, families with greater σ_i in their utility function (i.e., greater elasticity of substitution between quantity and quality) have lower decreases in utility moving from N^* to $N^* + 1$. The intuition is

[7] Another option is to give the child up for adoption, which, similarly, maintains the existing family size at some cost to the parents.

[8] Suboptimal timing, particularly when pregnancy happens sooner than expected, may also affect parental utility and child outcomes (Nguyen, 2018).

[9] I also assume for simplicity that a household does not have fewer children than it wanted. In reality, this is another possible deviation from fertility expectations that couples experience.

that parents with high elasticity find it easier to compensate the unexpected change in quantity by adjusting quality, and those with low elasticity are affected more severely and are more likely to choose abortion given the same cost.

Let σ^A represent the cutoff σ such that the willingness to avoid equals A. Anyone with $\sigma < \sigma^A$ decides to have an abortion. It follows that lower A leads to a greater threshold value of σ^A, and therefore more individuals decide to have abortions to terminate their pregnancies.

2.2 Model Implication: Family Size Effect and Selection Effect

The roll-out of abortion access can be modeled as an exogenous reduction to A. Legalization of abortion makes it easier to obtain, but there are still several monetary *and* psychic costs associated with terminating one's pregnancy even after abortion is legalized. The roll-out of abortion service providers does not simply eliminate A either, and parents might still find it desirable to give birth to the unwanted child. Nevertheless, a decrease to A effectively truncates the distribution of σ^A across all households, leading to more abortions and fewer households having unwanted children. One key implication is that abortion legalization affects cohort characteristics through two distinct channels — family size and selection.

Consider a measure of quality for the average child in a cohort. Since the average child represents the mean outcome of the entire cohort, the measure is given by:

$$Y = \int_{\sigma > \sigma^A} Q(N_\sigma) f(\sigma) d\sigma$$

As abortion becomes increasingly available, the cost of abortion drops from A_1 to A_2.

Reduced cost leads to more abortions since a family's willingness to avoid is more likely to exceed the cost, and families have smaller N. Any child from a family that would have found the cost of abortion prohibitively high, but now decides to have an abortion experiences the family size effect:

$$\frac{\Delta Q(N)/\Delta N}{\Delta N/\Delta A}$$

Importantly, this effect is experienced by all children in the family, including the existing children born before optimal family size is reached.

Meanwhile, abortion access also alters which pregnancies become births because families have different reactions to the cost reduction depending on their respective σ. Since some parents decide not to give birth, the average child changes due to the selection effect. As A decreases, σ^A increases and more of the lower end of the σ distribution is truncated. Cohorts born after abortion legalization comprise of fewer ‘marginal children’ due to selection into childbirth.

Collectively, in the measure of the average child's outcome, $Y = \int_{\sigma>\sigma^A} Q(N_\sigma)f(\sigma)d\sigma$, the change in the cost of an abortion affects two distinct channels — σ^A and N.[10]

An empirical strategy that compares cohorts born before and after abortion legalization measures the total treatment effect, consisting of both the family size and selection effects:

$$T = \int_{\sigma>\sigma^{A_2}} Q(N^{A_2})f(\sigma)d\sigma - \int_{\sigma>\sigma^{A_1}} Q(N^{A_1})f(\sigma)d\sigma$$

This total effect can be decomposed into the two channels:

$$\begin{aligned}
T &= \int_{\sigma>\sigma^{A^2}} Q(N^{A_2})f(\sigma)d\sigma - \int_{\sigma>\sigma^{A^1}} Q(N^{A_1})f(\sigma)d\sigma \\
&= \int_{\sigma>\sigma^{A^1}} Q(N^{A_2})f(\sigma)d\sigma - \int_{\sigma>\sigma^{A^1}} Q(N^{A_1})f(\sigma)d\sigma - \int_{\sigma^{A^1}}^{\sigma^{A^2}} Q(N^{A_2})f(\sigma)d\sigma \\
&= \underbrace{\int_{\sigma>\sigma^{A^1}} [Q(N^{A_2}) - Q(N^{A_1})]f(\sigma)d\sigma}_{\text{Family size effect}} - \underbrace{\int_{\sigma^{A^1}}^{\sigma^{A^2}} Q(N^{A_2})f(\sigma)d\sigma}_{\text{Selection effect}}
\end{aligned}$$

2.3 Family Size Effect: A Valid Identification Strategy

To identify the family size effect, consider the estimate obtained by comparing children born *before* abortion was legalized between legal and illegal regions. Individuals born before abortion legalization still experience the family size effect since they have different possibilities of having an unwanted younger sibling. The change in quality from $Q(N^{A^1})$ to $Q(N^{A^2})$ persists, whereas the change in composition from σ^{A^1} to σ^{A^2} is zero by construction. The treatment effect can be written

[10] Many studies empirically document the role of selection. This channel becomes an inevitable identification threat when estimating abortion's family size effect on a given family. Likewise, studies that focus on the selection channel also note the potential biases caused by changing family size. See Gruber, Levine and Staiger (1999).

as:

$$T = \int_{\sigma > \sigma^{A^1}} Q(N^{A^2}) f(\sigma) d\sigma - \int_{\sigma > \sigma^{A^1}} Q(N^{A^1}) f(\sigma) d\sigma$$
$$= \int_{\sigma > \sigma^{A^1}} [Q(N^{A^2}) - Q(N^{A^1})] f(\sigma) d\sigma$$

To estimate this effect, it is necessary to examine the Q and N of the average child in a cohort born *before* abortion was legalized in places where their parents had access, in comparison with that of the average child in places in which abortion was illegal or difficult to obtain. The identifying assumption that needs to be satisfied is that legalization, conditional on covariates, must be unrelated to family background and previous child investment decisions. Moreover, legalization can influence the children only through family size. The following sections describe the institutional background of abortion legalization and introduce several validity tests that support this identifying assumption.

3 History of Abortion Legalization and Service Roll-Out

3.1 Legalization and its Family Influence by State

For nearly three-quarters of the twentieth century, abortion was illegal everywhere in the United States. In 1970, five states became the earliest to effectively legalize abortion, when New York, Washington, Alaska, and Hawaii repealed anti-abortion laws, and California entered an era of de-facto legalization following a late-1969 ruling that abortion ban was unconstitutional. In January 1973, abortion became broadly available in all states, following the landmark decision in *Roe v. Wade*.

The impact of legalization was both immediate and large; the number of abortions increased sharply. The monetary cost of the procedure is estimated to have lowered significantly to \$80 from the previous \$400 to \$500 for illegal services (Kaplan, 1988), or the cost of traveling abroad (often to Europe) while pregnant. As a result, U.S. births dropped considerably during the early 1970s (Levine et al., 1996; Levine, 2004; Angrist and Evans, 2000). Levine et al. (1996) estimate an 8 percent reduction in birthrate following the actions of the early-legalizing states. Their primary difference-in-difference results suggest a decline in relative birth rates between early-legalizing states and the remainder of the country starting in 1970, which recovered when all states legalized abortion beginning 1973. Notably, the birthrate discussed here is contemporaneous. It remains unclear whether this dramatic change in birthrate represents a decrease in women's completed fertility.[11]

Using a longitudinal survey dataset from the Panel Study of Income Dynamics (PSID), I first examine the impact of abortion legalization on women's completed fertility. The dataset was constructed at the child level and contains individuals' years of birth, states of birth, and numbers of siblings during their lifetime, obtained from linked mothers' birth records.[12] These variables allowed me to examine whether abortion legalization led to smaller family sizes for the existing children.

If abortion availability increases the ability to avoid exceeding an optimally chosen family size, a decrease in family size in areas that legalized abortion is expected. Both demography and economics literature evidences the effectiveness of abortion on a family's ability to avoid unwanted childbirths. For existing older children who live in such families, it follows that they experience decreases in the number of younger siblings. Indirect evidence suggests that this effect is strong; most unwanted children in the United States are later-borns in a family, who would have been younger siblings to children in the same family born earlier (Child Trends, 2013; Lin, Pantano and Sun, 2019).[13]

Panel A of Figure 1 plots the covariate-adjusted difference in family size between early-legalizing states and the remainder of United States by birth year.[14] The pattern is largely as expected, and movement appears similar to what Levine et al. (1996) document for birthrate, shifted two years earlier. A reduction in average family size is visually evident after 1967 in early-

[11]One assessment of this topic can be found in Ananat, Gruber and Levine (2007), who document an increased number of childless women. Due to limited data, they document no completed fertility effect at the intensive margin, or influences on affected cohorts' long-run wellbeing.

[12]To ensure family size information reflected the *eventual* number of siblings, I included only children whose mothers most recently reported her number of children ever had after she turned 40. See Appendix A1 for detailed descriptions of the PSID data and sample construction.

[13]Unwanted children are rare among first births. Parents often consider early-born children mistimed and unintended but nevertheless wanted. Related terminology is well-developed in demography literature. See Santelli et al. (2003).

[14]The covariate-adjustment exercise was conducted using generalized difference-in-differences specification: $Y_{i,b,s} = \delta_s + x_{i,b,s}\beta + \phi_t \sum_{j=a}^{A} 1(b=j) + \pi_t \sum_{j=a}^{A} 1(b=j) \times Repeal_s + \epsilon_{i,b,s}$, where $Y_{i,b,s}$ is the total number of younger siblings of individual i born in state s in year b. Vector $x_{i,b,s}$ is a set of individual- and family-level covariates, including race, gender, and mother's completed education and age at birth. δ_s is either a set of state of birth fixed effects or an indicator of whether the state of birth was among early legalizers that repealed the abortion ban in 1970. The coefficient of interest, π_t reflects the change in the relationship between birth cohorts and their eventual number of younger siblings when early-repeal states legalized abortion and other states maintained the ban.

legalizing states. A gap appears and then closes within three birth cohorts. Important to this paper's identification strategy, the decline first appears among cohorts born two years before (in 1968) the abortion ban was lifted in early-legalizing states (in 1970). Cohorts born further before the repeal had more time elapsed between birth and access to abortion, and therefore had as many younger siblings as when abortion was illegal. The difference remains statistically indistinguishable from zero in every year before 1968, whereas cohort born immediately before legalization experienced the largest reduction in family size (0.232).

Family size depends on many factors, with one concern being that trends related to a mother's behaviors or characteristics might have changed around the same time abortion access varied. Appendix Figure A1 shows the difference in the number of siblings between early-legalizing and remaining states, stratified by several maternal characteristics. These characteristics are common predictors of unwanted pregnancies and thus those that relate closely to the probability of having an abortion. Indeed, all of these predictors appear to drive the reduction in the number of siblings more than the overall sample; the effect appears strongest among children born to unmarried women (significant at the 10% level), and among mothers who were over 35, had low-income, and non-white.

Panels B and C of Figure 1 show regression coefficients by parity. Patterns among first births and higher parities appear similar. The consistency indicates the events that might have created incentives for families to have their first child earlier either did not have a significant impact or the effect is uncorrelated with having a smaller family. Appendix Table A1 shows the magnitude of the effect by parity and with estimates placed into three-cohort bins to increase precision.

