Selection and Evolutionary Growth: Evidence from pre-Industrial Germany*

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August 2026

Abstract

Evolutionary growth theories suggest that natural selection on economically beneficial traits set the stage for modern growth (Galor and Moav 2001; Galor and Moav 2002; Clark 2007). This paper assesses whether selection was viable during the Malthusian era under imperfect intergenerational transmission of traits. I describe a simple model of intergenerational transmission in Malthusian equilibrium to quantify how much selection different parameter combinations deliver. I use micro-data from historical Germany to parametrize the model; I estimate the fertility differential across, and the transmission of, latent endowments (proxied by occupational status). High-status couples had ∼0.6–1.6 additional surviving children, and endowments were strongly transmitted ($\lambda \approx 0.59$). I find that the degree of selection depends on high intergenerational transmission, and on a dynamic social baseline. With a fixed intercept the German parameters can deliver 0.28 standard deviations cumulative drift, while under an imposed moving intercept (adapting social endowments), which the data do not identify, the model projects 1.78 standard deviations over 20 generations. These projections also imply that even when mean endowment drift is modest, the share of the population in the upper-tail of the endowment distribution increases substantially.

JEL Classification: N33, N13, J13, J62, J12, O40

Keywords: Evolutionary Growth; Fertility; Social Mobility; Survival of the Richest.

*I would like to thank Eric Schneider and Tom Raster for their excellent supervision. This paper has benefited from discussion with Oliver Brufal, Gregory Clark, Richard Franke, Hampton Gaddy, Oded Galor, Morgan Kelly, Omer Moav, Matteo Pograxha, Jean-Laurent Rosenthal, Romain Wacziarg, and seminar participants at the California Institute of Technology, University of Southern Denmark, London School of Economics, CEPH (Trinity College Dublin), IPECE (University College Dublin), and the Irish Economic Association. This paper would not have been possible without the efforts of Jochen Mehldau, who compiled the Wittgensteiner Familiendatei. I acknowledge funding from the Economic and Social Research Council [Grant No: ES/Y001869/1]. An earlier version of this paper was circulated as "Malthus in Germany? Reproductive Inequality and Status." Refine.ink was used to check the paper for consistency and clarity. The usual disclaimers apply.

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Introduction

Could natural selection on economically beneficial traits have set the stage for the transition to modern economic growth? Economists have long emphasized that fertility can vary by socioeconomic status (Malthus 1803). Across pre-Industrial societies the wealthy tend to have more offspring (e.g. Clark and Hamilton 2006; Cummins 2020; Kumon 2026). If wealth is associated with certain traits that are transmittable, a greater share of the subsequent generation would exhibit these features of the reproductively more successful classes. This is the fundamental building block of evolutionary growth theory (Galor and Moav 2001): fertility differentials across socioeconomic status lead to the diffusion of the traits and preferences of the economically successful. Slow compositional change towards traits that are growth-complementary thus contributes to (or causes) the transition to modern economic growth (Galor and Moav 2002; Clark 2007). Although such explanations are appealing in their theoretical clarity, their applicability to observed historical processes is open for debate.

Selection does not necessarily follow from assuming that particular *growth-inducing* traits were associated with higher socioeconomic status and thus higher fertility. Compositional drift only occurs if *latent* traits are transmitted from one generation to the next.[1] The transmission of traits – or endowments – could occur via genetic and extragenetic pathways (Lala and Feldman 2024), with authors placing differing weights on either (Clark 2023). However, even if I remain agnostic about the channel of transmission, its magnitude is important. Prominent evolutionary growth theories take very different approaches. Galor and Moav (2002) assume perfect transmission of traits. Clark (2007) acknowledges and stresses the importance of strong intergenerational transmission, but he never formalizes his model to derive explicit conditions.

This paper will test how much selection on *latent* endowments was possible during the Malthusian era while allowing for the *imperfect* intergenerational transmission of endowments. To this end, I specify an overlapping-generations model of an agricultural economy in Malthusian equilibrium with intergenerational transmission. I then leverage individual-level life histories from the historical principalities of *Sayn-Wittgenstein-Hohenstein* and *Sayn-Wittgenstein-Berleburg* in "Germany" (Mehldau 2011) to estimate key parameters, the fertility differential and the coefficient of intergenerational transmission, in a historical European setting.[2] After using the model to explore how much change in the distribution of

[1] McCloskey (2008) raises a similar critique regarding *survival of the richest*. Clark (2008) responds to this critique and uses back-of-the-envelope calculations to demonstrate that fertility differentials and the high intergenerational transmission of status could lead to survival of the richest. This paper formalizes these back-of-the-envelope calculations by calculating the magnitude of and threshold conditions for selection.

[2] See Figure A1 for a map depicting the location of the two principalities (by then absorbed into Prussia as a single county) in present-day Germany and 1817 Prussia.

endowments the German parameters could produce, I run a counterfactual using estimates for England. Last, I generalize across the entire parameter space, and alter the modeling assumptions, to establish when substantial selection becomes possible.

The model combines features of Becker and Tomes (1986)'s model of intergenerational endowment transmission and Galor and Moav (2002)'s evolutionary growth model. Since I am interested in selection *during* the Malthusian era, I abstract from the transition to modern economic growth and ignore technological feedback and the quantity-quality tradeoff. I solve the model with a population pin to yield the following. First, selection on endowments is compatible with stagnant per capita income because any gain in mean endowments is absorbed by a growing population. Thus, selection can operate on relative income/endowments alone. Second, the necessary condition for selection is a fertility differential across endowments, which scales with the fertility gradient, and the inverse of average fertility. Last, if there is imperfect transmission of endowments – i.e. $\lambda < 1$ – and the social endowments offspring receive irrespective of their parents are constant, selection can only deliver a one-time level shift of the distribution. The magnitude of this shift is increasing and convex in the coefficient of transmission, and increasing and linear in the fertility differential.

I parametrize this model with micro-data from historical Germany. I use community reconstitution data that are extensive in depth, temporal scope, and generational linkage. They contain the universe of ecclesiastically recorded demographic events for $\sim$ 150,000 individuals across contiguous parishes in two sovereign principalities between 1600 and 1900. I use occupational status as a proxy for socioeconomic status. Occupational titles are mapped to different status measures using LLM methods, and are cross-validated against manual and deterministic classifications.[3] More than 24% of men had their occupation recorded, which compares favorably with frequently used sources for France (18.7%) or England (10.3%) (Cummins 2020; Wrigley et al. 1997; Henry and Houdaille 1973).[4]

I find that couples of a high socioeconomic status had between $\sim$0.6 and 1.6 additional surviving children. The upper-bound estimate is obtained by implementing a partial identification strategy to account for measurement error. This fertility differential is driven by gross fertility and not by under-15 mortality. There is a null effect on the extensive margin of fertility, that is, whether individuals entered into childbearing. I find no statistically significant effects for childlessness and celibacy. To understand the mechanism behind the effect on gross fertility, I turn to variation in the starting, spacing, and stopping of reproductive behavior: 85% of the effect of occupational status on gross fertility is attributable to variation in mother's age at marriage (starting).

[3] Using LLM methods, instead of hand-coding occupational titles, has the advantage of reducing researcher degrees of freedom while also improving replicability.

[4] All results are robust to including this unobserved category as the lowest status group.

I draw on work by Stuhler (2012) to estimate the unattenuated coefficient of transmission via a ratio estimator (Del Pizzo et al. 2026). In a restricted sample the coefficient of transmission is equal to 0.59. I find no evidence for statistically significant differences across sub-samples when split by time or data restriction.

Other studies tend to draw on data from dispersed parishes (e.g. Boberg-Fazlic et al. 2011; Cummins 2020). Since I observe short-distance migration to neighboring parishes, using data from contiguous parishes increases the number of intergenerational links and complete life-histories. However, the internal validity of this sample comes at the cost of external validity. To alleviate such concerns, I draw on a large sample of digitized community reconstitutions across the extent of German settlement in Central Europe. For intergenerational transmission, I find a statistically indistinguishable coefficient. The source is not well suited to estimating fertility differentials since family histories are often incomplete. Still, I find a statistically significant and positive, albeit smaller, association between occupational status and net fertility.

I plug the estimated parameters into my model to estimate the degree of selection. When using the maximum selection envelope across all German parameters, the model produces a 0.28 standard deviation increase in mean endowment while increasing the share of the population above the 90th percentile of the initial distribution from 10 to 16%. This is a modest albeit meaningful change in the mean, and a substantial change in the number of individuals above the initial 90th percentile. The minimum scenario yields a much more conservative estimate of 0.05 standard deviations and 10$\rightarrow$11%. Determining the level of selection necessary for the theories of Galor and Moav (2002) and Clark (2007) is beyond the scope of this paper, but the compositional drift I estimate appears small. The estimated generational drift in mean endowments of $\sim$0.26% per generation is about an order of magnitude below the 2.5% per generation noted by Clark (2008, p. 189).

However, the Industrial Revolution did not start in Germany. To account for this, I run a counterfactual with maximum selection parameter envelope for England (Clark and Cummins 2015b; Clark and Cummins 2015a). Even with the highest estimates of intergenerational transmission, the English regime only produces 0.83 standard deviations, or $\sim$0.78% drift per generation. Albeit higher, this is still a moderate and bounded increase in mean endowments. Generalizing across the entire parameter space illustrates that the kind of selection underpinning evolutionary growth theory requires remarkably high *transmission* of the growth-adjacent traits.

Last, to obtain the most selection-inducing scenario, I relax my modeling assumptions, and let social endowments vary freely with selection. This removes the bound on selection, and implies 0.09 standard deviations of drift per generation in Germany, and 0.17 in

England. Using this model the English parameters produce drift above Clark's benchmark of 2.5%. Over 20 generations this constitutes substantial societal change, in line with evolutionary growth theories. As such, next to high intergenerational transmission and a fertility differential, selection is more probable with dynamic social endowments.

Related Literature. This paper contributes to the debate on the viability of evolutionary growth theory (Galor and Moav 2002; Clark 2007).[5] Instead of focusing on the transition to modern growth, I focus on the underlying selection mechanisms. Although several papers estimate fertility differentials or intergenerational transmission in the context of evolutionary growth (e.g. Boberg-Fazlic et al. 2011; Clark and Cummins 2015a; Clark and Cummins 2015b; de la Croix et al. 2019), this literature stops short of demonstrating whether these parameters are sufficient for selection to take place.[6,7] I specify a simple model of the pre-Industrial economy with imperfect transmission of traits to estimate the possible degree of selection. My results suggest that the viability of evolutionary growth depends on exceptionally high rates of intergenerational transmission, and on specifying a model of transmission with a moving intercept.[8] Qualitatively, this work is most similar to contributions illustrating that medium fertility families had an evolutionary advantage in the long run (Galor and Klemp 2019; Hu 2025). By testing the demographic feasibility of evolutionary growth theories, this paper contributes to our understanding of the transition from economic stagnation to growth.[9]

This literature overlaps with work testing Malthusian theory.[10] Malthus makes formal predictions about the relationships between wealth, fertility, and mortality (Malthus 1803).

[5]Another strand of literature discusses the relevance of reproductive inequality in contemporary settings (de la Croix and Doepke 2003; Doepke 2004; Vogl 2016).

[6]Klemp and Weisdorf (2019) test another tenet central to endogenous growth theory. They explore the relationship between medium fecundity and human capital in pre-Industrial England.

[7]de la Croix et al. (2019) include back-of-the-envelope calculations to show that their estimates would lead to a substantial expansion of the middle-class. However, their calculations assume perfect intergenerational transmission.

[8]The selection differential is equal to the fertility differential over mean fertility. Here I am calibrating the model at mean fertility of 3, and a fertility differential that produces a gap of 1 surviving child between the top and bottom decile of the endowment distribution.

[9]Another strand of literature uses genotyped DNA samples and GWAS poly-genic scores to identify evidence for genetic selection (Piffer and Connor 2025). But questions regarding population stratification (Hellwege et al. 2017) (i.e., where random variation in genotypes across environments drives spurious associations between genotype and environment-dependent phenotype) and the portability problem (Matthews 2022) (the limited out-of-sample applicability of poly-genic scores) raise important concerns about the reliability of these findings.

[10]Given the population-level predictions of the Malthusian model and the scarcity of individual-level records, macro-level inquiry – estimating the relationship between vital rates and real wages at a population level – prevails (Lee and Anderson 2002; Crafts and Mills 2009; Fernihough 2013; Pfister and Fertig 2020). Another strand of this literature uses historical event analysis to evaluate the contemporaneous individual-level demographic response to economic pressure and the variation of the latter across social groups (Bengtsson et al. 2004; Thiehoff 2015).

He predicts lower gross fertility (preventive check) and higher childhood mortality (positive check) among poorer couples (Malthus 1803). Several studies examine these associations at the individual level. For Europe, previous studies focus on England (Clark and Hamilton 2006; Boberg-Fazlic et al. 2011; Kelly and Ó Gráda 2014; Clark and Cummins 2015b; Cummins et al. 2016; de la Croix et al. 2019) and France (Weir 1995; Cummins 2020).[11] By exploring these associations in Germany, this paper expands our knowledge of the demographic history of Europe. France and England are the vanguards of demographic change and industrialization, respectively. Understanding how these associations shaped the demographic regime in a case removed from these extremes is an important step towards a more holistic understanding of demographic regimes of pre-Industrial Europe. The fertility differentials I estimate are comparable to the "super-fertility" of the rich in England, instead of the moderate advantage the richest enjoyed in pre-revolutionary France (Cummins 2020, p. 15).

Lastly, my findings contribute to a rich literature on social mobility. While several studies explore historical social mobility (Crew 1973; Kaelble 1984; Van Leeuwen and Maas 1996), the German case before 1900 is largely absent from the recent literature. Advancements in methodology and data availability led to a proliferation of estimates of social mobility for Anglo-American countries (e.g. Miles 1999; Mitch 2005; Mazumder 2005; Long and Ferrie 2013; Clark and Cummins 2015a; Braun and Stuhler 2018; Clark, Cummins, and Curtis 2023; Zhu 2024; Pérez 2019; Ward 2023). I contribute the first estimate of intergenerational transmission from pre-Industrial Germany. I estimate that the coefficient of transmission was 0.59 (1600-1875). Albeit high, implying limited mobility, the estimate falls below the proposed universal coefficient of $\sim$0.80 by Clark (2014) and is much closer to estimates for early-20th-century Germany by Braun and Stuhler (2018) of around 0.60.

The paper proceeds as follows. The next section develops a simple model of selection in Malthusian equilibrium. In section three, I introduce the setting and data. Sections four and five estimate the fertility differential and the coefficient of transmission. In section six, I probe the external validity of my findings. Section seven parametrizes the model and discusses the results. The final section concludes.

2 Model

I combine elements of a model of intergenerational endowment transmission (Becker and Tomes 1986; Stuhler 2012) and an evolutionary growth model (Galor and Moav 2002) to

[11] Several papers tested the individual-level dynamics of the Malthusian model outside of Europe (Feng et al. 1995; Lee and Feng 1999; Campbell and Lee 2002; Bandyopadhyay and Green 2013; Lee and Park 2019; Kumon and Saleh 2023; Hu 2023; Kumon 2026).

extract empirical predictions for selection during the Malthusian era. Instead of specifying a model with a quantity-quality tradeoff and endogenous technology (e.g. Galor and Weil 2000; Galor and Moav 2002; Galor and Michalopoulos 2012), I abstract from economic takeoff and focus on the Malthusian equilibrium.[12] This design choice keeps the model tractable, while enabling me to answer a specific question: how much selection was possible given different parameter combinations?

Setup. Suppose an overlapping generations economy with infinite discrete time, indexed by generations $g = 0, 1, 2, ...$. Every generation is alive for two periods. In period $g-1$ individuals are born, inherit a scalar *endowment* $x_{i,g} > 0$, and consume some of their parents' time. In period g *couples*, indexed by i, contribute to production, reproduce, and die.

2.1 Production and Income

The economy produces a single homogeneous good from land and efficient units of labor, where each unit of endowment $x_{i,g}$ is one efficient unit. Output is Cobb–Douglas, and with no property rights over land, the entire product accrues to labor. Individuals receive income according to their endowment (efficient units) and an economy-wide wage which is equal to the average product of labor (output per efficient unit).[13]

$$Y_g = H_g^{1-\alpha}(AX)^{\alpha}, \qquad y_{i,g} = w_g\, x_{i,g}, \qquad w_g = \left(\frac{AX}{H_g}\right)^{\alpha}. \tag{1}$$

Here $H_g = \sum_i x_{i,g} = N_g \bar{x}_g$ is total efficient labor, X is land, $A > 0$ the fixed level of technology, and $\alpha \in (0,1)$. A larger population N_g or mean endowment $\bar{x}_g$ raises H_g and lowers the wage.[14]

2.2 Endowment Transmission

Children inherit a *latent* endowment $x_{i,g}$ from their parents. This latent endowment captures direct transfers from parents (e.g., genetics or preferences) as well as environmental

[12] As such, this model is closer in spirit to the general articulations of evolutionary selection in Galor and Moav (2001) and Clark (2007) than a fully specified unified growth model.

[13] A rentier sector owning all land would complicate the wage equation but leave selection unchanged; only land distributed across the population would matter, with an ambiguous effect. Excluding the poor from land rents sharpens reproductive inequality, whereas large land rents would dilute selection on the other determinants of income (Kumon 2026).

[14] In logs, $\ln y_{i,g} = \ln w_g + z_{i,g}$ with $z_{i,g} \equiv \ln x_{i,g}$.

factors shaped by parents (e.g., location or social networks). I assume that these different facets are inherited as one package in a log-linear first-order Markov process according to $\lambda \in (0,1)$ where $z_{i,g} \equiv \ln x_{i,g}$.

$$z_{i,g} = a_g + \lambda\, z_{i,g-1} + v_{i,g} \qquad\qquad v \sim (0, \sigma_v^2),\ v \perp z, \tag{2}$$

where $v_{i,g}$ is an *iid* error term that captures luck and other random deviations from potential endowment that arise due to random chance (e.g., injury or war) or individual choice (e.g., pursuing poetry), and a_g captures social endowments that are common to all members of a society. $z_{i,0}$ is normalized to mean zero and a_g is set to $a_g = a = 0$ in the stationary case.

Modeling a_g as a stationary intercept assumes persistent social endowments (Becker and Tomes 1986, S5). This assumption relies on two conditions: (1) inherited endowments function as a composite package of various traits and pathways, and (2) the rigid cultural and institutional structures of pre-Industrial society compressed the realized societal value of these traits back toward a structural baseline. While this appears reasonable in the context of the Malthusian era, it constitutes a strong boundary condition on selection. If, for example, cultural norms and beliefs were highly dynamic and readily updated from generation to generation (e.g. McCloskey 2016; Mokyr 2016), or if, as proposed by Clark (2023), endowments were entirely genetic, a moving intercept would be more appropriate. In section 7, I explicitly explore the divergent implications of either model.

Assortative Mating. This model abstracts from mating decisions. Instead, each parent i represents a couple, implying perfect assortative mating. In early-modern societies where assortative mating was often high, this is a defensible assumption (Clark and Cummins 2022; Curtis 2026). Alternatively, Braun and Stuhler (2018) show that in the absence of perfectly assortative mating, λ is a reduced form of average transmission and the degree of assortative mating. Since mating was likely not perfectly assortative, this is my preferred interpretation. A full two-parent matching process would refine the interpretation of λ without changing the dynamics.

2.3 Households and Fertility

Parents with income y choose consumption c and the number of children surviving to reproductive age n. Preferences are Stone–Geary over *subsistence* consumption and children,

$$U = (1-\delta)\, \ln(c - \bar{c}) + \delta\, \ln n, \tag{3}$$

where $\bar{c}$ is the subsistence floor and $\delta \in (0,1)$ is the taste for children.[15] Rearing children to reproductive age (i.e. period 2) carries a time cost of πy and the budget constraint is $y \geq c + n\pi y$. Maximizing (3) subject to the budget yields the fertility function (see Appendix A):

$$n^* = \begin{cases} 0 & \text{if} \quad y \leq \bar{c} \\ \frac{\delta}{\pi}\left(1 - \frac{\bar{c}}{y}\right) & \text{otherwise.} \end{cases} \tag{4}$$

The first case is the corner solution when the subsistence constraint binds. Fertility is increasing and concave in income above subsistence.[16]

Adding preferences over the quality of children would materially affect economic takeoff in the model. It would, however, not change the process of selection. If anything, excluding the tradeoff lowers the necessary conditions for evolutionary growth, since the trait I am selecting on via income no longer mechanically reduces fertility.

2.4 Laws of Motion

Endowment. The mean endowment of generation g is the fertility-weighted mean of the endowments their parents (generation $g-1$) transmitted. Thus,

$$\begin{aligned} \bar{z}_g &= \lambda \frac{\sum_i n_{i,g-1}\, z_{i,g-1}}{\sum_i n_{i,g-1}} = \lambda\left(\bar{z}_{g-1} + S_{g-1}\right) \\ \text{where} \quad S_{g-1} &= \frac{\text{Cov}(n_{i,g-1}, z_{i,g-1})}{\bar{n}_{g-1}} \quad \text{is the } \textit{selection differential.} \end{aligned} \tag{5}$$

Selection also alters the second moment; directional selection and regression to the mean compress the variance $\sigma^2_{z,g}$, while the innovation σ^2_v replenishes it. To keep the model tractable, I assume these balance, so that variance is stationary, $\sigma^2_{z,g} = \sigma^2_z$, and the selection differential $S_g = S$ is constant.[17]

Population. The number of couples N_g is half the surviving births of the previous generation. A fraction $1-\mu$ of births reach adulthood where $\mu \in [0,1)$ captures losses such as emigration (Appendix A). I introduce this valve to keep the model compatible with a setting subject to population outflows. I assume emigration is orthogonal to endowments,

[15] I assume households have lexicographic preferences where for incomes $y \leq \bar{c}$, the household exhausts its budget on consumption, rendering $n^* = 0$.

[16] As $y \to \infty$, fertility saturates and approaches some biological ceiling $n_{\max} = \delta/\pi$.

[17] Appendix C provides a fuller discussion of the variance dynamics.

which holds in my data, where the probability of emigrating is not status-dependent (see Appendix F).[18]

2.5 Malthusian Closure

I close the model with a population pin. The economy is in equilibrium when mean fertility just replaces the population, i.e. $n^{\text{rep}} = 2/(1-\mu)$. I define the equilibrium income $\bar{y}^M$ as the income level where a couple with mean endowment exactly replaces itself; i.e. where $n^*(\bar{y}^M) = n^{\text{rep}}$.[19] Plugging these conditions into (4) and solving for income implies that mean endowments have no effect on mean income in equilibrium: $\partial\bar{y}^M/\partial\bar{x}_g = 0$. Instead, mean endowment sets the equilibrium population, which scales as $N^M \propto \bar{x}_g^{(1-\alpha)/\alpha}$. Any output gain from greater mean endowment is absorbed by a larger population: $\partial N^M/\partial\bar{x}_g > 0$ and $\partial\bar{y}^M/\partial\bar{x}_g = 0$. This is why the Malthusian era can produce selection on a growth-enhancing trait without any growth in mean income: one might expect a rise in mean endowment to lift living standards, but in equilibrium the wage falls to offset it. Thus, in equilibrium, selection operates only on relative endowments instead of on absolute income (see Appendix A).

