Nepotism vs. Intergenerational Transmission of Human Capital in Academia (1088–1800)

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Abstract

We build a comprehensive database of father-son pairs in premodern academia, measure their publications, and develop a general method to disentangle two determinants of occupational persistence: nepotism vs. inherited human capital. This requires jointly addressing measurement error in human capital and selection from nepotism. Exploiting multi-generation correlations and parent-child distributional differences, we identify the structural parameters of a Markov process of intergenerational transmission with nepotism. The human capital elasticity is lower than in standard multi-generation estimates ignoring nepotism. Nepotism was lower in science vs. law, in Protestant institutions, and in fields experiencing rapid changes in the knowledge frontier. It declined during the Scientific Revolution and the Enlightenment—testifying to the rise of meritocracy.

1 Introduction

Universities and scientific academies were essential for the Commercial Revolution (Cantoni and Yuchtman 2014), the Scientific Revolution (Applebaum 2003), and the Enlightenment (Mokyr 2010). Yet, these institutions are often criticised for remaining attached to old paradigms, selling diplomas, and accepting the appointment of relatives.[1] Between 1088 and 1800, one in twenty scholars worked in the same university or academy as his father, and sons tended to publish less than their peers—the library holdings of the average son are 83.4% those of the average outsider (see Figure 1). This may indicate that children benefited from their parents' social connections to get jobs ahead of better-qualified candidates (henceforth, nepotism). That said, family dynasties are common in high-talent occupations, which can be optimal if talent is scarce and children's human capital depends on parental investments and inherited knowledge and skills (henceforth, inherited human capital).

Disentangling inherited human capital from nepotism is important as their economic implications are fundamentally different: while dynasties based on inherited human capital can reflect meritocracy and increase productivity, nepotism leads to a misallocation of talent. Although the term "meritocracy" is a modern construct, its virtues are known since Plato and Confucius (Wooldridge 2021). Nepotism and talent misallocation, in contrast, is specially harmful in high-talent markets (Murphy, Shleifer, and Vishny 1991; Hsieh et al. 2019), affecting the production of ideas, upper-tail human capital, technological progress, and economic growth (Mokyr 2002).

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[1] See Dulieu (1983) on Montpellier's medical faculty, Slottved and Tamm (2009) on U Copenhagen, and Connor (1947) on the Cassini dynasty at the Paris Observatory and French Academy of Sciences.

Disentangling inherited human capital from nepotism, however, is challenging. Inherited human capital endowments are hard to measure, as they are only transformed into observed outcomes (e.g., occupation, earnings, performance) with noise. Recent studies suggest that this measurement error bias can be large, severely attenuating intergenerational elasticity estimates.[2] In addition, inherited social connections can lead to nepotism, creating barriers of entry to certain occupations. This introduces a different bias, selection, as sons of insiders are selected into top occupations under different criteria than outsiders. Finally, microdata with direct parent-child links is hard to come by in historical settings. Previous estimates on the parent-child transmission of human capital and social connections are limited to modern settings or rely on surname pseudo-links to study its evolution over centuries.[3]

This paper quantifies the relevance of nepotism vs. inherited human capital in academia over seven centuries. To do so, we build a comprehensive dataset with direct links between 1,837 sons and fathers in 116 universities and 63 academies from 1088 to 1800. We measure their scientific output using 4,106,901 library holdings by or about each scholar that are held in more than 10,000 libraries today (henceforth, publications). We develop a new structural method to overcome the empirical challenge of disentangling inherited human capital from nepotism. Our method jointly addresses the measurement error bias emerging because publications are a noisy proxy of human capital, and the selection bias emerging because of nepotism. We do so by exploiting two sets of moments: *(i)* correlations in publications across generations—a standard moment to assess intergenerational elasticities; and *(ii)* differences in the marginal publications distribution between the set of fathers and the set of sons—a novel moment. We find that nepotism was prevalent in pre-modern academia until it declined dramatically during the Scientific Revolution and the Enlightenment. Family dynasties did not disappear, but they now emerged as a result of the human capital, knowledge, and other productive endowments that children inherited from their parents. We find that such upper-tail human capital endowments were inherited with an elasticity of 0.6-0.65. This is a higher estimate than suggested by parent-child correlations; but lower than previous long-run estimates relying on surname pseudo-links.

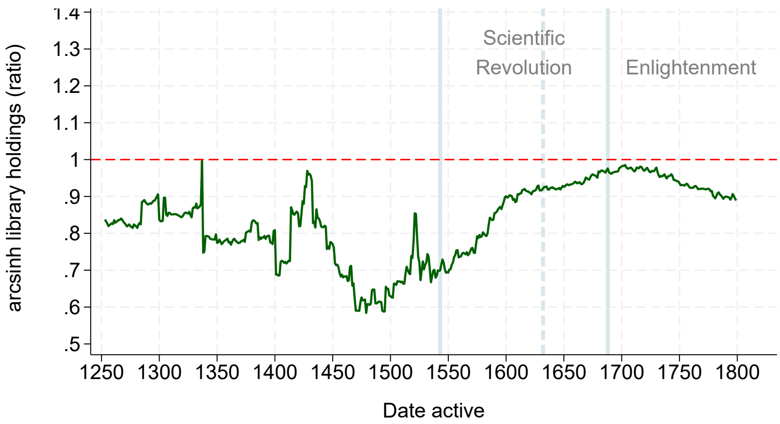
Figure 1 illustrates our main findings, as well as the empirical challenges associated with disentangling nepotism from inherited human capital. It shows the number of library holdings in modern libraries by or about an average scholar's son relative to an average outsider between 1250 and 1800 based on 20,500 scholars listed in WorldCat. Over 1250–1800, the ratio is always below one. On average, the publications of sons are 83.4% those of outsiders, suggesting that scholar's sons were less productive. The figure also shows that their publications converged over time. The publications of scholars' sons were 80% those of outsider scholars until 1400, and as low as 60% around 1500.[4] This pattern reversed slowly with the start of the Scientific Revolution and, by 1632—when Galileo's *Dialogue* was published, the average scholar's son published close to 90% as much as the average outsider. At the dawn of the Enlightenment (1687–1800) we observe no differences between the scientific out-

[2] For example, Lindahl et al. (2015) show that human capital endowments are more persistent across multiple generations than suggested by parent-child elasticities.

[3] Clark (2015), Häner and Schaltegger (2022), Barone and Mocetti (2020).

[4] This corresponds to 100-200 fewer library holdings in modern libraries.

FIGURE 1: Publications of sons of scholars relative to outsiders over time



Notes: The sample is 20,500 scholars from institutions with complete and broad coverage who are listed in Worldcat. The figure shows the ratio of the library holdings of the average son to the library holdings of the average outsider over time. We use the hyperbolic sine transformation of the number of library holdings. "Sons" means sons in the same institution as fathers. Trends are based on a 50-years moving average (100-years moving average before 1400). We exclude outliers (99-percentile) for both groups. We ommit trends before 1250 because the sample restrictions above reduce to 5 the number of sons before 1250.

put of scholars' sons and outsiders.

These trends motivate our main finding of a high incidence of nepotism, which allowed sons to use their father's connections and enter academia even if their human capital endowments were low. It also suggests that nepotism faded during periods of rapid scientific advancement like the Scientific Revolution, but that family dynasties remained in academia because human capital was transferred from parents to children with a high elasticity.

That said, the figure also highlights some of the empirical challenges associated with disentangling nepotism from inherited human capital, which we resolve in this paper. Comparisons between scholars' sons and outsiders are informative of general trends. However, they conflate the negative effect of nepotism with the positive effect of human capital transfers from fathers to sons. One cannot simply assume that, absent nepotism, the output of sons of scholars would equal that of outsider scholars. Absent nepotism, we would expect sons of scholars to publish as much as outsiders (ratio=1) if human capital reverted to the mean after just one generation, and more than outsiders (ratio>1) in the more realistic scenario where human capital transfers from fathers to sons mattered for a scholar's research output. The productivity gap, hence, depends on the elasticity at which parents transfer their human capital to their sons (β). In other words, to disentangle nepotism from human capital transfers and to quantify their relative importance over time, it is crucial to observe father-son links, infer the father's human capital endowments, and the elasticity at which these are transferred to their sons. In most empirical settings, there is no information about the parents of outsiders, their occupations (here, outside academia), or human-capital proxies comparable across occupations (here, a measure akin to publications in other occupations). Without this information, it is virtually impossible to estimate the inherited human capital endowments of outsiders and its importance relative to nepotism in explaining differences in publications with scholars' sons. We overcome these issues by exploiting direct parent-child links and comparing scholars' sons and fathers, and validate our results *ex post* with comparisons between outsiders and scholars' sons.

Our first contribution is to build a new dataset with direct parent-child links to study nepotism and the intergenerational elasticity of upper-tail human capital in pre-modern academia. We focus on university professors and members of academies. We build our dataset using hundreds of secondary sources, such as university catalogues, books on the history of each university, and compendia of professors. We establish and verify direct family links by matching each scholar to old biographical dictionaries and online encyclopedias (e.g., *Allgemeine Deutsche Biographie*, *Treccani*, and Dictionary of National Biography). Given the completeness of our sources, we collect the universe of father-son pairs in most institutions. We measure their publications using WorldCat—a comprehensive online catalogue of modern libraries worldwide. Specifically, we define publications as their library holdings in modern libraries, which capture the size as well as the long-run relevance of a scholar's scientific output.

We begin our analysis by documenting two stylized facts for families of scholars. The first fact is that a scholar's publications strongly depend on his ancestor's publications. The father-son correlation on the intensive margin is 0.375, and the grandfather-grandson correlation is stronger than predicted by iterating the father-son correlation. The second fact is that there are large differences between the marginal publications distribution of the set of fathers and the set of sons—that is, between first-generation scholars and subsequent insiders. The fathers' distribution first-order stochastically dominates the sons' distribution, and differences are largest at the bottom.

We show that these two facts cannot be reconciled with standard intergenerational models based solely on the transfer of human capital (Becker and Tomes 1979, 1986). Fact 1 implies that the underlying human-capital endowments determining publications were strongly transmitted from parents to children, and hence, that the advantages and disadvantages of ancestors vanished at a slow rate, i.e., a slow rate of mean reversion. In contrast, Fact 2 implies a substantially faster rate of mean reversion, as the set of sons has a substantially worse publication record than the set of fathers. These two apparently contradictory facts can be reconciled by extending this standard framework with the transfer of social connections that can result in nepotism. Nepotism allows scholars' sons to become scholars even when their human capital is lower than the marginal father, generating differences at the bottom of the distribution (Fact 2) even when publications on the intensive margin strongly persist across generations (Fact 1).

Our second contribution is to develop a general method to disentangle inherited human capital from nepotism, and to use the two facts described above to estimate their relative importance in pre-modern academia. Our method recovers the intergenerational human capital elasticity from the father-son correlations in publications; and nepotism from the excess distributional differences between the set of fathers and sons, net of the effect of mean reversion in human capital. Formally, we structurally estimate the parameters of a first-order Markov process of human capital transmission (Clark and Cummins 2015; Braun and Stuhler 2018), extended to account for nepotism.

Our model economy consists of a population of potential scholars whose unobserved human capital is transmitted from fathers to sons with an elasticity of β. Potential scholars with high human capital endowments become scholars, but there is a selection bias: the selection criterium for scholars' sons can be different because of nepotism. We characterize nepotism as the share of scholars' sons who would not have become scholars under the same criterium as a first-generation scholar. For selected scholars, the unobserved human capital endowments are transformed into an observed outcome, publications, with measurement error noise. The two sets of moments characterizing Facts 1 and 2 can be used to identify the deep parameters of this model: Father-son correlations in observed outcomes are a standard moment to characterize the rate β at which human capital is transmitted from fathers to sons.[5] Because publications are a noisy proxy of human capital, we also use correlations between grandparents and grandchildren, a method proposed by Lindahl et al. (2015) and Braun and Stuhler (2018) to correct for measurement error.Finally, when the distributional differences between the set of fathers and sons are larger than predicted by the rate of reversion to the mean, it reflects that parents and children are selected under different criteria, i.e., nepotism. This should be specially visible at the bottom of the distribution, that is, close to the selection threshold where nepotism is binding. The excess distributional differences, net of the effect of mean reversion, can hence be used to identify nepotism. We use the Simulated Method of Moments to obtain estimates for the intergenerational elasticity of human capital, β, and for nepotism by minimizing the distance between these empirical and simulated moments.

Our first result is that pre-modern academia was not free of nepotism. Between 1088 and 1543, we find that 48.8 of scholars' sons would not have become scholars under the same criteria than their fathers. This nepotism estimate declined to 20.35% in the Scientific Revolution (1543–1687) and to 8.3% in the Enlightenment (1687–1800).

We examine some of the historical processes behind the decline in nepotism around the Scientific Revolution. Consistent with the historical evidence, we document that a key mechanism was the foundation of modern, meritocratic institutions instead of structural reforms in existing institutions. To do so, we estimate our model separately for institutions established before vs. after the start of the Scientific Revolution, and show that the share of nepotic sons was more than 50% smaller in new institutions—such as the universities of Leiden, Jena, or the French Royal Academy of Sciences—than in the old institutions—such as the universities of Cambridge or Bologna. In addition, we show that nepotism was less prevalent in areas experiencing rapid changes in the knowledge frontier. We use data from De la Croix (2021b) on publications of all known scholars in 1500–1800 to calculate the yearly growth rate of publication by six fields of study in catholic and protestant universities. Our estimates show that nepotism was 9.2% among scholars entering academia in a time, place, and field of study which had experienced rapid changes in the knowledge frontier in the previous quarter century, and 25.3% among those entering academia in stagnant times. This suggests that the decline in nepotic practices in academia after the Scientific Revolution is explained by an increase in their costs: In eras of rapidly changing knowledge frontiers, the mismatch between talents and occupation becomes more costly, exceeding the benefits from the transmission of specific human capital from parents to children (Galor and Tsiddon 1997).

[5] See Solon (1999), Corak (2006), and Black and Devereux (2011) for reviews.

Altogether, this suggests that low levels of nepotism are associated with periods of buoyant scientific advancement. To the extent that this decline in nepotism reflects a broader decline in favouritism towards acquaintances and other societal changes reducing barriers to entry in academia, our findings suggest that meritocracy was complementary with Europe's scientific advancements before the Industrial Revolution.

Our second result is that the intergenerational elasticity of upper-tail human capital was 0.63 in pre-modern Europe. This estimate is higher than suggested by father-son elasticities in observed outcomes, confirming previous findings that two-generation estimates understate the rate at which inequalities persist over the very long run. Yet, our estimate is in the lower range of elasticities obtained via multiple generations, group-averages, or the informational content of surnames—three methods that ignore the transfer of social connections that can lead to nepotism. Specifically, elasticities obtained via multiple generations in our data are close to the 0.8–0.9 range estimated by Clark (2015). Hence, in settings with widespread nepotism, the standard multi-generational methods in the literature overstate the persistence of inherited endowments, skills, etc. which affect children's outcomes. In addition, our findings do not support Clark's hypothesis that the rate of persistence is constant through historical periods and, hence, that it reflects the transfer of genetic endowments.

We extend our analysis to examine heterogeneous effects. We find evidence of nepotism for 5–6.6% of scholars' sons in Protestant and for 29.4% in Catholic universities and academies. Catholic institutions relied more heavily on intra-family human capital transfers. We show that these differences partly explain the divergent path of Catholic and Protestant universities after the Reformation. We also document that nepotism was higher in law and medical faculties than in sciences, for sons appointed before their father's death, and for sons in the same field as their fathers. In addition, we conduct various robustness checks. First, we show that our estimates are not driven by selective reporting of father-son links in the sources used to build our data. Our estimates are robust to restricting the sample to sources covering all scholars in an institution, and hence, where we effectively identify the universe of father-son pairs. Second, we show that our findings—based on comparisons between scholars' sons and fathers—are consistent with comparisons between scholars' sons and outsiders. Third, we validate our identification strategy with a falsification test. We consider fathers and sons appointed at different institutions where, *ex ante*, we expect less nepotism. Consistently, we estimate a nepotism parameter of zero. This strongly suggests that our estimates do not conflate nepotism with other elements of the hiring process (e.g., information frictions) or with broader trends outside academia to which both our baseline and validation sample are exposed. Finally, we examine the robustness of our results to stationarity assumptions, drawing shocks from fat-tailed distributions, using unique works instead of library holdings, dropping library holdings on a scholar's work written by a different author, allowing for better access to publishers for scholars' sons, non-linearities human capital transmission, and father-son longevity differences.

Our paper contributes to three strands of literature. First, our paper is related to studies quantifying nepotism in top professions.[6] For modern academia, previous work has documented nepotism

[6] Examples are doctors (Lentz and Laband 1989), lawyers (Laband and Lentz 1992; Raitano and Vona 2018), politicians

(Durante, Labartino, and Perotti 2011) and favouritism towards acquaintances (Zinovyeva and Bagues 2015; Bramoullé and Huremović 2018) or scholars with home-ties (Fisman et al. 2018). Our paper is the first to quantify nepotism in pre-modern academia. Studies of favouritism in pre-modern organizations are scarce. An exception is Voth and Xu (2019), who find that promotions of connected British Navy officers reflected private information rather than favouritism. Methodologically, a common approach to identify nepotism in modern setings is to use natural experiments that alter the importance of connections to accessing jobs. Instead, our method allows to gauge the evolution of nepotism across time and space, beyond empirical settings where a natural experiment is available.

Second, we contribute to a large literature on social mobility by providing the first estimate for the intergenerational elasticity of upper-tail human capital over centuries. While previous long-run estimates of wealth, earnings, and occupation elasticities rely on surname pseudo-links (Clark and Cummins 2015; Barone and Mocetti 2020; Häner and Schaltegger 2022), our estimates are derived from true links across generations.[7] This allows us to directly evaluate Clark (2015)'s hypothesis that latent endowments are transmitted from parents to children at a constant rate of 0.8–0.9 over the very long run. More generally, we show that to obtain reliable intergenerational elasticities it is important to jointly address both measurement error and selection bias. Traditional elasticities bundle transfers of unobserved human capital and social connections and, hence, cannot address both biases jointly. Measurement error has been addressed using multiple-generation links (Lindahl et al. 2015; Braun and Stuhler 2018; Colagrossi, d'Hombres, and Schnepf 2019), group-averages for siblings (Braun and Stuhler 2018) and surnames (Clark and Cummins 2015; Häner and Schaltegger 2022), the informational content of surnames (Güell, Rodríguez Mora, and Telmer 2015), or horizontal kinship ties (Collado, Ortuno-Ortin, and Stuhler 2018). In line with these estimates, we find that inherited advantages are more persistent than what parent-child elasticities imply. That said, we show that, by ignoring the selection bias, these estimates can overstate the persistence of endowments like human capital or genetic advantages.[8] Importantly, here we estimate an elasticity for a specific group, fathers and sons in academia, while some of the literature estimates cited above correspond to population elasticities. Although we discuss that selection may also affect such elasticities, our critique is most relevant for long-run estimates, which are often derived from selected samples, e.g., individuals leaving a will (Clark and Cummins 2015) or ancestors and descendants that remain in one city (Barone and Mocetti 2020; Häner and Schaltegger 2022).

Third, upper-tail human capital, such as knowledge produced at universities and academies, has been deemed important for the rise of new Science (Mokyr (2002), Mokyr (2016), De la Croix et al. (2023)) and the Industrial Revolution (Galor and Moav (2002), Squicciarini and Voigtländer (2015)).

(Dal Bó, Dal Bó, and Snyder 2009), inventors (Bell et al. 2018), CEOs (Pérez-González 2006; Bennedsen et al. 2007), pharmacists (Mocetti 2016), self-employed (Dunn and Holtz-Eakin 2000), and liberal professions (Aina and Nicoletti 2018; Mocetti et al. 2018).

[7] Only two articles studied pre-modern social mobility using direct parent-child links: Shiue (2017) for the Tongcheng county in China (1300-1900), and Borgerhoff Mulder et al. (2009) for East Anglia (1540-1845).

[8] A related literature studies if human capital is genetically inherited (selection) or depends on parents' investments (causation) (Holmlund, Lindahl, and Plug 2011). Differently, we address the selection bias from nepotism to disentangle it from human capital transfers, but not whether the latter reflect nature or nurture.

Our contribution to this literature is to provide the first estimate of the intergenerational elasticity of upper-tail human capital before the Industrial Revolution. We find a slower rate of mean reversion after the Scientific Revolution. This lends credence to Galor and Moav (2002) and Galor and Michalopoulos (2012), who show that natural selection of growth-promoting traits (e.g., upper-tail human capital) is more likely when parents pass on such traits with a high probability. In addition, we show that periods of rapid scientific advancement were associated with less nepotism in universities and academies. This provides empirical support to the hypothesis by Greif (2006) and De la Croix, Doepke, and Mokyr (2018) that, in pre-industrial Europe, the dissemination of knowledge in corporations was not slowed down by family networks. Our finding that nepotism becomes more costly when the knowledge frontier is rapidly changing is related to an earlier literature showing that, in periods of rapid technological change, intergenerational mobility increases and the relative importance of the transmission of parental-specific human capital declines (Galor and Tsiddon 1997). This idea is also present in studies of the decline of the family firms over the course of development (Carillo, Lombardo, and Zazzaro 2019). Finally, we also provide new insights on the divergence of Catholic and Protestant universities beyond traditional explanations centred on religious values (Merton 1938) or the Counter-Reformation (Landes 1998; Blasutto and De la Croix 2023; Dewitte et al. 2022). More generally, our results relate to a large literature showing that distortions in high-talent markets can drastically affect the production of ideas (Bell et al. 2018). Examples of such distortions include family-successions of CEOs (Pérez-González 2006; Bennedsen et al. 2007).

2 Institutional background and data

2.1 Recruitment

Although norms varied across universities and academies, the recruitment process shared some general characteristics. The recruitment of university professors typically involved the faculty, who proposed to appoint a candidate to a chair, and an external authority (e.g., Monarch, Church, Municipality, Corporation), who approved it. Most chairs were filled by public competition, but appointments were sometimes transferred to a representative of the authorities (Rashdall 1895: vol 2, p. 192). For example, the University of Copenhagen initially appointed its professors. Following the introduction of Absolute Monarchy in 1660, these appointments had to be approved by the King. Both steps of the recruitment process were subject to nepotism. Slottved and Tamm (2009, 42-43) argues that Thomas Bartholin (1616–80) used his social connections both at the University of Copenhagen and at the court to promote his relatives' careers. On the one hand, his permanent position as Dean of the Medical Faculty gave him influence over matters of importance at the University, particularly over appointments. On the other hand, Bartholin ingratiated himself with the King's chancellor, who also served as Chancellor of the University. Other well-documented examples of nepotism are the Géraud de Vaxis and Lefranc families, who secured jobs at the University of Cahors for several generations (Ferté 2000). Interestingly, Ferté wonders why it was so important for them to secure those jobs given the low salary paid by the University. The reasons were prestige and notability, the same factors pushing people to publish (Mokyr 2016).

In academies, new members were elected by co-option—that is, they were elected at the discretion of existing members. In general, a member (or a group of members) sponsored an external candidate. All academy members then voted whether to accept this candidate (Foster and Rücker 1897). The available election certificates of Royal Society fellows shows that fathers never sponsored their sons there. This suggests that, if there was nepotism, it was the result of fathers influencing the vote of their fellows. In some academies, the candidates had to submit a written work for evaluation (Galand 2009). As in universities, the nomination of new academy members was sometimes subject to the approval of external authorities. For example, in the French and Spanish Academies, the votes for new members had to be approved by the King.

Besides chaired professors and academy members, our database contains a myriad of other scholarly positions. These include university regents in France, docents in Germany, or fellows in England, and various positions in academies, e.g., corresponding member, honorary member, free member. These positions were typically used as a stepping stone to a university chair or an academy membership. The recruitment rules for these intermediate positions varied across institutions, but generally they involved insiders, that is, the faculty or other academy members.

It is important to note that the decision to apply for an academic position was multifaceted and not a simple binary choice, as many scholars simultaneously held positions at universities and engaged in other occupations. In other words, taking an academic job did not imply abandoning other high-skilled jobs. For example, Polycarp Leyser (1586-1633), son of Polycarp Leyser (1552-1610), held a chair of theology at the university of Leipzig from 1613 (Junius Institute (2013)) but was also a canon at Wurzen, superintendent of Leipzig (since 1828), and provost in Zeitz (Allgemeinen Deutschen Biographie). Argentine Arsendi (1320-1388), son of Raniero Arsendi (1290-1358), was a professor of civil law at the University of Padova since 1351. This did not prevent him from acting as political negotiator and diplomat on behalf of the Lord of Padova, Francesco I da Carrara. According to Istituto dell'Enciclopedia Italiana (1961), he deserves to be remembered above all for the diplomatic activity in the service of the Carraresi. A position at university often implied teaching one or two courses, leaving ample time to conduct other activities at the same time. This is even more true for academies which only held meetings from time to time.

Although the virtues of meritocracy over nepotism are known since antiquity (Ciulla 2005),[9] academia became more open and meritocratic only from the 16th century onwards (Wooldridge 2021). The historical narrative suggests that this process was associated with the foundation of new institutions—such as the university of Göttingen (1737) and the various academies of sciences (see, e.g., Mokyr 2016), whereas old institutions—such as most medieval universities—remained attached to old paradigms. In Section 5.3, we provide the first systematic evidence supporting these claims.

More generally, the early prevalence of nepotism and the later increase in meritocracy in academia

[9] In Plato's Republic, children in the ideal society were raised by the state to undermine their nepotistic preferences for their relatives (Ciulla 2005).

was concomitant to broader trends in society. Early on, nepotism in society was recognized as problematic and discussed by several authors. For example, Simon Stevin (1548–1620), professor at the university of Leiden, wrote on how to change and improve the young Dutch state. Stevin "paid a lot of attention to, and expressed great concern about corruption and nepotism, two problems which current researchers have not recognized as prominent or topical in the early seventeenth-century Dutch Republic" (van Aelst 2020). Around the time when new universities and academies were established during the Scientific Revolution, other merit-based institutions began to appear, such as a civil service for the administration of India by the British Empire in the 17C (Kazin, Edwards, and Rothman 2010: p. 142).

2.2 Data

This section describes the dataset that we constructed for this paper and discusses the coverage and accuracy of the data. Appendix A lists the most important sources used and provides additional summary statistics and examples.

Father-son pairs in academia. We build a new dataset of fathers and sons in the same university or scientific academy in Europe from 1088 to 1800. To construct this dataset, we use 343 secondary sources together with encyclopedias and biographical dictionaries. First, we assemble a list of the scholars in each university and academy. To do so, we use historical catalogues of the scholars in an institution, compendia of professors, books with biographies and bibliographies of a university's scholars, and books on the history of a university or academy. Examples of these secondary sources are Mazzetti (1847)'s comprehensive list of University of Bologna professors since 1088, online catalogues of all members of the Royal Society and the Leopoldina academies, and Conrad (1960)'s list of all University of Tübingen chair holders. For universities and academies without a members' catalogue or a book on their history, we assemble a list of their scholars by combining multiple secondary sources listed in Appendix A. For example, for the University of Avignon, a sample of professors was drawn from Laval (1889) for the medical faculty, Fournier (1892) and de Teule (1887) for lawyers, and Duhamel (1895) for rectors.[10] The resulting list of scholars can be accessed at https://shiny-lidam.sipr.ucl.ac.be/scholars/. Not surprisingly, the gender distribution leans heavily towards men, with only 0.2% female scholars (De la Croix and Vitale 2023).

Second, we identify all father-son pairs in the list of scholars in each university and academy. The secondary sources described above often mention if a scholar was related to another scholar. In addition, we use biographical dictionaries, encyclopedias about universities, and encyclopedias on the regions where universities were located to identify all father-son links. Following on the examples described above, we use the Treccani encyclopedia, the Dictionary of National Biography, the *Allgemeine Deutsche Biographie*, and the biographical dictionary of the Department of Vaucluse (Barjavel 1841) to code fathers and sons in, respectively, the University of Bologna, the Royal Society, the

[10] Robert Stelter digitized chair holders at the University of Tübingen and Alice Fabre lawyers and rectors at the University of Avignon (see De la Croix et al. 2023).

Leopoldina and the University of Tübingen, and the University of Avignon.

In addition, we use these sources to record each scholar's birth, nomination, and death year and their field of study. We consider the following broad fields: lawyers, physicians, theologians, scientists, and arts and humanities' scholars. These fields correspond to the three higher faculties of early universities plus the arts faculty, where scientists gained importance over time. We also use Frijhoff (1996), British Library Board (2017) and McClellan (1985) to record the foundation date of each university and academy and its religious affiliation after the Protestant reformation. Finally, we follow the procedure described above to collect data on 507 father-sons pairs who were appointed to a different university or academy—whom we use to perform a validation exercise in Section 6.3.

Next, we discuss the limitations of our dataset and the accuracy of the father-son pairs. The main limitation is that we only observe the children of scholars who become scholars themselves. Hence, our estimates for the intergenerational elasticity of human capital are not population estimates, but reflect the transmission of upper-tail human capital in academia in 1088-1800. The biggest threat to estimate this elasticity, as well as nepotism, is if the sources used selectively report father-son links. One possibility is that links appear more frequently when fathers are famous: a father of no great account may be more likely to fall by the wayside than an underachieving son of a famous scholar. As a result, the data would be effectively selected on an outcome: father's publications. This sampling bias could explain the father-son distributional differences and attenuate intergenerational correlations in outcomes (Solon 1989), the two sets of moments used in our estimation. To assess the sensitivity of our analysis to this sampling bias, we classify the 343 sources used into three levels of completeness: First, sources with *complete coverage* cover all scholars in a university or academy, e.g., a catalogue of university professors. Under complete coverage we can fully rule out the possibility of sampling bias. Second, sources with *broad coverage* cover a large sample of scholars in an institution where a members' catalogue does not exist, e.g., a book on the history of the university. Under broad coverage, sampling bias is less likely, although we cannot fully rule it out. Third, sources with *partial coverage* describe the case where the sample of scholars in an institution was inferred by secondary sources from other institutions and/or by general thematic biographies. Under partial coverage, there is risk of sampling bias.

Table 1 shows the percentage of observations in our data under each coverage category. Around two thirds of our father-son pairs are from sources with complete coverage, 95.9% from sources with complete and broad coverage, and only 4.1% from sources with partial coverage. The data coverage does not change significantly over time. Before the Scientific Revolution (1088-1543), 62.2% of father-son pairs are from sources complete coverage. The corresponding figure for the Enlightenment (1688-1800) is 72.7%. Overall, the percentage of father-son pairs are identified from sources with complete or broad coverage ranges little across historical periods, from 93.3% in the second stage of the Scientific Revolution (1633–1687) to 97.1% in the pre-1543 period. In addition, the percentage of father-son pairs from complete and broad sources does not differ substantially by country, by century, by the religion of the university, by field of study, nor by the prestige of the university (see Appendix Figure A.1).[11]

[11] For example, we have a complete coverage for the University of Macerata – a small university in Italy, while there is

Table 1: Data coverage (in %)

	Complete	Broad	Complete and Broad	Partial	N
All	64.1	31.8	95.9	4.1	1,837
Pre-Scientific Revolution, 1088-1543	62.2	34.9	97.1	2.9	347
Scientific Revolution (I), 1543-1632	55.4	41.3	96.7	3.3	383
Scientific Revolution (II), 1633-1687	60.1	33.2	93.3	6.7	434
Enlightenment, 1688-1800	72.7	24.1	96.8	3.2	673

Altogether, this suggests that the sampling bias described above is not prevalent in our data. That said, we examine the sensitivity of our main results to this sampling bias by presenting separate estimates using data only from sources with complete coverage.

Our final dataset contains 1,621 fathers and 1,837 sons in the same university or academy. We also observe 176 families with three or more generations of scholars.[12] Sons who worked in the same institution as their fathers represent around 5% of the known faculty—although there is heterogeneity across time and institutions.[13] This percentage illustrates only the tip of the iceberg of favouritism in academia, as we only observe father-son connections but not nepotism towards other relatives (e.g., nephews, cousins) nor favouritism towards friends and acquaintances. Our dataset covers 116 universities and 63 scientific academies. These universities had, on average, 410 scholars and 11.8 academic dynasties; i.e, families in which more than one generation was employed in that university. We find the birth year for 76.6% of scholars, the death year for 86.4%, the nomination date for 91.1%, and the field of study for all.

Figure 2 shows the geographical distribution of our data. The recorded universities and academies (green circles) cover most of Europe. We observe 28 universities and 6 academies in the Holy Roman Empire, 32 universities and 24 academies in France, 7 universities and 8 academies in England, Scotland, and Ireland, and 7 universities and one academy in the Netherlands. In southern Europe, we cover 20 universities and 16 academies in Italy and 6 universities and one academy in Spain. We have several universities in eastern (e.g., Moscow, St. Petersburg) and northern Europe (e.g., Copenhagen, Lund, Turku, Uppsala). The map also displays birth places (orange for fathers, red for sons). Most scholars originate from north-west and central Europe and from Italy.

The dataset spans seven centuries from 1088—the year of the foundation of the University of Bologna—to 1800. Half of the universities in our dataset were established before 1500, e.g., the University of Paris (officially established in 1200, but starting before), Oxford (1200), Cambridge (1209), Salamanca (1218), Prague (1348). That said, most scholars under analysis are from after the 1400s. Figure 3 plots the number of father-son pairs over time. Before 1400, we observe 104 families. This number increases after 1400 and peaks during the Scientific Revolution.

no comprehensive catalogue of professors for the University of Paris.

[12] For example, the Chicoyneau and Mögling families had, respectively, four and six generations of scholars at the University of Montpellier and at Tübingen. See Figures A.2 and A.3 in Appendix A.3 for illustration.

[13] This figure is based on 20,500 scholars from De la Croix (2021b). See Section 6.2 for details.

FIGURE 2: Geographical distribution of father-son pairs

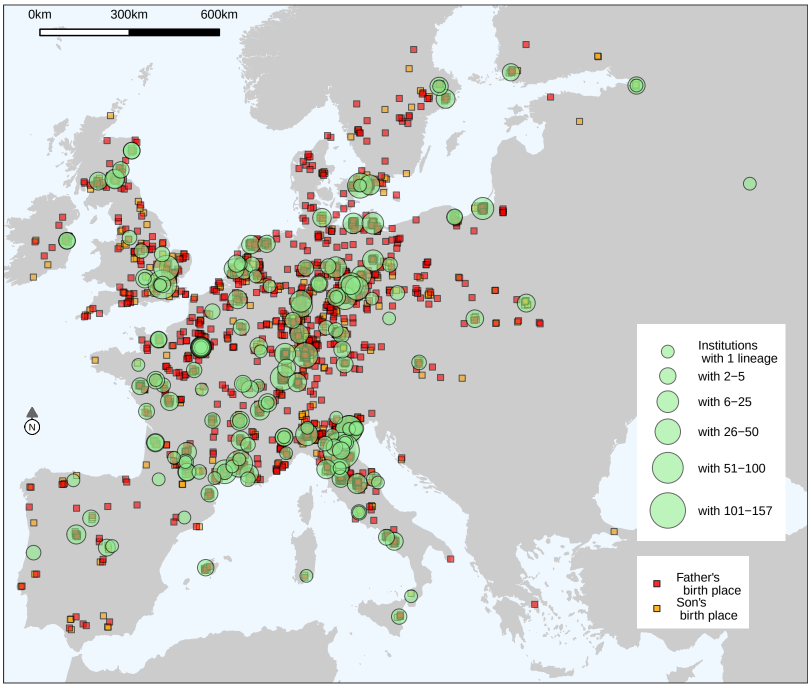
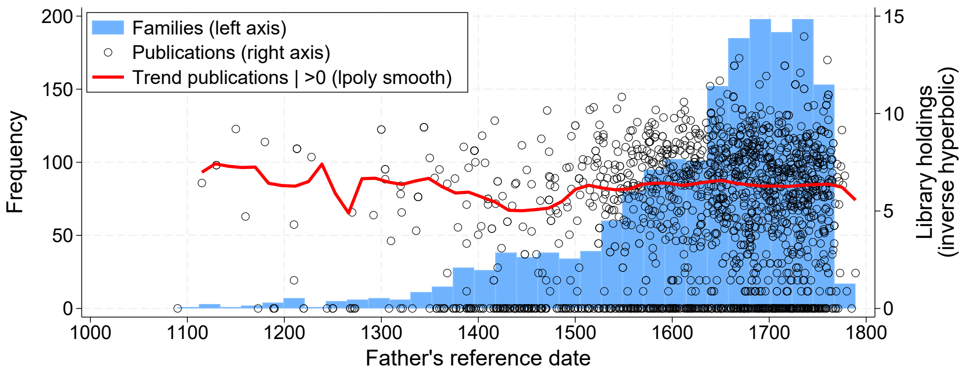


FIGURE 3: Number of scholar families and father's publications



Notes: Reference date based on birth year, nomination year, or approximative activity year.