3.2 Discussion of Cross-State Analysis

Since the innovative use of the staggered legalization of abortion for quasi-experimental research, many studies have examined the impacts on children and also discussed problems with relying on cross-state variation extensively. Cohorts born after abortion access experience lower infant mortality (Gruber, Levine and Staiger, 1999) and decreased instances of adolescent substance use (Charles and Stephens, 2006). Women who would have mothered these children experience significant increases in college graduation (Ananat et al., 2009). Yet these compelling results are debated constantly (Donohue and Levitt, 2004; Dills and Miron, 2006; Foote and Goetz, 2008; Donohue III and Levitt, 2008).

One major challenge is that cross-state comparisons of adolescent and adult outcomes are potentially subject to influences from factors other than abortion. States that repealed abortion bans prior to *Roe v. Wade* are special in that their social policies are progressive in other ways. Laws passed around the same time by these states might correlate with the legalization of abortion. Some of these laws are specifically related to reproductive health. For example, legal restrictions on the birth control pill varied considerably during the 1960s (Bailey, 2010) and were decided largely by states.[15] Other laws did not target family planning but created alternative channels that might affect later-life outcomes considerably. For example, state supreme courts ordered school finance reforms beginning in 1971 (Hoxby, 2001), overturning public K-12 school finance systems for the same cohorts affected most by abortion legalization. Claims that abortion legalization lowered crime rate are disputed by Joyce (2004) who notes the potentially confounding crack cocaine epidemic, which began during the late 1980s and arrived in New York and California earlier than elsewhere.

Even when changes occurred at the national level, they threaten identification in situations where they cause heterogeneous effects across states. Bailey and Chyn (2018) found a considerable fertility response during the Vietnam War when American families sought ways to avoid military service; one way to qualify for deferment was having children. The incentive appears significantly larger in the five states that legalized abortion earlier, since their anti-war sentiment was much higher than elsewhere in the United States. This differential response creates confounding factors for abortion estimates resulted from cross-state comparisons, since children born to families that had compelling reasons to avoid serving in the military might have grown up in very different childhood circumstances than others did.

To circumvent these concerns about cross-state analysis, this paper exploits the staggered availability of abortion clinics at the county level.[16]

[15] Bailey (2010) find that sales bans of the Pill does not correlate with repeal of abortion bans.

[16] As a proof of concept, I also implement a similar design as that used in previous cross-state comparisons. Appendix Table A2 presents state-level estimates on children's completed education using PSID. The results suggest that those born immediately before the 1970 policy change in early-repeal states tended to complete more years of education in comparison to older cohorts, and in comparison to individuals born in non-repeal states. In the preferred specification with year and state of birth fixed effects (column 2), consistent with the hypothesis that having unwanted younger siblings causes disruptions in parental investment and compromises family environments, the same cohorts of individuals who experienced a decrease in young siblings also experienced a 0.321-year increase in completed education. The effects appear driven by higher education, with college completion having had the largest effect (15.5 percentage points, column 8). If there were state-level changes, state-specific trends, or national events that imposed differential effects that correlated positively with both repeal of the abortion ban and education, then

3.3 County-level Roll-Out of Abortion Services

Although abortion was legalized by states and then nationwide later, its availability developed in a pattern much more dispersed across the country. As many critiques of state-level comparisons point out, an ideal research design would be to exploit some form of within-state variation of abortion access.

Using county-level information on service providers made available by the Guttmacher Institute,[17] Figure 2 shows the roll-out of abortion service providers in each continental U.S. county.[18] The map shows idiosyncratic variation in access timing; some counties were able to establish service access immediately, likely converting existing clinics to part-time providers. Others did not have service providers until much later. The staggered introduction is widely observed within each state.

Demonstrating the validity of a roll-out design is critical to this paper's analysis. A formal test would need to show that the year abortion services began in a county is unrelated to various county-level characteristics, and to potential underlying trends in fertility that are county-specific. One hypothesis is that abortion clinics first appeared in areas that are more progressive, highly-educated, and affluent. Economic circumstances is commonly associated with fertility declines (Foster, Rosenzweig et al., 2007), and areas might have varying demand for abortion based on education and income level. This would have likely caused some areas to gain access to abortion earlier and be more developed in human capital at the same time. Policymakers and service providers, and their funding sources, might also have targeted some areas. For example, resources might be directed first to areas with the highest incidence of unwanted births or observe an increasing trend. All of these channels create potential violations of the exclusion restriction, which requires abortion service roll-out to be uncorrelated with unobserved determinants of child development.

I examine such potential correlations in Table 1. I regress a set of county characteristics, collected from the 1970 County and City Data book, on the timing of first abortion service providers in counties that obtained access between 1970 and 1979. Among 16 characteristics, the only predictors of getting a service provider early during the 1970s are total population and share of urban population. It appears, as one might expect, that the service arrived first in the most populous places and

these estimates represent the upper bounds of abortion's human capital effects through family size.

[17]This information was cleaned and used by Bailey (2012)

[18]Information on the number of service providers in New York, California, and Washington between 1970 and 1972 is missing and was therefore extrapolated from 1973.

thus those most in need. The demographic pattern motivates use of urban-population-by-cohort time trends, which I control for in all empirical models.[19]

Notably, roll-out of abortion service providers does not appear to correlate with income, education, leading political party, or the age structure of a county's population. Service seems to have begun in both affluent and less-developed counties following the legalization, despite concerns about endogeneity arising from abortion's varying popularity. A more detailed comparison of different groups of counties appears in the Appendix.

Another threat to identification is that timing of abortion clinic's roll-out might be correlated with counties' fertility rates, which causes omitted variable bias because fertility rate is associated with an area's many unobserved characteristics that also determine children's development. Figure 3 examines this correlation directly. Using data from the NCHS Natality Detail Files, Panel A plots county-level general fertility rates on the timing of abortion clinic's roll-out. It excludes counties from early-legalizing states and focuses on the period between 1973 and 1979. Roll-out timing does not appear to correlate with the *level* of fertility rates immediately before *Roe v. Wade*.

Exploiting the staggered timing also requires careful examination of pre-trends. If clinics were funded to specifically target areas in which fertility rates remained high despite the national trend, were trending up, or were decreasing at a slower rate than the national average, then this identification strategy might capture spurious results due to endogeneity of roll-out timing. Panel B, Figure 3 plots changes in fertility rates between 1968 and 1972 and the timing of abortion clinic's roll-out. A proactively targeted roll-out scheme would predict a negatively slopped line fitted through the figure, but the actual pattern suggests no evidence of selective roll-out at the county level. Despite various hypotheses regarding how service might appear, actual roll-out of abortion service providers appears largely idiosyncratic.

[19] Share of urban population, u, was used to generate five categorical variables, indicating $u = 0$, $0 < u \leq 25$, $25 < u \leq 50$, $50 < u \leq 75$, $75 < u \leq 100$, and interacted with cohort trends.

4 Census, Administrative Data, and Research Design

4.1 The Data

An ideal dataset for analyzing the long-run impact of childhood family size requires information on 1) place of birth or place of residence in early childhood, 2) human capital and economic outcomes well into adulthood, and 3) a sample large enough to detect potentially small effects and that contains enough counties. This paper relies on large-scale, restricted-access data to achieve identification.

I use the newly available long-form 2000 decennial Census and the 2001-2016 American Community Surveys (ACS) linked to the Social Security Administration's Numident file through a protected identification key (PIK). The Census/ACS data include nearly one-quarter of the U.S. population and observe an extensive set of adult outcomes on educational attainment, labor-market participation, family income and poverty status, as well as living circumstances and neighborhood quality.

My sample is comprised of individuals born between 1960 and 1986 to focus around the roll-out of abortion services, and individuals between age 25 and 54 in their prime-earning years. The sample excluded individuals with allocated or missing values and counties with unknown numbers of abortion clinics. To minimize disclosure burden, I constructed a full-information sample to further exclude individuals who are missing any of the outcome variables, except for outcomes that are logged. The result is a final analysis sample of about fifteen million American adults.

Linkage to the Numident file provides information on individuals' exact places of birth, which are matched to counties of birth (Stuart, Taylor and Bailey, 2016). The dataset is then merged with county-level abortion service provider information to measure precisely the relative time between an individual's year of birth and the year of local service access. One shortcoming is lack of family background information in the data, which might help identify mechanisms or create a high-impact sample. Nevertheless, findings using PSID suggested a strong family size channel that can be plausibly isolated, and the sample size is sufficiently large to detect important effects that might be small in magnitude.

To minimize computational resources, I collapsed the data into cells by birth year, survey year, and county of birth. When used during analyses, the cells were weighted using the number of observations in each cell (Solon, Haider and Wooldridge, 2015). I also constructed indices by normalizing and grouping long-run outcomes into three categories — human capital, economic self-sufficiency, and living quality — which helped increase statistical power and mitigated issues related to multiple hypothesis (Kling, Liebman and Katz, 2007).

4.2 Research Design to Capture Abortion's Impact

My research design exploits the natural experiment of abortion service providers' roll-out from 1970 to 1979, with a flexible event-study framework:

$$Y_{bct} = \theta_c + \alpha_t + \delta_{s(c)b} + \boldsymbol{Z'_{cb}\beta} + \sum_{y} \pi_y 1\{b - T_c = y\} Abortion_c + \epsilon_{bct} \tag{2}$$

where Y_{bct} is a measure of adult human capital, self-sufficiency, or living quality at time of survey t for individual born in county c in year b. θ_c is a set of county fixed effects, α_t is a set of survey year fixed effects, and $\delta_{s(c)b}$ is a set of state-by-cohort fixed effects. Importantly, controlling for state-by-cohort effects alleviates concerns raised by critics regarding changes to state policies and varying prior trends that affected birth cohort differentially. Also notably, this level of flexibility accounts for changes in travel distance to one of the early-legalizing states, a factor highlighted by Joyce, Tan and Zhang (2013) and that is also captured by state-by-cohort effects.

To control for several time-varying characteristics at the county level, I include $\boldsymbol{Z'_{cb}\beta}$, a set of county-level observables and demographic controls measured in an individual's county and year of birth (see also Hoynes, Page and Stevens 2011; Bailey 2012; Bailey and Goodman-Bacon 2015). These observables are interacted with a linear trend in year of birth, except for a set of categorical indicators for county population, which is interacted with year of birth dummies to allow more flexible control of the changing population density. $Abortion_c$ is an indicator for ever having at least one abortion service provider between 1970 and 1979. All standard errors are corrected for heteroskedasticity and adjusted for an arbitrary within-county covariance structure (Arellano et al., 1987; Bertrand, Duflo and Mullainathan, 2004).

T_c denotes the year abortion first became available in county c. Event-time y represents an individual's birth year relative to the local roll-out of abortion services. The point estimates of interest are π_y, which capture the evolution, by event-time, of the educational or economic outcomes in counties with access net of changes in untreated counties. The omitted category is set to -8. For example, if the first abortion clinic increased human capital significantly for the cohort born two years before access (i.e., $t = -2$) in comparison to those born eight years before access, the point estimate for π_{-2} should be positive and statistically significant.

The expected results are that for the cohort born immediately before access became available ($t = -1$), the effect on educational and economic outcomes through decreasing numbers of younger sibling should be strongest. Individuals born after abortion became available ($t >= 0$) experienced a combination of the family size and selection effects. Abortion should have little effect on cohorts born earlier ($t <= -8$). Effects from π_{-8} to π_{-1} should increase gradually. Based on the estimated first-stage effect on family size, a test of joint significance for $\pi_{-3,-2,-1}$ should be positive and statistically distinguishable from zero.

