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The trade-off between fertility and education: evidence from before the demographic transition

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Abstract The trade-off between child quantity and quality is a crucial ingredient of unified growth models that explain the transition from Malthusian stagnation to modern growth. We present first evidence that such a trade-off indeed existed already in the nineteenth century, exploiting a unique census-based dataset of 334 Prussian counties in 1849. Furthermore, we find that causation between fertility and education runs both ways, based on separate instrumental-variable models that instrument fertility by sex ratios and education by landownership inequality and distance to Wittenberg. Education in 1849 also predicts the fertility transition in 1880–1905.

Keywords Schooling · Fertility transition · Unified growth theory · Nineteenth-century Prussia

JEL Classification N33 · O15 · I20 · J13

1 Introduction

The trade-off between the number of children and the human capital invested in each child—i.e., between fertility and education—is a crucial ingredient of unified growth theory, which models the transition from Malthusian stagnation to sustained growth in a unified framework (see Galor 2005a, 2010 for seminal reviews). In most of these models, the new technologies that emerged during the process of industrialization increased the demand for education, which in turn triggered the demographic transition at the end of the nineteenth century

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(Galor 2005b). But due to a dearth of historical data, empirical evidence that the so-called child quantity-quality (Q–Q) trade-off indeed existed before the demographic transition is missing so far. As Guinnane (2008, p. 30) points out, "we lack detailed empirical studies of the relationship between…education and the demand for children in the relevant period." Most existing evidence supporting the Q–Q trade-off is based on modern data, and the earliest analysis we are aware of stems from 1910 (Bleakley and Lange 2009), after the main phase of the fertility transition. However, the trade-off may well have emerged only during or after the demographic transition. Historians often question the thought that people in the pre-modern age had a strong tendency to follow such sophisticated "economic" thoughts as those underlying the Q–Q trade-off. What is more, it is well conceivable that the transition itself, which started at the end of the nineteenth century and brought fundamental changes to demographics, *created* the very trade-off that unified growth models take as an assumption for the transition to occur.

This paper aims to estimate the existence and magnitude of the child Q–Q trade-off in the middle of the nineteenth century, much earlier than existing studies. Based on a unique micro-regional dataset of more than 330 Prussian counties stemming from full Population and Education Censuses conducted in 1849 by the Prussian Statistical Office, we investigate the link between education and fertility behavior for a period before the demographic transition.[1] We find that a significant Q–Q trade-off indeed existed in Prussia before the main phase of the transition, in line with unified growth models that regard the trade-off as a fundamental one (Galor and Weil 2000). Our results also show that differences in education in 1849 predict cross-county variation in the fertility transition at the turn from the nineteenth to the twentieth century, suggesting that the Q–Q trade-off becomes apparent during the fertility transition. In addition, we address the issue of reverse causality between fertility and education and show that causation appears to run both ways.

Prussia constitutes an interesting case not only because of its unique data situation, but particularly because as early as 1763, Frederick the Great enacted the General Elementary School Regulation, which contained an explicit general invitation to attend school that is often interpreted as quasi-compulsory primary education. Yet, this law was not strictly enforced, as evidenced by the fact that full enrolment in primary school was achieved only towards the end of the nineteenth century. Therefore, Prussian families were able to decide whether or not to send their children to school. In fact, across Prussian counties, primary-school enrolment rates still varied from 33 to 99% in 1849. As we will show below, a significant part of this substantial variation in education can be traced back to variation in landownership inequality and in religious denomination.

Our empirical analysis starts by showing the existence of the trade-off in 1849 using ordinary least squares regressions. We then investigate both directions of causality employing an instrumental-variable approach. To identify the exogenous effect of education on fertility, we employ variation in education that stems from cross-county differences in landownership inequality and distance to Wittenberg. As Galor et al. (2009) show, landownership inequality depresses human-capital accumulation because large landowners have little interest in public schooling due to low land-human capital complementarity. Distance to Wittenberg provides another source of exogenous variation in education across Prussia because it predicts Protestants' urge for literacy to read the Bible (Becker and Woessmann 2009). The

[1] In the framework of the European Fertility Project (EFP), Knodel (1974) studied the demographic transition in Germany. Using a definition of a 10% decline of the index of marital fertility rate, he dated the onset of the fertility transition in Germany to 1895. Guinnane et al. (1994) discuss the limits of the EFP measures used to date the initial stages of the fertility transition. Other evidence from a village study implies that, at least in some parts of Germany, a fertility transition was underway earlier than suggested by the EFP (Knodel 1988).

identifying assumption is that neither landownership inequality nor distance to Wittenberg is directly related to fertility. Given one of these assumptions, we can test the second one by over-identification restrictions.

To identify the exogenous effect of fertility on education, we employ variation in fertility that stems from cross-county differences in sex ratios (the relative number of men and women). Sex ratios in the adult population affect the tightness of the marriage market (Angrist 2002; Abramitzky et al. 2010) and are thus associated with average fertility rates. The identifying assumption is that the adult sex ratio in 1849 is exogenously determined by differential birth and death rates, but is otherwise unrelated to education. To exclude the possibility that migration may violate this exclusion restriction, we control for temporary migration and also use the sex ratio of children aged 0–7 in 1816 as an additional instrument. The latter is unaffected by migration but is still related to fertility in 1849 as the cohort observed in 1816 is aged 33–40 in 1849.

We find evidence of a significant interaction between quantity and quality of children in 1849 Prussia. The results are robust to a rich set of controls for the level of economic development and to the incidence of mortality. Finally, we study the long-run impact of educational differences for the demographic transition using census data from the turn of the nineteenth to the twentieth century. Our results suggest that the fertility transition in Prussia at the turn of the century was more pronounced in those counties with higher educational enrolment in 1849.

Our findings on the negative relationship between education and fertility in the nineteenth century are consistent with unified growth models in which the fundamental trade-off between the quantity and quality of children is present also in the phases preceding modern economic growth (see Galor and Weil 2000; Galor and Moav 2002). In these models, the transition from stagnation to growth is ultimately brought about by a gradual change in the composition of the population. A gradual increase in the fraction of individuals with a higher valuation for offspring's quality, who gain an evolutionary advantage, raises overall investment in human capital. Galor and Moav (2002, p. 1144) point out that careful empirical analyses of the relevance of child quality in the pre-industrial revolution era is not available so far, due to the lack of corresponding historical data. Our analysis aims to fill this gap.

The remainder of the paper is structured as follows. Section 2 presents a simple theoretical framework derived from unified growth theory and places our contribution in the existing literature. Section 3 describes our new dataset and provides descriptive statistics. Section 4 develops our estimation strategy. Section 5 presents results on the child Q–Q trade-off in 1849. Section 6 reports instrumental-variable specifications that probe both directions of causality on the trade-off between education and fertility. Section 7 documents the long-run association between investments in education in 1849 and the fertility transition in 1880–1905. Section 8 summarizes and concludes.

2 Theoretical background and related literature

The industrial revolution was characterized by major developments in the economic and demographic environment. Between late eighteenth century and early twentieth century, most European countries witnessed the transition from stagnation to growth. Furthermore, in later phases of the industrialization, while income increased, fertility decreased and gave rise to the demographic transition. Whereas much previous research saw these transitions as discontinuous changes, recently, much scholarly attention has been devoted to explain the transition from stagnation to growth within a unified growth theory.[2] The main contribution of unified growth theory is to combine the elements of a typical Malthusian economy—displaying decreasing returns to labor, high fertility rates, and a positive correlation between income and population size—with the factors that characterize developed economies with high income and low fertility. In establishing the micro-foundations of the transition between these two phases, scholars were faced with the problem of explaining how the Malthusian positive link between income and population, for some countries, turned negative towards the end of the nineteenth century. In the majority of unified growth models, investments in human capital provide the explanation.[3]

In their seminal contributions, Galor and Weil (1999, 2000) and Galor and Moav (2002) stress the importance of human capital in explaining the transition from stagnation to growth. Their models describe the child Q–Q trade-off as one of the fundamental trade-offs that exist in nature. Using their framework, we can characterize the child Q–Q trade-off by a simple model in the spirit of Galor (2005a). Consider a household that draws utility u from consumption c, the number of (surviving) children n, and from the quality (human capital) h of those surviving children. Suppose parents maximize a log-linear utility function as in Galor and Moav (2002):

$$u = (1-\gamma)\ln c + \gamma\,(\ln n + \beta \ln h) \quad \text{where} \quad 0 < \gamma < 1 \quad \text{and} \quad \beta < 1 \tag{1}$$

For each child, parents spend a fraction τ^q of their time budget (and an according share of their potential income) on raising children. In addition, a fraction τ^e of parents' time is required for each unit of education e of each child. Raising one child of education (quality) e thus costs $\tau^q + \tau^e e$ units of time. Assuming that household potential income with full-time work is y, the household faces the following budget constraint:

$$yn(\tau^q + \tau^e e) + c \leq y \tag{2}$$

Optimization yields

$$n = \gamma/(\tau^q + \tau^e e) \tag{3}$$

and

$$e = e\left(\beta, \tau^q, \tau^e\right) \quad \text{where} \quad e'(\beta) > 0. \tag{4}$$

The household spends a constant fraction $(1-\gamma)$ of income on consumption: $c = (1-\gamma)\,y$. The remaining fraction γ is spent on children. Equation 3 shows a trade-off between the number n of children and their quality (education e).[4] That trade-off is driven by the cost of raising children τ^q, the cost of education τ^e, and by the preference for education β. We

2 See Galor and Weil (1999, 2000), Galor and Moav (2002), Hansen and Prescott (2002), Lucas (2002, Ch. 5), Doepke (2004), Galor (2005b), and Clark (2007) for examples of leading contributions.