Publications data. We measure the scientific output of each of the 1,621 fathers and 1,837 sons in our dataset. To do so, we use WorldCat—an online catalogue of the library holdings of more than 10,000 modern libraries worldwide. Specifically, we link each scholar to his entry in the WorldCat service and record his publications.

We compile three measures of a scholar's scientific output: the total number of library holdings in modern libraries written by and about each scholar, the library holdings written by each scholar, and the number of unique published works by or about each scholar. Our preferred measure is the total number of library holdings by and about each scholar. This includes all imprints/editions/copies of books, volumes, issues, or documents which he wrote that are available in WorldCat libraries today. It also includes publications about his work written by a different author.[14] Hence, our measure

[14] WorldCat identifies publications about a scholar's work by another author if the scholar's name appears in any meta-

captures both the size and the relevance of a scholar's production for today. In other words, library holdings by and abour a scholar is a measure akin to citations in modern academia, in the sense that it captures the quality of publications. In addition, while the number of unique published works may reflect nepotism or social connections in the publishing industry in the past, it is unlikely that the total number of library holdings in modern libraries today is affected by nepotism or social connections in the publishing industry centuries ago. Hence, we use library holdings by and about each scholar as our baseline measure of publications, and examine the robustness of our results to using unique publications and to excluding publications on a scholar's work written by others in Section 7. Unless we indicate otherwise, throughout the paper we define publications as the inverse hyperbolic sine of the number of library holdings. We do so to estimate elasticities on a variable with a skewed distribution and some zeros (Bellemare and Wichman 2020).[15]

We illustrate how this data was collected with an example: Honoré Bicais and his son Michel (see Appendix Figure A.4). Both are listed as University of Aix professors in Belin's *Histoire de l'Ancienne Universite de Provence* (De la Croix and Fabre 2019). Honoré's biography states that Michel succeeded him "in his chair and in his reputation."[16] To measure their publications, we link Honoré and Michel Bicais to their WorldCat entries. WorldCat considers different spellings of the family name (Bicais, Bicaise, Bicays, and the latinized Bicaisius and Bicaissius), which ensures that the matching of authors to publications is accurate. Honoré Bicais was a prolific scholar: there are 315 library holdings of books written by him. In contrast, modern libraries only hold 16 copies of the work of his son Michel. While Michel succeeded his father in his chair, it is less clear that he did so too in his academic reputation.

Our data on scholar's publications is comprehensive and accurate. Chaney (2020) conducted a validation exercise showing that WorldCat accurately approximates the population of known European authors. Specifically, he compared the Universal Short Title Catalogue (Andrews) to the references in the Virtual International Authority File (VIAF), on which WorldCat is based. Chaney successfully located 81% of USTC authors in the VIAF. In our setting, we do not find WorldCat entries for 38% of sons and for 29% of fathers. Given WorldCat's coverage, these scholars likely never published. Nevertheless, we account for the possible loss of their publications in two ways. First, our estimation uses separate empirical moments for the intensive margin (i.e., publications conditional on being listed in WorldCat) and the extensive margin (i.e., being listed in WorldCat). Second, our model accounts for separate measurement error in the intensive and extensive margins. That is, it accounts for the possible loss of a scholar's publications. This allows us to disentangle changes in the extensive margin of publications from other dynamics (e.g., changes in nepotism).

Our final dataset comprises 487,041 unique works and 4,106,901 library holdings. Figure 3 illustrates their time trends. It shows the inverse hyperbolic sine of the library holdings by and about

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data associated with the publications, such as title, dedicatee, subject, or other authors. A common example of this are compendia of a scholar's work written or edited by another author.

[15] Importantly, in our setting the arcsinh and log-distributions of the number of library holdings behave identically, and hence, we can interpret arcsinh-arcsinh specifications as elasticities. This is because the number of library holdings (in levels) of fathers and sons take on large values, with means well-above the 10 threshold proposed by Bellemare and Wichman (2020).

[16] *Les Bouches-du-Rhône, Encyclopédie Départementale*, by Masson (1931).

fathers. The figure suggests that there is no upward trend in the number of publications, conditional on being positive. Appendix G validates this finding using the De la Croix (2021b) data for all known pre-industrial scholars (not only fathers and sons). That said, we find evidence of a structural break around 1450 on the probability of being listed in WorldCat (see Appendix Figure G.2). The historical evidence suggests that this break is related to the changes brought about by the printing press, rather than with a change in the human capital distribution or in nepotism.[17] In other words, it affects fathers and sons similarly. Our model and estimation account for changes over time in the extensive margin of publications (Section 5.3), and Section 7 and Appendix G discuss the sensitivity of our results to this structural break.

3 Two facts about fathers and sons in academia

Anecdotal evidence suggests that both nepotism and inherited human capital mattered for the careers of pre-industrial scholars. For example, Jean Bauhin (1541-1613), professor in Basel, has a remarkable publication record: there are 1,471 library holdings of his work in modern libraries. Michaud's *Biographie Universelle* emphasizes how Bauhin's knowledge was inherited from his father, also a professor in Basel:

> Jean Bauhin (1541–1613) learned very early the ancient languages and humanities. His father, Jean Bauhin, was his first master in the study of medicine and of all the underlying sciences.

This contrasts with the case of the Benavente family at the University of Salamanca. Juan Alfonso Benavente has 108 publications available in WorldCat libraries today. According to the *Diccionario Biográfico Español*, he used his power and influence to pass down his chair to his son Diego Alfonso:

> After sixty years of teaching canon law in Salamanca, Juan Alfonso Benavente (–1478) retired in 1463. He retained his chair and his lectures were taught by substitutes, including his son Diego Alfonso Benavente (c. 1430–1512). Finally, on 1477, Benavente resigned his chair on the enforceable condition that his son was appointed to it.

Diego Alfonso Benavente proved less productive than his father. He only published a compendium of his father's work.

Table 2 documents two stylized facts for fathers and sons in pre-modern academia: a strong correlation in publications across generations and large differences in the marginal publications distribution of the set of fathers and sons. These two facts reflect the patterns outlined by the examples above: on the one hand, there was a strong transmission of underlying human capital endowments from fathers to sons in academia, which was later reflected in a strong correlation in their publication record. On the other hand, nepotism was present among pre-industrial scholars, generating father-son distributional differences over and above the rate of mean reversion implied by the intergenerational correlations in publications.

[17] Changes in the human capital distribution would affect trends on both the extensive and intensive margin.

Table 2: Moments used in the estimation

		value	s.e.	N
A. Intergenerational correlations				
Father-son, intensive margin	$\rho(y_t, y_{t+1} \mid_{y_t, y_{t+1}>0})$	0.375	0.03	982
Father-son with zero publications	$\Pr(y_t{=}0 \wedge y_{t+1}{=}0)$	0.211	0.01	1,837
Grandfather-grandson, intensive margin	$\rho(y_t, y_{t+2} \mid_{y_t, y_{t+2}>0})$	0.234	0.17	87
B. Father-son distributional differences				
Fathers with zero publications	$\Pr(y_t{=}0)$	0.288	0.01	1,621
Sons with zero publications	$\Pr(y_{t+1}{=}0)$	0.384	0.01	1,837
Fathers median	$Q50(y_t)$	5.075	0.14	1,621
Sons median	$Q50(y_{t+1})$	3.402	0.25	1,837
Fathers 75th percentile	$Q75(y_t)$	7.370	0.08	1,621
Sons 75th percentile	$Q75(y_{t+1})$	6.413	0.09	1,837
Fathers 95th percentile	$Q95(y_t)$	9.425	0.12	1,621
Sons 95th percentile	$Q95(y_{t+1})$	8.537	0.07	1,837
Fathers mean	$E(y_t)$	4.456	0.09	1,621
Sons mean	$E(y_{t+1})$	3.477	0.08	1,837

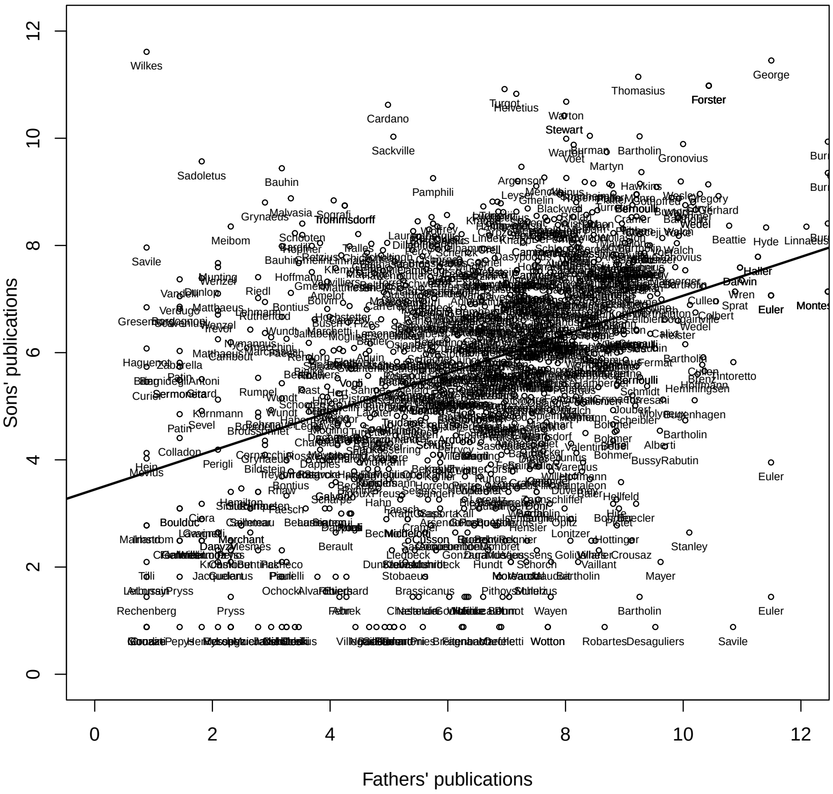
Notes: y: publications (inverse hyperbolic of library holdings by or about each scholar).

Fact 1: High correlation of publications across generations. Table 2, Panel A presents father-son correlations in publications, measured as the inverse hyperbolic sine of the number of library holdings. We distinguish correlations conditional on both father and son having at least one observed publication (intensive margin) from the proportion of pairs where father and son have zero publications (extensive margin). The correlation on the intensive margin is 0.375 (see also Figure 4). This implies that an increase of one percent in a father's publications is associated with an increase of 0.375 percent in his son's publications.[18] As for the extensive margin, in 21% of families both father and son have zero publications. In sum, publication records were persistent across two generations. This suggests that endowments determining publications, e.g., human capital, were partly transmitted from parents to children. In addition, lineages with three generations of scholars display high correlations in publications on the intensive margin. The correlation between grandfathers and grandsons is 0.23. This number is larger than predicted by the iteration of the two-generation correlation, i.e., $0.375^2 = 0.141$. In other words, the advantages of ancestors vanished at a slower rate than suggested by father-son correlations.

We perform two exercises to validate the accuracy of Fact 1. First, we compare the moments in Table 2 to moments obtained using subsamples of fathers and sons from data sources with better coverage (see Appendix Table F.1). The father-son correlation on the intensive margin is 0.36 (s.e. 0.04) when we only use sources with complete coverage, and 0.38 (s.e. 0.03) when we use sources

[18] In magnitude, the father-son correlation in academia is similar to the elasticity in wealth in pre-modern populations (Borgerhoff Mulder et al. 2009), and in education attainment in modern Sweden (Lindahl et al. 2015).

FIGURE 4: Father-son correlation in publications



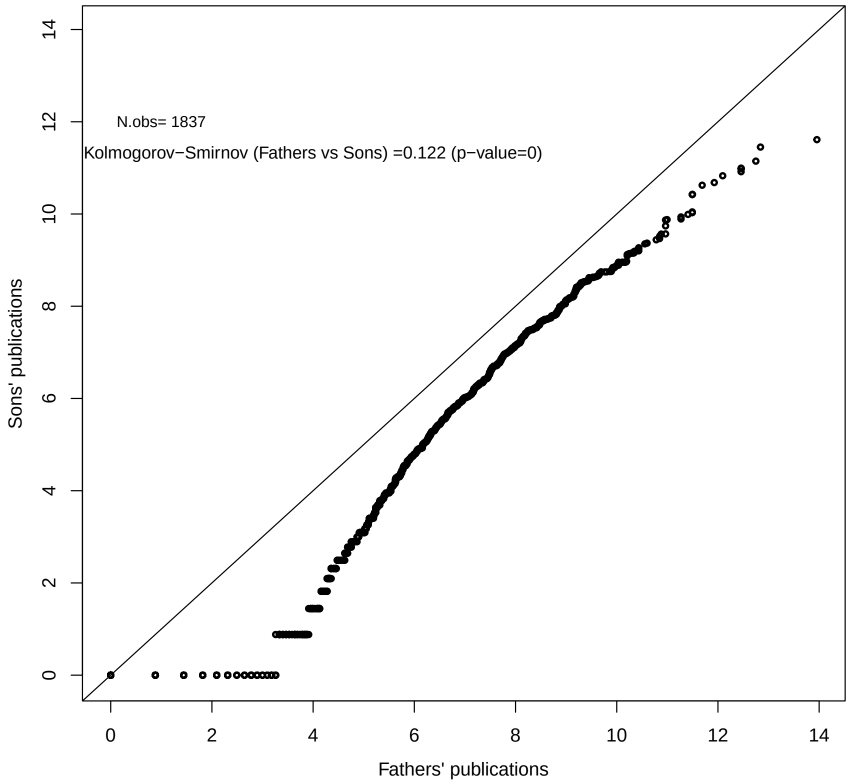
Notes: The sample are 982 father-son dyads in academia where both have at least one publication.

with complete and broad coverage. These moments are not statistically different than our baseline moment, 0.375 (s.e. 0.03). Similarly, the proportion of father-son pairs with zero publications and the grandfather-grandson correlation are not sensitive to restricting the data to sources with better coverage. Altogether, this strongly suggests that the high correlation of publications across generations (Fact 1) is accurate and not a by-product selective reporting of father-son links in sources that do not cover the universe of scholars in an institution.

Second, we show that Fact 1 is not driven by changes in the father's and son's marginal distributions which could reflect, e.g., trends in the quantity of publications over time. Note that Fact 1 is based on correlations in publications instead of on regression elasticities akin to an intergenerational elasticity (IGE). We prefer correlations because they are scale-invariant measures (Chetty et al. 2014, p.1561). Instead, regression elasticities conflate the join distribution of father and son ranks (the copula) with changes in the father's and son's marginal distributions. We confirm that our analysis is not driven by changes in the marginal distributions by showing that Fact 1 is robust to comparing father-son ranks in publications. We follow Chetty et al. (2014) and rank sons based on their publications relative to other sons in the same 50-year birth cohort. We rank fathers based on their publications relative to other fathers with sons in these 50-year birth cohorts. The father-son correlation in percentile-ranks is 0.39 in the intensive margin, almost identical to the coefficient reported in Table 2.

Fact 2: The publication's distribution of fathers first order stochastically dominates (FOSD) that of sons. Table 2, Panel B presents 10 moments describing the marginal distribution

Figure 5: Quantile-quantile plot



of publications for fathers and for sons. On the bottom of the distribution, 38% of sons had zero publications. The corresponding figure for fathers is 29%. The average father had more than two times more publications than the average son (43 vs. 16, in levels). Fathers also have two times more publications than sons in the 75th and the 95th percentile of the distribution. The difference is larger at the median: there, fathers published five times more than sons (80 vs. 15, in levels).[19]

To illustrate these differences, Figure 5 presents a QQ-plot; a plot of the quantiles of the fathers' distribution against the quantiles of the sons' distribution. If the two distributions were similar, the points would lie on the 45 degree line. Instead, in all quantiles fathers have larger publication records. That is, the father's publication distribution FOSD that of their sons. Importantly, the distributional differences are larger at the bottom of the distribution.

As before, we validate Fact 2 by comparing our baseline moments to moments obtained using data sources with better coverage that cover the universe of professors in an institution. Appendix Table F.1 shows that the fathers' publication distribution FOSD that of sons also when we only use father-son pairs from sources with complete coverage, and when we only use sources with complete and broad coverage. This shows that Fact 2 also holds in data sources that do not selectively report father-son links when, e.g., fathers are famous scholars. Hence, it is highly unlikely that the observed wedge between the publications of fathers vs. sons is driven by sampling bias in our sources.

The large distributional differences suggest that fathers had higher human capital endowments

[19] The differences in levels are $sinh(4.456) = 43.07$ vs. $sinh(3.477) = 16.17$ in the mean and $sinh(5.075) = 79.983$ vs. $sinh(3.402) = 14.995$ in the median.

than sons, which transformed into a better publication record. Partly, this difference in human capital endowments is explained by reversion to the mean. We are looking at a sample at the top of the human capital distribution, and hence, if there is reversion to the mean, sons should be worse than fathers. That said, the rate of mean reversion needed to explain away the observed distributional differences is implausibly high, especially in light of the high correlation in publications across generations (Fact 1). In other words, Facts 1 and 2 are hard to reconcile with standard mean-reversion models based solely on human capital transfers (Becker and Tomes 1979, 1986).

Instead, two pieces of evidence suggest that the bulk of these distributional differences reflect nepotism. That is, that fathers used their influence in the profession to allocate jobs to their sons ahead of outsiders, even when sons had low human capital endowments. The first is that sons of scholars had a worse publication record not only than their fathers, but also than outsiders whose parents were not academics (see Figure 1 and Section 6.2). The second is that distributional differences are larger at the bottom of the distribution. That is, close to the human-capital threshold that determines whether an individual is selected to become a scholar or not, and where nepotism could be binding. Altogether, this kind of nepotic hiring can generate father-son distributional differences, especially at the bottom of the distribution, even when human capital slowly reverts to the mean. In our estimation, we use these excess distributional differences, net of reversion to the mean, to identify nepotism.

4 Model of human capital transmission with nepotism

To account for these patterns, our model incorporates nepotism into a standard first-order Markov process of human-capital endowments' transmission. We consider a population of potential scholars who are heterogeneous in their human capital. The human capital of each potential scholar depends on a human capital endowment inherited from his father and on random ability shocks.[20] Potential scholars with high human capital are selected to be a scholar. We introduce the possibility of nepotism by allowing this selection criterion to be different for sons of scholars. For selected scholars, the unobserved human capital endowment translates into an observed outcome, publications, with noise.

Each potential scholar is indexed by $i \in \mathbb{I}$, their family, and by $\mathbf{t} = \{t, t+1, ...\}$, their generation. A potential scholar in generation t of family i is endowed with an unobserved human capital $h_{i,t}$. This is distributed according to a normal distribution with mean μ_h and standard deviation σ_h:

$$h_{i,t} \sim N(\mu_h, \sigma_h^2)\,. \tag{1}$$

The offspring of this generation, indexed $t+1$, partly inherit the unobserved human capital endowment under a first-order Markov process:

$$h_{i,t+1} = \beta h_{i,t} + u_{i,t+1}\,, \tag{2}$$

[20]In our empirical application we do not observe mothers. Under the assumption of positive assortative matching, the endowment inherited from father and mother is similar.

where β is the intergenerational human capital elasticity[21] and $u_{i,t+1}$ is an i.i.d. ability shock affecting generation $t+1$, which has a normal distribution, $N(\mu_u, \sigma_u^2)$.

At each generation, only a selected group of potential scholars with human capital above $\tau \in \mathbb{R}$ become scholars. We allow sons of scholars to become scholars if their human capital is above $\tau - \nu$. If $\nu > 0$, the selection process into becoming a scholar is subject to nepotism, in the sense that sons of scholars are selected into academia under a softer human-capital criterium than their fathers. Formally, the set $\mathbb{P}$ denotes the set of father-son pairs:

$$\mathbb{P} = \{i \mid h_{i,t} > \tau, h_{i,t+1} > \tau - \nu\} \subset \mathbb{I}. \tag{3}$$

For selected scholars, human capital is transformed into an observable outcome y with measurement error. In our case, scholars use their (unobservable) human capital to produce knowledge in the form of (observable) publications. We depart from previous literature and consider two sources of measurement error: one on the intensive margin and one on the extensive margin. On the intensive margin, we consider idiosyncrasies in the publication process, shocks to an individual's health, luck, etc. that can affect a scholar's number of publications independently of his human capital. On the extensive margin, our empirical application needs to account for the possibility that some publications might be lost or are not held in modern libraries. That is, that we are more likely to observe the publications of a scholar with a larger record of publications. Formally, the publications for fathers, $y_{i,t}$, and sons, $y_{i,t}$, in the set of observed scholar families $\mathbb{P}$ are:

$$y_{i,t} = h_{i,t} + \varepsilon_{i,t} \quad \text{if} \quad h_{i,t} + \varepsilon_{i,t} > \kappa, \quad y_{i,t} = 0 \text{ otherwise} \tag{4}$$

$$y_{i,t+1} = h_{i,t+1} + \epsilon \quad \text{if} \quad h_{i,t+1} + \epsilon_{i,t+1} > \kappa, \quad y_{i,t+1} = 0 \text{ otherwise} \tag{5}$$

where $\varepsilon_{i,t}, \epsilon_{i,t+1} \sim N(0, \sigma_e^2)$ are mean-preserving shocks affecting how human capital transforms into publications; and κ is the minimum number over which we observe a scholar's publications. The former captures measurement error on the intensive margin, the latter on the extensive margin.

We assume that the human capital of *potential scholars* in consecutive generations t and $t+1$ (that is, the human capital of fathers, $h_{i,t}$, and of sons, $h_{i,t+1}$ in $\mathbb{I}$) is drawn from the same distribution. This stationarity assumption allows us to put structure on how much of the distributional differences between *observed* fathers and sons in $\mathbb{P}$ can be explained by pure reversion to the mean—that is, independently of nepotism. Formally, $h_{i,t} \sim N(\mu_h, \sigma_h^2)$ and $h_{i,t+1} = \beta h_{i,t} + u_{i,t+1}$ imply that $h_{i,t+1} \sim N(\beta\mu_h + \mu_u, \beta^2\sigma_h^2 + \sigma_u^2)$. Imposing stationarity leads to the following parameter restrictions:

$$\mu_u = (1-\beta)\mu_h \tag{6}$$

$$\sigma_u^2 = (1-\beta^2)\sigma_h^2. \tag{7}$$

In Section 5.3, we relax this assumption. We assume that the human capital of a father and a son who are active in a given time period is drawn from the same distribution, but we allow the human capital distribution to change across periods.

[21] For simplicity, we assume that $h_{i,t}$ and $h_{i,t+1}$ are in logarithms.

We can now characterize our two main parameters of interest: the intergenerational elasticity of human capital and the magnitude of nepotism. First, the intergenerational elasticity of human capital, β, is given by Equation (2) and the stationarity conditions above, which imply:

$$h_{i,t+1} = \beta h_{i,t} + (1-\beta)\mu_h + \omega_{i,t+1}\,, \tag{8}$$

where $\omega_{i,t+1}$ is a shock distributed according to $N(0, (1-\beta^2)\sigma_h^2)$. Equation (8) suggests that a son inherits a fraction β of his father's human capital, draws a fraction $(1-\beta)$ from the population mean, and is subject to a mean-preserving shock ω. Hence, β determines the speed at which inherited human capital advantages revert to the mean. For low values of β, the rate of mean reversion will be fast, and hence, we will observe large father-son distributional differences independently of nepotism. For high values of β, the rate of mean reversion will be slow, and hence, father-son distributional differences will reflect a different human-capital selection criterium for fathers and sons into academia, that is, nepotism.

Second, we define the magnitude of nepotism, γ, as the share of sons in academia who would not have become scholars under the same selection criterium as their fathers. This share is determined by parameters ν and τ in Equation (3), but also by the distribution of human capital among all potential scholars and, as explained above, by the rate of mean reversion.[22] Formally,

$$\gamma = F_h(\tau \mid h_{i,t+1} \geq \tau - \nu)\,, \tag{9}$$

where $F_h(x)$ is the (stationary) cumulative distribution of human capital with mean μ_h and variance σ_h^2, and $F_h(x \mid h_{i,t+1} \geq \tau - \nu) = Prob\left(h_{i,t+1} \leq x \mid h_{i,t+1} \geq \tau - \nu\right)$ is the corresponding truncated cumulative distribution of sons' human capital in the set of observed scholars $\mathbb{P}$.

Note that γ is a conservative estimate of nepotism. First, according to Equations (4) and (5), the human capital endowment transmitted across generations, h, includes skills but also any other inputs that facilitate sons' publications, e.g., inherited social connections to publishers. In other words, γ is restricted to nepotic selection into academia net of any unobserved endowment that positively affects a scholar's research output—which here is captured by β. Second, γ considers sons hired in academia thanks to their parents' connections (hiring stage), but is conditional on the sons' choice of an academic career (candidate stage). Because we do not observe the universe of potential scholars, we cannot model the ex-ante problem of choosing between academia and other activities. Hence, our γ estimate abstracts from nepotism at the candidate stage. Although these two factors make nepotism a lower-bound estimate, the bias is likely small. This is because we use the number of library holdings in modern libraries instead of unique published works. Our measure captures research quality and relevance for today, and hence, is less sensitive to inherited connections to publishers omitted by γ. In addition, as explained in Section 2.1, academic jobs did not preclude scholars from taking up other high-skilled jobs. In this sense, the candidate-stage problem was less binding in our context, and hence, γ should be largely robust to changes in scholar's outside options over time or by field.

[22]Note that $\tau - \nu$ alone does not characterize the magnitude of nepotism. For example, the same $\tau - \nu$ can reflect low levels of nepotism if the mean μ_h and the variance σ_h^2 of the human capital distribution are high, and high levels of nepotism if μ_h and σ_h^2 are low (see Appendix Figure C.1 for an example).

Estimating Equations (8) and (9) is challenging for two reasons: First, human capital endowments h are often unobserved and only reflected in observed outcomes, y, with measurement error (see Equations (4) and (5)). Second, note that Equation (8) describes the mean-reversion process among *potential* scholars, while only those who actually become scholars are observed (see Equation (3)). Hence, estimates of β also need to address issues related to selection and, in this setting, of nepotism. Next, we explain how we address measurement error and selection in the form of nepotism to estimate Equations (8) and (9).

5 Identification of parameters and main results

We identify the deep parameters of our model of human capital transmission with nepotism using the two Facts described in Section 3. Specifically, we identify six parameters by minimizing the distance between 13 simulated and empirical moments in Table 2. This minimum distance procedure is used to estimate the intergenerational elasticity of human capital (β), the magnitude of nepotism (γ), the noise with which unobserved human capital is transformed into observed publications (σ_e and κ), and the shape of the human capital distribution (μ_h and σ_h). Finally, the parameters μ_u and σ_u are pinned down from the stationarity conditions (6) and (7). We assume $\tau = 0$ without loss of generality and recover ν from Equation (9).

5.1 Minimum distance estimation

The six parameters described above are identified by minimizing the distance between 13 simulated and empirical moments listed in Table 2: First, we consider the father-son correlation in publications conditional on both having at least one publication (intensive margin) and the proportion of father-son pairs with zero publications (extensive margin). Such father-son correlations in observed outcomes—especially, on the intensive margin—are widely used in the literature to estimate intergenerational elasticities and the rate of mean reversion, β (see Black and Devereux 2011). When observed, we also consider the grandfather-grandson correlation in the intensive margin. As proposed by Lindahl et al. (2015) and Braun and Stuhler (2018), we use these multi-generation correlations to address measurement error in the extent to which observed publications reflect unobserved human capital (σ_e and κ). Specifically, multi-generation correlations address measurement error under the assumption that this error is stable across generations.[23] We provide evidence supporting this assumption in Appendix Figure G.1.[24] Second, we consider ten moments describing the marginal distribution of publications for the set of fathers and sons: the mean, median, 75th and 95th percentiles, and the pro-

[23] Consider the Markov process in Equation (2) without selection. The elasticity of y between parents (t) and children ($t+1$) is $\beta\theta$, with $\theta = \sigma_h^2/(\sigma_h^2 + \sigma_\varepsilon^2)$. The elasticity of y between grandparents (t) and grandchildren ($t+2$) is $\beta^2\theta$. If measurement error is constant across generations, the ratio of these elasticities identifies β.

[24] In addition, the variance of the fathers' and sons' distributions—captured by the 75th and 95th percentiles—allows us to disentangle measurement error from nepotism: Larger measurement error increases the variance of both distributions, while larger nepotism increases the variance of the sons' distribution more. This allows us to address measurement error without resort to grandfather-grandson correlations (see Appendix C).

portion of zeros in the publications' distribution.[25] Together with the previous moments, father-son distributional differences help us to jointly identify the rate of mean reversion, β, and the magnitude of nepotism, γ. To see this, note that a slow rate of mean reversion will generate large father-son correlations in outcomes and small father-son distributional differences. In contrast, a large magnitude of nepotism will generate large father-son distributional differences at the bottom of the distribution (i.e., closer to the selection thresholds) even when intergenerational correlations suggest a slow rate of mean reversion. This is because, under nepotism, the human capital of selected sons will be low relative to that of selected fathers even under a slow rate of mean reversion. In addition, the 10 distributional moments also identify the shape of the human capital distribution (μ_h and σ_h^2). Appendix C illustrates our identification strategy with simulations.

In sum, our method recovers the intergenerational human capital elasticity from the father-son correlations in publications (the copula); and nepotism from the excess differences between the marginal distributions of fathers and sons, net of the effect of mean reversion in human capital.

Formally, we minimize the following objective function:

$$\min_{p} V(p) = \sum_{j} \lambda_j \left(\frac{\hat{m}_j(p) - m_j}{\sigma_{m_j}} \right)^2 \tag{10}$$

where j indexes the 13 moments described above, $p' = [\beta\ \gamma\ \mu_h\ \sigma_h\ \sigma_e\ \kappa]$ is the vector of model's parameters, m_j is j's empirical moment, $\hat{m}_j(p)$ is j's simulated moment, σ_{m_j} is the standard deviation of empirical moment j, and λ_j is the weight of moment j. We attach higher weights to three moments which are most useful for identification. The first two are the proportions of fathers and sons with zero publications, which capture distributional differences close to the selection thresholds. The third is the standard moment in the literature: the father-son correlation in the intensive margin. λ_j is arbitrarily large for these three moments, and $\lambda_j = 1$ otherwise.

This procedure belongs to the family of the Simulated Method of Moments (Gourieroux, Monfort, and Renault 1993; Smith 2008), a structural estimation technique used when theoretical moments cannot be computed explicitly and need to be simulated. To compute the simulated moments, we draw 50,000 hypothetical families consisting of three generations: father, son, and grandson. Each generation's human capital and publications are calculated according to Equations (1), (2), (4), and (5). Our simulated moments are computed from a sample of families in which fathers and sons meet the criteria to become scholars (Equation (3)). Our simulated grandfather-grandson correlation moments, in turn, are computed from a sample of these families in which scholar's grandsons also meet the (nepotic) criteria to become scholars, i.e., $h_{t+2} > \tau - \nu$. We minimize the objective function $V(p)$ using the Differential Evolution algorithm (Price, Storn, and Lampinen 2006) as implemented in R by Mullen et al. (2011). To compute standard errors, we draw 200 random samples from the original data with replacement, generate the 13 moments for each bootstrap sample, and estimate the model's parameters.

[25] We calculate the distributional measures on all fathers and sons based on the inverse hyperbolic sine of library holdings. We do so such that these measures are analogous to intergenerational correlations, which are based on this transformation.

5.2 Aggregate results (1088–1800)

Table 3 presents the identified parameters for the entire period, 1088 to 1800. Our main estimates are the magnitude of nepotism, γ, and the intergenerational elasticity of human capital, β. We find evidence of nepotism for one in six scholar's sons and an intergenerationa human capital elasticity of 0.63. Next, we discuss the identified parameters in detail.

Nepotism. Our γ-estimate shows that nepotism was present in pre-industrial academia. Between 1088 and 1800, 18.7 percent of scholars' sons were nepotic scholars. That is, they would not have become scholars under the same selection criteria as their fathers. The percentage of nepotic sons is precisely estimated and significantly different from zero. The magnitude of nepotism can also be illustrated by recovering the parameter ν from Equation (9). Specifically, our estimates imply that the human capital required to become a scholar is lower for sons of scholars, $\tau - \nu = -8.99$, than it was for their fathers, $\tau = 0$. Furthermore, our estimates for the mean, $\mu_h = 1.87$, and the standard deviation, $\sigma_h = 4.22$, of the human capital distribution imply that the son of a scholar could become a scholar even if his human capital was 2.6 standard deviations lower than the average potential scholar, and 2.1 standard deviations lower than the marginal outsider scholar (i.e., a scholar with human capital below but close to τ).

As explained in Section 2, the biggest threat to estimate nepotism is if our data sources selectively report father-son links. Table 3 shows that our results are robust to using fathers and sons from sources with complete coverage—where we can fully rule out sampling bias—and sources with complete and broad coverage—where sampling bias is unlikely. The percentage of nepotism, γ, is 18.7% when we use all the data (column 1), 14.8% when we use data with complete coverage only (column 2), and 18.2% when we use data with complete and broad coverage (column 3). These three estimates are not statistically different from each other, strongly suggesting that our results are not driven by sampling bias in the recording of father-son links.[26]

Finally, we perform a counterfactual exercise to gauge how nepotism may have impacted scientific production in our sample of fathers and sons in academia. We simulate our model with the estimated parameters and replace nepotic scholars by outsiders. That is, we replace sons who would not have become scholars under the same criteria as outsides by outsiders drawn randomly from the human capital distribution in academia under no nepotism. This would increase by 23.15 percent the scientific output of the average scholar in our simulated economy.

Human capital transmission. We estimate an intergenerational elasticity of human capital, β, of 0.63 among fathers and sons in academia. This implies that sons inherited 63 percent of their father's human capital. As before, Columns 2 and 3 of Table 3 show that this estimate is very similar and not statistically different when we restrict the data to sources with complete coverage (0.59) and sources with complete and broad coverage (0.64).[27]

[26] Section 2 shows that the geographical coverage is extense and not associated with the data coverage (complete, broad, or partial), suggesting that selection of universities and academies into our sample is not a source of bias.

[27] Solon (1989) shows that sampling bias tends to attenuate intergenerational elasticities. Our estimates are lowest for

TABLE 3: Identified parameters

		All [1]		Complete coverage [2]		Complete and broad coverage [3]	
IGE human capital	β	0.63	(0.04)	0.59	(0.05)	0.64	(0.04)
Nepotism, %	γ	18.7	(1.74)	14.8	(2.14)	18.2	(1.77)
Mean human capital	μ_h	1.87	(0.47)	2.84	(0.44)	1.96	(0.45)
SD human capital	σ_h	4.22	(0.20)	3.90	(0.22)	4.22	(0.21)
SD publications' shock	σ_e	0.39	(0.15)	0.25	(0.13)	0.38	(0.17)
Threshold publications	κ	2.12	(0.14)	2.15	(0.19)	2.13	(0.14)

Notes: SE in parenthesis from 200 bootstrapped samples with replacement; degrees of overidentification: 6

Our β-estimates are 11–17 percentage points larger than the parent-child elasticity in publications (0.46 with s.e. 0.02, see Table 7), a difference that is statistically significant.[28] This supports Clark (2015)'s hypothesis that underlying endowments transmitted across generations (in this case, human capital) are more persistent than suggested by parent-child elasticities in outcomes. That said, our estimate is smaller than those based on average outcomes across rare surname groups, which cluster around 0.8–0.9. It is also at the bottom range of estimates using multi-generation correlations (e.g., Braun and Stuhler 2018) and the informational content of surnames (e.g., Güell, Rodríguez Mora, and Telmer 2015). This suggests that, in empirical applications where selection and nepotism are relevant, the multiple-generation methods in the literature can provide upward-biased β-estimates. In Section 6.4, we provide evidence for this by comparing our estimates to those obtained using alternative methods in the literature.