5 How Abortion Impacts Family Size and Long-Run Wellbeing

5.1 Abortion Access and Family Size by County

I first examine the effect of abortion providers' roll-out on family size. Information on individuals' date and county of birth is obtained from the restricted-use geo-coded PSID, which I linked to abortion service providers data. I also linked the main PSID interviews with the PSID Childbirth and Adoption History file to obtain information on siblings. Appendix 2 describes the PSID data and sample construction.

I tested the primary empirical specification with several modifications. First, the dependent variable is the completed number of siblings for individual i born in county c in year b. To ensure the sibling count describes an individual's *completed* family size, I collected information only when the most recent observation of the mother was after she turned 40. Second, to improve the precision of the estimates, I controlled for a set of individual-level covariates, including gender, race, maternal education, and birth order in family. I also grouped event-time dummies into three-year bins to improve statistical power.

Figure 4 presents the event-study estimates of this first-stage effect. Compared to a control group of children born approximately eight years before an abortion service provider first appeared in their county, children born closer to abortion becoming available experienced significant reductions in their completed number of siblings, averaging 0.277 fewer siblings (statistically significant at the 10-percent level) for children born the year before the roll-out of service. In contrast, the change in completed family size remained indistinguishable from zero for individuals born eight years or more before abortion became available, since the roll-out did not arrive soon enough to affect younger siblings born after them.

An alternative identification strategy to the event-study specification above is to exploit cross-county variation in the intensity of providers. I replaced the set of event-time dummies, $\sum_y \pi_y 1\{b - T_c = y\} Abortion_c$ in equation (2), with a continuous measure of the number of abortion service providers per 1,000 women aged 15 to 44. This intensity measure is denoted $1(b = j)Prov_c^{73-78}$, which describes the average number of abortion service providers in a county within 5 years of *Roe v. Wade* (1973 to 1978) per 1,000 women aged 15 to 44 interacted with indicators of birth cohorts. Since information on abortion service providers is missing from 1970 to 1972, this exercise also excluded the five early-legalizing states from the sample.

Table 2 presents estimates from this service intensity analysis. For cohorts born just before 1973 in counties with more access to abortion immediately after *Roe v. Wade*, their completed family size experienced a significant decrease. The specification that includes state-specific trends and county characteristics interacted with linear time trends (Column 2) accounts for differential trends that might confound cross-sectional analysis. The 1970-1972 birth cohorts experienced a decrease in the number of younger siblings by 3.18 per increase in abortion service providers per 1,000 women aged 15 to 44, in comparison to the omitted category of 1960-1963 cohorts. Consistent with the event-study estimates in Figure 2, children born more than three years before *Roe v. Wade* do not display any negative relationships between family size and access to abortion. The magnitude of the estimate for 1970 to 1972 cohorts, when multiplied by the sample's average abortion service provider intensity of 0.07, implies an estimated decrease of 0.223 siblings. These results provide further evidence that within-state variation in the availability of abortion is a determinant of children's family size.

5.2 Long-Run Effects on Human Capital, Self-Sufficiency, and Living Quality

I next implement the event-study specification to examine the effect of fewer younger siblings on adult outcomes of these children using three indices — human capital, economic self-sufficiency, and neighborhood quality.

Figure 5 plots estimates for the overall sample in event-time. Among compositionally balanced birth cohorts (i.e., individuals born between fourteen years before and five years after the roll-out of the first abortion clinic), the patterns appear as expected. For each index, access to abortion service had no measurable effect on children born eight years or more before. For these children, it is likely that their parents gave birth to them and their younger siblings while abortion was still illegal or difficult to obtain. However, estimates for all three indices exhibit an increase between event time -8 and -1, when children were born closer to the year abortion service became available. The magnitudes of the effects appear particularly large when approaching event-time -1 (i.e., born just before abortion roll-out), and the joint test of $\pi_{-3,-2,-1}$ rejects the hypothesis of no effect on the human capital and neighborhood quality indices. To the right of event-time -1, the patterns become less consistent across outcomes. Indeed, in the case of economic self-sufficiency and neighborhood quality, estimates become larger than those at -1, potentially due to selection into childbirth.

Event-study estimates plotted in Figure 5 are intent-to-treat (ITT) effects. Table 3 presents the average treatment-effects-on-the-treated (ATET) by scaling event-time magnitudes using this paper's estimated effect on completed family size, a reduction of 0.277. To investigate which characteristics drive the increase in the indices, Table 3 also summarizes statistics and estimates for the main components that comprise three indices. Column 1 presents the mean of the outcome for the control group; individuals born 8 years before abortion roll-out. Column 2 summarizes ITT estimates and standard errors for each outcome of children born just before abortion roll-out (event-time -1). Column 5 presents ATET (coefficients in Column 2 divided by 0.277). Column 6 presents the percentage change implied by the ATET relative to the control group (the ratio of Column 5 to Column 1). I also include results from formal joint tests of zero effect on children born just before roll-out (i.e., event-time -3 to -1, Column 3), and on children too early to be affected by abortion access (i.e., event-time smaller than -8, Column 4). The total number of observations

(15, 891, 000) highlights the large sample size due to the restricted Census/ACS and administrative Numident data.

Overall, a decrease in family size by one caused the standardized human capital index to increase by 4.2 percent of a standard deviation, reflecting a 0.146-year increase in completed education. A major advantage of the large-scale data, besides improved precision, is the ability to investigate a rich set of outcomes that cover many dimensions well into children's adulthood. Estimates for the individual components of the human capital index suggest a moderate 1-percent increase in high school completion and a much larger increase in college completion of 8 percent (2.7 percentage points). College education is the costliest child investment in the United States, and the large estimate accords with the hypothesis that family size mostly affects whether parents can afford to send their children to college. The estimate also appears large for having a professional or doctoral degree, another expensive investment that might depend heavily on families' credit constraints. Individuals born 8 years or more before abortion became available experienced no effect since their family sizes were unlikely to have decreased. With the exception of professional or doctoral degree, tests of joint significance for $\pi_{y,y<-8}$ fail to reject the null hypothesis of no effect in all outcomes.

Since family environments might affect boys and girls differently, Table 4 presents estimates by gender. Gains in human capital appear driven by men, who experienced a 0.242-year gain in schooling, a 1.6-percent (1.5 percentage points) increase in high school completion, and a 13.4-percent (4.4 percentage points) increase in college completion. Having small families also significantly increased men's likelihood to obtain a professional or doctoral degree (1 percentage point) and work in a professional occupation (3.1 percentage points). Overall, for men who had one fewer sibling, the human capital index increased by 6.8 percent of a standard deviation (statistically significant at the 1-percent level).

Gains in male human capital also persisted as gains in economic self-sufficiency. On average, men experienced a 1.4-percent increase in labor supply (Table 4, column 6) on the extensive margin and a 1.5-percent increase in weeks of work. They also experienced a 4.5-percent gain in wage income, making them 17.6-percent less likely to live in poverty. Contrarily, the effect of family size on girls' long-run self-sufficiency appears to be mixed and imprecise despite gains experienced in years of schooling (0.05 year) and professional or doctoral degree completion (32.3 percent, 0.9

percentage points). Women's labor-force participation after growing up in small families appears to have *decreased*. This reduction might be surprising, but is consistent with evidence that college education for women experienced little change during this period, and those who did complete more education likely benefited in the marriage market and experienced a sizable income effect. This channel remains to be tested since one important dimension missing from this exercise is direct observation of family structure and marriage quality.

Nevertheless, estimates for a group of census tract characteristics provide additional evidence of how men and women experienced changes to their adult living standards. Panel C, Table 3 and Table 5 presents results on neighborhood quality measures. For females and males, children born just before abortion access experienced significant improvements in measures of quality of their census tract of residence in adulthood. On average, their neighborhoods have nearly 2-percent more home ownership (significant for women and marginally significant overall).[20] Smaller family also caused a 4.5-percent increase in neighborhood family income and a 6.9-percent decrease in share of children in poverty. The summary index of neighborhood quality experienced an increase of 4.8 standard deviation when decreasing family size by one.

5.3 Testing for Confounding Changes

The main empirical strategy implemented in this paper is valid only if the roll-out of abortion service providers is uncorrelated with other factors that might affect children's long-run outcomes besides family size. To directly examine this key assumption, this section investigates a variety of family background variables to test for changes in determinants of childhood environment that could arise in the treatment group in comparison to the control group. Specifically, my research design, which focuses on the cohorts born before abortion service providers became available, aims to circumvent the issue of selection causing compositional changes of children. Ruling out changes in average family-level observables provides compelling evidence that results are not driven by selection into childbirth.

A large existing literature has laid out several channels that are potentially confounding. For instance, other state-level changes that affect policies or demand for children could create challenges

[20] Corrections for multiple hypotheses testing is needed to interpret the p-values for individual outcomes. An exercise to account for multiple hypotheses appears in the Appendix.

to credible identification. For instance, sample selection on parent's level of education and age could cause changes in family sizes and confound my estimation. Changes that affects marriage and timing of births can have similar effects. Families also make decision about whether to have more children based on observed endowment or characteristics of their existing children. For example, if a child has low birth weight, it is likely that the parents will not have more children and therefore this realization leads to fertility stopping. If parents have such endogenous stopping rules, the validity of my research in identifying a causal relationship will be jeopardized.

To examine these confounding factors, Figure 6 applies the specification in equation (2), but replaces the dependent variable with a series of family-level observables. To examine whether sample selection happens to a certain parity, it also stratifies by first and higher-order births. The results show no detectable effects on maternal education, mother's marital status or age at birth, mother's age at birth, or probability of low birth weight. The effects on number of siblings is significant despite the non-results for these observable characteristics, which supports my hypothesis that for the cohorts born immediately before abortion legalization, the policy change affect their human capital *only* through an improvement in childhood family environment facilitated by a better planned family.

The concern this research design aims to address is that identification using cohorts born after abortion legalization will be confounded by changing composition of the sample. Figure 6 panel B shows this concern appears to be real. For the cohorts born after abortion became available, their parents appear to be more educated, older, and more likely to be married. It is also suggestive that they are less likely to have low birth weight. Therefore, even if significant improvements in life cycle outcomes can be observed for these cohorts, it would be questionable to attribute these impact solely to improved childhood family environment.

6 Interactive Effects: Complementarity or Substitutability?

A long-standing hypothesis about human capital intervention programs is that investing in early childhood generates substantial long-term returns. Disadvantages in early childhood grow over time (Currie and Thomas, 2000), and interventions at this stage are effective in breaking the cycle of poverty because they increase the returns to interventions at later stages; a feature formalized by

Cunha and Heckman (2007) as dynamic complementarity. Although it is impossible to pin down the exact stage when family size intervenes during the dynamic process of child development, evidence of complementarity or substitutability with other human capital programs provides useful insights for evaluating such programs as well as understanding the mechanisms of family size effect. This section examines interactive effects with several public programs introduced in the 1960s. These programs aimed to provide social safety net for disadvantaged children and tackled the causes poverty from several aspects. Exposure to these programs varied considerably in late 1960s and early 1970s across counties (Bailey and Duquette, 2014).

