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THE FERTILITY DECLINE IN THE UNITED STATES: SCHOOLING AND INCOME

CASPER WORM HANSEN
University of Copenhagen

PETER SANDHOLT JENSEN AND LARS LØNSTRUP
University of Southern Denmark

This study investigates the determinants of the fertility transition in the United States from 1850 to the end of the 20th century. We find a robust negative relation between years of schooling and fertility. The magnitude of our baseline estimate suggests that the rise in schooling accounts for about 60% of the US fertility decline. In contrast, we find no evidence of a robust relation between income per capita and fertility. This pattern corroborates theories stressing the importance of human capital investments in generating a transition from high to low fertility.

Keywords: Fertility Transition, Schooling, Income, US States

1. INTRODUCTION

All societies that undergo a process of economic development concurrently experience a fertility transition from high to low fertility rates. What are the main channels that link economic development and fertility? Becker and Lewis (1973) and Becker and Tomes (1976) were the first to show how higher income may be the causal factor of declining fertility. More recently, unified growth theories of Galor and Weil (2000), Galor and Moav (2002), and Cervellati and Sunde (2015) stress that rising investment in human capital is the key mechanism that relates fertility decline to economic development.[1]

The purpose of this paper is to study the empirical patterns of fertility, income, and schooling and examine if these patterns are consistent with the main hypotheses of fertility decline proposed by economists. Specifically, we present new evidence on the importance of both rising levels of income and schooling in explaining the fertility decline in the United States. The analysis exploits data on

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cohort fertility, cohort years of schooling, and income per worker for a panel of 48 US states observed from 1850 to 1980. By employing static and dynamic panel models using both fixed effects (FE) and generalized method of moments (GMM) estimation strategies, we find a robust negative relationship between schooling and fertility for cohorts between 1850 and 1980. Our baseline estimate suggests that one additional year of schooling of children is associated with 0.17 fewer children. This implies that the observed increase in schooling between 1850 and 1980 accounts for at least 60% of the fertility decline in that period. The effect of income per worker on fertility is not robust: FE estimates suggest a negative relationship, whereas a positive relationship is found when using a dynamic panel framework. We take these findings as supporting evidence for theories predicting that increasing investment in human capital is the key mechanism that creates a relationship between the process of economic development and the fertility transition.

Since the purpose of the paper is to investigate the extent to which the variation in income and schooling explains the variation in fertility, we note that the presented estimates should not be interpreted as causal effects. Hence, our main motivation for using a dynamic panel model and GMM estimation is to remove the mechanical bias resulting from the presence of FE and lagged dependent variables. For the empirical analysis, this would mean that we report robust correlations [Acemoglu et al. (2015)]. We therefore provide reduced-form relationships that, together with the existing empirical literature, serve to discipline theories of the fertility transition.

Our analysis is primarily related to the literature studying the causes of the demographic transition empirically. The most closely related recent studies are Murtin (2013) and Murphy (2015). Based on a panel of countries, Murtin (2013) finds that the average years of schooling in the workforce has a robust negative correlation with fertility, whereas the relationship between income and fertility varies across specifications. Murphy (2015), who studies the fertility decline in France during the 19th century, finds that higher literacy among parents is associated with lower fertility, whereas income per capita and fertility are, if anything, positively related. Similar to our results, his estimates show no significant partial correlation between mortality and fertility. Most importantly, and consistent with our results for the US fertility decline, these papers find that increasing investment in human capital was crucial for declining fertility, whereas the association between fertility and income is not robust.[2] Herzer et al. (2012), using panel cointegration, find that gross domestic product (GDP) per capita has a negative and significant relation to crude birth rates. However, for a smaller subset of countries studied in the 20th century they also find primary education to be significantly and negatively related to fertility.[3]

This paper shares its focus on the US fertility transition with Jones and Tertilt (2008) and Greenwood and Seshadri (2002). The study by Jones and Tertilt (2008) finds a bivariate negative relationship between income and fertility using historical US census data. They conclude that much of the difference in fertility experiences "can be accounted for by differences in income alone" [Jones and Tertilt (2008, p. 208)], but do not evaluate the relative contributions of increases in income and schooling in explaining the fertility transition, which is the purpose of the present paper.[4] Greenwood and Seshadri (2002) employ model simulations to examine what drove US fertility. In their theory, both rising wages and the increase in schooling play a role for the fertility decline.[5]

Our study has five major advantages, which set it apart from previous research. First, the data contain decadal observations for the period 1850–1980.[6] This is a longer period than the one used in existing within-country studies and covers the entire period of 1870–1930 during which most of the fertility transition took place in the majority of present day developed countries [Reher (2004)].[7] Second, since we use data from a single country (the United States), our study is less prone to effects from unobserved time varying effects, such as changes in culture, institutions as well as data quality, across observed units, compared to cross-country analyses.[8] Third, we follow Jones and Tertilt (2008) and use cohort fertility as a dependent variable. Since our measure of fertility is linked to specific cohorts, as opposed to period specific measures (for example, the total fertility rate), it provides a better description of the actual change in fertility behavior of cohorts.[9] Fourth, assuming a common lag structure is arguably more sensible across the US states than across different countries, which is a weakness of cross-country studies also pointed out by Herzer et al. (2012).[10] Finally, data on years of schooling and GDP per worker, similar to those used in cross-country studies, are available for the US states, unlike in other within-country studies.

2. THEORIES OF FERTILITY

This section begins by presenting a prototype theoretical model of fertility choice to motivate our empirical analysis. Next, we discuss the implications for the empirical analysis.

2.1. A Model of Fertility Choice

The model is based on a quantity–quality choice in the spirit of Becker and Lewis (1973). It is constructed to generate the main insights of Becker and Tomes (1976) and Galor and Weil (2000). In addition, we consider possible channels through which mortality affects fertility.

The preferences of individuals are represented by the following lifetime utility function:

$$U = v(c) + f(\pi b) + g(a, s),$$

where c is consumption, b is the number of births, s is the fraction of the total time endowment that the parent invests in the human capital of each child, and a is a parameter that influences the rates of return to human capital investment. A fraction, π, of the children survive to adulthood. Utility depends positively on the number of surviving offspring, πb, implying that individuals care about reproductive success. The felicity functions v, f, and g are assumed to be twice differentiable and strictly concave.

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The individual chooses b and s to maximize U subject to the following budget constraint:

$$c + b\pi y\,[\tau + s] \leq y,$$

where y is lifetime income and τ is the minimum fraction of the total time endowment that parents must spend to raise each surviving child. Suppose that the first-order conditions imply that the time invested in the human capital of each child is a single valued and increasing function of the rates of return to schooling, income, and the survival rate of children [i.e., $s = s(a, y, \pi)$ with $s_a > 0$, $s_y > 0$ and $s_\pi > 0$]. If these variables vary over time, then b is a single valued function of time given by $b = f\{y(t), s[a, y(t), \pi(t)], \pi(t)\} \equiv b(t)$. Hence, the model predicts that variation in fertility over time can be allocated into the following channels:

$$\frac{db}{dt} = \left[f_y + f_s s_y\right]\frac{dy}{dt} + f_s s_a \frac{da}{dt} + [f_\pi + f_s s_\pi]\frac{d\pi}{dt}.$$

We show in the appendix that $f_y \lesseqqgtr 0$, $f_s < 0$ and $f_\pi < 0$. The sign of f_y is determined by the relative strength of income and substitution effects and is thus determined by the curvature of the utility function. Galor and Weil (2000) assume that $f_y > 0$ for levels of income below a given threshold level which depends on subsistence requirements.

The main argument made by Becker and Lewis (1973) is that higher income causes lower fertility (i.e., in their model, it holds that $f_y + f_s s_y < 0$). Thus, even though the direct effect of income on the demand for children is positive (i.e., $f_y > 0$), they explain the observed negative relation between income and fertility by s_y being sufficiently large (numerically).[11]

The explanation for the fertility decline put forward in unified growth theories, for example in Galor and Weil (2000), is represented in the second term.[12] Their main hypothesis is that rapidly changing technology increases the returns to investing in schooling, since this provides children with human capital that is immune to shifts in the production processes. When each child obtains more schooling it implies that parent chooses to have more expensive children and, as a result have fewer children.

