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The Marginal Child throughout the Life Cycle:
Evidence from Early Law Variation

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Abstract

This paper tests whether the fetal origins hypothesis, which posits that disparities in the pre- and peri-natal environment can account for long-term disparities in life expectancy, is applicable to the case of variation in early circumstances due to wantedness. To identify the effects of wantedness on fertility rates and on life expectancy, we exploit over-time variation in the legal restrictions on abortion within U.S. states from 1850 to 1910; we demonstrate that the adoption of these legal restrictions cannot be predicted by other changes in state circumstances. We find that about 9 percent more children were born in times and places when abortion was restricted than in states and times in which it was freely available. Members of these larger cohorts were roughly 5 percent less likely than those in other cohorts to survive into their sixties, and somewhat less likely to be healthy throughout their lives. We identify childhood health and education as possible mechanisms through which wantedness may affect longevity. We conclude that wantedness, like other aspects of a child's early life circumstances, has important effects on life expectancy.

I. Introduction

The fetal origins hypothesis, originally developed within the field of epidemiology by Barker (1992) and others, posits that disparities in the pre- and peri-natal environment can account for not only short-term health disparities such as gaps in infant mortality rates but also long-term disparities, in particular life expectancy. In a separate literature within economics, researchers have recently estimated effects of "wantedness" on childhood living circumstances and early adult outcomes, and have imputed from these estimates that the "marginal child" avoided when legal abortion is available would have been born into disadvantaged circumstances (Gruber et al. 1999). Until now, however, the fetal origins and marginal child literatures have not been in dialogue, because the variations in wantedness exploited in previous papers occurred in the 1970s—too recently to allow researchers to test their effects on adult life expectancy and other long-term outcomes.

However, there have been previous changes to the legal status of abortion that amounted to natural experiments in which cohorts born in some states and years included fewer unwanted births than those born in other states and years. These changes have not previously been documented or exploited. In this paper, we take advantage of these legal changes by using 19th-century state legal codes to compile a dataset on the introduction and amendment of laws restricting activities related to abortion that occurred in states during the 19th century. In this paper, we document that the timing of the enactment of these laws, as well as the severity and comprehensiveness of the legal restrictions, varied significantly across states, and that these laws induced variation both in birthrates and in longevity.

Using Census data compiled by Carter et al. (2006) on the number of children aged 0-9 in each state and decade,[1] we demonstrate that introducing a law restricting abortion resulted in an increase in birthrates of approximately 9 percent, comparable to the effect sizes of laws restricting abortion in the 1970s. We find that more restrictive laws have slightly larger effects, while laws with significant exemptions for health professionals have weaker effects. We find that socially conservative laws that do not regulate fertility (i.e., restrictions on obscene songs) do not affect the birth rate. We demonstrate that the adoption of such laws cannot be predicted by potentially endogenous state characteristics such as: percent immigrant, lagged birthrates, the state ratio of female to male population, or the share of the population that died in the Civil War. We argue that these results imply that 19th-century state laws against abortion and birth control caused increases in the birthrate, and that these marginal births can be considered "unwanted" in the spirit of the 1970s legal reform and birth rate literature.

Using this variation in 19th-century legal environment, we investigate the effects of wantedness on adult life expectancy using Census data from 1900 to 2000. We find that individuals in the 1850-1910 cohorts born in states and years with laws outlawing abortion were 5 percent less likely to survive into their 60s or 70s. It is notable that this is the age range in life expectancy considered to be most affected by the fetal environment (Barker 1992). Laws restricting obscene songs, which did not affect birth rates, also do not affect longevity. These results imply that the marginal (or the typical "unwanted") child who was born due to fertility control restrictions was 35 to 45 percent less likely to survive to old age than was the average child born in that era.

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[1]Unfortunately the Census provides reliable 19th-century data on these measures only for whites (Carter et al. 2006); hence our results cannot be generalized to other races.

The rest of the paper proceeds as follows. Section II provides background and explains the historical variation used to identify the effects of wantedness. Section III describes the data. Section IV details the empirical methodology. Section V discusses the results. Section VI concludes.