3 Cervellati and Sunde (2005) provide a unified growth model where human capital is crucial not through its association with fertility, but through its association with life expectancy. For an example of a unified growth model in which human capital does not play a role, see Strulik and Weisdorf (2008).

4 In the economics literature, the Q–Q trade-off has originally been modeled by Becker (1960), who noted that the Malthusian model of a positive link between income and fertility did not take into account the role of child quality. He argued that the elasticity of child quantity with respect to income is usually small compared to the elasticity of child quality with respect to income, implying that with rising income emphasis shifts towards child quality. However, as noted by Galor (2010, Ch. 4), this preference-based model hinges on the assumption that parents' preferences "reflect an (unexplained) innate bias against child quantity beyond a certain level of income." See also Becker and Lewis (1973), Becker and Tomes (1976), Willis (1973), and Moav (2005) for extensions of the micro-foundations of the Q–Q model.

will draw on this maximization framework in deriving our empirical identification in Sect. 4 below.

Econometric studies of the Q–Q trade-off nearly uniformly use modern data (see Schultz 2008 for a detailed critical review), whereas we are interested in testing the Q–Q trade-off in a pre-demographic transition economy. Some studies express the Q–Q trade-off as a model where child quality is a function of child quantity (e.g., Hanushek's 1992 study of the dependence of test scores on family size), while others express the function the other way round (e.g., Doepke's (2004) macro simulation of the fertility decline). For econometric identification, it is important to note that both quantity and quality of children are endogenous variables in the Q–Q model, so that exogenous variations in fertility or education are required for causal identification of the Q–Q trade-off.

The first paper exploiting exogenous variations in fertility to identify the effect of child quantity on child quality is Rosenzweig and Wolpin (1980), who analyze household data from India. They use multiple births to measure exogenous increases in family size and show the resulting decrease in child quality.[5] Qian (2009) uses regional and time variation in China's one-child policy, as well as multiple births, to estimate the effects of family size on school enrolment in China. Perhaps surprisingly, her estimates suggest that relaxation of the one-child policy increased the enrolment rates of first-born children, a finding she explains by differential parental investment in first-born and later-born children. Along the same lines, Rosenzweig and Zhang (2009), re-examining China's one-child policy, cast doubt on studies using multiple births, in general. Furthermore, it is not clear a priori that an exogenous change in child quantity should have a bearing on the Q–Q trade-off: If a non-optimal choice of quantity is imposed on the household, it may not be optimal to deviate from the optimal investment in quality, but rather to make adjustments in other margins, such as adjusting intergenerational transfers (see Galor 2010).

The opposite direction of causation, from education to fertility, is analyzed as well. While a number of papers is concerned with the effect of a mother's education on her fertility (e.g., Breierova and Duflo 2004), few studies use micro data to analyze the effect of exogenous changes in (returns to) education of children on the number of children born. Bleakley and Lange (2009) exploit the introduction of a hookworm disease eradication policy in the American South which is argued to establish a shock to the price of quality because hookworm reduced the return to human capital investment, had a low case-fatality rate, and was hardly prevalent among adults. Hookworm eradication thus exogenously increased the returns to education and reduced the price of child quality. They show that the resulting increases in school attendance and literacy were accompanied by a significant fertility decline.

Bleakley and Lange (2009) is also the study going furthest back in time, to 1910, a period when the demographic transition was well under way. The Princeton European Fertility Project, which used nineteenth-century data to study the fertility transition (see Coale and Watkins 1986), largely ignored economic factors and did not address the child Q–Q trade-off.

Overall, most studies based on twentieth-century data find evidence consistent with a child Q–Q trade-off. We are not aware of micro-econometric studies of the Q–Q trade-off before the twentieth century.

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5 Angrist et al. (2005) and Black et al. (2005) also use multiple birth instruments.

3 Data

We address the child Q–Q trade-off in historical perspective using Prussian county data. In order to explore this relation, we analyze regional variation across more than 330 counties (*Kreise*).[6] Prussia was one of the largest European countries and, with a population of about 24.6 million, accounted for 60% of the population of the German Empire founded in 1871. Over the nineteenth century, the Prussian Statistical Office collected a remarkable amount of information on vital statistics (e.g., age distribution by gender, marital status, and religion). Demographers have found county-level data for nineteenth-century Prussia to be a unique source of highest-quality data for analyses at a disaggregated level (see Galloway et al. 1994).

Our main data sources are the Population, Education, and Occupation Censuses conducted in 1849. The Population Census contains information on age structure that allows us to infer fertility and mortality. The Education Census contains detailed information on enrolment rates in public primary schools as well as on the number of schools. The Occupation Census contains the employment structure and reveals the stage of industrialization, i.e., employment in industry vs. agriculture.[7] These censuses allow us to construct a coherent database with a detailed amount of demographic and socio-economic information in 1849. To our knowledge, these data have not yet been used for micro-econometric analyses.

We also use data from the first Prussian Census in 1816 (see Becker and Woessmann 2008, 2010). The 1816 Census contains demographic and educational information that we use as instrumental variables to identify exogenous variation in our variables of interest in 1849.

To measure fertility behavior, we use the child–woman ratio, defined as the number of children aged 0–5 per woman of child-bearing age (15–45).[8] Rather than using births, the number of children aged 0–5 captures surviving children, which we consider the relevant measure in the joint fertility-education decision (see Galor 2005b). Below, we ascertain that results are robust to controls for mortality differences, as measured by life expectancy at different ages and by infant mortality.[9] We measure education as enrolment rates in public primary school in 1849, defined as the number of children attending school divided by the number of children aged 6–14. Limitations of this measure, which is the only one available in the first half of nineteenth century Prussia, relate to the facts that attendance at the census date might not be the same as year-round attendance and that school attendance does not directly capture the amount of human capital accumulated.[10]

The covariates used in the regression analysis are: (i) a proxy for the level of industrialization specified as the share of people employed in manufacturing, (ii) the share of people making their living of agriculture, (iii) the share of urban residents (defined as the share of

[6] County-level data aggregates individual-level behavior to a regional level, thus reflecting average behavior in a region. If individual-level behavior follows a linear model, estimates based on aggregate data are unbiased. Regional aggregates are potentially problematic if individual-level behavior deviates strongly from a linear model. Lacking individual-level data, we cannot further explore this issue.

[7] The census documentation implies that the occupation data refer to workers and not to other family member, as is sometimes the case in historical data. Unfortunately, they do not distinguish between male and female labor-force participation.

[8] The choice of the child–woman ratio is driven by data availability, as birth statistics at the county level are available only from 1862 onwards.

[9] Child survival may also be interpreted as a form of child quality, in which case our fertility measure "number of (surviving) children" would already incorporate a dimension of quality. We therefore control for child mortality. We cannot exclude the possibility that young children might be under-enumerated in this kind of census and that this under-enumeration is related to child quality, which could potentially bias downward (in absolute terms) estimates of the child Q–Q trade-off.

[10] The data do not allow us to distinguish education by birth order.

the population living in towns with city rights),[11] (iv) population density, (v) the share of married women, (vi) the share of Protestants, (vi) a geographical dummy variable for the provinces with the highest shares of Slavic-language people (*Poland*),[12] (vii) life expectancy at ages 0 and 5 (calculated from age-specific population and mortality data using the standard approach as depicted, e.g., in Jayachandran and Lleras-Muney 2009),[13] (viii) the number of public primary schools per 100 children in school age (6–14), and (ix) a measure for temporary male migration (constructed as the difference between the total number of married males and the total number of married females, divided by the total number of married females).

Table 1 provides descriptive statistics of the variables. The first thing to note is the high level of school enrolment. In 1849, already 80% of the children aged 6–14 were enrolled in primary school on average. For comparison, in 1816, 58% of the children aged 6–14 were enrolled in primary school. The enrolment data reveals that the quasi-compulsory primary-school attendance regulation enacted in Prussia in the eighteenth century was not strictly enforced. One reason for this was a lack of resources which were directly linked to the taxes levied on landlords (Cubberley 1920; Galor and Moav 2006), which may also explain the cross-county variation in education. The minimum and maximum enrolment rates are 3% and 95%, respectively, in 1816 and 33% and 99%, respectively, in 1849. A direct comparison with other European countries is not possible due to lack of statistics for the same years. Yet, Galor and Moav (2006, p. 89) report that in England "the proportion of children aged 5–14 in primary schools increased from 11% in 1855 to 25% in 1870". In France, the proportion of children aged 5–14 enrolled in primary school increased from 51 to 86% in the period 1850–1901 (Flora et al. 1983).

