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Abstract

Does access to education facilitate the emergence of a human capital elite from which social activists, and thus, social change can emerge? Assembling a city-level panel of the political, intellectual, and economic elite throughout German history, we find that the opening of schools providing secondary education for women increased their representation among the human capital elite. These elites challenged the status quo and developed critical ideas that resonated in cities with higher human capital, connecting women to form a social movement. We find no evidence of other city-specific indicators of economic and gender-specific cultural change affecting our results. Differential returns to education are also unrelated to the increasing representation of women among the human capital elite, as the opening of gender-specific schools has no impact on the opposite gender.

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1 Introduction

Can access to education change societies? Historically successful attempts to change society often start with a small number of dedicated activists who develop and write about critical ideas that challenge the status quo (Wood and Tilly, 2012; Markoff, 2015; Della Porta and Mattoni, 2015). From Dr. Martin Luther King Jr. to Susan B. Anthony, from Nelson Mandela to V. I. Lenin, early activists across all genders and nations were often considerably more educated than their peers. And while their education was arguably crucial for their success in promoting social change, educational opportunities also emerged alongside economic development and cultural change (Goldin, 2006; Duflo, 2012). Thus, it remains unclear whether increasing access to education can lead to societal change by facilitating the emergence of a human capital elite.

In this paper, we show how access to education facilitated the formation of a female human capital elite that critically engaged with the status quo. These women became recognized for writing about —and fighting for—the self-determination of women in society. Their ideas resonated in areas with higher human capital were more women could reflect on their role in society. Access to education thus fulfills two roles: It enables the human capital elite to formulate critical ideas (Morris and Staggenborg, 2004; Squicciarini and Voigtländer, 2015) and provides the grounds for these ideas to turn into a social movement.

In the empirical analysis, we compare trends in women's human capital across cities, using a time-varying, city-level measure of the human capital elite between the year 800 and 1950. These data come from the *Neue Deutsche Biographie*, the definitive biographical dictionary of economic, cultural, and political figures in Germany (Hockerts, 2008; Dittmar and Meisenzahl, 2020). Our identification strategy leverages variation in the timing and location of Germany's first institutions providing secondary education to women—i.e. finishing schools (*Höhere Töchterschulen*). We treat the opening of a finishing school as a positive shock to the availability of secondary education for women, since the first finishing schools were opened by foreign Catholic orders, Ursuline nuns (1626) and the Congregation of Jesus (1627).

The curricula of finishing schools did not include provocative thinking in terms of social change, but focused on religious teachings and manners, complemented by foreign languages, and writing. Parents either wanted to increase the marriage chances of their female offspring, or wanted to afford their female offspring similar opportunities that had been afforded to them. However, the opening of finishing schools had unexpected positive consequences on women social movements.

We find that access to secondary education increased women's representation among the human capital elite. Cities with finishing schools are twice as likely to have a notable woman being born, and their representation in the human capital elite increased from 2.6% to 4.5% within 50 years. Female writers, women's rights activists, and women participating in the labor force are more than twice as likely to come from cities with finishing schools. Access to secondary education created a female human capital elite that formed critical ideas about society.

Cities that established finishing schools may have endogenously responded to differential pre-trends, unobservable factors, increased local demand for education, or gender-specific changes in

local culture. Such a selection process would be of concern to our interpretation if it correlates with women's status in society or a city's economic potential; then we would falsely attribute the increased representation of women among the human capital elite to increased access to education instead of these confounding factors. We address these concerns by establishing the absence of pre-trends; by employing an instrumental variables strategy; and, by exploiting a wide range of data to control for local non-linear changes in economic activity, the returns to education, and gender-specific changes in culture. We conclude that the availability of schooling increased women's representation among the human capital elite.

Next, we show that these women migrated to places in which they can act as multipliers of social change. We find that after the opening of finishing schools, the probability that a notable writer is mentioned in another woman's biography increased threefold.[1] Writers spreading critical ideas via books, journals and reading events require a high human capital environment to be effective multipliers of social change. We find that while 14.3% of cities without access to secondary education had a notable women, 53.8% of cities with finishing schools and active writers produced notable women.

Our results thus suggest that finishing schools produced a female upper-tail human capital elite that critically engaged with their status quo (writers and activists) who migrated to cities with higher human capital in which they could act as multipliers of social change. Access to secondary education thus provides a necessary (leading activists developing critical ideas about the status quo) and sufficient condition (an audience that can critically engage with the activists' ideas).