2.6 Selection

With fertility a function of relative endowment, and stationary endowment variance, the generation-to-generation change in mean endowment is:

$$\Delta\bar{z}_g = \lambda\, S - (1-\lambda)\bar{z}_{g-1}. \tag{6}$$

An increase in mean endowments requires two multiplicative components: intergenerational transmission and a selection differential. However, since $\lambda < 1$, regression to the mean, captured by the second term in (6), works against selection. Thus, in equilibrium, selection cannot deliver sustained growth in mean endowments. Instead, it can deliver a bounded increase to a fixed point. Iterating the change equation (6) forward from $\bar{z}_0 = 0$ yields the change in mean endowment by generation T, and by evaluating this function at

[18] Blanc and Wacziarg (2025) study how migration as a Malthusian valve could reduce demographic pressure and accelerate growth.

[19] This abstracts from income dispersion. Since $n^*(y_i)$ is strictly concave and increasing above subsistence, $\bar{n}^* < n^*(\bar{y})$. Thus, if the income distribution sits above subsistence, $\bar{y}^M$ understates the actual income necessary for replacement.

$T \to \infty$, I can find the fixed point $\bar{z}^*$:

$$\bar{z}_T = \frac{\lambda S}{1-\lambda}\left(1-\lambda^T\right) \qquad \text{and} \qquad \bar{z}^* = \frac{\lambda S}{1-\lambda}. \tag{7}$$

Thus, the selection differential S and transmission λ jointly pin down the size and pace of the increase.

2.7 Summary

Proposition 1 (Necessary Condition: Fertility Gradient)**.** *The long-run shift $\bar{z}^* = \lambda S/(1-\lambda)$ is positive only if the selection differential $S > 0$. Writing S through the fertility gradient $\beta = \mathrm{Cov}(n_{i,g-1}, z_{i,g-1})/\sigma_z^2$, the variance of log endowments σ_z^2, and mean fertility $\bar{n}$ gives $S = \beta\,\sigma_z^2/\bar{n}$. Selection is therefore possible only if the fertility gradient $\beta > 0$.*

Corollary 1 (Variation in Status)**.** *The selection differential $S = \beta\,\sigma_z^2/\bar{n}$ is increasing in the fertility gradient and in the dispersion of status, and is stronger the lower mean fertility is, i.e. as the economy nears natural replacement. Measured in standard deviations of log endowment — the units used throughout — the relevant quantity is the per-standard-deviation fertility differential $\beta\sigma_z$, so drift scales with $\beta\sigma_z/\bar{n}$.*

Proposition 2 (Bounded Selection & Intergenerational Transmission)**.** *For $\lambda \in (0,1)$, selection delivers a one-time level shift, not sustained growth, and the lifetime gain is increasing and convex in λ, diverging only as $\lambda \to 1$. Thus, sufficient selection to see a large increase in mean endowment necessitates high intergenerational transmission.*

Proof. All proofs are collected in Appendix A. □

Propositions 1 and 2 describe the parameter space for selection in a Malthusian equilibrium. A fertility differential is necessary but not sufficient. Only high intergenerational transmission can deliver a large increase in mean endowment. The necessary level of intergenerational transmission is decreasing in the fertility differential. Panel (a) of Figure 1 summarizes this relationship. Each cell represents a combination of λ and $\beta\sigma_z$ at $\bar{n} = 3$, and its color corresponds to the increase in mean endowment that parameter combination delivers over ∞ generations. Panel (b) illustrates the bounded nature of endowment drift; here I plot the trajectory of mean endowment over 50 generations at different levels of λ.

The core evidence in this paper will come from estimating λ, β, and σ_z^2 in a pre-Industrial German setting to probe the viability of selection. The model implies that increasing mean endowment by one standard deviation requires exceptionally high intergenerational

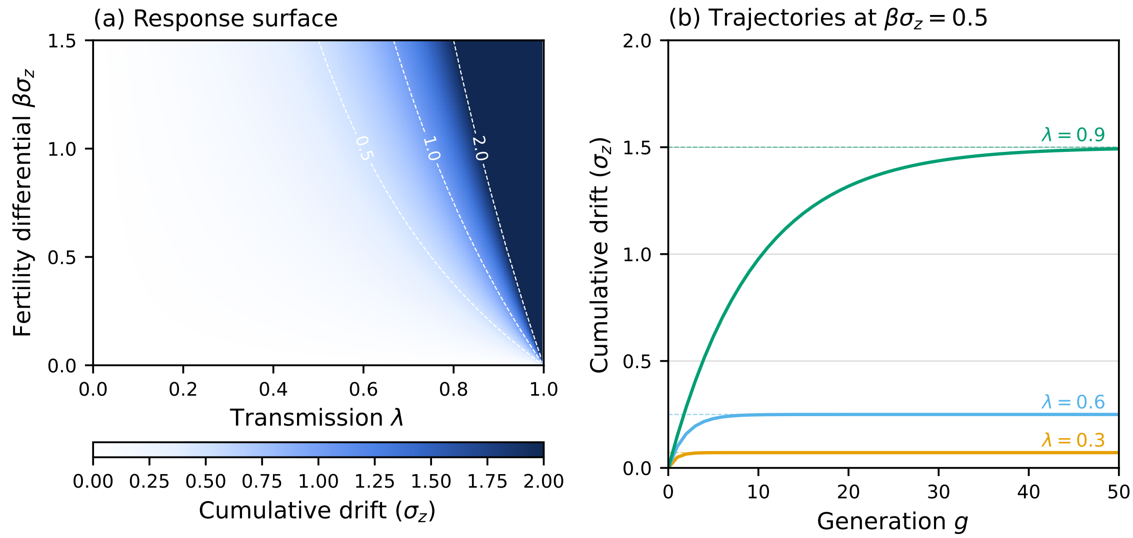

Figure 1: Cumulative Endowment Drift under Selection on Endowments.

Notes: The figure illustrates the model's comparative statics for cumulative status drift, measured in standard deviations of log endowment. Quantities are normalized ($z_0 \sim N(0, 1)$, so that $\sigma_z = 1$ and $\beta\sigma_z = \beta$; $\bar{n} = 3$). These calibrations are chosen to illustrate the dynamics of selection. Panel (a) plots long-run cumulative drift over the $(\lambda, \beta\sigma_z)$ plane; dashed white contours mark drift of 0.5, 1.0, and 2.0 standard deviations. Panel (b) traces the transient path of cumulative drift over 50 generations at a fixed per-standard-deviation differential $\beta\sigma_z = 0.5$ for $\lambda \in \{0.3, 0.6, 0.9\}$; each series converges to its steady-state fixed point (dashed horizontal lines), with higher λ producing larger and more slowly realized drift.

transmission, averaging $\lambda \approx 0.81$ along the one-standard-deviation contour in Figure 1, and a positive fertility gradient $\beta > 0$.

3 Data and Background

This section introduces the setting used to estimate these parameters. Because census or population-registry data of sufficient granularity are seldom available before the mid-19th century, studying pre-Industrial demographic behavior requires distinct sources (Campbell 2015). In this paper, I leverage the *community reconstitution* for the historical principalities of Wittgenstein (Mehldau 2011). Community reconstitutions contain linked life histories for all members of a specific 'community'.[20] Life histories are based on ecclesiastical records of baptisms, marriages, and deaths. Due to the labor-intensive process of linking demographic events, reconstitutions tend to focus on a single parish, or at most a collection of parishes, and rarely capture urban populations (Blanc 2023).

[20] Community reconstitutions are the non-academic analog to family reconstitutions, often compiled by hobby genealogists (Knodel and Shorter 1976).

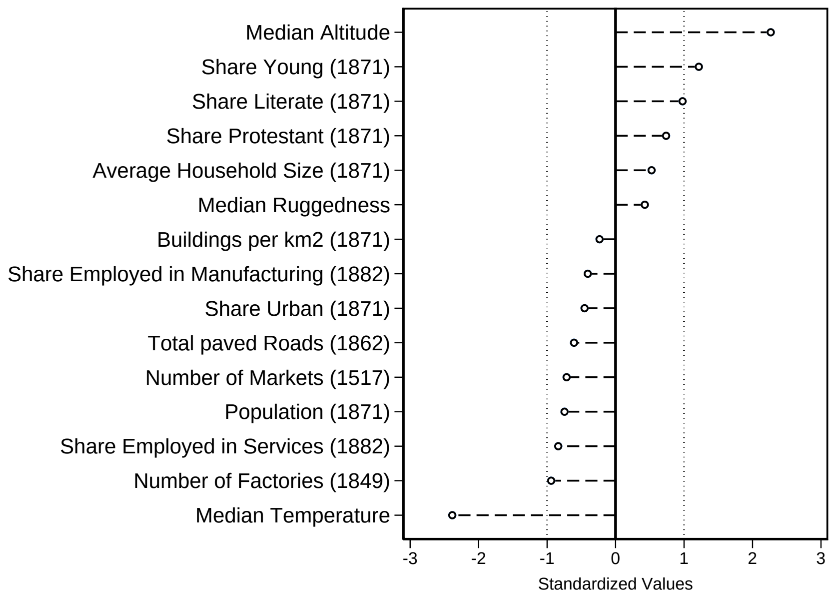

Figure 2: Wittgenstein Compared to the Prussian Average

Notes: Each marker reports the Wittgenstein value as a standardized deviation (z-score) from the mean of (western) Prussian counties; the solid line at zero is the Prussian average and the dotted lines mark ± 1 standard deviation. *Sources:* Religion, age structure, urbanization, household size, and manufacturing/services shares: Kersting et al. (2020), from the 1871 Population and 1882 Occupation Censuses. Literacy (1871): Becker and Woessmann (2009). Road length (1862), building density (1871), and factory counts (1849): ifo Prussian Economic History Database (Becker, Cinnirella, et al. 2014). Market-rights grants ($\sim$1517): Cantoni and Yuchtman (2014). Population and boundaries: Galloway Prussia Database (Galloway 2007). Altitude and ruggedness (TRI, Riley et al. (1999)): USGS GTOPO30 (U.S. Geological Survey 1996). Temperature: CRU 1961–1990 climatology (New et al. 2002).

The Wittgenstein reconstitution is exceptional for containing the universe of ecclesiastically recorded demographic events across two sovereign principalities. Further, compared to other amateur genealogical sources, the scientific rigor (i.e. citing the specific source for each event) makes this source particularly valuable. The dataset encompasses 150,000 individuals across 42,000 couples (Mehldau 2011). The approach of the Wittgenstein reconstitution is micro-historical. Instead of looking at a broad sample of remote parishes, I observe one cluster of neighboring parishes. This is advantageous since much early-modern migration occurred over short distances (e.g., neighboring parish); hence, in my sample, fewer life histories are censored by migration and more intergenerational links are preserved (Clark 1979; Patten 1976).[21]

Moreover, Wittgenstein constitutes a valuable case study of rural German demographic behavior. Before the dissolution of the Holy Roman Empire in 1806, the territory was

[21] Figure A13 compares Wittgenstein with the Cambridge Group 26-parish reconstitution (Wrigley et al. 1997) on the share of recorded births that never acquire a death record, the observable trace of a life history leaving the source. Over 1600–1760, before register closure censors either source, the share is 31% in Wittgenstein against 53% in the Cambridge parishes.

split between the two principalities of *Sayn-Wittgenstein-Hohenstein* in the south and *Sayn-Wittgenstein-Berleburg* in the north (Köbler 2007). Protestantism was adopted early; most of the population was Reformed Protestant, with a sizable Lutheran minority and smaller Roman Catholic and Jewish ones.[22] Given its mountainous geography, extensive forests, and poor agricultural suitability, Wittgenstein was characterized by fragmented farming instead of larger estates. Compounded by a partible inheritance structure, this meant that most inhabitants practiced some degree of subsistence agriculture.

By the 18th century, the geography that initially impeded Wittgenstein's economic fortunes favored the development of proto-industries in metallurgy and forestry, primarily for the production of charcoal to its more industrialized neighbors (Klein 1935). Although I do not observe socioeconomic observables during my study period, comparing Wittgenstein with other western Prussian counties at the end of the 19th century across several socioeconomic observables suggest reasonable representativeness (see Figure 2). Aside from the geographic variables (temperature and altitude), most are within one standard deviation of the mean western Prussian county. The picture of late 19th-century Wittgenstein that emerges is one of a somewhat economically backward region, with a young and relatively more educated population. Although it would be misguided to claim that these principalities are wholly representative of German demographic behavior, Wittgenstein is a pertinent case for demographic behavior in Germany.[23]

Sample Construction. Since community reconstitutions often link records from outside the core area of the study, they are at risk of over-sampling genealogies of particular interest to contributors or greater ease of access. The Wittgenstein one-place study contains complete reconstitutions for the 16 parishes of Wittgenstein and partial reconstitutions from other parishes (mostly neighboring Siegen). Although the reconstitution contains records from as early as 1525, observations from before 1600 are excluded. An 1876 law transferring the responsibility for recording demographic events from ecclesiastic to secular institutions marks the end of parish registers as a complete source. To ensure that I observe full fertility histories, only marriages prior to 1850 are included when I estimate the fertility gradient. When estimating intergenerational transmission, the sample is restricted to sons who married before 1876.[24] Throughout, to ensure estimates are not biased by the effect of remarriage, only bachelor-spinster marriages are included.

[22] Of the 16 parishes, 11 were Reformed Protestant, 1 Lutheran Protestant, and 4 Roman Catholic. After the sample restrictions set out below, only 159 of the 8,861 couples in the fertility sample are drawn from the Catholic parishes, so I am underpowered to study denominational differences.

[23] Figure A7 compares the distribution of occupational status in Wittgenstein against that in the German community books, with no sample restrictions imposed on either. The two distributions largely overlap, although Wittgenstein sits somewhat higher (mean HISCAM 58.4 against 55.2) and has a longer upper tail.

[24] Figure A2 plots the temporal distribution of marriages and births.

Table 1: Summary Statistics

Panel A: Demographic Variables						
	Mean	Std. dev	Min.	Max.	N	Level
Gross Fertility	5.29	2.98	0	17	8,861	Couple
Net Fertility	3.63	2.31	0	12	8,861	Couple
P(Childless)	0.05	0.21	0	1	8,861	Couple
Marriage Age: Mother	24.76	5.55	11	45	8,861	Couple
Marriage Age: Father	28.51	6.00	15	67	8,861	Couple
Birth-interval (avg. months)	35.14	15.18	0	388	7,810	Couple
Age at last birth: Mother	37.66	6.19	15	55	8,442	Couple
HISCAM^h	58.52	11.69	41	99	4,388	Couple
P(AgeDeath $\leq$ 5)	0.25	0.44	0	1	46,877	Birth
P(AgeDeath $\leq$ 15)	0.31	0.46	0	1	46,877	Birth
HISCAM^f	59.36	12.11	41	99	22,988	Birth

Panel B: Intergenerational Transmission			
Generation	Mean	Std. dev	N
Son (G3)	4.02	0.18	4,493
Father (G2)	4.05	0.19	4,493
Grandfather (G1)	4.08	0.20	2,755

Notes: Panel A reports summary statistics for the variables used in the fertility differential estimation, drawn from the couple-level and the birth-level sample. For birth intervals the zero-month minimum represents multiple births (e.g. twins) and the upper bound are verified outliers. Panel B reports summary statistics for the three-generation sample used in the intergenerational-transmission estimation; the reported variable is $\ln$(HISCAM) for each generation (G3 = son, G2 = father, G1 = grandfather).

When estimating the fertility gradient, I only include couples whose marriages were recorded in the core parishes, constituting the complete reconstitution. Individuals are not observed across their full lifetime but instead only enter observation at discrete instances when specific demographic events occur. Since migration is an unobserved event, I need to account for the migration-induced censoring of life histories (Campbell 2015). To this end, I only include non- and in-migrants prior to marriage who are observed up until their local deaths.[25] These restrictions result in a sample of 8,861 couples and 46,877 births (see Panel A, Table 1).

For intergenerational transmission, I impose less severe sample restrictions. To estimate status elasticities, I need to observe occupation for two subsequent generations (GX & G[X-1]). Fathers (sons) who are linked to sons (fathers) outside the core parishes do not bias the estimates. Delger and Kok (1998) outline how marriage registers – observing both father's and son's status at the instant of the son's marriage – underestimate mobility

[25] I also run regressions based on a sample that is only restricted by mothers' death, allowing for the out-migration of fathers. This has a negligible effect on results; hence, only the stricter restriction is reported throughout. For a full discussion of how migration can color my results, see Appendix F.

due to the difference in career progression. My sample is limited to fathers (G2) that I observe at marriage (either in or outside of Wittgenstein) and their legitimate children (G3). Because an individual's occupational status in the data is predominantly recorded at their own marriage, averaging these observations compares fathers and sons at roughly the same career stage.[26] Since my sample is fully hand-linked, I avoid concerns regarding false positives from automated record linkage (Bailey et al. 2020; Ó Gráda et al. 2023). Still, identifying the coefficient of transmission in a latent-variable model requires linked data across three generations. Leveraging the linkage depth of the reconstitution, I link backwards along the paternal line to find grandfathers (G1). The estimation sample contains 4,493 son–father (G3-G2) and 2,755 son–grandfather (G3-G1) links (see Panel B, Table 1).

3.1 Occupational Status & Endowments

Measurement Error. I never directly observe endowments $x_{i,g}$. Instead, I use occupational status $\tilde{y}_{i,g}$ as a noisy proxy for income $y_{i,g}$. I can write this as a simple classical measurement error model in logs:

$$\ln \tilde{y}_{i,g} = \ln y_{i,g} + e_{i,g} \qquad e \sim (0, \sigma_e^2), \quad e \perp y, \quad e_{i,g} \perp e_{i,g-1} \tag{8}$$

Combining (1) and (8) expresses occupational status as a function of log endowments;[27]

$$\ln \tilde{y}_{i,g} = \omega_g + z_{i,g} + e_{i,g} \qquad z_{i,g} \equiv \ln x_{i,g} \tag{9}$$

where $\omega_g = \ln w_g$ is the generation's wage intercept. If $e_{i,g}$ takes the form of classical measurement error, this will attenuate all estimates (Solon 1992). Although this issue is much acknowledged in the intergenerational mobility literature (e.g. Solon 1992; Ward 2023; Zhu 2024), prior studies of historical reproductive inequality do not deal with measurement error directly. Throughout, I will implement different measurement error correction and bounding strategies to recover unattenuated estimates.

From Occupational Descriptions to Status. The community reconstitution reports occupational descriptions for 24% of all men, corresponding to over 50% of couples in the analysis sample. The source does not report the occupation for subsistence farmers;

[26]The results are robust to conditioning on observing sons at marriage (see Table A1).

[27]This equation carries the assumption from subsection 2.1 forward that relative income is exactly equal to relative endowments. Introducing some returns-to-ability coefficient ρ such that $y_{i,g} = w_g x_{i,g}^{\rho} \Leftrightarrow \ln y_{i,g} = \ln w_g + \rho z_{i,g}$ does not affect λ; it scales the absolute selection differential S, but has no effect on selection *relative* to the distribution of endowment. Importantly, ρ is not identified in the data. Thus, I choose to abstract from it throughout.

thus, a substantial proportion of the remainder were likely smallholders.[28] For a subset of individuals, I observe multiple occupations, whether due to occupational mobility, differing occupational names, or because people pursued several trades.

To reduce researcher degrees of freedom and improve replicability, occupational descriptions are mapped to occupational status using natural-language methods. I implement parallel approaches to ensure the robustness of these mappings. The primary approach prompts a frontier model directly (Ferrara 2026). After deterministic string cleaning, I prompt the Anthropic Batch API in multiple steps.[29]

In *Stage A*, occupational descriptions are parsed, yielding 2,620 unique occupation titles from 5,829 unique occupation strings. In *Stage B*, these titles are translated to English and assigned to discrete status classes based on their (a) human capital, (b) economic capital or land, and (c) social capital requirements. Table 2 summarizes the classification and reports summary statistics for the classes.[30] *Stage C* assigns occupations to HISCO codes, a standard historical occupational classification scheme. This step lets me map occupational titles to a continuous measure (0–100) of occupational status (HISCAM) (Lambert et al. 2013).[31] When I observe multiple observations per individual, I average across the continuous measure, and take the highest of the discrete categories.[32]

I am able to validate the mapping produced by the primary approach against alternative LLM, classifier, and manual approaches. I use a pre-trained classifier that assigns occupational titles to HISCO codes (Dahl et al. 2024). The model receives the same pre-cleaned strings as the frontier LLM pipeline and returns HISCO codes, which are mapped to HISCAM. Additionally, in an earlier version of this project, I manually mapped occupational titles to HISCO (Ohler 2024). Last, to verify the assignment to discrete classes, I conduct an invariance check and use a model from another provider with the same input and prompt. Table A6 reports correlations and inter-rater agreement across the two HISCO/HISCAM benchmarks and, for the discrete scale, the second-provider invariance check. While agreement for the exact HISCO scores is moderate (∼60%), agreement across the broader categories is substantially higher (∼77–92%), and the correlations between the HISCAM scores are high across the board (0.78–0.92). For the discrete class scale, the invariance

[28] I alternatively exclude this group, or include it as a distinct category. The results are robust to either. See Table A2.

[29] For a more detailed discussion of this full approach, including all string cleaning steps and API prompts, see Appendix D.

[30] Table A3 reports the ten most common occupations per class. Additionally, I follow best practices and ask the LLM to justify each assignment to aid replicability (Ferrara 2026).

[31] HISCAM is based on the observed stratification of social interactions in historical societies. Throughout, I use the universal HISCAM scale, since the German-specific scale relies on a small sample.

[32] Averaging across multiple occupations reduces attenuation from measurement error, while taking the highest status occupation lets me account for life-time mobility. All results are robust to using different measures and status mappings; see Table A4 and Table A5.

Table 2: Status Classes

	Classification: Capital			Summary Statistics		
Class	Human	Physical	Social	N Couples	N Births	$\mathbb{E}$(HISCAM)
Lower	1.10	1.00	1.08	547	2,438	47.29
Lower-middle	2.13	1.74	2.02	798	3,922	53.39
Upper-middle	2.97	2.50	3.12	2,834	15,363	58.87
Upper	3.65	2.11	3.98	463	2,551	78.03
Unobserved	–	–	–	4,219	22,603	–

Notes: Each occupation string is classified by a large language model into a 4–level scale for human, physical, and social capital; social class is the discrete summary of this decomposition. N Couples counts couples at the husband's status; N Births counts children at the father's status; $\mathbb{E}$(HISCAM) is the class-conditional mean HISCAM score. The *Unobserved* category comprises couples and births with no recorded occupation string.

check shows that the two models assign the same class 84% of the time.

4 Reproductive Inequality

I now test Proposition 1 by estimating the fertility gradient β in log endowment $z_{i,g}$. Selection is only possible if endowments – proxied by occupational status – have a positive effect on net fertility. Net fertility is a composite of how many children were born (Malthusian preventive check) and how many died before reaching reproductive age (positive check). I investigate these components independently by estimating:

$$Y_i = \eta_{p,t} + \beta \, OccStat_i + \epsilon_{i,p,t} \tag{10}$$

where Y_i is the outcome of interest. i indexes the couple or, when estimating mortality, the individual birth. The model includes parish-decade fixed effects $\eta_{p,t}$ to account for local reporting practices and temporal differences.[33] The exposure variable $OccStat_i$ is a measure of occupational status: either discrete status categories $\sum_u \mathbb{1}\{Occ_i = SES_u\}$ or a continuous logged measure $\ln(\text{HISCAM}_i)$.[34] All baseline regressions are estimated using OLS, with standard errors clustered at the parish level.[35] Parish-specific occupational structure and recording practices induce correlation in the regressors and errors, which

[33] A separate intercept per decade absorbs the generation-specific wage term $\ln w_g$.