II. Background

The "fetal origins of health" hypothesis

Barker (1992) developed the hypothesis that poor nutrition of a mother during pregnancy could lead to adaptations by her fetus aimed towards surviving in an impoverished environment. Barker argued that these adaptations, in particular reduced fetal growth, could lead to chronic conditions including cardiovascular disease and diabetes. These illnesses, which typically do not manifest until late in life, can reduce life expectancy. Hence, Barker argued, much of adult health status may be determined early in life. This hypothesis has recently been explored, and confirmed, in economics research by testing the effects of early life circumstances such as being born during the 1918 influenza epidemic (Almond 2006) or during the summer (Costa and Lahey 2005) on longevity.

The "marginal child" literature

In the 1970s, abortion was legalized in the United States, first in five states and then nationwide. Levine et al. (1999) identified that, in the wake of this legalization, roughly six percent fewer children were born. Following that finding, Gruber et al. (1999)

investigated the question: how did the average characteristics of the children who were born change after legalization? Using the change in birthrate and the change in the average characteristics of these smaller cohorts, Gruber et al. then backed out the characteristics of the "missing" or "marginal" children who were *not* born because of the legalization of abortion. They determined that the "marginal child" would have been disadvantaged—more likely than average to have lived in a poor, single parent, or welfare-receiving household, more likely to have been low-birthweight, and more likely to have died in infancy. Subsequent research has explored the outcomes of the children born in these cohorts as young adults, and has determined that the marginal child would have been more likely than average, as a young adult, to use drugs (Charles and Stephens 2006), to be a single parent, not to graduate from college, and to receive welfare (Ananat et al. 2009).

In sum, these projects have found that "wanted" children tended to grow up in better-than-average circumstances and experienced lower-than-average deprivation in early years. Moreover, they have concluded that increased average levels of wantedness after Roe vs. Wade have had positive effects on cohorts in early adulthood. However, it will not be possible for another thirty or more years to determine the effect of the increased wantedness induced by abortion legalization on longevity. In this paper, we turn to earlier variation in U.S. laws regarding fertility control in order to identify the effects of wantedness on longevity.

Historical variation

Perhaps surprising to a modern audience, but well-known among historians, the nineteenth century U.S. market for technologies to limit fertility was an active one. Abortion technologies available in the 19th-century United States included herbal abortifacients, such as black cohosh, that were effective in early pregnancy; and surgical abortion, which was common throughout the 19th century and increased in frequency after the modern dilation and cutterage, or "D and C," method was popularized in mid-century. Devices, herbs, and medical procedures were prominently advertised in the many available 19th-century newspapers, while pamphlets (for the literate) and popular lecture circuits (for the illiterate and others) explained options. Perhaps in part because of this burgeoning industry (Lahey 2009), the American birthrate fell from one of the world's highest in 1800 to the world's lowest by 1900.

In the second half of the 19th century, a moral crusade against "vice" led to government limitations on the fertility control market. These laws, which were adopted, strengthened, weakened, and repealed at different times in different states, varied greatly in the share of activities they restricted, in their exemptions, and in their punishments. In the 1860s, states began to pass anti-abortion laws that outlawed advertisements for the procedure and that, for the first time, prohibited abortions even before "quickening" (abortions prior to observable movement of the fetus had traditionally been allowed under English common law). Many of these laws also, for the first time, provided for punishment not only of abortionists but also of the women seeking abortions. Although the courts were often sympathetic to women and abortionists when violations of these new laws were brought to trial, the publicity could permanently tarnish reputations and in many cases the official investigations and court trials amounted to harassment; in several

high-profile cases, the accused committed suicide before the court reached a verdict (Reagan 1991).

Figure 1 illustrates the frequency of state changes to the legal status of abortion over the period we study. In what follows, we exploit this variation in the presence of abortion laws by state and year as a measure of the access women had to fertility control. We argue that the environments produced by these laws induced variation in the "wantedness" of children born in different states and years.

III. Data

Laws

We have used archived state legal codes to compile a comprehensive dataset on the introduction and amendment of laws restricting activities related to abortion. For each of the 50 states, we collected laws from the earliest possible date through the 1920s. A number of secondary sources exist describing abortion laws: contemporary activists from both sides of the abortion debate provided snapshots of the laws as they existed at the time; additionally, historians have compiled lists of these laws for various time periods, and legal scholars have discussed specific laws in depth. To identify all state laws regulating abortion, we compiled and compared these secondary sources. In cases where there was a disagreement between sources, we obtained copies of the original laws from the Harvard Law Library's microfiche of superseded state statutes. We recorded each law's severity: whether the offense was classified as a misdemeanor or a felony (or left unclassified), as well as the punishment, if specified. We noted whether a woman as well as her abortionist could be held liable under the law. We also noted exemptions,

coding for each law whether it contained a clause indicating that it did not apply to practitioners of medicine (which could refer to physicians, nurses, druggists, midwives, etc.) or to activities for saving the life of the mother.