Finally, we identify two potential outcomes of a crictical mass of women discussing their role in society at the city level. If independent female writers and activists succeeded in changing the role of women in society, we should observe more discussion of a women's place in society and a greater female representation in parliament after suffrage had been achieved. First, digitizing letters to the editor of the first feminist newspaper (*Frauen-Zeitung*, 1849–1852), we observe a higher engagement with the critical discussion of women's role in society in cities with finishing schools. Compared to cities without finishing schools, they are three times as likely to send a letter to *Frauen-Zeitung* in support of the women's cause, indicating a more successful propagation of critical ideas in their city of origin. Women also started to organize in local chapters of the German women's rights movement that challenged their role in society: By 1909, only 37% of cities without finishing schools established a women's rights organization, compared to 78% of cities with finishing schools. Second, we analyze the complete record of German members of parliament and find an increased likelihood of a female parliamentarian coming from a city with finishing schools in all parliaments since 1919. As a case in point, in the first democratically elected parliament of the Weimar Republic (1919), at least 40% of female members of parliament had verifiably attended a finishing school and more than 50% had been a member of a women's rights organization.

In sum, our findings indicate that educational institutions, which foster the exchange of critical

[1] These connections are only recorded if they were substantial: for example, if women collaborated on the foundation of a local chapter of a women's rights association. An example of such a connection is the connection between Helene Lange and Gertrud Bäumer, who jointly published the feminist newspaper "*Die Frau*" from 1893 onwards.

ideas and provide the space to form networks, can function as important catalysts for the emergence of a new human capital elite. Then, leaders from this new elite can develop critical ideas and disseminate these ideas among a critically engaging audience. This way, they change their society into one where previously disadvantaged groups participate in social and political life.

Our results are highly robust. We find no evidence that finishing schools were established based on differential pre-trends or unobservable characteristics. Our empirical strategy also allows us to exploit idiosyncratic supply-side factors driving the establishment of finishing schools and isolate them from local demand drivers. We capture local demand for educational facilities by different population trends, the timing of the German Industrial Revolution and the emergence of the Enlightenment period. Our findings are inconsistent with the notion that cultural changes drove local demand for education. We conclude that finishing schools are the result of supply-side factors and idiosyncratic choices that led to establishment of a finishing school in a city.

We also find no evidence that finishing schools were established in response to other changes in (local) attitudes towards women. To distinguish the impact of education from other social changes, we test whether other important economic and cultural events predict a similar increase in the representation of women among the human capital elite. To this end, we employ a series of placebo exercises and test whether non-linear changes in (i) economic activity, (ii) the returns to education, and (iii) gender-specific changes in culture predict a similar increase in the emergence of notable women. First, using historical construction data, we find that the establishment of finishing schools did not coincide with a surge in economic activity. Second, we document that the staggered introduction of male schools does not predict women entering the human capital elite; similarly, finishing schools have no impact on men entering the human capital elite. Third, to alleviate concerns about non-linear gender-specific changes, we employ four markers of gender-specific cultural change as placebo treatments and find that none coincide with a rise in the female human capital elite. Finally, we show that our results are not driven by the Protestant Reformation arriving in cities.

We highlight the robustness of our results applying different weighting techniques (Callaway and Sant'Anna, 2021; de Chaisemartin and D'Haultfœuille, 2020) and the procedures outlined in Baker et al. (2021). We argue and provide evidence against negative treatment weights and effects in our setting: School openings would have to create *fewer* notable women. Aggregated treatment effects using (Callaway and Sant'Anna, 2021) and (Baker et al., 2021) produce similar results. We additionally replicate our main results in a classical differences-in-differences design, we define a set of cities based on whether they established a finishing school by 1850 (treatment group) or not (control group), and compare the shares of women entering the human capital elite after the opening of the first finishing school in 1626 (post period). We find no differential pre-trends, but a significant increase in women entering the human capital elite after the first finishing school was constructed.

Our paper expands upon a thriving literature in economics studying the increasing representation of women starting in the late 19th century (Bertocchi and Bozzano, 2016; Fernández, 2013;

Galor and Weil, 1996; Goldin, 1990, 2006). First, by disentangling the availability of secondary education from other cultural and societal changes, we show that education was a key driver behind the increasing status of women in society and ultimately the women's rights movement. Second, we extend this literature by studying how social movements, and their leaders, emerge in the first place. A prominent theory in sociology is that educational capital is the key resource for leaders, even when leaders arise from poorer segments of society (Morris and Staggenborg, 2004). By leveraging data spanning several centuries, we can study the emergence of social change from before its very beginning until it reached key milestones, such as the foundation of the women's rights movement in the mid-19th century and women's suffrage in 1919. Our findings support the notion that educational institutions that foster the exchange of critical ideas and network formation can serve as important catalysts of the emergence and success of social movements.

Our findings also speak to the literature studying the role of an emerging human capital elite in early-modern Europe and beyond. Here, the human capital elite constituted a herald of economic change in the lead-up to the Industrial Revolution (Galor and Weil, 2000; Diebolt and Perrin, 2013; Mokyr et al., 2015; Squicciarini and Voigtländer, 2015). The dispersion of this upper-tail human capital over space and time was shaped by the institutional environment such as welfare and educational policies (Dittmar and Meisenzahl, 2020; Squicciarini, 2020; Tabellini and Serafinelli, 2020). Countries with highly educated leaders showed higher rates of economic growth (Besley et al., 2011) and democratic participation (Glaeser et al., 2007). We extend these existing studies in two dimensions: first, we explicitly focus on the female human capital elite. Second, we show that in the context of the emergence of the German women's rights movement, this female human capital elite through its impact on early activists' efforts to disseminate critical ideas and institutionalize the movement constituted an important determinant of social change in and of itself.