Finally, we have incorporated effects from mortality.[13] The theory predicts a negative effect on fertility from a higher survival rate of children. This is due to a direct effect with the simple intuition that a higher survival rate increases the expected cost of having children, and to an indirect effect from the positive effect of the survival rate on schooling. The latter effect is incorporated to formalize the argument that exogenous changes in mortality may affect fertility by changing the returns of children relative to the returns to human capital investment in children.[14]

If family income comprises more sources than the wage earned by the parent who spends most time on child rearing, then an increase in this wage implies that the proportional increase in the price of children will be higher than the proportional increase in total family income. Thus, rather than changes in the level of income per se, Galor and Weil (1996) demonstrate that a narrowing of the wage gap between men and women during the process of industrialization can explain a decline in fertility.[15]

2.2. Implications for the Empirical Analysis

The model formalizes the idea that if it is mainly the level of income that determines the level of expenditure parents devote to each child, as suggested in Becker and Lewis (1973) and Becker and Tomes (1976), variation in income would explain a large part of the variation in fertility. In contrast, if it is a rise in the rate of return to human capital investment that mainly affects expenditure per child in the form of rising levels of schooling, one would expect schooling to vary independently of the level of income and have explanatory power for variation in fertility. In addition, the model shows that if mortality mainly affects fertility by changing the net of cost returns to schooling relative to the number of children, once variation in schooling is taken into account, mortality may have only a minor, if any, direct effect on fertility.

Although the model provides a foundation for studying how fertility is affected by income, schooling, and mortality, the correlations between fertility and these explanatory variables are likely to be a product of two-way causation. Therefore, we do not aim at making causal statements, but are merely interested in showing whether the evidence is consistent with the leading economic theories on the fertility transition. Thus, although GMM estimation may alleviate some of the concerns about endogeneity, we interpret these estimates as reflecting "robust correlations."

This also means that we do not test the quantity–quality theory even though we use this framework as the theoretical foundation for why fertility may be related to income and schooling. In fact, the quantity–quality model does not provide an unambiguous prediction regarding the effects of an exogenous increase in the level of schooling (e.g., mandatory schooling) on fertility or vice versa, as shown in Rosenzweig and Wolpin (1980). They note that "[. . .] as indicated by the expressions for the observed compensated cross price effects, the positive relationship between N and the shadow price of Q does not necessarily imply that an exogenous increase in N will reduce quality per child, since Q and N may be (strong) complements"; see also Aaronson et al. (2014), who show, in a quantity–quality framework, that a decrease in the price of education may lead to higher fertility at the extensive margin. This shows that in addition to the issue of endogeneity the signs of the correlations between measures of quality of children and fertility are not informative for refuting the quantity–quality theory.

One potential concern when taking these ideas to the data could be that in most economies the level of human capital and income would be strongly positively correlated, which would make it difficult to distinguish their respective partial correlation with fertility. However, nothing prevents contemporaneous levels of investment in human capital (here measured by years of schooling) and income to vary independently of each other. Indeed, this independent variation is what allows us to distinguish the above-mentioned hypotheses of fertility decline in the empirical analysis.

3. DATA

This section describes the variables used in the analysis. The data were compiled and, to some extent, constructed by Turner et al. (2006) and Murphy et al. (2008) using a number of sources described in more detail in the data appendix. The variables are measured at 10-year intervals, which correspond to one period in our empirical framework. We use information on the number of children ever born to married white women between the ages of 35 and 44, sampled on date τ in the US Census. Assuming that the fertility decisions for this cohort of women on average were determined 20 years prior to τ, we construct our fertility variable Fertility_{st}, which was influenced by the conditions in state s around year $t = \tau - 20$ years. For example, Fertility_{s1900} is then equal to the number of children ever born to married white women between the ages 35 and 44 sampled in 1920.

Murphy et al. (2008) calculate average years of schooling for cohorts in the United States. As our baseline measure of schooling, we use average years of schooling for the cohort born in year $t + 4$, School cohort_{st}. For example, School cohort_{s1900} captures years of schooling for the birth cohort of 1904, who started in the schooling system in 1910, across the US states. We use this measure in our baseline estimations to test whether parents who gave birth to fewer children on average provided more schooling to each child, which would imply a negative correlation between our baseline measure of schooling and fertility. The variable years of schooling was constructed by using a perpetual inventory methodology on official statistics on enrollment rates and other available statistics.[16] Turner et al. (2006) summarized their results as follows: "Our methodology results in state estimates similar to those reported in the US Census from 2000 back to 1940 and national, turn of the century estimates strikingly close to those presented by Schultz." This is reassuring for the reliability of the data.[17] Moreover, the methodology was recently applied by Morrison and Murtin (2009) for a cross-section of countries for the period 1870–1960, and these data were used by Murtin (2013). We also use an indicator for average years of schooling in the workforce, School *p.w.*, for robustness discussed in detail in Section 5.3. We use log GDP per worker measured in 2000 dollars to capture the income level of state s in year t. The 19th century GDP per worker data were constructed by Easterlin (1957, 1960) and Turner et al. (2006) based on agricultural and manufacturing production from the censuses. By contrast, historical cross-country estimates also rely on the information from multiple statistical bureaus.

Figures 1–3 depict how the three key variables changed from 1850 to 1980. The left-hand-side (LHS) panels show the development in the state average of these variables, whereas the right-hand-side (RHS) panels show them by state. Figure 1 reveals that fertility in the United States started its decline in 1870 from a level of

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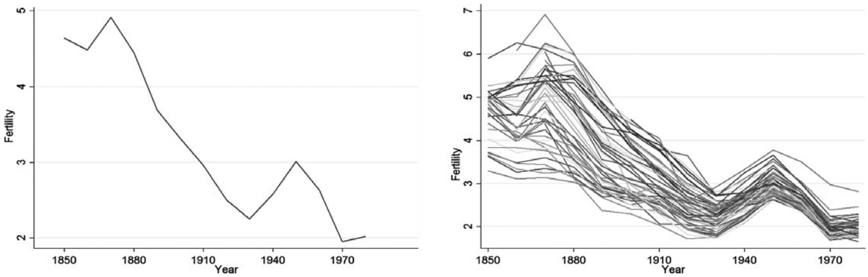

FIGURE 1. The US fertility decline. The LHS panel shows the state average of fertility, whereas the RHS panel shows it by state.

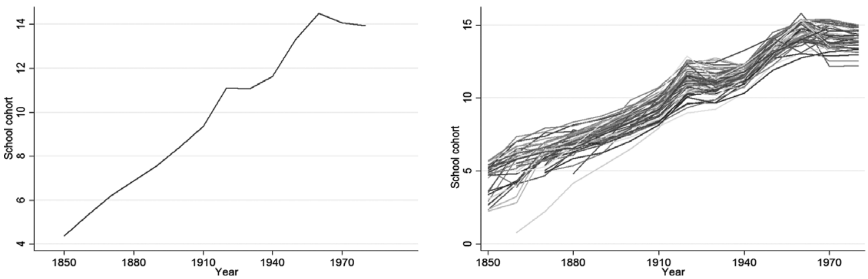

FIGURE 2. The rise in schooling (cohort). The LHS panel shows the state average of School cohort, whereas the RHS panel shows it by state.

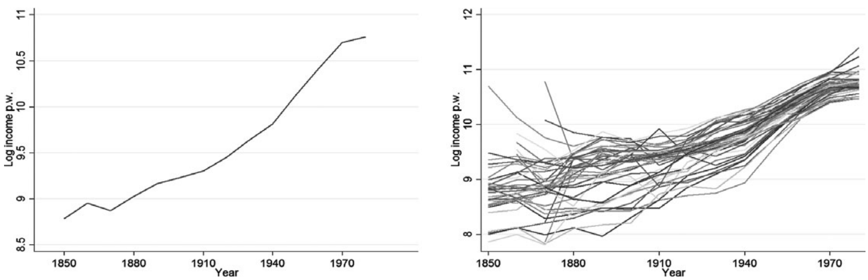

FIGURE 3. The rise in log income *p.w.* The LHS panel shows the state average of log income *p.w.*, while the RHS panel shows it by state.