[34] I choose this concave (or non-parametric) functional form to account for the biological limit of fertility (Kumon 2026, p. 159).

[35] The results are robust to estimation using PPML (for fertility) and logistic (for under-15 mortality) models; see Table A7.

Table 3: Reproductive Inequality

	Gross Fertility		$1(\text{AgeDeath} \leq 15)$		Net Fertility	
	(1)	(2)	(3)	(4)	(5)	(6)
ln(HISCAM)	1.775***		0.012		1.135***	
	(0.183)		(0.032)		(0.231)	
Lower-middle Class		0.505**		0.012		0.290**
		(0.177)		(0.016)		(0.132)
Upper-middle Class		0.908***		0.009		0.570***
		(0.120)		(0.015)		(0.107)
Upper Class		1.010***		0.011		0.622***
		(0.134)		(0.019)		(0.138)
Mean DV	5.235	5.224	0.305	0.306	3.632	3.620
Observations	4,367	4,623	22,987	24,274	4,367	4,623
Parishes (clusters)	15	16	16	16	15	16
R^2	0.095	0.094	0.024	0.024	0.100	0.097

Standard errors clustered by marriage parish in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
Notes: OLS estimates. Columns (1)–(2) are estimated at the couple level with gross fertility as the outcome; columns (3)–(4) are estimated at the birth level with an indicator for death at or before age 15 as the outcome (status measured by the father's occupation); columns (5)–(6) are estimated at the couple level with net fertility (gross fertility minus adjusted under-15 mortality) as the outcome. Odd columns use ln(HISCAM) as a continuous status measure; even columns use discrete social-class dummies (the lowest class omitted as the reference category). All specifications include marriage-decade-by-parish fixed effects.

would bias unclustered standard errors downward (Cameron and Miller 2015).[36]

Gross fertility is the number of children ever born to a couple. Childhood mortality is estimated at the individual level, where Y_i is an indicator variable equal to one if the age at death is 15 or younger. I use a repeat-naming approach to adjust for the under-reporting of infant deaths (Houdaille 1976; Cummins 2020).[37] This approach assumes that I observe child-deaths prior to 15 even if life histories are censored, since children were likely to live with their parents up to 15. Last, net fertility is gross fertility minus adjusted under-15 mortality at the couple level.

Table 3 reports the results. Columns (1) and (2) show a statistically significant positive association between occupational status and gross fertility. The magnitude of the fertility gradient is substantial; according to column (1), the fertility differential between the lowest and highest deciles of the status distribution is $1.775 \times \ln(87.2/45.1) = 1.170$.[38] Column (2)

[36]With 15–16 clusters, I report wild-cluster-bootstrap p-values (Cameron, Gelbach, et al. 2008) and robustness to the finer Parish×Decade level in Table A8.

[37]In pre-Industrial Europe, when a child died, the subsequent child was often given the same forename. Therefore, where a child has a subsequent sibling of the same name and is not linked to a burial record, it is assumed to have died as an infant. I compare siblings based on the string distance (Jaro-Winkler) between names. This approach enables me to also catch cases where parents reused names for the other gender, e.g., Peter (male) and Petra (female).

[38]To compute the total fertility differential, I take the mean occupational status of the top decile and

reveals a similar fertility differential; couples of the highest status class had 1.010 additional children compared to those of the lowest class. The relationship between occupational status and fertility is non-linear across the classes. The linear-log specification in column (1) accounts for this non-linearity: the fertility gradient levels off at higher occupational status. Assuming couples begin childbearing at age 25, continue until age 45, and maintain an average birth interval of 3 years, the maximum expected fertility is 6.7 births. As couples approach this limit, the marginal effect of occupational status becomes mechanically constrained.

The association between under-15 mortality and occupational status in columns (3) and (4) is estimated using a linear probability model. The coefficients correspond approximately to changes in the probability of dying before age 15. All estimates are statistically insignificant, and the point estimates are economically small. The probability of dying before age 15 is essentially flat across the status distribution, at $\sim$0.31 for both the lowest and the highest status decile. This finding is consistent with evidence from France and England (Boberg-Fazlic et al. 2011; Clark and Cummins 2015b; Cummins 2020). Given the dominance of infectious diseases as causes of under-15 mortality, the absence of an association is unsurprising. The better living standards of higher status couples did little to curb high childhood mortality from infectious diseases. Occupational status and income only became relevant in periods of sustained resource shortages when children of wealthier couples were less exposed to malnutrition. Kelly and Ó Gráda (2014) find that in late-medieval England, a status gradient in mortality only emerged in periods of sustained famine. In a similar vein, Malthus himself described famine as "the last, the most dreadful resource of nature" (Malthus 1803, p. 139). I extend this analysis to other age bands in Figure A3. The only statistically significant effect (at 99%) is a positive coefficient for women aged 25–45. This is likely the product of higher fertility among higher status couples, and thus greater maternal mortality in childbirth.

Given the absence of a statistically significant association between under-15 mortality and occupational status, the gradient in net fertility closely mirrors that in gross fertility. Based on the coefficient in column (5), couples in the top decile had $1.135 \times \ln(87.2/45.1) = 0.748$ additional surviving children. According to the discrete categories, the highest status class had 0.622 additional surviving children.[39]

the bottom decile of the status distribution: $E[HISCAM \mid HISCAM > q90] = 87.2$ and $E[HISCAM \mid HISCAM < q10] = 45.1$.

[39] One concern is that I observe a *conditional* fertility gradient. Although conditioning on marriage (see Table A22) and on observing an occupation (see Table A2) appear to not color my results, I attempt to measure an alternative *unified* fertility gradient. I re-estimate (10) where the dependent variable is the number of children per family I observe at marriage (see Table A9).

4.1 Mechanism

Why did couples with high occupational status end up with more children? Baudin et al. (2015) decompose group level fertility into an intensive and extensive margin. The intensive margin corresponds to the number of surviving children per couple. The extensive margin is determined by the share of all potential couples that do not have children, namely couples that remain childless and individuals who remain celibate. De la Croix et al. (2019) show that accounting for the extensive margin changes fertility differentials in England; due to celibacy and childlessness, the middle class out-reproduces the upper class. To explore whether this is the case in Germany, I regress indicator variables for childlessness and celibacy on $\ln(\text{HISCAM}_i)$. I find that, among the observed sample, the extensive margin of fertility did not vary with occupational status. Instead, reproductive inequality operated fully through the intensive margin.[40]

The intensive margin of gross fertility is a function of when reproductive behavior begins (starting, operationalized as *Mother's age at marriage*), when reproductive behavior ceases (stopping, *Mother's age at last birth*), and how frequently births occur within this period (spacing, *Birth Interval*).[41,42] Columns (1)–(3) of Table 4 report coefficients from regressing these demographic variables on the husband's occupational status $\ln(\text{HISCAM}_i)$. The status gradient in fertility is driven by starting and spacing. Comparing couples of the top to the bottom occupational status decile, women married 3.29 years earlier and had 4.30 months shorter birth intervals.[43] Reproductive behavior ceased around 39.4 years irrespective of occupational status.

To better understand the relative contribution of these factors, I conduct a simple decomposition exercise. In column (4) I regress gross fertility on age at marriage, age at last birth, and the average birth interval to obtain β_{start}, β_{stop}, and β_{space}; I also include $\ln(\text{HISCAM})$. The latter is insignificant, suggesting that there is no direct effect of occupational status on gross fertility, and that the entire effect runs through starting, stopping, and spacing. I obtain the indirect effects of occupational status via starting δ_{start}, stopping δ_{stop}, and spacing δ_{space} by taking the product of $\beta_{\text{start/stop/space}}$ and the coefficients in columns (1), (2), and (3), respectively. The total effect is $\delta = \delta_{\text{start}} + \delta_{\text{stop}} + \delta_{\text{space}} \quad \Leftrightarrow \quad 1.854 = 1.574 - 0.097 + 0.377$. Thus the relative contribution of starting to the fertility differential is

[40] For a fuller discussion, see Appendix G.

[41] Identifying deliberate birth spacing is complicated since it is subject to a range of non-volitional factors (Knodel 1987; Cinnirella et al. 2017; Clark and Cummins 2019). However, since I am interested in the mechanical mediators of fertility, I can simply operationalize spacing as the average interval.

[42] I report equivalent regressions for Father's age at marriage and last birth in Table A10. Fathers with high occupational status also marry earlier. While age at last birth does not vary statistically significantly in status for mothers, it does for fathers. This is driven by wealthier men marrying younger women, and thus being older when they complete their reproductive period.

[43] I also estimate the status gradient in birth spacing at the individual birth-interval level in Table A11.

Table 4: Starting, Spacing, and Stopping

	Age at Marriage	Age at Last Birth	Birth Interval	Gross Fertility
	(1)	(2)	(3)	(4)
ln(HISCAM)	-4.983***	-0.275	-6.527***	0.125
	(0.497)	(0.553)	(2.177)	(0.145)
Age at marriage (mother)				-0.316***
				(0.011)
Age at last birth (mother)				0.354***
				(0.005)
Birth interval (avg.)				-0.058***
				(0.005)
Mean DV	25.815	39.370	36.328	6.115
Observations	2,709	2,588	2,445	2,445
Parishes (clusters)	15	15	15	15
R^2	0.164	0.089	0.132	0.828

Standard errors clustered by marriage parish in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
Notes: OLS estimates at the couple level, restricted to the full-fertility-period sample with non-missing HISCAM. Column (1) reports age at marriage (mother); column (2) reports age at last birth (mother); column (3) reports the average inter-birth interval (months); column (4) reports gross fertility, controlling for age at marriage, age at last birth, and the average birth interval. The exposure variable is ln(HISCAM), the husband's occupational status. All specifications include marriage-decade-by-parish fixed effects.

85% and that of spacing 20%, while stopping contributes -5%.

The relevance of starting is comparable to findings from other European (Boberg-Fazlic et al. 2011; Cummins 2020) and non-European settings (Kumon 2026). This likeness across different societies is striking, and even where starting was not the primary mechanism, strong fertility gradients in wealth/status were present (Feng et al. 1995; Campbell and Lee 2002; Bandyopadhyay and Green 2013; Kumon and Saleh 2023; Hu 2023). This suggests that variation in the fertility differential across settings is an unlikely candidate explanation for why some places developed earlier than others.[44]

The contribution of spacing to the fertility differential is a novel result in cross-section. This finding clashes with Knodel (1987), who concludes that there was no deliberate spacing in pre-Industrial Germany.[45] However, even in the absence of deliberate spacing, non-volitional factors that affect birth intervals – e.g. breastfeeding and nutrition (Jaadla et al. 2020; Thiehoff 2015) – could lead to an occupational status gradient in spacing. For example, men from low income households often traveled to neighboring areas for seasonal

[44] Cummins (2020, p. 16) speculates whether different fertility gradients across England and France contributed to France's relatively slower industrialization.

[45] Dribe and Scalone (2010) use survival analysis on the same data as Knodel to argue for deliberate spacing. They cite the rapid fertility response among poorer couples in times of economic stress as evidence for deliberate adjustment via spacing.

work, leading to absences of several months (Klein 1935). Such absences mechanically increase birth intervals for low status couples. This form of labor migration was common across Germany, and increased in times of economic hardship. As such, it is a potential explanation for the variation in spacing, both across classes and time.

Another noteworthy feature of the age at marriage result is that irrespective of status, age at marriage was quite high – on average men were 29 and women 25 when they married. Together with relatively high rates of celibacy (17% for women, 26% for men), this result concurs with existing evidence regarding the European marriage pattern (EMP) (Hajnal 1965; Voigtländer and Voth 2013). While existing work highlights the role of the EMP in reducing demographic pressure, and thus raising living standards, my model implies that it could also increase selection in an evolutionary growth framework. Later ages at marriage reduce average fertility $\bar{n}$, and according to Corollary 1 this increases the selection differential S.

4.2 Measurement Error

One concern when interpreting the magnitude of these differentials is that measurement error attenuates my estimates (see Equation 8). The key parameter of interest is the semi-elasticity β of net fertility n_i in log endowments z_i. But since I only observe a noisy proxy for endowments, my empirical strategy recovers $\widehat{\beta}_{\text{OLS}} = \theta_\beta \, \beta$ where $\theta_\beta = \text{Var}(z) / \left(\text{Var}(z) + \sigma_e^2\right)$ is the reliability ratio.[46,47] The conventional approach to correcting for this measurement error is to use a repeated independent measure of occupational status as an instrument. However, the repeated measures approach does not work well with reconstitution data; not only is the population with multiple recorded occupations strongly selected, but if multiple are recorded, they are most often not independent measures of the same occupational status, but instead snapshots at different parts of the occupational life-cycle (e.g., marriage and death).[48]

In the absence of an appropriate IV, I conduct a bounding exercise by using a strong instrument that (in all likelihood) breaks the exclusion restriction. I leverage the generational depth of my sample and instrument own occupational status $\ln(\text{HISCAM}_{i,g})$ with father's occupational status $\ln(\text{HISCAM}_{i,g-1})$.[49] The exclusion restriction is violated if fathers have

[46] I already reduce attenuation slightly by averaging across multiple occupational observations where available. Thereafter $\theta_\beta = \text{Var}(z)/(\text{Var}(z) + \sigma_e^2 E[1/m_i])$ where m is the average number of occupational observations per couple.

[47] This is assuming that the parish-decade fixed effects account for any unobserved variable bias.

[48] See Table A12 for the number and reliability of multiple occupational observations.

[49] Table A13 reports estimates obtained from several instruments such as another occupational observation, or a different occupation-status mapping. Across the board all IV estimates suggest that σ_e^2 is substantial and $\widehat{\beta}_{OLS}$ is attenuated.

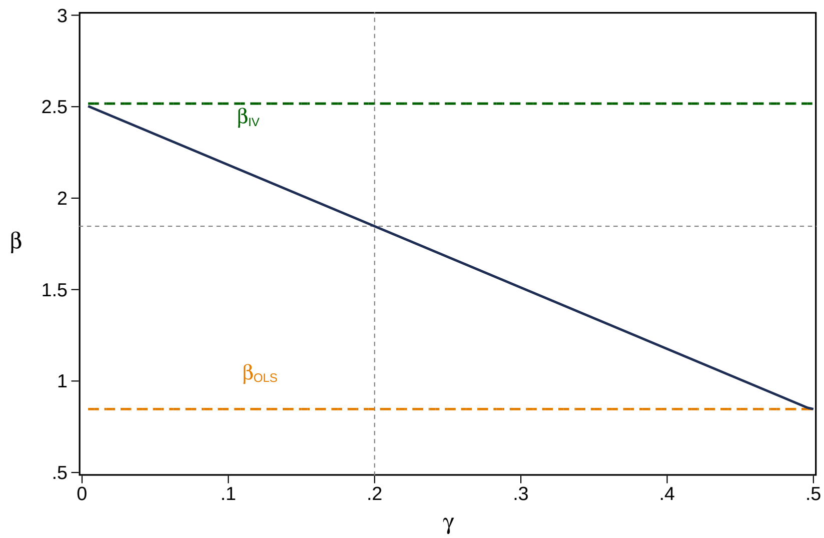

Figure 3: Partial Identification of the Fertility Differential

Notes: This figure depicts the relationship between the unbiased estimate of the fertility differential β and potential violations of the exclusion restriction described by the direct effect of the prior generation's occupational status on fertility γ. The true coefficient is bounded from above by the potentially inflated β_{IV} and from below by the attenuated β_{OLS}. Both are estimated on the couples with an observed grandfather, with marriage-decade-by-parish fixed effects and standard errors clustered by marriage parish.

an effect γ on the fertility of their offspring. If this is the case, $\beta_{\text{IV}} = \beta^* + \gamma/\varphi$ where φ is the first stage coefficient.[50] Thus, assuming $\gamma > 0$ and $\varphi > 0$, i.e. higher endowment parents have children with higher fertility and higher endowments, β_{IV} bounds the true β from above;

$$\widehat{\beta}_{\text{OLS}} \leq \beta \leq \widehat{\beta}_{\text{IV}}. \tag{11}$$

Figure 3 reports the results of this partial identification approach and plots the semi-elasticity β as a function of the exclusion restriction violation γ.[51] While I am unable to pin down the exact value of β, I observe that $\beta \in (0.847, 2.517)$.[52] When considering the German parameter space I will evaluate the model across this range. Since β is always greater than zero, I can conclude that the first necessary condition for selection under evolutionary growth set out by Proposition 1 is satisfied.

[50] This is assuming a structural equation: $n = \beta^* z + \gamma W + u$ where W is the father's status.

[51] See Table A14 for the underlying OLS and 2SLS estimates.

[52] The lower bound is smaller than column (5) of Table 3 because it uses a single occupational draw, whereas column (5) averages across observations, which already reduces attenuation from measurement error, and because these regressions use a subset of the full sample with two generations of status measures.

5 Transmission of Endowments

Next, I estimate the coefficient of intergenerational transmission of log endowment: λ. Studies typically estimate intergenerational elasticities β_{-1} by regressing log offspring income $\ln \tilde{y}_{i,g}$ on log parental income $\ln \tilde{y}_{i,g-1}$. However, interpreting this elasticity as a measure of intergenerational endowment transmission requires strong assumptions (Stuhler 2012; Braun and Stuhler 2018; Ward et al. 2025; Del Pizzo et al. 2026). Intergenerational elasticities only have a structural interpretation if I observe income without measurement error and there is no noise in the income-generating or intergenerational endowment transmission processes. This is an exceedingly strong assumption: as summarized by (1)–(2), even when I abstract from noise in the income equation, random luck in endowment transmission v and measurement error in my income proxy e inject substantial noise.

Stuhler (2012) shows that in a latent variable model, such as (2), intergenerational elasticities have a different structural interpretation.[53] The slope coefficient from regressing the observed status of the current generation $\ln \tilde{y}_{i,g}$ on the observed status of the prior generation $\ln \tilde{y}_{i,g-1}$ can be expressed as the product of the true coefficient of intergenerational transmission and a reliability ratio:

$$\beta_{-m} = \frac{\sigma_z^2}{\sigma_z^2 + \sigma_e^2} \cdot \lambda^m \tag{12}$$

The key insight is that, when estimated over m generations, the coefficient of transmission scales geometrically, while the reliability ratio only enters once.

If I carry forward my modeling choices, and assume stationary variances of underlying ability σ_z^2, endowment luck σ_v^2, and the measurement error σ_e^2, the reliability ratio is constant across generations. With these additional assumptions, I can recover λ via the *ratio* estimator:

$$\lambda = \frac{\beta_{-2}}{\beta_{-1}}, \tag{13}$$

where β_{-2} is the multi-generational elasticity between grandfathers and grandsons (G1–G3), and β_{-1} is the basic intergenerational elasticity between fathers and sons (G2–G3).[54]

Identifying Assumptions. Prior to estimating the coefficient of transmission λ, it is worth making explicit the identifying assumptions. First, I assume that the reliability ratio θ_λ is constant across generations. If the data quality changes from one generation to the next, or

[53] For a fuller discussion of this model and related indirect approaches, see Del Pizzo et al. (2026).

[54] As discussed in section 2, I abstract from assortative mating and interpret $\widehat{\lambda}$ as a reduced form combination of assortative mating and intergenerational transmission (Stuhler 2012).

if random noise in the endowment process changes across generations, λ is biased. Braun and Stuhler (2018) show that changes in θ_λ would have to be substantive and idiosyncratic to alter λ by much. Still, when λ is estimated by sub-period, where generations more closely resemble cohorts, systematic differences in θ_λ are a potential source of bias.

Second, the model assumes that endowment inheritance follows an AR(1) process (2). Although grandparent effects have received growing attention (Mare 2011; Ferrie et al. 2021), a substantial literature argues that they are statistical artifacts in the latent variable model (Braun and Stuhler 2018; Ward et al. 2025). The common tests for grandparent effects — excess persistence and multi-generational AR(2) regressions — both identify spurious effects in the presence of measurement error (Ward et al. 2025).[55] If the AR(1) model holds, correcting for measurement error should increase β_{-1} while decreasing the relative magnitude of multi-generational effects. I estimate multi-generational transmission in Appendix H; the pattern across coefficients suggests that grandfather effects are at least partially driven by measurement error. Given these results and the existing literature, I am confident in making the identifying assumption of no grandfather effects.

5.1 Coefficient of Transmission

Table 5 reports estimates of λ for different sub-samples. The first two columns report the number of multi-generational links that are used to estimate the elasticities across one β_{-1} and two β_{-2} generations. The coefficients, reported in the two subsequent columns, are estimated using OLS with fixed effects for father's marriage decade. The final column contains estimates for the coefficient of transmission $\widehat{\lambda}$. The first row reports results for the full sample, with all possible links. In subsequent rows, the sample is restricted to observations where I observe both the father's (G2) and grandfather's (G1) occupational status. I also split the sample based on the birth year of the youngest generation (G3). Rows three and four report the results for the pre- and post-1800 samples.

Across the board, the estimated coefficient of transmission is substantially larger than the intergenerational elasticities. Although this supports the contention that social mobility was lower than conventional estimation strategies imply, these estimates do not support Clark's "Law of Social Mobility", which states that the degree of persistence is constant across time and space at around 0.80 (Clark 2014). Although the point estimate changes across sub-periods, I fail to reject the null that the estimates are the same.[56]

[55] The first approach overstates the grandparent effect by construction because $\beta_{-2} = \theta_\lambda \lambda^2 > \beta_{-1}^2 = \theta_\lambda^2 \lambda^2 \quad \text{if} \quad \theta_\lambda < 1$. The second approach suffers a similar fate; coefficients are spuriously large and statistically significant due to contamination bias from the signal for the parental latent variable (Modalsli and Vosters 2024; Ward et al. 2025).

[56] These results are robust to using a different HISCO mapping, see Table A15, or to using more stringent

Table 5: Estimates of Intergenerational Transmission

	(1)	(2)	(3)	(4)	(5)	(6)
	N: G2–G3	N: G1–G3	$\widehat{\beta}_{-1}$	$\widehat{\beta}_{-2}$	$\widehat{\lambda}$	SE($\widehat{\lambda}$)
Full	4,493	2,755	0.396	0.252	0.636***	(0.073)
Restricted	2,755	2,755	0.425	0.252	0.593***	(0.053)
Early (pre-1800)	1,368	1,368	0.447	0.248	0.556***	(0.057)
Late (post-1800)	1,387	1,387	0.395	0.260	0.658***	(0.126)
Difference	-	-	-	-	-0.102	(0.138)

Cluster-bootstrap standard errors in parentheses (1,000 replications, resampled by the son's (G3) birth parish). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
Notes: This table reports $\widehat{\lambda} = \widehat{\beta}_{-2}/\widehat{\beta}_{-1}$, where $\widehat{\beta}_{-1}$ and $\widehat{\beta}_{-2}$ come from separate OLS regressions of the son's (G3) $\ln$(HISCAM) status on the father's (G2) and grandfather's (G1) average $\ln$(HISCAM) status, each absorbing fixed effects for the father's (G2) marriage decade. The unit of observation is a paternal lineage. Full uses all lineages with a G2–G3 or G1–G3 link; Restricted keeps only lineages with both links non-missing; Early and Late further split the Restricted sample by whether G3's birth year is on or before 1800 or after 1800.

The coefficient of transmission for pre-Industrial Germany is strikingly similar to estimates for 20th-century Germany by Braun and Stuhler (2018).[57] The Braun and Stuhler (2018) estimates pick up a century later, with the coefficient of transmission at approximately 0.60, comparable to the level of the early 19th century. Given different types of sources (genealogical data versus survey data) the similarity of the estimates is all the more remarkable. The coefficient of transmission may have oscillated in the intervening century, but the set of estimates suggests a stable range across three centuries.