The paper is structured as follows: In Section 2, we discuss the historical link between finishing schools and women's role in society. We discuss our data sources and construction in Section 3, before discussing the identification assumptions of our empirical strategy in Section 4. In Section 5 we present our main findings on the finishing schools' impact on female representation among the human capital elite. In Section 6, we conduct several placebo exercises to rule out confounding economic and cultural changes. In Section 7, we show that finishing schools facilitated networking and immigration of women. Before concluding, we replicate our finding using cross-sectional variation and discuss two potential outcomes of social change at a local level in Section 8: The dissemination of critical ideas, the organization of the women's rights movement, and modern-day representation in parliaments.

2 Historical Background

We begin by illustrating the links between social change and the emergence of religious finishing schools. In the aftermath of the Protestant Reformation, foreign Catholic women's orders began establishing finishing schools that focused on religious teachings but also included limited

aspects of secular secondary education. At these finishing schools, students and teachers alike found access to critical ideas and a network of like-minded women. Several graduates eventually disseminated critical ideas as writers and activists and contributed to changing womens' role in society. Religious finishing schools thus contributed to the formation of a group of pioneering women among the human capital elite, who acted as catalysts for social change.

2.1 Finishing schools

For the largest part of German history, only daughters from privileged families could obtain secondary education in the form of private tutoring. Access to secondary education for women improved when the orders of the Ursulines and the Congregation of Jesus, founded in Italy 1535 and Flanders 1609 respectively, expanded into Germany. In the aftermath of the Protestant Reformation, these orders aimed to strengthen women's adherence to Catholicism in religiously competitive areas of Germany: The Ursulines founded one of the first finishing schools in Cologne with the explicit goal of creating a "bulwark against emerging Protestantism" (Lewejohann, 2014, p. 57), while the Congregation of Jesus established their school in Munich to educate young women in "good Christian manners, virtues and other studies [Wissenschaften]" (Riedl-Valder, 2020, p. 2). Often these schools were supported by catholic monarchs such as King Max I. of Bavaria or Queen Katharina of Württemberg.

In response, two other types of schools were created: Protestant schools and private institutes. Pietists opened the first school in 1698, to combine biblical doctrine with a similar focus on Christian life and piety. Some ruling families took pride in sponsoring finishing schools in their territory, but compared to Catholic rulers of Bavaria and Wuerttemberg, "Prussian monarchs did not move as vigorously as others to support secondary schools for girls." (Albisetti, 1988, p. 29). Similarly, schools created to educate the daughters of the human capital elite only started to appear in the early 19th century. [2]

Finishing schools' primary goal was to strengthen women's adherence to the respective faith, while parents sent their girls to finishing schools to improve marriage opportunities. This focus on religious teachings and marketable housekeeping skills emphasizes that religious finishing schools were not established with the explicit aim of empowering women. However, these finishing schools also included limited instruction in German, foreign languages, and arithmetic, and were among the first to provide education at the secondary level to women in German history. In contrast to the rollout of secondary education in the United States (Goldin and Katz, 2003), women generally received lower quality education than men as female teachers were denied the same quality of education as male teachers. By 1850, more than 200 finishing schools provided secondary education to thousands of young women.[3]

[2]The establishment of finishing schools in Protestant areas only gained momentum after 1750, by which time already 40 finishing schools had been established in Catholic regions. When including covariates, we always control for religion and ruler fixed effects to capture these different tendencies. In addition, we provide a specification separating schools into 'Early' and 'Late' schools, to assess the severity of this potentially demand driven bias.

[3]These schools, while fundamental for girls' education, were not a groundbreaking local institutions for two reasons:

2.2 Female writers and social change

The impact of female writers on social change can be seen through three critical developments building on each other. First, female writers discussed the dependent and subordinate position of women. Second, female writers openly criticized this status quo and formulated demands for a more independent life for women. Third, and last, female writers organized in women's rights organizations and connected with politics to change society.

First, female writers began to discuss their status quo and articulated a stronger, more independent, role of women in society. Publishing anonymously, female writers started to use independent women as their main protagonists (e.g., Sophie La Roche, "*Geschichten des Fräuleins von Sternheim*", 1771) and proposed new and alternative ways for women to live their lives that contradict society's expectations for women at the time (e.g., Caroline Auguste Fischer, "*Gustav's Verirrungen*", 1801).

Second, an increasing number of female writers openly criticized the subordinate status of women in the male-dominated society of the early nineteenth century (Gerhard, 1990, 2020). Writers demanded access to comprehensive educational opportunities for women, right to female employment, abolition of the convenience marriage and right to live their lifes more independently. With these far-reaching demands, they transcended the boundaries of the social structure of the early nineteenth century (Möhrmann, 1999).