4.9 children per woman to 2.3 children in the 1930s. This was followed by a baby boom and a baby bust, but the fertility level in the baby-boom years 1950–1960 did not exceed the pre-1910 level. Figure 2 documents a pattern in schooling that seems to be inversely related to fertility: increasing steadily from 1860 to 1920, while being almost U-shaped from 1930 to 1980. In Figure 3, we observe that

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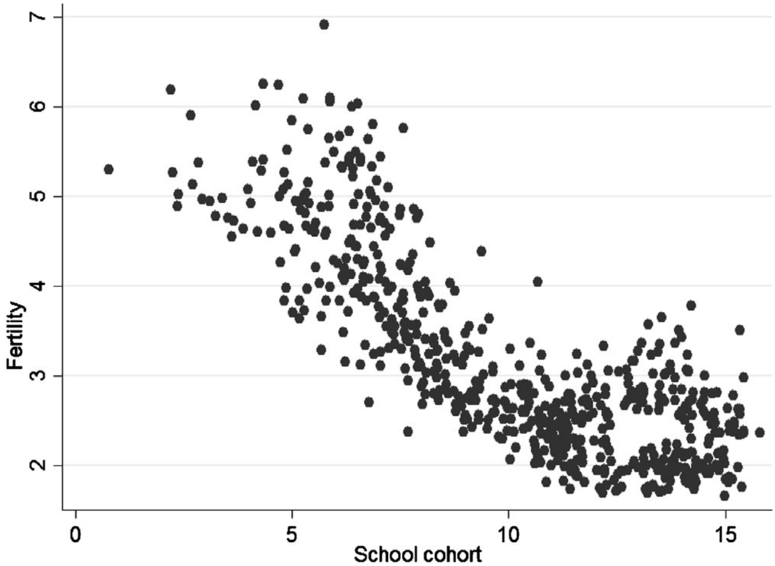

FIGURE 4. Scatter plot between fertility and school cohort.

income levels were generally rising during the observed period, although with some decade-to-decade fluctuations, in particular before World War II. Figures 4 and 5 show scatter plots between fertility and schooling and fertility and income, respectively. They also indicate that the unconditional relationships are negative, which is also evident from Table 2.

To control for the effect of mortality, we use the probabilities of dying in the age intervals 0–15 and 15–60 for white individuals in year t. These data were compiled by Murphy et al. (2008) from death registration statistics and census information.

Table 1 provides the summary statistics for the six variables that we have mentioned so far. Before we move on to present our empirical strategy, Table 2 reports the (partial) correlations between income and schooling. Although the unconditional correlation is positive and statistically highly significant, the important lesson to be learned is that there is *no* such relationship left after controlling for state and time FE [columns (1) and (2)]. This shows why our empirical analysis is able to disentangle the effects from schooling and income. Furthermore, a similar, though less clear cut, conclusion emerges for average years of schooling in the workforce, School *p.w.*, [columns (3) and (4)].

4. EMPIRICAL STRATEGY

In this section, we describe our econometric specifications. Our approach is to estimate a panel data model with state and time FE, while also allowing for

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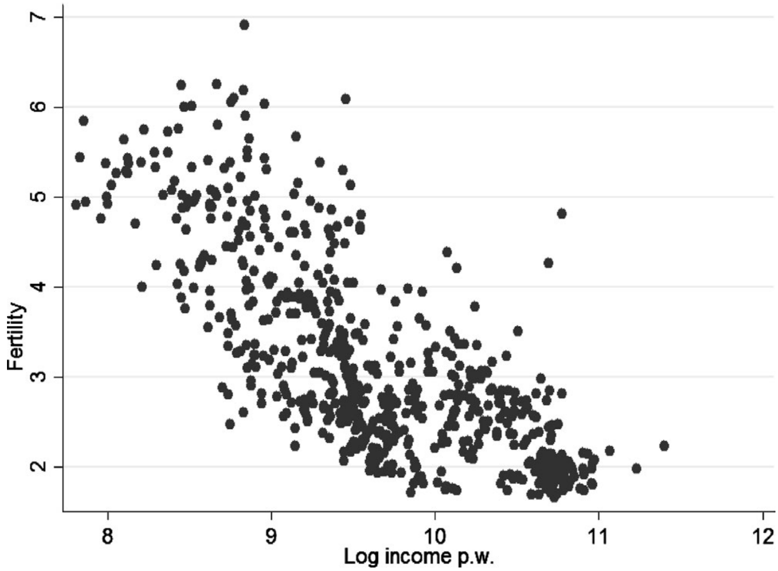

FIGURE 5. Scatter plot between fertility and log income *p.w.*

TABLE 1. Summary statistics

	(1) Obs	(2) Mean	(3) SD	(4) Min	(5) Max
Fertility	633	3.170	1.106	1.656	6.916
School cohort	633	10.11	3.325	0.787	15.79
log income *p.w.*	633	9.632	0.738	7.806	11.40
Mortality 0–15	633	0.357	0.174	0.112	0.864
Mortality 15–60	633	0.149	0.113	0.0139	0.488
Number of states	48	48	48	48	48

Notes: The table reports descriptive statistics for the main empirical analysis over the period 1850–1980.

dynamics in fertility. We follow two strategies to investigate the effect of income and schooling on fertility. The first strategy controls for state and time FE, which take into account that the US states differ in many permanent characteristics that we do not observe and which may also affect schooling and income. This model specification is presented in Section 4.1. The second strategy allows for mean-reverting dynamics and persistent effects in fertility that may be endogenous to income and schooling.[18] We present this model specification in Section 4.2.

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TABLE 2. Partial correlations

	Variable:			
	(1)	(2)	(3)	(4)
Variables:				
		log income *p.w.*		
School cohort	0.186***	0.0158		
	(0.00747)	(0.0252)		
School *p.w.*			0.196***	0.0809*
			(0.00752)	(0.0408)
Time effects	No	Yes	No	Yes
Fixed effects	No	Yes	No	Yes
States	48	48	48	48
Observations	633	633	633	633

Notes: The table reports (partial) correlations between the schooling variables and log income per worker.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

4.1. Fixed-Effects Model

The empirical specification for the FE model is given by

$$\text{Fertility}_{st} = \beta \text{ School cohort}_{st} + \gamma \log \text{income } p.w._{st} + \mathbf{Z}'_{st}\eta + \tau_t + \lambda_s + \varepsilon_{st}, \quad (1)$$

where Fertility_{st} is the average number of children per woman born around year t in state s, $\text{School cohort}_{st}$ is years of schooling for the cohort of children born around year t, income $p.w._{st}$ is the gross domestic product per worker in constant 2,000 dollars, and $\mathbf{Z}_{st}$ denotes a vector of other controls, which, for example, includes information on the cross-sectional mortality patterns in the age intervals from 0 to 15 and 15 to 60, respectively. Model (1) is estimated utilizing a panel of 48 US states, consisting of observations at 10-year intervals between 1850 and 1980, which allows us to nonparametrically control for state (λ_s) and time (τ_t) FE. The error term (ε_{st}) is clustered at the state level, so that our results are fully robust against serial correlation at the state level.