I use the Census/ACS data and implement an extension of the county-level event-study specification. In addition to the main effects of event-time dummies, I include terms that capture the interaction between each event-time dummy and an indicator of high exposure to each public program (Head Start, Food Stamps, Medicaid, and CAP[21]), $\sum_y \phi_y 1\{b-T_c=y\}Abortion_c \times HiExp_{bc}$, to test hypotheses of effect interactions.

The first hypothesis is that reduction in family size facilitates more parental time which complement other programs. Children from smaller families receive more help after school, more attention when having health conditions, and in general are better prepared to benefit of human capital interventions during their childhood. Table 5 suggests that both Head Start and Foot Stamps complement smaller family sizes. The family size effects on human capital and neighborhood quality is significantly larger in places where children were also young enough to attend preschool provided by Head Start. The Food Stamp Program has a significantly positive interaction with smaller family size on economic self-sufficiency outcomes. It appears that children supported by Food Stamps benefit more when they are in smaller families. These findings suggest evidence of complementarities between reduction in family sizes and two of America's largest safety net programs.

Another hypothesis it that reduction in family size and some programs are substitutes. One would expect a negative interaction if the decrease in family size help families avoid experiencing hardship that are targeted by some programs. For example, Gillezeau (2010) documents that the Community Action Program (CAP) decreased the number of riots in 1960s and 1970s. These programs might not have significant impacts if families are already living in peaceful neighbor-

[21] Information about Head Start grants is collected from the National Archives Community Action Program electronic files and made available through the work of Bailey and Goodman-Bacon (2015) and Bailey, Sun and Timpe (2018).

hoods. Consistent with this hypothesis, the CAP's effect on neighborhood quality, which includes average family income and property value in individual's census tract, is significantly smaller for children from smaller families. There also appears to be a negative albeit statistically insignificant interaction between the effects of CAP and smaller families on human capital and self-sufficiency.

Additionally, Appendix Table A4 uses the PSID data and shows the evidence also exists on the state level.[22] Among individuals born between 1960 and 1980, Head Start has a positive and significant effect on completed years of education. Moreover, having an abortion-induced reduction in family size, indicated by born before 1970 in an early-repeal state, increases Head Start's human capital benefit by 1.02 years of education. The effect of the interaction term is largely driven by attending some college, suggesting that the monetary cost of education could be a key channel. On the other hand, college completion rate responds only to the family size treatment (and is marginally significant for the Head Start treatment), but not to the interaction between the two treatments.

7 Concluding Remarks

Since Becker (1960), Becker and Lewis (1973) and Willis (1973) formalized the relationship between child quantity and quality in theory, many researchers have attempted to identify the family size effect in practice, and their findings have been mixed. Especially with modern identification strategies and more comprehensive data from developed countries, estimates have been remarkably imprecise and inconsistent with the theoretical predictions. The endogenous nature of choosing one's family size creates potential threats to identification, and most instrumental variables used previously might affect unobserved factors in the complicated process of raising children. Many researchers attribute the lack of evidence to these empirical caveats.

This paper provides new evidence on the impact of family size. The source of variation I exploit is the staggered county-level roll-out of access to abortion service providers. For the first time, I document that significant reductions in completed family size occurred for children born

[22] In addition to the terms specified earlier, this model includes an interaction between accesses to abortion immediately after birth, and access to Head Start in county of birth when the individual is age eligible (age 5 or younger), $1(b = j) \times Repeal_s \times HeadStart_{bc}$. Additionally, x_{bct} includes interactions between Head Start access and cohorts, and the Head Start indicator. The result is a fully interacted model between cohorts, an indicator of early-repeal state, and an indicator of being age-eligible when Head Start first begun in the individual's county of birth.

just before abortion became available; their parents became more capable of avoiding unplanned children and thus had smaller families in comparison to those who still could not obtain abortion services easily. To examine long-run outcomes, I use confidential Census/ACS data linked to the county and date of birth information in the large-scale administrative Numident file. I find that reductions in family size generated substantial long-run improvements in human capital, economic self-sufficiency, and neighborhood quality. Decreasing family size by one led to an 8-percent (2.7 percentage points) increase in college completion, a 1.4-percent (1.3 percentage points) increase in men's labor supply, and a 4.5 log-point increase in adult wages. For both men and women, smaller families led to better living quality in adulthood, represented by significant increases in average home ownership and average family income of the neighborhood in which they live.

My estimates came from cohorts born just before abortion roll-out instead of those born after. Literature on abortion legalization documents that abortion leads to selection. Children born after abortion access were selected into birth and came from different family backgrounds on average. Focusing on pre-abortion cohorts circumvents this problem and isolates the causal effects of decreased family size. I find no change in measures of family background among pre-abortion children; abortion access affected them only through family size. To this front, my findings also contribute to literature that evaluates consequences of abortion legalization, which commonly highlights a selection effect on the marginal child. My findings suggest that access to abortion also had substantial impacts on 'inframarginal' children.

It is worth noting that the existing quantity-quality literature provides an extensive discussion on why evidence on the family size effect remains inconclusive. Apart from the concern regarding the validity of existing instruments, families with more children might genuinely benefit from economies of scale or have a positive interaction between child quantity and quality in their preferences. Further, any particular instrumental variable can identify only effects on a group of individuals affected by the instrument (Imbens and Angrist, 1994; Angrist, Lavy and Schlosser, 2010). Effects might vary considerably on different margins, and in some cases might even change sign. This paper not only introduces a plausibly exogenous variation in family size that induces substantial effects, but focuses on the margin that aligns with the target of many reproductive health policies. I argue that a smaller family matters, particularly when it is achieved by avoiding having unplanned children in excess of a family's desired fertility.

In regards to implication for human capital and anti-poverty programs, this paper also demonstrates interactions between family size and social safety net programs aimed to support disadvantaged children. My findings imply significant policy complementarities; having small families increases long-run human capital returns to Head Start and the economic impacts of Food Stamps. Overall, these results elucidate how reproductive health policies can, by reducing family size, facilitate children's human capital formation, increase their economic opportunities, and help families escape poverty.

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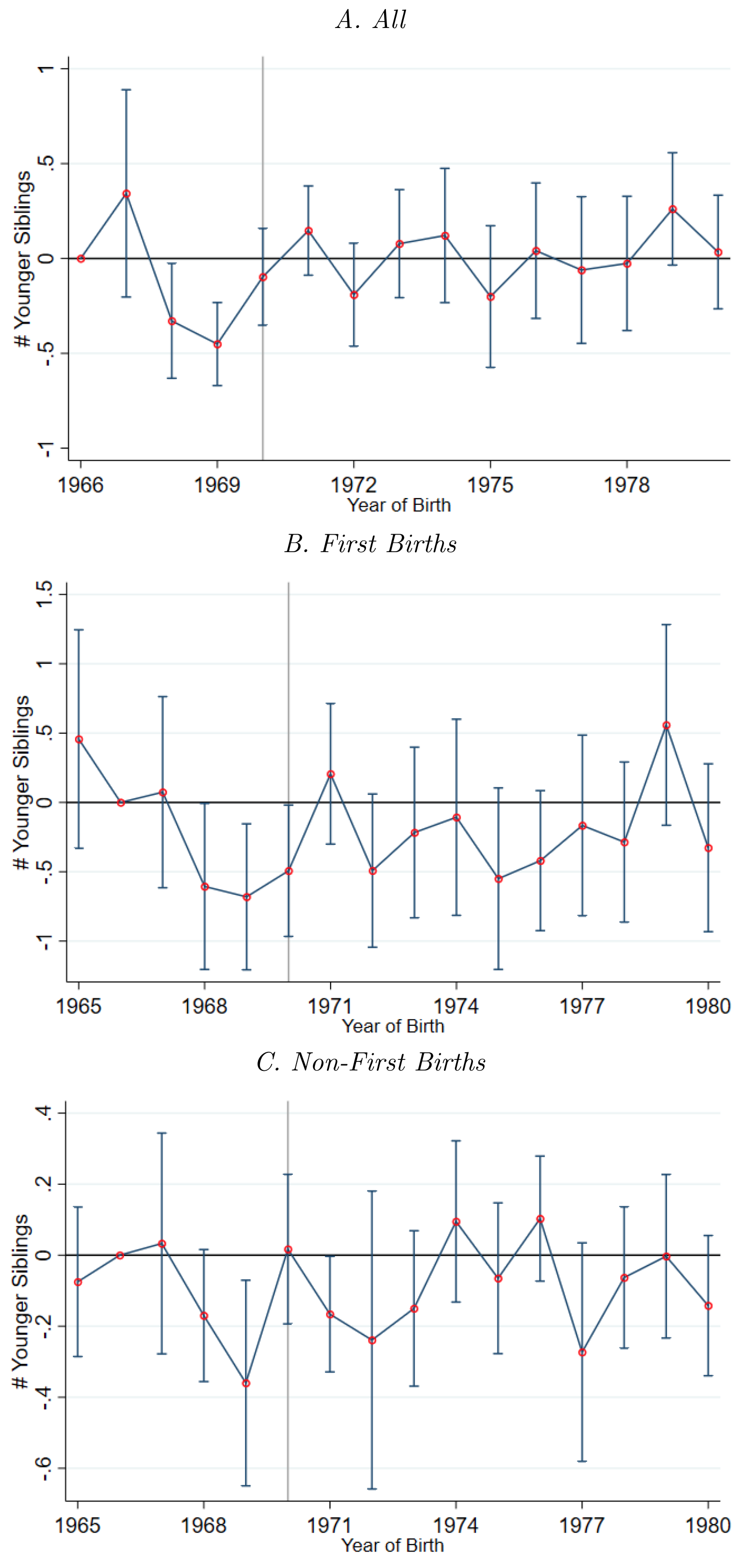
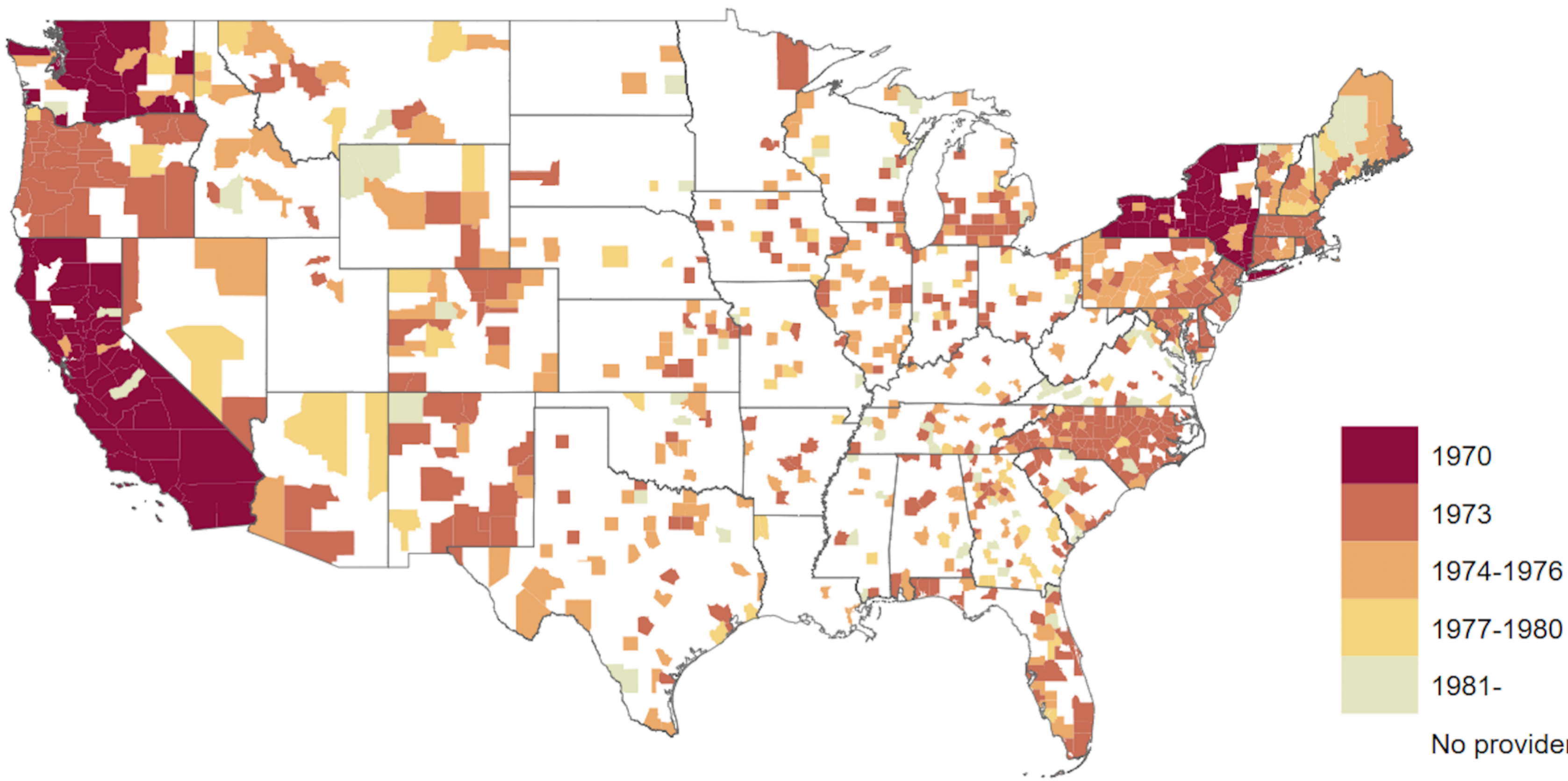
Figure 1. Difference in Number of Siblings, Early-Legalizing vs. Other States