Finally, female writers began to organize and connect with politics. Three central figures for changing women's place in society were the writer and activist Louise Otto-Peters (1818-1895), the writer and teacher Auguste Schmidt (1833-1902), and the politician Clara Zetkin (1857-1933). Actively arguing for women's rights since the 1840s, Louise Otto-Peters formulated her view on women's place in society in her novel "*Ludwig der Kellner*" (1843): "the participation of women in the interests of the state is not only a right, it is a duty of women."[4], and in many journals and newspapers: "We hold the belief that the real solution [to the women's question] can only be *found by women themselves, by their own strength and will*, and that any other solution is nothing but a makeshift that helps in the short run, but will have to be discarded soon after."[5]

Together with Auguste Schmidt, a teacher and director of a finishing school in Leipzig, Louise Otto-Peters founded the first women's rights organization in 1865. The "Allgemeiner Deutschen Frauenverein (ADF)" soon became the roof under which many local chapters organized and campaigned for women's rights. An early member of the ADF, and student of Auguste Schmidt in Leipzig, Clara Zetkin (1857-1933) was ultimately instrumental in connecting the women's rights

First, all cities with finishing schools had existing male schools, and second, their importance for the women's rights movement has only been rediscovered recently (Albisetti, 1988).

[4] "*Die Teilnahme der Frau an den Interessen des Staates ist nicht allein ein Recht, sie ist eine Pflicht der Frauen.*" Authors' translation. https://www.bpb.de/gesellschaft/gender/frauenbewegung/35309/louise-otto-peters, last accessed: 2022-01-13.

[5] "*Wir sehen alles mit Freuden geschehen, was geschieht, um die Frauenfrage ihrer Lösung immer näherzuführen. An der Überzeugung aber halten wir fest, daß ihre wirkliche Lösung nur gefunden werden kann durch die Frauen selbst, durch ihren eigenen Willen und ihre eigene Kraft, daß jede andere Lösung nichts ist als ein Präservativ, das nur auf kurze Zeit helfen kann, dann aber doch wieder als unnütz beiseite geworfen werden muß.*" Authors' translation. (Otto-Peters, „Das Recht der Frauen auf Erwerb", 1997, S. 99; emphasis in the original).

movement to the Social Democratic Party (founded in 1869) and bringing equal suffrage into their party program, and thus into mainstream politics.

Thus, the increasing female representation of women among the human capital elite of cities stimulated a critical discussion among female writers about women's role in society. While these discussions were descriptive at first, they soon proposed alternative ways for women to live their lives. Subsequent generations of women used these demands as the founding principles behind the first formal women's rights organization in 1865. As this movement grew into organizing more than a quarter of million women of all socio-economic backgrounds, political parties could no longer ignore women's rights and adopted many, though not all, demands of the earliest female writers and activists in their party's programs (Evans, 1980).

In short summary, we argue that within two centuries, women's status in society had improved significantly, thanks to the early efforts of independent women, writers and activists promoting a development of female personality free from societal demands. Without finishing schools, neither teachers nor students would have had comparable access to critical ideas and a network of like-minded women. Thus, they contributed to the formation of a group of pioneering women among the human capital elite, united by their opposition against women's status as second-class citizens. Crucially, these pioneering women disseminated their ideas to the broader public, thus acting as catalysts for societal change.

3 Data

We assemble a novel dataset to study the role of secondary education in promoting the emergence of a female human capital elite. Our main outcome variable is derived from the biographies of all notable individuals born between 800 and 1950 CE within modern-day boundaries of Germany. Our explanatory variable "finishing schools" captures the availability of secondary education for women between 1626 and 1850 in all German cities. We combine these data to a balanced panel of cities in half-century periods, indicating the birth of notable women and the availability of secondary education at the nearest city.

Biographies of Notable Women We obtain detailed micro data on the universe of notable German women and men for the period 800 to 1950 CE from the "*Neue Deutsche Biographie*" (NDB) and follow Dittmar and Meisenzahl (2020) to construct measures of women's representation among the human capital elite.

The NDB is "considered the single most relevant biographic encyclopedia of the German language" and includes biographies detailing the professions and nobility of historically relevant men and women (Historische Kommission der Bayerischen Akademie der Wissenschaften, 2019). It incorporates its direct predecessor, the "*Allgemeine Deutsche Biographie (1912)*", and in scope is comparable to the "*Dictionary of National Biography*" for British notable men and women. It originated in the historical commission of the Bavarian academy of science in 1858 and never mentions

the word women in its founding charter or any report thereof.[6]

It was designed to address and eliminate selective inclusion by networks or kinship and instead document the "thoroughly pluralistic foundation" of German cultural achievement (Hockerts, 2008, p. 238). The editorial inclusion criteria targeted everyone, "whose deeds or works contributed to the development of Germany in history, science, art, trade, or business, in short in every corner of political or cultural life" (von Liliencron and Wegele, 1875, p. V-VI).[7] This precludes the inclusion of time-bound personalities in the realm of women's education, and only includes individuals who shaped the course of women's rights and society as a whole.[8]