These are the first estimates for the coefficient of transmission for pre-Industrial Germany. At a coefficient of 0.59, latent endowments were strongly transmitted. The difference between simple intergenerational correlations ($\beta_{-1} = 0.425$) and the estimated coefficient of transmission ($\lambda = 0.593$) is marked. The former implies that occupational status regresses to the mean in three generations — "from shirtsleeves to shirtsleeves in three generations." (Becker and Tomes 1986, S28) — while according to λ it takes close to five.

6 External Validity

I am drawing on a single community reconstitution; while the high quality of this data source increases the internal validity of my estimates, it raises concerns regarding their external validity. I draw on a large alternative data source to check external validity.

sample restrictions, see Table A1.

[57] Figure A4 plots estimates from Braun and Stuhler (2018) alongside estimates based on sub-periods from my sample.

Family reconstitutions were collected, partially standardized, and digitized by the German Association for Computer Genealogy (*Verein für Computergenealogie*). I scraped and anonymized the linked online records for 1,072 reconstitutions containing over 17 million unique individuals. Figure A5 plots the spatial distribution of these reconstitutions. Not all community reconstitutions follow the rigor and completeness of the Wittgenstein data. This is a smaller issue when estimating the intergenerational transmission of endowments, where I primarily rely on intergenerational links. However, when estimating fertility differentials, which require fully linked and uncensored families to reconstruct fertility, this is a virtually insuperable challenge (Campbell 2015). Still, with stringent sample restrictions, these data are useful for testing the external validity of my results, although I place less emphasis on the magnitude of the fertility estimates.

When estimating intergenerational transmission, I use all available linked grandfather–father–son (G1–G2–G3) lineages, a sample of 262,251 links. For the fertility differential, I start by dropping incomplete reconstitutions.[58] I then impose sample restrictions that are second-best to those of the main Wittgenstein sample, resulting in 225,093 couples. I use a variant of the status-mapping approach for these data. Due to the size of the data, I combine *Stage A* of the LLM process with the existing string-to-HISCO mapping and `OccCANINE` for the remainder. Based on this pipeline, I am able to assign occupational status to 2,702,653 individuals. Summary statistics are reported in Table A16.

I estimate equation (10) for gross fertility, under-15 mortality (at the couple-level), and net fertility. Column (1) of Panel A of Table 6 reports a positive and statistically significant gross fertility gradient, supporting the external validity of the findings in section 4.[59] The status gradient in under-15 mortality in column (2) is significant but economically small, implying that the top decile had a 1.31 percentage point lower under-15 mortality rate (sample mean 26.5%).[60] Net fertility in column (3) therefore tracks the gross fertility gradient: couples of higher occupational status ended up with more surviving children. The estimated fertility gradient is smaller than in Wittgenstein. But I am hesitant to interpret this as a qualitative difference, since censoring in the community books is likely status dependent, and since greater measurement error in occupational status attenuates the estimates.[61]

[58] See Appendix E for a discussion of how I select which reconstitutions to drop. The sign and statistical significance of my results are robust to different sample restrictions, while the magnitude of the fertility gradient increases with the strictness of the completeness restriction.

[59] Figure A6a plots the distribution of $\widehat{\beta}$ across all constituent community reconstitutions.

[60] Since the recording of childhood mortality is less reliable in the community books, I place less weight on this margin.

[61] Instrumenting one occupational draw for the father with a second, independent draw yields a net fertility gradient of 0.773 (SE 1.150; Kleibergen–Paap $F = 13.4$). Only $7{,}377$ couples have two independent occupational observations, so the estimate is imprecise, but its magnitude is roughly twice the corresponding OLS coefficient, consistent with substantial attenuation.

Table 6: External Validity: Fertility and Transmission

Panel A: Fertility Differentials

	Gross Fertility	Under-15 Mortality	Net Fertility
	(1)	(2)	(3)
ln(HISCAM)	0.397***	-0.020**	0.374***
	(0.112)	(0.010)	(0.092)
Mean DV	5.027	0.265	3.529
Observations	103,376	103,228	103,376
Community Books (clusters)	274	274	274
R^2	0.136	0.121	0.110

Panel B: Intergenerational Transmission

	(1)	(2)	(3)	(4)	(5)	(6)
	N: G2–G3	N: G1–G3	$\widehat{\beta}_{-1}$	$\widehat{\beta}_{-2}$	$\widehat{\lambda}$	SE($\widehat{\lambda}$)
Restricted	262,251	262,251	0.484	0.329	0.680***	(0.014)

Standard errors clustered by community book in parentheses (Panel A); bootstrap standard errors in parentheses (250 replications, Panel B). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
Notes: The sample is drawn from German *Ortsfamilienbücher* (community books) and is used to assess the external validity of the Wittgenstein results. Panel (A) reports OLS estimates of gross fertility, the under-15 mortality share, and net fertility on ln(HISCAM), absorbing community-book-by-marriage-decade fixed effects. The estimation sample applies restrictions R1–R5: complete reconstitutions within the extent of German settlement in Central Europe (R1); marriage observed in the community book, in the range 1600–1850, and coded as a marriage record (R2); both spouses marrying for the first time (R3); births observed in the community book, or the couple childless (R4); and both parents' deaths observed (R5). Panel (B) reports the intergenerational-transmission ratio $\widehat{\lambda} = \widehat{\beta}_{-2}/\widehat{\beta}_{-1}$, where $\widehat{\beta}_{-1}$ and $\widehat{\beta}_{-2}$ come from separate OLS regressions of the son's (G3) ln(HISCAM) status on the father's (G2) and grandfather's (G1) average ln(HISCAM) status, each absorbing fixed effects for the father's (G2) marriage decade, on the restricted linked sample of couples within the extent of German settlement in Central Europe with the father's marriage observed, the son (G3) born 1600–1900, and all three generations' occupational status observed.

Panel B reports the intergenerational (G3–G2) and multi-generational (G3–G1) elasticities of occupational status in columns (3)–(4) alongside the estimate for the intergenerational transmission λ in column (5). I find that the coefficient of transmission is statistically indistinguishable from the one I estimate for the restricted Wittgenstein sample.[62] While Wittgenstein sits close to the median coefficient of transmission, I observe substantial heterogeneity across space and time (see Figure A6b).[63] This adds to the body of evidence against Clark's "Law of Social Mobility" (Clark 2014), which states that the coefficient of transmission for socioeconomic status was high and stable across places and over time (Braun and Stuhler 2018; Santavirta and Stuhler 2024; Del Pizzo et al. 2026).

[62]The difference between the community-book estimate ($\widehat{\lambda} = 0.680$, SE 0.014) and the restricted Wittgenstein estimate ($\widehat{\lambda} = 0.593$, SE 0.053; Table 5) yields $t = 1.59$ ($p = 0.11$).

[63]A formal test for heterogeneity confirms that 69.5% of the observed variance in estimates is driven by true underlying cross-community differences rather than sampling noise (Cochran's Q p-value < 0.01).

While magnitudes differ, this analysis supports the external validity of the parameters estimated for Wittgenstein. In pre-Industrial Germany, the highest-status couples had more children, and while social persistence, as measured by the coefficient of transmission, was high at ∼0.60-0.70, it was consistently below Clark's "Law of Social Mobility".

7 Selection and Growth

How much selection could the German parameters produce? Evolutionary growth presumes that selection on a growth-enhancing trait increased the representation of this trait during the Malthusian era. Most theories are somewhat vague with regard to the degree of compositional change necessary. Clark (2008, p. 189) suggests that growth in mean endowment of 2.5% per generation on average could enable the economic takeoff and the Industrial Revolution. Galor and Moav (2002) model selection on a discrete trait and never quantify how much the composition of the population has to change to enable the phase change. Thus, instead of evaluating the model with reference to a specific compositional threshold, I focus on the compositional change — in terms of standard deviation changes in mean endowment and the population share above the 90th percentile of the initial endowment distribution — that different parameter combinations produce over $T = 20$ (approx 500 years) or ∞ generations.

Working in moments of the distribution also mitigates concerns about the scale of the latent endowment. The main objects I draw conclusions from are invariant to any affine reparametrization $z \mapsto a + bz$ of the latent log scale.[64] Although it does not make the results invariant to the choice of which monotone transform of raw occupational status is treated as linear in endowment, Table A17 illustrates that the standardized fertility differential does not depend on the choice of status parametrization.[65] The one quantity that is not scale-free is the translation of the drift into a percentage change in mean endowment for comparison with Clark's 2.5%, which reintroduces σ_z; I read that comparison as indicative of order of magnitude rather than a scale-invariant statement.

[64] $\beta \mapsto \beta/b$ exactly offsets $\sigma_z \mapsto b\sigma_z$ and the origin is absorbed by the generation wage intercept ω_g. This invariance also absorbs the unidentified returns-to-ability ρ, which enters the log scale multiplicatively and is therefore affine (cf. footnote to Equation 9).

[65] I adopt $\ln$(HISCAM) as a useful approximation of the fertility function and because it is the cardinalization under which the fertility gradient and the transmission are jointly linear. Table A17 re-anchors the drift to the HISCAM level, the discrete class score, and the three capital indices, each entered as a single cardinal regressor. To isolate the sensitivity of the fertility gradient to alternative status codings, I hold structural transmission fixed at the preferred baseline estimate ($\lambda = 0.593$); since these rows are non-linear recodings rather than affine rescalings, the comparison is conditional on a common transmission coefficient. The implied long-run drift stays within $[-11\%, +19\%]$ of the $\ln$(HISCAM) baseline – a band of 0.07–0.10 standard deviations at the German anchor – confirming that the fertility gradient behind the headline is not an artifact of the log-HISCAM cardinalization.

Table 7: Summary of Parameter Estimates

	Minimum Calibration		*Maximum* Calibration	
Panel A: Germany				
λ	0.59	Table 5, col. (5)	0.68	Table 6, col. (5)
$\beta\sigma_z$	0.13	Table A14, col. (1)	0.48	Table A14, col. (3)
$\bar{n}$	3.63	Table 3, col. (5)	3.63	Table 3, col. (5)
Panel B: England				
λ	0.68	Zhu (2024)	0.80	Clark (2014)
$\beta\sigma_z$	0.23	Boberg-Fazlic et al. (2011)	0.61	Clark and Cummins (2015b)
$\bar{n}$	2.95	Wrigley et al. (1997)	2.95	Wrigley et al. (1997)

Notes: Each Minimum/Maximum pair spans the parameter envelope least and most conducive to selection; the pairs are scenarios, not identified statistical bounds. λ is the coefficient of transmission; $\beta\sigma_z$ is the per-standard-deviation fertility differential, I obtain the unattenuated standard deviation of endowments based on the ratio estimator whereby $\sigma_z^2 = \theta_\lambda \sigma_{Occ}^2$, and the product of the log-status net-fertility gradient β (cited column) and the dispersion of log status σ_z. $\bar{n}$ is mean net fertility. Panel A (Germany) crosses the two German samples: the minimum scenario pairs the Wittgenstein restricted-sample transmission coefficient with the OLS gradient and the measurement-error-corrected dispersion, the maximum scenario pairs the community-book transmission coefficient with the father-IV gradient and the raw dispersion; $\bar{n}$ is the Wittgenstein couple-sample mean, common to both scenarios. Panel B (England) is literature-sourced; English $\bar{n}$ is the author's own calculation of mean net fertility on the Wittgenstein-condition Cambridge 26-parish sample built from the Wrigley et al. (1997) reconstitution. The English $\beta\sigma_z$ is a published between-group net-fertility gap B divided by the inferred log-status standard-deviation span between the compared groups: the minimum scenario takes $B = 0.60$ across seven status categories, read as a decile span $2\,\Phi^{-1}(0.9) \approx 2.56$, giving 0.23; the maximum scenario takes $B = 1.33$, a top-versus-bottom tercile-of-wealth group gap, divided by the tercile-group mean span ≈ 2.18, giving 0.61. The standard-deviation difference between the groups is inferred, since the actual English status distribution is not observed.

7.1 Model Parametrization

To evaluate the degree of selection, I plug my estimated parameters into Equation 7. All free parameters are identified from my data. Instead of foregrounding one parameter combination, I construct two parameter envelopes, constituting the most and least selection-inducing combinations possible (see Table 7). The rectangle between these two combinations describes the possible parameter space for the setting. While I use only the Wittgenstein estimates for the fertility differential, I do include the all-German coefficient of transmission since the external validity data is well suited to estimating it. For both, the average number of children $\bar{n}$ is observed directly from the Wittgenstein data.[66]

Using the maximum calibration, I find that the German parameters could increase

[66] Throughout, I maintain the constant variance assumption, and hold the selection differential S fixed. Since directional selection compresses the endowment variance, this stationary-variance projection is a deliberate upper bound provided innovation does not exceed the level that offsets that contraction, i.e. $\sigma_v^2 \leq \sigma_v^{2*}$. If instead $\sigma_v^2 > \sigma_v^{2*}$, dispersion and hence S rise across generations and cumulative drift can exceed the projection; under that configuration constant variance is a benchmark rather than a general bound (see Appendix C).

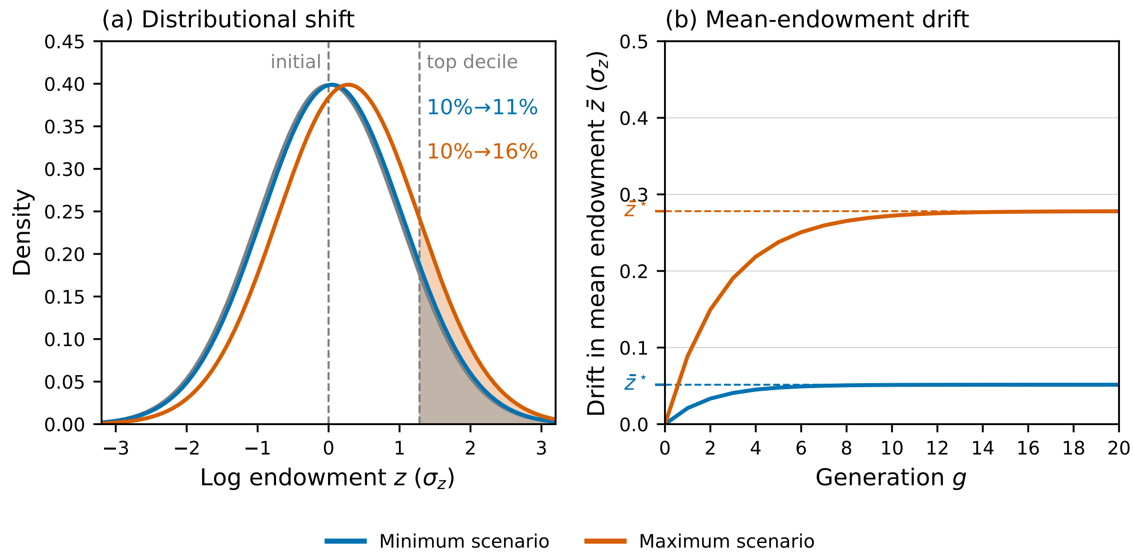

Figure 4: Distributional Shift and Mean-Endowment Drift under the German Parameters
Notes: Closed-form projection of selection on endowments under the German parameter envelope, evaluated at the stationary-variance benchmark. Panel (a) shows the induced shift in the log-endowment distribution (in standard-deviation units), with the initial top decile shaded and its post-selection share labeled; panel (b) traces the drift in mean endowment over twenty generations as it converges to its long-run limit $\bar{z}^*$. Each panel reports the minimum and maximum parameter envelopes from Table 7.

mean endowment by 0.28 standard deviations while increasing the share of the population in the top decile from 10% to 16%. Figure 4 plots the time trend of mean endowment and the shift in the endowment distribution over 20 generations. The fixed point, where regression to the mean halts selection, was reached after 12 generations. When I translate this to the average percent change over 20 generations, I find that Germany was an order of magnitude below the benchmark of 2.5% per generation (Clark 2008). Still, while the entire distribution shifts uniformly, this modest mean drift generates a large expansion in the absolute size of the upper tail: the share of the population exceeding the *initial* 90th-percentile threshold increases by 60%. This pattern — a modest shift in the mean and a much larger proportional change in the number of individuals above a high threshold — is a compositional change that matters if economic takeoff relies on a critical mass of highly endowed individuals, and is compatible with theoretical and empirical work on the relevance of upper-tail human capital to the Industrial Revolution (Mokyr 2002; Mokyr 2016; Squicciarini and Voigtländer 2015).

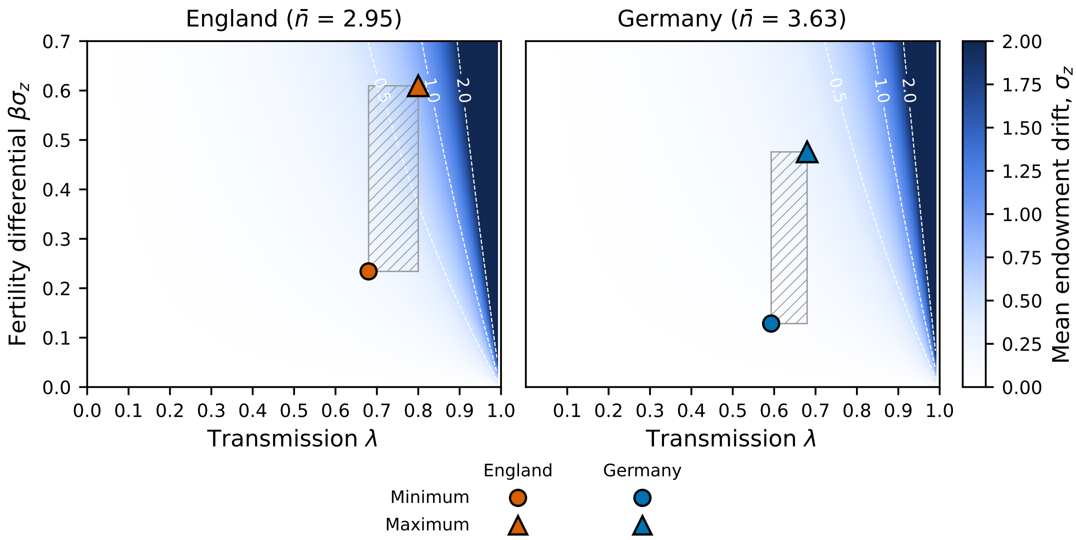

Figure 5: Long-Run Endowment Drift across the Parameter Space

Notes: Long-run drift in mean endowment (in standard-deviation units, in the limit $g \to \infty$) as a function of the transmission coefficient λ and the per-standard-deviation fertility differential $\beta\sigma_z$. The two panels fix mean net fertility at the English ($\bar{n} = 2.95$) and German ($\bar{n} = 3.63$) values; dashed contours mark drift of 0.5, 1, and 2 standard deviations. Markers locate the minimum and maximum parameter envelopes for each setting, and the hatched box spans the implied parameter region.

7.2 Counterfactual and the Parameter Space

While the above results suggest that mean endowment drift due to selection was modest for the German parameters, Germany was also not the cradle of industrialization. To investigate whether my model supports selection in the birth-place of modern growth, I parametrize it with estimates of λ, β, and $\bar{n}$ from different studies on England (see Table 7). In Figure 5, I plot results for the maximum and minimum parameter configurations for England and Germany on the broader parameter space. Each plot corresponds to a level of mean fertility, England on the left and Germany on the right. The standardized fertility differential and the coefficient of transmission vary along the y– and x-axis, respectively. The color of the cell at each parameter combination corresponds to the mean endowment drift it produces.

Even the most selection-inducing parameters for England only produce a 0.83 standard deviation increase in mean endowments. However, while this still falls short of Clark's average growth of 2.5% per generation — assuming pre-Industrial English status dispersion was broadly comparable to the observed German scale[67] — it does correspond to a

[67] Expressing an English drift as a percentage change in mean endowment reintroduces σ_z and is therefore not scale-free. English status dispersion is not identified by my data and is not reported on a comparable scale in the literature, so I apply the raw German $\sigma_z = 0.189$ throughout: I assume pre-Industrial English

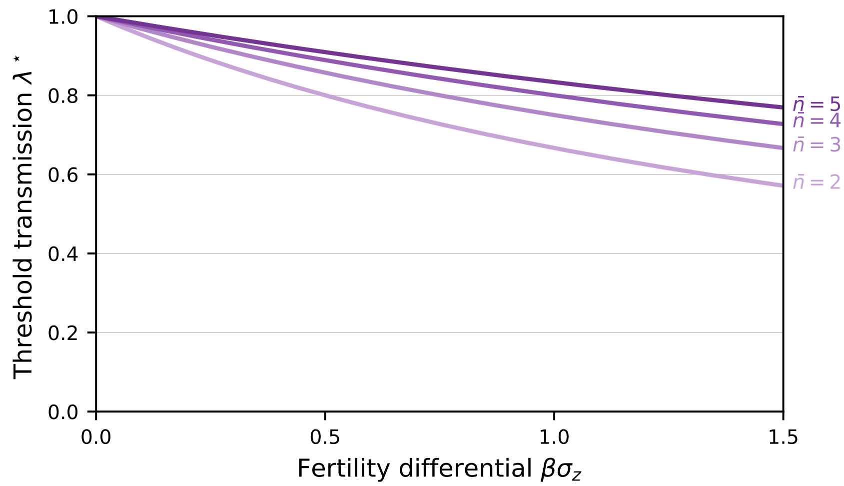

Figure 6: Transmission Threshold for a One-Standard-Deviation Drift

Notes: Threshold transmission coefficient $\lambda^\star$ required to sustain a one-standard-deviation increase in mean endowment, plotted against the per-standard-deviation fertility differential $\beta\sigma_z$. Each curve fixes mean net fertility $\bar{n}$ at a different value; the threshold $\lambda^\star = \bar{n}/(\bar{n} + \beta\sigma_z)$ falls as the fertility differential rises.

substantially larger expansion of the upper decile: here from 10 to 33% of the population. These results again illustrate that a uniform shift of the distribution translates into a far larger proportional change in the number of individuals above a high threshold than in the mean itself. The rectangular area between the maximum and minimum parameter envelopes represents the possible parameter space for selection in either setting. For England, the average cell produces a 0.42 standard deviation increase, while Germany registers an even more modest 0.15.

So when is selection viable? Figure 6 plots the threshold coefficient of transmission $\lambda^\star$ required to sustain a one-standard-deviation increase in mean endowment. This is an arbitrary benchmark that serves to illustrate how the threshold behaves. While the threshold is increasing in average fertility and decreasing in the size of the fertility differential, selection requires high intergenerational transmission across the board. This suggests that the kind of selection underpinning evolutionary growth was only possible for strongly heritable traits, and was substantially more likely in settings with low fertility and large fertility differentials, i.e. in very unequal societies close to or at subsistence.

status dispersion was broadly comparable to the observed German scale, both being northern European pre-Industrial economies. The drift in standard deviations does not depend on this assumption; only its translation into percent does.