Notes: Point estimates of cohort indicators interacted with the early-legalizing indicator are plotted. Sample includes individuals from PSID Childbirth and Adoption History File whose mother's most recent observation is after she turns 40 years old. Covariates include gender, race, mother's age at birth, and categories of maternal education. Regression controls for year of birth and state of birth fixed effects. Confidence intervals presented are on the 90% level. Heteroskedasticity-robust standard errors clustered at the state level.

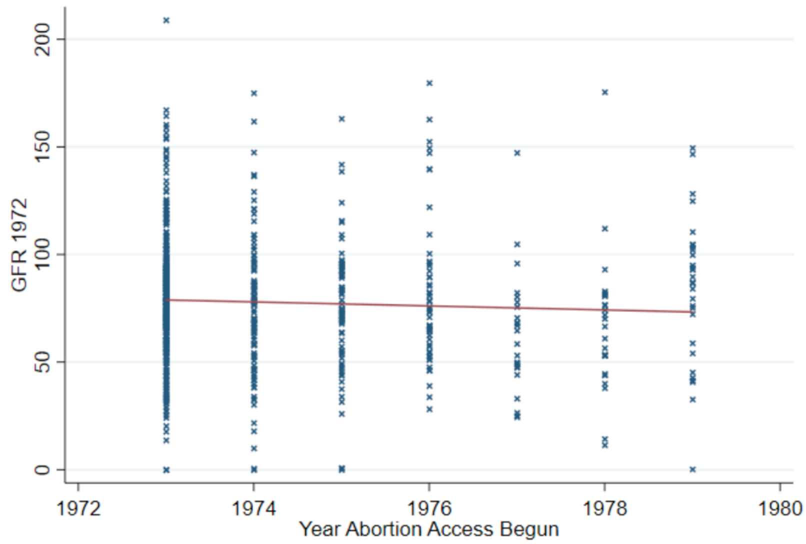
Figure 2. Abortion Service Providers Roll-Out, 1970-1987



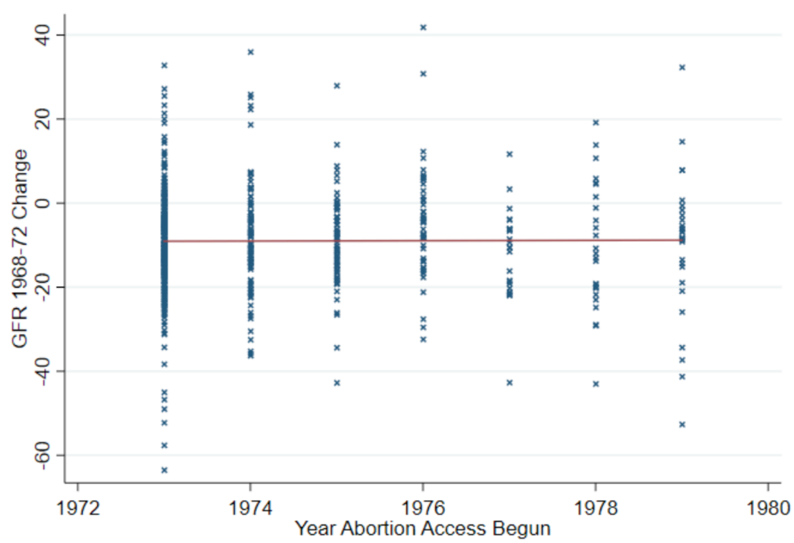
Notes: Number of providers between 1970 to 1972 in early-legalizing states are assumed to be identical to the number observed in 1973.
Source: Guttmacher Institute.

Figure 3. County Fertility Rates and Service Roll-Out

A. Year of Roll-Out and General Fertility Rate (GFR)

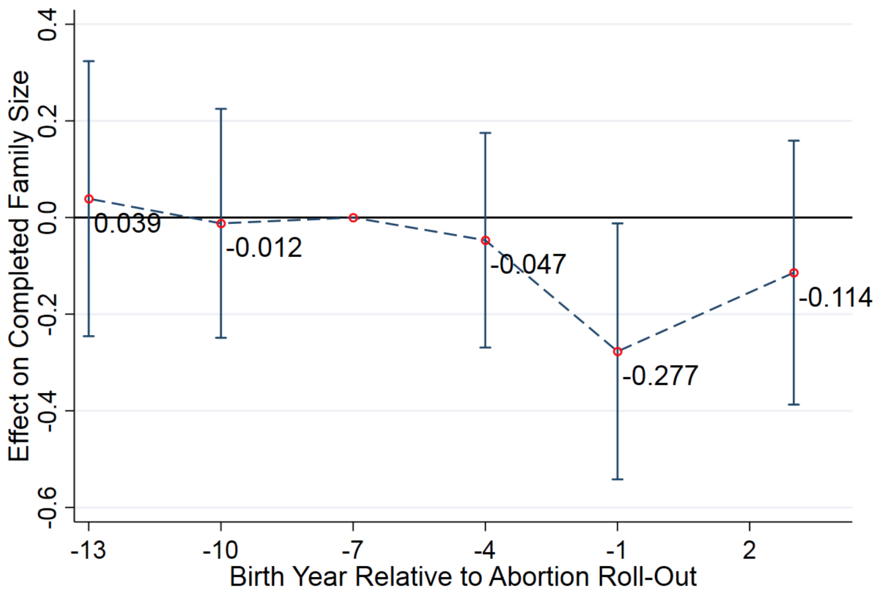


B. Year of Roll-Out and 1968 to 1972 Change in the GFR



Notes: The line indicates the estimated relationship between the GFR or the change in GFR and the year of first abortion service provider in the county. *Source:* NCHS Natality Detail Files, 1968 and 1972.

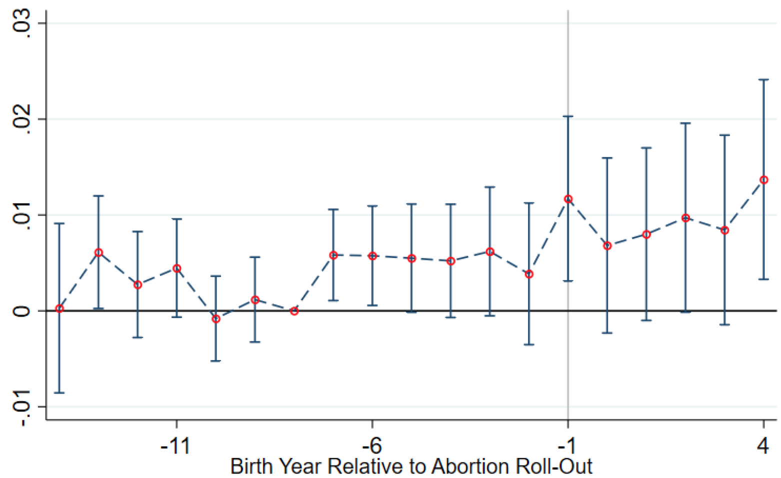
Figure 4. Abortion Roll-Out and Family Size, Event-Study Estimates



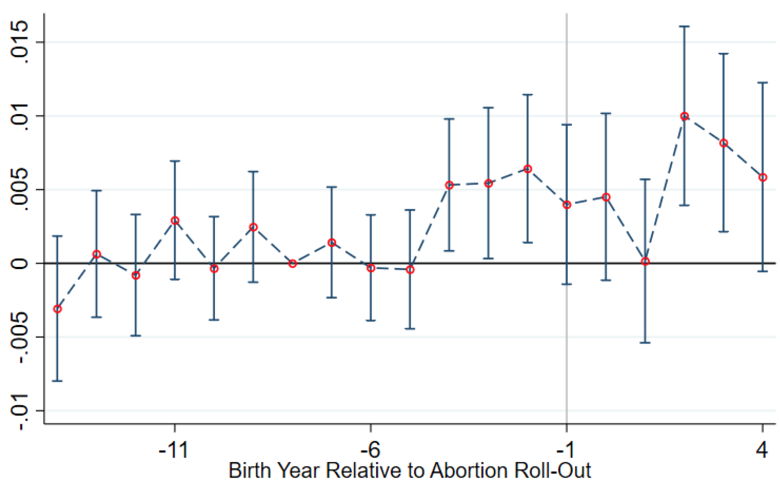
Notes: Dependent variable is completed family size. Point estimates of event-time grouped into three-year bins are plotted. Sample includes individuals from PSID Childbirth and Adoption History File whose mother's most recent observation is after she turns 40 years old. Regression controls for state-by-cohort fixed effects and county fixed effects. 90-percent, point-wise confidence intervals for each estimate are presented. Heteroskedasticity-robust standard errors clustered at the county level. *Source:* Restricted PSID Individual and Childhood and Adoption History File.