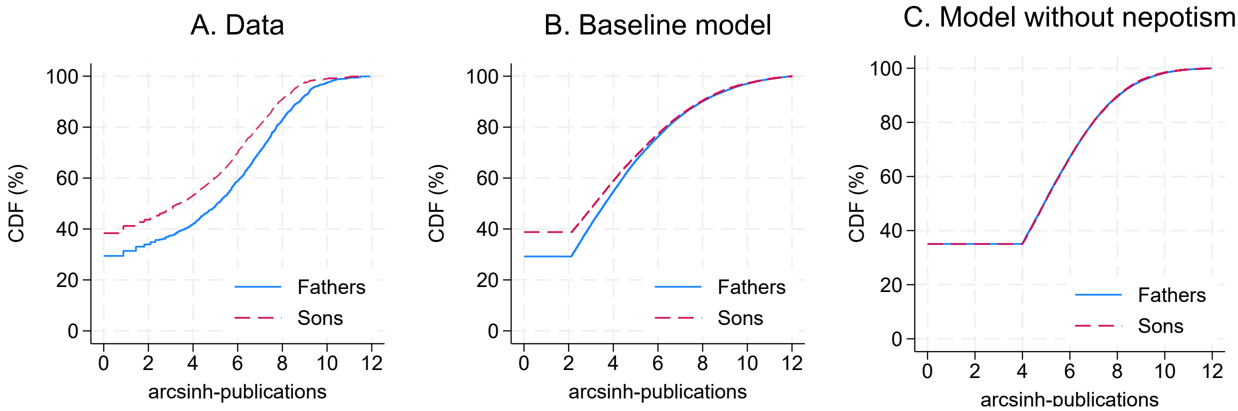
Other parameters. Table 3 shows that the human capital distribution among potential scholars has a mean of $\mu_h = 1.87$ and a standard deviation of $\sigma_h = 4.22$. This implies that the average potential scholar can become a scholar, but not those one standard deviation lower than the mean—unless their father is a scholar. Using stationarity conditions (6) and (7) we pin down the mean and standard deviation of the random ability shock to human capital: $\mu_u = 0.69$ and $\sigma_u = 3.28$. We also find an imperfect relation between human capital and the production of ideas: The shock affecting how scholar's human capital transforms into publications, ϵ, has a standard deviation of $\sigma_e = 0.39$. We also estimate a high $\kappa = 2.12$, implying that a scholar who published 2 or 3 works may have no library holdings today. In other words, we do not impose a zero human capital (or nepotism) to scholars with no library holdings, but allows the possibility that his publications may be lost and are not held in modern libraries.

Model fit. Here we compare the empirical moments to those simulated by our model; the details

data with complete coverage, suggesting that our sources do not selectively report father-son links.

[28]The difference is smallest when the sample is restricted to sources with complete coverage ($\beta = 0.59$ vs. 0.48 parent-child elasticity). This is because the wedge between these two measures is partly explained by measurement error, which is smallest under complete coverage sources (see Appendix Equation (16)).

Figure 6: Empirical and simulated distribution of publications



are available in Appendix D. We reproduce Fact 1, that is, the high correlation of publications between fathers and sons in the intensive (0.375 in the model vs. 0.375 in the data) and extensive margin (0.17 vs. 0.21). Our model also matches the grandfather-grandson correlation (0.19 vs. 0.23), as well as the empirical observation that this correlation is larger than predicted by iterating the two-generation correlation ($0.375^2 = 0.14$).

We also reproduce Fact 2. This is illustrated in Figure 6, which shows the empirical (Panel A) and simulated (Panel B) cumulative distribution function (CDF) of publications by the set of fathers and sons.[29] We reproduce the observed distributional differences at the bottom of the distribution but also below the median. Our parsimonious model does not generate large differences at the very top, suggesting that these emerge independently of nepotism or human capital transfers. Importantly, Panel C shows that nepotism is crucial to reproduce the observed distributional differences. We consider an alternative model with $\gamma = 0$, that is, where scholars' sons and outsiders are selected into academia under the same criteria. In this alternative model, only mean reversion can generate distributional differences—since scholars are at the top of the human capital distribution, mean reversion will worsen the sons' publications relative to that of their fathers. This effect should be larger for top vs. average scholars' sons. The model without nepotism reproduces some small distributional differences in the 75th (6.58 for fathers vs. 6.57 for sons) and 95th percentile (8.98 vs. 8.89). That said, it fails to match Fact 2, as the CDFs are largely identical. In other words, the observed distributional differences are hard to reconcile with a model of *pure* mean reversion *à la* Becker and Tomes 1979, 1986, where persistence is explained with human capital transfers but not with inherited social connections and nepotism. Interestingly, the alternative model estimates a larger β of 0.72, suggesting that ignoring the selection bias arising from nepotism can overstate intergenerational elasticities.

5.3 Results over time

This section studies the evolution of nepotism and the intergenerational elasticity of human capital between 1088 and 1800. Were periods of rapid scientific advancement associated with a decline in

[29] Note that we show our targeted moments (median, Q75, and Q95) but also percentile moments that are not used in the estimation. Hence, the figure is in the spirit of an overidentifying restrictions test.

nepotism, and hence, a better allocation of talent in academia? How important was the knowledge transmitted from parents to children during these periods? To answer these questions, we narrow our focus to the two proclaimed roots of all modern technological advances: the Scientific Revolution (Wootton 2015) and the Enlightenment (Mokyr 2010).

We divide our families of scholars into four periods based on the father's reference date. We use standard dates marking the Scientific Revolution and the Enlightenment: **(i)** before 1543, when Copernicus published *De revolutionibus orbium coelestium*; **(ii)** 1543–1632, the beginning of the Scientific Revolution, which focused on recovering the ancients' knowledge; **(iii)** 1632–1687, the Scientific Revolution, from Galileo's *Dialogue* to Newton's 1687 *Principia*; and **(iv)** 1687–1800, the age of Enlightenment.

Appendix Figures E.1 and E.2 show QQ-plots for the marginal publications distribution in each historical period. The distribution for the set of fathers always first-order stochastically dominates that of scholars' sons. That said, differences become smaller in the Scientific Revolution and in the Enlightenment. This suggests that, in later periods, the underlying human capital endowments determining publications were similar for fathers and sons selected into academia.

Our results show that these patterns emerged due to a decline in nepotism. Table 4 presents estimates of our model for each period separately.[30] Before 1543, forty-eight percent of sons of scholars would not have become scholars under the same selection criteria as their fathers. This is reduced to 20.35 percent during the Scientific Revolution and to 8.3 percent during the Enlightenment. These percentages are precisely estimated and significantly different from each other: a Clogg, Petkova, and Haritou (1995)'s z-test rejects the null hypothesis of no difference in nepotism between the period before 1543, the Scientific Revolution, and the Enlightenment. In other words, in periods of rapid advancement, sons of scholars were selected more meritocratically. The dramatic differences in nepotism across time likely had large effects on the production of knowledge over time. To gauge this, we perform a counterfactual exercise where we replace all the nepotic scholars with average potential scholars. We find that this would increase the output of the average scholar in our sample by 69% before 1543, but only by 9% in the Enlightenment.

Next, we turn to examine one of the mechanisms behind the decline in nepotism around the Scientific Revolution. The decline of nepotism could be the result of two processes: That *existing* universities and academies undertook structural reforms to eliminate nepotism from their hiring decisions; and/or that *new* institutions were established under more modern, meritocratic principles. The evidence supports the latter. In Table 4, we compare families of scholars in institutions established before vs. after 1543, the start of the Scientific Revolution. We only consider families who were active after 1543 such that both groups are comparable. We find that the percentage of sons hired under nepotism, γ, was substantially smaller in new institutions than in existing institutions which had

[30] This relaxes the stationarity assumption in Equations (6) and (7). We now assume that the human capital of fathers and sons is drawn from the same distribution within a period, but we allow the distribution to change across periods. In addition, these estimates account for the possibility that the selection process to enter academia changed over time. This is because we allow all our model parameters to change across historical periods, including the parameters determining the "nepotic" entry threshold $\tau - \nu$.

TABLE 4: Results over time

	β	γ	μ_h	σ_h	σ_e	κ	N
Pre-Scientific Revolution, 1088-1543	0.18 (0.14)	48.82 (10.42)	-0.46 (0.93)	3.26 (0.82)	3.60 (1.21)	2.39 (0.53)	347
Scientific Revolution (I), 1543-1632	0.62 (0.08)	20.35 (3.98)	1.63 (1.05)	4.28 (0.43)	0.21 (0.19)	2.01 (0.28)	385
Scientific Revolution (II), 1633-1687	0.59 (0.08)	17.96 (2.74)	2.30 (0.68)	4.22 (0.30)	0.22 (0.14)	1.68 (0.22)	429
Enlightenment, 1688-1800	0.67 (0.06)	8.29 (3.51)	3.77 (0.73)	3.47 (0.47)	0.38 (0.21)	2.34 (0.47)	673
Institution established pre-1543	0.63 (0.06)	19.48 (3.20)	1.61 (0.79)	4.13 (0.33)	0.49 (0.19)	2.22 (0.21)	730
Institution established post-1543	0.61 (0.05)	8.80 (2.16)	3.93 (0.42)	3.59 (0.26)	0.38 (0.10)	1.88 (0.26)	760

Notes: SE in parenthesis obtained from 200 bootstrapped samples with replacement.

been funded before the Scientific Revolution. Specifically, the percentage of nepotic sons are 19.5 for existing and 8.8 for new institutions, a difference that is statistically significant. This result is consistent with the historical narrative in Section 2, which suggests that the establishment of new academic institutions with more modern, meritocratic values was a key mechanism behind the modernization of academia in general, and behind the reduction of nepotism in particular.

Altogether, these estimates show that nepotism declined dramatically between 1088 and 1800. If seen as a witness of a broader downturn in favouritism towards relatives, friends, and acquaintances, the decline in nepotism is complementary with the accumulation of knowledge during the Scientific Revolution and the Enlightenment. In the next section, we provide more evidence in support of a relationship between changes in the knowledge frontier and the cost of nepotism.

Finally, we examine whether the father-son transmission of human capital changed over time. We find that, during the Scientific Revolution (1543-1632) and the Enlightenment (1715-1789), scholars inherited human capital endowments from their parents at a higher rate than pre-1543 scholars. Our β-estimate ranges from 0.18 before 1543 to 0.67 in 1688–1800, a difference that is statistically significant. This shows that, for individuals at the upper-tail of the human capital distribution, the intergenerational transmission of human capital is subject to changes in the environment and is not a universal constant as suggested by Clark (2015). Why would β increase over time? On the one hand, β captures the inheritability of skills, preferences, or genes, which is unlikely to vary much over time. On the other hand, β also captures the transmission of other endowments which boost the sons' research output—such as scientific knowledge, academia-specific human capital acquired at home, know-how on how to publish, on editors etc. These are endowments that can be transmitted at different rates in different periods. Although we cannot distinguish empirically between these two elements, the fact that our β-estimate increases over time suggests that the importance of academia-specific knowledge increased after the Scientific Revolution. That said, our β-estimate is relatively stable for a period of

450 years, from the start of the Scientific Revolution to 1800.

Interestingly, our estimates show an inverse relationship between nepotism, γ, and human capital transmission, β. In early academia, scholars used their influence to appoint their sons, even when these had low human capital. With the Scientific Revolution and the Enlightenment, nepotism faded but father-son pairs did not disappear. The reason is that sons of scholars inherited large human capital endowments from their parents, giving them a natural advantage over outsiders. In other words, lineages of scholars became more meritocratic. This suggests that the establishment of open universities and the emergence of meritocratic lineages in pre-industrial Europe was a stepping stone to the production of new ideas and the accumulation of upper-tail human capital.

6 Heterogeneity and validation

6.1 Heterogeneity

Here we explore heterogeneous effects with respect to universities' religion, fields of study, changes in the knowledge frontier, whether sons were appointed during their father's lifetime, and different types of academic institutions.[31]

Protestant vs. catholic institutions. The Protestant Reformation is often associated to the rise of modern science. Merton (1938) argues that Protestant values encouraged the Scientific Revolution because science was seen as proof of God's influence on the world. Others argue that, in Catholic countries, the Scientific Revolution was hindered by the Counter-Reformation (Lenski 1963; Landes 1998; Blasutto and De la Croix 2023).[32] We contribute to this debate by showing that differences in the scientific output of Protestant and Catholic universities are associated to differences in nepotism and human capital transfers within the family.

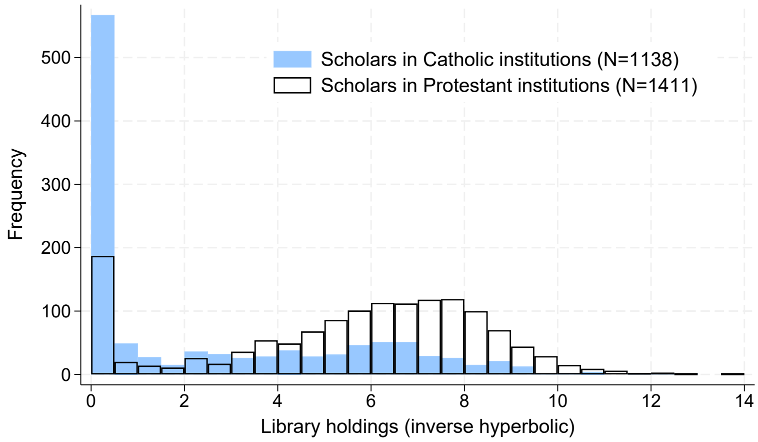
We begin by showing that scholars in our dataset were more productive in Protestant than in Catholic institutions. To do so, we classify scholars according to the religious affiliation of their university or academy. We exclude all father-son pairs before 1527—when the first Protestant university, Marburg, was created. Figure 7 shows that the percentage of scholars with zero publications was 13.3% in Protestant institutions and 49.8% in Catholic institutions. Conditional on having at least one publication, the average scholar had more than foure times more publications in a Protestant institution than in a Catholic institution (298 vs 66 in levels). At the upper-tail of scientific production, there is a larger frequency of scholars with more than 1,000 library holdings (ca. 7.6 arcsinh-publications) in Protestant institutions.

The larger scientific output in Protestant institutions is associated with a smaller prevalence of nepotism. Table 5, Panel A shows the estimated model's parameters for Protestant and Catholic in-

[31] All QQ plots for these differences are shown in Appendix Figures E.3 to E.7.

[32] Lenski (p. 176) argued that "[i]n the centuries before the Reformation, southern Europe was a centre of learning and intellectual inquiry [...] The Protestant Reformation ... gave a big boost to literacy, spawned dissents and heresies, and promoted the scepticism and refusal of authority that is at the heart of the scientific endeavour. The Catholic countries, instead of meeting the challenge, responded by closure and censure."

FIGURE 7: Publications, by institution's religious affiliation



Notes: The sample are 2,549 scholars nominated after 1527 who belong to a scholar's lineage.

stitutions separately. In Catholic institutions, 29.4% scholar's sons were a by-product of nepotism; they would not have been selected into academia under the same criterium as their fathers. In contrast, in Protestant universities we only identify 6.6% of scholars' sons as nepotic. These percentages are precisely estimated, and the z-test of Clogg, Petkova, and Haritou (1995) rejects the null hypothesis that our nepotism measure, γ, is equal in Catholic and Protestant institutions (see row 3 of Panel A). We also find that β was 28 ppts larger in Catholic institutions, a difference that is statistically different from zero. In other words, Catholic institutions were more nepotic and relied more on the transmission of knowledge from fathers to sons in academia.

We find that differences in nepotism account for 16% of the Protestant-Catholic gap in publications in our data. Specifically, we perform a counterfactual exercise where we replace nepotic scholars with average potential scholars. This increases the publications of the average scholar by 41% in Catholic and by 7% in Protestant institutions. While the observed Protestant-Catholic gap in the son's mean arcsinh-publications is 3.1, in this counterfactual scenario with no nepotism the gap is 2.6, which corresponds to a 16.1% reduction.

Note that many theology scholars were priests or pastors who could only be succeeded by their sons in Protestant institutions. In addition, nepotism was low in theology because appointments often required the approval of external Church authorities. We rule out that Protestant institutions appear more meritocratic because of this composition effect. To do so, we exclude theology scholars from the analysis (see row 4 of Panel A). The estimated percentage of nepotic sons, γ, is stable in Protestant institutions (5.0 vs. 6.6), and remains statistically different from the nepotism estimate in Catholic institutions (see row 5 of Panel A).

Altogether, these results suggest that nepotism and inherited human capital were relevant factors behind the decline of Catholic universities after the Protestant Reformation.

Field of study. Next, we examine heterogeneity across fields of study. This is motivated because different types of upper-tail human capital can have different economic implications. For example, Murphy, Shleifer, and Vishny (1991) and Maloney and Valencia Caicedo (2022) emphasize the im-

TABLE 5: Heterogeneity

	Intergen. HC elasticity	Nepotism, %	Other model parameters:				
	β	γ	μ_b	σ_b	σ_e	κ	N
A. Religion of university, post-1527							
Catholic	0.77 (0.04)	29.4 (3.6)	-1.7 (0.8)	4.5 (0.3)	1.0 (0.4)	2.1 (0.2)	660
Protestant	0.49 (0.05)	6.6 (1.5)	4.6 (0.3)	3.4 (0.2)	0.3 (0.1)	1.5 (0.2)	838
Difference p-value	[0.00]	[0.00]					
Protestant without theology	0.47 (0.06)	5.0 (2.0)	4.9 (0.4)	3.2 (0.3)	0.2 (0.1)	1.7 (0.5)	607
Difference p-value	[0.00]	[0.00]					
B. Field of study (of father)							
Lawyer	0.71 (0.07)	33.8 (3.5)	-2.0 (0.7)	4.8 (0.5)	0.4 (0.3)	2.5 (0.4)	451
Physician	0.62 (0.07)	19.6 (3.4)	1.8 (0.7)	4.1 (0.3)	0.4 (0.2)	2.1 (0.2)	523
Theologian	0.55 (0.08)	12.0 (3.0)	3.6 (0.6)	3.9 (0.3)	0.4 (0.1)	1.1 (0.3)	259
Scientist	0.65 (0.07)	10.7 (3.3)	3.6 (0.7)	3.9 (0.4)	0.3 (0.1)	1.4 (0.4)	285
Father-son in same field	0.68 (0.05)	21.2 (2.2)	1.1 (0.7)	4.4 (0.3)	0.3 (0.2)	2.1 (0.1)	1,341
Father-son in different field	0.56 (0.07)	14.8 (3.2)	3.0 (0.6)	4.0 (0.3)	0.2 (0.1)	1.9 (0.3)	496
Difference p-value	[0.16]	[0.10]					
C. Growth rate in publications							
Rapidly growing knowledge frontier	0.64 (0.04)	9.2 (2.3)	3.7 (0.5)	3.5 (0.3)	0.3 (0.1)	2.1 (0.3)	1,048
Stagnant knowledge frontier	0.78 (0.06)	25.3 (4.1)	-1.1 (1.3)	5.0 (0.5)	0.7 (0.2)	1.8 (0.3)	290
Difference p-value	[0.05]	[0.00]					
D. Appointment date of son							
After father's death	0.52 (0.06)	15.0 (2.6)	2.9 (0.5)	3.8 (0.2)	0.4 (0.1)	1.8 (0.2)	731
Before father's death	0.72 (0.05)	21.4 (2.0)	0.8 (0.9)	4.9 (0.3)	0.4 (0.2)	1.6 (0.1)	777
Difference p-value	[0.01]	[0.05]					
E. Universities vs. Academies							
Universities	0.68 (0.04)	16.5 (2.1)	2.1 (0.6)	4.2 (0.2)	0.4 (0.1)	1.9 (0.2)	1,032
Academies	0.59 (0.07)	10.1 (3.5)	3.7 (0.6)	3.7 (0.4)	0.5 (0.2)	2.2 (0.4)	458
Difference p-value	[0.26]	[0.12]					

Notes: Standard errors in parenthesis from 200 bootstrapped samples with replacement; P-values in brackets from Clogg, Petkova, and Haritou (1995)'s z-test on null hypothesis of no differences in β or nepotism.

portance of engineers for modern development. In medieval Europe, university training in Roman law helped to establish markets during the Commercial Revolution (Cantoni and Yuchtman 2014). During the Scientific Revolution, research and teaching in science gained importance relative to philosophy, music, and history.[33]

Table 5, Panel B presents separate estimates for four fields of study: science (arts), law (canon and Roman law), medicine (including pharmacy and surgery), and theology.[34] Father-son pairs are sorted into fields according to the father's field. Our estimates show that nepotism was most prevalent in law faculties and among physicians: 33.8% of law scholars' sons and 19.6% of physicians' sons became scholars thanks to nepotism. This is in line with Lentz and Laband (1989), Mocetti (2016), and Raitano and Vona (2018), who find high levels of nepotism for modern lawyers, pharmacists, and doctors. Differently, only 10.7% of scientists' sons were nepotic scholars, suggesting that applied sciences were more open to newcomers. Although splitting the data by field of study reduces sample sizes, our nepotism estimates are precise, and the differences between fields are statistically significant. Finally, note that nepotism was low in theology. This reflects the fact that such appointments often required approval by Church authorities, and hence, universities had less discretion in filling this positions.

Next, we compare sons who followed their father's footsteps in the same field with sons who published or taught in a different field than their fathers.[35] This is interesting in two respects: First, one would expect sons in the same field to be less meritocratic—a son's inherited social connections may be more important for obtaining a job in the same faculty as his father. Second, this exercise allows us to separate the transmission of general human capital from the transmission of human capital specific to the father's field of study.

Table 5 presents the results. Families with fathers and sons in the same field were less meritocratic: 21.2% of sons in their father's field became scholars because of nepotism; higher than the 14.8% of nepotism for fathers and sons in different fields. We also find a stronger transmission of human capital from fathers to sons in the same field, although β is high for fathers and sons in different fields. This highlights the importance of general upper-tail human capital in our setting. Finally, these findings add credence to our identification strategy. It shows that the negative relation between nepotism, γ, and inherited human capital, β, over time is not an artificial by-product of our model or our estimation strategy. Where we expect both high transmission of human capital and high nepotism—such as among fathers and sons in the same field—our estimates for γ and β are positively related.

Changing vs. stable knowledge frontier. Our results suggest that nepotism is more prevalent in stagnant environments (e.g., in catholic universities after 1527) than in dynamic societies or sectors experimenting structural changes (e.g., in scientific fields after the Scientific Revolution). This is consistent with the idea that under a rapid change in the knowledge frontier, technological progress, or cultural change, the cost from a mismatch between talents and occupation (i.e., nepotism) exceeds

[33] Some faculties of arts, however, missed on fields such as cartography and astronomy. This led scientists like Copernicus, Kepler, or Galileo to quit their universities (Pedersen 1996).

[34] We omit other fields belonging to the faculty of arts, e.g., Hebrew, Philosophy, and Rhetoric.

[35] For fathers and sons in multiple fields, we consider them in the same field if at least one field coincided.

the benefits from the transmission of specific human capital from parents to children (i.e., a high β). The reverse holds true under a stable environment where the knowledge and social connections of one generation are still useful for the next. This idea has been examined in the context of the transmission of human capital and technological progress (Galor and Tsiddon 1997), managerial capital in family firms (Carillo, Lombardo, and Zazzaro 2019), and cultural persistence (Giuliano and Nunn 2020).

Here we test this hypothesis in the context of pre-modern academia. We do so by estimating nepotism and the elasticity of human capital separately for families where the son entered academia at a time, society, and field of study that was rapidly changing vs. stagnant.

Specifically, we proceed in three steps. First, we use data from De la Croix (2021b) on 40,800,000 publications of all known scholars active between 1500–1800 to calculate, for each year, the growth rate of publications over the previous 25 years by six fields of study: law, medicine, theology, humanities, science, and applied science. We further distinguish between field-specific growth rates in catholic and protestant institutions after 1527.[36] We use an HP filter to smooth out short-run fluctuations and to preserve observations at the beginning and end of our time series. Appendix Figure E.8 displays the different field-institution growth rates in publications over time. In general, theology, law, and humanities experienced eras of stagnation starting shortly before the 1600s, while applied sciences always display positive growth rates. All fields of study display a higher growth in protestant than catholic institutions, although theology and law become stagnant in protestant institutions after, respectively, the 1700s and the 1650s. Second, we classify families of scholars into two groups: those who worked at a time, society (catholic or protestant), and field of study experiencing rapid changes in the knowledge frontier vs. experiencing stagnation. In detail, we classify families into the first group if the field-institution growth rate in publications was positive at the time the son entered academia; and into the second group if the field-institution growth rate in publications was zero or negative at the time the son entered academia. Third, we estimate all our model's parameters separately for these two groups.

Table 5, Panel C presents the results. Nepotism is less prevalent where the knowledge frontier was rapidly changing than where it was stable or stagnant. We find a nepotism estimate of 25.3% among scholars who were active at a time, society, and field with a stagnant production of knowledge. In contrast, only 9.2% of scholars were nepotic among those active in eras when their field was experiencing rapid changes in the knowledge frontier. These percentages are precisely estimated, and we reject the null hypothesis that nepotism was equal across these two groups (see row 3 of Panel C). We also find that the transmission of specific human capital from parents to children, β, was 14 percentage points larger in eras of stagnation than in eras of rapid change, a difference that is statistically significant. These results are robust to alternative groupings of families: Appendix Table E.1 shows similar results from a classification based on whether the field-institution growth rate in publications was above or below the median at the time the son entered academia (instead of above or below zero). Results are also robust to grouping families into rapidly changing vs. stagnant fields based on the date at which the father entered academia.

[36] As above, we exclude years before 1527 because the catholic-protestant distinction is not relevant before that date.

Overall, these results confirm the hypothesis that the observed decline in academic nepotism after the Scientific Revolution and in particular fields and institutions is complementary to a rapidly changing knowledge frontier. This raised the cost from a mismatch between talents and occupation, exceeding the benefits from the transmission of specific human capital from parents to children.

Nomination before vs. after father's death. A father may use his social connections to nominate his son to a chair, or secure a university chair as part of his family's assets and pass it down to his son upon his death. We distinguish these two expressions of nepotism by estimating our model for father-son pairs in which the son was nominated before vs. after his father's death. Table 5, Panel D shows that 21.4% of sons nominated during their father's lifetime were nepotic scholars. Alternatively, we find nepotism in 15% of sons nominated after their father's death. The z-test of Clogg, Petkova, and Haritou (1995) can reject the null hypothesis of no difference with a p-value of 0.05. This suggests that, in our setting, nepotism is characterized mostly by fathers using their social connections to nominate their sons, but also by fathers passing down their chairs as part of the inheritance.

Universities vs. academies. Academies were often seen as superior institutions than universities. Many outstanding scholars joined the academies created during the Scientific Revolution, e.g., Royal Society of London (1662), Académie des Sciences (1666), and the Leopoldina (1677). These academies formalized the Republic of Letters and were an engine of cultural change (Mokyr 2016). Table 5, Panel E compares families of scholars in universities vs. academies after 1543, the start of the Scientific Revolution. Our findings do not support the negative views about universities: both the father-son transmission of human capital (β) and the percentage of nepotism (γ) are not statistically different in universities vs. academies. This suggests that nepotism declined after the Scientific Revolution in academies, but also in newly established universities.

6.2 Validation using outsider scholars

So far, our analysis has focused on comparing the publications of scholars' sons and their fathers. Here we show that our results are consistent with comparisons between scholars' sons and the universe of outsiders in academia. That is, scholars who did not belong to a family dynasty. Figure 1 already showed some preliminary evidence that our main findings are consistent with rough comparisons of the research productivity of the average scholar's son and the average outsider over time. Here we further validate our main findings by extending our estimation strategy to incorporate outsiders and showing that our results are unchanged. Specifically, we now use data on outsiders and quantify the nepotistic behavior of fathers in favor of their children by comparing the selection criteria (i.e., entry barriers) faced by sons of scholars with those applied to outsiders in the same generation.

In order to conduct this exercise, we need to extend our estimation strategy in three dimensions. First, we extend our theoretical model to incorporate outsiders. As before, our model economy consists of a population of potential scholars whose unobserved human capital is transmitted from fathers to sons with an elasticity of β (Equations (1) and (1)), and transformed into publications with measurement error noise (Equations (4) and (5)). Potential scholars with human capital endowments above τ become scholars. We now allow the selection criterium to be different for sons of scholars, not only relative to their fathers (Equation (3)) but also relative to outsiders in the same cohort who do not have family connections in academia. Formally, we extend Equation (3) as follows:

$$\mathbb{P} = \left\{ i \mid h_{i,t} > \tau, h_{i,t+1} > \tau - \hat{\nu} \right\} \tag{11}$$

$$\mathbb{O} = \left\{ i \notin \mathbb{P} \mid h_{i,t+1} > \tau \right\}, \tag{12}$$

where $\mathbb{P}$ denotes the set of father-son pairs in academia and $\mathbb{O}$ the set of outsiders. Specifically, sons of scholars are individuals in generation $t+1$ who fulfill the (nepotic) criterium to become scholars, $h_{i,t+1} > \tau - \hat{\nu}$, and whose fathers are in academia, $h_{i,t} > \tau$. Outsiders are individuals in generation $t+1$ who fulfill the criterium to be scholars, $h_{i,t+1} > \tau$, and who do not have family connections in academia in the previous generation, $i \notin \mathbb{P}$.

This changes the interpretation of $\hat{\nu}$, which now measures the distance in human capital between the marginal scholars' son and the marginal outsider *in their same generation*, i.e., $\hat{\nu} = \tilde{h}_{i \in \mathbb{O},\, t+1} - \tilde{h}_{i \in \mathbb{P},\, t+1}$. That is, $\hat{\nu}$ captures how much scholars' sons are favored relative to all their potential competitors in the same cohorts who do not have any family connection.[37] Similarly, we define the magnitude of nepotism, $\hat{\gamma}$, as the share of sons in academia who would not have become scholars under the same selection criterium as outsiders in the same cohorts. We recover γ from Equation (9), which now is based on this modified interpretation of the entry barriers to academia for sons of scholars, $\hat{\nu}$.[38]

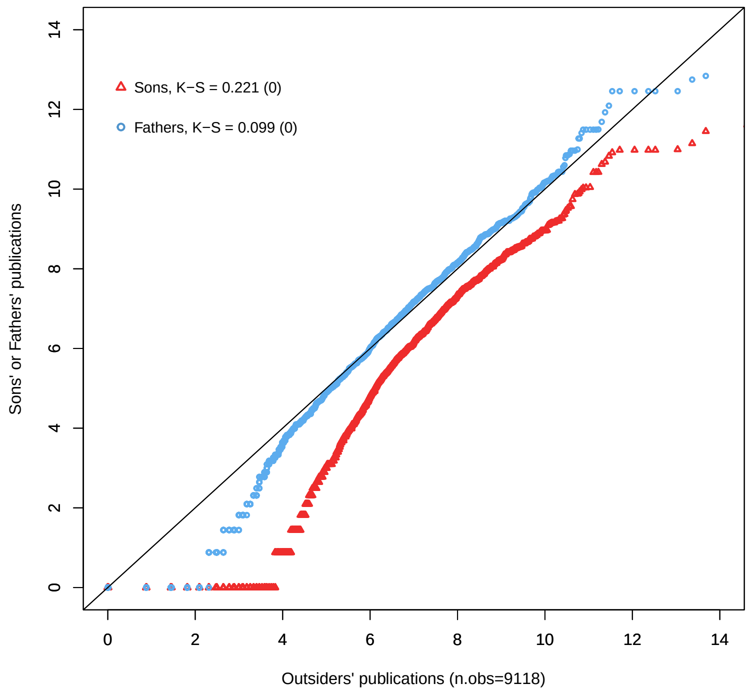
The second extension to our estimation strategy is to use data not only on families of scholars but also on the universe of all known scholars, including outsiders. We select the relevant set of outsiders by applying two sample restrictions: On the one hand, we consider only outsiders who started working in the same decade and institution as at least one scholar's son. This restriction is done such that we effectively recover nepotism by comparing sons of scholars and their potential competitors in the same cohorts. On the other hand, scholars belonging to a dynasty tend to be better documented than outsiders. For example, conditional on the number of publications, cohort, field, and institution fixed effects, a scholar's son is more likely to have a Wikipedia page than an outsider (see Appendix Table E.2). To increase the comparability between the two groups and make sure that our results are not driven by this difference, we take a conservative approach and consider only outsiders and families of scholars who are listed in Worldcat or Wikipedia.

The third extension to our estimation strategy concerns the targeted moments. In addition to our two baseline sets of moments — intergenerational correlations and father-son distributional differences — we now also target differences in the publications' distribution of scholar's sons, $f(y_{i \in \mathbb{P},\, t+1})$, and outsiders, $f(y_{i \in \mathbb{O},\, t+1})$, in the same generation $t+1$. In detail, we target our baseline 13 moments and 5 additional moments: the share of outsiders with zero publications, the mean, median, 75th and 95th percentile of the outsiders' publications distribution. These moments are illustrated in Figure 8 (see Appendix Table E.3 for all moments). It shows a QQ plot of the quantiles of the publications' distribution of outsiders (x-axis) against the quantiles of scholar's sons in the same cohorts

[37] Before, ν captured how much sons are favored relative to first-generation scholars in the set of families.

[38] This extended model also assumes the stationarity conditions (6) and (7). Hence, β can still be characterized by in terms of mean reversion in the human capital distribution of potential scholars (Equation (8)).

Figure 8: Quantile-quantile plot of outsiders, fathers, and sons in academia



Notes: The sample are 1,482 families of scholars and 9,118 outsiders listed in Worldcat or Wikipedia. Outsiders are restricted to those entering academia in the same decade and institution as a scholar's son. Publications are the inverse hyperbolic sine of library holdings by or about each author.

and institutions, as well as their fathers (y-axis). The publications' distribution of outsiders and fathers is similar, as most quantiles lie on the 45 degree line. In other words, outsiders and fathers (i.e., first-generation scholars) had similar research productivity, which suggests that they were subject to similar selection criteria into academia. In contrast, in all quantiles, fathers and outsiders have larger publication records than scholar's sons. That is, the outsider's publication distribution FOSD that of scholars' sons competing in the same cohorts and institutions (Fact 3). This provides some preliminary evidence that incorporating outsiders into the analysis will not substantially alter the nepotism estimates obtained from comparisons between the set of sons and fathers in academia. To confirm this hypothesis, we use the additional Fact 3, together with the two Facts on fathers and sons described before (see Section 3), to estimate nepotism and the intergenerational human capital elasticity.

Formally, we identify the six parameters in our extended model (β, $\hat{\gamma}$, μ_h, σ_h, σ_e, κ) by minimizing the distance between these 18 simulated and empirical moments, using an objective function analogous to Equation (10).[39] As before, intergenerational correlations and father-son distributional differences allow us to estimate the intergenerational human capital elasticity between fathers and sons, net of measurement error in how observed outcomes (publications) reflect unobserved endowments (human capital). Importantly, we now recover the nepotism parameters ($\hat{\gamma}$, which in turn is based on $\hat{\nu}$) from the distributional differences between the publications of sons of scholars and ousiders who were active in the same decade and institution. In our baseline model, these were estimated solely from excess father-son distributional differences net of mean reversion.

[39]As before, we fix $\tau = 0$ and recover μ_u and σ_u from the stationarity conditions.