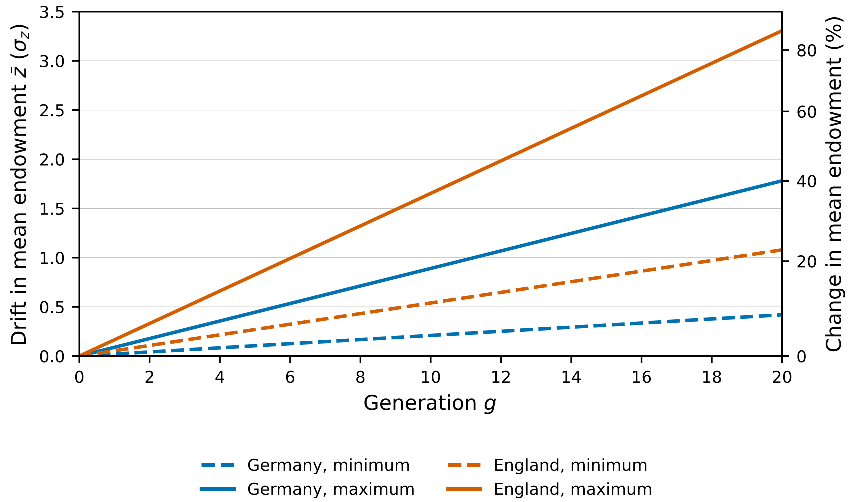

Figure 7: Mean-Endowment Drift under a Moving Transmission Intercept

Notes: Cumulative drift in mean endowment (in standard-deviation units) over twenty generations when the transmission intercept moves with the contemporaneous generational mean. The intercept absorbs regression to the mean, so the drift accumulates at a constant $\lambda\,\beta\sigma_z/\bar{n}$ per generation and grows linearly in g, with no fixed point (cf. Figure 4). Each line is one of the four parameter envelopes of Table 7: color marks the country (Germany blue, England vermilion) and line style the scenario (minimum dashed, maximum solid). The right axis converts standard deviations into a percent change in mean endowment at the raw German σ_z; assuming that the pre-Industrial distribution of occupational status was similar across England and Germany.

7.3 Changing Social Endowments

So far I assume that social endowments are stationary. This is the assumption that delivers the strong bound on selection. With a moving intercept a_g in the endowment transmission process, meaning that the societal *baseline* adjusts dynamically each generation, the endowment law of motion changes substantially. For example, if offspring are assumed to inherit the mean of their parent's generation plus the inheritable part of their parent's deviation from the mean, the moving intercept would be $a_g = (1-\lambda)\bar{z}_{g-1}$, which exactly offsets regression to the initial mean. Here, mean endowments evolve according to λS. This means that when social endowments adjust freely generation to generation, selection can deliver an unbounded and persistent increase in mean endowments. Figure 7 plots the evolution of mean endowments over 20 generations for the four parameter envelopes under this endowment transmission regime. The increase in mean endowments is substantially larger across the board. Taking the maximum selection envelopes, mean endowments increase by 1.78 standard deviations in Germany and by 3.31 in England. This is a 4x increase in the English case, and an even larger 6.4x one for Germany. While the German growth rate, at 1.70%, is still under the 2.5% benchmark, England surpasses it with 3.18%.

Selection is a much stronger force when I allow for a moving intercept that offsets regression to the mean. However, it is not clear whether a moving intercept is appropriate when modeling social endowments. While it would be overly restrictive to assume that social endowments are immovable, assuming that social endowments offset all regression to the mean in conservative structurally constrained pre-Industrial societies constitutes a similar stretch. In reality, social endowments were neither fully stationary nor dynamic. While increasing individual endowments pull social endowments upwards, structural constraints (e.g. conservative religious education) hold them back. I can operationalize this "inbetweenness" by introducing a constant $\vartheta \in [0, 1]$ that describes the fluidity of social endowments: $a_g = \vartheta(1 - \lambda)\bar{z}_{g-1}$. If $\vartheta = 1$ social endowments are fully dynamic; their evolution is not constrained by structural features of the society and baseline endowments are simply the mean of the last generation. If, however, $\vartheta = 0$ social endowments are fixed, society is so structurally conservative that the increase produced among a subset of the population cannot move the societal baseline. For any $\vartheta < 1$ mean endowments still converge, and the fixed point becomes:

$$\bar{z}^* = \frac{\lambda S}{(1 - \vartheta)(1 - \lambda)}. \tag{14}$$

This nests the stationary fixed point (7) at $\vartheta = 0$ and diverges as $\vartheta \to 1$.

Panel (a) of Figure 8 plots cumulative drift over $T = 20$ and $T = \infty$ for different levels of ϑ and the German maximum parameter envelope. As $\vartheta \to 1$, cumulative drift is no longer constrained and drift over 20 and ∞ generations diverge since $\bar{z}^* \to \infty$. It is worth noting that, even if $\vartheta = 0$ constrains the mean drift, it still enables substantial growth of the population above the initial 90th percentile. ϑ is unidentified from my data, thus I cannot draw inference on the appropriate level of cumulative drift, but, still, this exercise illustrates the importance of my model of social endowments to the viability and magnitude of selection. Panel (b) goes beyond the German case and plots cumulative drift over 20 generations as a function of the coefficient of transmission λ and the fluidity of social endowments ϑ. This illustrates the dual conditions for substantial growth: the fluidity of the social environment sits alongside, and is a potential substitute for, high intergenerational transmission. If the rigid institutions of the Malthusian era anchored the social baseline, it was this stationary environment — rather than a lack of demographic selection — that bounded evolutionary growth.

7.4 Decomposing Endowments

So far, I abstracted from the content and the transmission mechanism of endowments. The model treats transmitted traits as one package of "endowments"; however, socioeconomic

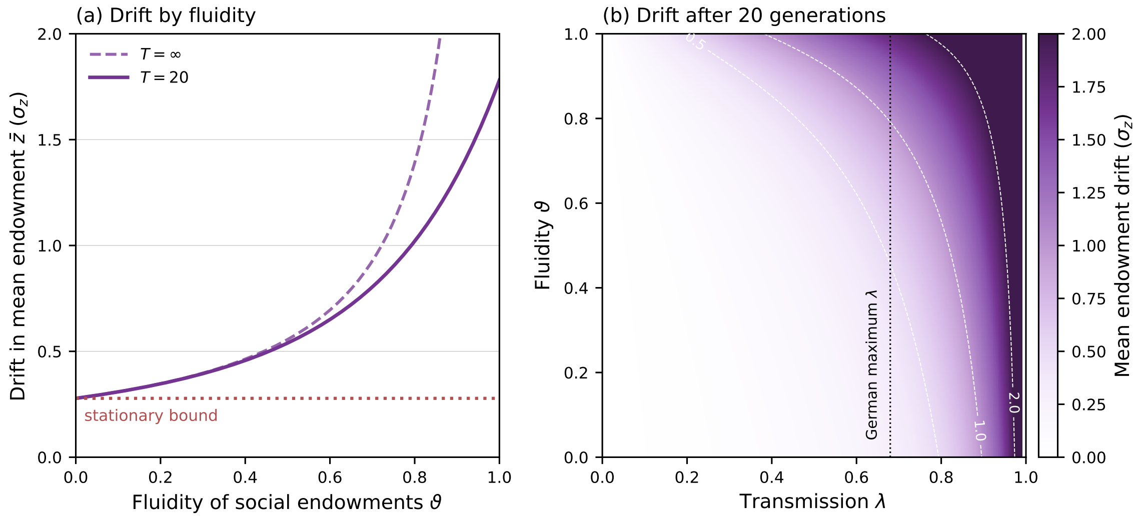

Figure 8: Mean-Endowment Drift and the Fluidity of Social Endowments

Notes: Cumulative drift in mean endowment (in standard-deviation units) when the transmission intercept is partially fluid, $a_g = \vartheta(1-\lambda)\bar{z}_{g-1}$. Mean endowments then follow an AR(1) with persistence $\lambda + \vartheta(1-\lambda)$, and drift after T generations is given by Equation A7, derived in Appendix A: $\vartheta = 0$ returns the stationary fixed point of Figure 4 and $\vartheta = 1$ the linear moving-intercept path of Figure 7. Panel (a) plots drift against ϑ after twenty generations (solid) and in the long run (dashed, Equation 14); the long-run path leaves the axis as $\vartheta \to 1$, where the fixed point ceases to exist, and the dotted red line marks the stationary bound at $\vartheta = 0$. Panel (b) plots twenty-generation drift over the (λ, ϑ) plane, with white dashed iso-drift contours and the dotted vertical line at the German maximum λ; the color scale is capped at two standard deviations. All parameters not on an axis are held at the German maximum envelope of Table 7.

status is overdetermined by a multitude of heritable and nonheritable factors (Collado et al. 2023). Del Pizzo et al. (2026) show that indirect estimators (i.e. λ) recover a weighted average across different factors, and that the choice of estimator dictates the weights. Therefore, depending on the specific component of endowments that evolutionary growth theory targets, λ may under- or overestimate its transmission. Moreover, if a constituent sub-component only affects fertility indirectly via its contribution to overall socioeconomic status, the aggregate fertility gradient β acts as a bottleneck for the indirect selection pressure on that sub-component. As such, even if a set of parameters delivers substantial selection on broad socioeconomic endowments, they do not necessarily deliver selection on the relevant growth-inducing sub-component (e.g., work ethic). Vice versa, if the aggregate parameters deliver modest or bounded selection, the environment might still induce strong selection on a specific, highly transmittable sub-component (e.g., a preference for child quality).

This delineation of sub-components and transmission mechanisms is also relevant when thinking about the intercept in the endowment process. Aside the degree of intergenerational transmission, the modelling of the intercept is the main determinant of cumulative drift. Which intercept is appropriate depends chiefly on the content of endowments and how they change across the population. As mentioned in section 2, if I follow Clark (2023) and view endowments as genetic, a moving intercept is appropriate. However, interpreting the intergenerational transmission of latent endowments as pure genetic heritability constitutes an intellectual leap. Markel et al. (2026) use random variation in the genetic similarity between siblings to estimate the respective contributions of genetic inheritance, family environment, and individual environment to socioeconomic outcomes. While they find substantial genetic heritability for traits such as height, BMI, or cognitive ability, they report null effects for all labor market outcomes. Instead, family environment and individual environment play an outsize role in determining socioeconomic status.[68] In a similar vein, Collado et al. (2023) find that genetic advantages explain a small share of socioeconomic outcomes.[69]

But even if genetic traits constitute a small component of endowments, this does not rule out a moving intercept. Mokyr (2016) draws on Cavalli-Sforza and Feldman (1973) to describe a model of vertical, horizontal, and oblique cultural evolution. If the oblique pathways are strong enough to shift overall culture from one generation to the next, and these cultural traits, such as attitudes to technical work or McCloskey's (2016) "bourgeois values", increase socioeconomic status (i.e. Andersen and Raster 2024), social endowments were likely dynamic. However, there are also conservative structural forces that constrain cultural evolution. In reality, as encapsulated by ϑ, social endowments were neither entirely fixed nor fully dynamic. Here, the magnitude of ϑ depends on the cultural and institutional environment.

The environment is also important when thinking about the strength of transmission. Even if we make the exceedingly strong assumption that social preferences and attitudes — such as the growth-inducing *je ne sais quoi* of the rich in Clark (2007), or a preference for educating children in Galor and Moav (2002) — were as heritable as, for example, height, heritability never exists in isolation. It is, instead, shaped by environmental factors, and the same holds for social preferences and attitudes (Feldman et al. 2000). The heritability of height falls during periods of nutritional stress that produce stunting (Silventoinen 2003). The intergenerational transmission of any behavioral trait is overdetermined in a complex system of genetic, extra-genetic, and epigenetic processes, most of which depend on and interact with the environment (Lala and Feldman 2024). Across the social sciences, a substantial body of literature stresses the relevance of environmental factors (e.g. Cole 2019; Chetty et al. 2016). As such, intergenerational transmission can only be understood in the context of its environment.

[68] Although their estimates are based on contemporary data, they constitute indicative evidence for other periods.

[69] Black et al. (2020) and van Alten et al. (2025) provide further evidence on the significant but limited contribution of direct genetic effects on socioeconomic status.

This section has argued that the two most important levers for selection — the strength of intergenerational transmission and the fixity of social endowments — depend on the cultural and institutional environment. Mechanisms of genetic transmission are similar across populations, while extra-genetic transmission varies substantially across different cultural and institutional environments. Societies that fostered the intergenerational transmission of growth-inducing traits such as human capital via extra-genetic pathways may have enjoyed a growth advantage. The variation in intergenerational transmission I observe across places in Figure A6b, perhaps as a consequence of differences in the cultural and institutional environment, supports the possibility of such a mechanism. Similarly, freeing social endowments from structural constraints, whether due to the weakening of religious institutions, access to education, or information technologies, can change the degree to which selection could alter the composition of the population. When evolutionary growth theory accounts for the multiple pathways of transmission, and acknowledges the co-evolution of traits, culture, and environment, it can offer an appealing explanation for growth (e.g. Galor 2022). Further empirical work is needed to identify these links between the cultural and institutional environment, selection, and growth.

Conclusion

This paper specifies a model of a Malthusian economy with intergenerational transmission to study the viability of the natural selection underpinning evolutionary growth theory. Selection on transmitted *latent* endowments can only deliver a bounded level shift in the endowment distribution if social endowments are fixed. Substantial selection is only possible with a positive fertility differential and high levels of intergenerational transmission, or with fluid social endowments. The necessary level of intergenerational transmission is decreasing and convex in the fertility differential, and increasing in average fertility. I estimate the relevant parameters in a historical German setting to quantify the level of selection.

To this end, this paper is the first to estimate fertility differentials and the intergenerational transmission of endowments for pre-Industrial Germany. I find that high occupational status couples had between $\sim$0.6 and 1.6 additional surviving children. This gradient is driven by differences in gross fertility and specifically variation in the age at marriage for women. Whether these fertility differentials create selection pressure depends on the intergenerational transmission of endowments. I use a ratio estimator (Stuhler 2012) to identify the unattenuated coefficient of intergenerational transmission. In the restricted sample, the coefficient of transmission is equal to 0.59. This is the first estimate of intergenerational transmission for pre-Industrial Germany (1600–1875). While substantially higher than implied by simple intergenerational correlations, the coefficient is much smaller than 0.80 as proposed by Clark (2014), and similar to estimates for Germany from the 20th century ($\sim$0.60) (Braun and Stuhler 2018).

I find that the parameters for Germany would have delivered a small increase in mean endowments. The maximum scenario delivers a 0.28 standard deviation increase in mean endowments, but a large 60% increase in the upper-tail endowment share above the 90th percentile of the initial distribution. Even for England, the maximum scenario drawn from estimates reported in the literature only yields a 0.83 standard deviation increase in mean endowments. If instead I specify a model with fluid social endowments, Germany still falls short of the benchmark set by Clark (2008), but England surpasses it. These results delineate the underlying assumptions and conditions for selection on latent endowments in an evolutionary growth framework.

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A Model Derivations and Proofs

Fertility Function. Substituting the budget $c = y(1 - n\pi)$ into the utility (3) and maximizing over n gives the first-order condition

$$-\frac{(1-\delta)y\pi}{y(1-n\pi)-\bar{c}} + \frac{\delta}{n} = 0, \tag{A1}$$

which solves to the fertility function (4).

Population and Malthusian Equilibrium. The number of couples evolves as

$$N_g = \frac{1-\mu}{2} \sum_{i=1}^{N_{g-1}} n^* \left(y_{i,g-1} \right). \tag{A2}$$

Imposing the replacement pin $n^*(\bar{y}^M) = 2/(1-\mu)$ on (4) and solving for income gives the equilibrium income[70]

$$\bar{y}^M = \frac{\bar{c}\,\delta(1-\mu)}{\delta(1-\mu) - 2\pi}, \tag{A3}$$

which is independent of $\bar{x}_g$. A finite and positive $\bar{y}^M$ exists only if $\delta(1-\mu) > 2\pi$, i.e. if the fertility ceiling δ/π strictly exceeds replacement fertility $2/(1-\mu)$: at equality replacement is approached only as $y \to \infty$, and below it no replacement income exists. I maintain this restriction throughout; it is not implied by the parameter ranges stated in the text. Substituting the wage (1) into the pin and solving for population yields

$$N^M = k \left(\bar{x}_g\right)^{(1-\alpha)/\alpha}, \qquad k = (AX)\left(\bar{y}^M\right)^{-1/\alpha}. \tag{A4}$$

With income pinned, writing $y_i = \bar{y}^M r_i$ turns the fertility function into

$$n^*(x_i) = \frac{\delta}{\pi}\left(1 - \frac{\bar{c}}{\bar{y}^M\, r_i}\right), \tag{A5}$$

a function of relative endowment $r_i = x_i/\bar{x}_g$ alone.

Proof of Proposition 1. By (7), the fixed point $\bar{z}^* = \lambda S/(1-\lambda)$ has, for $\lambda \in (0,1)$, the sign of

[70] This is a representative-couple approximation. The population law (A2) requires $\mathbb{E}[n^*(y_i)] = 2/(1-\mu)$, whereas the pin imposes $n^*(\bar{y}^M) = 2/(1-\mu)$; with fertility concave above subsistence and cornered below it, $\mathbb{E}[n^*(y_i)] < n^*(\bar{y}^M)$, so (A3) and the constant k in (A4) understate the income required for aggregate replacement. The scale-independence of equilibrium income does not rely on the approximation: if the distribution of relative endowments r_i is stationary, $\mathbb{E}[n^*(\bar{y}r_i)]$ is a function of $\bar{y}$ alone, so the aggregate condition also pins income independently of $\bar{x}_g$ — only its level is approximate.

S, so a positive shift in mean endowment requires $S > 0$. From (5), $S = \mathrm{Cov}(n, z)/\bar{n}$; with $\beta \equiv \mathrm{Cov}(n, z)/\sigma_z^2$ this is $S = \beta\sigma_z^2/\bar{n}$, positive if and only if $\beta > 0$. $\square$

Proof of Corollary 1. Immediate from $S = \beta\sigma_z^2/\bar{n}$: the differential rises with the fertility gradient β and with the dispersion of status σ_z, and falls with mean fertility $\bar{n}$. Dividing through by σ_z to express S in standard deviations of log endowment gives $S/\sigma_z = \beta\sigma_z/\bar{n}$, so drift scales with the per-standard-deviation fertility differential $\beta\sigma_z$. $\square$

Proof of Proposition 2. From (7), $\bar{z}_T = \frac{\lambda S}{1-\lambda}\left(1 - \lambda^T\right) \to \bar{z}^* = \frac{\lambda S}{1-\lambda}$ for $\lambda \in (0, 1)$: a finite fixed point, i.e. a one-time level shift rather than sustained growth. Since $\frac{d}{d\lambda}\frac{\lambda}{1-\lambda} = (1-\lambda)^{-2} > 0$ and $\frac{d^2}{d\lambda^2}\frac{\lambda}{1-\lambda} = 2(1-\lambda)^{-3} > 0$, $\bar{z}^*$ is increasing and convex in λ, and diverges as $\lambda \to 1$. $\square$

Moving Transmission Intercept. Section 7 relaxes the stationary intercept $a_g = 0$ of (2) and lets the social endowment track the previous generation's mean, $a_g = \vartheta(1-\lambda)\bar{z}_{g-1}$, where $\vartheta \in [0, 1]$ is the fluidity of social endowments. Taking the fertility-weighted mean of (2) as in (5), the intercept carries through unchanged and the mean follows the linear recursion

$$\bar{z}_g = \rho\,\bar{z}_{g-1} + \lambda S, \qquad \text{where} \quad \rho \equiv \lambda + \vartheta(1-\lambda). \tag{A6}$$

A fluid intercept therefore raises the persistence of the mean from λ to ρ, while leaving the per-generation selection gain λS unchanged. Iterating (A6) forward from $\bar{z}_0 = 0$ gives $\bar{z}_T = \lambda S \sum_{g=0}^{T-1} \rho^g$, and since $1 - \rho = (1-\vartheta)(1-\lambda)$, the geometric sum yields

$$\bar{z}_T = \frac{\lambda S}{(1-\vartheta)(1-\lambda)}\left(1 - \rho^T\right) \quad \text{for } \vartheta < 1, \qquad \text{and} \qquad \bar{z}_T = T\,\lambda S \quad \text{for } \vartheta = 1. \tag{A7}$$

For $\vartheta < 1$, $\rho < 1$, so $\rho^T \to 0$ and $\bar{z}_T$ converges to the fixed point (14); at $\vartheta = 0$ this collapses to the stationary case (7). At $\vartheta = 1$ the intercept exactly offsets regression to the mean, $\rho = 1$, and the geometric sum degenerates to T: mean endowments grow linearly and without bound, so no fixed point exists. Since $\partial\bar{z}^*/\partial\vartheta = \lambda S\left[(1-\vartheta)^2(1-\lambda)\right]^{-1} > 0$, the fixed point is increasing and convex in ϑ and diverges as $\vartheta \to 1$.

B Additional Tables and Figures

Figure A1: Location of Wittgenstein within Germany
Notes: Locator map showing the position of the Wittgenstein principalities within present-day Germany.

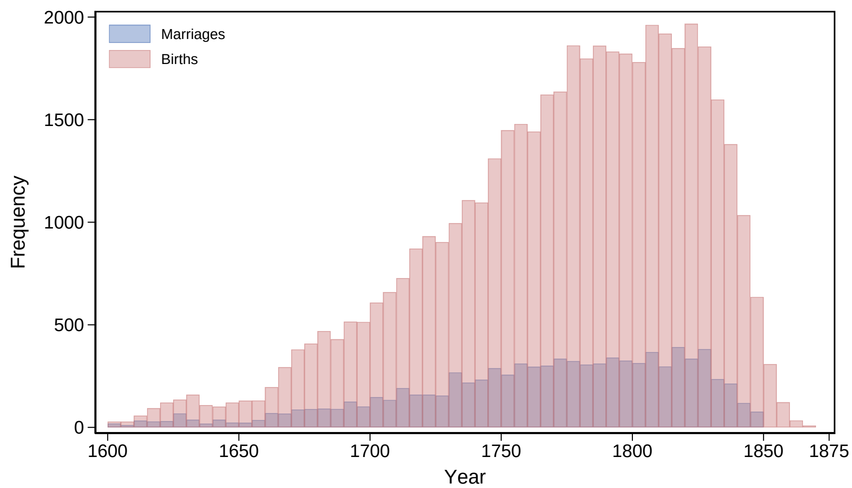


Figure A2: Temporal Distribution of Marriages and Births
Notes: Temporal distribution of the Wittgenstein reconstitution: counts of recorded marriages and births in five-year bins, 1600–1876.

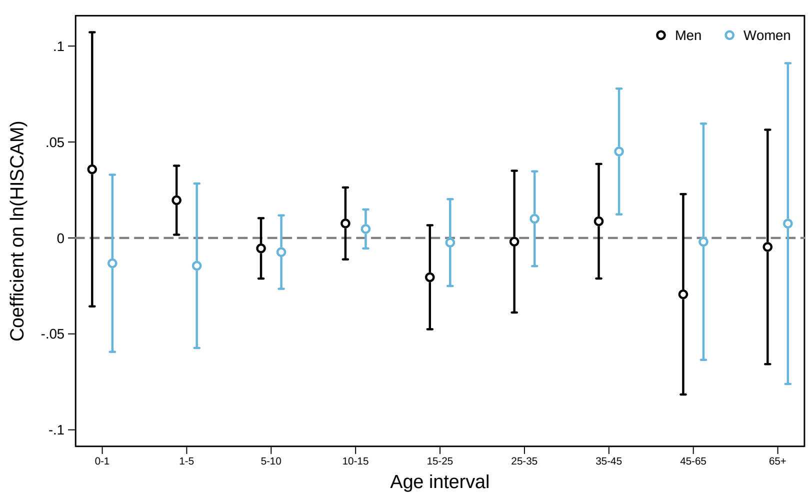


Figure A3: Status Gradient in Mortality by Age Interval
Notes: Status gradient in mortality by age interval. Each marker is the coefficient on $\ln(\text{HISCAM})$ from a linear-probability regression of an indicator for death within the given age interval, interacted with sex, with separate marriage-decade and marriage-parish fixed effects; whiskers are 95% confidence intervals.