We link 2,172 non-noble secular women to cities of birth within in the modern-day boundaries of Germany after 800 CE, as well as 586 women from the nobility, who we use as a placebo to ensure our estimates are not affected by differential population growth between cities. Thus, for each city and period, our data records the number of women born who later became recognized for their achievements. Of all 2,732 women, 41% became notable for being an artist, 31% for being a writer, 16% for being born into nobility, and 11% each for being a politician or Teacher (Table A.2). We use the place and date of birth of notable women alongside with the reported biographical information to trace women's representation among the human capital elite across cities and periods. Our main dependent variables are (i) an indicator for whether at least one non-noble secular woman was born in a given city and period who became notable later in life, (ii) the log number of non-noble secular women, (iii) and the share of non-noble secular women among all non-noble secular individuals. These variables measure the extensive and intensive margin of women's representation among the human capital elite.

Finishing Schools We link the birthplaces of all notable women to the historical emergence of finishing schools providing secondary education obtained from the "*Data Handbook of German Education History*". This handbook covers all traditional female finishing schools constructed between 1626–1850. (Neghabian et al., 2005).[9] We match finishing schools to our data on notable women based on their location and opening date. The first finishing schools were established by female

[6]It is also unlikely that an all-male panel was influenced by the existence of finishing schools. The German word "Mädchenschule" ("girl") already lends to a trivialization of women.

[7]There is no evidence that editors or exports are selected based on the existence of finishing schools: "The editors don't just rely on their own judgment; it bases its decisions on the advice of experts, on the advice of scientific institutes and professional organizations. Basically, it is assumed that the local and time-bound personalities have to be eliminated. In the areas of intellectual culture, it is primarily the independent, forward-looking performance that decides, in the case of persons in a high position of responsibility, the impact on the general social course." (Bayerische Akademie der Wissenschaften. Historische Kommission, 1953, p. IX, own translation).

[8]The NDB is not a book of educators. A separate source about important German educators in the nineteenth century only list 14 women (and 469 men) (Beyer, 1903). Being a principal of a school was not enough to enter the NDB as only 6 women who shaped the course of society are listed in both sources. In general, only 12% of individuals are listed as important for female education, highlighting again the low importance attributed to female education in the nineteenth century.

[9]We focus on these schools with continuous operation selected by Neghabian et al. (2005) as the most comprehensive data on finishing schools ("*Höhere Töchterschulen*") in Germany before the emergence of the women's rights movement. Other schools existed, especially in later years, but Neghabian et al. (2005) do not include these schools for two main reasons: First, these schools often operated only for a few years and closed down quickly for unknown reasons. Second, it is often unclear weather these schools provided a curricula that extended beyond basic primary education.

orders of the Catholic church who, following an invitation by the ruling houses, settled near existing monasteries to educate and "protect the women's mind from the falsities of their time".[10] Protestant or city schools only started to emerge after 1750. In total we record 209 school openings in 129 cities between 1626 and 1850, without a clear spatial pattern in location or timing.[11]

An important limitation with these data is that we cannot link individuals to their school in a systematic way. However, 40% of the female members of parliament in 1919 and 60% of female entries in a compendium on the German educational system of the nineteenth century (Beyer, 1903) have verifiably attended a finishing school. We thus interpret the opening of a school as a positive shock to the probability of attending secondary education for women.

Cities Since birthplaces of notable women and the location of finishing schools do not overlap perfectly, we utilize data from Voigtländer and Voth (2012) and construct a panel of 388 German cities that existed in 1300.[12] We merge the biographies of women and the emergence of finishing schools to the nearest city and period in our sample, thus covering all of modern Germany. This procedure has two advantages: First, it does not rely on any political or geographical boundary as the matching procedure is solely based on distance.[13] Second, we can use the rich set of covariates from Voigtländer and Voth (2012) to flexibly capture economic, religious, and educational factors, as measured in 1300, in every period.

4 Empirical strategy

We study the role of expanding access to education in promoting the emergence of a female human capital elite, eventually promoting social change. We begin by descriptively assessing the effect of finishing schools on women's likelihood of entering the human capital elite. We normalize each year of birth by the year the first finishing school opened in her city of birth, and for every city and period, calculate the likelihood that a non-noble secular women was born in that city and period. In this setup, we conduct an event-study exercise including city and period fixed effects, and plot

[10] "*...vor allem den unteren Volksschichten das religiöse Leben (zu) heben und den Frauen Ansichten und Grundsätze (zu) vermitteln, durch die sie gegen Irrtümer ihrer Zeit gesichert und für eine gesunde Erweiterung ihres Lebensinhaltes befähigen würden*" https://de.wikipedia.org/wiki/Erzbischöfliche_Ursulinenschule_Köln, cited from Festschrift der Ursulinenschule, Köln 2014, S. 261, last accessed 2021-02-09.