Birthrate

We observe fertility behavior at the level of state and decade. Ideally, to test the effect of the introduction of laws on childbearing behavior, we would like to have individual birth cohort data by year, i.e., the number of children born in each state in 1860, in 1861, etc. We would then predict those observations of cohort size using an indicator for whether there was a law in place in that state in the year before that cohort was born, when abortion or birth control policy would have been relevant for that cohort. Unfortunately, Census information on single years of birth is not available—the Census tables only provide population data by five-year age groups (0-4, 5-9, etc). Moreover, historical Census tables do not provide information on childbearing linked to mothers.

Instead, the standard measure of 19th century fertility is the child:woman ratio, calculated as the ratio of the number of children aged 0-9 to the number of women of childbearing age, or 15-44.[2] The measure captures fertility rate and spacing between children; it is also highly correlated with total fertility (Haines and Hacker 2006). Child:woman ratios were calculated by state-decade for 1860-1910 from tabulated census data from Haines Census tables in the *Historical Statistics of the United States* (Carter et al. 2006), which use Census data cleaned by Haines (many earlier studies used a version of the data presented by Kuznets). Unfortunately, because historical Census tables provide counts only for the white population, our investigation of birthrates is limited to

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[2] We use the Yasuba (1966) interpolation for 40-44 year olds from data for 40-49 year olds.

whites. More thorough discussions of the benefits and limitations of these measures can be found in Easterlin (1976a), Haines and Hacker (2006) and Yasuba (1966). We also limit our sample to states that became states before 1890, because they have census table information on them during the most of the period studied (collected as territory information before they became states).

Longevity

We compiled data from the 1900 through 2000 decennial Censuses on the number of individuals born in a given state and year who survived into their 40s, 50s, 60s, 70s, 80s, and 90s. These data come from the University of Minnesota IPUMS (Ruggles et al. 2003). Within each decennial Census, we identified the number of people born in a given state and year who survived to the time of the Censuses taken during their 50s through their 80s, as a share of the number who were observed in mid-adulthood, in their 40s. While we would like information on individual year of death, no dataset exists for the entirety of the 20th century that provides state of birth, as does the decennial Census. Hence, we can only observe the number surviving within each state-year cohort once per decade. Therefore our measure of longevity is the share of the cohort observed at the time of the Census taken during its sixth decade (when members are between 50 and 59), its seventh decade, and so on.

Health

We compiled data from decennial Censuses on the health of individuals born in a given state and year. Direct measures of health in the Census are few: only the 1910 Census provides information on blindness and deafness. However, we are also able to

examine outcomes known to be related to health. These include: veteran status (meeting physical requirements for military service is a marker for having had good health in early adulthood) in the 1940-1970 Censuses; having attended school (many studies have found that receiving education leads to improved health), available in all 20th-century Censuses; having married and having had children, both of which are associated with increased longevity, also available in all 20th-century Censuses.

IV. Methodology

To examine the impact of restrictions on the number of children born, we exploit the quasi-experiment provided by the variation across states in the timing of passage of restrictive laws. We limit our analysis to 1860 and later because many states did not exist before 1860 and did not have state law books. We drop states that did not exist as states before 1890 in order to keep a standard universe of states throughout the time period studied.

Because we can observe only the entire number of children born over the ten years prior to each Census (i.e., those aged 0 to 9), we cannot identify the relationship between a law passed in a given year and the number of children born the next year. To capture the fact that a law that passed between Censuses affected only those pregnancies that began afterward, we measured for what portion of a decade there was a valid law in place. This measure captures the share of the decade for which the law was relevant to childbearing. This variable is lagged one year, because on average abortions in year 0 cause a change in births in year 1.