Table 6: Identified parameters using estimation strategy with outsiders

		Baseline estimation [1]	Estimation with outsiders [2]	Difference [3]
IGE human capital	β	0.572 (0.04)	0.562 (0.04)	-0.010 [0.919]
Nepotism, %	γ	14.38 (1.76)	14.55 (1.65)	0.170 [0.944]
Mean human capital	μ_h	3.116 (0.36)	3.425 (0.35)	0.309 [0.536]
SD human capital	σ_h	4.079 (0.20)	4.510 (0.21)	0.431 [0.131]
SD publications' shock	σ_e	0.309 (0.13)	0.248 (0.09)	-0.061 [0.697]
Threshold publications	κ	1.268 (0.14)	1.354 (0.14)	0.086 [0.655]
Entry barriers	ν or $\hat{\nu}$	8.954 (1.18)	8.850 (0.57)	-0.104 [0.937]

Notes: Col. [1] targets 13 intergenerational correlations and fathers' and sons' distributional moments (N=1,482); Col. [2] targets, in addition, 5 outsiders' distributional moments (N=9,118); SE in parenthesis from 200 bootstrapped samples with replacement; P-values in brackets from Clogg, Petkova, and Haritou (1995).

Table 6 presents the results. Column (1) reports estimates from our baseline strategy comparing fathers and sons. Note that the sample and results are different than those in Section 5.2 because of the additional sample restrictions described above. The share of nepotic sons is 14.38%, in the lower range of estimates in Table 3. Column (2) reports estimates from our extended strategy, which recovers the different entry barriers ($\hat{\nu}$) and nepotism ($\hat{\gamma}$) by comparing sons of scholars and outsiders who were active in the same decade and institution. The share of nepotic sons is 14.55%, identical to the estimate obtained from our baseline strategy comparing fathers and sons. Similarly, the parameters ν in the baseline estimation and $\hat{\nu}$ in the estimation with outsiders are similar and not statistically different from each other.[40] This suggests that the entry barriers faced by outsiders relative to scholars' sons in the same generation were similar to the the entry barriers faced by first-generation scholars relative to their sons one generation later. We find no significant differences in the estimated intergenerational elasticity of human capital, β, in our baseline (0.572) vs. extended estimation using outsiders (0.562). The other model's parameters capturing the noise with which unobserved human capital is transformed into observed publications (σ_e and κ), and the shape of the human capital distribution (μ_h and σ_h) are also not significantly different across strategies.

Altogether, our estimates are robust to comparing the entry barriers faced by scholar's sons and outsiders in the same generation. This strongly suggest that our baseline estimation strategy comparing scholars' sons and their fathers provides a credible characterization of nepotism and inherited human capital transfers in pre-modern academia.

6.3 Validation using families at different universities

In this section we perform a validation test on an alternative sample where, *ex ante*, we expect less nepotism: fathers and sons at different institutions. Social connections may be more important for

[40] Although ν is redundant to γ, we report its value to further compare the two estimation approaches.

obtaining a job where one's father is employed than in a different university or academy. Hence, sons appointed at a different institution than their father were more likely to be hired meritocratically.

We estimate our model for an alternative sample of 507 scholars appointed to at least one different university or academy than their fathers. 63.5 percent of these father-son pairs are also in the baseline sample—that is, they held positions in the same and in different institutions. The remaining 36.5 percent were never in the same institution. Since we expect these father-son pairs to be more meritocratic, a large estimate for the nepotism parameter would falsify our identification strategy. It would suggest that our nepotism parameter reflects sampling issues or that it captures other elements of the university's hiring process—e.g., information frictions affecting scholars' sons and outsiders differently. A large nepotism estimate could also suggest that broader trends outside academia—to which both our baseline and validation sample are exposed—are important for our results over time.

Appendix Table E.4 provides the empirical moments and the estimates for this alternative sample. As expected, fathers and sons appointed to different institutions have a better publication record: the share with zero publications is lower, and the mean, median, and 75th percentile are higher than for fathers and sons in our baseline sample. In addition, the distribution of publications of fathers no longer first-order stochastically dominates that of sons.

We find that nepotism was negligible in this alternative sample: only 0.04% of sons appointed to a different institution than their fathers were hired because of nepotism.This estimate is statistically significantly lower for fathers and sons in different vs. in the same institution. In addition, fathers and sons in different institutions transmitted their human capital with an elasticity of 0.81, not significantly different than our baseline elasticity (0.63).

Admittedly, these two sets of families are different in other dimensions, and successful professors may have had some sway in placing their sons in other institutions. That said, other than validating our identification strategy, this result is interesting in its own right. It shows that mobile families of scholars, where fathers and sons worked in different institutions, were not the result of nepotism. This supports the hypothesis that the establishment of an academic market with hiring across universities (De la Croix et al. 2023) might have fostered modern, open universities not subject to nepotism.

6.4 Comparison to other methods to estimate intergenerational persistence

Here we compare our β-estimates to those obtained using alternative methods in the literature. As explained above, our estimate is consistent with Clark's hypothesis that endowments transmitted across generations are more persistent than suggested by parent-child correlations in outcomes, but falls at the bottom of the range of estimates using rare surname groups (Clark 2015), multi-generation correlations (e.g., Braun and Stuhler 2018), and the informational content of surnames (e.g., Güell, Rodríguez Mora, and Telmer 2015). All these methods address the measurement error bias in parent-child correlations, but ignore selection in the form of nepotism (see Appendix B). To evaluate if our different estimate reflect the importance of addressing selection in the form of nepotism or is just a byproduct of the specifics of our setting, we use our data on father-son scholars to estimate intergen-

TABLE 7: Intergenerational elasticites amongs scholars, different methods

method		value	s.e.	N	reference
Log-log elasticity, all	$\hat{b}$	0.46	0.019	1,837	Equation (13)
Rank-rank slope, all	ρ_{PR}	0.49	0.022	1,837	Chetty et al. (2014)
Log-log elasticity, intensive margin	$\hat{b}_I$	0.36	0.028	982	Equation (13)
Rank-rank slope, intensive margin	$\rho_{PR,I}$	0.39	0.027	982	Chetty et al. (2014)
Multiple-generations' ratio	$\hat{\beta}$	0.91	0.077	216	Braun and Stuhler (2018)
Multiple-generations' ratio	$\hat{\beta}_A$	0.79	0.070	216	Braun and Stuhler (2018)
Model's β	β	0.63	0.042	1,837	-

Notes: The sample are 1,837 scholars and their fathers. In rows 3 and 4, this is restricted to 982 families in which both father and son have at least one publication. Rank-rank elasticities estimated from equation (13) using a scholar's percentile-rank in publications within 50-year birth cohorts instead of his log-publications. In rows 5 and 6, the sample are 216 scholars (G3), their fathers (G2), and grandfathers (G1); $\hat{\beta} = b_{G1-G3} \,/\, b_{G2-G3}$ and $\hat{\beta}_A = b_{G1-G3} \,/\, average\,(b_{G1-G2}, b_{G2-G3})$, where $b_{Gi-Gj} = cov(y_{Gi}, y_{Gj}) \,/\, var(y_{Gi})$ is the elasticity of publications between generations Gi and Gj. Bootstrapped standard errors.

erational elasticities using standard methods in the literature.

First, we estimate a standard log-log elasticity:

$$y_{i,t+1} = b\, y_{i,t} + e_{i,t+1}\,, \tag{13}$$

where y is an outcome for fathers, t, and sons, $t+1$. In our setting, y is the logarithm of 1 + the number of library holdings.

Second, we estimate rank-rank slopes as proposed by Chetty et al. (2014). We rank scholars' sons based on their publications relative to other scholars' sons in the same 50-year birth cohort. We then rank scholars' fathers based on their publications relative to other scholars' fathers with sons in these 50-year birth cohorts. We estimate the rank-rank slope by regressing the son's percentile-rank in publications on their father's percentile-rank in publications.

Table 7 presents the results. We find a log-log elasticity, $\hat{b}$, of 0.46. This implies that a 1% increase in a father's publications is associated with a 0.46% increase in his son's publications.[41] The log-log elasticity for fathers and sons with at least one publication, that is, the elasticity in the intensive margin, is $b_I = 0.36$. The rank-rank slope estimates are very similar: $\rho_{PR} = 0.49$ for all scholars and of $\rho_{PR,I} = 0.39$ for fathers and sons with at least one publication. In comparison, our model's β-estimate is larger than both the log-log elasticities and the rank-rank slopes estimated with our data.[42] This suggests that our larger intergenerational elasticity estimates do not only stem from the specifics of our setting, but also reflect methodological differences. Specifically, they reflect that the measurement error in father-son log-log elasticities and rank-rank slopes can attenuate intergenerational estimates.

Third, we estimate multiple-generations' methods proposed by Braun and Stuhler (2018) to address measurement error. They consider a Markov process as in Equation (2), where the endowments

[41] In terms of magnitude, this estimate for the father-son elasticity of publications in academia is similar to Braun and Stuhler (2018)'s population estimates for the elasticity of education attainment in Germany.

[42] A means t-test rejects the null that our model's β is the same as the estimates $\hat{b}$, $\hat{b}_I$, ρ_{PR}, and $\rho_{PR,I}$.

transmitted across generations, h, are not observed and are normally distributed with a mean μ_h and a variance σ_h^2. As in our setting, these unobserved endowments are transformed into observable outcomes y with measurement error: $y_{i,t} = h_{i,t} + \varepsilon_{i,t}$, where $\varepsilon_{i,t+1}$ is an independent noise term with a standard deviation of σ_ε.[43] Differently from us, they do not consider selection in the form of nepotism (Equation (3)). Under their framework, the elasticity in outcomes across n generations is $\beta^n\ \theta$, where β is the intergenerational transmission of endowments and $\theta = \sigma_h^2 \ / \left(\sigma_h^2 + \sigma_\varepsilon^2\right)$ is the measurement error bias. As $\theta < 1$, this is an attenuation bias. To correct for it and identify β, they use the ratio between the grandfather-grandson elasticity ($\beta^2\theta$) and the father-son elasticity ($\beta\theta$).

Table 7 presents estimates for this ratio using our sample of 176 families with three generations. These families contain 216 scholars (generation 3) with their fathers (generation 2) and one of their grandfathers (generation 1) in academia.[44] We report estimates of $\hat{\beta}$, the ratio of the elasticity between generations 1 and 3 to the elasticity between generations 2 and 3. We also report $\hat{\beta_A}$, the ratio of the elasticity between generations 1 and 3 to the average elasticity between generations 2 and 3 and generations 1 and 2. These methods yield a β between 0.91 and 0.79, a larger value than our model-based β and closer to the estimates of Clark (2015). This suggests that in empirical applications where selection is relevant, as is our case, the multiple-generation methods in the literature can provide upward-biased β-estimates.

Addressing this selection bias is important for studies of the intergenerational transmission of occupations, especially where nepotism is commonplace. That said, even in empirical applications where nepotism is absent, the type of entry barriers/selection bias described here may also affect intergenerational elasticities. Specifically, long-run estimates of the intergenerational elasticity of wealth, earnings, or occupations typically rely on selected samples, such as probate records—where only those leaving wealth above a legal threshold are sampled (Clark and Cummins 2015), or ancestors and descendants in a particular city—where only non-migrants are sampled (Barone and Mocetti 2020; Häner and Schaltegger 2022). Although these selection processes are different in nature to nepotism, they are related to the inherited endowments, and hence, can potentially lead to similar selection biases in intergenerational elasticities. For empirical applications studying the transmission of years of schooling (e.g., Braun and Stuhler (2018),Lindahl et al. (2015)) selection can take on different forms. For example, the inherited connections and social circles of sons may facilitate their access to more prestigious, post-graduate institutions ahead of better suited candidates. Moreover, even if these empirical applications typically use census data covering the population, families are not sampled if intergenerational links are not observed, e.g., because children emigrate or die before observable outcomes y (e.g., income, years of education) are realized. These estimates are potentially subject to a selection bias as the one described above, since whether observations are sampled or not (attrition) can be correlated with unobserved endowments h inherited by children (e.g., if there is negative selection into migration by parental endowments).

[43] This is akin to our Equations (4) and (5) but ignoring measurement error on the extensive margin, i.e., $\kappa = 0$.

[44] The number of observations is larger than the number of families because some families consist of more than three generations and some families contain brothers.

7 Robustness

We perform several additional robustness checks. This section briefly describes them; the detailed results are available in the Appendix.

Stationarity. Our estimation assumes that the human capital of fathers and sons in the population of *potential scholars* is drawn from the same distribution. This stationarity assumption is standard in the literature, but its importance to estimate intergenerational elasticities is rarely discussed (Nybom and Stuhler 2019). In Section 5.3, we relax this assumption. Specifically, we assume that a father and a son who were active in a given historical period draw their human capital from the same distribution, but we allow the human capital distribution to change across periods. Hence, we allow publications to exhibit time trends on both the extensive or intensive margin. In addition, Appendix G examines the stationarity assumption further. First, it examines trends among potential scholars using the De la Croix (2021b) dataset on all known scholars (not only fathers and sons). The mean and the standard error of publications, our proxy for human capital, are stable over time, suggesting a stationary human capital distribution. The probability of being listed in WorldCat changes around 1450, but this break is related to the introduction of the printing press rather than to changes in the human capital distribution, and is accounted by in our estimation by the κ parameter (see Section 5.3). Second, the appendix shows that under stationarity our nepotism estimates are conservative, lower-bound estimates. The reason is that our estimation uses distributional differences to identify nepotism but does not attribute all these differences to it. We allow for distributional differences to be the result of a second force: mean reversion. That is, that top scholar's sons may be "naturally" worse than their fathers, even if no nepotism is involved. In a non-stationary environment where the human capital distribution improves over time, mean reversion would explain less of the father-son distributional differences in publications. Hence, under a non-stationary environment, our nepotism estimates would be larger.

Shocks from fat-tailed distributions. Like most of the literature, we draw shocks affecting human capital from a normal distribution. An attractive alternative consists in drawing shocks from fat-tailed distributions, giving higher likelihood to the emergence of geniuses. In Appendix H we re-estimate our model under different distributional assumptions. We show that, although fat tailed distributions for human capital shocks seem *a priori* to be appealing, they do not fit the data well, which is very normally distributed after all. Our nepotism estimates are however robust to assuming fat-tailed shocks, although the estimated intergenerational persistence is not.

Linear human capital transfers. We assume that β is linear, that is, that parents at the top and bottom of the distribution transmit their endowments at the same rate. This assumption would be violated, e.g., if successful fathers spent less time with their children, reducing their human capital transfers systematically.[45] Appendix I shows evidence supporting our assumption. Specifically, OLS

[45]Note that this would generate father-son distributional differences in publications at the top of the distribution. Instead, we identify nepotism mainly from differences at the bottom of the distribution.

elasticity estimates are identical to elasticities estimated non-parametrically. The latter allow elasticities to differ across families with different publication records, and hence, with different human capital endowments.

Publication threshold. To capture measurement error on the extensive margin, our model considers κ, the minimum number of works to observe a scholar's publications. Admittedly, this parameter may differ across scholars. For example, the work of the son of a famous scholar may capture the attention of publishers more easily—even if it is of lower quality. Appendix J examines whether this can explain away our results on nepotism. We re-estimate our model allowing the publication threshold κ to be lower for scholars' sons. Our estimates are robust to this modification.

Measure of publications. Our preferred measure of the size and relevance of a scholar's output is the total number of library holdings in modern libraries by or about each scholar. This includes all the copies of work written by a scholar, but also library holdings about his work written by a different author. In Appendix K, we show that our results are robust to excluding library holdings about his work written by a different author, and to using the number of unique works by or about a scholar instead of the total library holdings. Using these two alternative measures suggests that 18.7 and 18.8% of scholars' sons were nepotic, indistinguishable from our baseline result of 18.7%. The β estimates are also similar across measures (respectively, 0.63, 0.62, and 0.61).[46]

Longevity. On average, scholar's sons in our sample died at age 61.7, six years earlier than their fathers. Since longevity is important for publications, Appendix L shows that our results are not driven by this differential longevity. We use OLS and simultaneous-quantile regressions to estimate the marginal effect of living an additional year on the mean, median, 75th and 95th percentile and on the proportion of sons with zero publications. We then use these estimates to adjust the distributional moments for the set of sons. The adjusted and baseline moments are very similar. Even after accounting for longevity differentials, the fathers' publications distribution FOSD that of sons, especially at the bottom of the distribution (*Fact 2*). This strongly suggests that our nepotism and β-estimates are not driven by differences in longevity.

Fertility differentials. Appendix M discusses the sensitivity of Fact 2 and our nepotism estimates to fertility differentials between scholars with more and less publications, and shows that estimates are unchanged when we exclude scholars with more than one son in academia.

8 Conclusion

From the Bernoullis to the Eulers, families of scholars have been common in academia since the foundation of the first university in 1088. In this paper, we have shown that this was the result of two factors: Initially, scholars' sons benefited from their fathers' connections to get jobs at their fa-

[46]Note that unique publications may reflect nepotism in the publishing industry in the past, as this measure does not distinguish between work that is highly or scantly reproduced in modern libraries. The fact that results using both measures are similar rules out that the decline in γ simply reflects an increased ability of sons to leverage their fathers' nepotic connections to obtain more publications (in addition to obtaining academic positions).

thers' university. Between 1088 and 1543, about one in two scholars' sons benefited from nepotism. They became academics even when their underlying human capital was lower than that of marginal first-generation scholar. After the Scientific Revolution, nepotism faded but families remained in academia. The reason is that scholars transmitted to their sons a set of underlying endowments, i.e., human capital, that were crucial to produce scientific knowledge. Our estimates suggest a large intergenerational elasticity of such endowments, as high as 0.6-0.65.

To disentangle the importance of nepotism vs. inherited human capital endowments, we proposed a new method to characterize intergenerational persistence. Our method exploits two sets of moments: one standard in the literature — correlations in observed outcomes across multiple generations — another novel — distributional differences between adjacent generations in the same occupation. Under a standard Becker and Tomes (1979) model of intergenerational human capital transmission, a slow rate of reversion to the mean strengthens the correlations across generations and (should) reduce the distributional differences between fathers and sons. When these distributional differences are larger than predicted by reversion to the mean, it reflects the fact that parents and children are selected under different criteria, i.e., nepotism. In other words, excess parent-child distributional differences within a top occupation can be used to identify and to quantify the prevalence of nepotism.

Our results have two important implications for measuring the rate of intergenerational persistence. First, we argue that estimates that bundle the transmission of human capital and social connections may provide biased estimates of the true rate of intergenerational persistence. The reason is that each of these two elements is associated with a different econometric bias: measurement error and selection. Our estimate for the transmission of human capital endowments is higher than estimates ignoring both biases—i.e., parent-child correlations—but in the lower range of estimates ignoring selection—i.e., multi-generational correlations, group averages, or the informational content of surnames. Specifically, when we omit nepotism, we find large intergenerational human capital elasticities among scholars, close to the 0.8–0.9 range estimated by Clark (2015). Hence, failing to account for selection can overstate the true rate of persistence of underlying human capital endowments. This problem is particularly acute in historical studies of social mobility over the very long run, which typically rely on selected samples.

Second, our proposed method circumvents some of the data requirements that have limited the study of intergenerational persistence in historical contexts. By modelling selection explicitly, our method only requires data from a well-defined universe, for example, a top occupation. Historical data of such occupations, e.g., scholars, artisans, artists, or government officers, is more common than the census-type evidence required by some of the alternative methods in the literature (Güell, Rodríguez Mora, and Telmer 2015, Lindahl et al. 2015, Braun and Stuhler 2018, Collado, Ortuno-Ortin, and Stuhler 2018). In addition, we build a novel dataset with direct links across generations over 1088–1800. This allows us to overcome the empirical challenges associated with using surname pseudo-links to estimate intergenerational elasticities over centuries Clark (2015). Finally, relative to the literature examining the concentration of certain families in top occupations, our approach allows us to estimate nepotism across time and space, beyond the specific settings where a natural experiment is available.

Finally, this paper sheds new light on the production of upper-tail human capital and its importance for pre-industrial Europe's take-off (Cantoni and Yuchtman 2014, Mokyr 2002, 2016, Squicciarini and Voigtländer 2015, De la Croix, Doepke, and Mokyr 2018). We find that the transmission of human capital within the family and nepotism follow an inverse relationship over time. Periods of advancement in sciences, like the Scientific Revolution or the Enlightenment, are associated with less nepotism in universities and scientific academies. In contrast, nepotism is prevalent in periods of stagnation and in Catholic institutions that fell behind in the production of scientific knowledge. This is consistent with the idea that in eras of rapid change in the knowledge frontier, technological progress, or cultural change, the cost from a mismatch between talents and occupation caused by nepotism exceeds the benefits from the transmission of specific human capital from parents to children (Galor and Tsiddon 1997; Carillo, Lombardo, and Zazzaro 2019).

Although nepotism only concerns fathers and sons, it is likely to reflect other forms of favouritism towards relatives, friends, and acquaintances. Hence, the high levels of nepotism might reflect broader inefficiencies and talent misallocation in early academia. Altogether, our evidence suggests that during the Scientific Revolution and the Enlightenment some of these inefficiencies were removed and that the resulting modern, open universities contributed to Europe's scientific advancements. The extent to which these changes explain Europe's rise to riches is an intriguing question for future research.

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Online Appendices

A Data appendix

This appendix lists the 343 most important secondary sources used to construct our dataset of 1,621 and 1,837 in universities and academies between 1088 and 1880. First, we complement the description in the main text on the coverage and accuracy of the data by providing additional summary statistics. Next, we describe in detail the secondary sources used for the largest universities and academies in our dataset and list all the data sources used for each institution in our dataset. Finally, we presents two examples: one to illustrate multiple-generation lineages of scholars (the Chicoyneau and Mögling dynasties), another to illustrate our data collection process (Honoré Bicais and his son Michel).

A.1 Additional descriptives on data coverage

As explained in Section 2 of the main text, we distinguish three levels of completeness in the sources used to construct our dataset of father-son pairs in pre-industrial academia (1088–1880):

- A *partial* coverage describes a situation in which the sample of scholars in an institution was informed by sources from other universities and general thematic biographies only. Under partial coverage, there is risk of sampling bias: On the one hand, if a scholar had a father who was briefly or no great account a professor, this is more likely to fall by the wayside than an underachieving son of a famous professor. On the other hand, if a scholar had a son who was briefly or no great account a professor, this is more likely to fall by the wayside than an underachieving father of a famous professor.

- A *broad* coverage is for father-son pairs in institutions where a members' catalogue listing all scholars in that institution does not exist. Instead, we use a combination of sources covering that particular institution, for example, a book on the history of that particular university or academy. Sources with broad coverage cover a large sample of scholars in an institution. Under broad coverage, hence, sampling bias is less likely, although we cannot fully rule it out.

- A *complete* coverage is for father-son pairs in institutions that are covered by an existing catalogue, compendium, website, or book whose aim is to list all scholars in that institution. For example, a source with complete coverage is a catalogue of all professors in a particular university or academy. Under complete coverage it is possible to distinguish whether a scholar's father was a professor or not with certainty.

Table A.1 shows the number of institutions and of father-son pairs by each coverage category. Around two thirds of our father-son pairs are from sources with complete coverage, 95.9% from sources with complete and broad coverage, and only 4.1% from sources with partial coverage. At the institution level, half of the universities and academies in our dataset have secondary sources with complete coverage, and 86 percent have secondary sources with complete and broad coverage. Importantly, the quality of the coverage is not related to the prestige of the university. We have an excellent coverage of the University of Macerata—a small university in Italy, while there is no comprehensive list of professors for the University of Paris.

Table A.1: Breadth of coverage

Coverage	Number of institutions	Number of sons
Complete	90	1,178
Broad	64	585
Partial	25	74
Total	179	1,837

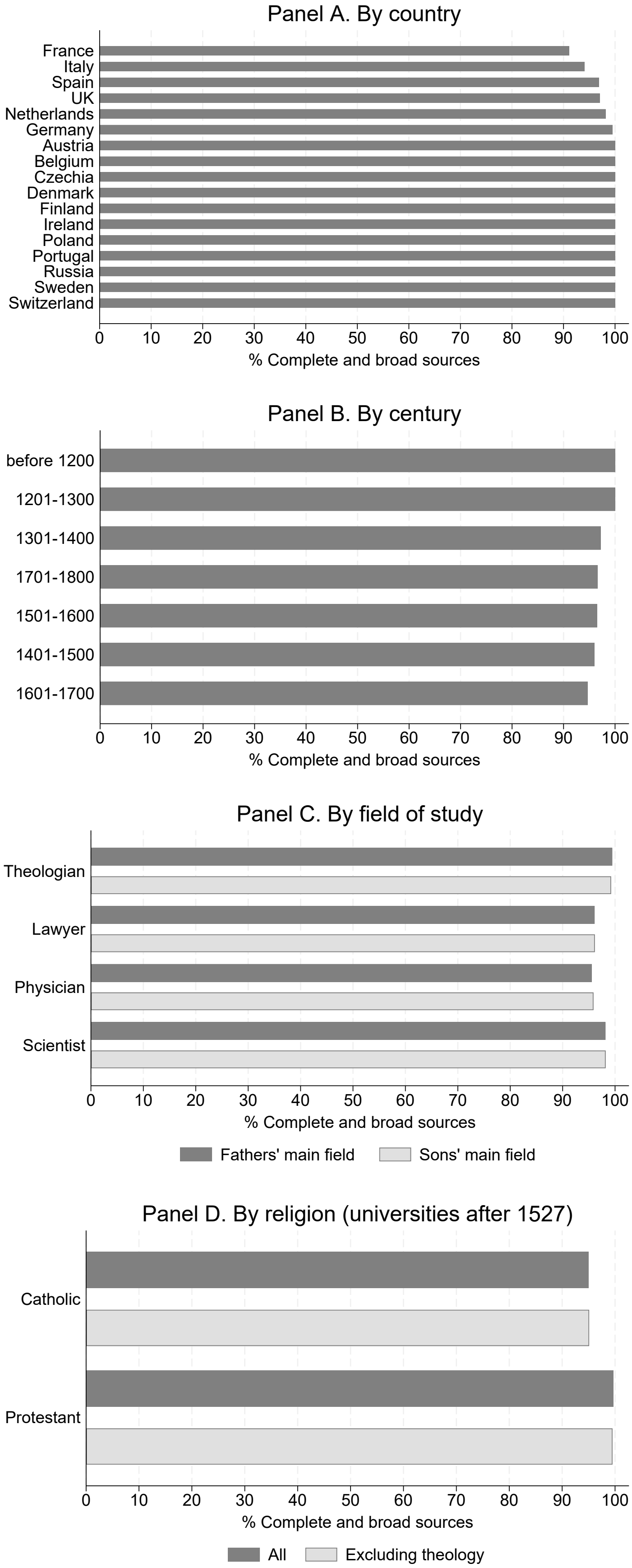
Next, we show that the share of father-son pairs coded from better sources is not heterogeneous across time, space, field of study, and religion. Specifically, Panel A of Figure A.1 shows the percentage of father-son pairs under complete and broad coverage by the country where the university or academy is located. Countries are based on modern borders. There is very little variation in this percentage, which ranges from ca. 90 to 100%. Note also that the countries where the percentage of father-son pairs from complete and broad sources is below 100 percent are both from north-west (e.g., UK) and southern Europe (e.g., Italy), and include both catholic and protestant countries.

In the main text, Table 1 showed that the coverage of the sources used to identify father-son pairs was stable across the four historical periods in our analysis: the period before 1543, the beginning of the Scientific Revolution (1543-1632), the second part of the Scientific Revolution (1633-1687), and the Enlightenment (1688-1800). Panel B of Figure A.1 complements this evidence by showing that the share of father-son pairs identified from better sources, by century. Specifically, it sorts father-son pairs by centuries based on the fathers' reference date—which includes a combination of their birth year, nomination year, or approximate activity year. The figure shows that the percentage of father-son pairs under complete and broad coverage is always above 90 percent, independenty of the century.

Similarly, Panel D of Figure A.1 shows that fathers and sons in the main fields of study that we consider in our analysis—theology, law, medicine (physicians), and science—are recovered from data sources of similar quality. As before, the percentage of fathers and sons under sources with complete and broad coverage varies little across their respective fields of study.

Finally, Panel E of Figure A.1 shows the breadth of the coverage by religion. We consider the religion of Universities after 1527—when the first Protestant University was established in Marburg. In both catholic and protestant universities, 95-100 percent of father-son pairs are based on sources with complete and broad coverage. We obtain a very similar result when we exclude theology scholars, who were typically priests or pastors and, hence, could only have (legitimate) descendants in protestant universities.

Figure A.1: Percent of father-son pairs recovered from sources with complete and broad coverage



Altogether, this evidence shows that our main results, our results over time, and or heterogeneity analysis are based on sources with very good coverage, where the possibility of selective reporting of father-son links is unlikely. In other words, it is unlikely that our estimates are driven by sampling bias in the father-son links, or by composition effects where the groups compared are based on data sources with different coverage and accuracy. Nevertheless, to fully rule out the possibility of sampling bias, in the main text we examine the robustness of our results to using data with complete coverage alone.

A.2 Data sources

Table A.2 summarizes the ten institutions with more father-son pairs in our dataset. Table A.3 lists the secondary sources used for each of the 116 universities and 63 scientific academies included in our database. Specifically, the table provides the name of the university or academy, its foundation date (and, when applicable, closure date), the number of father-son pairs in that institution, the references for the secondary sources used, the coverage of these sources (3 =Complete, 2 =Broad, 1 =Partial), and the reference to the issue of the *Repertorium Eruditorum Totius Europae* if it exists.

TABLE A.2: Institutions with the largest number of father-son pairs

Institution (dates)	N	Main Sources	Biographical dictionary†
U. Bologna (1088-)	171	Mazzetti (1847)	Treccani
Royal Society (1660-)	78	www.royalsociety.org/	DNB
Accad. dei Ricovrati (1599-)	61	Maggiolo (1983)	Treccani
U. Padova (1222-)	60	Facciolati (1757), Del Negro (2015)	Treccani
U. Avignon (1303-1793)	58	Laval (1889), Fournier (1892) de Teule (1887), Duhamel (1895)	Barjavel (1841)
U. Cambridge	48	Walker (1927), Venn (1922)	DNB
U. Tübingen (1476-)	48	Conrad (1960)	ADB
U. Copenhagen (1475-)	47	Slottved (1978)	www.geni.com
U. Basel (1460-)	45	Herzog (1780)	Attinger (1928)
Leopoldina (1652-)	44	www.leopoldina.org/	ADB

Notes: ADB: Allgemeine Deutsche Biographie; DNB: Dictionary of National Biography; Treccani: Enciclopedia italiana; N: number of father-son pairs; †Main biographic dictionary used.

Table A.3: Sources used, number of father-son pairs, and coverage, by institution (1/7)

Institution	City	Country	Dates		Nb.	Sources	Cov.	RETE
University of Bologna	Bologna	ITA	1088		171	Mazzetti (1847)	3	1:1–10
Royal Society of London (···)	London	GBR	1660		78	https://royalsociety.org/	3	7:11–19
Accademia dei Ricovrati	Padova	ITA	1599		61	Maggiolo (1983)	3	51–63
University of Padua	Padova	ITA	1222		60	Pesenti (1984), Casellato and Rea (2002), Facciolati (1757),Del Negro (2015)	3	3:33–42
University of Avignon	Avignon	FRA	1303	1793	58	Laval (1889), Fournier (1892), de Teule (1887), Duhamel (1895), Barjavel (1841)	2	
University of Tubingen	Tübingen	DEU	1476		48	Conrad (1960)	3	7:21–30
University of Cambridge	Cambridge	GBR	1209		48	Walker (1927), Venn (1922)	2	
University of Copenhagen	København	DNK	1475		47	Slottved (1978)	3	2:21–29
University of Basel	Basel	CHE	1460		45	Herzog (1780), Junius Institute (2013) Rosen (1972)	2	
Academy of Sciences Leopoldina	Halle	DEU	1652		44	http://www.leopoldina.org/	3	
University of Montpellier	Montpellier	FRA	1289	1793	37	Astruc (1767), Dulieu (1975, 1979, 1983)	2	
University of Leipzig	Leipzig	DEU	1409		35	von Hehl and Riechert (2017),Schwinges and Hesse (2019)	3	8:33–42
University of Jena	Jena	DEU	1558		30	Günther (1858)	3	1:25–32
Univ. of Pavia	Pavia	ITA	1361		29	Raggi (1879)	3	8:45–52
University of Königsberg	Kaliningrad	RUS	1544		27	Naragon (2006), Schwinges and Hesse (2019)	2	
University of Marburg	Marburg	DEU	1527		25	Gundlach and Auerbach (1927)	3	
University of Greifswald	Greifswald	DEU	1456		24	https://www.uni-greifswald.de	2	
University of Giessen	Gießen	DEU	1607		24	Haupt and Lehnert (1907)	3	2:31–38
University of Helmstedt	Helmstedt	DEU	1575	1809	22	Gleixner (2019)	3	
Royal Prussian Acad. of Sciences	Berlin	DEU	1700		22	BBAW (2022)	3	3:1–9
French Academy of Sciences	Paris	FRA	1666	1793	22	Maury (1864), Rozier (1776)	3	9:1–10
University of Strasbourg	Strasbourg	FRA	1538		22	Berger-Levrault (1890)	3	8:7–15
University of Paris	Paris	FRA	1200	1793	21	Antonetti (2013), Courtenay (1999), Hazon and Bertrand (1778)	2	
University of Rostock	Rostock	DEU	1419		19	Krüger (2019)	3	
University of Wittenberg	Wittenberg	DEU	1502	1813	17	Kohnle and Kusche (2016)	2	10:47–55

Table A.3: Sources used, number of father-son pairs, and coverage, by institution (2/7)

Institution	City	Country	Dates		Nb.	Sources	Coverage	RETE
University of Leiden	Leiden	NLD	1575		17	Leiden (2019)	3	
University of Lund	Lund	SWE	1666		17	Tersmeden (2015),Delen and Weibull (1868)	3	5:9–16
University of Perugia	Perugia	ITA	1308		16	Frova and Zucchini (2017),Zucchini (2008)	2	
University of Edinburgh	Edinburgh	GBR	1582		15	Junius Institute (2013), Grant (1884)	3	
University of Geneva	Genève	CHE	1559		14	Junius Institute (2013), Borgeaud (1900)	3	1:41–47
Académie royale d'architecture	Paris	FRA	1671	1793	14	www.cths.fr	1	
Académie Royale (· · ·) de Lyon	Lyon	FRA	1700	1790	13	Dominique (2017)	3	
Collège Royal	Paris	FRA	1530		13	Collège de France (2018)	3	1:19–24
University of Cahors	Cahors	FRA	1332	1751	10	Ferté (1975)	2	
University of Halle	Halle (Saale)	DEU	1694	1817	13	Schopferer (2016)	3	
University of Pisa	Pisa	ITA	1343		13	Fabroni (1791)	3	11:25–33
University of Salamanca	Salamanca	ESP	1218		13	Addy (1966),Arteaga (1917), Vidal y Díaz et al. (1869)	3	7:1–9
University of Louvain	Leuven	BEL	1425	1797	12	Ram (1861), Nève (1856),Brants (1906), Lamberts and Roegiers (1990)	2	4:53–66
University of Heidelberg	Heidelberg	DEU	1386		12	Drüll (1991), Drüll (2002)	3	6:25–34
Academy of(· · ·) Mainz	Erfurt	DEU	1754		12	Kiefer (2004)	3	
Royal Swedish Academy of Sc.	Stockholm	SWE	1739		12	http://www.kva.se, Dahlgren (1915)	3	
University of Valence	Valence	FRA	1452	1793	12	Brun-Durand (1900), Nadal (1861)	2	2:13–20
University of Aix	Aix-en-Provence	FRA	1409	1793	11	Belin(1905), Fleury and Dumas (1929), De la Croix and Fabre (2019)	2	4:35–44
Accademia Fiorentina	Firenze	ITA	1540	1783	11	Boutier (2017)	1	
University of Oxford	Oxford	GBR	1200		11	Emden (1959), Foster (1891)	2	
University of Franeker	Franeker	NLD	1585	1811	11	Feenstra, Ahsmann, and Veen (2003)	2	
University of Kiel	Kiel	DEU	1652		11	Volbehr and Weyl (1956)	3	
Collegium Carolinum	Zurich	CHE	1525		11	Junius Institute (2013), Attinger, Godet, and Türler (1928)	2	
University of Poitiers	Poitiers	FRA	1431	1793	10	Boissonade (1932)	2	
Uppsala University	Uppsala	SWE	1477		10	Von Bahr (1945), Astro.uu.se (2011), Jensen (2018)	2	