4.2. System GMM

In order to disentangle the income–fertility and schooling–fertility relationships from persistence in fertility, we also consider the following dynamic specification:

$$\begin{aligned} \text{Fertility}_{st} &= \alpha \text{Fertility}_{st-1} + \beta \text{School cohort}_{st} + \gamma \log \text{income } p.w._{st} \\ &\quad + \mathbf{Z}'_{st}\eta + \tau_t + \varepsilon_{s,}, \qquad (2) \\ \varepsilon_{st} &= \mu_s + v_{st}, \qquad (3) \end{aligned}$$

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where the variables are defined as above, though we let $t = 1, 2, \ldots, T$, where each period is a decade. We estimate equation (2) by the System GMM estimator, where all covariates are treated as endogenous. We apply the System GMM estimator, which requires instruments w_{it} satisfying $E(w_{it}\Delta\varepsilon_{st}) = 0$ and $E(\Delta w_{it}\varepsilon_{st}) = 0$ for consistency [see Roodman (2009)]. In the absence of serial correlation in v_{st} (i.e., no second-order serial correlation in Δv_{st}) appropriately lagged values of the dependent variable and the covariates are valid instruments for the differenced equation, and differenced variables can be used as instruments for the variables in the level equation.[19] Murtin (2013) chooses $w_{it} = y_{i,t-l}$ for $l \geq 3$ and $\Delta w_{it} = \Delta y_{it-1}$ for $t \geq 4$ with the maximum lag set to the 7th lag. This choice amounts to using 30–70 year lags. However, it may lead to "too many instruments" as noted by Roodman (2009) and weak power of J-tests of over-identifying conditions. A solution is to extract principal components of the original instrument set in order to reduce the number of actual instruments so as to avoid the problem of "too many instruments." [20] We adopt this solution using 30–70 year lags for instruments. The principal components are a smaller instrument set that is maximally representative of the original; see e.g., Mehrhoff (2009, p. 5), who also provides Monte Carlo evidence that using principal components yields better results.[21] Thus, we use principal components to address concerns regarding J-tests, but note that our results are unaffected by this choice.

5. RESULTS

This section presents the results. We first discuss the results based on pooled and FE ordinary least squares (OLS) estimation in Section 5.1, System GMM results follow in Section 5.2, and our empirical analysis ends with a robustness analysis in Section 5.3.

5.1. Pooled OLS and Fixed-Effects Estimates

Table 3 provides the results of estimating equation (1) by pooled OLS. For consistency, this estimation method requires that the explanatory variables are unrelated to the composite error $\nu_{st} = \lambda_s + \varepsilon_{st}$, conditional on time FE across the US states; that is, $E(\nu_{st}|\mathbf{X}'_{s,t}) = 0$, where $\mathbf{X}'_{s,t} \equiv$(School cohort$_{st}$, log income $p.w._{st}$, $\mathbf{Z}'_{st}$, τ_t). All regressions include time FE. Column (1) starts by only including the cohort schooling variable. The estimated coefficient shows that fertility and years of cohort schooling were negatively associated over the last 130 years. Column (2) shows a corresponding result for income. The next two columns contain School cohort$_{st}$ and log income $p.w.$ at the same time, but without and with controls for mortality, respectively. The coefficients on both variables are negative, statistically significant, and similar in magnitude to the univariate results from the first two columns. Adding the 10-year lag of Fertility$_{st}$ reduces the statistical significance of the coefficient on log income $p.w.$, while the human-capital variable retains

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TABLE 3. Pooled OLS estimates

	Dependent variable is fertility				
	(1)	(2)	(3)	(4)	(5)
School cohort	-0.330^{***}		-0.254^{***}	-0.217^{***}	-0.0864^{***}
	(0.0423)		(0.0441)	(0.0408)	(0.0196)
log income *p.w.*		-0.768^{***}	-0.606^{***}	-0.543^{***}	-0.124^{*}
		(0.0867)	(0.0723)	(0.0878)	(0.0627)
Fertility$_{t-1}$					0.713^{***}
					(0.0525)
Mortality 0–15				-3.287^{**}	-0.802^{*}
				(1.311)	(0.434)
Mortality 15–60				2.554^{***}	0.537
				(0.907)	(0.339)
Time effects	Yes	Yes	Yes	Yes	Yes
Fixed effects	No	No	No	No	No
States	48	48	48	48	48
Observations	633	633	633	633	633

Notes: The table reports OLS estimates. The unit of observation is the US state over the period 1850–1980. Constants are not reported. Standard errors clustered by state in parentheses.
$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

its significance [column (5)]. Furthermore, in comparison to the estimates in the former columns, the magnitudes of the effect of both variables are decreased.

Table 4, which parallels the structure of the first table, presents our basic results which include controls for state FE. The FE estimator will be consistent if $E(\varepsilon_{st}|\mathbf{X}'_{st}, \lambda_s) = 0$. This estimation method does not require that the explanatory variables are orthogonal to the state FE. However, as can be seen from the estimates reported in Tables 3 and 5, a similar picture emerges when FE are not included in the regressions, indicating that the US states are actually relatively homogenous in terms of time invariant factors affecting both fertility and the explanatory variables.

In our baseline FE specification, reported in column (4), the effect of schooling on fertility is -0.17 with a standard error of 0.03. Taken at face value, this estimate implies that one additional year of schooling reduces the number of siblings by 0.17, implying that the rise in schooling from 4.4 to 14.0 years over the period 1850–1980 explains circa 60% of the US fertility transition.[22] In comparison, the international evidence, reported in Murtin (2013), suggests that when average years of primary schooling in the workforce increase from 0 to 6 years, the fertility rate decreases by 40–80%. Moreover, our baseline estimate suggests that 21% of the decline in fertility between 1870 and 1910 is due to the rise in schooling, whereas Murtin's (2013) estimate suggests that for the same period only 8.8% of the fall in fertility is explained by schooling. This discrepancy is likely to be explained by the fact that we use a cohort-specific measure of fertility, whereas

TABLE 4. Baseline fixed-effects estimates

	Dependent variable is fertility				
	(1)	(2)	(3)	(4)	(5)
School cohort	-0.202^{***}		-0.193^{***}	-0.166^{***}	-0.115^{***}
	(0.0370)		(0.0319)	(0.0308)	(0.0220)
log income *p.w.*		-0.625^{***}	-0.601^{***}	-0.548^{***}	-0.263^{***}
		(0.107)	(0.0873)	(0.0811)	(0.0901)
Fertility_{t-1}					0.539^{***}
					(0.0716)
Mortality 0–15				-2.874^{***}	-1.252^{*}
				(1.008)	(0.656)
Mortality 15–60				2.557^{***}	0.973^{**}
				(0.679)	(0.400)
Time effects	Yes	Yes	Yes	Yes	Yes
Fixed effects	Yes	Yes	Yes	Yes	Yes
States	48	48	48	48	48
Observations	633	633	633	633	633

Notes: The table reports the FE estimates. The unit of observation is the US state over the period 1840–1980. Constants are not reported. Standard errors clustered by state in parentheses.
$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Murtin (2013) uses a time-specific measure of fertility, which changes more slowly since it is an averages of fertility of all the women in the reproductive age.

Table 4 also shows that the effect associated with a 10% increase in income per worker is -0.05, which is statistically significant at the 1% level. In the last column of the table, we include the lagged outcome variable Fertility_{st-1}. Although the FE estimator by construction is biased, $\text{Cov}(\text{Fertility}_{st-1}, \varepsilon_{st}) \neq 0$, and the estimate therefore must be interpreted with caution, the regression coefficients associated with income and schooling remain negative and significant, although they decrease in magnitude as can be seen in Table 3. Regarding mortality, the positive correlation between adult mortality and fertility is consistent with the above-mentioned theories where decreasing mortality lowers fertility. However, the negative correlation between mortality at age 0–15 and fertility seems, at first hand, at odds with these theories. One possible explanation is that the mortality variable for age 0–15 may be measured with considerable errors relative to mortality for age 15–60. Moreover, the coefficients for both variables drop considerably in numerical value when the lagged value of fertility is included as explanatory variable, which calls for a cautious interpretation of these coefficients.