For example, for a law to be relevant to the cohorts of children aged 0 to 9 in 1880, the law must have been passed in the period 1870-1879. A law passed in 1876 was relevant to those children born in 1877, 1878, and 1879—that is, it was relevant for roughly 30% of the children who were aged 0 to 9 in 1880. We therefore coded such a law with an indicator equal to 0.3 for the decade ending in 1880. A law passed in 1870 or earlier was coded with an indicator value of 1.0 for the decade ending in 1880. If a state did not have a law for any of the period 1870-1879, the indicator has a value of 0 for the decade ending in 1880.[3]

For our first-stage analysis of the effects of legal restrictions on abortion on the birthrate, we estimate models of the form:

(1) $\ln(F_{ds}) = \beta_1 \, havelaw_{ds} + \delta_d + \delta_s + d*\delta_s + d^2*\delta_s + e_{ds}$

where F_{ds} represents ten-year fertility in decade d in state s, and $havelaw_{ds}$ is a continuous indicator variable ranging in value from 0 to 1 that reflects the share of the decade for which a state has a law restricting fertility control. We include state-specific (δ_s) and decade-specific (δ_d) fixed effects to capture longstanding differences in fertility patterns across states over time as well as aggregate patterns of changing fertility preferences over time. We also allow the state-specific differences to trend over time by including an interaction between δ_s and decade d, and an interaction between δ_s and d^2. The coefficient β_1 measures the difference in ten-year fertility between states for which a law was in effect for the entire decade ($havelaw_{ds} = 1$) and states for which a law was never in effect in that decade ($havelaw_{ds} = 0$).

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[3] Results are robust to the use of a binary indicator for having a law the majority of the decade, although the estimates are not as precise.

For our reduced-form analysis of the effects of wantedness, as proxied by abortion laws, on longevity, we estimate models of the form:

(2) $\ln(S_{y,s}) = \beta_1\ havelaw_{y-1,s} + \delta_y + \delta_s + y*\delta_s + y^2*\delta_s + a_y + e_{y,s}$

in which $S_{y,s}$ represents the share of those born in year y in state s and observed in their 40s who are still alive for the Census taken in their sixth, seventh, eighth, or ninth decade of life. Note that in this case $havelaw_{y-1,s}$ is a zero-one indicator for whether a law existed in state s in the prior year, y-1. Because in the modern Censuses we can observe population counts for single years of birth, we can exploit exact timing of laws and cohort size, unlike in the first-stage estimates. This exact timing will tend to make our reduced-form analysis more precise than our first-stage analysis.

However, we observe each cohort only once every 10 years, meaning that cohorts born in 1874 are observed at age 55 in 1920 and age 65 in 1930, while cohorts born in 1876 are observed at age 53 in 1930 and age 63 in 1940. Therefore we include indicators for single years of age in the regressions as well, as typically there will be fewer people born at the beginning of each decade observed in their 50s, 60s, etc., than there are people born near the end of the decade. While it would be desirable to control separately for age effects, cohort effects, and census-year effects, the well-known linear dependence between these three unfortunately requires us to omit census-year effects.

For our reduced-form analysis of potential mechanisms by which wantedness may impact longevity, we estimate the equation:

(3) $P(outcome_{ys}) = \beta_1\ havelaw_{ys} + \delta_y + \delta_s + y*\delta_s + y^2*\delta_s + a_y + e_{ys}$

where $outcome_{ys}$ represents either a direct measure of a cohort's average health, including share blind or deaf, a proxy for health in early adulthood (veteran status), or a potential

socio-economic pathway (share with no schooling, unmarried, with no children) from wantedness to longevity.

V. Results

First stage

The results of OLS estimates of equation (1) are shown in Table 1. The left-hand panel of Table 1 reports estimates of the effects of having an abortion law in place on the level of children 0-9 in a state normalized by the number of women 15-44. The estimates suggest that a law restricting abortion access led to an increase in the number of children per 1000 women of childbearing age of roughly 100. The right-hand panel of Table 1 reports estimates in logs, which are highly consistent with the estimates in levels: having an abortion law in place for the decade prior to the Census leads to a 10 percent increase in the child:woman ratio. This estimate is similar to results from measuring the effect of 1970s changes in abortion legality on the contemporaneous birthrate, which range from 5 percent for the population overall (Levine et al. 1999) to 12 percent for teens (Angrist and Evans 1999).