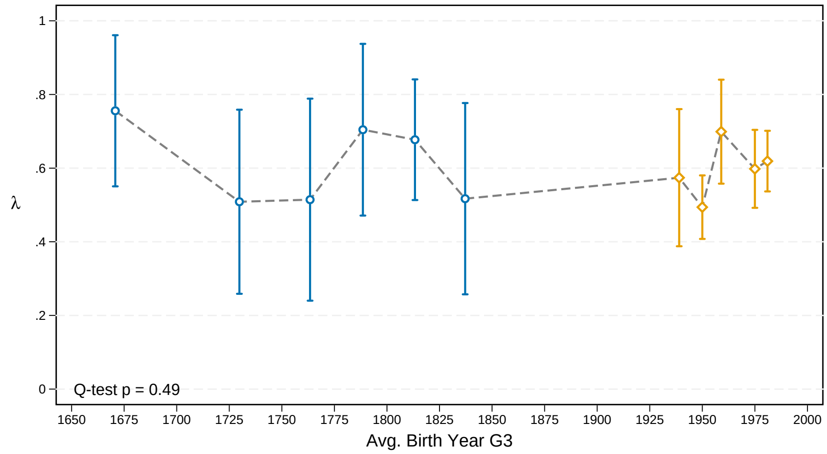


Figure A4: Trend in the Coefficient of Intergenerational Transmission

Cluster-bootstrap standard errors, resampled by the son's (G3) birth parish; whiskers are 95% confidence intervals.

Notes: Estimates of λ for successive son (G3) birth cohorts — 1600–1700, 1700–1750, then 25-year bins to 1850 — alongside 20th-century estimates from Braun and Stuhler (2018).

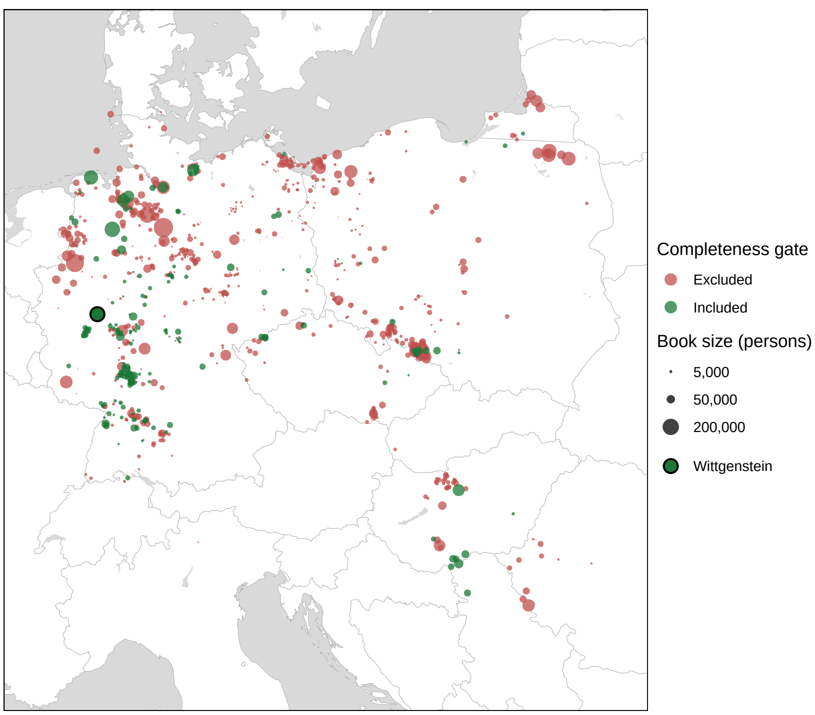
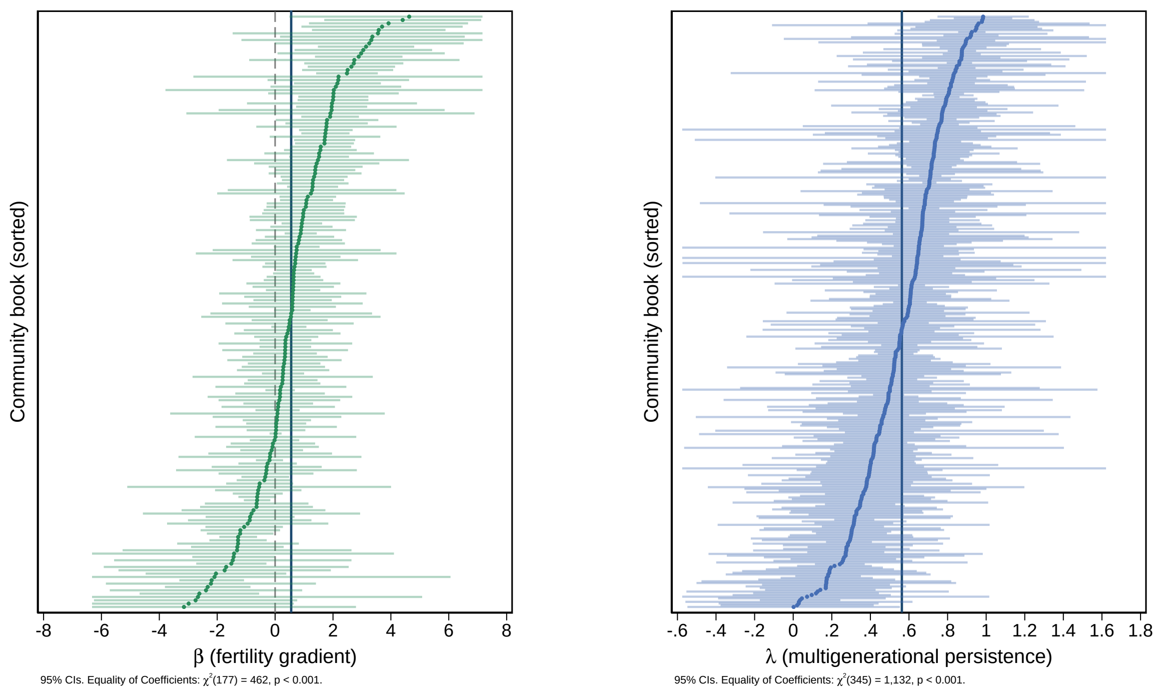


Figure A5: Location of German Family History Books

Notes: Each point is a German community family book (*Ortsfamilienbuch*) in the external-validity sample, placed at its parish location. Marker size is proportional to the number of reconstituted persons in the book, and color indicates whether the book passes the completeness gate — Included (a complete reconstitution, retained for analysis) versus Excluded. The Wittgenstein sample is highlighted with a black-bordered marker. The map is restricted to the extent of German settlement in Central Europe (books east of Poland's eastern border are dropped). *Source:* German *Ortsfamilienbücher*; author's compilation.



(a) Distribution of β

(b) Distribution of λ

Figure A6: Distribution of Parameter Estimates across Community Books

Notes: Distribution of parameter estimates across the individual German community books. Panel (a) plots the estimated fertility gradient β; panel (b) plots the coefficient of transmission λ (Braun-Stuhler ratio of the grandfather–son to the father–son HISCAM coefficient, with decade fixed effects). Each row is a single community book with at least 100 couples, sorted by the plotted estimate.

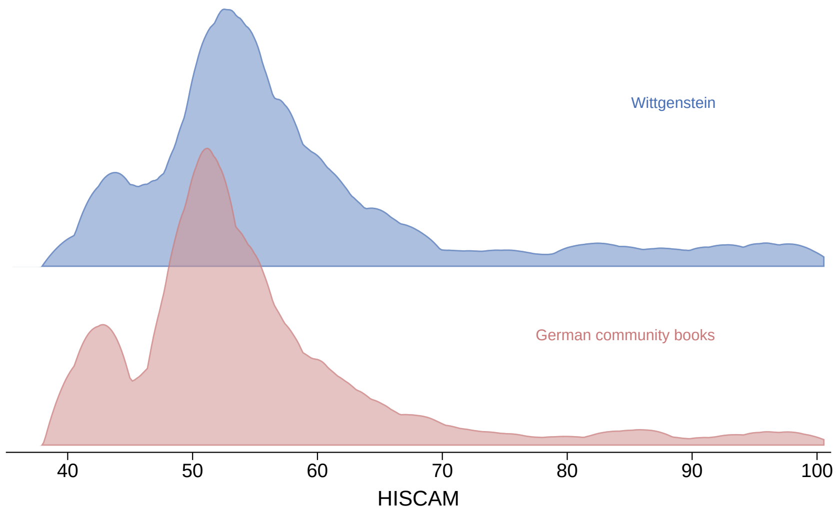


Figure A7: Distribution of Occupational Status, Wittgenstein and the German Community Books

Notes: Distribution of occupational status (HISCAM, averaged across an individual's recorded occupations) in Wittgenstein and in the German community books, plotted as kernel densities on a common bandwidth with the community books on the baseline and Wittgenstein raised onto its own. Each density is separately normalized, since the two series differ by two orders of magnitude (18,788 against 2,436,962 individuals). Both are every individual with a coded occupation; no book-completeness, migration, or fertility restriction is applied to either, so neither corresponds to an estimation sample used elsewhere in the paper. The community books are restricted to those located within Germany, dropping the books whose parish falls outside the modern border or carries no recovered coordinate (9.8% of coded individuals). Wittgenstein has a mean of 58.4 and a median of 54.8, against 55.2 and 52.8 across the community books; the tenth and ninetieth percentiles are 44.2 and 81.6 against 41.8 and 68.7. The community books' lower mode collects unskilled labourers (*Arbeiter*, *Tagelöhner*) and farmers (*Bauer*, *Landwirt*); consistent with the compiler's convention that agricultural occupations went unrecorded in Wittgenstein, HISCO 61110 is 1.2% of Wittgenstein's coded occupations against 11.7% of the community books'. Wittgenstein correspondingly carries proportionally more professional and skilled titles, so the two sources differ in recording practice at both ends of the scale rather than only in the agricultural share. *Source: Ortsfamilienbücher* status distribution frozen by `7_ofb/pipeline/10_hiscam_dist.py`.

Table A1: Intergenerational Transmission (λ): Robustness with Sample Criteria

	(1)	(2)	(3)	(4)	(5)	(6)
	N: G2–G3	N: G1–G3	$\widehat{\beta}_{-1}$	$\widehat{\beta}_{-2}$	$\widehat{\lambda}$	SE($\widehat{\lambda}$)
Unrestricted	3,506	3,506	0.447	0.273	0.610***	(0.048)
G2	2,755	2,755	0.425	0.252	0.593***	(0.053)
G2 & G3	2,383	2,383	0.419	0.238	0.569***	(0.055)
G1 & G2 & G3	1,885	1,885	0.407	0.224	0.549***	(0.062)

Cluster-bootstrap standard errors in parentheses (1,000 replications, resampled by the son's (G3) birth parish). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
Notes: $\widehat{\lambda} = \widehat{\beta}_{-2}/\widehat{\beta}_{-1}$, estimated by OLS (father's (G2) marriage-decade fixed effects). $\widehat{\beta}_{-1}$ is the father-to-son (G2-to-G3) coefficient, $\widehat{\beta}_{-2}$ the grandfather-to-son (G1-to-G3) coefficient; N: G2–G3 and N: G1–G3 are the corresponding regression sample sizes. Rows report increasingly restrictive samples, from no additional restriction to requiring an observed marriage for G1–G3 in the main parishes.

Table A2: Robustness to the Unobserved Status Category

	Net Fertility	
	(1)	(2)
Unobserved	0.291***	
	(0.094)	
Lower-middle Class	0.259*	
	(0.123)	
Upper-middle Class	0.596***	
	(0.110)	
Upper Class	0.642***	
	(0.129)	
$\ln$(HISCAM)		1.228***
		(0.215)
Mean DV	3.630	3.630
Observations	8,845	8,845
Parishes (clusters)	16	16
R^2	0.082	0.083

Standard errors clustered by marriage parish in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
Notes: OLS estimates of net fertility on the discrete social-class measure or $\ln$(HISCAM), with marriage-decade-by-parish fixed effects, testing robustness to folding in couples with otherwise-missing status. In column (1), the social-class measure is recoded so that couples with missing status form their own category (1), the original lowest class becomes the omitted baseline (0), and the remaining classes (2–4) are unchanged. In column (2), couples with missing average occupational status (HISCAM) are assigned the value 49.91 for small-hold farmers.

Table A3: Top 10 Occupation Strings by Social Class

	Lower		Lower-Middle	
1	day labourer (tageloehner)	1,511	soldier (soldat)	1,126
2	female domestic servant (dienstmagd)	442	charcoal burner (koehler)	475
3	shepherd (schaefer)	435	tailor (schneider)	388
4	farmhand / manservant (knecht)	417	miner (bergmann)	339
5	cowherd (kuhhirt)	272	linen weaver (leinweber)	293
6	herdsman (hirt)	247	weaver (weber)	210
7	recruit (rekrut)	108	carter / wagoner (fuhrmann)	165
8	smelting works / foundry worker (huettenarbeiter)	104	tenant farmer (paechter)	149
9	lackey / footman (lakai)	98	musketeer (musketier)	108
10	coachman (kutscher)	83	farmer / estate farmer (hofmann)	76
	Upper-Middle		**Upper**	
1	shoemaker (schuhmacher)	718	teacher (lehrer)	371
2	church elder (kirchenaeltester)	671	pastor (pfarrer)	350
3	mason / bricklayer (maurer)	425	mayor (buergermeister)	196
4	smith (schmied)	396	town councillor (ratsherr)	164
5	merchant / trader (handelsmann)	347	village mayor / headman (schultheiss)	151
6	master tailor (schneidermeister)	339	schoolmaster (schulmeister)	144
7	joiner / cabinetmaker (schreiner)	322	head forester (oberfoerster)	66
8	hammer smith (hammerschmied)	295	chapel master (church official) (kapellenmeister)	40
9	forester (foerster)	286	surgeon (chirurg)	32
10	master shoemaker (schuhmachermeister)	272	councillor (rat)	30

Notes: The 10 most frequent occupation strings within each of the four social classes, ranked by frequency of occurrence (English gloss, with the original German term in parentheses); N is the number of person-occupation observations coded with that string.

Table A4: Robustness to Alternative Socioeconomic Status Measures

	Net Fertility									
	LLM				CANINE	Manual	LLM	OpenAI	LLM	Manual
	avg	draw	low	high	avg	avg				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
$\ln$(HISCAM)	1.135***	1.080***	1.080***	0.941***	1.091***	1.016***				
	(0.231)	(0.232)	(0.273)	(0.205)	(0.164)	(0.184)				
Lower-middle Class							0.290**	0.067		
							(0.132)	(0.079)		
Upper-middle Class							0.570***	0.507***		
							(0.107)	(0.085)		
Upper Class							0.622***	0.442***		
							(0.138)	(0.134)		
Workers									0.166	0.373***
									(0.104)	(0.091)
Craftsmen									0.212	0.307***
									(0.129)	(0.083)
Traders & Supervisors									0.390***	0.518***
									(0.099)	(0.082)
Farmers									0.557	0.362
									(0.377)	(0.476)
Professionals & Academics									0.685***	0.729***
									(0.096)	(0.076)
Gentry, Officers, & Executive Officials									0.636***	0.791***
									(0.180)	(0.219)
Mean DV	3.632	3.632	3.632	3.632	3.606	3.614	3.620	3.619	3.669	3.615
Observations	4,367	4,367	4,367	4,367	4,423	4,526	4,623	4,622	3,686	4,577
Parishes (clusters)	15	15	15	15	15	15	16	16	16	16
R^2	0.100	0.100	0.099	0.100	0.098	0.099	0.097	0.098	0.110	0.100

Standard errors clustered by marriage parish in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
Notes: OLS estimates of net fertility on alternative status measures, all with marriage-decade-by-parish fixed effects. The omitted baseline in columns (7)–(8) is the lowest social class and in columns (9)–(10) is Servants & Laborers. Where multiple occupations are observed avg takes the mean, draw is a random draw, and low/high take the lowest or highest status occupation respectively.

Table A5: Other Status Measures

	Net Fertility				
	(1)	(2)	(3)	(4)	(5)
Human Capital					
Level 2	0.310*				
	(0.156)				
Level 3	0.569***				
	(0.114)				
Level 4	0.488*				
	(0.258)				
Social Capital					
Level 2		0.265**			
		(0.109)			
Level 3		0.531***			
		(0.102)			
Level 4		0.748***			
		(0.099)			
Physical Capital					
Level 2			0.562***		
			(0.055)		
Level 3			0.569***		
			(0.091)		
Level 4			1.013**		
			(0.359)		
OhDAB Occupational Skill					
Semi-skilled				-0.064	
				(0.187)	
Skilled				0.206	
				(0.242)	
Skilled Supervisors				0.405*	
				(0.212)	
Skilled Executive				0.722***	
				(0.160)	
Ownership reported in Marriage Contracts					
Home-owner					0.238
					(0.233)
Landed Home-owner					1.072***
					(0.335)
Mean DV	3.620	3.620	3.620	3.601	3.462
Observations	4,623	4,623	4,623	4,430	1,613
Parishes (Clusters)	16	16	16	16	15
R^2	0.096	0.099	0.100	0.100	0.143

Standard errors clustered by marriage parish in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
Notes: OLS estimates of net fertility on alternative components of the status/capital decomposition, each estimated in a separate regression with marriage-decade-by-parish fixed effects; the omitted baseline in each column is the lowest level of the respective measure: unskilled for the occupational-skill scale in column (4), and renters/lodgers for the ownership scale in column (5). The occupational-skill scale is from an independent coding of the Wittgenstein occupations by Moeller et al. (2023). The Ownership variable is recorded by Mehldau (2011), and was transcribed from marriage contracts for a subset of the sample.

Table A6: Coding Agreement: LLM (Claude) vs. Manual, OccCANINE, and OpenAI

	LLM (Claude) vs.		
	Manual (1)	OccCANINE (2)	OpenAI (3)
HISCO exact agreement (%)	62.7 (44.2)	60.2 (44.2)	
HISCO 1-digit agreement (%)	92.1 (81.7)	88.4 (76.5)	
HISCO 2-digit agreement (%)	81.4 (62.9)	76.5 (60.2)	
HISCAM correlation	0.921 (0.825)	0.821 (0.780)	
Social class agreement (%)			84.0 (76.4)
Person-occupations compared	23,942 (1,635)	24,761 (2,392)	25,856 (2,564)

Notes: Agreement between the LLM (Claude) occupational coding and three benchmarks: a hand-coded manual benchmark, OccCANINE, and an OpenAI-based replication of the same coding prompt. Each cell reports the person-occupation-level agreement rate (one observation per person-occupation instance); the string-level agreement rate (one observation per unique occupation string) is in parentheses. The HISCO exact/1-digit/2-digit rows compare classification codes at decreasing levels of precision; HISCAM correlation is the Pearson correlation of the continuous status scale; social-class agreement compares the discrete social-class measure. The final row reports the number of person-occupations, with the number of unique occupation strings in parentheses, underlying each comparison.

Table A7: Robustness to Alternative Estimators

	Gross Fertility	Under-15 Mortality	Net Fertility
	PPML (1)	Logit (2)	PPML (3)
$\ln(\text{HISCAM})$	0.326*** (0.032)	0.060 (0.155)	0.301*** (0.058)
Mean DV	5.237	0.307	3.634
Observations	4,365	22,828	4,365
Parishes (clusters)	15	16	15
R^2			

Standard errors clustered by marriage parish in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
Notes: Robustness of the $\ln(\text{HISCAM})$ fertility association to nonlinear estimators, in place of the linear OLS specifications used in the main text; all include marriage-decade-by-parish fixed effects. Column (1) reports gross fertility, estimated by Poisson pseudo-maximum-likelihood. Column (2) reports an indicator for death at or by age 15, estimated by a logit model at the birth (individual) level. Column (3) reports net fertility, estimated by Poisson pseudo-maximum-likelihood.

Table A8: Priest-Clustered Standard Errors

	Gross Fertility	Under-15 Mortality	Net Fertility
	(1)	(2)	(3)
ln(HISCAM)	1.775***	0.012	1.135***
	(0.259)	(0.020)	(0.216)
Wild-bootstrap p-value	[0.000]***	[0.729]	[0.001]***
Mean DV	5.235	0.305	3.632
Observations	4,367	22,987	4,367
Parishes	15	16	15
Decade X Parish (Clusters)	235	254	235
R^2	0.095	0.024	0.100

Standard errors clustered by marriage-decade-by-parish ("priest") in parentheses; parish-level wild-cluster-bootstrap p-values (999 replications) in square brackets. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
Notes: OLS estimates of gross fertility (column 1), an indicator for death by age 15 (column 2), and net fertility (column 3) on ln(HISCAM), each with marriage-decade-by-parish fixed effects. Clustering at the priest level rather than by parish alone raises the number of clusters; the bootstrapped p-values guard against over-rejection with few clusters.

Table A9: Unified-Bound Fertility

	OLS		IV	
	(1) Baseline	(2) Unified	(3) Baseline	(4) Unified
ln(HISCAM)	0.833***	0.680***	2.447***	2.466***
	(0.256)	(0.188)	(0.812)	(0.620)
Mean DV	3.772	2.714	3.772	2.714
Kleibergen–Paap F			43.35	43.35
Observations	2,073	2,073	2,073	2,073
Parishes (clusters)	15	15	15	15
R^2	0.125	0.154		

Standard errors clustered by marriage parish in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
Notes: Estimates with marriage-decade-by-parish fixed effects. Baseline columns use net fertility; Unified columns use the number of the couple's children ever observed marrying in the registers. A child missing from that count either died before marrying, left the parish, or never married, so the unified measure folds child mortality, out-migration, and celibacy into one extensive-margin outcome, and its status gradient bounds the corresponding selection differential. Marriage cohorts are capped at 1830 in all columns, so that children of the latest couples still reach marriage age within the period the registers cover. Columns (1)–(2) are OLS; columns (3)–(4) instrument ln(HISCAM) with the grandfather's status, as in Table A14. All four columns are estimated on the couples with an observed grandfather, so the OLS and IV estimates describe one population rather than two. R^2 is not reported for the two-stage least squares columns.