Overall, the initial results indicate that both income and schooling were significant determinants of the US fertility decline over the period 1850–1980. However, questions regarding the interpretation of the estimates remain unanswered. For example, it is possible that $\text{Cov}(\mathbf{X}_{st}, \varepsilon_{st}) \neq 0$ because of a reverse causality: Lower fertility naturally leads to a smaller population size, which in a decreasing

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TABLE 5. Baseline system GMM estimates

	Dependent variable is fertility				
	(1)	(2)	(3)	(4)	(5)
Fertility$_{t-1}$	0.422***	0.511***	0.620***	0.682***	0.765***
	(0.131)	(0.108)	(0.120)	(0.116)	(0.165)
School cohort	−0.150		−0.254***	−0.278***	−0.269***
	(0.0977)		(0.0614)	(0.0817)	(0.0850)
log income *p.w.*		−0.259*	0.206	0.237	0.371
		(0.152)	(0.223)	(0.153)	(0.272)
Mortality 0–15				1.242	2.258
				(1.740)	(2.108)
Mortality 15–60				−0.764	−1.382
				(0.836)	(1.701)
AR(1) *p*-value	0.012	0.000	0.000	0.000	0.000
AR(2) *p*-value	0.680	0.321	0.330	0.243	0.282
Hansen *J* *p*-value	0.177	0.005	0.103	0.192	0.278
Time effects	Yes	Yes	Yes	Yes	Yes
States	48	48	48	48	48
Observations	633	633	633	633	633

Notes: The table reports the System GMM estimates. The unit of observation is the US state over the period 1850–1980. Columns (1)–(3) use lags 3–7 as instruments, whereas column (4) uses lags 3–4. All RHS variables are treated as endogenous. Constants are not reported. Standard errors clustered by state in parentheses.
$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

returns to scale economy tends to increase the level of income. Moreover, the FE estimator might be inconsistent if unobserved time varying variables are correlated with the regressors in the model. For example, social norms which correlate with our observables may persist over time, and this may lead to persistence in fertility. Below, we consider the GMM system estimator to deal with these issues.

5.2. System GMM Estimates

Table 5 reports the System GMM regressions of equation (2). In column (1), we include the schooling variable along with the lagged dependent variable. The estimate on School cohort$_{st}$ is −0.15 with a standard error of 0.10. Column (2) reports a negative estimate for the coefficient on income which is statistically significant at the 10% level. However, once both variables are included in column (3), we find that the relation between income and fertility becomes positive but statistically insignificant. Thus, the negative FE estimates for income—presented in Section 5.1—are *not* robust to this alternative estimation strategy, suggesting that rising income was not instrumental for the US fertility transition. In contrast, the association between schooling and fertility remains robust for these alternative specifications, as the estimate for the coefficient on schooling is negative and statistically significant. The estimate of α in column (3) implies that the long-run effect of one additional year of schooling on the number of siblings is $-0.25/(1 - 0.62) = -0.66$. In column (4), we enter income and schooling together along with the mortality variables [i.e., we now study the full model as specified in equation (2)]. The estimates for the coefficients on income and schooling remain largely unaffected both in terms of magnitude and statistical significance. The first four columns of the table use lags with a length of between three and seven time periods as instruments (i.e., the variables are lagged 30–70 years). In column (5), we change the lags to be between three and four periods. Again, our estimate on schooling is stable in magnitude and significance.

Finally, it is notable from the bottom part of Table 5 that all the regressions pass the tests of first- and second-order serial correlation. First-order serial correlation is present in the differenced residuals, whereas second-order serial correlation cannot be detected. This is in line with the modeling assumptions of the estimators. Moreover, the specifications in columns (1), and (3)–(5) are accepted with respect to the validity of their instruments with p-values that are not implausibly high, suggesting that we effectively address the concern regarding the "too-many-instruments" problem. It should be mentioned that the model in column (2), which only includes log income *p.w.,* appears misspecified, as the J-test rejects the validity of the over-identifying restrictions.

Overall, the estimates reported in Table 5 suggest that the rise in schooling is the primary reason why economic development and the fertility transition are related, whereas income is, if anything, positively related to fertility.

5.3. Robustness

This section presents various extensions to the baseline FE and System GMM results reported in the preceding section.

Table 6 shows results from examining additional channels through which schooling and fertility may be related. Since Cochrane (1979), it has been widely recognized that there is a negative correlation between parents' level of education and fertility. Various mechanisms that link these variables have been suggested in the literature.[23] First of all, the education of parents may influence fertility via its effect on the income of parents through the channels shown in the theoretical model. In addition, the time devoted to children may leave less time for human capital accumulation (either formal schooling or on the job training), which directly creates a negative relation between the variables—a mechanism that is described in Cervellati and Sunde (2015). Although this argument suggests that causation runs from schooling to fertility, the studies of Angrist and Evans (1998) and Cohen et al. (2011) show evidence of a negative effect of childbearing on the education of mothers. Thus, as is the case in the context of the quantity–quality trade-off, there is a two-way causation, caused by the trade-off parents face between their own education and the number of children they have.[24] With these caveats in mind, we follow the related literature and consider the association between

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TABLE 6. Schooling in the workforce

	Dependent variable is fertility			
	Fixed effects		System GMM	
	(1)	(2)	(3)	(4)
Fertility$_{t-1}$			0.673***	0.845***
			(0.132)	(0.167)
School *p.w.*	−0.253***	−0.205***	−0.271***	−0.174***
	(0.0683)	(0.0549)	(0.0844)	(0.0557)
School cohort		−0.138***		−0.254***
		(0.0214)		(0.0830)
log income *p.w.*	−0.508***	−0.511***	0.432	0.452**
	(0.0803)	(0.0758)	(0.301)	(0.206)
AR(1) *p*-value	–	–	0.001	0.000
AR(2) *p*-value	–	–	0.708	0.384
Hansen *J* *p*-value	–	–	0.070	0.132
Time effects	Yes	Yes	Yes	Yes
States	48	48	48	48
Observations	633	633	633	633

Notes: Columns (1) and (2) report the FE estimates. Columns (3) and (4) report the System GMM estimates. The unit of observation is the US state over the period 1850–1980. All regressions include the mortality variables: Mortality 0–15 and Mortality 15–60 (not reported). Columns (3) and (4 use lags 3–7 as instruments. All RHS variables are treated as endogenous. Constants are not reported. Standard errors clustered by state in parentheses.
$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

fertility and the average years of schooling in the workforce, School *p.w.*, which proxies for the level of parental schooling. Considering the basic specification, columns (1) and (3) replace cohort schooling with average years of schooling in the workforce, whereas columns (2) and (4) augment the basic model with average years of schooling. The association between average years of schooling in the workforce and fertility is negative and significant in all four specifications. For example, when our cohort-based measure of schooling is not included, column (3) shows that the coefficient estimated by System GMM is −0.27 with a standard error of 0.08. Moreover, as expected, the point estimate of the partial correlation coefficient between schooling years of the cohort and fertility is reduced once we control for School *p.w.* Reassuringly, the coefficient retains the negative sign and is statistically significant at the 1% level. The System GMM estimate, reported in column (4), implies that the rise in schooling between 1850 and 1980, as measured by School cohort, accounts for about 50% of the fertility transition. We note that the test statistics associated with the System GMM method in the full model in column (4) is passed, but the *p*-value of the Hansen *J*-test is 0.132, which is lower than in our baseline specification; see the bottom of column (4).[25] In sum, the evidence in Table 6 shows that the level of human capital of the parents as well as the human capital level of their children is negatively related to the number of children born per woman.

Table 7 suggests that our main results are robust to alternative time periods. Columns (1) and (4) focus on the period 1850–1920, which is the period before the onset of the fertility decline for the United States as a whole according to Reher (2004), whereas columns (2) and (5) cover the subsequent period from 1930 to 1980. By studying the period 1870–1980, column (6) addresses the concern that our main results are affected by the fact that we use different measures of fertility before and after 1870 (see the appendix).[26] For all three subperiods, we find a consistent negative association between schooling and fertility, both in the FE and the System GMM regressions, albeit the coefficient, reported in column (5), is imprecisely estimated. Again, as in our baseline specification, the coefficient estimate on income per worker becomes positive in columns (4) and (6) when we apply the System GMM estimator.