Figure 5. Effects of Abortion Roll-Out on Long-Run Indices

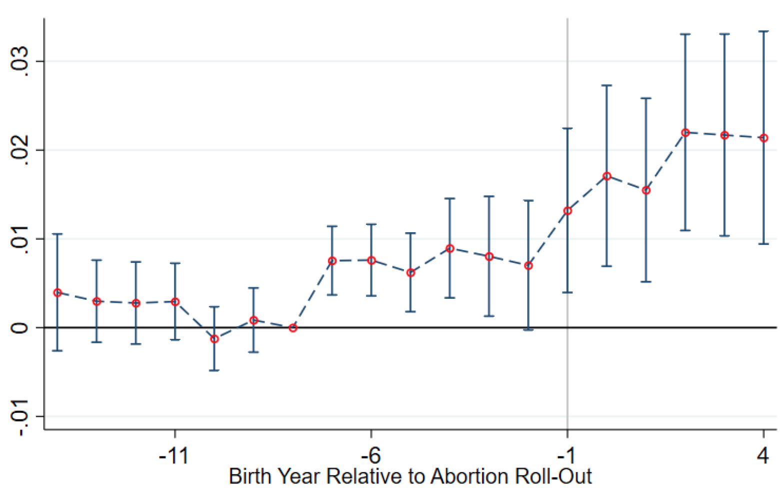
A. Human Capital Index



B. Economic Self-Sufficiency Index

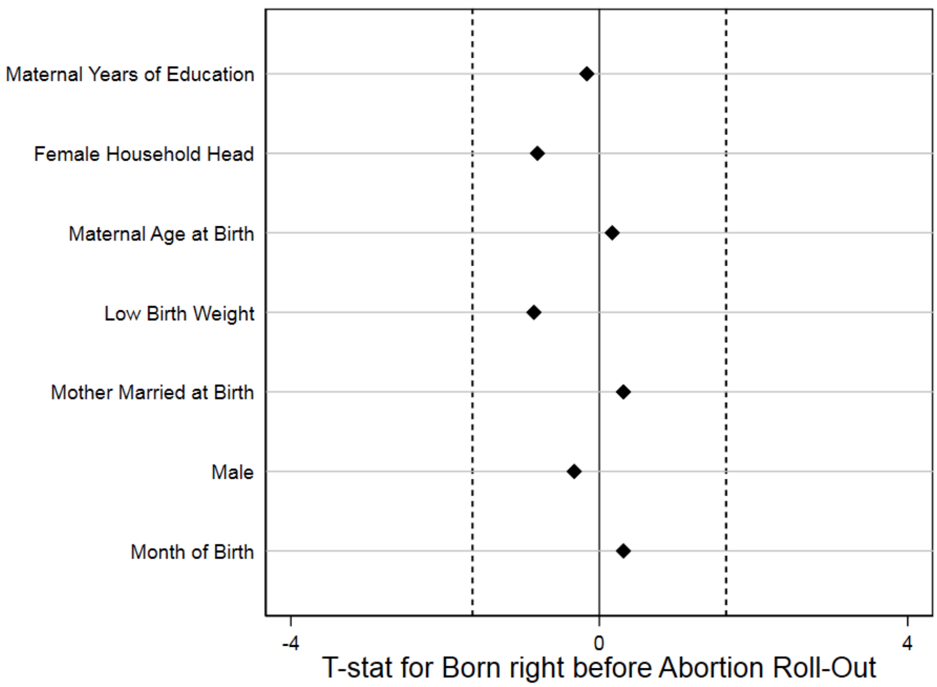


C. Neighborhood Quality Index



Notes: Each figure plots event-study estimates for long-run outcomes using the specification in equation (2). Standard errors clustered at the county level. 90-percent, point-wise confidence intervals for each estimate are presented. *Source:* Restricted Census/ACS and SSA Numident file.

Figure 6. Test of Potential Threats to Identification



Notes: Sample includes individuals from PSID Childbirth and Adoption History File whose mother's most recent observation is after she turns 40 years old. Each point plotted is the event-time estimate of the -1 to -3 bin. Heteroskedasticity-robust standard errors clustered at the state level. Vertical dashed lines are drawn at ± 1.645.
Source: Restricted PSID Individual and Childhood and Adoption History File.

Table 1. Year of Abortion Provider Roll-Out and 1970 County Characteristics

Independent Variable	(1) Total population, 1970	(2) % population growth 60-70	(3) % pop growth net migration 60-70	(4) % female, 1970	(5) % urban, 1970	(6) % nonwhite, 1970	(7) % of population aged 0-4 years, 1970	(8) % of population aged 65+ years, 1970
Coefficient	-3.16e-07***	0.00879	0.00917	-0.0618	-0.0357***	-0.0162	0.191	-0.00450
(s.e.)	(1.04e-07)	(0.00770)	(0.00810)	(0.0474)	(0.00899)	(0.0111)	(0.147)	(0.0335)
Observations	779	779	779	779	779	779	779	779
R-squared	0.256	0.245	0.244	0.241	0.254	0.244	0.244	0.240

Independent Variable	(9) Median age (years), 1970	(10) Birth rate per 1,000 population, 1968	(11) Death rate per 1,000 population, 1968	(12) Median years schooling completed (1970)	(13) % less than 5 yrs schooling (1970)	(14) % 12 or more yrs schooling (1970)	(15) Median family income, all families, 1969 ($)	(16) Leading party = Democratic
Coefficient	-0.0209	-0.00333	-0.0368	-0.117	0.0409	-0.00148	-6.80e-05	0.111
(s.e.)	(0.0230)	(0.0435)	(0.0305)	(0.114)	(0.0290)	(0.0101)	(8.01e-05)	(0.194)
Observations	779	779	779	779	779	779	779	779
R-squared	0.240	0.240	0.242	0.241	0.242	0.240	0.241	0.240

Notes: Dependent variable is year of provider roll-out in each county. Each column presents coefficient from a simple linear regression. *Source:* 1970 County and City Data Book.

Table 2. Number of Younger Siblings and Service Intensity since Roe v. Wade

	# Younger Siblings		Any Younger Siblings	
	(1)	(2)	(3)	(4)
Born 1964-66	-0.106	-0.136	-0.125**	-0.139**
	[0.231]	[0.228]	[0.054]	[0.053]
Born 1967-69	0.450	0.375	0.035	0.009
	[0.328]	[0.317]	[0.100]	[0.098]
Born 1970-72	0.795**	0.755*	0.073	0.050
	[0.387]	[0.376]	[0.130]	[0.129]
Born 1964-66 x # Prov/1k	0.541	0.712	0.689	0.745
	[2.521]	[2.463]	[0.742]	[0.730]
Born 1967-69 x # Prov/1k	-1.606	-1.538	-0.428	-0.439
	[1.904]	[2.059]	[0.636]	[0.655]
Born 1970-72 x # Prov/1k	-3.312**	-3.175**	-0.430	-0.368
	[1.590]	[1.523]	[0.870]	[0.873]
State FE x Trends	✓	✓	✓	✓
County Covariates x Trends		✓		✓
Observations	1,713	1,713	1,713	1,713
Mean Y	1.300	1.300	0.680	0.680
Mean # Service Prov	0.0700	0.0700	0.0700	0.0700

Notes: Sample includes individuals from PSID Childbirth and Adoption History File born between 1960 and 1972 whose mother's most recent observation is after she turns 40 years old. Covariates include gender, race, mother's age at birth, and categories of maternal education. Heteroskedasticity-robust standard errors clustered at the state level.

Table 3. Summary of Event-Study Estimates on Long-Run Outcomes

Dependent Variables *(N=15,891,000)*	(1) Control Mean	(2) Event Study at -1 (s.e.)	(3) F −3 to −1 joint	(4) F left of −8 joint	(5) ATET	(6) ATET %
A. Human capital index		0.0117 (0.00522)	2.835	1.191	0.0422	
Years of schooling	13.790	0.0404 (0.01810)	3.476	1.317	0.1458	1.1%
High school or GED completed	0.932	0.0027 (0.00151)	2.087	1.139	0.0097	1.0%
Completed 4 year college	0.332	0.0073 (0.00336)	2.642	0.935	0.0265	8.0%
Professional or doctoral degree	0.031	0.0025 (0.00081)	3.416	1.812	0.0090	29.1%
Has a professional job	0.370	0.0033 (0.00274)	0.809	0.883	0.0119	3.2%
B. Economic self-sufficiency index		0.0040 (0.00329)	1.598	1.401	0.0144	
Worked last year	0.857	-0.0001 (0.00174)	1.157	0.468	-0.0003	0.0%
Number of weeks worked last year	40.990	-0.0192 (0.09520)	1.925	0.794	-0.0693	-0.2%
Usual hours works per week	35.590	0.1200 (0.09290)	0.919	1.134	0.4332	1.2%
Positive labor income (wage only)	0.810	0.0008 (0.00193)	1.331	0.338	0.0027	0.3%
Log labor income (wage only)	10.610	0.0084 (0.00471)	1.089	1.035	0.0303	
In poverty	0.100	0.0006 (0.00200)	0.851	1.389	0.0021	2.1%
C. Neighborhood quality index		0.0132 (0.00562)	2.153	0.504	0.0477	
Mean home ownership in tract	0.730	0.0034 (0.00181)	1.401	1.334	0.0123	1.7%
Positive small family income in tract	0.941	0.0013 (0.00039)	3.594	1.511	0.0045	0.5%
Log family income to poverty ratio in tract	5.939	0.013 (0.00442)	2.679	0.664	0.0451	
Share single head of family in tract	0.434	-0.001 (0.00093)	0.361	1.153	-0.0026	-0.6%
Share children in poverty in tract	0.201	-0.004 (0.00146)	2.863	1.031	-0.0138	-6.9%

Notes: In Column 1, the control mean is calculated using the individuals born 8 years before abortion access. Column 2 presents the estimated intent-to-treat (ITT) effect evaluated at event-time −1 using the specification in equation (2) (see Figure 5). Column 3 and Column 4 present test statistics for joint-significance of event-time −3 to −1 and of event-time smaller than −8, respectively. The average treatment-effect-on-the-treated (ATET) estimate in Column 5 divides the ITT effect by the estimated reduction in completed family size, 0.277. Column 6 computes the percentage change implied by the ATET relative to the control mean (the ratio of Column 5 to Column 1). Percent change is inapplicable and therefore left blank for standardized indices and for logged outcomes. *Source:* Restricted Census/ACS and SSA Numident file.