Columns 3, 4, 7, and 8 of Table 1 test whether birth rates respond immediately to the implementation of an abortion law, or whether abortion laws are part of an ongoing (nonlinear, so not absorbed by our linear state trend controls) social trend by examining the relative strength of an indicator for having an abortion law two years prior to the birth. Introducing this measurement error in law timing leads to an attenuation of each coefficient in Table 1. This attenuation suggests that the effects observed in Columns 1,

2, 5, and 6 are "true" effects of law changes, not reflections of contemporaneous social changes.

Table 2 tests whether birth rates appear to "respond" to law changes that have not yet occurred. Since it is impossible for future laws to cause current birth rates, such a response would, again, suggest that both the adoption of abortion laws and changes in the birth rate. Again, however, regression current abortion rates on legal status 10 or 20 years in the future leads to the attenuation of the coefficients, suggesting that changing the dates of implementation introduces measurement error.

Table 3 presents a different type of falsification check for the first stage relationship between laws restricting abortion and the birthrate. While the timing evidence in Tables 1 and 2 suggests a tight relationship between laws and fertility, it is possible that laws restricting abortion are passed precisely at times when society becomes more conservative, for example, and that the birthrate rises during such times as well; if this is the case, then the implementation of laws may sharply predict higher birthrates without actually causing them. To test for this possibility, we estimate the relationship between birthrates and another set of laws that may reflect conservative attitudes, laws which outlaw the singing of obscene songs. These laws should not have any direct effect on fertility; a significant relationship between laws against obscene songs and birthrates would cast doubt on causal interpretations of our main first-stage estimates. The estimates in Table 3 of the relationships between laws against obscene songs and both the level and log of fertility are precisely estimated zeros.

Table 4 presents a set of checks for the possibility of legislative endogeneity, i.e. the possibility that laws were adopted because states desired to increase fertility. We

hypothesize several observable reasons that a society might want to increase fertility: a high rate of immigration in the previous decade that has reduced the percent native in the state; a low birthrate in the previous decade, a high (or low) female:male population ratio, or a large population loss in the Civil War. We use a linear probability model to estimate the relationship of each of these state characteristics on the probability that a state adopted a law restricting access to fertility control. We estimate equations of the form:

$$(4) \qquad L_{d+1,s} = \beta_1 \, demographics_{ds} + \delta_d + \delta_s + d*\delta_s + d^2*\delta_s + e_{ds}$$

Where $L_{d+1,s}$ is an indicator for whether a law was passed in state s in decade d+1. $Demographics_{ds}$ is a measure of either the immigration rate, the birthrate, or the female:male ratio in state s in the previous decade d, or is the share of the population that died in the Civil War interacted with an indicator that decade d is post-Civil War.

None of the characteristics has predictive power for adoption of a law, nor do all of them when included together in column (5). These checks provide further confidence in the interpretability of our first stage as causing variation in the birthrate that is independent of other demographic changes in the state.

Reduced form estimates of effects on longevity

The top row of Table 5 presents estimates of the impact of a law being in effect the year before the birth on the share of a cohort that survives to older ages, as described in equation (2). Column 1 of Table 5 replicates the results from Table 1 using cohort size, rather than the fertility ratio, as the outcome. The result is highly similar: cohorts are roughly 10 percent larger when an abortion law is in place, and the estimate is highly statistically significant. Column 2 of Table 5, however, shows that much of this larger

cohort size has disappeared by midlife. In their 40s, cohorts born under abortion restrictions are not significantly larger than other cohorts, and the point estimate for the increase in cohort size is only 3 percent. While this estimate may be smaller because of differences between the 20th-century IPUMS and 19th-century Haines data, it is also possible that cohorts born under abortion restrictions have higher early-life mortality.

Being in a cohort born under restricted abortion access has a negative but insignificant 2.1 percent effect on the share of the cohort that survives to its 50s, as shown in column (3). It reduces the share of the cohort that survives to both its 60s and its 70s by a significant 4.9 percent (columns 4 and 5). The effects on survival to the 80s and 90s are less precise, but marginally significant point estimates suggest that the effects may be even larger, at 6.6 percent and 21.6 percent, respectively.

The bottom rows of Table 5 show falsification checks from introducing measurement error into the timing of law implementation. An indicator for having a law in place 2 years prior to conception has a consistently smaller estimated relationship with longevity than does the "true" law indicator. This finding is consistent with the attenuation seen from introducing measurement error in estimates of fertility, and provides further evidence that the laws impact fertility and longevity through the same mechanism, by increasing the number of unwanted births.