Table 8 presents FE and System GMM estimates for different functional forms. The table shows negative and significant coefficients regardless of whether human capital is measured in years of schooling or log years of schooling, and regardless of whether fertility is measured in levels or logs. To compare our results with those of Murtin (2013), the model specifications reported in columns (1) and (3) have the same functional forms as the baseline model in his analysis. Murtin (2013) estimates the effect of schooling on fertility in the range from −0.11 to −0.04, whereas the US evidence indicates that the effect is close to −0.06 (i.e., the FE estimate is −0.04 and the System GMM is −0.06), and a similar estimate is recovered using average years of schooling in the workforce (not reported). In addition, the estimated coefficients on log income *p.w.* parallel those presented in the former tables.[27] Since the period-specific fertility variables used in Murtin (2013) change more slowly when behavior changes compared to the cohort-specific fertility variables which we use; the coefficients that we estimate on the correlation between fertility and schooling are not directly comparable. Nevertheless, the qualitative results, that the rising level of schooling is the main determinant of fertility decline, are very similar to Murtin (2013).

Table 9 shows results from FE and System GMM specifications allowing the relationship between income and fertility to be nonmonotonic. Although the Beckerian theories propose that income has a negative effect on fertility, positive shocks to productivity have caused temporary surges in income and fertility for most of human existence. This observation was first made by Thomas Malthus and which laid the foundation for the Malthusian theory.[28] Together, these theories suggests that income has a nonmonotonic effect on fertility if the relative strength of the negative and the positive effects of income on fertility varies with the level of income. Consequently, we test for a nonlinear relation between income and fertility in the empirical analysis. Columns (1) and (2) report the FE estimates, whereas columns (3) and (4) report the System GMM estimates. The estimated coefficients on log income p.w. and the square of log income p.w. in column (1) indicate that fertility is U-shaped in income. However, the turning point is

TABLE 7. Sample splits by time periods

	Dependent variable is fertility					
	Fixed effects			System GMM		
	1850–1920	1930–1980	1870–1980	1850–1920	1930–1980	1870–1980
	(1)	(2)	(3)	(4)	(5)	(6)
Fertility$_{t-1}$				0.615**	0.812***	0.672***
				(0.286)	(0.201)	(0.121)
School cohort	−0.102**	−0.0727***	−0.162***	−0.489**	−0.0866	−0.293***
	(0.0450)	(0.0189)	(0.0387)	(0.198)	(0.100)	(0.0902)
log income *p.w.*	−0.274**	−0.368***	−0.622***	0.434	−0.169	0.258
	(0.117)	(0.0709)	(0.0974)	(0.351)	(0.329)	(0.170)
AR(1) *p*-value				0.155	0.200	0.000
AR(2) *p*-value	-	-		0.414	0.170	0.017
Hansen *J* *p*-value	-	-		0.054	0.128	0.217
Time effects	Yes	Yes	Yes	Yes	Yes	Yes
States	48	48	48	48	48	48
Observations	345	288	564	345	288	564

Notes: Columns (1)–(3) report the FE estimates. Columns (4)–(6) report the System GMM estimates. The unit of observation is the US state over the period 1850–1980. All regressions include the mortality variables: Mortality 0–15 and Mortality 15–60 (not reported). Columns (4)–(6) use lags 3–7 as instruments. All RHS variables in the GMM specifications are treated as endogenous. Constants are not reported. Standard errors clustered by state in parentheses.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

TABLE 8. Functional form specifications

	Dependent variable					
	log Fertility				Fertility	
	Fixed effects		System GMM		Fixed effects	System GMM
	(1)	(2)	(3)	(4)	(5)	(6)
log Fertility$_{t-1}$			0.759***	0.679***		
			(0.105)	(0.0864)		
School cohort	−0.0447***		−0.0642***			
	(0.00726)		(0.0191)			
log School cohort		−0.171***		−0.674***	−0.789***	−2.610***
		(0.0568)		(0.217)	(0.172)	(0.722)
log income *p.w.*	−0.143***	−0.147***	0.0871*	−0.00876	−0.563***	0.152
	(0.0241)	(0.0270)	(0.0468)	(0.0564)	(0.0914)	(0.156)
Fertility$_{t-1}$						0.626***
						(0.0837)
AR(1) *p*-value	–	–	0.000	0.001	–	0.014
AR(2) *p*-value	–	–	0.230	0.618	–	0.578
Hansen *J* *p*-value	–	–	0.231	0.123	–	0.100
Time effects	Yes	Yes	Yes	Yes	Yes	Yes
States	48	48	48	48	48	48
Observations	633	633	633	633	663	633

Notes: Columns (1), (2), and (5) report the FE estimates. Columns (3)–(5) report the System GMM estimates. The unit of observation is the US state over the period 1850–1980. All regressions include the two mortality variables (not reported). Columns (3), (4), and (6) use lags 3–7 as instruments. All RHS variables are treated as endogenous in the GMM specifications. Constants are not reported. Standard errors clustered by state in parentheses.
$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

TABLE 9. Nonmonotonic income effects

	Dependent variable is fertility			
	Fixed effects		System GMM	
	(1)	(2)	(3)	(4)
Fertility$_{t-1}$			0.633***	0.713***
			(0.135)	(0.103)
School cohort	−0.164***	−0.165***	−0.297***	−0.258
	(0.0319)	(0.0322)	(0.0902)	(0.214)
log income *p.w.*	−4.706***	10.89	−1.464	−85.33
	(1.242)	(13.03)	(2.563)	(61.90)
(log income *p.w.*)^2	0.227***	−1.431	0.0998	8.741
	(0.0667)	(1.380)	(0.141)	(6.416)
(log income *p.w.*)^3		0.0584		−0.296
		(0.0486)		(0.221)
AR(1) *p*-value			0.000	0.000
AR(2) *p*-value			0.454	0.748
Hansen *J* *p*-value			0.173	0.135
Time fixed effects	Yes	Yes	Yes	Yes
States	633	633	633	633
Observations	48	48	48	48

Notes: Columns (1) and (2) report the FE estimates. Columns (3) and (4) report the System GMM estimates. The unit of observation is the US state over the period 1850–1980. All regressions include the mortality variables: Mortality 0–15 and Mortality 15–60 (not reported). Columns (3) and (4) use lags 3–7 as instruments. All RHS variables are treated as endogenous in the GMM specifications. Constants are not reported. Standard errors clustered by state in parentheses.
$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

not within the sample. Thus, the income–fertility relation is negative over the considered period, which is in line with our baseline FE results. In column (2), we add the cube of log GDP per worker. The FE estimates now suggest that the association between income and fertility is first positive, then negative, and finally positive, although the estimates are statistically insignificant. Nonetheless, when we use these point estimates, calculations of the turnings points indicate that the relationship between income and fertility in the United States between 1850 and 1980 was flat or slightly negative as can be seen in Figure 6. In the System GMM specification, reported in column (4), the signs of the estimates are the opposite of those in column (2) and not statistically different from zero.[29] Figure 7 also reveals that the turning points are out of the sample range for log income *p.w.*, and therefore the income–fertility relation is basically flat (or slightly positive). As observed in columns (1)–(4), the relation between schooling and fertility remains negative and statistically significant in all the specifications but the one reported in the final column, as this specification is estimated less precisely (i.e., the coefficient magnitude stays the same; however, the estimated standard error inflates a little).

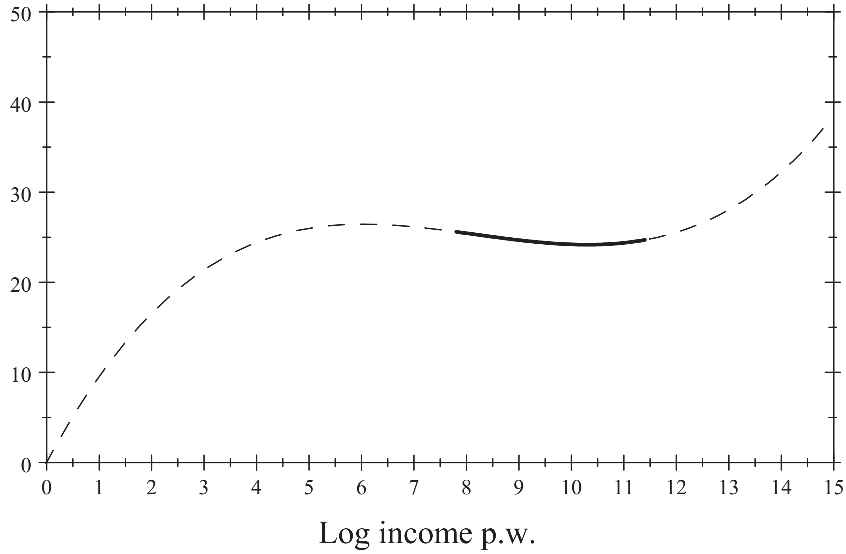

FIGURE 6. The partial fertility–income relationship (FE). The figure shows the estimated partial relation between fertility and log income p.w., reported in column (2) in Table 9. The solid line indicates the sample range for log income *p.w.*