Table A.3: Sources used, number of father-son pairs, and coverage, by institution (3/7)

Institution	City	Country	Dates		Nb.	Sources	Coverage	RETE
Royal Society of Edinburgh	Edinburgh	GBR	1783		10	Waterston and Shearer (2006)	3	10:1–7
University of Toulouse	Toulouse	FRA	1229	1793	10	Deloume (1890), Barbot (1905), Lamothe-Langon (1823)	2	8:53–63
Académie des inscriptions (· · ·)	Paris	FRA	1663		10	Boutier (2018)	2	
University of Bordeaux	Bordeaux	FRA	1441	1793	10	Gaullieur (1874),Pery (1888)	2	
University of Lausanne	Lausanne	CHE	1537		10	Junius Institute (2013),Kiener and Robert (2005)	3	
University of Salerno	Salerno	ITA	1231		9	Sinno (1921), De Renzi (1857)	2	
University of Torino	Torino	ITA	1404		9	Vallauri (1875)	2	7:31–38
University of Glasgow	Glasgow	GBR	1451		5	Coutts (1909), University of Glasgow (2020)	2	
Zamojski University	Zamosc	POL	1594	1784	9	Kedzoria (2021)	2	10:31–37
University of Freiburg	Freiburg	DEU	1457		9	Bauer (1957)	2	9:59–68
Societas Privatas Taurinensis	Torino	ITA	1757	1792	8	Accademia delle Scienze di Torino (1973)	3	5:43–51
University of Göttingen	Göttingen	DEU	1734		8	Ebel (1962)	3	4:1–8
University of Angers	Angers	FRA	1250	1793	8	Rangeard and Lemarchand (1868), De Lens (1880), Denéchère and Matz (2012), Port (1876)	2	
Jagiellonian University	Krakow	POL	1364		8	Pietrzyk and Marcinek (2000), http://www.archiwum.uj.edu.pl/	3	6:35–42
Accademia della Crusca	Firenze	ITA	1583		7	Parodi (1983)	3	10:39–45
Utrecht University	Utrecht	NLD	1636		7	Dorsman (2011)	3	
Jardin Royal des Plantes	Paris	FRA	1635	1793	7	Jaussaud and Brygoo (2004)	3	
Academy of St Petersburg	Saint-Petersburg	RUS	1724	1917	7	Shemivot (1873)	3	5:17–25
University of Siena	Siena	ITA	1246		7	Frova, Catoni, and Renzi (2001)	2	
French Academy	Paris	FRA	1635		7	http://www.academie-francaise.fr/	3	6:17–23
University of Groningen	Groningen	NLD	1612		7	https://hoogleraren.ub.rug.nl/	3	
Åbo Akademi University	Turku	FIN	1640		7	Klinge et al. (1988)	2	
Académie d'agriculture	Paris	FRA	1761	1793	7	Marion (2019)	3	
University of Pont-à-Mousson	Pont-à-Mousson	FRA	1572	1768	7	Martin (1891)	3	2:1–6
Dutch Academy of Sciences	Haarlem	NLD	1752	1804	7	https://khmw.nl/historische-leden/	3	
University of Florence	Firenze	ITA	1321	1515	7	Prezziner (1810), Gherardi (1881)	2	

TABLE A.3: Sources used, number of father-son pairs, and coverage, by institution (4/7)

Institution	City	Country	Dates		Nb.	Sources	Coverage	RETE
Académie des Sciences et B.-L.	Toulouse	FRA	1729	1793	7	Taillefer (1985)	3	6:51–58
University of Dole	Dole	FRA	1422	1793	7	Beaune and d'Arbaumont (1870)	2	6:43–50
Accademia delle scienze	Bologna	ITA	1714		7	Ercolani (1881)	3	7:57–65
Royal Spanish Academy	Madrid	ESP	1713		6	https://www.rae.es/la-institucion/	3	3:11–17
University of Perpignan	Perpignan	FRA	1350	1793	6	Carmignani (2017), Capeille (1914),Izarn (1991)	2	
Bavarian Academy of (···)	München	DEU	1759		6	Bayerische Akademie der Wissenschaften (2022)	3	
University of Nantes	Nantes	FRA	1460	1793	6	Chenon (1890), Grünblatt (1961)	2	
"Mersenne" Academy	Paris	FRA	1635	1648	6	de Coste (1648)	3	2:7–12
University of Dublin	Dublin	IRL	1592		6	Kirkpatrick (1912), Burtchaell and Sadleir (1935)	2	
University of Caen	Caen	FRA	1432	1793	6	de Pontville (1997)	1	
University of Ingolstadt	Ingolstadt	DEU	1459	1800	6	Wolff (1973)	3	
Académie des Sciences et B.-L.	Bordeaux	FRA	1712	1793	6	Courteault (1912)	3	4:9–17
Académie des Sciences et B.-L.	Dijon	FRA	1725	1793	6	Milsand (1871)	3	11:17–24
Old University of Aberdeen	Aberdeen	GBR	1495		6	Anderson (1893)	3	4:27–34
University of Altdorf	Altdorf bei Nürnberg	DEU	1578	1809	5	Flessa (1969)	2	
Royal College of Physicians	London	GBR	1518		5	Munk (1878)	1	
University of Modena	Modena	ITA	1175		5	Mor and di Pietro (1973)	2	9:25–32
University of Valladolid	Valladolid	ESP	1280		5	Alcocer Martinéz (1918)	3	1:11–18
University of Ferrara	Ferrara	ITA	1391		5		1	
New University of Aberdeen	Aberdeen	GBR	1593		5	Anderson (1898)	3	
Majorcan cartographic school	Palma	ESP	1330	1500	5	Pastor and Camarero (1960)	2	
Royal Dublin Society	Dublin	IRL	1757		5	Berry (1915)	3	6:9–15
University of Rinteln	Rinteln	DEU	1620	1809	4	Hänsel (1971)	3	
University of Mainz	Mainz	DEU	1476	1792	4	Benzing (1986)	3	
Académie des Arts et B.-L.	Caen	FRA	1705	1793	4	de Pontville (1997)	3	
University of St Andrews	Saint-Andrews	GBR	1411		4	Smart (2004)	2	
Académie (.) de la Rochelle	La Rochelle	FRA	1732	1744	4	Marion (2019)	2	
Universty of Naples	Napoli	ITA	1224		4	Origlia Paolino (1754)	2	
University of Montauban	Montauban	FRA	1598	1659	4	Bourchenin (1882)	3	

TABLE A.3: Sources used, number of father-son pairs, and coverage, by institution (5/7)

Institution	City	Country	Dates		Nb.	Sources	Cov.	RETE
University of Sedan	Sedan	FRA	1599	1681	4	Bourchenin (1882)	3	
Akademisches Gymnasium Danzig	Gdansk	POL	1558		4	Hirsch (1837)	3	
Société Royale des Sciences	Montpellier	FRA	1706	1793	3	Dulieu (1983)	3	1:33–39
University of Harderwijk	Harderwijk	NLD	1647	1811	3	van Epen (1904)	3	
Braunschweig University (· · ·)	Braunschweig	DEU	1745		3	Albrecht and Gundler (1986)	3	
University of Orléans	Orléans	FRA	1235	1793	3	Bimbenet (1853), Duijnstee (2010)	2	
University of Rome	Roma	ITA	1303		3	Renazzi (1803)	2	
Gottingen Academy of Sciences	Göttingen	DEU	1752		3	Krahnke (2001)	3	
University of Catania	Catania	ITA	1444		3	Sabbadini (1898)	2	
University of Douai	Douai	FRA	1559	1793	3	Collinet (1900)	2	
Académie des Sciences, ...	Clermont-Ferrand	FRA	1759	1793	3	Mège (1999)	3	8:27–32
Academy of Spalding	Spalding	GBR	1710	1770	3	Matthew, Harrison, and Long (2004)	2	
University of Saumur	Saumur	FRA	1596	1685	2	Bourchenin (1882)	3	
Académie des belles-lettres, (· · ·)	Marseille	FRA	1726	1793	2	http://www.academie-sla-marseille.fr/	2	
University of Würzburg	Würzburg	DEU	1402		2	Walter (2010)	2	
Viadrina European University	Frankfurt O.	DEU	1506	1811	2	Junius Institute (2013),Schwinges and Hesse (2019)	2	
Universite of Die	Die	FRA	1601	1684	2	Bourchenin (1882)	3	
University of Macerata	Macerata	ITA	1540		2	Serangeli (2010)	3	2:39–45
Academy of Gorlitz	Gorlitz	DEU	1773		2	https://www.olgdw.de/gesellschaft/	3	
Agriculture Society of Lyon	Lyon	FRA	1761		2	Marion (2019)	1	
University of Erlangen	Erlangen	DEU	1742		2	Wachter (2009)	3	
Danzig Research Society	Gdansk	POL	1743	1936	2	Schumann (1892)	3	1:49–54
University of Vienna	Wien	AUT	1465		2	Lackner (1976), Schwinges and Hesse (2019), von Aschbach (1865)	2	
Literary and philosophical society	Manchester	GBR	1781		2	Anonymous (1896)	3	
University of Parma	Parma	ITA	1412		2	Rizzi (1953)	2	6:1–8
Accademia Aldina	Venezia	ITA	1494	1515	2	Da-Rio (1828)	2	
University of Coimbra	Coimbra	PRT	1308		2	Rodrigues (2003),Rodrigues (1992)	3	9:49–57
Derby Philosophical Society	Derby	GBR	1783	1858	2	Sturges (1978)	3	

TABLE A.3: Sources used, number of father-son pairs, and coverage, by institution (6/7)

Institution	City	Country	Dates		Nb.	Sources	Coverage
Academy of Castres	Lyon	FRA	1648	1670	2	Marion (2019)	1
Academy of the Unknown	Venezia	ITA	1626	1661	1	The British Library (2021)	1
Athenaeum Illustre of Amsterdam	Amsterdam	NLD	1632	1877	1	http://www.albumacademicum.uva.nl/	3
Academy of the Burning Ones	Padova	ITA	1540	1545	1	The British Library (2021)	1
Freiberg University (· · ·)	Freiberg	DEU	1765		1	Appelt and Wulkow (2022)	3
Nijmegen University	Nijmegen	NLD	1655	1679	1		1
Veneziana (Seconda Accademia)	Venezia	ITA	1594	1608	1	The British Library (2021)	2
University of Nîmes	Nîmes	FRA	1539	1663	1	Bourchenin (1882)	3
University of Moscow	Moskow	RUS	1755		1	Andreev and Tsygankov (2010)	3
Academy of the Invaghiti	Mantova	ITA	1562	1738	1	The British Library (2021)	2
University of Rennes	Rennes	FRA	1735	1793	1	Chenon (1890)	3
University of Prague	Prague	CZE	1348		1	Svatoš and Čornejová (1995), Čornejová and Fechtnerová (1986)	2
University of Erfurt	Erfurt	DEU	1379		1	Schwinges and Hesse (2019)	1
Royal Botanic Garden	Kew	GBR	1759		1		1
Academie de Beziers	Béziers	FRA	1723	1793	1	Marion (2019)	1
University of Cervera	Cervera	ESP	1714	1821	1	Rubio y Borras (1914)	3
Academy of the Umorists	Roma	ITA	1603	1670	1	The British Library (2021)	2
Academy of the Filateri	Ferrara	ITA	1554	1563	1	The British Library (2021)	2
University of Bourges	Bourges	FRA	1464	1793	1		2
University of Orange	Orange	FRA	1365		1		1
Society of Observers of Man	Paris	FRA	1799	1804	1		1
University of Oviedo	Oviedo	ESP	1574		1	Canella Secades (1873)	1
Royal Danish Science Society	Copenhagen	DNK	1742		1	Lomholt (1950)	3
French Academy of Medecine	Paris	FRA	1731	1793	1		1
University of Duisburg	Duisbrug	DEU	1654		1		1
University of Cagliari	Cagliari	ITA	1606		1	Tola (1837), Pillosu (2017)	2
Accademia Roveretana	Rovereto	ITA	1752		2		1
University of Alcala	Alcala de H.	ESP	1499		1	Torrecilla, Arboniés, and Torres (2013)	2

Table A.3: Sources used, number of father-son pairs, and coverage, by institution (7/7)

Institution	City	Country	Dates		Nb.	Sources	Coverage	RETE
University of St Petersburg	St. Petersburg	RUS	1724		1	Shemivot (1873)	2	
Philosophical Society	Dublin	IRL	1683	1778	1	Wilde and Lloyd (1844)	1	
Accademia Botanica	Firenze	ITA	1739	1783	1		1	
University of Dijon	Dijon	FRA	1722	1792	1		1	
University of Fermo	Fermo	ITA	1585		1	Brizzi (2001), Curi (1880)	2	5:27–34
University of Pau	Pau	FRA	1722	1793	1	Maisonnier (1972)	1	
University of Besancon	Besancon	FRA	1691	1793	1	Lavillat (1977)	3	
University of Nancy	Nancy	FRA	1768	1793	1		1	
Académie des sciences, ...	Angers	FRA	1685	1793	1	Marion (2019)	2	
University of Mondovi	Mondovi	ITA	1560		1	Vallauri (1875)	3	
Académie des Sciences, ...	Besancon	FRA	1709	1709	1	Defrasne et al. (2002)	3	7:39–45
Scuole Palatine	Milano	ITA	1647	1647	1		1	
Oxford Philosophical Society	Oxford	GBR	1658	1658	1	Gunther (1925)	2	8:17–25
Accademia Boreliana	Palmi	ITA	1673	1673	1	Maylender (1930)	1	
Société des Sciences,...	Agen	FRA	1778	1778	1	Labande (1901)	3	
Académie des belles lettres	Montauban	FRA	1738	1738	1	Forestié (1888)	3	
Accademia Filopatria	Torino	ITA	1759	1759	1	Calcaterra (1943)	3	
Société des sciences physiques	Lausanne	CHE	1785	1785	1	Société de Lausanne (1790)	3	
Societatis Regiae Scientiarum Upsaliensis	Uppsala	SWE	1700	1700	1	Karlberg (1977)	2	

Notes: Missing sources correspond to families which were mentioned in sources about other institutions.
Coverage: 3=Complete, 2=Broad, 1=Partial.

A.3 Examples

Multi-generation lineages of scholars. Our database contains 176 families with three or more generations of scholars at the same university or scientific academy. For the sake of illustration, Figure A.2 shows one of these dynasties of scholars: the Chicoyneau. The Chicoyneaus had four generations of scholars, all employed at the University of Montpellier. For almost a century (from 1659 to 1758), there was at least one Chicoyneau at the University of Montpellier. This lineage was reconstructed using Dulieu (1983). Note that some Chicoyneaus developed a prolific career. For example, François Chicoyneau (1672-1752) was a professor at Montpellier and was also appointed at the Académie des Sciences. Other members of the dynasty were appointed professor at very early ages. The last member of the dynasty, Jean-François Chicoyneau (born in 1737), was made a professor in 1752—that is, at the tender at age of 15. In principle, dynasties like the Chicoyneaus may emerge because human capital was strongly transmitted across generations, because of nepotism, or because of a combination of both.

Similarly, Figure A.3 displays another multi-generation lineage of scholars: the Mögling family at the University of Tübingen (Conrad 1960). This lineage spans six generations, from the sixteenth to the eighteenth century. The first three generations were professors in medicine. After Johan David Mögling (1650-1695), however, the family switched to law (in section 6.1 of the main text, we exploit such field switches). In the first and fifth generation, the lineage members held a professorship elsewhere: Daniel Mögling (1546-1603) at Heidelberg, Johan Friedrich Mögling (1690-1766) at Giessen.

In the main text, we exploit these multi-generation lineages to address measurement error in estimates for the transmission of human capital. Specifically, we use multi-generation lineages to compute correlations in observed publications across multiple generations. Elsewhere it has been shown that, under the assumption that measurement error is constant across generations, these multi-generation correlations reflect the transmission of (unobserved) underlying human-capital endowments. In other words, multi-generation lineages help us tackle the measurement error bias in parent-child publication elasticities.

Data collection example - Honoré and Michel Bicais. In Section 2 on the main text, we illustrate the data collection process by using the example of Honoré Bicais and his son Michel, both professors at the University of Aix. Figure A.4 shows the different sources mentioned in the main text: **(a)** Honoré Bicais' biography from Belin's *Histoire de l'Ancienne Universite de Provence* (1905) — used to identify Honoré (and Michel) as professors at the University of Aix; **(b)** The biographical dictionary of Aix's Department, *Les Bouches-du-Rhône, Encyclopédie Départementale* by (Mason 1931) — used to retrieve birth years and the quote that Michel Bicais succeeded his father in "in his chair and in his reputation;" and **(c)** Honoré and Michel Bicais' WorldCat entries — used to measure their scientific output in the form of library holdings by or about them in modern libraries.[47]

[47] The WorldCat entries in Figure A.4 were accessed on 30th of November, 2020. The number of library holdings may change slightly if modern libraries acquire/retire copies of the works by or about these authors.

Figure A.2: The Chicoyneau dynasty

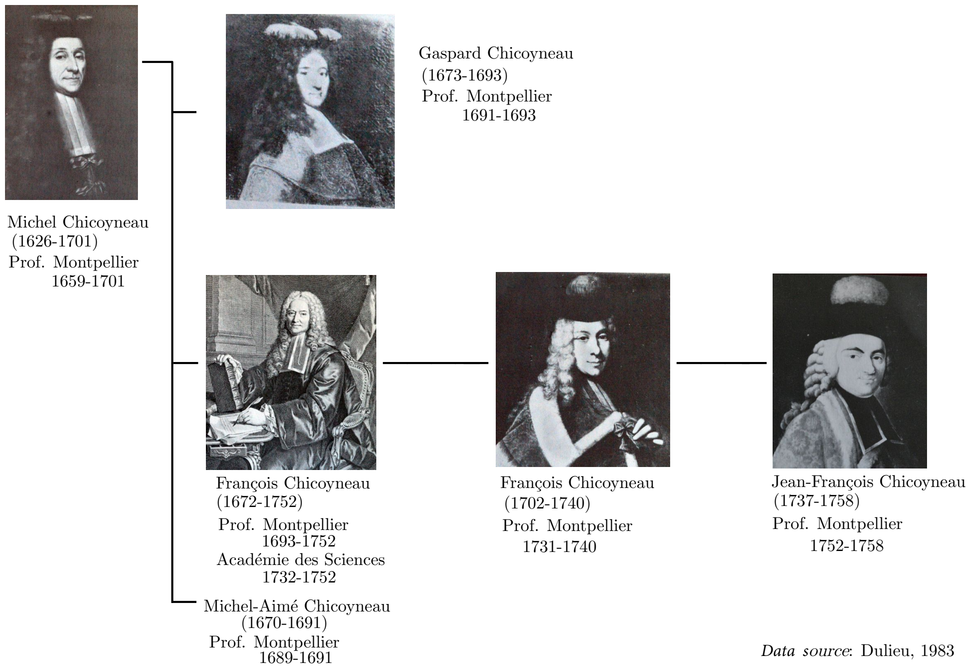


Figure A.3: The Mögling dynasty

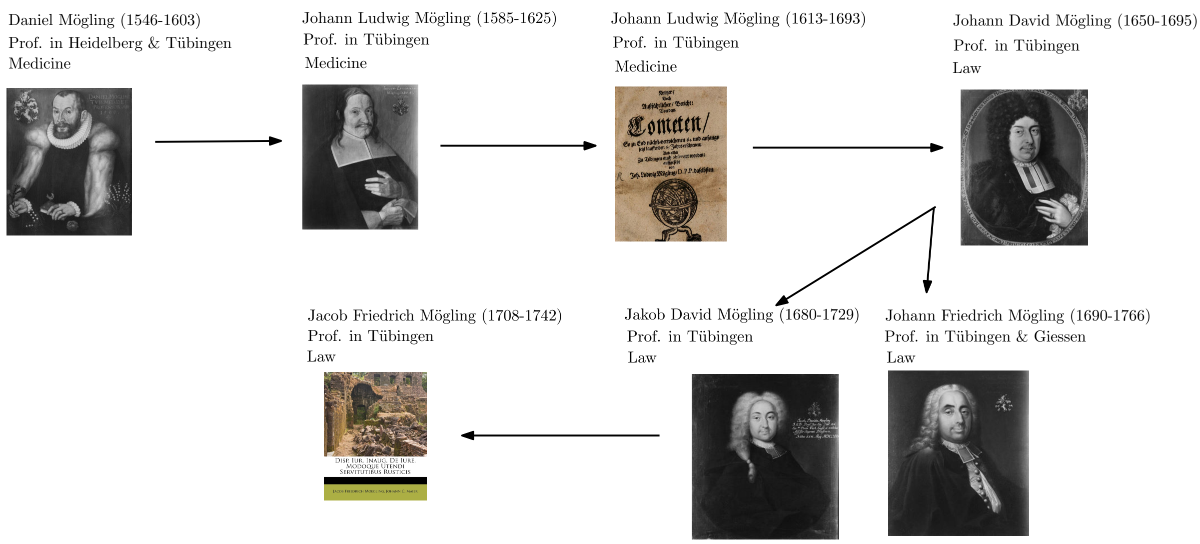
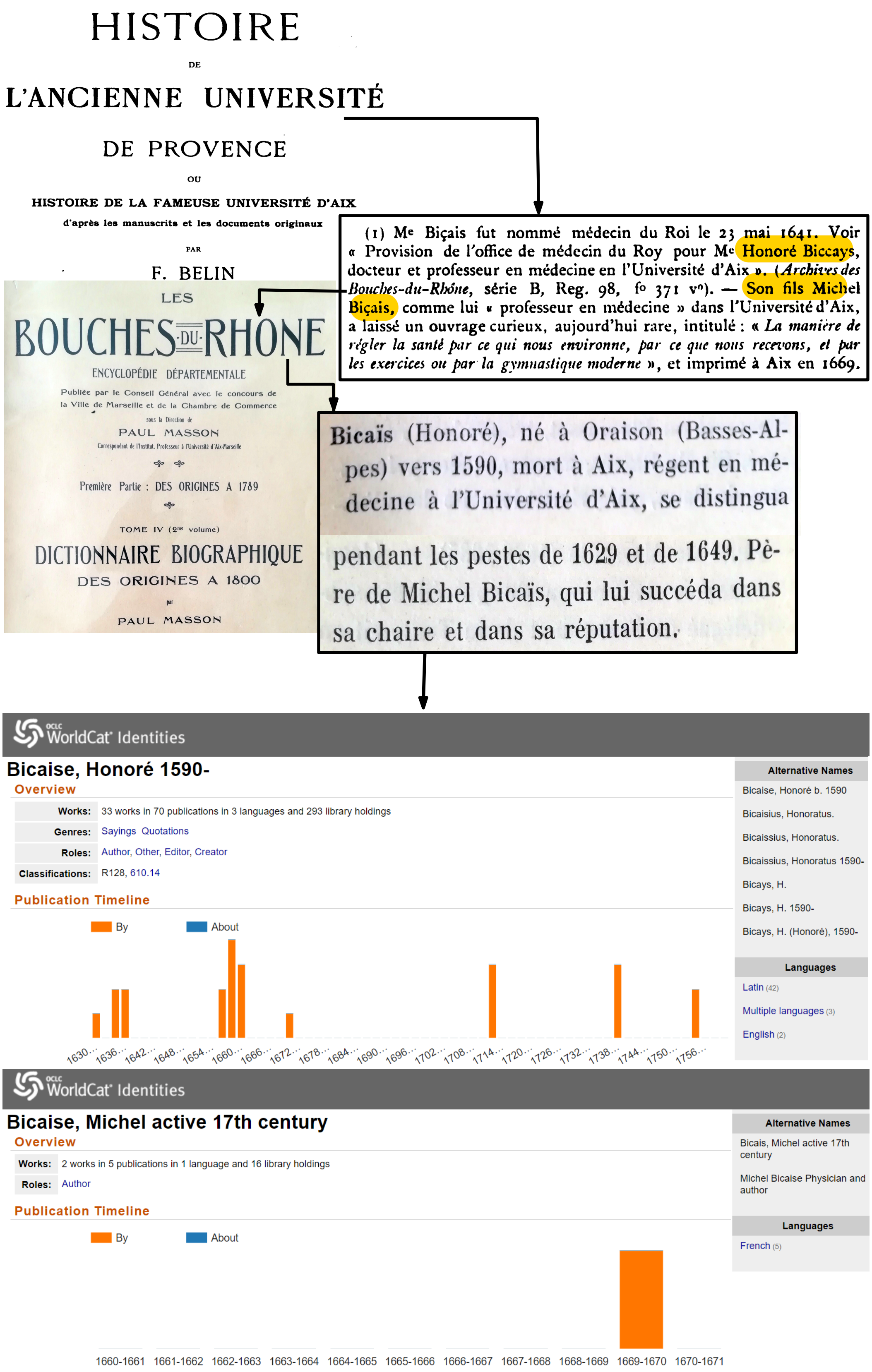


Figure A.4: Example of data collection - Honoré and Michel Bicais



B Intergenerational estimates in the literature

This appendix describes existing methods in the literature to estimate intergenerational elasticities, and highlights two potential biases: measurement error and selection of families in the data.

Parent-child elasticities. To study the extent to which inequalities are transmitted across generations, economists typically estimate coefficient b in:

$$y_{i,t+1} = b\, y_{i,t} + e_{i,t+1}\,,$$

where i indexes families, t parents, and t+1 children. The outcome y reflects social status (e.g., income, wealth, education, occupation) and is in logarithms. The coefficient b is the intergenerational elasticity of outcome y. It determines the speed at which outcomes revert to the mean. To see this, note that the half-life of y (the generations until the gap to the mean halves) is $t_{\frac{1}{2}} = -\ln(2) \,/\, \ln(|b|)$, which depends negatively on b.

Panel A of Table B.1 shows estimates of b in the literature.[48] Parent-child elasticities vary across time and space, but are generally below 0.5. This implies a half-life of $t_{\frac{1}{2}} = 1$. That is, half the gap to the mean is filled after one generation. In three generations, almost all advantages will revert to the mean.

Measurement error bias. Recent studies looking at multiple generations show that social status is more persistent than suggested by parent-child elasticities. One possible reason is that there is a highly-persistent inherited endowment that wealth, income, or occupation only reflect noisily. Children do not inherit their socio-economic outcomes directly from their parents. Instead, children inherit an unobserved human capital endowment h (e.g., knowledge, skills, genes, preferences) which then transforms into the observed outcome y imperfectly. This is modeled as a first-order Markov process of endowments transmission where endowments are observed with measurement error (Clark and Cummins 2015; Braun and Stuhler 2018):

$$h_{i,t+1} = \beta h_{i,t} + u_{i,t+1}\,, \tag{14}$$

$$y_{i,t+1} = h_{i,t+1} + \varepsilon_{i,t+1}\,, \tag{15}$$

where $h_{i,t} \sim N(\mu_h, \sigma_h^2)$ and $u_{i,t+1}$ and $\varepsilon_{i,t+1}$ are independent noise terms. The coefficient β captures the extent to which the parents' endowment h is inherited by their children. In this sense, β is the parameter governing the true rate of persistence of social status across generations. In contrast, Equation (15) determines how well this endowment is reflected in the observed outcome y. A larger variance in the noise term, σ_ε^2, is associated with a lower observability of the endowment h.

The intergenerational elasticity of outcome y estimated from Equation (13) is:

$$E(\hat{b}) = \beta\, \frac{\sigma_h^2}{\sigma_h^2 + \sigma_\varepsilon^2} := \beta\, \theta, \tag{16}$$

[48] For a more thorough review, see Solon (1999), Corak (2006), and Black and Devereux (2011).

TABLE B.1: Persistence of social status in the literature

Panel A: Estimates of b		
$\hat{b}$	y_t	Country & Source
0.31–0.41	Wealth	Agricultural societies (Borgerhoff Mulder et al. 2009)
0.48–0.59	Wealth	UK (Harbury and Hitchins 1979)
0.225	Wealth	Norway (adoptees) (Fagereng, Mogstad, and Ronning)
0.6	Earnings	USA (Mazumder 2005)
0.34	Earnings	USA (Chetty et al. 2014)†
0.47	Earnings	USA (Corak 2006)
0.19–0.26	Earnings	Sweden (Jantti et al. 2006)
0.11–0.16	Earnings	Norway (Jantti et al. 2006)
0.46	Education	USA (Hertz et al. 2007)
0.71	Education	UK (Hertz et al. 2007)
0.35	Education	Sweden (Lindahl et al. 2015)
0.35	Body Mass	USA (Classen 2010)
Panel B: Estimates of β		
$\hat{\beta}$	y_t	Data & Source
0.70–0.75	Wealth	UK probate (1858–2012) (Clark and Cummins 2015)
0.70–0.90	Oxbridge	UK (1170–2012) (Clark and Cummins 2014)
0.61–0.65	Occupation	Germany, 3 gen. (Braun and Stuhler 2018)
0.49–0.70	Education	Germany, 4 gen. (Braun and Stuhler 2018)
0.6	Education	Spain, census (Güell, Rodríguez Mora, and Telmer 2015)
0.61	Schooling	Sweden, 4 gen. (Lindahl et al. 2015)
0.49	Earnings	Sweden, 4 gen. (Lindahl et al. 2015)
0.74	Education	EU-28, 3 gen. (Colagrossi, d'Hombres, and Schnepf 2019)
0.8	Education	Spain, census (Collado, Ortuno-Ortin, and Stuhler 2018)

† Rank-rank slope instead of log-log elasticity.

where $\theta < 1$ is an attenuation bias for β.

Several methods have been used to identify β. One is to exploit correlations in y across multiple generations.[49] According to the first-order Markov process described above, the elasticity of outcome y is $\beta\theta$ between parents, t, and children, $t+1$, and $\beta^2\theta$ between grandparents, t, and grandchildren, $t+2$ (as long as the signal-to-noise ratio is stable across generations). Hence, the ratio of these elasticities identifies β. Intuitively, β is identified because the endowment h is inherited, but the estimation bias θ is not—it is the same across two or three generations. Another identification strategy for β is to estimate intergenerational regressions of Equation (13)'s form with group-average data for siblings (Braun and Stuhler 2018) or for people sharing rare surnames (Clark and Cummins 2015). By grouping individuals with similar inherited endowments, the noise term ε is averaged away. Güell,

[49] Lindahl et al. (2015), Braun and Stuhler (2018), Colagrossi, d'Hombres, and Schnepf (2019).

Rodríguez Mora, and Telmer (2015) propose to identify β through the informational content of rare surnames (ICS)—a moment capturing how much individual surnames explain the total variance of individual outcomes.[50] This method only requires cross-sectional data, i.e., it does not require linking data across generations. Similarly, Collado, Ortuno-Ortin, and Stuhler (2018) estimate β using horizontal kinship correlations in the cross-section.

Panel B of Table B.1 reports estimates of β from these different approaches. The estimates range between 0.49 and 0.90, and hence are substantially larger than the parent-child elasticities b. Furthermore, Clark (2015)'s comprehensive evidence suggests that β is close to a "universal constant" across societies and historical periods. This finding is disputed by studies using the ICS (Güell et al. 2018) or multi-generation links (Lindahl et al. 2015; Braun and Stuhler 2018; Colagrossi, d'Hombres, and Schnepf 2019) instead of surname-averages.

In light of this evidence, the unobserved endowment that children inherit from their parents has often been interpreted as skills, preferences, or even genes. First, because these endowments reflect well the measurement error problem described here: wealth, income, education, etc. only reflect skills and innate abilities with noise. Second, because if β is a universal constant, it should reflect nature rather than nurture. In other words, if β does not vary substantially across time and space, an obvious conclusion is that institutions, social policies, or processes of structural economic transformation cannot affect social mobility in the long run.

We argue that these estimates may be subject to another source of bias in settings where favoritism or nepotism are prevalent. That is, where those with power and influence give preference to friends and relatives ahead of better-qualified outsiders. For example, estimates of occupational or wage persistence may be affected by the fact that certain jobs have higher entry barriers for outsiders than for sons of insiders. Econometrically, this introduces a different bias: selection.

Selection bias. Beyond measurement error, parent-child elasticities may be subject to sample selection: whether observations are sampled or not may be correlated with the unobserved endowment h inherited by children. This additional source of bias is inherent to data used to evaluate social mobility. It is present in applications that focus on a subgroup of the population, e.g., an occupation or those leaving wills. Specifically, in certain occupations relatives of insiders may be more likely to be observed. This kind of selection bias is typically addressed using natural experiments. Similarly, wealth elasticities rely on wills and probate records, where only those leaving wealth above a legal requirement are sampled (Clark and Cummins 2015). This sampling criterion is likely to be correlated with h, an individual's inherited endowments (e.g., social competence, skills, genes). Sample selection may also arise in applications covering the entire population. In census data linking several generations, families are not observed if a generation migrates or dies before outcomes are realized (e.g., wage, occupation choice). This attrition can be correlated with the underlying endowment h. Finally, life-history data collected retrospectively may suffer from recall bias. This bias depends on h if families with large endowments have better knowledge of their ancestors.

[50] The ICS is the difference in the R^2 of regressing y on a vector of dummies indicating surnames vs. a regression in which this vector indicates "fake" surnames.

To see how selection affects intergenerational elasticity estimates, let s be a selection indicator such that $s_i = 1$ if family i is used in the estimation, and $s_i = 0$ if it is not. The intergenerational elasticity of y estimated from Equation (13) is:

$$E(\hat{b}) = b + \frac{\text{Cov}\left(s_i y_{i,t},\ s_i e_{i,t+1}\right)}{\text{Var}\left(s_i y_{i,t}\right)}.$$

Note that if $\text{Cov}\left(s_i y_{i,t},\ s_i e_{i,t+1}\right) = 0$, then $\hat{b}$ is an unbiased estimate of b and a biased estimate of β due to measurement error, i.e., $\hat{b} = \theta\beta$. If the selection indicator, s_i, is correlated with the underlying endowments transmitted across generations, $h_{i,t}$ and $h_{i,t+1}$, then the condition above is violated and $\hat{b}$ is a biased estimate of b.

These two biases are fundamentally different. As described above, measurement error can be corrected using multiple generations. The reason is that across n generations, the underlying endowment is inherited $n-1$ times at a rate β but only twice transformed into the observed outcome y with measurement error. This is not true for the selection bias, which depends on the h, and hence is 'inherited' $n-1$ times. For example, consider grandparent-grandchild (and parent-child) correlations in outcomes: The correlations depend on β—which is inherited twice (once), on the measurement error with which h is twice (twice) transformed into y, and on the selection bias—which is also 'inherited' twice (once). Hence, the ratio of grandparent-grandchild to parent-child correlations does not correct for selection. Moreover, if selection changes over time (e.g., due to changes in the prevalence of nepotism) this bias may differ across two and three generations. In other words, the ratio of grandparent-grandchild to parent-child correlations may provide upward or downward biased estimates of β.[51]

Henceforth, we restrict our analysis to sample selection—the bias emerging when inherited human capital is correlated to whether families are sampled or not. Another issue is whether human capital endowments (h) are genetically inherited (selection) or are determined by parental investments (causation).[52] We abstract from this selection story as our main purpose is to disentangle nepotism from human capital endowments, regardless of whether the latter are determined by nature or nurture. That said, in our empirical application it is possible that a scholar strategically invests in the human capital of his most endowed son, i.e., the son with higher chances of becoming a scholar *ex ante*. Unfortunately, we only observe the children of scholars who become scholars themselves. Hence, we cannot use sibling comparisons to address this issue. That said, such strategic investments in the most endowed son would lead to understating the rate of mean reversion in scholars' human capital and to overstating nepotism—which we already estimate to be low in periods of rapid scientific advancement.

[51] Formally, this ratio is an upward biased estimate of β if $\dfrac{\text{Cov}\left(s_i\, y_{i,t},\ s_i\, e_{i,t+2}\right)}{\text{Cov}\left(s_i\, y_{i,t},\ s_i\, e_{i,t+1}\right)} > 1$.

[52] Different strategies have been used to address this kind of selection: twin studies (Behrman and Rosenzweig 2002), adoptees (Plug 2004; Jantti et al. 2006; Sacerdote 2007; Black et al. 2019; Fagereng, Mogstad, and Ronning), and policy changes affecting parents' outcomes exogenously (Black, Devereux, and Salvanes 2005). See Holmlund, Lindahl, and Plug (2011) and Black and Devereux (2011) for reviews.