Reduced form estimates of effects on life outcomes

Table 6 examines through what mechanisms lower rates of wantedness among affected cohorts may lead to reduced longevity. In order to reduce the effects of selection

into survival on our estimates of cohort characteristics, regressions are limited to the population under age 60, which means that we rely on the 1900 to 1970 Censuses.

Columns 1 and 2 of Table 6 measure the effect of abortion restrictions on the probabilities of blindness and deafness, respectively. These are the only two indicators of health for this era provided in the Census, to our knowledge. We find a significant .03 percentage point increase in the rate of blindness (from a base rate of .16 percent). This estimate, though reflecting an outcome that impacts relatively few individuals, is suggestive that affected cohorts have higher rates of a serious health problem. However, we find a precisely-estimated zero effect of abortion restrictions on the share of a cohort that is deaf. Similarly, Column 3, which measures the effect of abortion restrictions on the share of a cohort that are veterans (regression is restricted to males), shows no effect of abortion restrictions.

Columns 4, 5, and 6 of Table 6 measure important social outcomes that are believed to impact well-being (financial, psychological, etc.) overall as well as health in particular. Column 4 reports that abortion restrictions at conception predict a significant 0.4 percentage point increase the share of a cohort that has no schooling (from a base of 2.6 percent). Column 5 reports that abortion restrictions predict a significant 1.2 percentage point increase in the share of a cohort that has never married (from a base of 7.4 percent). Column 6 reports a precisely estimated zero relationship between abortion restrictions and the share of a cohort that is childless.

The bottom panel of Table 6 again reports estimates from misspecifying the timing of abortion laws. The relationship between having a law in place two years prior to conception and all outcomes are attenuated, with the exception of the relationship with

marriage. However, since individuals often marry others from nearby cohorts, it is perhaps not surprising that the effect of law implementation on a cohort is less specific to immediately impacted cohorts for this outcome. All in all, these estimates provide further evidence that the laws impact health and social characteristics through the same mechanism that they impact fertility and longevity, by increasing the number of unwanted births.

VI. Conclusion

Wantedness affects life expectancy in adulthood, particularly the probability that an individual lives to his or her 60s or 70s. This result is consistent with the Barker hypothesis on the fetal origins of health. Evidence that blindness is more common among those born under abortion restrictions suggests that fetal health may be relatively compromised among the less wanted.

Our findings are also consistent with the hypothesis that early child living circumstances associated with wantedness, as identified in the abortion legalization literature, persist into adulthood and affect outcomes of public interest throughout the life cycle. In particular, evidence that cohorts born under abortion restrictions were less likely to attend school are consistent with findings in the 1970s abortion literature on lower college attendance among relatively unwanted cohorts, although the 1970s results naturally reflect a different educational margin.

Our estimate of a 10 percent increase in birthrates is also highly consistent with research on recent (1970s-era) changes in legal access to abortion and birth control. That

research, which exploits identifying variation from the liberalization of abortion policy and access to oral contraceptives, finds overall changes in the birthrate of about 6 percent (Levine et al. 1999), and higher effects on some at-risk groups in the 1970s (Ananat and Hungerman 2007, Angrist and Evans 1999). The consistency of the birthrate response to restrictions on fertility control is remarkable particularly because of the lower efficacy and higher risks associated with 19th-century methods of abortion. Our results suggest that demand for increased fertility control has been persistent since the 19th century, rather than being a recent social development driven merely by shifting gender roles or increased labor market opportunities.

If we assume that the entire 4.9 percent (or 2.9 percentage point) change in the probability of living to one's 60s that we observe is due to lower life expectancy among the marginal births induced by restrictions on fertility control, then we can impute that the "marginal child" born due to restrictive laws was one-third less likely to live to past age 60 than the average child. This estimate is similar in magnitude to estimated effects of recent variation in fertility control on offspring's early life outcomes such as receiving welfare or attending college. Our results suggest that the relationship between wantedness and long-term outcomes are comparable across births in the nineteenth and twentieth centuries. This consistency also suggests, although it cannot demonstrate, that the life-cycle effects of the increased wantedness produced by 1970s improvements in access to fertility control may have salutary effects on life expectancy of current cohorts.