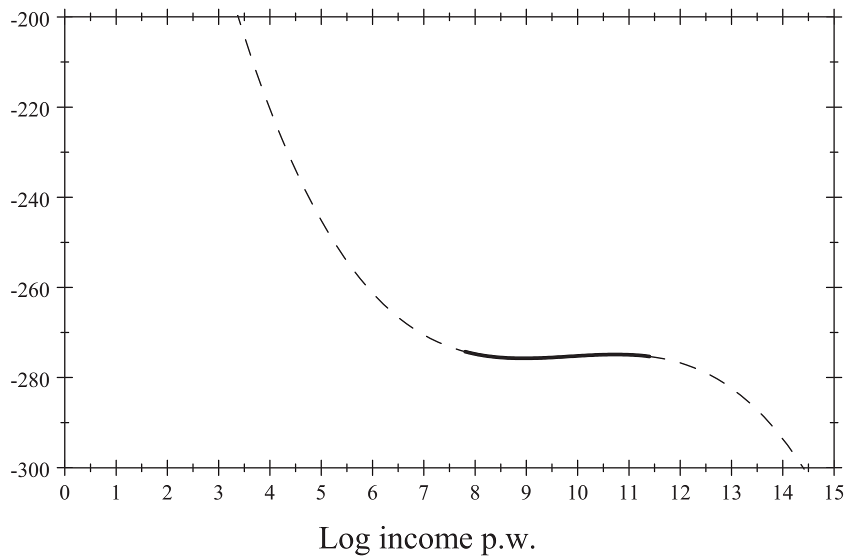

FIGURE 7. The partial fertility–income relationship (GMM). The figure shows the estimated partial relation between fertility and log income *p.w.*, reported in column (4) in Table 9. The solid line indicates the sample range for log income *p.w.*

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TABLE 10. Public school expenditures

	Dependent variable is fertility							
	Fixed effects				System GMM			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Fertility$_{t-1}$					0.771***	0.577***	0.758***	0.781***
					(0.0717)	(0.110)	(0.120)	(0.109)
log school expenditures per pupil	−0.442***	−0.362***	−0.150*	−0.179**	−0.0161	0.176	−0.0150	−0.0801
	(0.0911)	(0.0880)	(0.0793)	(0.0767)	(0.0867)	(0.118)	(0.132)	(0.165)
School cohort		−0.178***	−0.178***	−0.144***		−0.207**	−0.215***	−0.205**
		(0.0431)	(0.0371)	(0.0403)		(0.0964)	(0.0791)	(0.0992)
log income *p.w.*			−0.609***	−0.501***			0.303	0.402
			(0.112)	(0.0988)			(0.305)	(0.322)
AR(1) *p*-value								
AR(2) *p*-value								
Hansen *J* *p*-value								
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
States	563	563	563	563	563	563	563	563
Number of state	48	48	48	48	48	48	48	48

Notes: Columns (1)–(4) report the FE estimates. Columns (5)–(8) report the System GMM estimates. The unit of observation is the US state over the period 1870–1980 (due to the variable log school expenditures per pupil). All regressions include the mortality variables: Mortality 0–15 and Mortality 15–60 (not reported). Columns (5)–(8) use lags 3–7 as instruments. All RHS variables are treated as endogenous in the GMM specifications. Constants are not reported. Standard errors clustered by state in parentheses.

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

Overall, the findings presented in Table 9 indicate that the relation between income and fertility is rather flat, which might explain the mixed evidence from the FE and System GMM specifications regarding the income–fertility association.

Finally, in Table 10 we show results from studying the role of public school expenditure in the fertility decline so as to investigate whether our results are confounded by this variable. Columns (1)–(4) report FE estimates: The first specification only includes log school expenditures per pupil, whereafter we stepwise include schooling years, log income *p.w.*, and the mortality measures (not reported). Columns (5)–(8) report the System GMM estimates, but are otherwise structured in a similar way. The FE specifications reveal a negative and statistically significant relation between school expenditure and fertility, although the numerical magnitude of the estimates reduces significantly once the baseline controls are included. In the System GMM approach, however, the point estimates are in three out of four specifications very close to zero and always statistically highly insignificant [columns (5)–(8)]. Therefore, in this sense we find no robust relation between school expenditures and fertility. Importantly, however, our baseline conclusions about the relations between fertility and schooling and fertility and income remain unchanged when controlling for log school expenditures per pupil.

6. CONCLUSION

This research studies the relationships between economic development and the fertility transition in the United States. Allowing for mean-reverting dynamics and persistent effects in fertility, which may be endogenous to income and schooling, this paper suggests that rising levels of schooling account for about 60% of the fertility decline over the past two centuries. In addition, our analysis shows no robust relation between fertility and income. Our findings are consistent with both a trade-off between schooling per child and the number of children and a negative relation between the level of schooling of parents and their fertility.

Future research may look for exogenous variation in the returns to schooling across time and states to study the causal effect of schooling on fertility.[30] However, relying solely on exogenous variation in schooling makes it difficult to compare the effect of schooling with that of income as such a comparison also requires exogenous variation in income. For this reason, we believe that our study makes an important contribution in evaluating the relative importance of rising levels of income and schooling for the observed interrelation between the transition from low to high stages of economic development and the transition from high to low fertility.

NOTES

1. For an overview of unified growth theories, see Galor (2011).
2. Becker et al. (2010), studying fertility in Prussia in the 19th century, use instrumental variables to establish a causal negative effect of schooling on fertility and vice versa. See also Klemp and Weisdorf (2012), reporting evidence of a trade-off between number of children and education using data from

historical England. Becker et al. (2013) find a negative effect of women's education on fertility in county data from Prussia in 1816, 1849, and 1867.

3. See also Angeles (2010), who focuses on the relation between mortality and fertility, but also finds little support in his analysis for explaining fertility decline on the basis of rising levels of income.

4. Jones and Tertilt focus only on bivariate relationships, both between income and fertility and between education of fathers and fertility.

5. See also Haines and Hacker (2006), who study causes of fertility in the period 1800–1860. They investigate a number of reasons for the possible early decline in fertility in antebellum United States. They provide evidence based on county level data consistent with a role for income without controlling for schooling, and individual-level analysis which shows that the mother's literacy is negatively correlated with fertility, but without controlling for income directly.

6. As indicated in Footnote 5, the US fertility decline may have started before the period studied here begins, see Hacker (2003). However, evidence by Hacker (2003, p. 605) suggests that "unlike previous estimates that showed a long-term decline in overall fertility beginning at or before the turn of the nineteenth century, the new estimates suggest that US fertility did not begin its secular decline until circa 1840."

7. Since we have data for the 48 contiguous states from 1840, we use more variation from the 19th century than most of the existing studies.

8. See Spolaore and Wacziarg (2014) for the importance of cultural diffusion for the fertility transition. For theoretical work on this topic, see Baudin (2012).

9. For example, Herzer et al. (2012) and Murtin (2013) use the crude birth rate, which is the number of births per 1,000 individuals. Since this measure of fertility is affected by the number of fertile people in the population, it provides a less precise description of changes in individual behavior over time than the cohort measure we use. Angeles (2010) uses the total fertility rate and net reproduction rate which assume that a new baby girl has the same age specific fertility profile as the current population. All of these period-specific measures are averages of fertility behavior of a cross section of cohorts and they adjust more slowly than the actual change in behavior across cohorts.

10. Moreover, Roodman (2009) demonstrates an econometric challenge with GMM panel estimators known as the "too-many-instruments" problem. By using techniques that reduce the number of instruments we are able to address this problem.