[11] Pattern shown in Appendix J.1a. Some later schools might have been a response to local demands of the population. We report the same results for when using schools constructed in the period 1650–1750 or 1750–1850 in the Table C.4. We also report similar sized point estimates for every treatment period in Table G.2. Schools are not spatially correlated (Moran's I: 0.002, p-value 0.156), yet we follow two additional strategies to deal with any remaining spatial autocorrelation. First, we report standard errors corrected for spatial correlation in Table E.1. Second, we randomly distribute the actual number of schools build in every period across Germany and show the distribution of point estimates in Figure E.1.

[12] We only use cities if they lie in the present-day borders of Germany to control for spillovers from suburban towns to cities. Results are robust to changing the year a city existed to 800 (Table C.1), changing to 25 year periods (Table C.2), and including city×period fixed effects in a panel setting with gender×city×period as the level of observation (Table 2).

[13] In an alternative approach explored in Appendix C.2, we instead use administrative boundaries of territories in 1619 and merge all data based on whether city 'c' was in territory 't'. As our results remain qualitatively unchanged, we argue that sample selection does not introduce a bias in our setting.

the point estimates for each cohort in Figure 1. A woman aged thirty at the time the first finishing school in her city of birth opened, would not have had the chance to attend the school during her formative years, and thus serves as our reference group ("50-25"). A ten year old women in the same year however ("25-0"), had the chance to attend a finishing school and is thus 10 percentage points more likely to appear in the record of all notable individuals in German history, than her thirty-year old counter part.

This first descriptive exercise suggests that finishing schools indeed increased women's representation among the human capital elite. However, this approach exhibits limitations that prevent a more thorough investigation. First, cities that never establish finishing schools cannot be included in this setup to comprehensively assess pre-trends or employ a standard differences-in-differences regression. Second, when we continue to explore alternative hypotheses using the opening of male schools, universities, increased economic activity, changing culture, or migration, normalization around the event would change the framework and data, limiting comparability.

In our main exercise below we thus create a balanced panel for all cities to keep a common framework for all our estimation results. For each city, we create 50 year periods from 800 until 1950 CE to ensure a sufficient overlap between the opening of a finishing school and its effect on women being recognized for their achievements in our biographical database. Finally, we assign each finishing school and birthplace to its closest city and period.[14]

4.1 Identification Strategy

We combine the staggered introduction of religious finishing schools and unique biographical microdata on the universe of notable women in German history to a balanced panel of 388 cities between 800 and 1950 CE. The key empirical challenge is then to isolate the impact of finishing schools from potential confounders that are correlated with both finishing school opening and the increase in women's representation among the human capital elite.

Cities that establish finishing schools may differ on a wide range of characteristics. Even if these schools were established by idiosyncratic decisions that are uncorrelated with local economic conditions or the demand for education, a causal interpretation of the impact of finishing schools requires that all unobservable factors that influence women's representation among the human capital elite must be orthogonal to finishing school opening. However, as production technologies change, increased returns to education could also induce a rise in the demand for education, although the guild system prevented female entry into most occupations until its dissolution. Similarly, wars or natural catastrophes that disproportionately affect the male population increase the demand for female labor and thus the demand for educated women. These local, often unobservable, factors can increase the adoption of educational policies and thus change the relative wages between cities. Then, cross-sectional evidence or failing to control for local factors risks overstating the true effect of finishing schools on women's representation among the human capital elite.

[14]Conditional on having a school, the average distance between cities and schools is 5.5km with a median of 1.1km.

We address local differences between cities by including city and period fixed effects in a Two-Way-Fixed-Effects setup, capturing all observable and unobservable time-invariant factors that vary between cities and periods in our sample.

$$Y_{c,t} = \beta\, Finishing\ school_{c,t} + \alpha_c + \alpha_t + \alpha_c \times t \qquad \text{(Baseline)}$$

$$+ \sum_{\tau=800}^{T=1950} [X_{e,c} \times \alpha_\tau + X_{r,c} \times \alpha_\tau + X_{s,c} \times \alpha_\tau] + \varepsilon_{c,t} \qquad \text{(Additional Controls)}$$

In our baseline specification, we regress a binary outcome of whether a woman who became notable later in life was born in city c and period t, on an indicator of the presence of a finishing school. We use two definitions of this indicator $Finishing\ school_{c,t}$: In our main specification, this indicates whether a finishing school is present in city c at time t. In Appendix G, we abstract from the variation in timing and define this variable as the classical differences-in-differences estimator, comparing 129 cities with finishing schools to 259 cities without after 1650: $Finishing\ school_c \times \mathbf{1}(t \geq 1650)$.[15] We include city α_c and period α_t fixed effects as well as city-specific linear time trends $\alpha_c \times t$. This baseline set of fixed effects captures all unobservable city-specific trends that evolve linearly over time. We cluster our standard errors at the city level c and report standard errors corrected for spatial correlation in Appendix E, Table E.1.