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Figure 1.

**Number of State Abortion Laws Passed
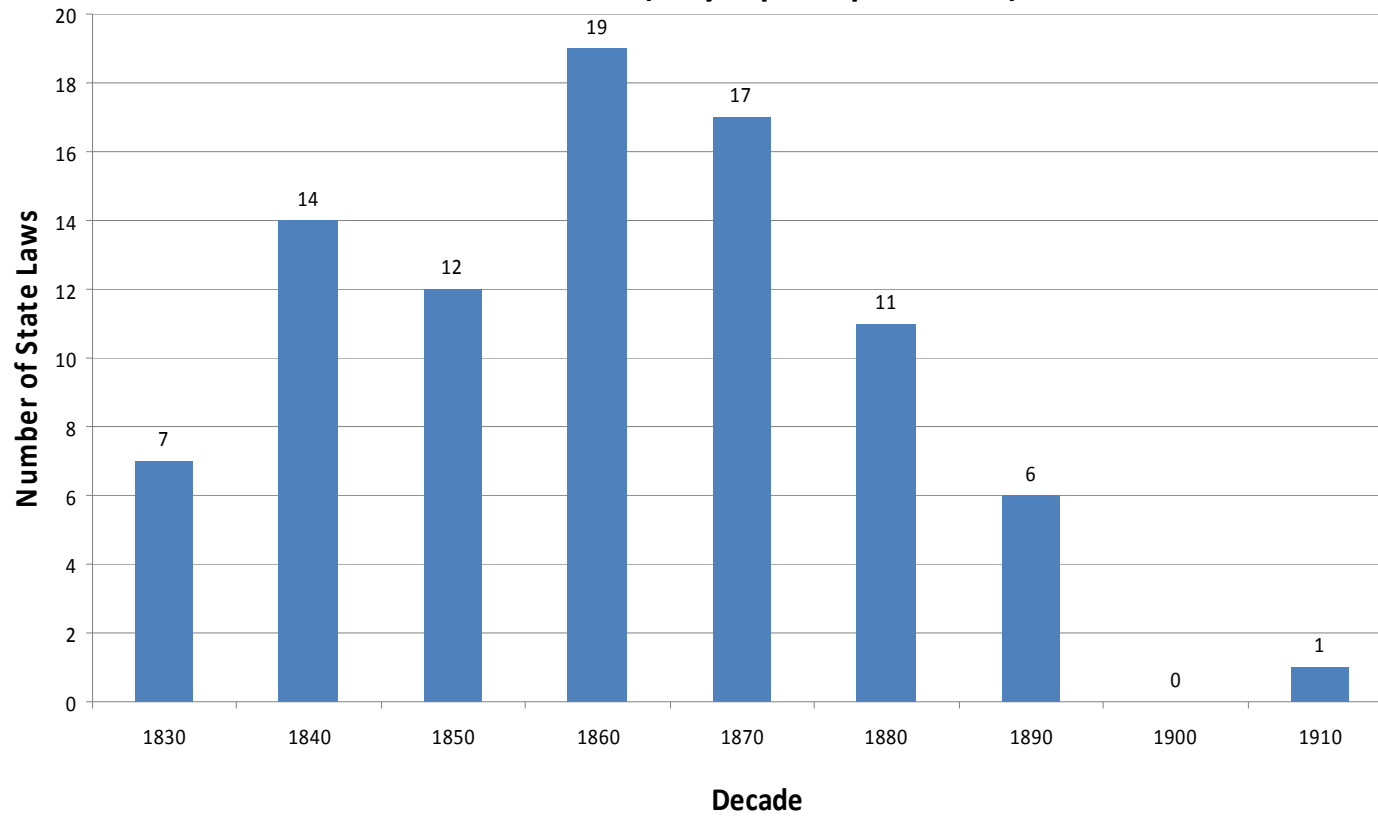(may replace prior laws)**

Table 1. Effects of abortion restrictions on 10-year fertility

	Child-woman ratio (levels)				Child-woman ratio (logs)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Have a Law	95.3741**	116.1485**			0.0993**	0.1005**		
	(30.9612)	(31.9971)			(0.0265)	(0.0280)		
Had a law 2 years prior			85.1840**	94.5075**			0.0914**	0.0831**
			(30.0111)	(24.1207)			(0.0254)	(0.0241)
State Dummies?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Dummies?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Trends?	No	Yes	No	Yes	No	Yes	No	Yes

Robust standard errors in parentheses. Regressions report the results from equation (1), the effect of a state having an abortion law on the child 0-9/women 15-44 ratio. Years included are 1850-1910 and states include all states extant before 1890 excluding the Dakotas. State trends include linear trends. There are 291observations.