C Identification

This appendix describes in more detail how our 13 moments identify the model's parameters and illustrates our identification strategy with simulations.

We identify the deep parameters of our model of human capital transmission with nepotism using the two Facts described in Section 3, Table 2. Specifically, we identify the intergenerational elasticity of human capital (β), the magnitude of nepotism (γ), the noise with which unobserved human capital is transformed into observed publications (σ_e and κ), and the shape of the human capital distribution (μ_h and σ_h) by minimizing the distance between 13 simulated and empirical moments in Table 2.[53] The 13 empirical moments used in the estimation can be grouped into two categories: First, as is standard in the literature, we consider correlations in observed outcomes across generations. Specifically, we consider the father-son correlation in publications conditional on both having at least one observed publication (intensive margin) and the proportion of families where father and son have zero publications (extensive margin). When observed, we also consider the grandfather-grandson correlation in the intensive margin. Second, we depart from the previous literature and consider ten moments describing the empirical distribution of publications for fathers and sons. These moments are the mean, the median, the 75th and 95th percentiles, and the proportion of zeros.

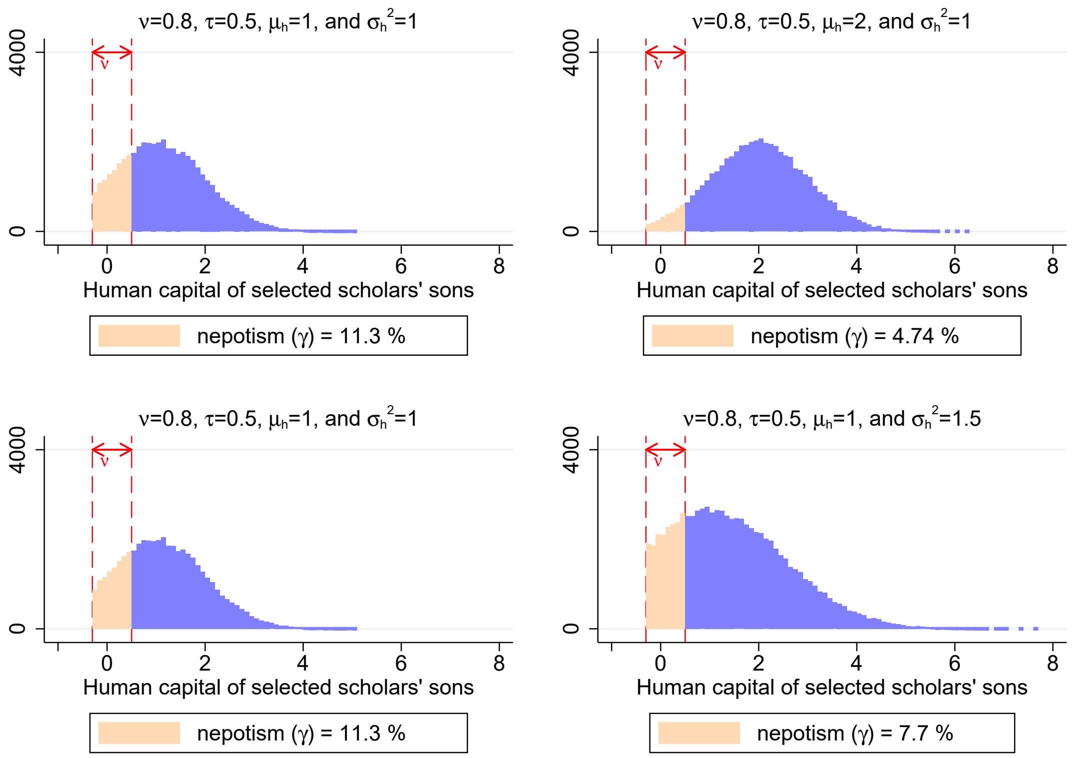
Before describing how the empirical moments used in the estimation identify the model's parameters, it is worth noting how γ, the magnitude of nepotism, depends on the other model's parameters. Specifically, γ is determined by parameters ν and τ, but also by the distribution of human capital among all potential scholars. In other words, $\tau - \nu$ alone does not characterize the magnitude of nepotism. For example, the same $\tau - \nu$ can reflect low levels of nepotism if the mean μ_h and the variance σ_h^2 of the stationary human capital distribution are high, and high levels of nepotism if μ_h and σ_h^2 are low. This is illustrated in Figure C.1, which shows the simulated distribution of human capital of sons of scholars under different model's parameters. All panels consider the same $\tau - \nu = 0.3$, but a different mean, μ_h, and variance, σ_h^2, for the human capital distribution. Specifically, the left panels consider a benchmarck scenario with $\mu_h = 1$ and $\sigma_h^2 = 1$. The top right panel considers a scenario with a larger mean ($\mu_h = 2$ and $\sigma_h^2 = 1$), and the bottom right panel a scenario with a larger variance ($\mu_h = 1$ and $\sigma_h^2 = 1.5$). Although ν is constant across panels, the share of nepotic sons varies considerably.

Next, we describe how our 13 empirical moments identify the model's parameters. Father-son correlations provide biased estimates of β due to measurement error, governed by σ_e and κ, and due to selection from nepotism, γ. We address both biases by comparing not only observed *outcomes* across generations, but also the corresponding *distributions*. These comparisons respond differently to measurement error and nepotism, and hence can be used to identify the model's parameters.

In terms of observed *outcomes*, an increase in measurement error reduces the extent to which father-son correlations reflect β. The reason is that measurement error alters these correlations but not the underlying human capital endowments. In contrast, an increase in nepotism alters the human

[53] The parameters μ_u and σ_u are pinned down from the stationarity conditions (6) and (7). We assume $\tau = 0$ without loss of generality and recover ν from Equation (9).

Figure C.1: The magnitude of nepotism (γ) and other model's parameters



Notes: Based on 50,000 simulated families of potential scholars.

capital distributions for selected fathers and sons, and also the corresponding father-son correlations. Hence, these correlations may become more informative of β.

In terms of observed *distributions*, nepotism and measurement error also have different implications. Measurement error is not associated with differences in the distribution of the observed outcome y across generations. In contrast, nepotism lowers the selected sons' human capital relative to that of their fathers. This generates distributional differences across generations (beyond those generated by reversion to the mean), as suggested by Figure 5. Intuitively, the distributional differences generated by nepotism are stronger at the bottom of the distribution, i.e., closer to the selection thresholds. Our estimation strategy, hence, puts additional weight on the proportion of father's and sons with zero publications. In addition, the variance of the distributions—captured by the 75th and 95th percentiles—also helps to disentangle measurement error from nepotism: an increase in measurement error increases the variance of both distributions, while an increase in nepotism increases the variance of the sons' distribution relatively more. In theory, this allows to correct for measurement error without resorting to grandfather-grandson correlations. That said, in our empirical application measurement error is governed by two parameters, σ_e and κ. This additional moment, i.e. grandfather-grandson correlations, helps to identify σ_e and κ separately.[54]

[54] In other words, for datasets in which κ is not binding, the measurement error bias is governed by one parameter, σ_e. This can be identified with the variance of the observed outcome's distribution across generations, without resorting to

In sum, our identification strategy exploits the fact that an increase in the degree of nepotism (measurement error):

(i) generates (does not generate) father-son distributional differences;

(ii) increases (does not increase) the variance of sons' outcomes vs. their fathers';

(iii) increases (reduces) the information that father-son correlations convey about intergenerational human capital transmission.

Hence, by comparing both outcomes and distributions across generations, we can disentangle measurement error from selection and identify our model's parameters.

We illustrate our identification strategy with simulations. Figure C.2 shows the simulated distributions of the underlying (human capital) and the observed outcome (publications), father-son correlations in publications and the corresponding QQ plot. Column A presents a benchmark simulation for 10,000 potential scholars with $\beta = 0.6$, $\gamma = 13.5\%$, $\mu_e = 1$, $\pi = 0$, $\mu_h = 2$, $\sigma_h = 5$, $\sigma_e = 0.25$, $\tau = 0$, and $\nu = -1$. In Column B, we increase σ_e^2 to 3. That is, we generate measurement error by reducing the extent to which human capital translates into publications. The distribution of h is not altered with respect to the benchmark case, but that of y is: both fathers and sons present a larger mass of zero publications and a larger variance. Since y is similarly affected for fathers and sons, the QQ plot does not reflect distributional differences across generations. However, the increase in measurement error attenuates the father-son correlation in y, which drops from 0.45 to 0.27 with respect to the benchmark case.

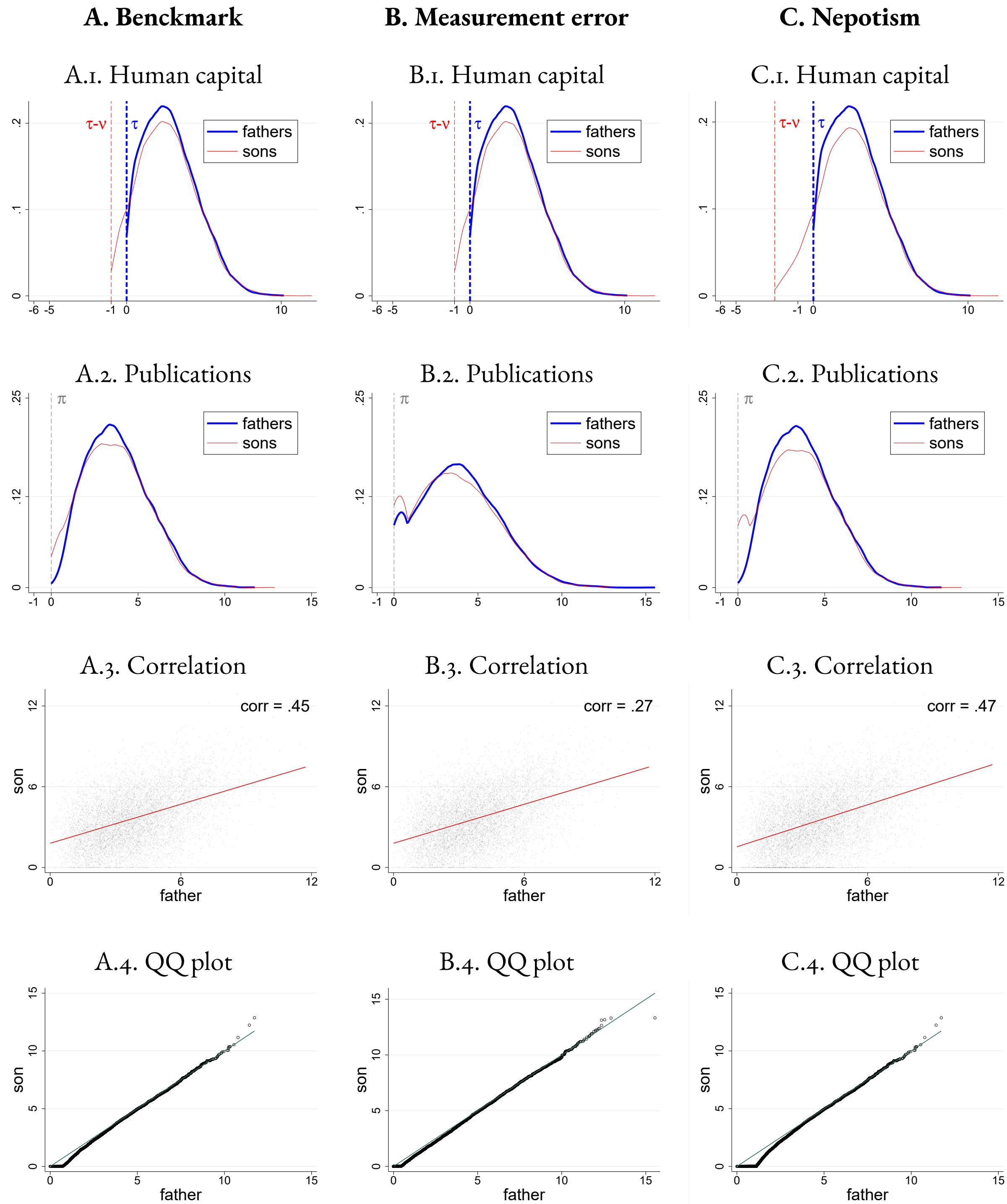
Column C increases nepotism with respect to the benchmark case by setting $\gamma = 40\%$ (or, alternatively, by setting $\nu = -2.5$ with the remaining model parameters being constant). In contrast to the previous exercise, this affects the distribution of both h and y, as sons with low levels of human capital now can become a scholar.[55] This generates distributional differences in observed publications between fathers and sons, reflected in the QQ plot. Most evidently, the mass of sons with zero publications and the variance of sons' publications is now larger than their fathers'. Since nepotism alters both the human capital's and the observed outcome's distribution, father-son correlations become more informative of β than in the benchmark case: the correlation increases from 0.45 to 0.47.

In sum, measurement error and nepotism have different implications for father-son correlations, distributional differences (especially, at the bottom of the distribution), and the relative variances of observed outcomes.

grandfather-grandson correlations.

[55]The father's h distribution is also affected, albeit to a lesser degree. The reason is that marginal fathers, i.e., fathers with an h just above the threshold τ, are now more likely to be in the set of selected families. Before, these fathers were mostly excluded, as their sons were likely to have low realizations of h, falling below the (nepotic) threshold to become a scholar. Similarly, this may decrease the variance of fathers' publications.

Figure C.2: Identification example based on model simulations



Notes: The benchmark simulation is for 10,000 potential scholars with $\beta = 0.6$, $\gamma = 13.5\%$, $\mu_e = 1$, $\pi = 0$, $\mu_h = 2$, $\sigma_h = 5$, $\sigma_e = 0.25$, $\tau = 0$, and $\nu = -1$. Column B increases σ_e to 3, Column C increases nepotism by setting $\gamma = 40.2\%$.

D Model fit

This appendix provides additional descriptive statistics and a detailed discussion on model fit, which is summarized in the main text (see Section 5.2).

Table D.1 shows the 6 parameter estimates and the 13 empirical and simulated moments for our baseline model as well as for the alternative model without nepotism discussed in Section 5.2. Panel A presents parameter estimates, Panel B empirical and simulated moments. Specifically, the top rows present the simulated and empirical moments regarding correlations across generations, and serve to evaluate Fact 1; the bottom rows evaluate the fit for the fathers' and sons' marginal distribution of publications, and serve to evaluate Fact 2.

Our baseline estimates reproduce Fact 1. Our model with nepotism matches the father-son correlation on the intensive margin of publications – that is, conditional on both father and son having at least one observed publication. This is the correlation to which our objective function attaches additional weight. Interestingly, this correlation is below the estimate of β. This implies that father-son correlations in outcomes under-predicts the extent to which children inherit human capital endowments from their parents. Our model with nepotism matches the proportion of families where father and son have zero publications (extensive margin) and the correlation between grandfathers and grandsons in the intensive margin. That said, we slightly under-predict these moments compared to the model without nepotism. Importantly, our baseline model matches the empirical fact that the grandfather-grandson correlation is larger than predicted by iterating the two-generation correlation. Specifically, our simulated grandfather-grandson correlation is 0.189. In contrast, iterating the simulated two-generation correlation yields $0.375^2 = 0.1406$.

Our estimates also reproduce Fact 2, that is, that the publications' distribution of fathers first-order stochastically dominates that of sons. Table D.1 shows that we fit both distributions: we perfectly match the proportion of fathers and sons with zero publications—the two moments to which our objective function attaches additional weight. We also match the sons' mean and median. For fathers, we underestimate the number of publications, especially in the 75th percentile. That said, we reproduce the father-son distributional differences described in Fact 2. We match the fact that fewer fathers have zero publications, that fathers on average published more than sons, and that the median father, the father on the 75th, and on the 95th percentile published more than the corresponding sons. We also reproduce the empirical observation that the gap between fathers' and sons' publications is more prominent at the bottom of the distribution: our simulated moments reflect larger father-son gaps in the mean and the median than in the 75th and 95th percentile.

As explained in the main text, nepotism is crucial for reproducing the father-son distributional differences. Specifically, we estimate an alternative model without nepotism, $\gamma = 0$. This model, where distributional differences can only be generated by mean reversion, fails to match Fact 2, and only generates very small distributional differences above the median.

Table D.1: Simulated and empirical moments for different models

	Model w/o nepotism	Baseline model	Data
A. Parameters:			
β	0.723	0.634	.
γ	0 *(imposed)*	18.74	.
μ_b	4.853	1.865	.
σ_b	2.204	4.219	.
σ_e	1.119	0.393	.
κ	3.989	2.121	.
B. Moments:			
Father-son correlation†	0.375	0.375	0.375
Father-son with zero publications	0.205	0.170	0.211
Grandfather-grandson correlation†	0.245	0.189	0.234
Fathers with zero publications	0.350	0.289	0.288
Sons with zero publications	0.350	0.383	0.384
Median, fathers	4.921	3.678	5.075
Median, sons	4.918	3.231	3.402
75th percentile, fathers	6.584	5.963	7.370
75th percentile, sons	6.574	5.832	6.413
95th percentile, fathers	8.979	9.612	9.425
95th percentile, sons	8.888	9.539	8.537
Mean, fathers	4.110	3.844	4.456
Mean, sons	4.105	3.463	3.477

Notes: † correlation on the intensive margin.

E Additional figures and tables

E.1 QQ plots

Figure E.1: Quantile-quantile plot by historical period

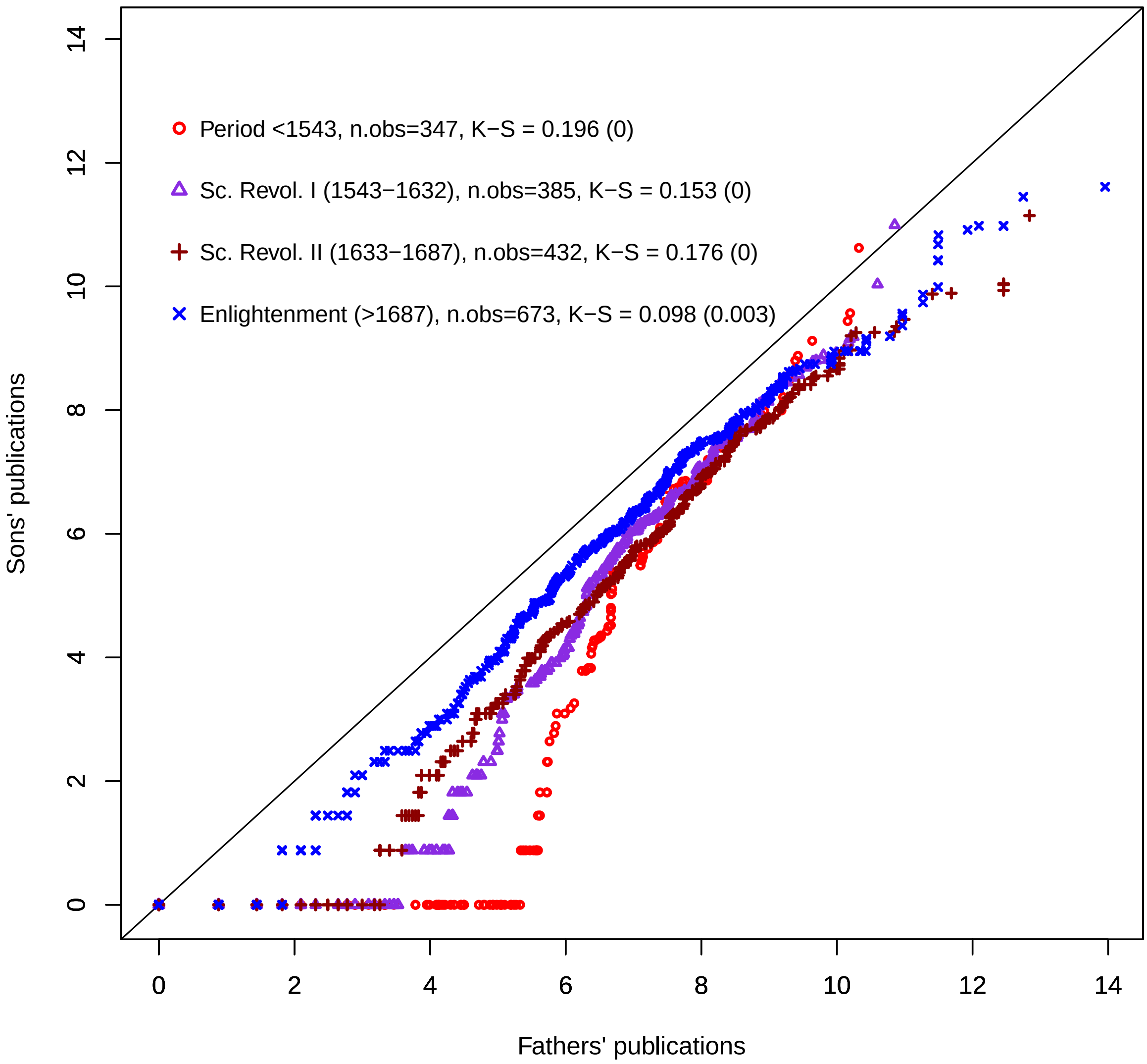


Figure E.2: Quantile-quantile plot by age of institution

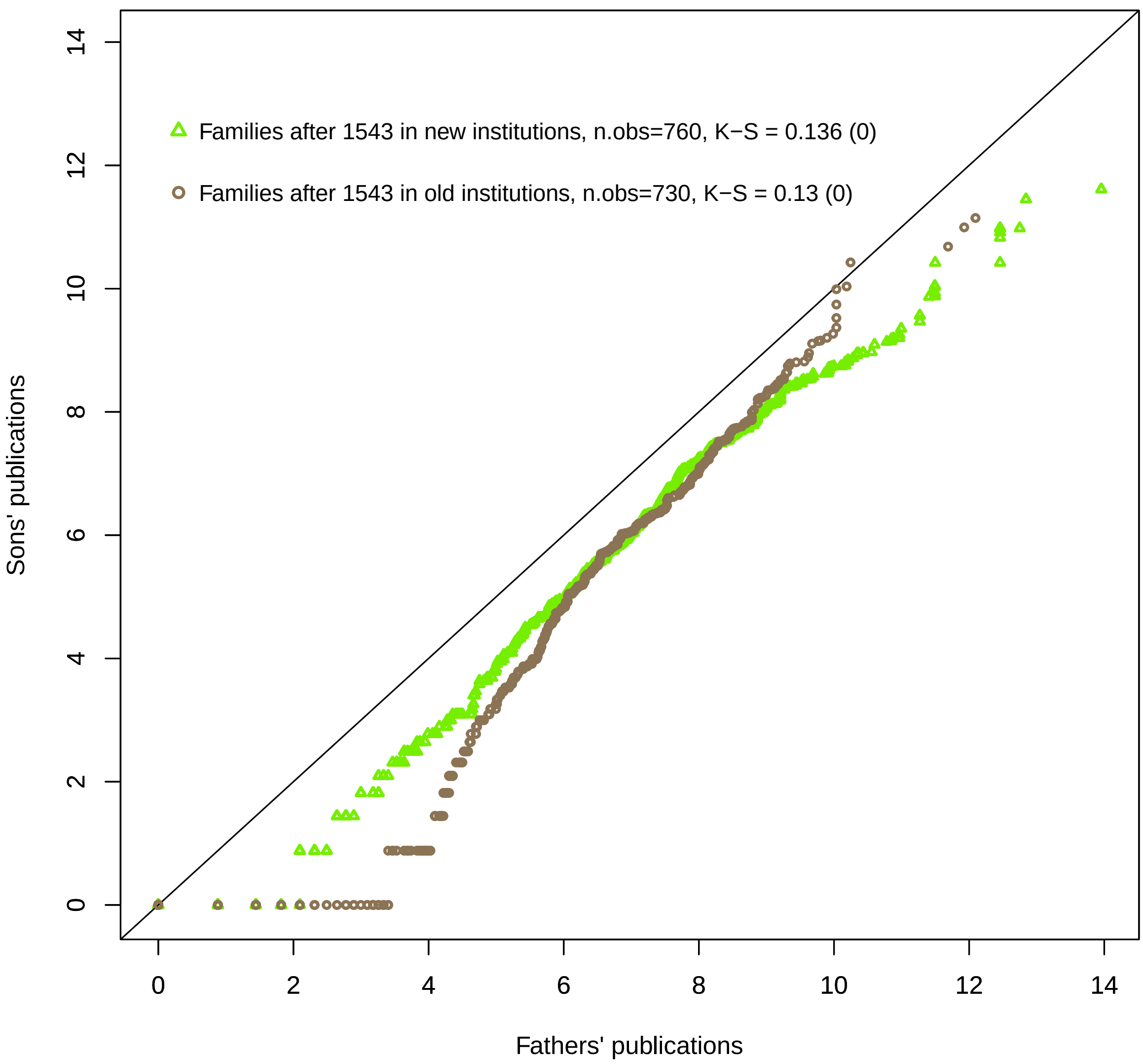


Figure E.3: Quantile-quantile plot by religious affiliation

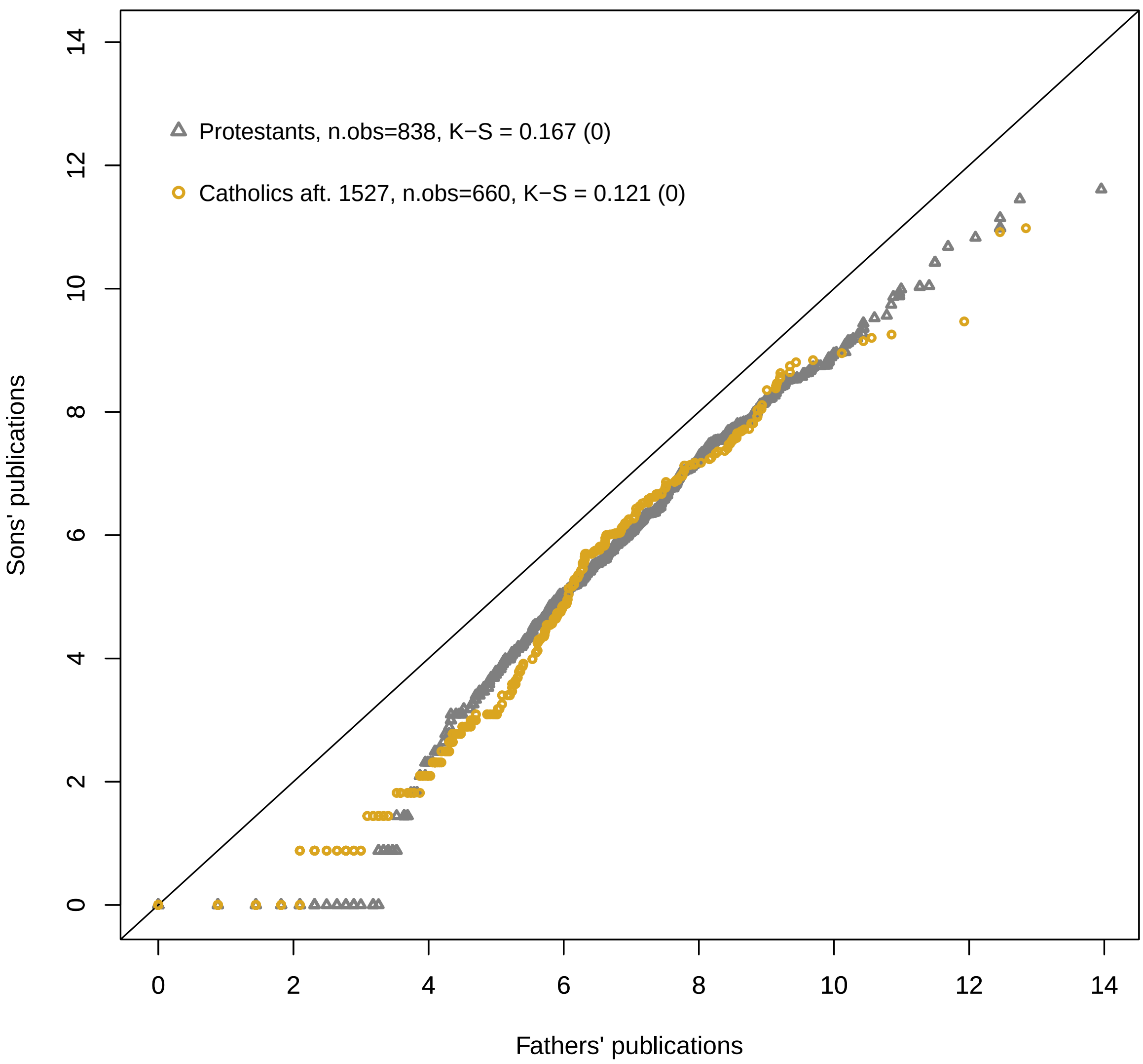


Figure E.4: Quantile-quantile plot by field of study

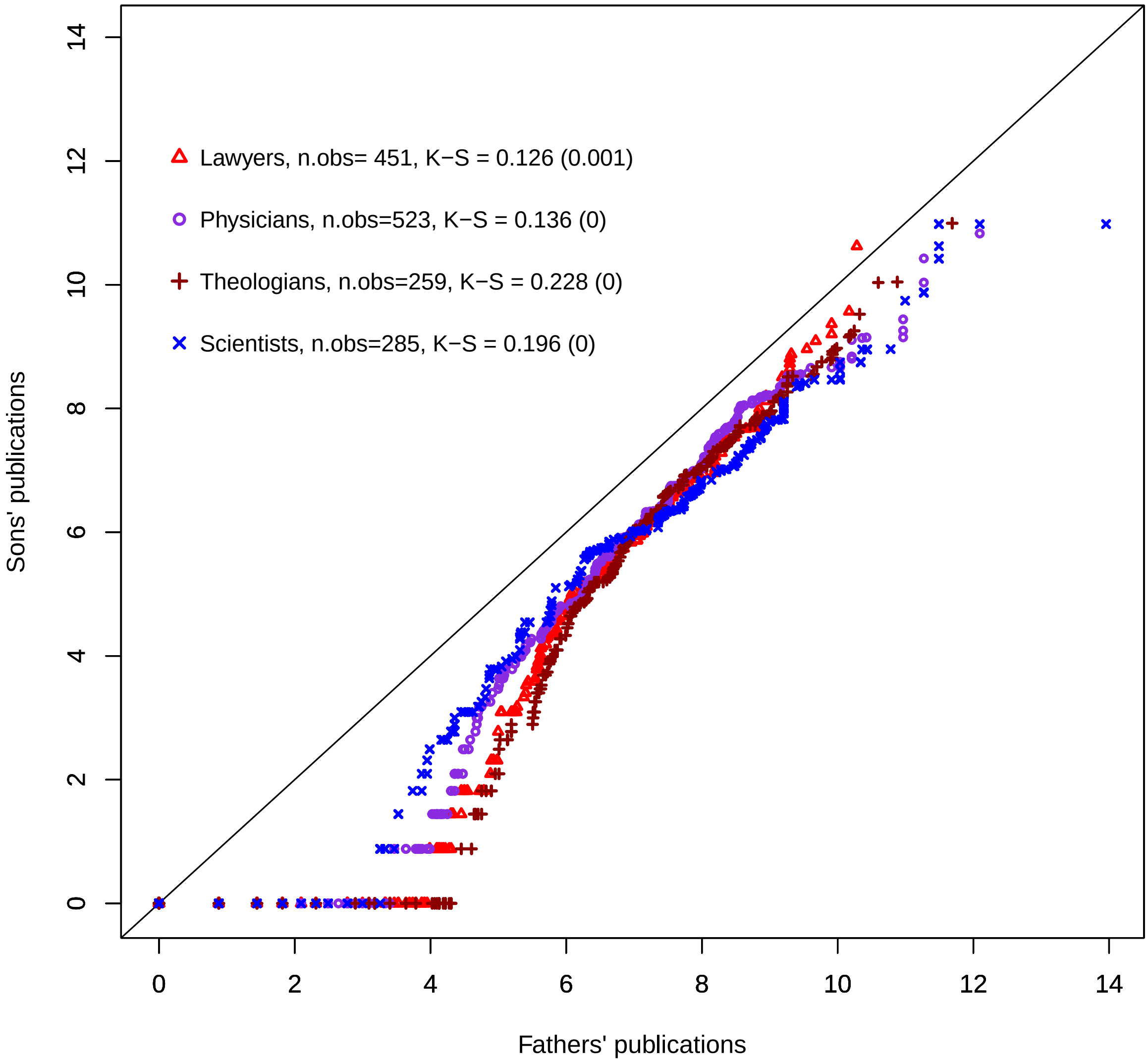


Figure E.5: Quantile-quantile plot by fathers and sons in same vs different fields