Longitudinal data on fertility clearly show that in the period under consideration, Prussia is not yet going through a demographic transition. Fertility and birth rates show only short-term fluctuations around a stable level over most of the nineteenth century and do not start to trend downwards before the 1880s (see Fig. 1 in Galloway et al. 1994). In fact, the child–woman ratio in 1849 (at 0.64) is very similar to that in 1816 (at 0.67). The small change over time is likely to be due to short-term fluctuations: Birth rates registered in the 2 years 1847 and 1848 are very low as a response to the shortfall in basic food supplies experienced in Europe in 1845–1847 (Berger and Spoerer 2001). The average birth rate is 43.3‰ in 1816 and 42.3‰ in 1849, whereas it is 35.9‰ in 1847–1848.

Figure 1 shows the geographical distribution of school enrolment rate and child–woman ratio in 1849. The relatively industrialized western area (Rhineland) shows high levels of enrolment rates. The agricultural East and West Prussia shows comparatively high levels of fertility.

In the final part of our analysis, we study the long-run impact of the accumulation of human capital in the mid-nineteenth century on the demographic transition at the turn to the twentieth century. Data from a number of Prussian Population Censuses in the late nineteenth and early twentieth century allow us to compute changes in crude birth rates between 1880

[11] A definition of urban population based on the definition of towns by authorities is quite common in historical statistics and thus in empirical applications that make use of them. While such a definition may not fully capture town sizes, it has the advantage of capturing what at the time were considered urban areas. Our results are robust to an alternative urbanization measure based on the 172 large and medium-sized towns defined by the Prussian Statistical Office in 1816.

[12] According to census data from 1890, these are the regions of East Prussia, West Prussia, Poznan, and Silesia. Results are qualitatively the same when using a dummy for counties with a share of more than 30% Slavic-language people based on these data, or a dummy for counties located in Poland today.

[13] Specifically, our life expectancy measure is calibrated on male mortality tables as they are more detailed in the Prussian data than female mortality tables.

Table 1 Summary statistics

	Mean	Std. dev.	Min	Max
Census 1849				
Child–woman ratio	0.64	0.08	0.35	0.84
School enrolment rate	0.80	0.12	0.33	0.99
Share in industry	0.03	0.03	0.01	0.32
Share in agriculture	0.43	0.17	0.00	0.85
Share urban	0.24	0.19	0.00	1.00
Population density (1000 people per km^2)	0.20	1.12	0.02	14.98
Share married women	0.70	0.06	0.43	0.85
Share Protestants	0.60	0.39	0.00	1.00
Poland	0.42	0.49	0.00	1.00
Life expectancy at age 0	35.48	7.53	12.19	49.48
Life expectancy at age 5	45.84	6.92	20.05	56.62
Schools per 100 children (6–14)	0.82	0.27	0.27	1.72
Temporary male migration	0.00	0.03	−0.04	0.52
Marital fertility rate	0.70	0.06	0.43	0.85
Landownership inequality	0.01	0.01	0.00	0.08
Distance to Wittenberg (in 100 km)	3.33	1.64	0.00	7.31
Sex ratio adults 15–45	0.99	0.08	0.82	1.39
Census 1816				
Child–woman ratio	0.67	0.12	0.38	1.72
School enrolment rate	0.58	0.20	0.03	0.95
Sex ratio children 0–7	1.01	0.06	0.48	1.13
Demographic data 1880–1905				
Crude birth rate (1880)	34.78	4.34	24.57	50.75
Crude birth rate 1880–1905 (% change)	−0.10	0.10	−0.46	0.20
Marital fertility rate (1890)	27.03	3.66	17.81	34.36
Marital fertility rate 1890–1905 (% change)	−0.08	0.10	−0.40	0.21
Net migration per 1000 inhabitants (1880)	−1.75	3.79	−32.22	2.37

Child–woman ratio is the number of children aged 0–5 over the number of women aged 15–45. School enrolment rate is the share of children aged 6–14 enrolled in public primary schools. Crude birth rate is the number of legitimate births per 1000 people. Marital fertility rate in the number of legitimate births per 100 married women in child-bearing age (15–49)
Source: Data for 334 counties from the Prussian Censuses 1816 and 1849 and demographic data for different years; see main text and Appendix for details

and 1905 and in marital fertility rates between 1890 and 1905, which we use to analyze the role played by investments in education for the demographic transition.[14] The crude birth rate is measured as the number of legitimate births per 1000 people per year. The marital fertility

[14] When studying the standard period of the demographic transition, we use the crude birth rate and the marital fertility rate to be consistent with the literature on the German demographic transition (Galloway et al. 1994, 1998; Brown and Guinnane 2002, 2007). Because of the bias introduced by infant mortality, the child–woman ratio, used in the analyses relating to 1849, is generally used as an index of fertility only when birth statistics are not available. However, using the child–woman ratio would not change qualitatively the results discussed in Sect. 7.

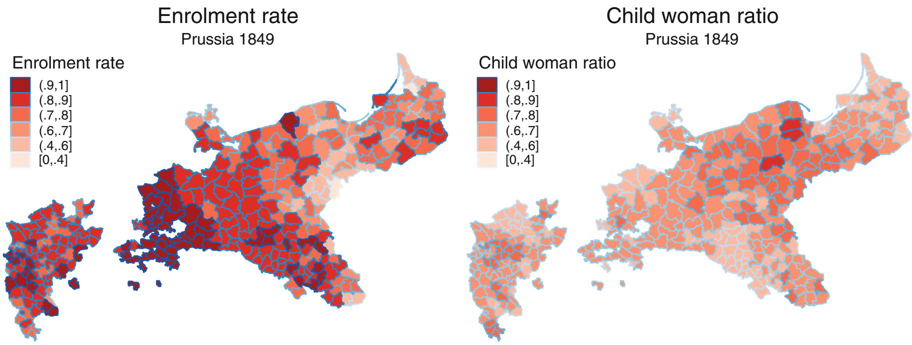

Fig. 1 The geographic distribution of primary school enrolment and fertility. *Source*: County-level data from the Prussian Census 1849; see main text and Appendix for details

rate is measured as the number of legitimate births per 100 married women aged 15–49. To test the robustness of our long-run estimates, we additionally control for net migration (per 1000 inhabitants) in 1880, constructed as the difference between immigrants and emigrants over the total population. The Appendix describes all our data sources in greater detail.

4 Empirical model

We aim to estimate the trade-off between fertility and education in the middle of the nineteenth century. We start by establishing the descriptive association between fertility and education in ordinary least squares (OLS) regressions that first express fertility as a function of education and then education as a function of fertility:

$$fertility_i = \alpha_1 \cdot education_i + \mathbf{X}_{i1}\delta_1 + e_{i1} \tag{5}$$

$$education_i = \alpha_2 \cdot fertility_i + \mathbf{X}_{i2}\delta_2 + e_{i2} \tag{6}$$

where *education* is the enrolment rate in public primary schools and *fertility* is the child–woman ratio for county i, both measured in 1849.[15] The **X**'s are vectors of control variables as described above. The α coefficients are our parameters of interest. The null hypothesis is that both α_1 and α_2 are equal to zero, i.e., parents essentially have a random number of children and only trade off spending on consumption versus spending on children, but do not trade number of children against education of children.

As already pointed out, fertility behavior and decisions about children's education are taken simultaneously, so that OLS estimates of Eqs. 5 and 6 are subject to endogeneity bias and do not necessarily depict causal effects. To the extent that there is a recursive relationship between fertility and education, OLS estimates of the α's will be biased downwards, towards larger negative estimates. The fertility-education association may be additionally affected by omitted variable bias. To the extent that third factors that affect both fertility and education, such as income, urbanization, and life expectancy, are not perfectly measured, so that the **X** vectors are not fully specified, the error terms of OLS models will be correlated with the

[15] Our analysis is cross-sectional, so we cannot directly deal with individual-level heterogeneity in the OLS regressions. In the IV analysis below, we try to partially address the issue by exploiting exogenous variation in the main variables of interest.

dependent variables. For example, a pure income effect would make such omitted factors positively related to both fertility and education, so that the omitted variable bias is likely to bias OLS estimates of the α's towards less negative estimates.

To address the issues of simultaneity and omitted variables, we employ instrumental-variable (IV) methods. The two equations are estimated separately because of the greater robustness of single-equation methods compared to a simultaneous-equation approach.[16] We closely tie our IV specifications to the theoretical model presented in Sect. 2 in that they explicitly consider several channels through which the Q–Q trade-off is triggered. In particular, our instruments are meant to capture cross-county differences in the price of education, in parents' preferences for education, and in the cost of raising children.