11. Becker and Tomes (1976) provide an example of why this may be the case based on a specific functional form of the production function for quality. For a detailed presentation of theories explaining a negative relationship between income and fertility, see Jones et al. (2011).

12. The interrelationship between education and fertility due to the quantity–quality trade-off is also present in later contributions within literature, such as Galor and Moav (2002), de la Croix and Doepke (2003), Doepke (2004), Rahman (2013), and Cervellati and Sunde (2015). Caldwell (1980) describes potential channels through which children's education might affect fertility.

13. For more studies on relationship between fertility and mortality, see, e.g., Strulik (2004), Strulik (2008), and Strulik and Weisdorf (2014).

14. In the empirical analysis, we also control for the effects of adult mortality. This is motivated by the theoretical literature on the effect of decreasing adult mortality on fertility. See, e.g., Lagerlöf (2003), Elgin (2012), de la croix and Licandro (2013), Cervellati and Sunde (2015), and Yasui (2016).

15. To the extent that industrialization also captures higher demand for human capital, schooling may also affect fertility by narrowing the gender wage gap. Due to data limitations, we cannot test the importance of the gender wage gap. For evidence in line with this hypothesis, see Schultz (1985).

16. Barro and Lee (1993) used this methodology to construct their initial estimates of years of schooling across countries.

17. They show that the national estimate for 1900 lies in between other known estimates. Other evidence suggests that in some cases the data may underestimate schooling. Census data for Iowa yield an average of about 6.9 years for the population for 1914. For the population aged 10 or above, the average is 8.43 years. Turner et al.'s data say 6.7 years in 1914, suggesting that the data underestimate years of schooling of the workforce, but better tracks the total population. If the measurement is systematic upwards, this variation will be picked up by state FE.

18. This address concerns about the persistence of, for example, cultural factors or social norms.

19. Roodman's manual for the STATA module xtabond2 suggests that the use of orthogonal deviations may be preferable when some panel units have missing observations for some years. We have implemented this alternative and obtained similar results.

20. Roodman (2009) implemented this solution in the aforementioned STATA module for estimating dynamic panel models using GMM. The module manual states that "principal components analysis is run on the correlation, not covariance, matrix of the 'GMM-style' instruments. By default xtabond2 will select all components with eigenvalues of at least 1, and will select more if necessary to guarantee that instruments are at least as numerous as regressors, favoring those with largest eigenvalues."

21. The principal components explain about 89% of the variation in the original instruments and reduce the number of instruments from 126 to 41 in the case of our full model in the baseline sample.

22. This number is calculated using state averages [i.e., $(14.0 - 4.4) \times (-0.17)/(2.02 - 4.63)$].

23. For example Moav (2005) argues that better educated parents have a comparative advantage in the production of child quality. This implies that better educated parents have fewer children and provide more schooling to each child. Although this theory suggests a role for parental schooling in the fertility transition, parental schooling affects fertility through the quantity–quality trade-off and the effect would therefore be captured by our measure of cohort schooling.

24. See also Bloom et al. (2009), who consider the effect of fertility on female labor supply.

25. When we do not include the cohort-based schooling measure, the Hansen J-statistic fails to reject the null at the 5% level but not at the 10% level, see column (3). This suggests weak evidence of misspecification.

26. The point estimate for the coefficient on schooling is close to our baseline results [compare columns (3) and (6) with Tables 2 and 3]. For the coefficient on income, the FE estimates are largely unchanged, whereas the System GMM estimates are slightly larger but still insignificant.

27. We also obtain the same results using income in levels instead of logs.

28. For evidence on the Malthusian theory, see, e.g., Ashraf and Galor (2011).

29. The GMM specifications in Murtin (2013) give rise to the same conclusion.

30. Reduced form evidence on this link can be found in, for example, Andersen et al. (2016), Aaronson et al. (2014), and Bleakley (2007).

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APPENDIX

A.1. DATA SOURCES

Descriptive statistics of the variables used in the estimations are reported in Table 1, and their precise definition and sources are as follows:

1. Fertility is measured as children ever born to women between the ages of 35 and 44. The data were compiled by Murphy et al. (2008). The period data from 1880 to 1890 are from published volumes of the census of the population. For the census years 1850–1880, they use information on the number of children under the age of 1 and between ages 1 and 5. These censuses include information on the number of deaths by age category and by state. This allows them to construct children ever born for women between the ages of 15 and 44. We use fertility 20 years into the future, so we use cohort fertility from 1870 and children ever born for the previous years.
2. School cohort measures child school attainment of a child born around year t. These data are from Murphy et al. (2008), who construct years of schooling for a child who is 6 years old at a given time. This measure is based on observed average enrollment rates. The methodology is a perpetual inventory method and was also employed by Barro and Lee (1993) and Turner et al. (2007) as well as Morrison and Murtin (2009). The underlying data come from census reports and other official statistics.
3. School *p.w.* is the average years of schooling of the workforce, estimated using the perpetual inventory method. Source: Turner et al. (2006).
4. log income p.w. is real state output (until 1920) or income (from 1929) per worker in 2,000 dollars. The data are from Turner et al. (2006). The data for 1840, 1880, 1900, and 1919–1921 are originally from Easterlin (1957, 1960). The sources for the construction of the GDP per worker data for 1840 are described in Easterlin (1960, Appendix B) and for 1880, 1900, 1919–1921 are described in Easterlin (1957, pp. 708–740). The information comes from census data and other published data. The data for 1850, 1860, 1870, 1890, and 1900 were constructed by Turner et al. (2006) using census data on agricultural production and manufacturing value added and information on mining value added. Some of these are not available in the years 1850 and 1860, and they therefore use other observable variables, such as the agricultural labor force and manufacturing labor force to impute the missing variables.

5. Probabilities of dying in the age intervals 0–15 and 15–60 for white individuals are from Murphy et al. (2008). These data come from official death registrations and the census. When death registration data are unavailable they have relied on answers to survey questions in the census data.
6. log school expenditures per pupil is K-12 spending (current expenses and "outlays, new buildings, sites, and new equipment" for public day schools) per pupil. The data are taken from the Statistical Abstracts of the United States of America from 1920 and the report of the commissioner for education for 1913, which gives data back to 1870.

A.2. THEORETICAL MODEL

Consider a model of fertility choice with notation as described in the paper. Lifetime utility is given by

$$U = v(c) + f(\pi b) + g(a, s),$$

and the lifetime budget constraint is

$$c + b\pi y[\tau + s] \leq y.$$

The utility maximizing levels of b and s fulfill the following first-order conditions, respectively:

$$U_b = -v_c y\pi[\tau + \sigma s] + \pi f_b = 0,$$

$$U_s = -v_c y\pi b\sigma + g_s = 0.$$

Suppose that the first-order conditions imply that $s = s(a(t), y(t), \pi(t))$ with $s_a > 0$, $s_y > 0$, and $s_\pi > 0$. This implies that $U_b(b)$. Since $U_{bb}(b) < 0$ for all interior values of b, the utility maximizing level of b, if it exists, is uniquely determined. Differentiating $U_b(b(t)) = 0$ implicitly with respect to t yields

$$\frac{db}{dt} = \left[f_y + f_s s_y\right]\frac{dy}{dt} + f_s s_a \frac{da}{dt} + [f_\pi + f_s s_\pi]\frac{d\pi}{dt},$$

where $f_y \equiv \frac{-\pi[\tau+\sigma s][v_{cc}c - v_c]}{-\left[f_{bb}+[y\pi[\tau+\sigma s]]^2 v_{cc}\right]} \lesseqgtr 0$, $f_s \equiv \frac{y\pi[\tau+\sigma s]v_{cc} - v_c}{-\left[f_{bb}+[y\pi[\tau+\sigma s]]^2 v_{cc}\right]} y\pi b\sigma < 0$, and $f_\pi \equiv \frac{\left[v_{cc}\left[\pi b[y[\tau+\sigma s]]^2\right]+f_{bb}\pi b\right]}{-\left[f_{bb}+[y\pi[\tau+\sigma s]]^2 v_{cc}\right]} < 0$.