Table 4. Event-Study Estimates on Human Capital and Self-Sufficiency, by Gender

Dependent Variables	(1) Control Mean	(2) Event Study at -1 (s.e.)	(3) F -3 to -1 joint	(4) F left of -8 joint	(5) ATET	(6) ATET %
A. Male (N=7,601,000)						
i. Human capital index		0.0189	3.033	0.692	0.0682	
		(0.00648)				
Years of schooling	13.730	0.0671	3.495	0.977	0.2422	1.8%
		(0.02270)				
High school or GED completed	0.923	0.0041	1.717	0.389	0.0147	1.6%
		(0.00202)				
Completed 4 year college	0.328	0.0122	3.104	0.676	0.0440	13.4%
		(0.00411)				
Professional or doctoral degree	0.036	0.0028	2.513	1.025	0.0100	27.7%
		(0.00119)				
Has a professional job	0.357	0.0086	2.426	0.345	0.0310	8.7%
		(0.00351)				
ii. Economic self-sufficiency index		0.0110	3.817	1.065	0.0397	
		(0.00385)				
Worked last year	0.920	0.0035	1.687	0.970	0.0127	1.4%
		(0.00169)				
Number of weeks worked last year	45.110	0.1870	2.845	1.111	0.6751	1.5%
		(0.09800)				
Usual hours works per week	41.460	0.3100	3.776	0.709	1.1191	2.7%
		(0.10300)				
Positive labor income (wage only)	0.865	0.0046	2.047	1.490	0.0165	1.9%
		(0.00200)				
Log labor income (wage only)	10.880	0.0124	2.167	2.384	0.0448	
		(0.00552)				
In poverty	0.074	-0.0036	1.994	1.424	-0.0130	-17.6%
		(0.00191)				
B. Female (N=8,290,000)						
i. Human capital index		0.004	1.175	0.989	0.0149	
		(0.00551)				
Years of schooling	13.850	0.014	1.960	1.059	0.0513	0.4%
		(0.01880)				
High school or GED completed	0.940	0.001	1.154	1.155	0.0052	0.6%
		(0.00165)				
Completed 4 year college	0.337	0.003	1.328	0.836	0.0094	2.8%
		(0.00372)				
Professional or doctoral degree	0.026	0.002	2.293	1.950	0.0085	32.3%
		(0.00097)				
Has a professional job	0.384	-0.002	0.168	0.728	-0.0083	-2.2%
		(0.00336)				
ii. Economic self-sufficiency index		-0.001	1.538	1.178	-0.0034	
		(0.00390)				
Worked last year	0.797	-0.003	1.589	1.016	-0.0117	-1.5%
		(0.00247)				
Number of weeks worked last year	37.160	-0.197	2.126	0.760	-0.7112	-1.9%
		(0.13200)				
Usual hours works per week	30.130	-0.048	0.358	1.432	-0.1736	-0.6%
		(0.11200)				
Positive labor income (wage only)	0.759	-0.003	1.599	0.886	-0.0098	-1.3%
		(0.00266)				
Log labor income (wage only)	10.320	0.001	0.139	0.735	0.0035	
		(0.00623)				
In poverty	0.123	0.004	1.845	1.283	0.0145	11.8%
		(0.00258)				

Notes: See Table 3 notes.

Table 5. Event-Study Estimates on Neighborhood Quality, by Gender

Dependent Variables	(1) Control Mean	(2) Event Study at -1 (s.e.)	(3) F -3 to -1 joint	(4) F left of -8 joint	(5) ATET	(6) ATET %
A. Male (N=7,601,000)						
Neighborhood quality index		0.0168	2.708	1.075	0.0606	
		(0.00599)				
Mean home ownership in tract	0.745	0.0026	1.115	2.339	0.0094	1.3%
		(0.00196)				
Positive small family income in tract	0.943	0.0015	4.276	1.155	0.0054	0.6%
		(0.00045)				
Log family income to poverty ratio in tract	5.941	0.0145	3.399	1.782	0.0523	
		(0.00471)				
Share single head of family in tract	0.434	-0.0012	1.178	2.197	-0.0043	-1.0%
		(0.00105)				
Share children in poverty in tract	0.200	-0.0044	2.830	0.928	-0.0160	-8.0%
		(0.00158)				
B. Female (N=8,290,000)						
Neighborhood quality index		0.010	1.509	0.664	0.0347	
		(0.00605)				
Mean home ownership in tract	0.749	0.004	2.798	0.865	0.0147	2.0%
		(0.00201)				
Positive small family income in tract	0.942	0.001	2.624	1.226	0.0038	0.4%
		(0.00039)				
Log family income to poverty ratio in tract	5.938	0.010	2.032	0.663	0.0372	
		(0.00467)				
Share single head of family in tract	0.433	0.000	1.830	0.650	-0.0006	-0.2%
		(0.00107)				
Share children in poverty in tract	0.202	-0.003	2.103	0.665	-0.0116	-5.7%
		(0.00152)				

Notes: See Table 3 notes.

Table 6. Effects Heterogeneity Interaction between Family Size and Public Programs

	Abortion Access Interacted with:			
	Head Start	Food Stamps	Medicaid Expenditure	Community Action Programs
Outcomes:				
Human Capital Index	0.0157	0.00551	0.00805	-0.00617
(s.e.)	(0.00612)	(0.00486)	(0.00857)	(0.00456)
Economic Self-Sufficiency Index	0.00467	0.00579	-0.00301	-0.00327
(s.e.)	(0.00401)	(0.00316)	(0.00548)	(0.00337)
Neighborhood Quality Index	0.0098	0.00137	0.00997	-0.00838
(s.e.)	(0.00394)	(0.00409)	(0.00822)	(0.00356)

Notes: The empirical model is similar to equation (2) but additionally includes each event-time dummy interacted with an indicator of high exposure to a public program, $\sum_y \psi_y D_c 1\{b - T_c = y\} Abortion_c$. Estimates of the interaction effect ψ_y are presented. Estimates are intent-to-treat (ITT) effects evaluated at event-time -1. *Source:* Restricted Census/ACS and SSA Numident file.

8 Appendix

8.1 Theories on the Interaction between Child Quantity and Quality

Theoretically, a large literature in demography and economics examines the effect of family size on children's human capital. The quantity-quality trade-off model (Becker, 1960; Becker and Lewis, 1973; Willis, 1973) of fertility decisions and investment in children provides a framework for the study of family size. It conceptualizes parents' economic behavior when faced with a trade-off between having more children and investing more parental time and financial resources in each child. However, to date, empirical evidence on the effects of family size remains inconclusive (Duflo, 1998; Black, Devereux and Salvanes, 2005; Cáceres-Delpiano, 2006). Moreover, it is unclear whether the result from a particular IV strategy can be generalized to a broader group of individuals (Imbens and Angrist, 1994; Angrist, Lavy and Schlosser, 2010). The concern is that public policy cannot alter twinning or the gender of children, and the families that can be effectively targeted by policies may experience completely different effects than where the identification comes from. This concern seems to be confirmed by the inconsistent estimates across instruments. For instance, Black, Devereux and Salvanes (2010) find no significant effect of family size on children's IQ score when using sex composition as an instrument, but discover negative and significant estimates using twins instead.

In the classic quantity-quality model, all children are the results of lifetime fertility decisions made in a static setting. There are several proposed ways of expanding the quantity-quality model. One crucial addition would be to incorporate sequentiality in fertility decisions (Heckman and Willis, 1976) and the possibility of failure to avoid unwanted births after deciding the ex-ante optimal number of children (Michael and Willis, 1976). Empirical evidence on this front has been sparse, with the exception of Lin et al. (2017) who examine the distinction between planned and unplanned increase in family size.

Families that have imperfect ability to control fertility and end up having unwanted children after they decide to stop might experience substantial impacts on the children they already have. Unwanted births indicate disruption in parental plans regarding child investment, and has many documented negative impacts. A growing literature in economics and demography shows the negative life-cycle consequences of unwanted children. Many empirical studies leverage unique data sources that directly document pregnancy intention information from women (Rosenzweig and Wolpin, 1993; Joyce, Kaestner and Korenman, 2000, 2002; Miller, 2009; Lin and Pantano, 2015; Lin et al., 2017).

8.2 PSID Data and Sample Construction

My primary source of survey data is the Panel Study of Income Dynamics (PSID), a longitudinal survey of a nationally representative sample of U.S. individuals and families. The PSID cross-year individual file allows this paper to construct completed years of education, current and retrospective self-reported health status, and labor market participation and income in adulthood. Critical to this project, the PSID also contains valuable information about Head Start participation in childhood, complete childbirths and adoption history records by individual's parents, and geo-coded county of birth and county grew up reported by individuals.[23] Crucially, the complete childbirths history records allow me to link an individual to her mother, and to the mother's total number of children after age 40 and thus construct the individual's eventual total number of siblings after her mother has completed fertility.

The PSID data is merged with the source of variation in family size – access to legal abortion overtime across states and counties in the U.S. On the state level, in 1970, five states became the earliest in modern U.S. history where abortion was broadly available. Abortion was then legalized nationwide in 1973 after the Roe v. Wade decision. On the county level, I use the abortion service provider information provided by the Guttmacher Institute. The measure of county-level intensity accounts for within-state changes in the provision of abortion service between 1970 and 1979.

8.3 State-Level Variation in Abortion and Family Size

Appendix Table A1 presents the magnitude of the estimates. Cohorts that are affected during the variation of abortion access are put into bins of being conceived between 1967 and 1969, and between 1970 and 1972.[24] Among all individuals and using my preferred specification (column 3

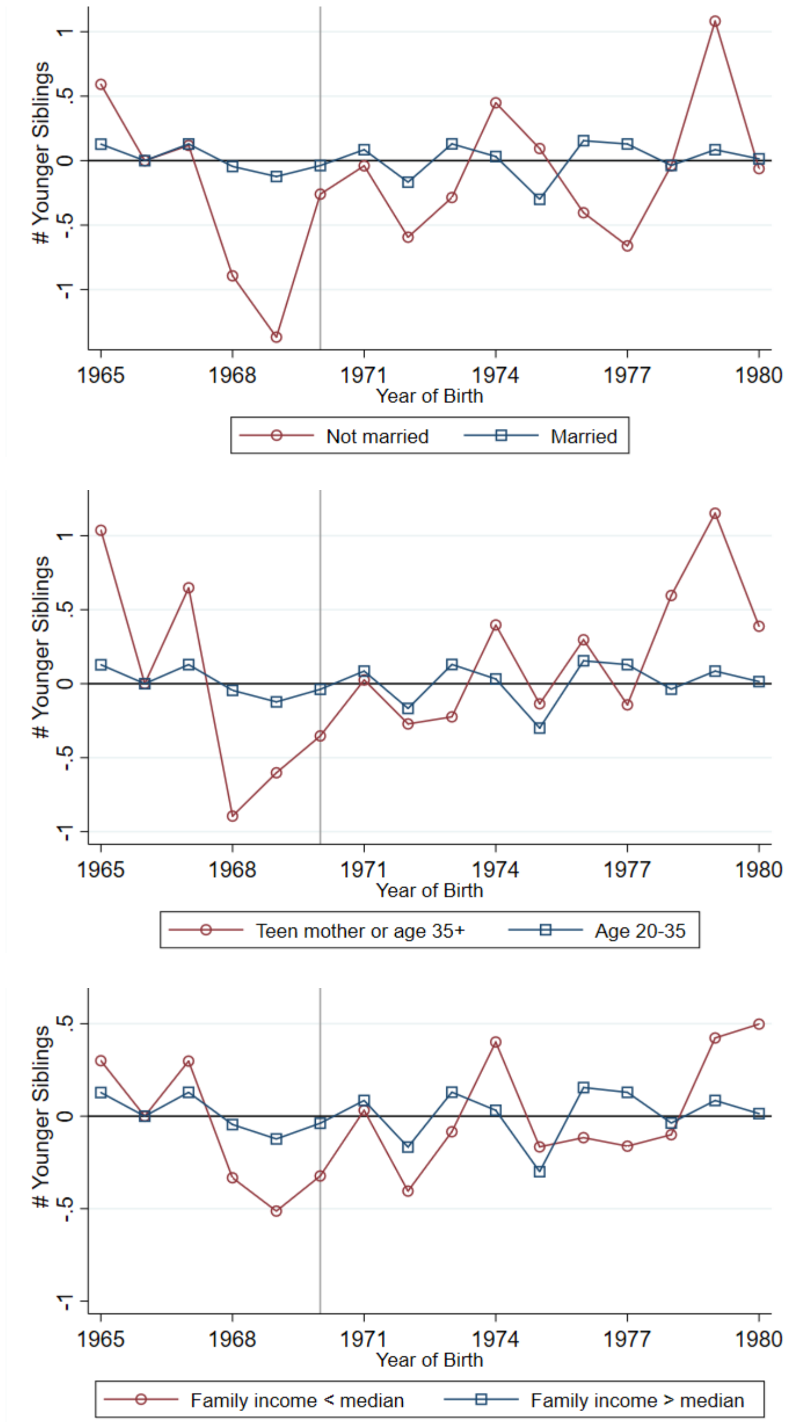
[23] County-level geo-code analysis requires approval to use PSID restricted data in the MICDA enclave.