In the fertility Eq. 5, we use landownership inequality and distance to Wittenberg as instruments for primary education to identify the effect of education on fertility. The idea of using landownership inequality builds on Galor et al. (2009), who present a theoretical model where inequality in the distribution of landownership negatively affects the implementation of human-capital-promoting institutions. This is due to the fact that large landowners would not benefit from the accumulation of human capital given the low complementarity between land and human capital. Galor et al. (2009) also provide empirical evidence for the United States in the twentieth century which corroborates this prediction. For their empirical evidence, they use the share of land held by large landowners. Similarly, we have information on the number and size of estates[17] which allows us to construct an index of landownership inequality. The index is constructed as the ratio of the largest land holdings (greater than 600 *Morgen*) over the total number of land holdings. Results are perfectly robust to using the two largest categories (greater than 300 *Morgen*) in the numerator.[18] In terms of the model presented in Sect. 2, higher land concentration, due to the opposition of the landed nobility to education, corresponds to a rise in the cost of education τ^e. Such an increase is predicted to decrease investments in child quality $(\partial e\left(\beta, \tau^e, \tau^q\right)/\partial \tau^e < 0)$ and to increase the number of children $(\partial n/\partial \tau^e > 0)$ if the own-price elasticity of child quality is larger than one, a reasonable assumption for the time period we are looking at.

We also use distance to Wittenberg as a second instrument for primary education. Becker and Woessmann (2009) observe that at the times of Martin Luther, Protestantism in Prussia had a tendency to spread in circles around Wittenberg, where Luther preached that every Christian should be able to read the Bible. They show that as a consequence, distance to Wittenberg gives rise to a decreasing prevalence of education in Prussia, and that Protestantism is unlikely to have had substantial economic effects besides its indirect effect through education. In terms of the above model, the rise in the preference for education, due to proximity to Wittenberg, increases investment in child quality $(\partial e\left(\beta, \tau^e, \tau^q\right)/\partial \beta > 0)$ and is thus predicted to decrease fertility $(\partial n/\partial \beta < 0)$. We thus use distance to Wittenberg as an additional source to identify exogenous variation in Prussian enrolment rates.

[16] In this framework, a simultaneous-equation approach is not recommended as the autonomy requirement (Wooldridge 2002, pp. 209–210) is not met and the two equations may not be fully specified. However, we estimated simultaneous-equation models and results are qualitatively identical to those reported here.

[17] The categories are: less than 5 Morgen, 5–30 Morgen, 30–300 Morgen, 300–600 Morgen, and greater than 600 Morgen. 1 Morgen is ca. 0.25 hectars.

[18] We also have information on the extension of *agricultural* land. Yet, by using this as denominator of our index for landownership inequality, we lose information on 15 counties. However, results are robust to the use of this alternative denominator. Landownership inequality data were missing for two counties in 1849. We imputed the data based on landownership inequality data available in 1858. Given the high correlation of 0.96 of the 1858 data with the 1849 data, we regressed the 1849 data on the 1858 data and predicted the two missing 1849 values.

The exclusion restrictions of the two instruments are that landownership inequality and distance to Wittenberg are not directly related to fertility. Becker and Woessmann (2009) show that distance to Wittenberg is orthogonal to several measures of pre-Lutheran economic and educational development, which suggests that it is unrelated to possible important pre-existing correlates of fertility. In terms of landownership inequality, Bengtsson and Dribe (2006) show that total marital fertility, as well as age-specific marital fertility, are very similar across socioeconomic groups with different landownership in pre-transition Sweden.[19] To account for the fact that inequality in landownership is more prevalent in the agrarian East of Prussia, we can show that a strong correlation between landownership inequality and school enrolment holds when controlling for a county's latitude and longitude.[20] As long as the exclusion restrictions are accepted for one of the two instruments, over-identification tests allow us to test the validity of the second instrument. We will see below that the estimates of our parameter of interest are remarkably stable irrespective of the instrument used.

In the education Eq. 6, we use the adult sex ratio in 1849 as instrument for fertility in 1849. The adult sex ratio constitutes a measure of marriage market tightness affecting marriage rates and fertility (Angrist 2002; Abramitzky et al. 2010). It is defined as the number of males aged 15–45 over the number of females aged 15–45. A lower sex ratio (less men than women) establishes a constraint on the number of children that a typical household may have, e.g., because it leads to later marriage or decreases the marriage rate of women, pushing fertility rates down. In terms of the theoretical model above, since the budget share devoted to child-rearing γ is constant, the decrease in the supply of children is predicted to lead to higher child quality. Econometrically, the identifying assumption for this instrument is that the sex ratio is exogenously determined by differential birth and death rates and that it affects fertility behavior only through its influence on the probability of finding a mate, but is otherwise unrelated to education.

Because migration might affect the adult sex ratio in a way that might be correlated with education, it constitutes a potential problem for this identification. We directly address this issue by introducing a proxy for migration. In addition, as an alternative instrument we use the sex ratio of children aged 0–7 in 1816 which is not affected by migration. Yet, this instrument is correlated with our fertility measure in 1849, being a good predictor of marriage market tightness in 1849 (referring to the cohort aged 33–40 then), and has no obvious other link with school enrolment in 1849.[21]

5 Evidence on the quantity-quality trade-off in 1849

To document the existence of a trade-off between education and fertility before the demographic transition, Table 2 presents OLS estimates of Eq. 5. The dependent variable is the child–woman ratio in 1849. The bivariate regression (column 1) shows that, indeed,

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[19] This argument does not completely rule out that, in our data, landownership inequality might be directly related to fertility. As with any instrumental-variable strategy, the exclusion restriction is ultimately untestable. However, the stability of the IV results with different instruments and the results from the over-identification tests are favorable signs (see below).

[20] Estimates not presented here but available upon request.

[21] We also experimented with fertility in the previous generation (namely in 1816) as an additional instrument for fertility in 1849. Fertility in 1816 is significantly associated with fertility in 1849. We can control for school enrolment in 1816 to exclude the possibility that previous-generation fertility may have affected current education by its association with previous-generation education. Results (not shown here but available upon request) are very similar, and over-identification tests do not reject instrument validity.

Table 2 The association between education and fertility

Dependent variable	Child–woman ratio					
	(1)	(2)	(3)	(4)	(5)	(6)
School enrolment	−0.080** (0.040)	−0.075** (0.037)	−0.159*** (0.036)	−0.148*** (0.033)	−0.199*** (0.036)	−0.174*** (0.037)
Share in industry		0.431*** (0.112)	0.420*** (0.101)	0.394*** (0.106)	0.333*** (0.104)	0.341*** (0.101)
Share in agriculture		0.091*** (0.033)	0.097*** (0.033)	0.106*** (0.032)	0.114*** (0.031)	0.116*** (0.030)
Share urban		−0.069** (0.034)	−0.025 (0.029)	0.017 (0.030)	0.003 (0.029)	−0.024 (0.031)
Population density		−0.010** (0.004)	−0.002 (0.004)	−0.004 (0.003)	−0.004 (0.004)	−0.004 (0.003)
Share married women			0.561*** (0.085)	0.676*** (0.093)	0.677*** (0.088)	0.658*** (0.084)
Share Protestants				−0.045*** (0.009)	−0.043*** (0.009)	−0.038*** (0.010)
Poland					−0.031*** (0.009)	−0.045*** (0.010)
Life expectancy at age 0						−0.002*** (0.001)
Constant	0.702*** (0.033)	0.665*** (0.040)	0.325*** (0.060)	0.249*** (0.063)	0.303*** (0.066)	0.367*** (0.069)
Observations	334	334	334	334	334	334
R^2	0.015	0.170	0.297	0.342	0.372	0.388

OLS regressions. Dependent variable: child–woman ratio. Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Child–woman ratio is the number of children aged 0–5 (0–7 in 1816) over the number of women aged 15–45. School enrolment rate is the share of children aged 6–14 enrolled in public primary schools
Source: County-level data from the Prussian Census 1849; see main text and Appendix for details

Table 3 The association between fertility and education

Dependent variable	School enrolment					
	(1)	(2)	(3)	(4)	(5)	(6)
Child–woman ratio	−0.187** (0.093)	−0.199** (0.090)	−0.188** (0.092)	−0.158* (0.089)	−0.244*** (0.084)	−0.180** (0.082)
Share in industry		0.756*** (0.286)	0.774*** (0.290)	0.767*** (0.285)	0.498** (0.210)	0.443** (0.208)
Share in agriculture		−0.159*** (0.051)	−0.158*** (0.052)	−0.165*** (0.052)	−0.105** (0.045)	−0.112** (0.045)
Share urban		−0.161*** (0.043)	−0.174*** (0.044)	−0.204*** (0.045)	−0.221*** (0.039)	−0.156*** (0.041)
Population density			0.005 (0.005)	0.009* (0.005)	0.008 (0.005)	0.007 (0.005)
Share Protestants				0.051*** (0.015)	0.046*** (0.014)	0.035*** (0.014)
Poland					−0.096*** (0.012)	−0.061*** (0.014)
Life expectancy at age 0						0.004*** (0.001)
Constant	0.920*** (0.060)	1.013*** (0.063)	1.008*** (0.064)	0.968*** (0.060)	1.052*** (0.057)	0.862*** (0.075)
Observations	334	334	334	334	334	334
R^2	0.015	0.112	0.113	0.141	0.283	0.313