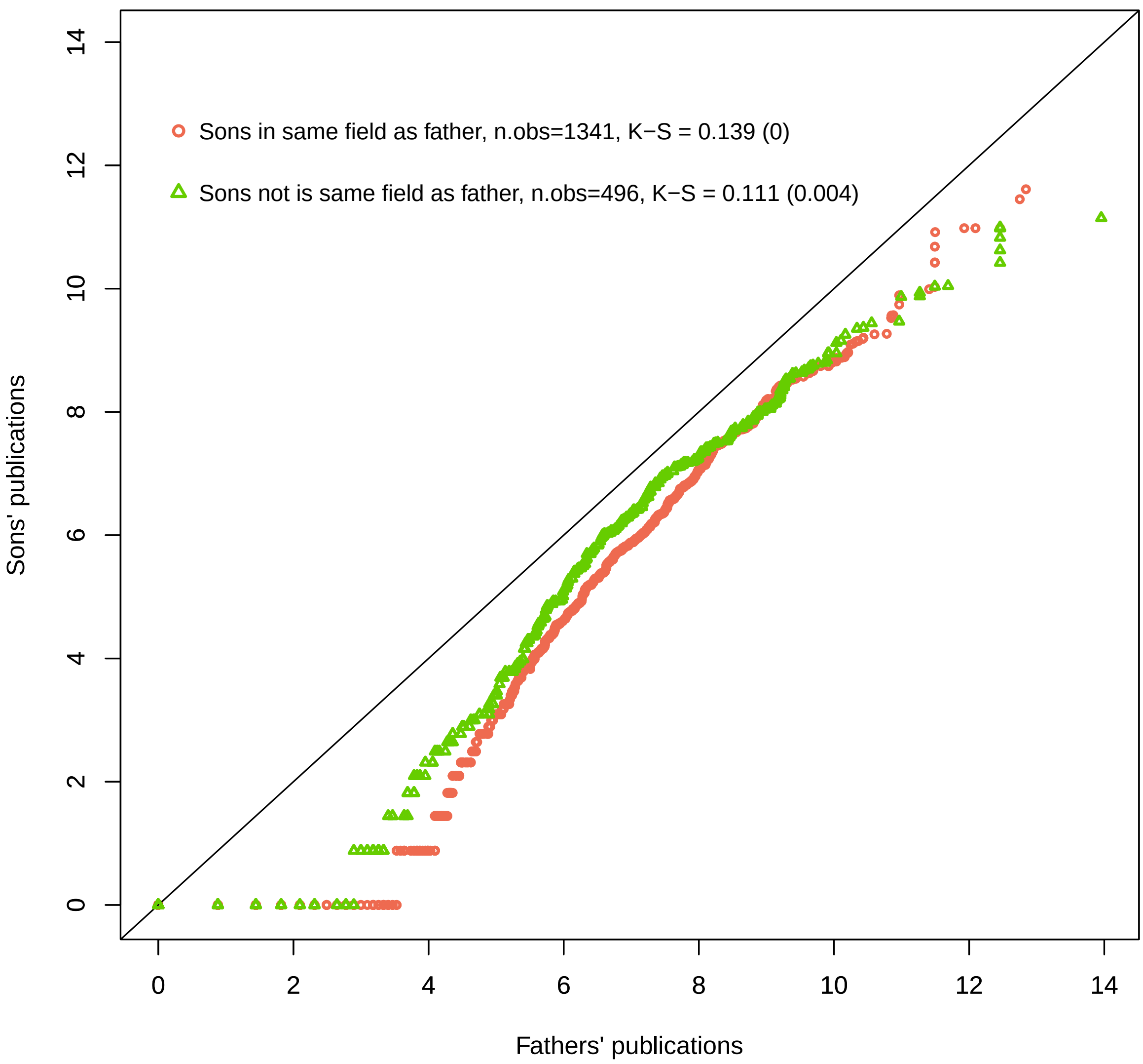


Figure E.6: Quantile-quantile plot by nomination before/after father's death

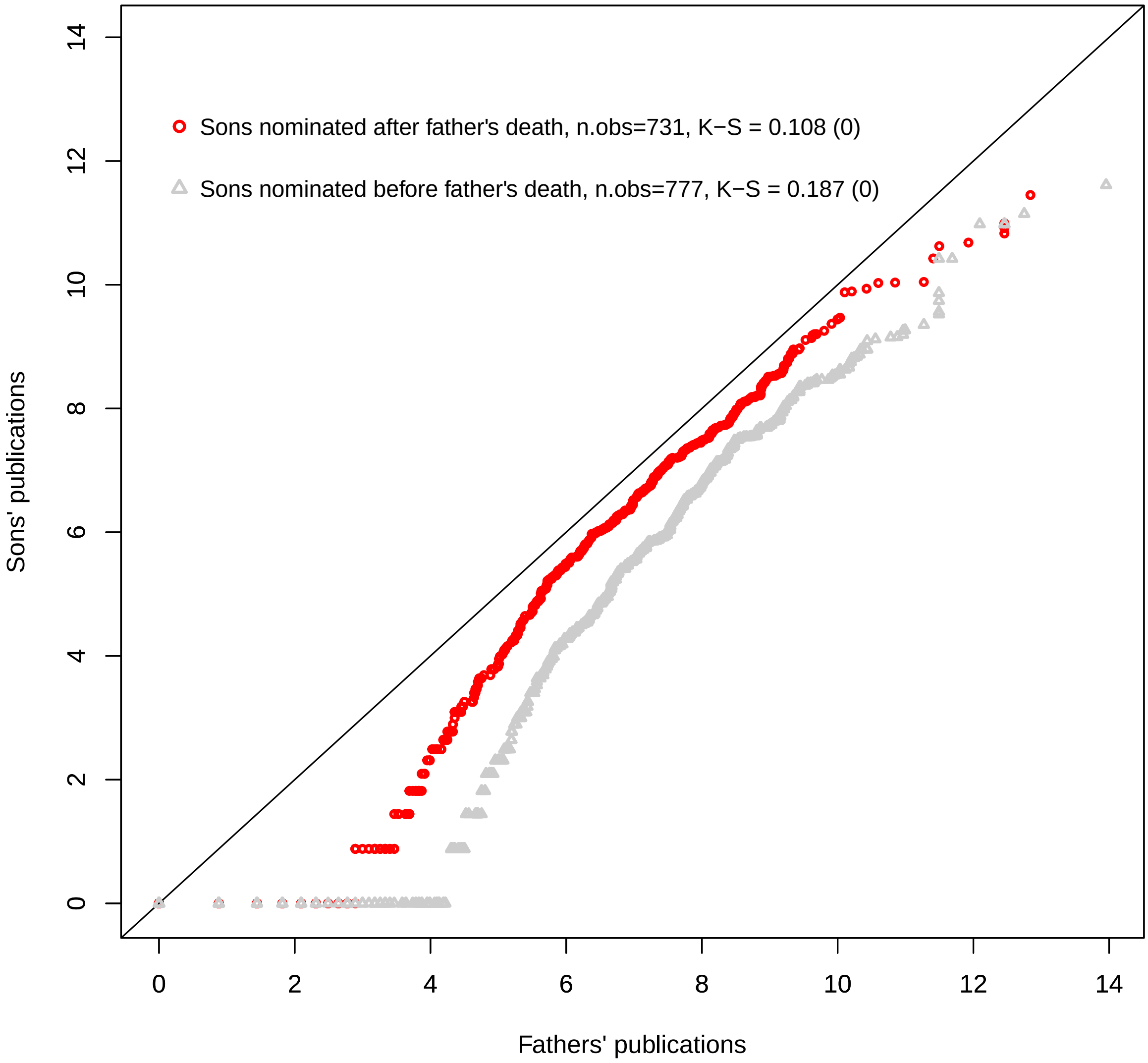
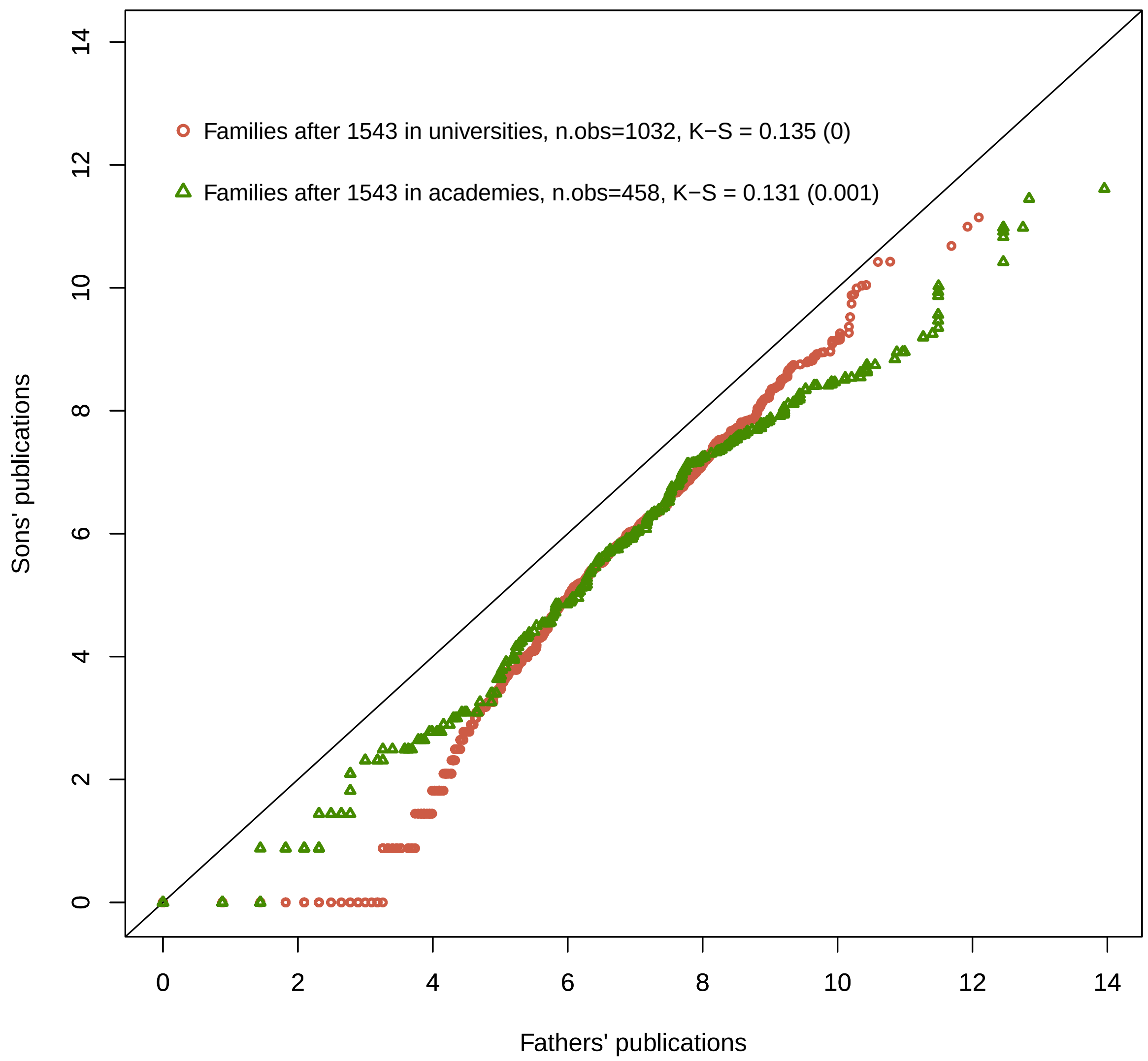


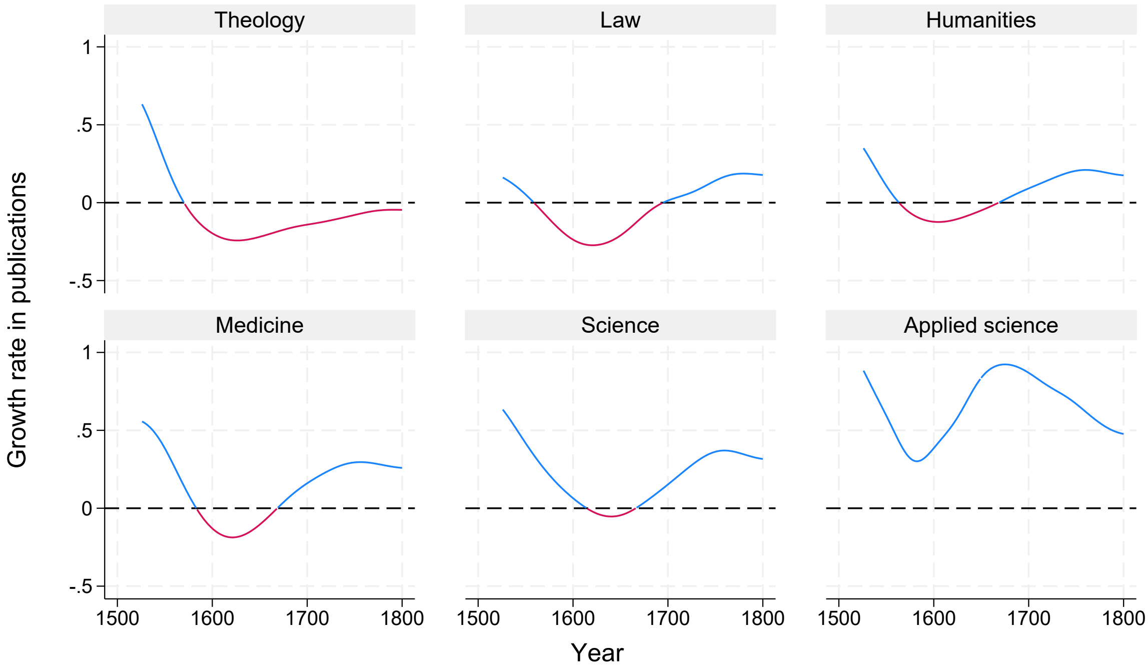
Figure E.7: Quantile-quantile plot by type of institutions



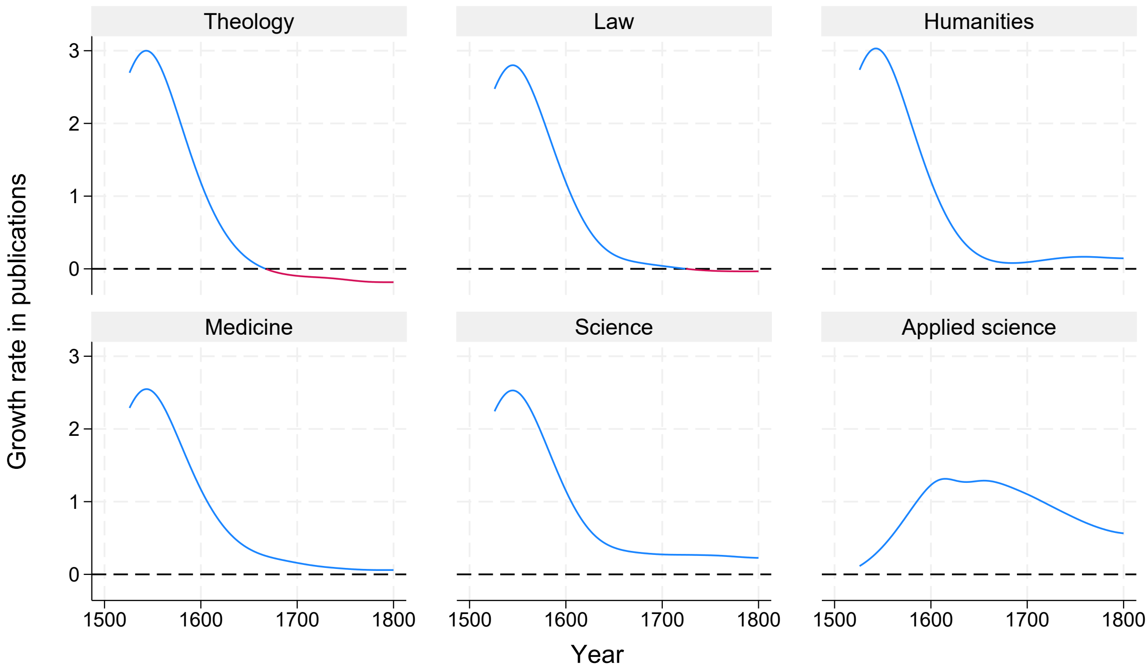
E.2 Additional figures and tables for heterogeneity analysis

Figure E.8: Field-institution growth rates in publications over time

A. Catholic institutions



B. Protestant institutions



Notes: This figure uses data from De la Croix (2021b) on 40,800,000 publications of all known scholars active between 1500–1800. It displays, for each year, the growth rate in publications over the previous 25 years by field of study and type of institution (catholic vs. protestant). Blue is for eras of rapidly changing knowledge frontier (growth rate > 0); red is for eras of stagnation (growth rate ≤ 0).

Table E.1: Heterogeneity by rapid vs. stagnant eras, under different thresholds for growth rate of publications (g)

		[1]	[2]	[3]	[4]
		Rapidly growing knowledge frontier $g > 0$	Stagnant knowledge frontier $g \leq 0$	Rapidly growing knowledge frontier $g > median$	Stagnant knowledge frontier $g \leq median$
IGE human capital	β	0.64	0.78	0.65	0.67
		(0.04)	(0.06)	(0.06)	(0.05)
Nepotism, %	γ	9.2	25.3	7.3	18.21
		(2.3)	(4.1)	(3.36)	(2.32)
Mean human capital	μ_h	3.7	-1.1	3.98	1.79
		(0.5)	(1.3)	(0.67)	(0.68)
SD human capital	σ_h	3.5	5.0	3.33	4.30
		(0.3)	(0.5)	(0.50)	(0.28)
SD publications' shock	σ_e	0.3	0.7	0.25	0.38
		(0.1)	(0.2)	(0.22)	(0.16)
Threshold publications	κ	2.1	1.8	2.24	1.89
		(0.3)	(0.3)	(0.54)	(0.18)
N		1,048	290	670	668

Notes: SE in parenthesis from 200 bootstrapped samples with replacement; degrees of overidentification: 6

E.3 Additional figures and tables for validation exercises

Table E.2: Dep. variable = 1 if scholar has Wikipedia page.

	[1]	[2]	[3]	[4]	[5]
Scholar's son (0/1)	0.074***	0.081***	0.075***	0.059***	0.062***
	(0.014)	(0.014)	(0.013)	(0.013)	(0.015)
Publications (arcsinh)	0.106***	0.106***	0.097***	0.094***	
	(0.001)	(0.001)	(0.001)	(0.001)	
Cohort FE	.	Y	Y	Y	Y
Institution FE	.	.	Y	Y	Y
Field FE	.	.	.	Y	Y
Number of library holdings FE	.	.	.	.	Y
Observations	20,500	20,394	20,354	20,354	18,616
R-squared	0.262	0.292	0.384	0.397	0.425

Notes: The sample is 20,500 scholars from institutions with complete and broad coverage who are listed in Worldcat. Cohort fixed effects are based on a scholar's earliest activity dat, and institution fixed effects on a scholar's first appointment. For scholars working in multiple fields, we consider the main field. Standard errors clustered by cohort in parenthesis; *p<.05; **p<.01; ***p<.001.

Table E.3: Eighteen moments targeted in extended estimation using outsiders

		value	s.e.	N
A. Intergenerational correlations				
1. Father-son, intensive margin	$\rho(y_t, y^s_{t+1} \mid_{y_t, y_{t+1}>0})$	0.375	0.032	982
2. Father-son with zero publications	$\Pr(y_t{=}0 \wedge y^s_{t+1}{=}0)$	0.022	0.004	1,482
3. Grandfather-grandson, intensive margin	$\rho(y_t, y^g_{t+2} \mid_{y_t, y^g_{t+2}>0})$	0.234	0.172	87
B. Distributional moments				
4. Fathers with zero publications	$\Pr(y_t{=}0)$	0.131	0.009	1,328
5. Outsiders with zero publications	$\Pr(y^o_{t+1}{=}0)$	0.030	0.002	9,118
6. Sons with zero publications	$\Pr(y^s_{t+1}{=}0)$	0.236	0.011	1,482
7. Fathers median	$Q50(y_t)$	6.050	0.117	1,328
8. Outsiders median	$Q50(y^o_{t+1})$	6.085	0.031	9,118
9. Sons median	$Q50(y^s_{t+1})$	4.927	0.140	1,482
10. Fathers 75th percentile	$Q75(y_t)$	7.714	0.082	1,328
11. Outsiders 75th percentile	$Q75(y^o_{t+1})$	7.616	0.031	9,118
12. Sons 75th percentile	$Q75(y^s_{t+1})$	6.893	0.092	1,482
13. Fathers 95th percentile	$Q95(y_t)$	9.656	0.163	1,328
14. Outsiders 95th percentile	$Q95(y^o_{t+1})$	9.616	0.044	9,118
15. Sons 95th percentile	$Q95(y^s_{t+1})$	8.689	0.081	1,482
16. Fathers mean	$E(y_t)$	5.439	0.083	1,328
17. Outsiders mean	$E(y^o_{t+1})$	5.738	0.027	9,118
18. Sons mean	$E(y^s_{t+1})$	4.310	0.080	1,482

Notes: The sample comprises (a) families of scholars and (b) outsiders in the same cohorts and institutions as at least one scholar's son. Note that this sample is different from that in Table 2 because here we take a conservative approach and restrict the sample to outsiders and families of scholars who are listed in Worldcat or Wikipedia; y: publications (inverse hyperbolic of library holdings by or about each scholar).

TABLE E.4: Fathers and sons at different universities

			Baseline sample		Different universities	
Parameters:	Interg. elasticity human capital	β	0.63	(0.04)	0.81	(0.15)
	Nepotism magnitude, %	γ	18.7	(1.74)	0.04	(0.07)
	Mean human capital distribution	μ_h	1.87	(0.47)	5.79	(0.31)
	S.D. human capital distribution	σ_h	4.22	(0.20)	2.13	(0.20)
	S.D. shock to publications	σ_e	0.39	(0.15)	2.26	(0.47)
	Threshold observable publications	κ	2.12	(0.14)	2.25	(0.58)
Data moments:	Fathers with zero publications		0.29		0.16	
	Sons with zero publications		0.38		0.10	
	Median, fathers		5.08		6.12	
	Median, sons		3.40		6.92	
	75th percentile, fathers		7.37		7.63	
	75th percentile, sons		6.41		8.10	
	95th percentile, fathers		9.43		9.47	
	95th percentile, sons		8.54		9.55	
	Mean, fathers		4.46		5.37	
	Mean, sons		3.48		6.13	
	Father-son correlation†		0.38		0.29	
	Father-son with zero publications		0.21		0.05	
	Grandfather-grandson correlation†		0.23		-0.10	
	N		1,837		507	

Notes: †on the intensive margin. SE from 200 bootstrapped samples with replacement.

F Moments used in estimation with complete and complete & broad coverage

In the main text, we examine the sensitivity of our analysis to sampling bias. That is, to the possibility that the secondary sources used to construct our dataset selectively report father-son links when fathers are famous. In short, we show that this scenario is unlikely for four reasons: First, almost two thirds of our father-son pairs are based on sources with complete coverage where we can rule out sampling bias, and the remaining third comes mostly from sources whit broad coverage where sampling bias is unlikely. Second, the coverage of the data (complete, broad, or partial) does not vary substantially over the historical periods under analysis, over centuries, across countries, across fields of study, and by the religion of the university (protestant vs. catholic). Third, it is not obvious why secondary sources would selectively record famous fathers of underachieving son more often than underachieving fathers of famous sons. Fourth, we present separate estimates restricting the data to sources with complete coverage and to sources with complete and broad coverage, and show that our results are robust. In this appendix, we provide the detailed summary statistics of the moments used in the estimations with complete coverage and with complete and broad coverage. We show that the two Facts used in our estimation strategy are robust to the accuracy of our sources, and hence, are also not a by-product of sampling bias.

Specifically, Table F.1 presents the 13 moments used for our baseline estimation (column 1), for our estimation restricted to sources with complete coverage (column 2), and for our estimation restricted to sources with complete and broad coverage (column 3). Panel A shows the moments capturing intergenerational correlations. If the historical sources used are subject to sampling bias, we would expect our baseline intergenerational correlations to be downward biased relative to those calculated using sources where we can rule out sampling bias (Solon 1989). Instead, we find that the father-son elasticity of publications is 0.375 for all families, 0.36 for families with complete coverage only, and 0.38 for families with complete and broad coverage. Similarly, the grandfather-grandson elasticity of publications is, respectively, 0.23, 0.18, and 0.23. On the extensive margin, the proportion of fathers *and* sons with zero publications is around 0.20 for all families, families with complete coverage, and families with complete and broad coverage. This suggests that the relatively high elasticity of publications across generations (Fact 1) is not a by-product of sampling bias in our sources, as it is observed also in the subsample restricted to complete coverage where we observe the universe of father-son pairs.

Panel B of Table F.1 shows the moments capturing father-son distributional differences. If fathers who were scholars of no great account are more likely to fall by the wayside than an underachieving son of a famous scholar, we would expect this sampling bias to drive the wedge between the fathers' and sons' publication distribution. In other words, we would expect our baseline distributional differences to be substantially larger than those calculated using sources where we can rule out sampling bias. Instead, we find that the distributional moments are very similar for all families, for families with complete coverage only, and for families with complete and broad coverage. For example, the

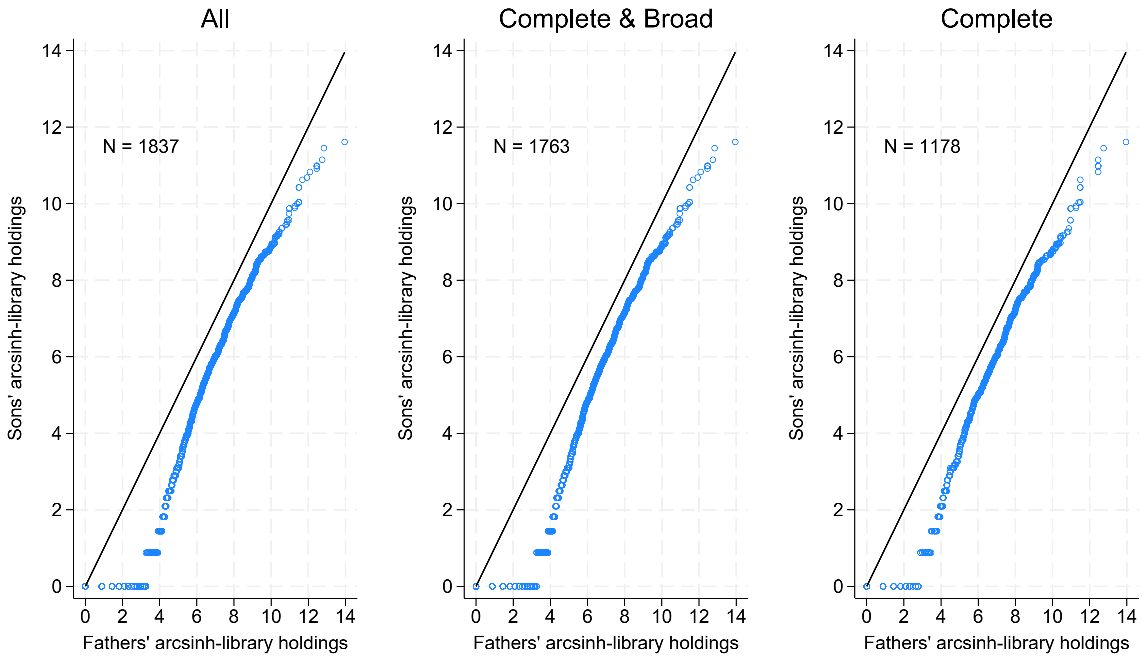
Table F.1: Moments, by coverage of data sources

		All [1]	Complete [2]	Complete and Broad [3]
A. Intergenerational correlations				
Father-son, intensive margin	$\rho(y_t, y_{t+1} \|_{y>0})$	0.375	0.36	0.38
Father-son with zero publications	$\Pr(y_t = y_{t+1} = 0)$	0.21	0.18	0.21
Grandfather-grandson, intensive margin	$\rho(y_t, y_{t+2} \|_{y>0})$	0.23	0.18	0.23
B. Father-son distributional differences				
Fathers with zero publications	$\Pr(y_t=0)$	0.29	0.26	0.29
Sons with zero publications	$\Pr(y_{t+1}=0)$	0.38	0.33	0.38
Fathers median	$Q50(y_t)$	5.08	5.43	5.11
Sons median	$Q50(y_{t+1})$	3.40	4.34	3.58
Fathers 75th percentile	$Q75(y_t)$	7.37	7.51	7.37
Sons 75th percentile	$Q75(y_{t+1})$	6.41	6.74	6.44
Fathers 95th percentile	$Q95(y_t)$	9.43	9.68	9.44
Sons 95th percentile	$Q95(y_{t+1})$	8.54	8.63	8.55
Fathers mean	$E(y_t)$	4.46	4.73	4.49
Sons mean	$E(y_{t+1})$	3.48	3.86	3.53
Father-son pairs	N	1,837	1,178	1,763

proportion of sons with zero publications is 9, 7, and 9 percentage points larger than the proportion of fathers with zero publications in, respectably, our baseline sample with all observations, observations with complete coverage, and observations with complete and broad coverage. The median, 75^{th} and 95^{th} percentile, and mean are also larger for fathers than for sons across these three groups.

To illustrate the similarity of the father-son distributional differences, Figure F.1 presents QQ plots for each group. The fathers' distribution of publications first order stochastically dominates that of sons independently of the coverage of the sources. Altogether, this shows that the distributional differences between fathers and sons (Fact 2) holds when we restrict our data to complete sources where we can rule out sampling bias. In other words, it is highly unlikely that the data is selected on father's publications and that this drives the observed wedge between the publications of fathers vs. sons.

Figure F.1: Quantile-quantile, by data coverage



G Stationarity and time trends in publications

To estimate nepotism and the intergenerational human capital elasticity, we assume that the human capital distribution is stationary among *potential scholars*. That is, among individuals with high human capital endowments who could potentially become scholars—whether they are in our dataset or not. This assumption is standard in the literature estimating intergenerational elasticities, but its importance is rarely discussed (Nybom and Stuhler 2019). In this appendix, we first discuss the use of the stationarity assumption in the literature and the sensitivity of our β-estimates to it. Next, we show that, under stationarity, our (already large) nepotism estimates are a lower-bound to the true level of nepotism. Finally, we use a dataset on all pre-modern scholars (not only fathers and sons) collected by De la Croix (2021b) to examine time trends in observed outcomes. These trends support the stationarity assumptions for both our nepotism and β-estimates. In addition, Section 5.3 of the main text relaxes the stationarity assumption. Specifically, we assume that the human capital of a father and a son who were active in a given time period is drawn from the same distribution, but we allow the human capital distribution to change across periods. This allows publications to exhibit time trends on both the extensive or intensive margin.

G.1 Stationarity in the intergenerational literature

Theory. Steady-state assumptions play a critical role for intergenerational elasticities, especially when the endowments that parents transmit to children are unobserved.[56] To see this, consider the following first-order Markov process:

$$\begin{aligned} h_{i,t+1} &= \beta h_{i,t} + u_{i,t+1}\,, \\ y_{i,t+1} &= h_{i,t+1} + \varepsilon_{i,t+1}\,, \end{aligned}$$

where $h_{i,t} \sim N(\mu_{h,t}, \sigma^2_{h,t})$ is an unobserved endowment (human capital) that parents t transmit to children $t+1$ at rate β; y is an observed outcome (publications) noisily related to h; and $u_{i,t+1}$ and $\varepsilon_{i,t+1}$ are noise terms with standard deviation σ_u and σ_e. Here $\mu_{h,t}$ and $\sigma_{h,t}$ are time dependant. In other words, we do not impose stationarity over the human capital distribution.

We can estimate β using correlations in y across multiple generations.[57] The OLS elasticity of y between parents and children (b_1) and the corresponding elasticity between grandparents and grandchildren (b_2) are:

$$\begin{aligned} b_1 &= \beta \left[\sigma^2_{h,t+1} \,/\, (\sigma^2_{h,t+1} + \sigma^2_{\varepsilon})\right]\,, \\ b_2 &= \beta^2 \left[\sigma^2_{h,t+2} \,/\, (\sigma^2_{h,t+2} + \sigma^2_{\varepsilon})\right]\,, \end{aligned}$$

Hence, the ratio b_2/b_1 identifies β under the assumption that $\sigma_{h,t+1} = \sigma_{h,t+2}$. That is, when the signal-to-noise ratio is constant across three generations. This condition is satisfied when the human capital

[56] See, e.g., Clark and Cummins (2015), Adermon, Lindahl, and Waldenström (2018).

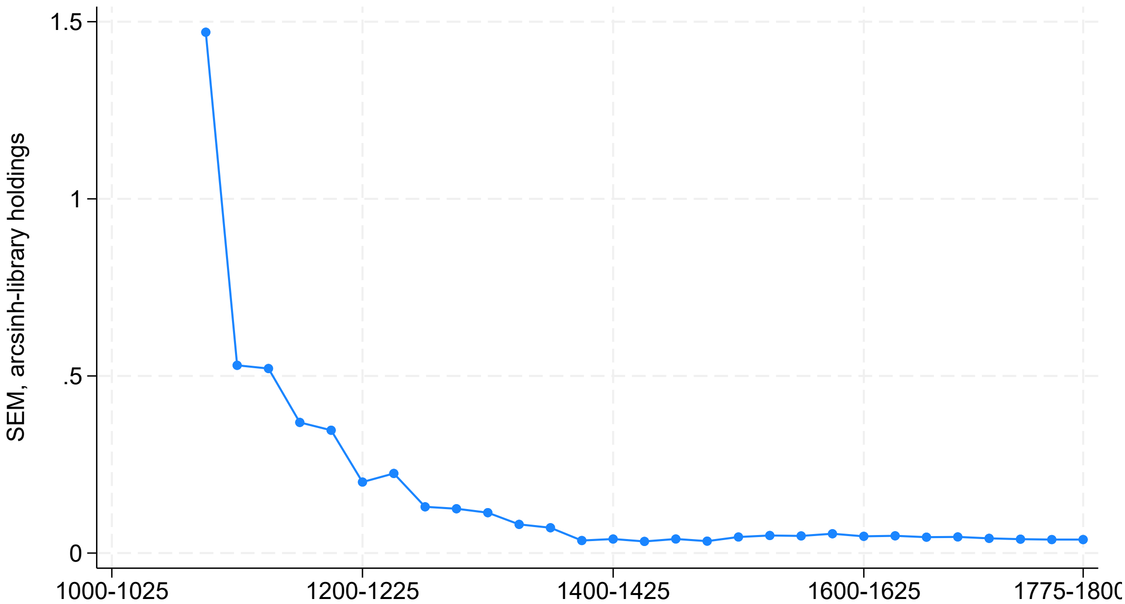
[57] Lindahl et al. (2015), Braun and Stuhler (2018), Colagrossi, d'Hombres, and Schnepf (2019).

distribution is stationary. However, this stationarity assumption is often implicit, and its importance in estimating β is rarely acknowledged in the literature (Nybom and Stuhler 2019).

Evidence. Next, we present evidence supporting the stationarity assumption $\sigma_{h,t+1}$=$\sigma_{h,t+2}$. Ideally, we would show that the standard deviation of human capital h is constant over time for the universe of *potential scholars*. Since, by construction, we do not observe h, we will show trends in the standard error of the mean for our observed human-capital proxy: publications. To evaluate a universe resembling all *potential scholars*, we use the De la Croix (2021b) data on 58,251 pre-modern scholars (not only fathers and sons) with a reference date in 1088–1800.[58]

Figure G.1 presents these trends, calculated over 25-year intervals. After 1350, the standard error of the mean of log-publications is extremely stable. This supports the assumption of a stable variance in the human capital distribution over time, that is, that $\sigma_{h,t+1} = \sigma_{h,t+2}$ is satisfied. Admittedly, the standard error is much larger before 1350. That said, in our dataset we have 36 families where both father and son's reference date is before 1350. Hence, it is unlikely that the large changes in standard error before 1350 are driving our aggregate β-estimates.

FIGURE G.1: Trend in standard error of the mean, arcsinh-library holdings



Notes: The sample is all scholars in De la Croix (2021b) with a reference date between 1088 and 1800 ($N = 58,251$). Standard error of the mean in arcsinh-library holdings calculated over 25-year periods.

[58]Note that here we do not restrict the sample to institutions with a certain data coverage or to individuals listed in WorldCat. Hence, this sample is larger than the one used in Figure 1. Reference dates are based on birth year, nomination year, or approximate activity year.

G.2 Stationarity and nepotism

Theory. Our estimates for nepotism are also sensitive to the stationarity assumption. Here we argue that, under stationarity, our nepotism estimates are lower-bound estimates. Note that we identify nepotism using two sets of moments: The first are correlations in observed outcomes across multiple generations. These allow us to uncover the true intergenerational human capital elasticity, which will be important to estimate nepotism. The second are distributional differences in observed outcomes between fathers and sons. We argue that the observed distributional differences may be the result of two forces: on the one hand, nepotism lowers the selected sons' human capital relative to that of the selected fathers, generating distributional differences in publications. That said, not all the distributional differences are directly attributed to nepotism. The second force at place is mean-reversion. If human capital strongly reverts to the mean, the sons of individuals at the top of the human-capital distribution will perform worse than their fathers even if no nepotism is at place. To gauge how much do distributional differences depend on nepotism and how much on mean-reversion, we assume stationarity in the distribution of human capital over all potential scholars. The stationarity assumption and our first set of moments (which identify the intergenertional human capital elasticity β) allow us to uncover the rate of mean-reversion. That is, how different fathers and sons are supposed to look like in the absence of nepotism. Hence, any excess distributional differences, net of reversion to the mean, can be attributed to nepotism. Formally, imposing stationarity implies that the difference in human capital between fathers and sons should follow:

$$h_{i,t+1} = \beta h_{i,t} + (1-\beta)\mu_h + \omega_{i,t+1}\,,$$

where $\omega_{i,t+1}$ is a shock distributed according to $N(0, (1-\beta^2)\sigma_h^2)$. In the absence of nepotism, this differences in human capital would be directly translated into the following differences in publications:

$$y_{i,t} = \max(\kappa, h_{i,t} + \epsilon_{i,t})$$

$$y_{i,t+1} = \max(\kappa, \beta h_{i,t} + (1-\beta)\mu_h + \omega_{i,t+1} + \epsilon_{i,t+1})$$

If the father-son difference in publications is larger than suggested by the previous equations, then an additional force must be in place. A force selecting fathers and sons differently, such that the later can become scholars with lower human capital endowments. In our setting, this additional force is nepotism.

How would our nepotism estimate change in a non-stationary environment? In our setting, it is reasonable to assume that if the human capital distribution is non-stationary, then it *improves* over time. Under this scenario we would expect more sons with higher human capital than their fathers than under stationarity. This implies that, in the absence of nepotism, we would expect virtually no distributional differences in publications between fathers and sons. In extreme scenarios, we would even expect the sons publication's distribution to dominate that of their fathers. Hence, we would need a larger nepotism parameter to reconcile the large observed father-son distributional differences in publications with the small expected differences. In other words, under stationarity, a share of the father-son distributional differences is attributed to nepotism, and another to mean-reversion. In a non-stationary environment, mean-reversion would explain a lesser share of the father-son distributional differences, and hence, our nepotism estimate would have to be larger. Therefore, under stationarity, our nepotism estimates are conservative, lower-bound estimates.