To identify the impact of finishing schools on women's representation among the human capital elite, we must argue that conditional on our set of fixed effects, either school assignment is as good as random or that observed increases in women's representation among the human capital elite can only be attributed to finishing schools. Since the former is unlikely, the latter requires us to relate the increase in the number of notable women being born after the opening of the first finishing school to the long-term trends that determine women's representation among the human capital elite and finishing schools. Then, to identify the impact of finishing schools, cities need not exhibit different trends prior to the establishing of the first finishing school. In addition, since our baseline specification already captures differences between cities that grow linearly over time (e.g. population growth), our identifying assumption necessitates to sufficiently capture all remaining non-linear, city-specific, confounding factors.

With our additional controls we capture three sets of potential confounders that might non-linearly predict women's representation among the human capital elite and the opening of finishing schools: economic, religious, and educational characteristics. The first set of covariates capture the potential direct effects of economic characteristics that influence the decision to open finishing schools ($X_{e,c}$). We proxy for the economic and financial development using membership in the Hanseatic League, Jewish settlements and pogroms against Jews (Voigtländer and Voth, 2012). We complement these covariates with population data in 1600 from Bairoch et al. (1988), female specific labor demand as proxied by religious battles during the 30 Years' War affecting sex-ratios and local weather conditions affecting agricultural production from Leeson and Russ (2017). Com-

[15] Using this classical differences-in-differences design we find no evidence for pre-trends (Figure G.1) and similar point estimates (Table G.1). Further, we find no evidence of heterogeneous treatment effects across treatment periods (Table G.2).

bined, these covariates, measured before the opening of the first school, capture demand factors of productivity and relative wages that may impact the decision to establish a finishing school.

The second set of covariates capture the potential influence of religion on school opening and women's representation among the human capital elite. Since almost all early finishing schools were established by religious orders, this set of covariates capture any direct effects of religious differences across cities ($X_{r,c}$). We include whether the city was a bishopric seat (Voigtländer and Voth, 2012) and distance to Wittenberg to proxy for the diffusion of Protestantism (Becker and Woessmann, 2009; Cantoni, 2015). We determine which cities were Protestant or Catholic in 1619 by digitizing cartographic material in Engel et al. (1995), and include the distance to the inner-German denominational boundary to capture religious competition between the major religious denominations. In combination, our religious controls thus address two major concerns regarding the comparison between Protestant and Catholic cities: first, early finishing schools were built by Catholic orders and Protestant cities did not establish secondary educational institutions in significant numbers until 1750. Second, as highlighted in Becker and Woessmann (2009), since Protestantism is generally associated with a greater proportion of women receiving (limited) primary education, we might wrongly attribute an effect of Protestantism to finishing schools.

Finally, we address the direct effects of differential returns to education across cities ($X_{s,c}$) by determining whether a city had a university or provided higher male education in 1650.[16] In addition, we control for different educational preferences of different heads of state by controlling for the ruling house of each city as of 1619 using Engel et al. (1995).[17] Combined, male schools, universities and the educational preferences of ruling houses capture local returns to education across all genders at the time the first finishing schools were established in Germany.[18]

In Table H.4, we show how these variables and our main outcome in 1600 predict whether a finishing school was established in a city thereafter. We begin by showing that the fraction of non-noble secular women among the human capital elite is not predictive of finishing school construction (Panel A), speaking against a local demand for establishing finishing schools. We continue in Panel B and show that larger cities and cities that lie further away from the religious divide between Catholicism and Protestantism were more likely to establish a finishing school.

To capture these differences and isolate the effects of finishing schools from these confounding factors, we include and interact all covariates with period fixed effects and show robustness to a matching strategy creating a balanced sample based on pre-treatment variables. Our identifying variation is thus limited to within-city, off the linear time trend of any unobservable confounding factor and the non-linear evolution of observable economic, religious, and educational differences across time. Hence, all remaining violations of the main identifying assumption must arise from

[16] Obtained from https://bit.ly/2OHH4tp and https://bit.ly/3mG9mRr, last accessed 2021-02-09.

[17] An example is Prince Bishop Ferdinand of Bavaria who, in response to religious competition, pushed for female education to win over the minds of women.

[18] In the spirit of Galor and Weil (1996) we assume that local returns to education are not impacted by directed technical change that would increase the returns to education for one specific gender. However, estimating a panel with city $\times$ year fixed effects and gender $\times$ year fixed effects in Table 2 captures this variation and the point estimates are not statistically different from our baseline.

unobservable non-linear confounding factors which explain both the opening of a finishing school as well as the subsequent increase in women's representation among the human capital elite.

4.2 Evaluating pre-trends

We evaluate the validity of our empirical design by testing for differential pre-trends in the event-study graph of Figure 2.[19] Here, we limit our sample to all cities in which a finishing school has ever been established and estimate the impact of the first finishing school four centuries before and two centuries after its opening. Thus, potential differences between cities with and without finishing schools do not affect the validity of this analysis.