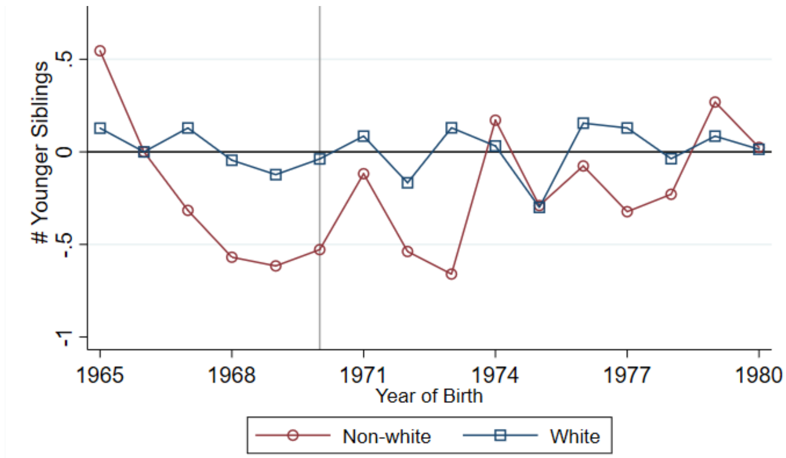
[24] Year of conception is constructed using year of birth and month of birth. It is set as one year before birth when the month of birth is between January and May. Although many children born after May are also conceived in the previous year, I assume for them access abortion is still possible if abortion becomes legal in their year of birth. The results are robust to different definitions of year conceived.

and column 6), having access to legal abortion one to three years after birth reduces number of younger siblings on average by 0.271. It also reduces chances of ever having any younger sibling by 9 percentage points. The estimate appears to be robust to the addition of individual-level covariates and state and year of birth fixed effects. When stratifying by birth order, results appear to be similar albeit less precise, indicating a statistically significant treatment effect not driven by a change of composition of the families.[25]

[25]Note that the estimates tend to be smaller for the later-borns. This is not surprising given that they have fewer younger siblings than the first-borns on average.

Figure A1. Difference in Number of Siblings, Early-Legalizing vs. Other States, by Family Background





Notes: Sample includes individuals in PSID cross-year individual file and Childbirth and Adoption History File whose mother last reported number of children after the age of 40. Point estimates of year-of-birth indicator interacted with repeal-state indicator are plotted. Covariates include gender, indicators of mother's race, mother's age at birth, and categorical variables of maternal completed education. Regression controls for year of birth and state of birth fixed effects. Confidence intervals presented are on the 90% level. Heteroskedasticity-robust standard errors clustered on the state level are used.

Table A1. Abortion Legalization and Younger Siblings - Regression

	DV: # Younger Siblings			DV: 1 = Any Younger Siblings		
	(1)	(2)	(3)	(4)	(5)	(6)
A. All						
Conceived 1967-69 x Repeal	-0.250***	-0.243***	-0.271***	-0.082***	-0.081***	-0.090***
	[0.085]	[0.084]	[0.077]	[0.021]	[0.022]	[0.020]
Conceived 1970-72 x Repeal	-0.057	-0.047	-0.066	0.038	0.035	0.023
	[0.085]	[0.087]	[0.077]	[0.029]	[0.030]	[0.028]
B. First Births						
Conceived 1967-69 x Repeal	-0.176	-0.192	-0.243**	-0.032	-0.027	-0.032
	[0.134]	[0.130]	[0.117]	[0.032]	[0.035]	[0.033]
Conceived 1970-72 x Repeal	-0.033	-0.048	-0.085	0.094***	0.093***	0.079**
	[0.085]	[0.089]	[0.083]	[0.031]	[0.034]	[0.033]
C. Non-First Births						
Conceived 1967-69 x Repeal	-0.169	-0.171*	-0.178*	-0.058	-0.054	-0.060
	[0.101]	[0.101]	[0.100]	[0.042]	[0.040]	[0.038]
Conceived 1970-72 x Repeal	-0.173	-0.107	-0.115	-0.058	-0.037	-0.042
	[0.118]	[0.125]	[0.109]	[0.054]	[0.055]	[0.050]
Additional covariates	MGSY	MGESY	MGE	MGSY	MGESY	MGE
Year of Birth FE	No	No	Yes	No	No	Yes
State of Birth FE	No	No	Yes	No	No	Yes
Observations	6,456	6,435	6,434	6,456	6,435	6,434
Mean DV	1.100	1.100	1.100	0.630	0.630	0.630

Notes: Sample includes individuals in PSID cross-year individual file and Childbirth and Adoption History File born between 1965 and 1983 whose mother last reported number of children after the age of 40. Regressions use heteroskedasticity-robust standard errors clustered on the state level. Column 2 includes control variables for mother's race and age at birth M, individual's gender G, and indicator of born in early-repeal states S, and indicators of different birth cohort bins Y.

Table A2. State-Level Abortion Ban Repeal and Human Capital

	Completed Yrs of Edu		High School or More		Some College or More		College Completion	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Conceived 1967-69 x Repeal	0.365**	0.321**	-0.051*	-0.054**	0.037	0.021	0.158***	0.155***
	[0.146]	[0.140]	[0.027]	[0.025]	[0.038]	[0.036]	[0.044]	[0.044]
Conceived 1970-72 x Repeal	0.598***	0.578***	0.021	0.020	0.178***	0.164***	0.070	0.074
	[0.117]	[0.116]	[0.018]	[0.018]	[0.035]	[0.036]	[0.046]	[0.048]
Black	-0.565***	-0.523***	-0.031*	-0.022	-0.084***	-0.084***	-0.138***	-0.130***
	[0.106]	[0.104]	[0.017]	[0.015]	[0.025]	[0.023]	[0.021]	[0.024]
Female	0.688***	0.685***	0.043***	0.043***	0.152***	0.152***	0.110***	0.109***
	[0.046]	[0.046]	[0.009]	[0.009]	[0.014]	[0.014]	[0.012]	[0.012]
Maternal Age at Birth	0.051***	0.052***	0.004***	0.004***	0.007***	0.007***	0.012***	0.012***
	[0.006]	[0.006]	[0.001]	[0.001]	[0.001]	[0.001]	[0.001]	[0.001]
Maternal Yrs of Edu	0.347***	0.336***	0.024***	0.024***	0.066***	0.063***	0.064***	0.062***
	[0.017]	[0.017]	[0.003]	[0.003]	[0.003]	[0.003]	[0.004]	[0.004]
Year of Birth FE	No	Yes	No	Yes	No	Yes	No	Yes
State of Birth FE	No	Yes	No	Yes	No	Yes	No	Yes
Observations	4,358	4,357	4,358	4,357	4,358	4,357	4,358	4,357
Mean Y	13.70	13.70	0.910	0.910	0.590	0.590	0.300	0.300

Notes: Sample includes individuals in PSID cross-year individual file and Childbirth and Adoption History File born between 1965 and 1983 whose mother last reported number of children after the age of 40. Regressions use heteroskedasticity-robust standard errors clustered on the state level.

Table A3. Abortion Provider Roll-Out and 1970 County Characteristics

	(1)	(2)	(3)	(4)	(5)	(6)
	First service provider in					
	1970	1973	1974-75	1976-88	Ever	Never
Number of Counties	118	373	191	215	897	2138
Total population, 1970	275,765	238,770	69,748	54,524	163,485	20,458
% population growth 1960-1970	19.86	16.14	12.7	12.67	15.07	1.26
% population growth from net migration 1960-1970	9.14	3.34	2.19	1.01	3.3	-7.53
% female, 1970	50.4	51.13	50.83	50.67	50.86	50.73
% urban, 1970	54.78	61.72	46.6	42.82	53.06	26.38
% nonwhite, 1970	4.25	12.01	7.94	10.51	9.76	10.09
% of population aged 0-4 years, 1970	8.15	8.42	8.27	8.44	8.36	8.11
% of population aged 65+ years, 1970	10.5	9.4	11.08	10.49	10.16	12.77
Median age (years), 1970	29.06	27.91	29.15	28.42	28.45	30.56
Birth rate per 1,000 population, 1968	16.53	17.68	16.93	17.21	17.26	16.29
Death rate per 1,000 population, 1968	9.8	9.41	10.19	9.93	9.75	11.35
Median years schooling completed, persons 25+ (1970)	12.15	11.57	11.39	11.19	11.52	10.64
% persons 25+ with less than 5 yrs schooling (1970)	3.82	6.02	6.06	7.39	6.07	8.04
% persons 25+ w/ 12 or more yrs schooling (1970)	57.18	50.79	48.93	47	50.33	42.2
% female civilian labor force participation	27.69	29.84	27.01	27.31	28.35	24.5
% of families with female head, 1970	8.97	10.36	9.08	9.25	9.64	8.67
Median family income, all families, 1969 ($)	9649.88	8992.86	8147.29	7965.29	8652.95	6945.88
Leading party = Democratic	0.25	0.24	0.22	0.23	0.23	0.21

Table A4. State-Level Analysis of Family Size and Head Start Interaction

	(1) Completed Yrs of Edu	(2) High School or More	(3) Some College or More	(4) College Completion
Head Start x Conc 1967-69 x Repeal	1.02166*	-0.02786	0.23232*	-0.01049
	0.612	0.19	0.138	0.127
	0.096	0.884	0.094	0.934
Head Start x Conc 1970-72 x Repeal	-0.19414	0.86688***	0.13422	-0.64916***
	0.883	0.211	0.208	0.138
	0.826	0	0.519	0
Head Start	0.43378*	0.0453	0.08727	0.05282
	0.259	0.061	0.081	0.036
	0.095	0.461	0.285	0.145
Conc 1967-69 x Repeal	-0.07806	0.07392	-0.13891	0.16477**
	0.528	0.167	0.112	0.066
	0.883	0.658	0.215	0.014
Conc 1970-72 x Repeal	-0.12578	-0.54666***	-0.27619	0.37573***
	0.645	0.167	0.193	0.114
	0.846	0.001	0.155	0.001
Observations	20,108	20,108	20,108	20,108
R-squared	0.335	0.287	0.32	0.299
Y-mean	12.39	0.81	0.312	0.0896

Notes: Sample includes individuals in PSID cross-year individual file and Childbirth and Adoption History File born between 1960 and 1980 whose mother last reported number of children after the age of 40. Covariates include gender, indicators of mother's race, mother's age at birth, and categorical variables of maternal completed education. Regressions also include for indicator of Head Start interacted separately with repeal, conceived in 1967-69, conceived in 1970-72, as well as year of birth and state of birth fixed effects, resulting in a fully interacted specification. Confidence intervals presented are on the 90% level. Heteroskedasticity-robust standard errors clustered on the state level are used.