OLS regressions. Dependent variable: school enrolment. Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Child–woman ratio is the number of children aged 0–5 (0–7 in 1816) over the number of women aged 15–45. School enrolment rate is the share of children aged 6–14 enrolled in public primary schools

Source: County-level data from the Prussian Census 1849; see main text and Appendix for details

education and fertility are significantly negatively correlated. In the subsequent columns, we progressively introduce control variables. The occupational structure and urbanization are strongly associated with fertility. As already shown in the literature (Galloway et al. 1998), in urban and more densely populated environments fertility rates tend to be lower, whereas in counties with a larger share of employment in manufacturing, fertility rates are higher.[22] The latter might be interpreted as a positive income effect where children are considered as normal goods. Controlling for the share of married women increases the explanatory power of the model and the estimated coefficient on education. Fertility is lower in Protestant areas as well as in counties with a majority of Slavic population. Life expectancy, which is expected partly to capture variation in infant mortality, is negatively associated with fertility. Lower infant mortality might have allowed families to be more effective in reaching the desired number of children and reduced the "hoarding" effect (Doepke 2005; Kalemli-Ozcan 2003).

The education coefficient remains statistically significantly negative throughout the different specifications. Furthermore, the association between education and fertility is economically non-trivial. Considering the richest specification in column 6, an increase in the enrolment rate by 10% points is associated with a decrease in the child–woman ratio by about 1.8 children per 100 women in child-bearing age. In terms of standard deviations, an increase of school enrolment by one standard deviation is associated with a decrease in fertility of about 0.26 standard deviations.

Given that economic theory depicts the Q–Q trade-off as a mutual association, Table 3 presents estimates of Eq. 6 where school enrolment is a function of the child–woman ratio, to make sure that the association holds both ways. The specification confirms the significant and robust negative association between fertility and education. In this specification, an increase in the child–woman ratio by one standard deviation is associated with a decrease in school enrolment by about 0.11 standard deviations. The level of industrialization is positively and the incidence of agriculture negatively associated with education, possibly mirroring corresponding demand for education linked to the economic structure. In line with the results of Becker and Woessmann (2009), Protestants show a higher propensity to invest in education. Life expectancy at age 0 is positively related to school enrolment, consistent with the prediction of standard economic theory that investment in education increases with the length of productive life.

The OLS estimates of the Q–Q trade-off show that, indeed, there is a significant and robust negative association between fertility and education already in a pre-demographic transition economy. However, given the simultaneity of the decisions about the quantity and quality of children and possible omitted variable biases, these coefficients do not necessarily have a causal interpretation.

6 The mutual causation between education and fertility: instrumental-variable evidence

6.1 The effect of education on fertility

In Table 4, we present estimates of the causal effect of education on fertility. School enrolment rates in 1849 are instrumented by landownership inequality and distance to Wittenberg. Columns 1–3 show the first-stage estimates, where the instruments first enter separately (columns

[22] When adding an interaction term between urbanization and education, it is statistically significantly negative, suggesting that the Q–Q trade-off is somewhat stronger in urban counties.

Table 4 The effect of education on fertility

Dependent variable	IV first stage School enrolment			IV second stage Child–woman ratio		
	(1)	(2)	(3)	(4)	(5)	(6)
School enrolment				−0.557*** (0.172)	−0.570*** (0.155)	−0.564*** (0.105)
Landownership inequality	−1.974*** (0.378)		−2.082*** (0.358)			
Distance to Wittenberg		−0.025*** (0.004)	−0.027*** (0.004)			
Share in industry	0.233 (0.209)	0.439** (0.206)	0.308 (0.198)	0.478*** (0.113)	0.483*** (0.115)	0.481*** (0.105)
Share in agriculture	−0.054 (0.043)	−0.060 (0.042)	0.002 (0.041)	0.067* (0.039)	0.065* (0.039)	0.066* (0.037)
Share urban	−0.077* (0.040)	−0.189*** (0.042)	−0.155*** (0.040)	−0.077** (0.034)	−0.079** (0.033)	−0.078** (0.030)
Population density	0.013** (0.005)	0.016*** (0.005)	0.015*** (0.005)	0.003 (0.005)	0.004 (0.005)	0.003 (0.004)
Share married women	0.660*** (0.102)	0.251** (0.119)	0.281** (0.113)	0.779*** (0.145)	0.787*** (0.152)	0.783*** (0.128)
Poland	−0.071*** (0.012)	−0.060*** (0.012)	−0.043*** (0.012)	−0.062*** (0.018)	−0.063*** (0.015)	−0.063*** (0.013)
Constant	0.427*** (0.080)	0.790*** (0.097)	0.763*** (0.093)	0.540*** (0.106)	0.546*** (0.089)	0.543*** (0.078)
Observations	334	334	334	334	334	334
R^2	0.360	0.370	0.430			
Partial F-statistic 1st stage				18.007	34.233	27.985
Sargan–Hansen p-value						0.958

2SLS regressions. Second-stage estimates in columns (4), (5), and (6) correspond to first-stage estimates displayed in columns (1), (2), and (3), respectively. Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.
Source: County-level data from the Prussian Census 1849; see main text and Appendix for details

1 and 2) and then jointly (column 3). The first-stage estimates suggest that landownership inequality and distance to Wittenberg are strongly associated with education, as mirrored by the large values of the partial F-statistics of the instruments in the first stage. When both instruments are used jointly, the Sargan–Hansen test of over-identification restrictions does not reject the hypothesis that, as long as one instrument is valid, the other one is as well.

The corresponding second-stage estimates in columns 4–6 present our main results which qualitatively confirm the OLS estimates. School enrolment rates have a statistically significant and large negative impact on fertility. The IV estimates are notably larger (in absolute value) than the OLS estimates of Table 2, suggesting that the dominating bias of the latter stems from omitted variables that drive them towards zero.[23] As suggested above, such bias in the OLS estimates could stem from imperfection of the income measures which gives rise to a positive association between fertility and education. In the IV models, an increase in the enrolment rate by 10% points causes a decline in the child–woman ratio by about 5 children per 100 women in child-bearing age. Given that the average child–woman ratio in 1849 is about 64 children per 100 women, education had a large and significant effect on fertility in Prussia already before the demographic transition.[24]

Table 5 presents several robustness checks which show that the IV coefficients for education are robust to different model specifications. To isolate the effect of parental preferences for sending their children to school from the availability of schools, column 1 adds the number of schools per 100 children in school age to account for differences in school supply. The variable is not statistically significant and does not change our coefficient of interest.

As mentioned above, households' decisions to invest in children's education may have been affected by differences in mortality and thus in the expected length of productive life, which affects the potential returns to education and, indirectly, the desirable number of surviving children (e.g., Cervellati and Sunde 2005, 2009; Soares 2005; Jayachandran and Lleras-Muney 2009). Thus, columns 2 and 3 control for life expectancy at ages 0 and 5, respectively. Life expectancy at age 0 mirrors cross-county differences in mortality rates across all age groups, including infant mortality. Life expectancy at age 5 is expected to mirror mortality differences beyond infant mortality. In both cases, the coefficient on education remains statistically significant and is of similar magnitude. The same holds when life expectancy is measured at age 25 and when survival rates at different ages are used as controls (not shown). An additional concern is that our measure of fertility, the child–woman ratio, which has the number of children aged 0–5 as its numerator, may be affected by infant mortality patterns. However, controlling directly for the infant mortality rate also does not affect the reported results (not shown).

As another robustness check, column 4 estimates the effect of school enrolment on the marital fertility rate in 1849, which considers married women rather than all women in child-bearing age in the denominator.[25] Using the marital fertility rate, the coefficient is still negative and highly significant: An increase in school enrolment by 10% points is associated with a decrease in the marital fertility rate by about 7 children per 100 married women. The share of married women is strongly associated with fertility, in line with the fact that children were predominantly born in wedlock (Hajnal 1965).

23 In addition, the bias towards zero may stem from measurement error in the enrolment data.

24 While our result is consistent with the child Q–Q model that models a *contemporary* trade-off between education and fertility, it is in principle also consistent with models that stress the link between *parental* education and their fertility. However, when we add our measure of school enrolment in 1816 as a proxy for education of the previous generation to the models of Table 4 (not shown), this does not enter significantly and does not change the qualitative results, supporting a child Q–Q model interpretation.