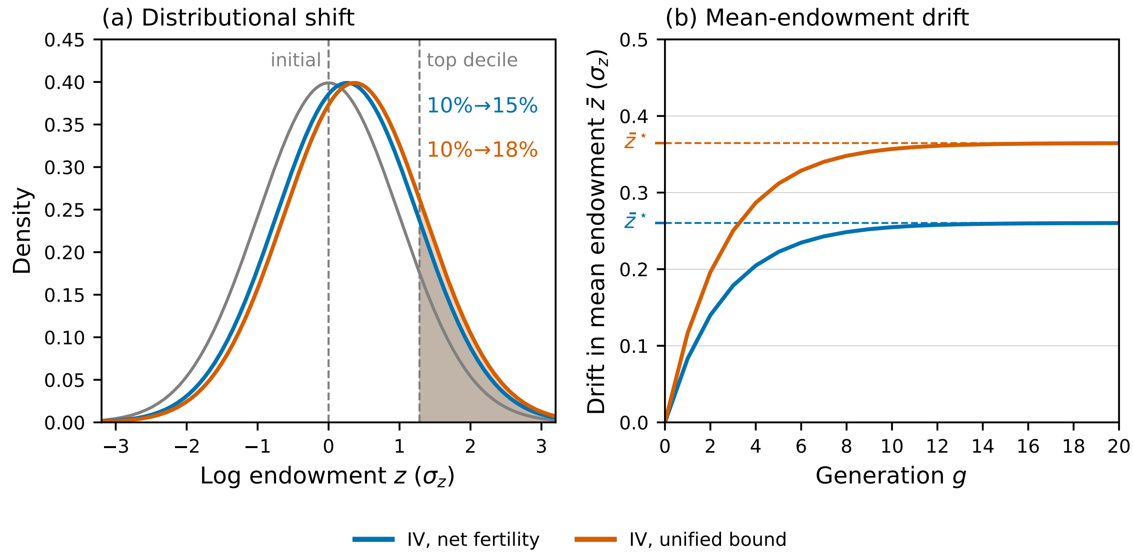


Figure A8: Distributional Shift and Mean-Endowment Drift under the Unified-Bound Parameters

Notes: The projection of Figure 4, re-run on the parametrization most conducive to selection. Both envelopes take the transmission coefficient and the raw status dispersion from the German maximum scenario of Table 7, and take the fertility gradient and mean net fertility from the two grandfather-IV columns of Table A9, which are estimated on couples with an observed grandfather. The envelopes differ only in the outcome: net fertility (col. 3) versus the unified count of children ever observed marrying (col. 4), which counts a child as lost if they died before marrying, out-migrated, or stayed celibate, and whose much smaller $\bar{n}$ makes it the more selection-inducing bound. Panel (a) shows the induced shift in the log-endowment distribution (in standard-deviation units), with the initial top decile shaded and its post-selection share labeled; panel (b) traces the drift in mean endowment over twenty generations as it converges to its long-run limit $\bar{z}^*$.

Table A10: Starting and Stopping (Men)

	Age at marriage	Age at last birth
	(1)	(2)
ln(HISCAM)	-2.570***	2.516***
	(0.774)	(0.438)
Mean DV	29.045	42.808
Observations	4,367	2,588
Parishes (clusters)	15	15
R^2	0.086	0.116

Standard errors clustered by marriage parish in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
Notes: OLS estimates of the father's age at marriage (column 1) and age at last birth (column 2, restricted to the full-fertility-period sample) on $\ln$(HISCAM). All specifications include marriage-decade-by-parish fixed effects.

Table A11: Birth Spacing

	(1) Birth interval	(2) Birth interval
$\ln(\text{HISCAM})$	-5.997***	-5.599***
	(0.944)	(1.050)
Birth order		0.579***
		(0.050)
Mother's age at marriage		0.223***
		(0.056)
Mean DV	33.436	33.436
Observations	12,409	12,409
R^2	0.044	0.054

Standard errors clustered by marriage parish in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
Notes: OLS estimates of the birth interval (in months) between successive births to the same mother on $\ln(\text{HISCAM})$, for the full-fertility-period sample with non-missing HISCAM; column (2) adds birth order and the mother's age at marriage as controls. All specifications include marriage-decade-by-parish fixed effects.

Table A12: Occupation Reliability

	Occupation 1 (HISCAM)				
	(1)	(2)	(3)	(4)	(5)
Occupation 2 (HISCAM)	0.627***				
	(0.010)				
Occupation 3 (HISCAM)		0.587***			
		(0.021)			
Occupation 4 (HISCAM)			0.551***		
			(0.044)		
Occupation 5 (HISCAM)				0.483***	
				(0.082)	
Occupation 6 (HISCAM)					0.403**
					(0.159)
N	4,205	1,056	313	115	43

Conventional (non-robust) OLS standard errors in parentheses; no fixed effects. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
Notes: Measurement-reliability check: each column reports OLS estimates regressing the HISCAM score of an individual's first recorded occupation on the HISCAM score of their k-th recorded occupation ($k = 2, \ldots, 6$), among individuals with multiple recorded occupations.

Table A13: Alternative IV Strategies

	Net Fertility				
			2nd Occ	Father	OccCANINE
	(1)	(2)	(3)	(4)	(5)
	OLS	OLS	2SLS	2SLS	2SLS
$\ln(\text{HISCAM})^{G3}$	1.080***				
	(0.232)				
$\ln(\text{HISCAM})^{G3,\text{avg}}$		1.135***			
		(0.231)			
$\ln(\widehat{\text{HISCAM}})^{G3}$			1.469*	2.517***	1.693***
			(0.808)	(0.820)	(0.309)
Mean DV	3.632	3.632	3.622	3.678	3.620
Kleibergen–Paap F stat			119.511	44.235	615.666
Observations	4,367	4,367	1,296	2,281	4,266
Parishes (clusters)	15	15	14	15	15

Standard errors clustered by marriage parish in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
Notes: All columns include marriage-decade-by-parish fixed effects. Columns (1)–(2) report OLS estimates of net fertility on the son's (G3) $\ln(\text{HISCAM})$, using a single occupational draw (column 1) and the average across draws (column 2). Columns (3)–(5) report two-stage least squares (2SLS) estimates instrumenting the son's (G3) $\ln(\text{HISCAM})$ with a second, independent occupational draw (column 3), the father's average $\ln(\text{HISCAM})$ (column 4), or the OccCANINE $\ln(\text{HISCAM})$ obtained from an independent machine-learning classifier (column 5). The Kleibergen–Paap F statistic is the first-stage weak-instrument test for the 2SLS columns.

Table A14: IV: Measurement Error

	Net Fertility	$\ln(\text{HISCAM})^{G3}$	Net Fertility
	(1)	(2)	(3)
	OLS	FS	2SLS
$\ln(\text{HISCAM})^{G3}$	0.847***		
	(0.250)		
$\ln(\text{HISCAM})^{G2}$		0.298***	
		(0.045)	
$\ln(\widehat{\text{HISCAM}})^{G3}$			2.517***
			(0.820)
Mean DV	3.678	4.053	3.678
Kleibergen–Paap F stat			44.235
Observations	2,281	2,281	2,281
Parishes (clusters)	15	15	15

Standard errors clustered by marriage parish in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
Notes: Column (1) reports OLS estimates of net fertility on the son's (G3) $\ln(\text{HISCAM})$. Column (2) reports the first-stage regression of the son's (G3) $\ln(\text{HISCAM})$ on the father's (G2) average $\ln(\text{HISCAM})$, the excluded instrument. Column (3) reports the corresponding two-stage least squares (2SLS) estimate of net fertility on the instrumented son's (G3) $\ln(\text{HISCAM})$, used to correct for measurement error in the status measure. All specifications include marriage-decade-by-parish fixed effects; the Kleibergen–Paap F statistic in column (3) is the weak-instrument test.

Table A15: Intergenerational Transmission (λ): Robustness with OccCANINE

	(1)	(2)	(3)	(4)	(5)	(6)
	N: G2–G3	N: G1–G3	$\widehat{\beta}_{-1}$	$\widehat{\beta}_{-2}$	$\widehat{\lambda}$	SE($\widehat{\lambda}$)
Full	4,422	2,707	0.280	0.163	0.585***	(0.084)
Restricted	2,683	2,683	0.317	0.166	0.524***	(0.068)
Early (pre-1800)	1,314	1,314	0.310	0.184	0.594***	(0.076)
Late (post-1800)	1,369	1,369	0.330	0.138	0.417***	(0.123)

Cluster-bootstrap standard errors in parentheses (1,000 replications, resampled by the son's (G3) birth parish). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
Notes: $\widehat{\lambda} = \widehat{\beta}_{-2}/\widehat{\beta}_{-1}$, estimated by OLS (father's (G2) marriage-decade fixed effects) using HISCAM values assigned by OccCANINE in place of the LLM-coded HISCAM, as a robustness check on the occupational-status classifier. $\widehat{\beta}_{-1}$ is the father-to-son (G2-to-G3) coefficient, $\widehat{\beta}_{-2}$ the grandfather-to-son (G1-to-G3) coefficient; N: G2–G3 and N: G1–G3 are the corresponding regression sample sizes. All samples restrict to main-line G2 marriages. Restricted additionally requires non-missing G1 and G2 HISCAM; Early/Late split the Restricted sample at G3 birth year 1800.

Table A16: Summary Statistics: German Community Histories

Panel A: Couples

	Mean	Std. dev	Minimum	Maximum	N
Gross fertility	4.80	2.99	0	23	225,093
Net fertility	3.37	2.23	0	17	225,093
Deaths under 15	1.44	1.68	0	19	225,093
HISCAM	55.78	10.78	41	99	103,951
Observations	225,093				

Panel B: Mobility

	Mean	Std. dev	Minimum	Maximum	N
HISCAM^{G3} (Son, G3)	54.90	9.95	41	99	262,251
HISCAM^{G2} (Father, G2)	54.75	9.72	41	99	262,251
HISCAM^{G1} (Grandfather, G1)	55.07	10.43	41	99	262,251
Observations	262,251				

Notes: The sample is the German external-validity data built from the Ortsfamilienbücher (community books), restricted to books flagged as complete and located within the extent of German settlement in Central Europe. Panel A further restricts to marriages in 1600–1850, an observed marriage parish, both spouses on their first marriage, an observed (or unresolved) birth parish, and observed parental death dates, and an observed fertility history. Panel B further restricts to main-line G2 marriages, a G3 birth year in 1600–1900, and non-missing HISCAM for the grandfather (G1), father (G2), and son (G3).

Table A17: Robustness of the Fertility Differential Across Status Measures

	$\widehat{\beta}$ (1)	σ_s (2)	$\widehat{\beta}\,\sigma_s$ (3)	$\bar{z}^\star/\sigma_z$ (4)	$\Delta\%$ (5)
$\ln$(HISCAM) (baseline)	1.135	0.180	0.204	0.082	—
HISCAM (level)	0.017	11.684	0.198	0.079	-2.8
Social class (1–4)	0.251	0.822	0.206	0.082	$+1.1$
Capital: human (1–4)	0.237	0.765	0.181	0.072	-11.2
Capital: physical (1–4)	0.295	0.747	0.221	0.088	$+8.2$
Capital: social (1–4)	0.266	0.908	0.242	0.097	$+18.7$

Notes: Each row re-cardinalizes occupational status and enters it as a single cardinal regressor. Column (1) is the OLS net-fertility gradient $\widehat{\beta}$ on the status measure named in the row, with marriage-decade-by-parish fixed effects and standard errors clustered by marriage parish, estimated on the fertility sample ($N = 4{,}367$, held constant across rows). Column (2) is the in-sample standard deviation σ_s of that measure; column (3) the standardized (per-standard-deviation) fertility differential $\widehat{\beta}\,\sigma_s$ that enters the selection differential $S = \widehat{\beta}\,\sigma_z^2/\bar{n}$. Column (4) is the implied long-run drift in mean endowment, $\bar{z}^\star/\sigma_z = \frac{\lambda}{1-\lambda}\,\frac{\widehat{\beta}\,\sigma_s}{\bar{n}}$, evaluated at the German anchor $\lambda = 0.593$ (Table 5, restricted sample) and mean net fertility $\bar{n} = 3.63$; only $\widehat{\beta}\,\sigma_s$ varies across rows, since λ and $\bar{n}$ are held fixed. Holding λ fixed is deliberate: these rows are non-linear recodings or distinct measures, not affine rescalings, so re-estimating the ratio $\widehat{\beta}_{-2}/\widehat{\beta}_{-1}$ on each would confound discretization and proxy-specific measurement error with structural transmission. The table is therefore a *ceteris paribus* exercise in λ that isolates the sensitivity of the fertility gradient to alternative status codings, not a claim that the transmission estimate is itself invariant to them. Column (5) is the percent deviation of the drift (equivalently of $\widehat{\beta}\,\sigma_s$) from the $\ln$(HISCAM) baseline.

C Variance Dynamics

Figure A9 traces how the variance dynamics feed into mean drift. The variance of log endowments follows an AR(1) process,

$$\sigma^2_{z,g+1} = \lambda^2 \phi\, \sigma^2_{z,g} + \sigma^2_v, \tag{A8}$$

where λ^2 is the regression to the mean from imperfect transmission, σ_v^2 the innovation that replenishes variance, and $\phi \leq 1$ the compression from directional selection (the Bulmer effect).[71] Assuming stationary variance implies that the *iid* error term in the endowment process is large enough to offset both the variance erosion from regression to the mean and that from the Bulmer effect. In a setting where socioeconomic status depended to a large part on one's family, this is a liberal assumption. Still, the data suggest that assuming

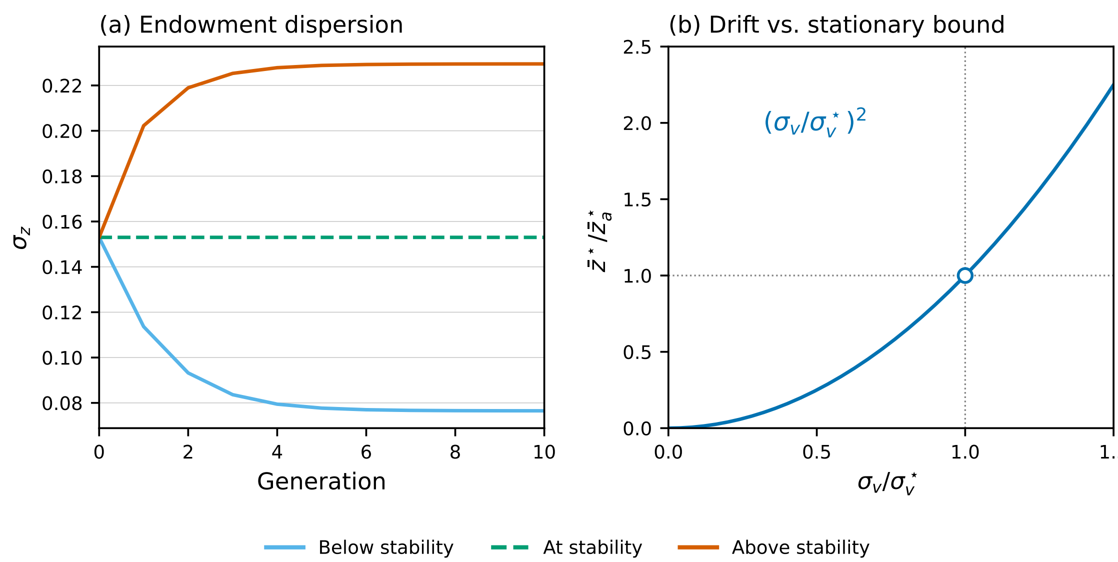


Figure A9: Robustness of the Variance Dynamics

Notes: Robustness of the closed-form variance dynamics under the Wittgenstein calibration. Panel (a) plots the path of endowment dispersion σ_z over ten generations when the transmission-noise variance lies below, at, or above the stability point; panel (b) plots the long-run mean-endowment drift relative to the stationary-variance bound as a function of $\sigma_v/\sigma_v^\star$. The stationary-variance assumption maintained in the main text corresponds to $\sigma_v = \sigma_v^\star$; the panels show how a more or less volatile transmission process would raise or lower the drift.

[71] The bound $\phi \leq 1$ is a property of the fertility schedule (4) rather than an additional assumption. Selection reweights the parent distribution by $n_i/\bar{n}$, which shifts the variance by $\mathrm{Cov}\left(n_i, (z_i - \bar{z})^2\right)/\bar{n} - S^2$. Because fertility is zero below subsistence and concave above it, these weights are non-negative and rise less than proportionally with endowment, so the covariance term cannot be positive, and the remaining term is a square. Selection can therefore only compress the distribution, with $\phi = 1$ in the limiting case of a flat schedule, where $S = 0$ and there is nothing to compress.

stationary variance is reasonable since I see little secular change in the dispersion of occupational status across my sample. Conditional on this assumption not holding, if σ_v^2 is smaller than the σ_v^{2*} that balances contraction, a constant-S projection overstates the drift, since S_g would shrink with the variance compression.

D Occupational Coding and Status Mapping

Recorded occupational titles are mapped to status in four steps: deterministic string cleaning, a three-stage large language model (LLM) pipeline that parses, classifies, and HISCO-codes each title, and a status-construction step that collapses the coding to the person level. Every LLM stage is queried at *temperature* $= 0$ with schema-constrained output, so no ill-formed or out-of-schema value can be returned. Figure A10 summarizes the workflow and the counts at each stage, Table A18 the model settings.

Deterministic cleaning. Before any model call, each raw occupation string is folded to ASCII digraphs (`ae/oe/ue/ss`); place names, embedded numbers, abbreviation tokens (`gfl`, `eine`, `ein`, `grl`, `wtg`, `ghz`, `ghg`, `hft`, `ftl`, `kgl`), and trailing "`bei ...`" / "`in ...`" phrases are stripped; everything but letters, spaces, and hyphens is removed; and the string is lower-cased. Persons whose string is empty after cleaning are dropped, leaving 5,829 distinct strings.

Stage A — Parse strings into titles. A cheaper model (`claude-haiku-4-5-20251001`) splits each cleaned string into its distinct occupation titles, keeping compound titles (a qualifier plus a trade) as one title and ignoring employers, workplaces, and honorifics without an occupation. This produces 2,620 unique titles (prompt 1).

Stage B — Translate and classify. A frontier model (`claude-sonnet-4-6`) processes one unique title per message and returns an English translation, ratings of the three forms of capital the occupation commands on a 1–4 scale, the four-level social class that is the paper's discrete status scale, and a one-sentence justification of each assignment, following best practice for LLM coding (Ferrara 2026). Ratings and class are restricted to $\{1, 2, 3, 4\}$ or *none*; titles returned as *none* are left unclassified (prompt 2).

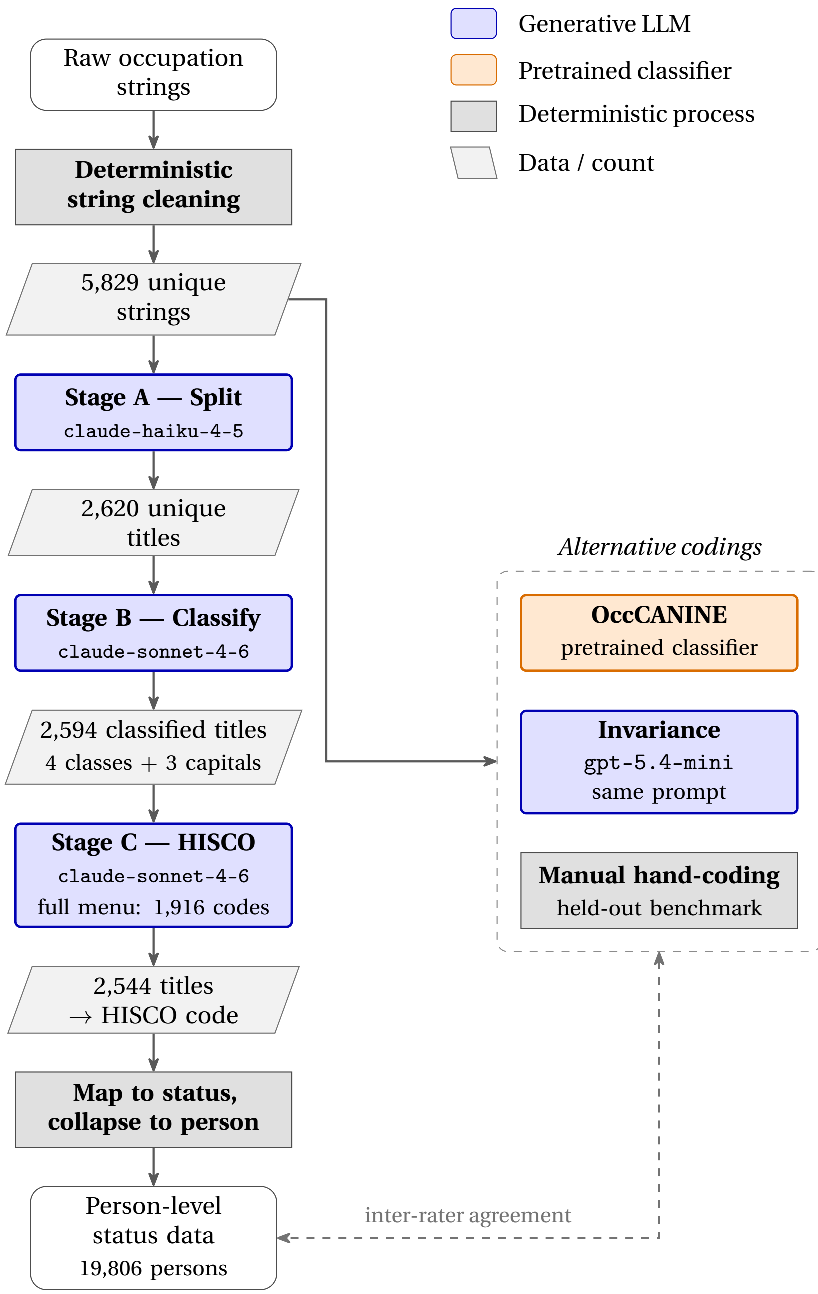


Figure A10: Occupational coding and status-mapping workflow. Rounded blue boxes are generative LLM stages, rounded orange boxes are the pretrained classifier, and square gray boxes are deterministic (rule-based) steps; parallelograms carry the input/output counts. The main column is the primary coding pipeline; the dashed panel holds the three alternative codings used to validate it.

Table A18: Language-model settings by stage

	Stage A (split)	Stage B (classify)	Stage C (HISCO)
Model	`claude-haiku-4-5`	`claude-sonnet-4-6`	`claude-sonnet-4-6`
Temperature	0	0	0
Restricted output	list of titles	class + 3 capital ratings	one of 1,916 HISCO codes

Stage C — HISCO coding. The same frontier model assigns each classified title a HISCO code from the *identical full menu* of all 1,916 codes, so invalid codes are impossible by construction. Given the German title and its Stage B translation, it picks the single most specific label matching the actual work content, or *none* when no code genuinely fits (prompt 3). The assigned code maps to the continuous HISCAM scale through a standard crosswalk (Lambert et al. 2013).

Alternative codings. The primary coding is validated against three independent mappings of the same cleaned strings: `OccCANINE`, a pre-trained classifier that assigns HISCO codes directly; a second-provider invariance check that re-runs the Stage B prompt through `gpt-5.4-mini-2026-03-17`; and the earlier manual hand-coding of the Wittgenstein titles, reconstructed at the string level and used as held-out ground truth. Agreement across codings is reported in Table A6.

System prompts. The three stage prompts are reproduced verbatim.

