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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 (Barker 1992), 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 birth control and abortion within U.S. states from 1850 to 1920; we demonstrate that the adoption of these legal restrictions cannot be predicted by other changes in state circumstances, and that legal restrictions that do not directly impact fertility control do not affect our outcomes of interest. We find that about 7 percent more children were born in times and places when fertility control technologies were inaccessible than in states and times when such technologies were freely available. Members of these larger cohorts were roughly 5 percent less likely than those in other cohorts to survive their sixties or seventies. We impute that the typical unintended (or "marginal," in the terminology of Gruber et al. 1999) child born solely due to legal restrictions on access to fertility control was almost 70 percent less likely to live to old age than the average child; this magnitude is similar to estimated effects of recent variation in fertility control on offspring's early life outcomes such as receiving welfare or attending college. 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 fertility control 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 legal changes that, like abortion legalization and oral contraception diffusion, 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. However, 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 birth control and 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 state ratios of children aged 0-9 to women aged 15-44,[1] we demonstrate that introducing a law restricting these activities resulted in an increase in birthrates of approximately 7 percent, comparable to the effect sizes of laws restricting abortion and birth control 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 birth control and abortion (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 1850-1919 cohorts born in states and years with laws outlawing abortion and birth control were 5 percent less likely to survive their 60s or 70s. It is notable that these are the age ranges 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

[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 typical "unwanted") child who was born due to fertility control restrictions was 69 percent less likely to survive to old age than was the average child born in that era.

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., forthcoming).

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. Birth

control and abortion technologies available in the 19th-century United States included: condoms (which became inexpensive after the vulcanization of rubber in 1844); diaphragms and cervical caps (then called pessaries); intrauterine devices (now known as IUDs); the rhythm method (which required instruction); herbal abortifacients 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 practices such as the rhythm method and sexual techniques (e.g. *coitus interruptus*) that reduced the probability of pregnancy. Perhaps in part because of this burgeoning industry (Ananat and Lahey 2007), 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).

There were few laws referencing birth control before the passage of the federal Comstock Act in 1873 (St. John-Stevas 1960). The Act, the result of a moral crusade by Anthony Comstock that was bankrolled by the newly-formed YMCA, defined birth control as obscene and prevented contraceptive information from being distributed across state lines or through the U.S. postal system. This federal law was followed by state "mini-Comstocks" that were often more restrictive than the original, the most famous of which was Connecticut's ban on the actual use of birth control, which was not overturned until 1965. Although quantitative evidence is not available, historians have argued that the frequency of advertisements, lecture tours, and the distribution of print matter dropped sharply after the introduction of such laws; while some advertisers got around bans by using creative code words or selling their items as feminine hygiene devices, reputable firms left the market, prices increased, and information was suppressed (Brodie 1994).

In what follows, we exploit variation in the presence of these 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 birth control and 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 and birth control 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 superceded state statutes. We compiled birth control and obscenity laws from primary sources at the Harvard Law Library, and in cases where that library did not have these sources, the original laws were obtained from state legal archives.

We collected any law concerning: obscene supplies ("articles or instruments of immoral use," such as devices, appliances, apparatuses, drugs); obscene information (any material containing obscene language or images, including information on how to obtain supplies); laws specifically outlawing information or actions related to the "prevention of conception" (birth control); and laws specifically outlawing information or actions related to "procuring a miscarriage" (abortion). For each law, we noted how what share of possible activities it restricted: importing; sale; advertisement; distribution (including circulating or printing information); verbal communication (including "uttering" or giving oral information); using the postal service; possession; possession with intent to

sell or distribute; or singing. 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 also noted exemptions, coding for each law whether it contained a clause indicating that it did not apply to the following: scientific or medical works or books; medical colleges; practitioners of medicine (which could refer to physicians, nurses, druggists, midwives, etc.); artists and works of art; or 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 1860-1920 from tabulated census data from Haines Census tables in the *Historical Statistics of the United States* (Carter et

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

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 is limited to 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).

Longevity

We compiled data from the 1900 through 2000 decennial Censuses on the number of individuals born in a given state and decade 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 90s, 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.

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. (Some areas of the United States had not yet achieved statehood by 1860, and therefore lack Census information on fertility as well as state legal codes for the period we examine; these states were excluded from the analysis.)

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 abortions in year 0 cause a change in births in year 1.

For example, 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]

In addition, we coded characteristics of the law or laws in place during each decade. For example, if a law prohibiting birth control was passed in 1874, and a new

[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.

law exempting druggists was passed in 1876, then the prohibition law was coded as 0.5 for the decade ending in 1880, and the indicator for druggist exemption was coded as 0.3. To measure the extent of the prohibition on birth control, several dimensions were used. In an alternative definition, we coded each law for the share of major activities—importation, circulation, sales, advertisement, and possession with intent to sell—it prohibited. For example, a law that prohibited only importation and sales was coded as a 0.4 if it was in place for the whole decade, and as 0.2 (0.4*0.5) if it was passed in 1874.

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

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

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).

For our second-stage analysis of the effects of wantedness on longevity, we estimate models of the form:

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

in which $S_{y+1,s}$ represents the share of those born in year y+1 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 $havelaw_{ys}$ is a zero-one indicator for whether a law existed in state s in the prior year y. 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 second-stage analysis more precise than our first-stage analysis.