25 Unfortunately, the number of married women is not specified by age structure in the census.

Table 5 The effect of education on fertility—robustness checks

Dependent variable	Child–woman ratio			Marital fertility rate
	(1)	(2)	(3)	(4)
School enrolment	−0.566*** (0.097)	−0.568*** (0.097)	−0.552*** (0.103)	−0.721*** (0.157)
Share in industry	0.479*** (0.107)	0.486*** (0.107)	0.494*** (0.107)	0.733*** (0.157)
Share in agriculture	0.067 (0.041)	0.066 (0.041)	0.062 (0.040)	0.104* (0.058)
Share urban	−0.078*** (0.030)	−0.086*** (0.032)	−0.094*** (0.030)	−0.133*** (0.046)
Population density	0.003 (0.004)	0.004 (0.004)	0.004 (0.004)	0.002 (0.007)
Share married women	0.786*** (0.120)	0.789*** (0.119)	0.791*** (0.118)	−0.277 (0.234)
Poland	−0.063*** (0.012)	−0.068*** (0.012)	−0.071*** (0.011)	−0.095*** (0.016)
Schools per 100 children	−0.0022 (0.020)	−0.0003 (0.019)	0.0028 (0.019)	0.002 (0.026)
Life expectancy at age 0		−0.001 (0.001)		−0.001 (0.001)
Life expectancy at age 5			−0.001 (0.001)	
Constant	0.544*** (0.076)	0.565*** (0.078)	0.595*** (0.077)	1.730*** (0.121)
Observations	334	334	334	334
Partial F-statistic 1st stage	39.135	46.505	41.271	46.505
Sargan–Hansen p-value	0.969	0.814	0.605	0.981

2nd stage of 2SLS regressions. Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. School enrolment is instrumented with landownership inequality and distance to Wittenberg. Marital fertility rate is the number of children aged 0–5 over the number of married women. Results in this table correspond to the specification in column (6) of Table 4

Source: County-level data from the Prussian Census 1849; see main text and Appendix for details

6.2 The effect of fertility on education

Table 6 investigates the opposite direction of the Q–Q trade-off, from fertility to education. We estimate Eq. 6 instrumenting the child–woman ratio in 1849 by the adult sex ratio in 1849 and the sex ratio of children aged 0–7 in 1816.[26] Both instruments are strongly correlated with our measure of fertility for 1849 as they are a good proxy of the tightness of the marriage market. While the adult sex ratio in 1849 might be correlated with migration, the sex ratio of children 0–7 in 1816 (referring to the cohort aged 33–40 in 1849) circumvents this problem. In fact, the adult sex ratio is positively correlated with urbanization, which would be consistent with the fact that urban environments attracted relatively more male migrants.[27] By contrast, the sex ratio of children in 1816 is not correlated with urbanization in 1816, nor in 1849. In any event, the estimates are relatively stable irrespective of the instrument used, which reduces the potential problem of migration.

In the first stage, the adult sex ratio (column 1) is significantly associated with our measure of fertility with a comforting partial F-statistic. The second instrument, the child sex ratio in 1816, is also positively associated with fertility at a similar magnitude, though with a lower partial F-statistic. When using both instruments together, the Sargan–Hansen test does not reject the over-identification restrictions and speaks in favor of the validity of the instruments.

In the second stage, the IV coefficients have the expected negative sign and are statistically significant. In terms of magnitude, the IV estimate when using both instruments implies that an increase of the child–woman ratio by 1 child per 10 women is associated with a decrease of the enrolment rate by about 12% points.

Table 7 presents a set of robustness checks which confirm our results. Controlling for the number of schools per 100 children in school age (column 1) in order to focus on the effect of parental preferences given the availability of schools does not alter the results. As one would expect, the more schools are available—i.e., the higher the school density and the easier it is to send children to school—the higher enrolment. Again, controlling for life expectancy (column 2) does not alter the effect of fertility on education.

As discussed before, the adult sex-ratio instrument may be affected by gender-specific migration patterns, which might introduce endogeneity. However, the consistency and robustness of the results when using the sex ratio of children in 1816 already provide evidence that migration may not substantially affect our estimates. In order to provide additional support, we add a measure of temporary male migration proxied by the share of married men currently not in the county as a control variable. Again, results are qualitatively unaffected (column 3). The same is true when the effective number of migrants per capita (unfortunately available only from 1862 onwards) is used as an alternative measure of migration (not shown).

The analysis conducted to this point, drawing on variation triggered by cross-county differences in the price of education, in parents' preferences for education, and in the cost of raising children, suggests that causality between education and fertility runs in both directions. These results corroborate the thesis put forward by unified growth theory that there is a child Q–Q trade-off even before the demographic transition.

[26] As county borders changed after 1816, standard errors are clustered at the level of the 280 units of observation in the 1816 data.

[27] For example, male migrant workers with a specific education pattern might migrate to work in factories in other counties to be able to afford a family at home.

Table 6 The effect of fertility on education

Dependent variable	IV first stage Child–woman ratio			IV second stage School enrolment		
	(1)	(2)	(3)	(4)	(5)	(6)
Child–woman ratio				−1.101*** (0.351)	−1.470*** (0.532)	−1.216*** (0.333)
Sex ratio adults 15–45 (1849)	0.276*** (0.063)		0.264*** (0.061)			
Sex ratio children 0–7 (1816)		0.230** (0.089)	0.210** (0.083)			
Share in industry	0.248** (0.113)	0.285** (0.122)	0.225** (0.112)	0.766*** (0.216)	0.882*** (0.271)	0.802*** (0.218)
Share in agriculture	0.100*** (0.034)	0.104*** (0.035)	0.090*** (0.033)	−0.005 (0.059)	0.038 (0.076)	0.008 (0.058)
Share urban	−0.096*** (0.031)	−0.057* (0.032)	−0.103*** (0.031)	−0.261*** (0.057)	−0.279*** (0.065)	−0.267*** (0.058)
Share Protestants	−0.006 (0.012)	−0.024** (0.011)	−0.010 (0.012)	0.029 (0.026)	0.021 (0.030)	0.027 (0.027)
Population density	−0.015*** (0.005)	−0.012*** (0.004)	−0.015*** (0.005)	−0.003 (0.009)	−0.007 (0.012)	−0.004 (0.009)
Poland	−0.010 (0.011)	−0.023** (0.010)	−0.014 (0.010)	−0.113*** (0.021)	−0.121*** (0.025)	−0.116*** (0.021)
Constant	0.350*** (0.067)	0.392*** (0.089)	0.160 (0.106)	1.578*** (0.227)	1.804*** (0.335)	1.648*** (0.217)
Observations	334	334	334	334	334	334
R^2	0.240	0.210	0.263			
Partial F-statistic 1st stage				19.265	6.667	11.758
Sargan–Hansen p-value						0.449

2SLS regressions. Second-stage estimates in columns (4), (5), and (6) correspond to first-stage estimates displayed in columns (1), (2), and (3), respectively. Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Standard errors are clustered at the level of the 280 units of observation in the 1816 data due to a change of borders after 1816

Source: County-level data from the Prussian Censuses 1849 and—for the sex ratio of children 0–7—from the 1816 Census; see main text and Appendix for details

Table 7 The effect of fertility on education—robustness checks

Dependent variable	School enrolment		
	(1)	(2)	(3)
Child–woman ratio	−1.174*** (0.325)	−1.134*** (0.363)	−1.119*** (0.351)
Share in industry	0.858*** (0.232)	0.836*** (0.246)	0.838*** (0.248)
Share in agriculture	−0.030 (0.074)	−0.035 (0.072)	−0.035 (0.072)
Share urban	−0.243*** (0.064)	−0.223*** (0.067)	−0.221*** (0.066)
Share Protestants	0.010 (0.034)	0.007 (0.032)	0.007 (0.032)
Population density	−0.005 (0.009)	−0.004 (0.009)	−0.004 (0.009)
Poland	−0.108*** (0.019)	−0.098*** (0.030)	−0.097*** (0.030)
Schools per 100 children	0.065 (0.049)	0.066 (0.049)	0.068 (0.049)
Life expectancy at age 0		0.001 (0.002)	0.001 (0.002)
Temporary migration			−0.085 (0.103)
Constant	1.584*** (0.211)	1.515*** (0.283)	1.503*** (0.273)
Observations	334	334	334
Partial F-statistic 1st stage	11.553	10.027	10.601
Sargan–Hansen p-value	0.493	0.452	0.447

2nd stage of 2SLS regressions. Dependent variable: school enrolment. Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Standard errors are clustered at the level of the 280 units of observation in the 1816 data due to a change of borders after 1816. The child–woman ratio is instrumented with the sex ratio of adults aged 15–45 in 1849 and the sex ratio of children aged 0–7 in 1816, i.e., results in this table correspond to the specification in column (6) of Table 6

Source: County-level data from the Prussian Censuses 1849 and—for the sex ratio of children 0–7—from the 1816 Census; see main text and Appendix for details

7 Human capital and the demographic transition

Our results so far suggest that the child Q–Q trade-off indeed existed in a cross-section before the main phase of the demographic transition, as in the framework of seminal unified growth theory contributions like Galor and Weil (2000). In their framework, however, the Q–Q trade-off results in an actual comprehensive longitudinal decline of fertility only in the second stage of the industrial revolution. In this section, we explore the possibility of such long-run effects of investments in human capital on the fertility transition by estimating the predictive power of school enrolment in 1849 for the decline in the crude birth rate (CBR) and in the marital fertility rate (MFR) during the Prussian demographic transition at the turn to the twentieth century.[28]

An extensive literature focuses on explaining the determinants of the demographic transition. For instance, the Princeton European Fertility Project (EFP) of the 1960s/70s aimed at studying the fertility patterns of most Western European countries (Coale and Watkins 1986). In particular, Knodel (1974) documented the demographic transition in Germany through an analysis of 71 administrative areas. The EFP concluded that the spread of new moral and cultural norms together with birth control technology were responsible for the fertility decline in Europe. This view has been strongly criticized. Numerous studies have successively argued

[28] Another interesting question to look at would be whether *changes* in education at the end of the nineteenth century are related to the fertility decline. Unfortunately, there is no Prussian school enrolment data similar to 1849 at the end of the nineteenth century which would allow such an analysis.