1. *Stage A — split.*

```
Extract individual German occupation titles from historical text
(Wittgenstein parish registers, 1600-1850). German special characters
appear folded (ae/oe/ue/ss).

Rules:
- Extract each distinct occupation separately. Do NOT split compound
  occupations (a qualifier plus trade is ONE title).
- Ignore names of employers (eg Kern), places of employment (eg Hof- or
  Papierfabrik), place names, personal names, and honorifics without an
  occupation.
- Return an empty list if the text contains no occupation.

Examples:
"meister schmied und baecker" -> ["meister schmied", "baecker"]
"koeniglicher foerster"       -> ["foerster"]
"ackermann schneider"         -> ["ackermann", "schneider"]
"zu ilsenbach"                -> []
```

2. *Stage B — classify.*

```
You are a researcher classifying historical German occupational titles
(Wittgenstein region, 1600-1850) by social class. You receive ONE
occupation title per message; German special characters appear folded
(ae/oe/ue/ss). Translate the title to English, rate the three forms of
capital it commands, and assign the overall class. Do not split compound
titles; classify the compound as a whole.

Rate each capital dimension 1-4:

human_capital -- skills, training, education the occupation requires:
1 = none or minimal (day labour, domestic service, herding)
2 = basic skills learned on the job (farming, common trades, soldiering)
3 = extensive training or guild apprenticeship (master crafts, clerks,
    skilled trades)
4 = higher or professional education (clergy, medicine, law, teaching,
    higher officials)

physical_capital -- land, tools, workshop or business capital typically owned:
1 = none (wage labour, servants, vagrants)
2 = modest land or tools (smallholders, tenant farmers, common craftsmen)
3 = a workshop, substantial land, or a business (master craftsmen,
    innkeepers, traders, full farmers)
4 = large capital (estates, factories, wholesale trade)

social_capital -- standing, authority, and networks in the local community:
1 = marginal (beggars, vagrants, servants, day labourers)
2 = ordinary standing (farmers, common trades, common soldiers)
3 = respected position or local office (guild masters, clerks, foresters,
    sextons, innkeepers)
4 = elite authority (higher officials, clergy, officers, large merchants,
    nobility)

social_class -- the overall judgment across the three capitals:
1. Lower Class: minimal skills, no capital or land, low social status
   (eg Knecht, Tageloehner, Dienstbote, Fabrikarbeiter, Hirte, Bettler)
2. Lower-Middle Class: basic skills or modest land, moderate standing
   (eg Bauer, Kleinbauer, Paechter, Ackermann, Weber, Spinner, Korber,
    Soldat, Unteroffizier)
3. Upper-Middle Class: significant skills or business ownership, good standing
   (eg Schmied, Zimmermann, Goldschmied, Kraemer, Gastwirt, Buchhalter,
    Schreiber, Beamter, Forstmeister)
4. Upper Class: high education, capital or land ownership, elite status
   (eg Lehrer, Arzt, Pfarrer, Apotheker, Grosshaendler, Fabrikbesitzer,
    Offizier, Gutsherr, Adliger)

The ratings must be consistent: social_class reflects the title's
combination of capitals, but the dimensions need not be equal -- a pastor
commands high human and social capital with little physical capital.

Use "none" on all fields only if the text is not a classifiable occupation
(a place, a name, an illegible fragment).

Source-specific notes: "kapellmeister" in these registers denotes the holder
of a church office, not a musician. Purely agricultural occupations were not
recorded in this source, so farm-related titles that do appear (ackermann,
```

```
hofmann) usually mark land-holding status.

Examples:
"tageloehner"   -> english "day labourer", human_capital 1, physical_capital 1,
                   social_capital 1, social_class 1
"ackermann"     -> english "farmer", human_capital 2, physical_capital 3,
                   social_capital 2, social_class 2
"meister schmied" -> english "master smith", human_capital 3, physical_capital 3,
                   social_capital 3, social_class 3
"pfarrer"       -> english "pastor", human_capital 4, physical_capital 1,
                   social_capital 4, social_class 4
"wtgstein"      -> english "", all fields "none"
```

3. *Stage C — HISCO.*

```
You are an expert at coding historical occupations into HISCO (Historical
International Standard Classification of Occupations).

You receive one occupation title (German original plus English translation)
and a list of candidate HISCO codes with their standard English labels. Pick
the single code whose label best matches the occupation’s actual work content
-- choose the most specific candidate that fits what the person actually did.
If no candidate genuinely fits the occupation, answer "none" -- do not force a
poor match.
```

E External Validity: Sample Restrictions

To establish when a reconstitution is incomplete, I compute three measures. (1) The number of horizontal links (marriages) per 1,000. (2) The number of vertical links (parent-child) per 1,000 normalized by the number of generations. Lastly, (3) I adopt a rule used by Blanc (2023), Blanc (2024), and Gay et al. (2026) to exclude crowd-sourced genealogies that under-reported fertility to the community reconstitution level by computing the share of individuals with an ancestor in the past four generations that has fertility strictly greater than one. Due to differing rates of censoring from migration, the cut-offs across these measures are not clear *a priori*. Thus, I take a data-driven approach and benchmark them against the Wittgenstein reconstitution. The rule is one-sided: a reconstitution is retained only if it lies no more than one standard deviation *below* Wittgenstein on each of the three measures, so that books recording more densely than the benchmark are kept rather than screened out. The standard deviation is that of the measure across the community books, not a dispersion internal to Wittgenstein. This leaves a fertility sample of 310 community reconstitutions. Figure A11 plots these restrictions across all three measures.

As a robustness exercise, I estimate the fertility differential across different sample restrictions in Figure A12: β tends to increase with sample restrictions, but the significance and direction of the coefficient persist even in a fully unrestricted sample.

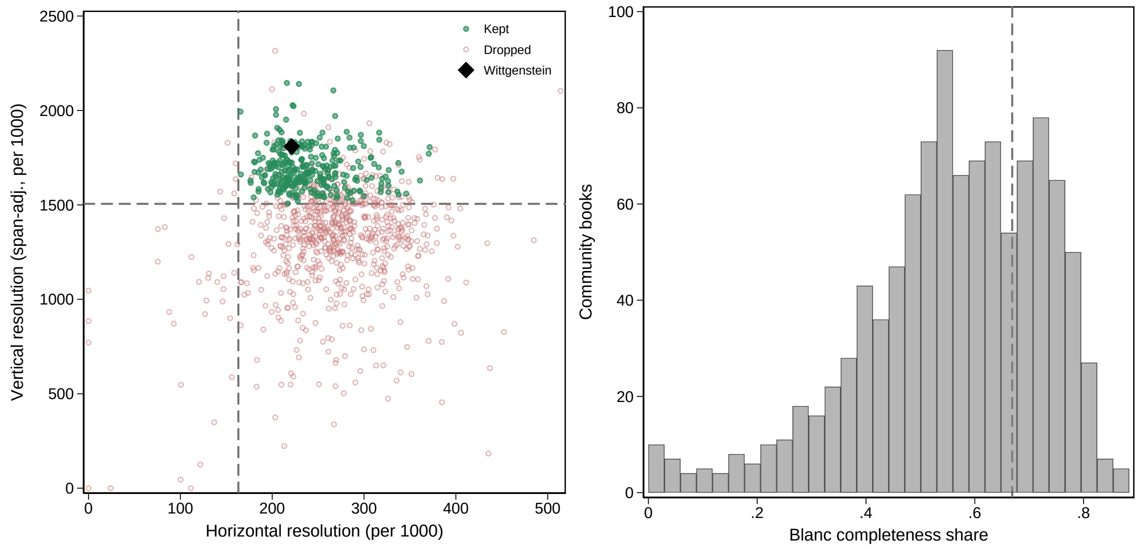


Figure A11: Distribution of Community Books by Completeness

Notes: Distribution of the German community books across the completeness measure used to screen incomplete reconstitutions, following Blanc (2023). A book is dropped from the fertility sample if it falls more than one standard deviation below the Wittgenstein benchmark on any of the three measures. The standard deviation is taken across the community books, and the rule is one-sided, so books recording more densely than Wittgenstein are retained.

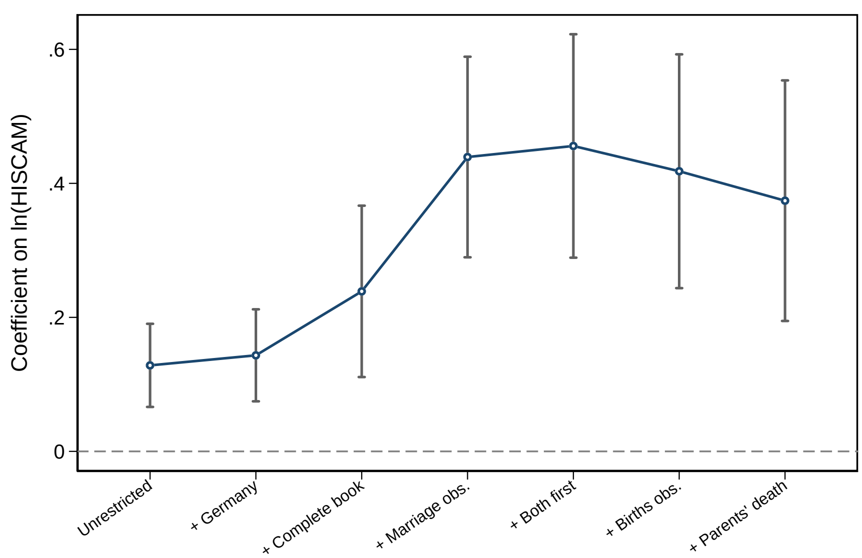


Figure A12: Fertility Gradient by Sample Restriction

Standard errors clustered by community book; whiskers are 95% confidence intervals.

Notes: OLS estimates of the fertility gradient (the coefficient β on $\ln(\text{HISCAM})$, with community-book-by-marriage-decade fixed effects) as the German community-book sample is progressively restricted to more complete reconstitutions. The gradient increases in the strictness of the completeness restriction, consistent with under-reporting of fertility in the least complete books.

F Migration

Migration-induced censoring of life histories constitutes another source of bias. As discussed in section 3, only non-migrants and in-migrants prior to marriage are included in the sample. This restriction can become problematic through two related, albeit distinct, routes. First, if the demographic behavior of the uncensored subset of the population is not representative of the general population, the results are subject to selection bias. Second, if migration is a function of both the exposure and outcome – e.g., if celibacy and low status are associated with greater rates of emigration – this introduces collider bias since the inclusion restrictions condition the sample on migration.[72]

To circumvent the imperceptibility of out-migrant outcomes, de la Croix et al. (2019) suggest looking at the differences between in-migrants and non-migrants. This test is

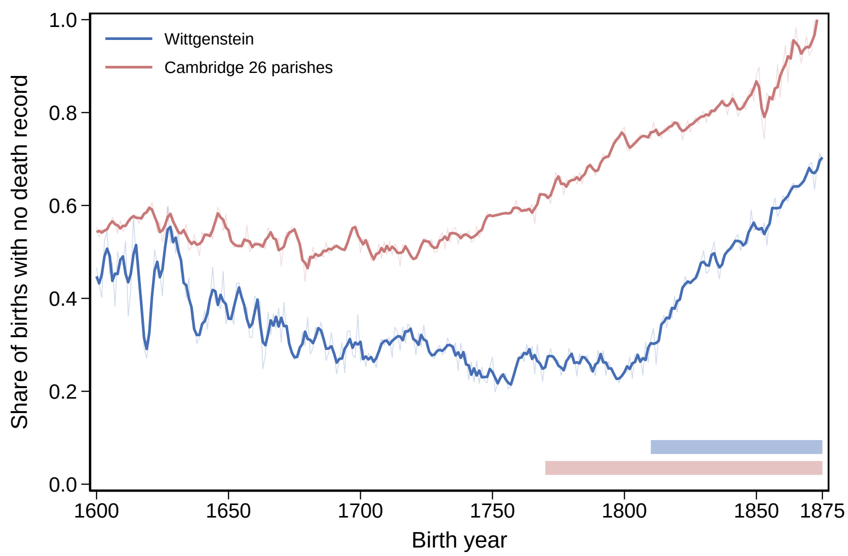


Figure A13: Births without a Recorded Death, Wittgenstein and the Cambridge Reconstitution

Notes: Share of recorded births whose life history never reaches a burial or death record, by birth year; faint lines are the annual series and bold lines a centred three-year moving average. A birth that never acquires a death record has left observation, so the share is a censoring proxy that reads as out-migration. The Cambridge series is constructed identically on the 26-parish reconstitution (Wrigley et al. 1997). Both sources also lose their youngest cohorts to register closure — Wittgenstein in 1876, Cambridge in 1837 — because a birth shortly before closure may still be alive when recording stops. The two bars above the axis, one per source in its own colour, mark the birth years falling within seventy years of each closure, where censoring rather than migration dominates. Over the unmarked stretch, 1600–1760, the share is 31.1% in Wittgenstein against 53.4% in the Cambridge parishes. The Wittgenstein series covers births in the sixteen core parishes, excluding stillbirths; its greater volatility before roughly 1650 reflects thin annual counts.

[72]For a fuller discussion of collider bias, see Schneider (2020).

Table A19: Fertility by Migration Status

	Non-migrants	In-migrants	No Int. Migr. (post)	No Int. Migr. (full)
	(1)	(2)	(3)	(4)
ln(HISCAM)	0.967***	1.755**	1.155***	0.748*
	(0.231)	(0.578)	(0.231)	(0.344)
Mean DV	3.710	3.241	3.632	3.735
Observations	3,663	638	3,804	2,316
Parishes (clusters)	15	13	13	13
R^2	0.111	0.211	0.109	0.123

Standard errors clustered by marriage parish in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
Notes: OLS estimates of net fertility on ln(HISCAM) within four migration-status subsamples, each with marriage-decade-by-parish fixed effects. Column (1), Non-migrants: both parents born in one of the main Wittgenstein parishes. Column (2), In-migrants: at least one parent born outside the main marriage parish. Column (3), No Internal Migration (post): couples with no internal (within-territory) migration after marriage. Column (4), No Internal Migration (full): couples with no internal migration at any point observed. Columns (3) and (4) are not exclusive of columns (1) and (2), and column (4) is a subset of column (3).

Table A20: Intergenerational Transmission (λ): Migrants vs. Non-Migrants

	(1)	(2)	(3)	(4)	(5)	(6)
	N: G2–G3	N: G1–G3	$\widehat{\beta}_{-1}$	$\widehat{\beta}_{-2}$	$\widehat{\lambda}$	SE($\widehat{\lambda}$)
Non-migrants	351	351	0.602	0.416	0.691***	(0.129)
In-migrants	2,404	2,404	0.385	0.220	0.572***	(0.066)
Difference	–	–	–	–	0.119	(0.145)

Cluster-bootstrap standard errors in parentheses (500 replications, resampled by the son's (G3) birth parish). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
Notes: $\widehat{\beta}_{-1}$ and $\widehat{\beta}_{-2}$ are the coefficients on the father's (G2) and grandfather's (G1) average ln(HISCAM) status, respectively, from two separate OLS regressions of the son's (G3) ln(HISCAM) status, each absorbing fixed effects for the father's (G2) marriage decade; $\widehat{\lambda} = \widehat{\beta}_{-2}/\widehat{\beta}_{-1}$ is the implied transmission coefficient. The Difference row reports the non-migrant minus in-migrant gap in $\widehat{\lambda}$ with its bootstrap standard error.

contingent on the assumption that the unobserved out-migrants (who immigrate to a similar nearby parish) are the same group as the observed in-migrants (who emigrated from such a parish). In the Wittgenstein reconstitution, where short-distance migrants are observed as non-migrants, this appears unlikely. However, a comparison between the two groups still yields some insights. Estimating the primary specification for in- and non-migrants reveals that while point estimates vary, I cannot reject equality of the estimates (see Table A19). Non-migrants here are couples in which both spouses were born in one of the main Wittgenstein parishes, so a move between contiguous parishes before marriage does not make a spouse an in-migrant. Here, in columns (3) and (4), I also check whether couples that migrated internally behaved differently, but I find no statistically significant evidence for this. These two columns are not exclusive of columns (1) and (2): they re-cut

Table A21: Migration Decision

	$\mathbb{1}\{\text{OutMigrant} = 1\}$	
	(1)	(2)
$\ln(\text{HISCAM})$	-0.038	
	(0.025)	
Lower-middle Class		-0.023
		(0.018)
Upper-middle Class		-0.039**
		(0.015)
Upper Class		-0.019
		(0.023)
Mean DV	0.341	0.341
Observations	22,987	24,274
Parishes (clusters)	16	16
R^2	0.181	0.178

Standard errors clustered by marriage parish in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
Notes: OLS estimates of an indicator for child out-migration with marriage-decade-by-parish fixed effects. Column (1) uses $\ln(\text{HISCAM})$; column (2) uses the discrete social-class measure, with the lowest class as the omitted baseline.

the same couples by whether a move is observed after marriage (3) or at any point (4), so column (4) is a subset of column (3). The same applies to intergenerational transmission, where the point estimate for in-migrants is smaller, but I again cannot reject equality (see Table A20).

To evaluate whether collider bias affects my results, I would need to test whether migration is a function of occupational status and demographic outcomes. Although demographic outcomes are unobservable for out-migrants, I can run individual-level linear probability models to estimate the effect of a father's occupational status on the choice to emigrate. I operationalize emigration as an indicator variable equal to one if there is no death record, or the death record is not from a core parish of the reconstitution. I find no statistically significant effect for $\ln(\text{HISCAM})$ (see Table A21). However, I do find that children born to upper-middle-class fathers are statistically significantly less likely to emigrate. This relationship between status and migration would bias the results if migration decisions are also affected by the demographic outcome in question. Turning to the aforementioned example of celibacy, if rates of out-migration are greater among celibate women – i.e., to migrate to an urban center with a larger marriage market – the results reported in Table A22 likely underestimate rates of celibacy among women with lower occupational status since these are *a priori* more likely to be part of the excluded group. For such an association to drive results, the demographic outcomes would have to be a central driver of the migration decisions. Since it is impossible to verify whether the outcome affects migration, all results presented in section 4 are interpreted under the identifying assumption of no such association.

G Extensive Margin of Fertility

To study childlessness, I regress a binary indicator for childlessness $\mathbb{1}\{\text{Childless}_i\}$ on $\ln(\text{HISCAM})$ in a linear probability model; the coefficient is insignificant and economically small (Column (1), Table A22). The probability of remaining childless, conditional on marriage, is 0.036 irrespective of occupational status. Remaining childless was likely driven by biological chance and infertility instead of socioeconomic factors. To study celibacy, I look at the life histories of all surviving children born in Wittgenstein. To ensure that celibacy is not biased upwards for more geographically mobile groups, only individuals whose burial is recorded in Wittgenstein are included. I regress an indicator variable equal to one if I do not observe a marriage $\mathbb{1}\{\text{NoMarriage}_i\}$ on father's $\ln(\text{HISCAM})$.[73] For both men and women who stayed in Wittgenstein, there is no statistically significant gradient in celibacy (Columns (2) & (3), Table A22). The average rate of celibacy for men is statistically significantly higher than for women (0.263 vs 0.174). This could be a manifestation of the dynamics described by Guinnane and Ogilvie (2014), whereby certain groups of men were excluded from the marriage market, although these groups do not overlap with my occupational status measure. Alternatively, these rates could reflect differing propensities to out-migrate. While celibate men stayed in the parental household, women who did not succeed in the local marriage market were sent abroad to seek out better prospects.[74]

[73] Occupation was normally recorded at marriage or at offspring's baptisms. Thus, celibate men were much less likely to have recorded occupations. The sample of men who have a recorded occupation regardless is thus highly selected on more notable occupations (that were recorded at death).

[74] Since I can only identify celibacy for individuals who did not migrate, I cannot test this empirically. Across the entire sample, men were statistically significantly more likely to out-migrate. However, it is difficult to pinpoint how this interacted with marriage markets.

Table A22: Extensive Margin

	Childlessness	Celibacy	
	(1)	(2) Male	(3) Female
$\ln(\text{HISCAM})$	0.032	-0.006	-0.004
	(0.025)	(0.044)	(0.039)
Mean DV	0.036	0.263	0.174
Observations	2,249	4,567	4,884
Parishes (clusters)	15	14	15
R^2	0.117	0.175	0.118

Standard errors clustered by marriage parish (column 1) and by birth parish (columns 2–3) in parentheses.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
Notes: OLS linear-probability estimates. Column (1) regresses an indicator for childlessness on $\ln(\text{HISCAM})$ for couples observed over the full fertility period in the non-migrant sample, with marriage-decade-by-parish fixed effects. Columns (2)–(3) regress an indicator for never marrying (celibacy) on the father's $\ln(\text{HISCAM})$, separately for sons and daughters, with birth-decade-by-birth-parish fixed effects.

H Multi-generational Transmission

I estimate elasticities of status across one ($G3 - G2$), two ($G3 - G1$), and three ($G3 - G0$) generations.

$$\ln(\text{HISCAM}_{i,G3}) = \kappa + \beta_{-m} \ln(\text{HISCAM}_{i,G3-m}) + \eta_{t,G3} + \epsilon_{i,G3} \tag{A9}$$

Additionally, I estimate an AR(3) model that regresses G3 status on the occupational status of the three prior generations. In the absence of measurement error, this model estimates the effect of fathers (G2) conditional on direct grandfather (G1) and great-grandfather (G0) effects. Throughout, occupational status is measured using HISCAM. In the baseline specification, when individuals have multiple status observations, I use a random draw. In later specifications, to test the identifying assumption and account for measurement error, occupational status is averaged across available occupations. To account for time variation in average occupational status, decade fixed effects $\eta_{t,G3}$ are included. All standard errors are clustered at the parish level.

Columns (1), (3), and (5) of Table A23 report baseline estimates for multi-generational elasticities. Across the board, the multi-generational effect is larger than the predicted effect based on an AR(1) transmission mechanism. Accordingly, the AR(3) model in column (7) implies that the grandfather effects persist – both in statistical and economic significance – when I condition on the direct father effect. However, as discussed in

Table A23: Multi-generational Elasticities of SES

	$\ln(\text{HISCAM})^{G3}$							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\ln(\text{HISCAM})^{G2}$	0.296***	0.392***					0.242***	0.322***
	(0.051)	(0.045)					(0.034)	(0.024)
$\ln(\text{HISCAM})^{G1}$			0.212***	0.266***			0.115***	0.088*
			(0.049)	(0.060)			(0.021)	(0.042)
$\ln(\text{HISCAM})^{G0}$					0.156***	0.185***	0.082***	0.075**
					(0.043)	(0.051)	(0.024)	(0.027)
Predicted Effect			0.087	0.153	0.026	0.060		
Excess Persistence			59	42	83	67		
Observations	1,530	1,530	1,530	1,530	1,530	1,530	1,530	1,530
R^2	0.188	0.224	0.145	0.162	0.131	0.137	0.217	0.240

Standard errors clustered by the son's (G3) birth parish in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
Notes: OLS estimates with fixed effects for the father's (G2) marriage decade. The dependent variable is the son's (G3) $\ln(\text{HISCAM})$; the regressors are the father's (G2), grandfather's (G1), and great-grandfather's (G0) $\ln(\text{HISCAM})$. Odd-numbered columns use the status recorded at a single observation; even-numbered columns use the individual's average status across observations, which partially corrects for measurement error. All columns are estimated on the same sample of paternal lineages in which occupational status is observed for all four generations. Predicted Effect is the multi-generational coefficient implied by iterating the one-generation elasticity estimated on this sample, $\widehat{\beta}_{-1}^{k}$. Excess Persistence is the share of the observed coefficient that iteration leaves unexplained, $100 \times (\widehat{\beta}_{-k} - \widehat{\beta}_{-1}^{k})/\widehat{\beta}_{-k}$, in percent.

section 5, assigning a structural interpretation to these associations is rife with problems. As such, these coefficients do not necessarily imply a direct grandfather effect (Ward et al. 2025). Comparing estimates to ones that partially correct for measurement error – see columns (2), (4), (6), and (8) – suggests spurious multi-generational effects. Although using average occupational status is only a partial remedy, it suffices to increase the magnitude of β_{-1} by 33%, while reducing excess persistence by 17 pp for the grandfather effect (β_{-2}), and 16 pp for the great-grandfather effect (β_{-3}). Similarly, in the AR(3) model, the measurement error correction reduces the relative magnitude of the grandfather effect by 20 pp. In the absence of multiple independent occupational-status measurements per observation, I am unable to fully account for measurement error. Thus, it is impossible to conclude whether the significant grandfather effects are statistical artifacts. Still, given this pattern and existing evidence on multi-generational mobility, it appears likely that the remaining effect is driven by measurement error. All subsequent analyses are conducted under the identifying assumption of no structural grandfather effect.