*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 2. Falsification Test: Effect of Future Laws

	Child-woman ratio (levels)			Child-woman ratio (logs)		
	actual	10 yrs	20 yrs	actual	10 yrs	20 yrs
Have a Law	116.1485**	74.1622*	-3.7479	0.1005**	0.0404	-0.0168
	(31.9971)	(31.4977)	(30.8423)	(0.0280)	(0.0247)	(0.0233)
State Dummies?	Yes	Yes	Yes	Yes	Yes	Yes
Year Dummies?	Yes	Yes	Yes	Yes	Yes	Yes
State Trends?	Yes	Yes	Yes	Yes	Yes	Yes

Table 3. Falsification Test: Effect of Singing Restrictions on 10-Year Fertility

	Child/woman	Log(child/wom)
Have obscene singing law	-4.397	0.002
	(19.379)	(0.018)
Observations	326	326

Table 4. Falsification Tests: Predicting Law Adoption

	Outcome: State got a law in the following decade				
	(1)	(2)	(3)	(4)	(5)
percent immigrant	0.4134				0.0873
	(1.0254)				(1.0050)
ln(child:woman ratio)		-0.2151			-0.4901
		(0.3798)			(0.4153)
ln(female: male ratio)			1.6416		2.3405
			(1.6088)		(1.6454)
Percent of population lost in the Civil War				-0.6026	0.5342
				(1.8872)	(1.5066)

Table 5. Long-Run Health

	Birth and Survival						
	cohort size		*probability of surviving to:*				
Predictor variable:	Measured in childhood (age 0-9)	Measured in adulthood (age 40-49)	50s	60s	70s	80s	90s
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Abortion Law in Place in Year of Conception	0.0949**	0.0325	-0.0212	-0.0486**	-.0494**	-0.0606+	-.2161+
	(0.0336)	(0.0205)	(0.0170)	(0.0133)	(0.0168)	(0.0345)	(0.1247)
Abortion Law in Place 2 Years Prior to Conception	.0822*	0.0261	-0.0185	-0.0314*	-.0414*	-0.0498	-.1328+
	(0.0396)	(0.0219)	0.0146	(0.0122)	(0.0166)	(0.0339)	(0.0676)
State Dummies?	Y	Y	Y	Y	Y	Y	Y
Age Dummies?	Y	Y	Y	Y	Y	Y	Y
Year Dummies?	Y	Y	Y	Y	Y	Y	Y
State Trends?	Y	Y	Y	Y	Y	Y	Y

Notes: Standard errors in parentheses. Residuals are clustered by state and corrected for heteroskedasticity. Outcomes are weighted by observed population. Each cell reports a coefficient from a separate regression.

Table 6. Potential Mechanisms for Longevity

Predictor variable:		Outcome:					
		blindness	deafness	veteran status	no schooling	never married	no children
		(1)	(2)	(3)	(4)	(5)	(6)
Abortion Law in Place in Year of Conception		0.0003*	0.0000	0.0043	0.0039*	.0119**	0.0011
		(0.0001)	(0.0002)	(0.0268)	(0.0015)	(0.0037)	(0.0021)
Abortion Law in Place 2 Years Prior to Conception		-0.0001	0.0000	-0.0031	0.0034*	0.0121**	0.0003
		(0.0002)	(0.0001)	(0.0208)	(0.0015)	(0.0037)	(0.0026)
State Dummies?		Y	Y	Y	Y	Y	Y
Age Dummies?		N	N	Y	Y	Y	Y
Year Dummies?		Y	Y	Y	Y	Y	Y
State Trends?		Y	Y	Y	Y	Y	Y
Year measured		1910	1910	1940-1970	1900-1970	1900-1970	1900-1970

Notes: Standard errors in parentheses. Residuals are clustered by state and corrected for heteroskedasticity. Outcomes are weighted by observed population. Each cell reports a coefficient from a separate regression.