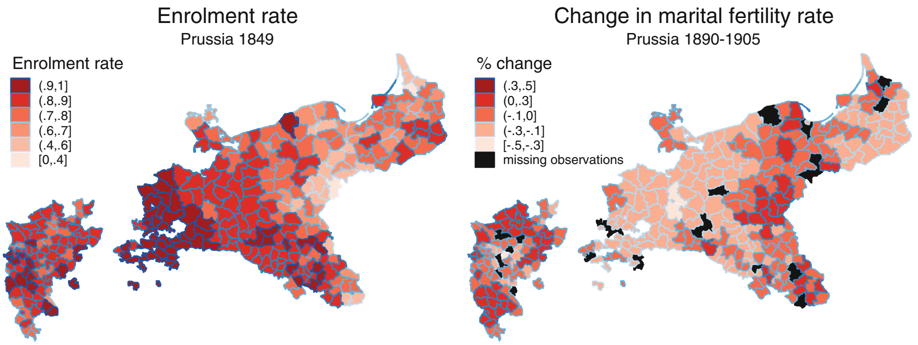

Fig. 2 The geographic distribution of education in 1849 and the fertility transition in 1890–1905. *Source*: County-level data from the Prussian Census 1849 and vital statistics for different years; see main text and Appendix for details

for a significant role played by economic factors in triggering the fertility transition (e.g., Galloway et al. 1994; Brown and Guinnane 2007; Bleakley and Lange 2009).

Similarly, we suggest that the economic factor of the child Q–Q trade-off and thus the accumulation of human capital may play a notable role in explaining the fertility transition in Prussia. Figure 2 shows the geographic distribution of school enrolment rates in 1849 alongside the change in MFR in 1890–1905. The highest enrolment rates are concentrated in the central counties of Prussia (left panel); the same regions later tend to experience the steepest decline in MFR (right panel). In order to test the hypothesis that human-capital accumulation was important for the fertility transition, we estimate the predictive power of school enrolment in 1849 for the decline in the CBR and MFR during the Prussian demographic transition:

$$rate\ of\ change\,(fertility)_{i,1880/90-1905} = \alpha_3 \cdot education_{i,1849} + \mathbf{X}_{i3}\delta_3 + e_{i3} \tag{7}$$

where the dependent variable is, in a first specification, the percentage change in the CBR between 1880 and 1905, and, in a second specification, the percentage change in the MFR between 1890 and 1905.[29]

Given the time lag between the dependent variable measured in 1880–1905 and the explanatory variable measured in 1849, there is no direct simultaneity in this specification. To address remaining issues raised by possible omitted variables that may create simultaneity bias and by the intergenerational correlation of fertility, we add the 1849 level of fertility to the control vector $\mathbf{X}_3$ in specification (7), in addition to the previous controls. To address the issue of migration, we add a variable for net migration per 1000 inhabitants, computed as the difference between immigrants and emigrants divided by the total population in 1880. Also, to account for any differential fertility development that might have occurred before 1880 (1890), the start of our observation of transition in CBR (MFR), we test for robustness to adding the initial 1880 (1890) level of CBR (MFR). The time span of about 50 years between 1849 and the turn of the century amounts to a difference of 1–2 generations. Estimated effects are thus likely to measure how education of the generation of parents and grandparents influenced fertility behavior of children and grandchildren.

[29] The fact that we use slightly different time intervals for the two dependent variables is exclusively determined by data availability. The dependent variable is not truncated at zero, so that both increases and decreases in fertility rates in the specified time period are considered.

Results are reported for the CBR in Table 8 and for the MFR in Table 9. The estimates show that those counties in which the investments in education were higher already in 1849 experienced a steeper fertility decline in terms of both CBR and MFR. Given the already high levels of primary school enrolment rates in 1849, this suggests that the demographic transition was faster in those counties where the demand for post-primary education was subsequently stronger. The estimates are particularly robust and significant for the MFR, but only marginally significant in some of the specifications of the CBR model.

Regarding the interpretation of the coefficients, if in 1849 the school enrolment rate had been 10% points higher, the MFR would have declined by an *additional* 1.3% (column 6 of Table 9). In terms of explanatory power, the richest model (column 6) accounts for 59% of the variation across counties of changes in the MFR. The results are robust to the choice of different truncation years for the dependent variables.[30] As for covariates, a higher net migration rate (i.e. relatively more immigrants than emigrants) is associated with faster demographic transition. The estimates indicate convergence in that counties with a higher child–woman ratio in 1849 and with higher CBR (MFR) in 1880 (1890) experience the fastest fertility transition. Consistent with the previous literature (Galloway et al. 1998), the fertility transition is more accentuated in urban environments. The transition is also stronger in Protestants counties, indicating a possible role for cultural factors.

Our results indicate that differences in educational investment in 1849 are associated with the strength of the fertility transition in 1880–1905 across Prussian counties. The analysis has an explorative character, not least because of limitations in data availability and because the sources of the initial differences in human capital accumulation by 1849 are not explicitly modeled in these specifications. Although the results thus cannot identify any specific channel for the association exclusively, they are consistent with an interpretation suggested by unified growth theory and recent empirical studies (Galor 2005b; Galor and Moav 2006; Goldin and Katz 2008). According to these studies, returns to education increased during the second phase of the industrial revolution—which corresponds roughly to the second half of the nineteenth century in Prussia—due to complementarities between technological change and human capital, and households responded by investing more in children's education and by limiting fertility. In line with such reasoning, initial technology-driven differences in the demand for human capital across counties may be behind the results of Tables 8 and 9.

8 Conclusion

Understanding the dynamics which allowed developed economies to escape the Malthusian world and enter a path of rapid economic growth is important in its own right. It becomes even more relevant as many developing countries are nowadays experiencing a demographic transition similar to that faced by western economies at the turn from the nineteenth to the twentieth century. Much effort has been spent on modeling such dynamics. Most of the theoretical literature assigns a major role to demography and its interaction with human capital. Yet, historical econometric evidence which tests and quantifies these theoretical predictions is rather scarce.

We use a unique dataset of more than 330 county-level observations in Prussia in the mid-nineteenth century. We find that a trade-off between quantity and quality of children was indeed already in place in 1849, about one generation before the conventional dating of the main phase of the German demographic transition. Thus, our results support the assumption

30 Data on CBR are available until 1914.

Table 8 The long-run effect of education on the fertility transition: crude birth rates

Dependent variable	Crude birth rate 1880–1905 (% change)					
	(1)	(2)	(3)	(4)	(5)	(6)
School enrolment	−0.132*** (0.043)	−0.079* (0.047)	−0.048 (0.047)	−0.069 (0.049)	−0.042 (0.049)	−0.094* (0.048)
Child–woman ratio	−0.227*** (0.078)	−0.299*** (0.070)	−0.315*** (0.069)	−0.290*** (0.069)	−0.345*** (0.070)	−0.145 (0.089)
Share in industry	0.110 (0.165)	0.010 (0.169)	−0.068 (0.176)	−0.081 (0.172)	−0.070 (0.170)	0.063 (0.151)
Share in agriculture	−0.008 (0.040)	0.021 (0.037)	0.061 (0.041)	0.057 (0.040)	0.042 (0.040)	0.024 (0.039)
Share urban	−0.330*** (0.036)	−0.254*** (0.036)	−0.270*** (0.037)	−0.242*** (0.040)	−0.240*** (0.040)	−0.254*** (0.038)
Population density	0.017*** (0.004)	0.007* (0.004)	0.008* (0.004)	0.008* (0.005)	0.007 (0.005)	0.008* (0.004)
Share Protestants		−0.118*** (0.013)	−0.102*** (0.014)	−0.106*** (0.014)	−0.105*** (0.014)	−0.111*** (0.014)
Poland		−0.016 (0.011)	−0.021* (0.011)	−0.008 (0.013)	−0.001 (0.013)	0.000 (0.013)
Schools per 100 children			−0.067*** (0.023)	−0.066*** (0.023)	−0.075*** (0.023)	−0.064*** (0.021)
Life expectancy at age 0				0.002**(0.001)	0.002*** (0.001)	0.001 (0.001)
Net migration per 1000 inhabitants (1880)					−0.004*** (0.001)	−0.004*** (0.001)
Crude birth rate (1880)					−0.006*** (0.002)	
Constant	0.223*** (0.065)	0.278*** (0.069)	0.300*** (0.068)	0.232*** (0.072)	0.229*** (0.073)	0.400*** (0.082)
Observations	309	309	309	309	309	309
R^2	0.243	0.426	0.443	0.451	0.470	0.498