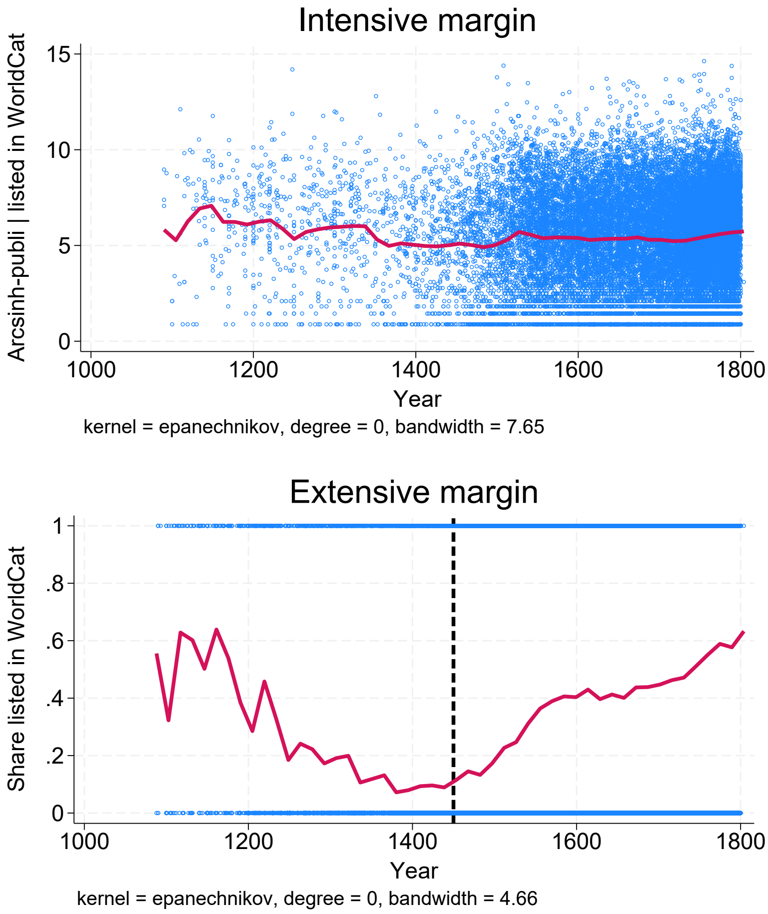
Evidence. The fact that our (already large) nepotism estimate is a conservative estimate is reassuring. Here we present additional evidence supporting the stationarity assumption, and hence, that our nepotism estimate is not severely downward biased. Ideally, we would show that the mean of the human capital distribution, μ_h, is constant over time for all *potential scholars*. Since we do not observe h, we will focus on trends in our observed human-capital proxy: publications. To evaluate a universe resembling all *potential scholars*, we use the dataset collected by De la Croix (2021b) on 58,251 pre-industrial scholars.

Figure G.2 shows the trend in publications on the intensive margin (top panel). That is, it shows the inverse hyperbolic sine of the number of library holdings, conditional on having at least one publication listed in WorldCat. To calculate trends, we use a kernel-weighted local polynomial regression of publications on a scholar's reference date. The figure shows no trend in the intensive margin of publications, supporting our stationarity assumption.[59]

The bottom panel shows trends on the extensive margin of publications: that is, whether a scholar is listed in WorldCat. The figure shows a U-shaped pattern. Before 1350, the extensive margin of publications is high because of a selection effect: top scholars are more likely to be observed. That said, we have a limited number of observations from this period (36), and hence, it is unlikely that this has a large impact on our aggregate results. Around 1450, when the printing press was introduced, there is a structural break in the extensive margin of publications. There are two reasons to believe that this structural break does not reflect a change in the human capital distribution but a change in the technology for printing and preserving books: The first reason is that the printing press massively increased the diffusion and preservation of scholar's books (Dittmar 2019). This alone could explain the observed trend without resort to changes in the human capital distribution. Formally, this trend is related to our parameter κ, the measurement error on the extensive margin of publications, and not to μ_h, the mean of the human capital distribution among potential scholars. This is supported by our higher estimates for κ for the earlier period (1088–1543) (see Section 5.3). The second reason why it is unlikely that this trend reflects changes in the human capital distribution is because such a change would affect the trends in *both* the extensive and the intensive margin of publications. We only observe a trend in the former, suggesting that the changes are related to improvements in the printing and book-preservation technology. Finally, this increasing trend implies that, around 1450, some sons benefited from the existence of the printing press to publish and preserve their work. In contrast, we are more likely to observe zero-publications for their fathers, whose work was not printed and may have been lost. Correcting for this bias would increase the father-son distributional differences. Hence, it would lead to larger nepotism estimates.

[59] The fluctuations before 1350 are driven by a smaller sample in the earlier periods.

Figure G.2: Trend in log-publications, intensive and extensive margin



Notes: The sample is all 58,251 scholars in De la Croix (2021b). Trends calculated with a kernel-weighted local polynomial regression. The dashed line is for the introduction of the printing press.

In sum, the De la Croix (2021b) dataset comprising 58,251 scholars shows no trend on the intensive margin of publications. This supports our stationarity assumption for the human capital distribution. On the extensive margin, we find evidence of a structural break around 1450. That said, this is related to the changes brought about by the printing press in terms of book diffusion and preservation, rather than with a change in the human capital distribution.

H Robustness to distributional assumptions

The intergenerational transmission of wealth is often modeled assuming a normal distribution for the initial distribution of wealth $h_{i,t}$ and the idiosyncratic shock $u_{i,t+1}$. How do these distributional assumptions affect our results? Could the large nepotism estimate be a by-product of these distributional assumptions? Here we consider an alternative to normality: drawing shocks from fat-tailed distributions. This distributions give a higher likelihood to the emergence of geniuses, which is appealing in our setting with individuals at the very top of the talent distribution.

Before re-estimating our results, we need to consider two issues: the first concerns the targeted moments, the second the set of feasible fat-tailed distributions. Some of the commonly targeted moments when shocks are normal are not defined when shocks are fat tailed. This is the case of Pearson correlation and of the mean. Hence, if we want to use shocks from fat tailed distributions, we need to target an alternative set of moments ($V_S(p)$). Specifically, we replace the Pearson correlation for the Spearman rank correlation—which remains well-defined with any distribution—and we drop the two means from the targeted moments. We thus have four overidentifying restrictions instead of six. To show that these changes are not crucial for our results, we first conduct our baseline estimation under this new set of moments to define a new benchmark.

Table H.1 presents the results of the new benchmark and compares them to the estimation in the main text ($V(p)$). The Spearman correlations ρ_S are identical to their Pearson counterparts ρ, and all estimates are similar under the two different objectives. In detail, our two main estimates—the intergenerational human capital elasticity, β, and the magnitude of nepotism, γ—are not significantly different when we target the moments in $V(p)$ or in $V_S(p)$. Overall, the table shows that targeting this alternative set of moments does not alter our baseline results, and hence, that we can use them to check the robustness to using fat tailed shocks.

The second issue is the set of feasible fat tailed distributions. We need distributions with closed-form expressions for the density to verify that their shape is preserved (up to scale and shift) under addition. To see why, note that the sum-stable property of the normal distribution implies that its shape remains the same across all generations once transformed by Equation (2), that is, $h_{i,t+1} = \beta h_{i,t} + u_{i,t+1}$. Only its parameters change. This stability property is not a theoretical curiosity. Without it, we lack of coherence in modeling, as the initial distribution of human capital could not be rationalized by the model, its shape having vanished after one period. There are two families of fat-tailed distributions where one can verify that the sum-stable property is satisfied as in the normal distribution: The Cauchy and Levy distribution (Nolan 2003). Here we use the Cauchy distribution, which is fat tailed but, unlike the Levy distribution, is defined over $\mathbb{R}$.

After discussing these two issues, we can now analyze the effect of using fat tailed distributions on our results. Specifically, the theoretical model of human capital transmission with nepotism where shocks are Cauchy is as follows. A potential scholar in generation t of family i is endowed with an unobserved human capital $h_{i,t}$. Human capital follows a Cauchy distribution with location x_h and

TABLE H.1: Benchmark estimation under different set of moments

Objective:		$V(p)$	$V_S(p)$
Panel A. Moments:			
Father-son correlations:			
Pearson, intensive margin	$\rho(y_t, y_{t+1})$	0.375	.
Spearman, intensive margin	$\rho_S(y_t, y_{t+1})$	.	0.375
Grandfather-grandson correlations:			
Pearson, intensive margin	$\rho(y_t, y_{t+2})$	0.234	.
Spearman, intensive margin	$\rho_S(y_t, y_{t+2})$	.	0.265
Distribution means:			
Father mean log-publications	$E(y_t)$	YES	.
Son mean log-publications	$E(y_{t+1})$	YES	.
Remaining distributional moments:		YES	YES
Panel B. Identified parameters:			
Intergen. elasticity of human capital	β	0.63	0.70
		(0.04)	(0.04)
Nepotism, %	γ	18.7	19.4
		(1.7)	(1.7)
Mean of human capital distribution	μ_h	1.87	1.25
		(0.47)	(0.65)
SD of human capital distribution	σ_h	4.22	4.43
		(0.20)	(0.24)
SD of shock to publications	σ_e	0.39	0.55
		(0.15)	(0.19)
Threshold of observable publications	κ	2.12	2.00
		(0.14)	(0.13)
Degrees of overidentification		6	4

Notes: τ normalized to 0. S.E. between parentheses obtained by estimating parameters on 200 bootstrapped samples with replacement

scale parameter s_h:

$$h_{i,t} \sim \text{Cauchy}(x_h, s_h)$$

The offspring of this generation, indexed $t+1$, inherit the unobserved human capital endowment under the first-order Markov process in Equation (2). The noise term $u_{i,t+1}$ is an i.i.d. ability shock affecting generation $t+1$, and has now a Cauchy distribution, Cauchy(x_u, s_u).

Human capital is stationary among potential scholars. That is, we assume that, conditional on the model's parameters being constant, the human capital of generations t and $t+1$ is drawn from the same distribution. Formally, $h_{i,t} \sim \text{Cauchy}(x_h, s_h)$ and $h_{i,t+1} = \beta h_{i,t} + u_{i,t+1}$ implies $h_{i,t+1} \sim$

Cauchy$(\beta x_h + x_u, |\beta| s_h + s_u)$.[60] Stationarity leads to the following two restrictions:

$$\begin{aligned} x_u &= (1-\beta) x_h \\ s_u &= (1-|\beta|) s_h \,. \end{aligned}$$

Equations (4)-(5) give the publications for fathers, $y_{i,t}$ and sons, $y_{i,t+1}$ in the set of scholar families $\mathbb{P}$. The shocks affecting how human capital translates into publication now follow a fat-tailed distribution: $\epsilon_{i,t}$, $\epsilon_{i,t+1} \sim$ Cauchy$(0, s_e)$.

Finally, the magnitude of nepotism, γ, is defined analogously to our baseline model. Formally,

$$\gamma = F_h^{cauchy}(\tau \mid h_{i,t+1} \geq \tau - \nu)\,,$$

where $F^{cauchy}(x;\ x_h, s_h)$ is the (stationary) Cauchy cumulative distribution of human capital with location x_h and scale parameter s_h, and

$$F^{cauchy}(x \mid h_{i,t+1} \geq \tau - \nu) = Prob\left(h_{i,t+1} \leq x \mid h_{i,t+1} \geq \tau - \nu\right)$$

is the corresponding truncated cumulative distribution of sons' human capital in the set of observed scholar families $\mathbb{P}$.

There are three variants to the model of the main text (Model I). One with Cauchy distribution for all shocks (Model II), another with Cauchy distribution for shocks to human capital and Normal distribution for shocks to publications (Model III), and another with Normal distribution for shocks to human capital and Cauchy distribution for shocks to publications (Model IV). We evaluate Models II and III, as they lead to non-normal human capital distribution.

Table H.2 shows the results. The value of the objective to be minimized is an order of magnitude higher when human capital shocks are modeled with a Cauchy (794 vs. 6,613 and 4,940, respectively). In other words, the data cannot be fitted well to a distribution with fat tails. For example, the 50th percentile for the son's publication distribution is 3.4 arcsinh-library holdings in the data (Table D.1), 3.1 in the simulation with the Normal distribution, and 1.2 (Model II) and 0.9 (Model III) in the simulations with the Cauchy. Finally, the nepotism estimates are robust to assuming Cauchy shocks, although the intergenerational elasticity β is not.

In sum, using fat tailed distributions for human capital shocks seems, *a priori*, an appealing alternative to the usual normality assumption. However, fat tailed distributions do not fit the data, which is very normally distributed after all.

[60] Because if $X \sim$ Cauchy(x_0, s_0) we have $kX + \ell \sim$ Cauchy$(kx_0 + \ell, |k| s_0)$. And if $Y \sim$ Cauchy(x_1, s_1), $X + Y \sim$ Cauchy$(x_0 + x_1, s_0 + s_1)$.

Table H.2: Identified parameters under different model assumptions

Parameter		Model I	Model II	Model III
Intergen. human capital elasticity	β	0.703	0.298	0.437
Nepotism, %	γ	19.35	17.57	25.58
Std. dev. of shock to publications	σ_e	0.553	.	2.259
Scale of shock to publications	s_e	.	0.100	.
Threshold of observable publications	κ	2.002	0.820	0.002
Mean of human capital distrib.	μ_h	1.248	.	.
Location of human capital distrib.	x_h	.	0.847	0.040
Std. dev. of human capital distrib.	σ_h	4.430	.	.
Scale of human capital distrib.	s_h	.	1.094	0.920
Value of objective $V(p)$		793.8	6,613.2	4,940.1

Notes: τ normalized to 0; degrees of overidentification: 4

I Linearity of beta

So far, we assumed that parents with high and low human capital transmit their endowments at the same rate β. This linearity assumption would be violated, e.g., if successful fathers with many publications could spend less time with their children, reducing their human capital transmission systematically. Here we show empirically that, in our setting, the linearity assumption is satisfied.

To do so, we examine the parent-child elasticity of publications in the intensive margin. A large literature derives estimates of β directly from such parent-child elasticities. Here we compare elasticities obtained using OLS vs. estimated non-parametrically. The latter allow elasticities to differ in families with different levels of publications, and hence, with different human capital endowments.

Formally, our OLS elasticity estimates, b^{ols}, are:

$$y_{i,t+1} = c + b^{ols}\, y_{i,t} + e_{i,t+1}\,, \tag{17}$$

where $y_{i,t+1}$ and $y_{i,t}$ are the inverse hyperbolic sine of library holdings for, respectively, sons and fathers with at least one publication in WorldCat. That is, b^{ols} is the publications' elasticity in the intensive margin. In our setting, we can interpret arcsinh-arcsinh specifications as elasticities because the number of library holdings (in levels) of fathers and sons take on large values, with means well-above the threshold proposed by Bellemare and Wichman (2020).[61] This specification assumes that b^{ols} is linear.

Conversely, non-parametric estimates for the publication's elasticity, b^{np}, are:

$$y_{i,t+1} = g(y_{i,t}) + e_{i,t+1}\,, \tag{18}$$

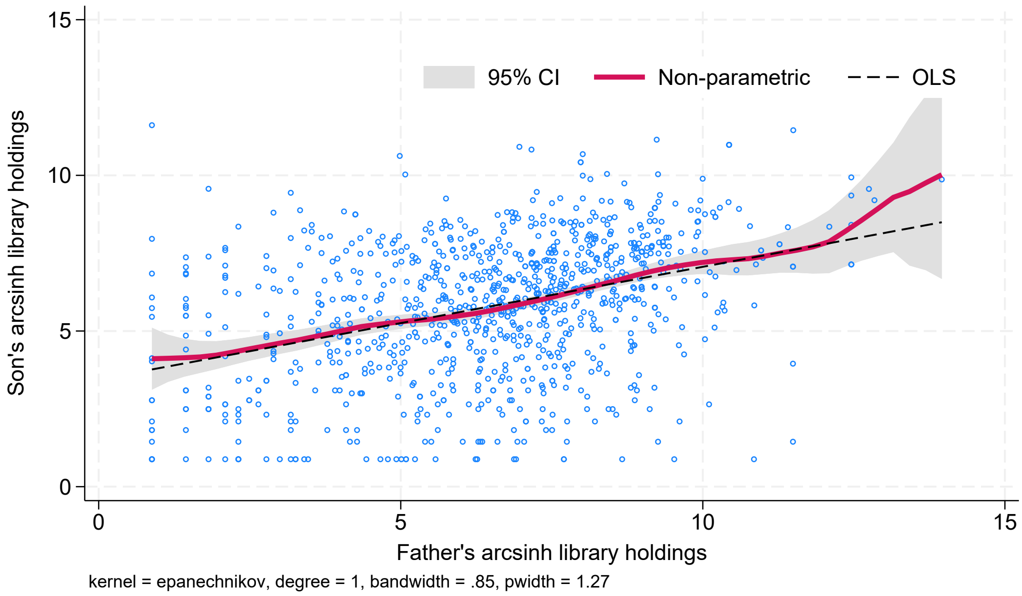
where $g(.)$ does not follow any given parametric form but is derived from the data. Hence, this specification accounts for any polynomial form for $g(.)$, i.e., $g(y_{i,t}) = c + \sum_j b_j^{np}\, y_{i,t}^j$ for all $j \in \mathbb{Z}$. This allows elasticities to be different across families with different levels of publications. The non-parametric elasticity b^{np} corresponds to the marginal effect of $y_{i,t}$, obtained as averages of the derivatives.

Figure I.1 compares OLS and non-parametric elasticity estimates. It shows a scattergram of fathers' (y-axis) and sons' (x-axis) publications, OLS fitted values from eq. (17) (dashed line), and non-parametric fitted values and 95% confidence intervals from eq. (18) (thick red line and grey area). Specifically, the latter plots the smoothed values of a kernel-weighted local polynomial regression of $y_{i,t+1}$ on $y_{i,t}$. To further capture non-linearities, we choose a polynomial of degree one for the smoothing. Finally, note that in this figure the OLS and non-parametric elasticities correspond to the slopes of the plotted lines.

Overall, the figure shows that there is no statistically significant difference between OLS and non-parametric estimates. This is true at all levels of father's publications. For fathers with fewer than 12 arcsinh-publications (more than $\leq 80,000$ in levels), the fitted OLS and non-parametric values are identical. In turn, the parent-child elasticity in publications (i.e., the slope of the lines) is tightly identified around 0.36 for both estimates. At the very top of the distribution, we also do not observe

[61] See footnote 15 in the main text for details.

Figure I.1: Parent-child publications' elasticity (intensive margin), OLS and kernel-weighted local polynomial regression



Notes: The sample are fathers and sons with at least one recorded publication.

significant differences between OLS and non-parametric estimates, although the confidence intervals are wider due to fewer number of observations.

Table I.1 confirms this pattern for different historical periods. It shows the OLS (eq. 17) and non-parametric (eq. 18) elasticities for all families (row 1); for families before the Scientific Revolution (row 2); during the Scientific Revolution (rows 3 and 4); and during the Enlightenment (row 5). For all periods, the OLS and non-parametric estimates are almost identical.

Table I.1: Parent-child publications' elasticity (intensive margin), OLS and Non-parametric estimates

	OLS [1]		Non-parametric [2]		
All	0.36***	(0.03)	0.36***	(0.03)	N=982
Pre-Scientific Revolution (1088–1543)	0.06	(0.11)	0.06	(0.10)	N=83
Scientific Revolution (1543–1632)	0.36***	(0.07)	0.38***	(0.07)	N=196
Scientific Revolution (1632–1687)	0.32***	(0.05)	0.30***	(0.07)	N=271
Enlightenment (1688–1800)	0.44***	(0.04)	0.44***	(0.04)	N=432

Note: The sample are fathers and sons with at least one publication; SE in parenthesis; Non-parametric SE obtained with 1,000 bootstrapped replications; ***p<.01,** p<.05,* p<.1

Altogether, we find identical elasticities using OLS and non-parametric techniques. This suggests that the parent-child elasticity of publications is linear. In other words, it is identical for parents with high and low publications. This lends credence to the assumption that human capital endowments are transmitted at the same rate β by parents with high and low human capital endowments.

J Heterogeneity in publication thresholds

The parameter κ is the minimum number of publications needed to observe a scholar's work in modern libraries. So far, we assumed that κ is the same for fathers and sons. An alternative is to assume that the threshold is lower for sons: the work of a famous scholar's son may capture the attention of publishers and librarians more easily—even if it is of lower quality.

Here we examine the robustness of our results to this alternative assumption. We define the sons' threshold as κ_s, possibly lower than the father's threshold κ_f and estimate the corresponding model in Table J.1. We find that the constraint $\kappa_s \leq \kappa_f$ is saturated: our estimated κ_s and κ_f are almost identical. Hence, our estimation results are unchanged: we find very similar intergenerational human capital transmission β (0.634 vs. 0.626) and percentage of nepotic sons γ (18.74 vs. 19.04%).

TABLE J.1: Results under alternative assumptions for κ

Parameter		benchmark	different κ's
Intergenerational elasticity of human capital	β	0.634	0.626
Nepotism, %	γ	18.74	19.04
Mean of human capital distribution	μ_h	1.865	1.846
Std. deviation of human capital distribution	σ_h	4.219	4.251
Std. deviation of shock to publications	σ_e	0.393	0.415
Threshold of observable publications - all	κ	2.121	.
Threshold of observable publications - fathers	κ_f	.	2.118
Threshold of observable publications - sons	κ_s	.	2.118

Notes: τ normalized to 0; degrees of overidentification: 6

K Alternative measures of publications

In the main text, we defined publications as the number of library holdings in modern libraries by or about each scholar. This includes all imprints/editions/copies of books, volumes, issues, or documents which a scholar wrote that are available in WorldCat libraries today. It also includes library holdings about his work written by a different author. We chose this measure of publications as our baseline measure because it captures two important characteristics of a scholar's research: its size and its relevance for today in a manner akin to modern citation data. Although we believe both characteristics to be important, it is interesting to examine the robustness of our results to measuring only the size of a scholar's work. To do so, here we consider two alternative measure of publications: The first measure is the number of library holdings written by each scholar, i.e., omitting library holdings about his work written by a different author. The second measure is the number of unique works by or about each scholar instead of the total number of library holdings.

Table K.1 provides the empirical moments for our baseline measure (the arcsinh of library holdings by and about each author) in column [1], and for our alternative measures (the arcsinh of library holdings by each author and the arcsinh of unique works) in columns [2] and [3]. Panel A shows that the inter-generational correlations are very similar on the intensive margin across these three measures. On the extensive margin, the correlation is equal by construction. Overall, this indicates that the high inter-generational elasticity (Fact 1) is visible on the library holdings by and about each author, on the library holdings by each author, and on the number of unique works.

Panel B shows the moments characterizing father-son distributional differences. The levels are different by construction: the library holdings written by each author and, especially, the number of unique works are equal or smaller that the total number of library holdings written by and about each author. That said, the properties of the distribution and the father-son distributional differences (Fact 2) are robust to using different publications' measures. To see this, note that the father's median, mean, 75th and 95th quantile are substantially higher than their sons' in the three measures. To further show that the properties of the fathers' and sons' distribution are similar, Table K.2 shows quantile ratios. The median/Q75 ratio, the median/Q95 ratio, and the median/mean ratio are similar for fathers and sons independently of whether one uses library holdings by and about each author, library holdings by each author, or unique works as the measure of research output.

Finally, Table K.3 re-estimates our model targeting the moments defined with library holdings by and about each author (column 1), with library holdings by each author (column 2), and with unique works (column 3). Our estimates for the intergenerational elasticity of human capital and for nepotism are remarkably similar across specifications. Specifically, excluding publications about a scholar's work written by a different author leads to a β-estimate of 0.62, very similar to our baseline estimate that includes them (0.63). The nepotism estimate, γ, is idential across measures, suggesting that 18.7% of scholars' sons were nepotic. Similarly, using the number of unique works, we find a β of 0.61 and a nepotism estimate of 18.8.

Altogether, the evidence presented in this appendix suggests that our main estimates for the intergenerational elasticity of human capital and for nepotism are robust to how we measure a scholar's research output. Specifically, our results are not a byproduct of whether our definition of publications includes work written by a different author or is based on library holdings instead of unique works.

Table K.1: Targeted moments with alternative measures of publications

	[1]	[2]	[3]
	Library holdings by & about author	Library holdings by author	Unique works
A. Intergenerational correlations			
Father-son, int. margin	0.375	0.366	0.357
Father-son with zero pubs.	0.211	0.211	0.211
Grandfather-grandson, int. margin	0.234	0.212	0.230
B. Father-son distributional differences			
Fathers with zero pubs.	0.288	0.290	0.290
Sons with zero pubs.	0.384	0.384	0.384
Fathers median	5.075	4.238	3.801
Sons median	3.402	2.615	2.440
Fathers Q75	7.370	6.300	5.762
Sons Q75	6.413	5.415	4.950
Fathers Q95	9.425	8.213	7.568
Sons Q95	8.537	7.306	6.748
Fathers mean	4.456	3.752	3.441
Sons mean	3.477	2.893	2.664

Table K.2: Comparison of distributions

	[1]	[2]	[3]	
		Library holdings by & about author	Library holdings by author	Unique works
Q50/Q75	Fathers	0.69	0.67	0.66
Q50/Q75	Sons	0.53	0.48	0.49
Q50/Q95	Fathers	0.54	0.52	0.50
Q50/Q95	Sons	0.40	0.36	0.36
Q50/mean	Fathers	1.14	1.13	1.10
Q50/mean	Sons	0.98	0.90	0.92

Table K.3: Identified parameters with alternative measures of publications

		[1]	[2]	[3]
		Library holdings by & about author	Library holdings by author	Unique works
IGE human capital	β	0.63	0.62	0.61
Nepotism, %	γ	18.7	18.7	18.8
Mean human capital	μ_h	1.87	1.65	1.60
SD human capital	σ_h	4.22	3.56	3.37
SD publications's shock	σ_e	0.39	0.29	0.20
Threshold publications	κ	2.12	1.82	1.74

Notes: τ normalized to 0; degrees of overidentification: 6

L Longevity

Longevity is an important factor for the number of publications of scholars. In our setting, scholars' fathers may have lived longer than scholars' sons. The reason is that, by construction, the former are recorded in our data conditional on living until they have a child, while the latter are recorded even if they die early after their nomination. In our sample of scholars with known birth and death year, the mean longevity is 67.65 (s.e 0.32) for fathers and 61.67 (s.e. 0.44) for sons. Here we show that this differential longevity does not affect our results.

To do so, we adjust the son's distributional moments accounting for the 5.98 year father-son gap in longevity. We do this in two steps. First, we calculate the marginal effect of living one additional year on the proportion of sons with zero publications and on the mean, median, 75th, and 95th percentile of the sons' log-publications. Second, we adjust the baseline distributional moments for sons by adding the marginal effects above times 5.98; the differential longevity between fathers and sons. That is, we calculate what the sons' distributional moments would look like if they had, on average, lived as long as fathers of scholars.

Formally, we first estimate the following equation by OLS:

$$y_{i,t+1} = \alpha + \delta(mean) \cdot L_{i,t+1} + e_{0\ i,t+1}\,, \tag{19}$$

where i indicates families of scholars and $t+1$ that the observation corresponds to a scholar's son; $y_{i,t+1}$, is the inverse hyperbolic sine of the number of library holdings; and $L_{i,t+1}$ is the son's longevity, in years. Hence, $\delta(mean)$ captures the marginal effect of one additional year of life on the sons' arcsinh-publications. Estimating $\delta(mean)$ by OLS allows to understand this relationship for the *average* son.

We calculate analogously $\delta(zeros)$, the marginal effect on the proportion of sons with zero publications. That is, we estimate 19 by OLS where the dependent variable, $y_{i,t+1}$, is an indicator equal to one if a son had zero publications.

Next, we run a simultaneous-quantile regression to estimate the relation between longevity and publications at other distributional moments than the mean. Formally, we estimate:

$$Q_{y_{i,t+1}}(q|L_{i,t+1}) = \alpha_i + \delta(q) \cdot L_{i,t+1}\,, \tag{20}$$

where q is the quantile of interest; $\delta(Q50)$, $\delta(Q75)$, and $\delta(Q95)$ are the marginal effect of living one additional year on the median, 75th and 95th percentile of the sons' publication distribution; are all coefficients are estimated simultaneously

Table L.1 presents the corresponding estimates. Column [1] confirms that longevity is important for publications. One additional year of life is associated with an increase of 0.023 arcsinh-publications. Hence, if sons lived as long as fathers, their mean arcsinh-publications would increase, on average, by $5.98 \times 0.023 = 0.138$. Column [2] shows the corresponding marginal effect for the proportion of sons with zero publications. Note that this marginal effect is small and implies that, if sons lived as long as fathers, their probability to die without ever publishing would be reduced by $5.98 \times 0.0015 = 0.00897$, or 0.897 percentage points of a sample mean of 38 percent. This suggests that the high proportion of sons with zero publications is not a by-product of sons dying early after their nomination. This is important as our identification of nepotism partially hinges on father-son distributional differences at the bottom of the distribution. Finally, columns [3] to [5] show that one additional year of life is associated with an increase of 0.03 arcsinh-publications at the median and 75th percentile, and with an increase of 0.014 arcsinh-publications at the 95th percentile of the sons' publications distribution. Hence, if sons lived as long as fathers on average, their arcsinh-publications would increase by $5.8 \times 0.03 = 0.179$ at the median and 75th percentile; and by $5.8 \times 0.014 = 0.084$ at the 95th percentile.

Table L.1: The effect of Longevity on son's distributional moments

	[1]	[2]	[3]	[4]	[5]
	OLS	OLS	simultaneous-quantile regression		
	$\partial(mean)$	$\partial(zeros)$	$\partial(Q50)$	$\partial(Q75)$	$\partial(Q95)$
Longevity (years)	0.023***	-0.0015**	0.031***	0.030***	0.014**
	(0.005)	(0.0007)	(0.007)	(0.005)	(0.007)
Observations	1,329	1,329	1,329	1,329	1,329

Note: The sample is scholars' sons with known birth and death year;$^{***}p<.01$,$^{**}p<.05$,$^{*}p<.1$

Finally, Table L.2 shows the adjusted sons' distributional moments. Column [1] shows the baseline moments and column [2] the adjusted moments if scholars' sons had lived as long as scholars' fathers. The adjusted moments are $m + \partial(m) \times 5.98$; where m is the baseline value, $\partial(m)$ the marginal effect of longevity at moment m, and 5.98 the father-son differential longevity.

The baseline and adjusted moments are very similar. The proportion of sons with zero publications (0.38) is almost not altered by adjusting for the fathers-sons longevity differential (0.37). The mean, median, 75th and 95th percentile of the sons' log-publications are larger when we impute the same longevity to sons and fathers. For example, if sons lived as long as fathers on average, their mean arcsinh-publications would have been 3.61 instead of 3.48—which corresponds to an increase of 0.14 arcsinh-publications. That said, the adjusted distributional moments are consistent with *Fact 2*. After accounting for longevity differentials, the publication's distribution of fathers first order stochastically dominates that of sons. On the bottom of the distribution, 30% of fathers and 37% of sons had zero publications, even after accounting for longevity differentials. These distributional differences are also visible at the mean, median, 75th and 95th percentile. For example, the average father had 4.5 arcsinh-publications (45 in levels), more than twice as much as the average son (18.5 in levels) even after accounting for longevity differentials. The father-son differences at the median are reduced by 0.18 arcsinh-publications after adjusting for longevity, but the median father still published substantially more more than the median son. Importantly, this implies that, after adjusting for longevity differentials, the father-son distributional differences are relatively larger at the bottom of the distribution than at the mean or at the median.

Altogether, the evidence suggests that longevity affects publications, but that father-son longevity differences do not explain away father-son distributional differences (*Fact 2*). This shows that our estimates for nepotism and the intergenerational human capital elasticity are not driven by differences in longevity.

Table L.2: Distributional moments adjusted for longevity differentials

		Baseline [1]	Adjusted [2]	Difference [1]-[2]
Fathers with zero pubs.	$\Pr(y_t=0)$	0.29	.	.
Sons with zero pubs.	$\Pr(y_{t+1}=0)$	0.38	0.37	0.01
Fathers median	$Q50(y_t)$	5.08	.	.
Sons median	$Q50(y_{t+1})$	3.40	3.58	-0.18
Fathers 75th percentile	$Q75(y_t)$	7.37	.	.
Sons 75th percentile	$Q75(y_{t+1})$	6.41	6.59	-0.18
Fathers 95th percentile	$Q95(y_t)$	9.43	.	.
Sons 95th percentile	$Q95(y_{t+1})$	8.54	8.62	-0.08
Fathers mean	$E(y_t)$	4.46	.	.
Sons mean	$E(y_{t+1})$	3.48	3.61	0.14

M Fertility differentials in academia

This appendix examines the sensitivity of Fact 2—i.e., that the publication's distribution of fathers first order stochastically dominates (FOSD) that of sons—and of our nepotism estimates to fertility differentials between scholars.

As for fertility in general, we unfortunately do not have data on complete families of the professors in the sample. Baudin and De la Croix (2023), however, reconstruct families of professors from Northern Europe, for whom there are many genealogies available in the crowdsourced genealogical websites such as geni.com. That paper shows that the differential fertility between more and less successuful scholars changes over time. From 1625 to 1700, scholars who were more successful at publishing also had more children. From 1700 to 1800, the relationship is reversed, and more successful scholars have fewer children than scholars who published less. These fertility differentials are small, around 0.1-0.2 sons for scholars above vs. below the median in terms of publications. Hence, it is unlikely that the differential fertility in favor of more successful scholars is large enough over our entire sample period to explain away our FOSD fact or our nepotism estimates. That said, because our aim is to study the transmission of upper tail human capital within academia, we believe that conditioning on individuals in our sample being in academia is the right choice.

As for fertility in the sense of number of kids in academia, Table M.1 presents the distribution of parities in our sample. That is, it shows the number of fathers (and their mean publications) by the number of sons they had who entered in academia.

Table M.1: Distribution of parities

parity x	$x = 1$	$x = 2$	$x = 3$	$x = 4$
No. fathers with x children in academia	1320	165	27	3
Mean arcsinh-publications of fathers	4.25	4.4	5.03	6.28
S.E. of the mean	0.1	0.26	0.81	3.16

We have 1,320 fathers with one child in academia, 165 fathers with two children in academia, 27 fathers with 3 children in academia, and 3 fathers with 4 children in academia. The fathers with one and two academic children have similar publications (4.25 and 4.4 respectively). The (few) fathers with more than two children in academia seem more successful in publishing, but the difference is not statistically significant. It is unlikely that these 30 very successful fathers, or even the 195 fathers with more than one child, would bias our nepotism estimates, as they represent a small proportion of our sample and the differences in mean publications are not that large. Nevertheless, to examine this, we re-estimate the parameters of our model excluding them, i.e., excluding fathers with more than one child in academia.

The results are presented in Table M.2. Reassuringly, the estimations are very similar. Specifically, our nepotism estimate (γ) and our intergenerational human capital elasticity (β) are almost identical when we include or not fathers with more than one child in academia. A Clogg et al. test cannot

TABLE M.2: Robustness to fertility differentials within academia

		All [1]		Scholars with one child in academia [2]		Difference [3]	
IGE human capital	β	0.63	(0.04)	0.68	(0.05)	0.05	[0.435]
Nepotism, %	γ	18.7	(1.74)	19.4	(2.13)	0.70	[0.799]
Mean human capital	μ_h	1.87	(0.47)	1.41	(0.61)	0.46	[0.733]
SD human capital	σ_h	4.22	(0.20)	4.36	(0.25)	0.14	[0.662]
SD publications' shock	σ_e	0.39	(0.15)	0.47	(0.18)	0.08	[0.550]
Threshold publications	κ	2.12	(0.14)	2.06	(0.15)	0.06	[0.770]

Notes: SE in parenthesis from 200 bootstrapped samples with replacement; degrees of overidentification: 6; Column [3] shows the difference (col. [2]-[1]) and the corresponding p-value based on Clogg, Petkova, and Haritou (1995)'s test.

reject the null that and the estimates are equal with a p-value of 0.435 for β, and of 0.799 for γ. Hence, we can conclude that the presence of fathers having multiple children in academia does not bias the benchmark results.

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