V. Results

First stage

The results of OLS estimates of equation (1) are shown in Table 1. (Regressions using the level rather than log of fertility as the dependent variable, which are not shown, give similar results.) The estimate in column (1) of Table 1 suggests that a law restricting fertility control led to an increase in the number of children per woman of roughly 7 percent. Column (2), which presents the effect of laws weighted by the share of major activities they restrict, suggests that a having a highly restrictive law increased the number of children per woman by as much as 9 percent. Column (3) reveals that controlling for whether a law included a major exemption does not change the estimate of the main effect of the law; the estimates of the law effects in Columns (1) through (3) all statistically significant at conventional levels. The effect of having a major exemption for medical practitioners (i.e. physicians, pharmacists, and midwives) is not statistically

significant at conventional levels, but the point estimate suggests that such an exemption may reduce the impact of a law by 40 percent.

Specification, falsification, and legislative endogeneity checks

Column (4) of Table 1 presents a falsification check for the first stage relationship between laws restricting fertility control and the birthrate. It is possible that laws restricting fertility control are passed at times when society becomes more conservative, for example, and that the birthrate tends to rise during such times as well; if this is the case, then laws may 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. However, the estimate in column (4) is near zero, more than an order of magnitude smaller than our main estimates, and the coefficient is not close to statistical significance. This falsification test provides some confidence in the validity of our main first-stage estimates.

Table 2 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 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:

$$(3) \qquad L_{d+1,s} = \beta_1 \, demographics_{ds} + \delta_d + \delta_s + d*\delta_s + d^2*\delta_s + e_{ds}$$

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 panel of Table 3 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 the Census conducted when cohort members are in their 50s, 60s, 70s, 80s, and 90s, as described in equation (2). Being in a cohort that was relatively large due to a law that restricted access to fertility control has no effect on the share of the cohort that survives to its 50s, as shown in column (1). However, it reduces the share of the cohort that survives to its 60s by 5 percent, and has a similar effect on the share that survives to its 70s. It reduces the share that survives to its 80s by 4 percent. All of these effects, which are shown in columns (2) through (4) are highly statistically significant. The effect on surviving to age 90 or above, shown in column (5) is large but imprecisely estimated.

The lower panel of Table 3 presents a falsification check of the effect of having a law against obscene songs in the state and year of birth on longevity. Because such laws have no effect on the birthrate, as shown in Table 1, column (4), they should not affect longevity as long as the only way that laws affect longevity is through wantedness. Again, the estimates of the relationship between laws against obscene songs and life expectancy are very small (less than one-tenth the size of the main estimates) and are not statistically significant.

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. It is 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.

Our estimates are 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), which is comparable to our estimate of about 7 percent, although effects on some at-risk groups in the 1970s were higher (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 and birth control. 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 5 percent change in longevity 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 69% 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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Table 1

First stage: Effect of Restrictions on Access to Fertility Control on the Child:Woman Ratio

	Standard specification: Effect of the share of the decade that a law was in place	Effect of (law*share of restricted activities)	Effect of law plus practitioner exemption	Falsification check: Effect of a law against singing obscene songs
	(1)	(2)	(3)	(4)
Law	0.0727	0.0920	0.0785	0.0059
	(0.0322)	(0.0377)	(0.0331)	(0.0265)
Exemption			-0.0310	
			(0.0226)	

Notes: Each coefficient represents the estimate from a separate regression. Standard errors in parentheses. Observations are weighted by the number of women aged 15 to 44; residuals are clustered by state and corrected for heteroskedasticity. The unit of observation is a given state and decade; the outcome is logged. Regressions include state fixed effects, decade fixed effects, and state-specific linear and quadratic decade trends. N=326.

Table 2

Checks for Legislative Endogeneity

	Outcome: State got a law in the following decade				
	(1)	(2)	(3)	(4)	(5)
Percent immigrant	0.5312				0.1692
	(6.1706)				(6.3069)
Child:woman ratio		0.5375			0.7548
		(1.3713)			(1.3479)
Female:male ratio			4.7160		5.7661
			(6.0548)		(6.0096)
Percent of population lost in Civil War * post-Civil War				-4.3398	-7.2398
				(10.9470)	(11.8825)

Notes: Each coefficient represents the estimate from a separate regression. Standard errors in parentheses. Observations are weighted by the number of women aged 15 to 44; residuals are clustered by state and corrected for heteroskedasticity. The unit of observation is a given state and decade; the outcome is logged. Regressions include state fixed effects, decade fixed effects, and state-specific linear and quadratic decade trends. N=326.

Table 3

Reduced-Form: Effect of Restrictions on Access to Fertility Control on Longevity

	Share surviving to 50s (1)	Share surviving to 60s (2)	Share surviving to 70s (3)	Share surviving to 80s (4)	Share surviving to 90s (5)
Law	0.0139	-0.0505	-0.0497	-0.0393	-0.1098
	(0.0146)	(0.0153)	(0.0197)	(0.0171)	(0.0765)
Falsification test: Law against obscene songs	0.0112	-0.0028	-0.0054	0.0012	-0.0066
	(0.0190)	(0.0130)	(0.0072)	(0.0039)	(0.0042)

Notes: Each coefficient represents the estimate from a separate regression. Standard errors in parentheses. Observations are weighted by cell population at age 40-49; residuals are clustered by state and corrected for heteroskedasticity. The dependent variable is the population of a given state and year of birth as a share of the population when measured at age 40-49; the outcome is logged. Regressions include state fixed effects, year of birth fixed effects, and state-specific linear and quadratic trends. N=2879.