OLS regressions. Dependent variable: crude-birth rate 1880–1905 (% change). All right-hand side variables refer to 1849, except where other year is indicated in parentheses. Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Crude birth rate is defined as the number of legitimate births (in 1000 s) over the total population
Source: County-level data from the Prussian Census 1849 and demographic data for different years; see main text and Appendix for details

Table 9 The long-run effect of education on the fertility transition: marital fertility rates

Dependent variable	Marital fertility rate 1890–1905 (% change)					
	(1)	(2)	(3)	(4)	(5)	(6)
School enrolment	−0.165*** (0.036)	−0.100*** (0.032)	−0.107*** (0.034)	−0.113*** (0.037)	−0.093** (0.037)	−0.130*** (0.039)
Child–woman ratio	−0.022 (0.070)	−0.109** (0.052)	−0.105** (0.053)	−0.097* (0.054)	−0.138** (0.056)	−0.062 (0.063)
Share in industry	−0.138 (0.180)	−0.259 (0.194)	−0.243 (0.191)	−0.247 (0.191)	−0.238 (0.191)	−0.194 (0.181)
Share in agriculture	0.045 (0.039)	0.081** (0.034)	0.072* (0.038)	0.071* (0.038)	0.060 (0.038)	0.062* (0.037)
Share urban	−0.246*** (0.035)	−0.153*** (0.032)	−0.149*** (0.032)	−0.141*** (0.035)	−0.140*** (0.034)	−0.156*** (0.036)
Population density	0.010*** (0.003)	−0.002 (0.003)	−0.002 (0.003)	−0.002 (0.003)	−0.002 (0.003)	−0.001 (0.003)
Share Protestants		−0.143*** (0.011)	−0.147*** (0.012)	−0.148*** (0.012)	−0.147*** (0.012)	−0.168*** (0.014)
Poland		−0.019** (0.009)	−0.018** (0.009)	−0.014 (0.010)	−0.009 (0.010)	−0.007 (0.010)
Schools per 100 children			0.014 (0.019)	0.015 (0.019)	0.008 (0.019)	0.011 (0.018)
Life expectancy at age 0				0.001 (0.001)	0.001 (0.001)	0.001 (0.001)
Net migration per 1000 inhabitants (1880)					−0.003*** (0.001)	−0.003*** (0.001)
Marital fertility rate (1890)					−0.005*** (0.002)	
Constant	0.103* (0.061)	0.169*** (0.049)	0.164*** (0.048)	0.143*** (0.053)	0.141*** (0.053)	0.260*** (0.070)
Observations	309	309	309	309	309	309
R^2	0.265	0.566	0.567	0.568	0.580	0.591

OLS regressions. Dependent variable: marital fertility rate 1890–1905 (% change). All right-hand side variables refer to 1849, except where other year is indicated in parentheses. Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Marital fertility rate is defined as the number of legitimate births per 100 married women in child-bearing age (15–49)

Source: County-level data from the Prussian Census 1849 and demographic data for different years; see main text and Appendix for details of unified growth theory that the child quantity-quality trade-off existed early on during the industrial revolution.

We further contribute to the literature by establishing the two-way causation between the two components of the child Q–Q trade-off in the sense that exogenous variation in one component leads to changes in the other component. Causal identification in our models builds on instrumental-variable strategies, separately exploiting exogenous variation in fertility and in education. The results support a mutual causation between education and fertility, in line with the theoretical conceptualization of the trade-off. The findings are robust when accounting for differences in mortality patterns.

Furthermore, we show that education levels in 1849 are a strong predictor of the strength of the fertility transition between 1880 and 1905. Counties with higher school enrolment rates in 1849 show a steeper fertility decline, both in terms of crude birth rates and marital fertility rates, at the turn from the nineteenth to the twentieth century. Thus, in line with unified growth theory, human capital appears to have played a significant role in the fertility transition and as such in the transition from Malthusian stagnation to modern economic growth.

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Appendix: County-level data for Prussia in the 19th century

1849 Census

The source of the 1849 census data is the Prussian Statistical Office, which published the data in the period 1851–1855 under the title "Tabellen und amtliche Nachrichten über den Preussischen Staat für das Jahr 1849". The census of 1849 contains seven volumes. We use Vol. 1 for population data, Vol. 2 for education and mortality data, and Vol. 6a for factory data. All data are available for 334 counties.

The 1849 education data contain information on the number of schools and students for public elementary schools (*Öffentliche Elementarschulen*) and public middle schools for boys and girls (*Öffentliche Mittelschulen für Söhne und Töchter*). We combine enrolment in elementary and middle schools to obtain primary school enrolment, matching the fact that children at recommended school age (6–14 years) could either attend elementary schools or middle schools.

We construct measures of life expectancy at different ages by calculating age-specific mortality rates from population and mortality data, which is reported in age groups of varying size, usually encompassing 5 years.

The 1849 factory data contain information on the number of factories and workers for the Prussian counties. 119 types of factories are distinguished by the products fabricated. Our variable for industrialization in 1849 refers to the share of population working in textile, metal, and other factories. The textile sector includes factories for spinning, weaving, dyeing, and apparel. The metal sector includes processing of metals and production of metal products and machinery, as well as manufacture of stone and glass products. The other industrial sectors include such factories as those producing food, wood, paper, wax, and rubber.

1816 Census

1816 is the earliest year for which the Prussian Statistical Office, founded in 1805, collected detailed data at the county and municipality level. The 1816 data refer to 330 counties and 172 large and medium-size towns with detailed information on the age structure by gender. Due to a change of county borders after 1816, the 1816 data had to be re-grouped to match the county delineation in 1849, so that the ultimate number of units of observation of the 1816 data is 280.

The county data provide information on the number of public elementary schools (*Öffentliche Elementarschulen*), the only school type equally available in rural areas and towns at the time, and the number of students therein. In addition, the town data report on the number of schools and students in the following school types available only in towns: private elementary schools (*Privat-Elementarschulen*), public middle schools for boys and girls (*Öffentliche Bürger- oder Mittelschulen für Söhne und Töchter*), and private middle schools (*Private Bürger- oder Mittelschulen für Söhne und Töchter*). To capture all children at recommended school age, county and town enrolment data are aggregated to compute total primary enrolment. The source of the 1816 Population Census data is Alexander A. Mützell (1825), *Neues Topographisch-Statistisch-Geographisches Wörterbuch des Preussischen Staats*, Vol. 6, Halle: Karl August Kümmel.[31]

Demographic data 1880–1905

In order to generate the variables crude birth rate (CBR), marital fertility rate (MFR), and net migration rate, we use data on vital statistics and population censuses, respectively. Vital statistics are available on an annual basis for the period 1862–1914. For the MFR, the information on the number of married women aged 15–45 and the number of newborns are available only for the years 1890, 1895, 1900, and 1905. To construct the CBR variable, we use data from *Preussische Statistik*, Vol. 42 and Vol. 249. For the MFR variable, we use data contained in *Preussische Statistik*, Vol. 121a, Vol. 148a, Vol. 177, and Vol. 206a. For the net migration rate in 1880, we use data contained in *Preussische Statistik*, Vol. 42.

After 1849, some bigger counties were split in two (or more) separate counties; we aggregated the post-1849 data up to the 334 counties existing in 1849. To match the 1849 data, the analysis for the late nineteenth/early twentieth century is also restricted to Prussia in its 1849 borders, even though Prussia had annexed several territories since.

[31] The 1816 school enrolment data were missing for the eleven counties of the district of Cologne. We imputed the data based on school enrolment data available in 1829 for all 59 counties of the Rhine Province. Given a correlation of 0.59 of the 1829 data with the 1816 data for the 48 counties with both datasets available, we regressed the 1816 data on the 1829 data and predicted the 1816 values for the eleven Cologne counties based on their 1829 values. Data on the age structure in 1816 were also missing for the eleven counties of the district of Cologne, which report population totals only. We used the share of the female age group 0–7 and 15–45 of the contiguous provinces (Arnsberg, Duesseldorf, Aachen, Koblenz) to impute the missing child-woman ratio of 1816 for the eleven Cologne counties.

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