In Figure 2, we provide evidence in favor of our identification assumption as finishing schools have a precisely estimated zero impact on the birth of a non-noble secular women in all periods prior to opening. After the first school opened to girls, the probability of a non-noble secular woman being born in the city and becoming notable later in life increases immediately. This relationship remains robust when including all control variables non-linearly (dashed line). In the remaining Panels of Figure 2, we document the absence of pre-trends when using the number of women born (Figure 2b) and the share of women among all notable individuals born in the same city and period (Figure 2c). We observe a significant treatment effect in the first period after opening that is slightly increasing in the rights panels when controlling for covariates.

We find no evidence that this slight increase is driven by cohort-specific treatment effects biasing our estimates. This problem is most pressing in settings without a never-treated control group: Here, later-treated cohorts function as the control-group for earlier-treated cohorts, potentially creating negative treatment weights biasing the estimate (Goodman-Bacon, 2020). Using the suggested decomposition, we find non-negative weights and point estimates that result from the difference between never-treated cities and cities with finishing schools. We thus leverage cities that never establish a finishing schools as a pure control group in our setting and follow Baker et al. (2021) in providing three sets of evidence against heterogeneous treatment effect biasing our estimates: First, we provide the main event-study graph with and without controls (Figure 2). Second, we provide estimates for each treatment-cohort (Table G.2). Third, in Appendix F we implement the aggregation methods suggested by de Chaisemartin and D'Haultfœuille (2020) and Callaway and Sant'Anna (2021), as well as include never-treated cities to the event-study design. We find no evidence of treatment-effect heterogeneity or differential pre-trends and report similar point estimates in all treatment groups and methods.

Finally, choices when creating the data might affect the observability of pre-trends. In our data, we merge women and finishing schools to a balanced panel of 388 cities, including never-treated

[19] We estimate the Event-Study equivalent of our baseline equation with and without covariates:

$$Y_{c,t} = \alpha_c + \alpha_t + \sum_s \beta_s \mathbf{1}\{t - E_c = s\} + \varepsilon_{c,t}$$

$\{t - E_c = s\}$ denote relative time periods to opening of the finishing schools. Cities enter this sample 400 years prior to the establishing the first school and leave it 150 years after.

cities, and 50-year periods. This, however, does not fully utilize the underlying premise of event studies: the exact treatment period of each school. In Appendix C.3, we construct alternative intervals around each exact opening year of finishing schools and show the resulting event-study graphs. Again, we find no evidence for a pre-trend in any specification, a significant uptick after opening, and point estimates that are not statistically different from our baseline. Thus, we use our balanced panel of cities, allowing us to include never-treated cities and control variables in a two-way-fixed-effects estimation, and take this result as additional evidence against pre-trends or heterogeneous effects biasing our estimates.

5 Women's representation among the human capital elite

Our hypothesis is that the opening of finishing schools increased women's representation among the human capital elite. Women belong to the human capital elite of their city of birth if their names were recorded in the *Neue Deutsche Biographie*. Using data on notable women from 800 to 1950 CE, we document a sustained impact of the opening of finishing schools on an indicator of whether a notable woman was born, the number of notable women, and the share of notable women relative to their male counterparts.

We present our main results in Table 1, using our baseline empirical specification including all cities and periods. We report estimates from three different specifications of our dependent variable to address the sparsity in our outcome variable. In columns (1) - (3), we regress an indicator variable of whether a notable woman was born in city c at period t on our indicator variable for finishing schools that turns on after the opening of the first finishing school in city c period t. Our baseline estimate is reported in column (1) of Panel A and suggests a 20-percentage point increase (s.e. 0.029) in the propensity to observe a woman being born and becoming notable later after the establishment of the finishing school.

To capture the impact of city-specific differences on the establishment of finishing schools and notable women, we conduct two analyses: First, we interact economic, religious, and educational covariates with period fixed effects in column (2). The point estimate of 0.134 (s.e. 0.031) suggests a smaller impact of finishing schools on women's representation among the human capital elite, with finishing schools doubling the likelihood of observing a notable woman in periods after their establishment.[20] Second, we match each city with finishing schools to a city without finishing schools based on pre-treatment variables in column (3) and include match fixed effects to confine the variation to within each city pair. We find that the point estimate in this balanced sample (0.157) is statistically indifferent from the point estimate in our baseline specification (column 1).

In the remaining columns (4)–(9) we explore the intensive margin of the effect of finishing schools on women's representation among the human capital elite. Using the log number of women born in city c at period t, we find that the number of notable women increases by 16.5%, even

[20] If there were a survival bias of schools and we assume schools have a positive impact, our estimates would be downward biased as control observations would be treated as well. In addition, we report reduced form estimates, unaffected by selection, using Monasteries in 1300 as an instrument around 10km of the religious divide in Figure G.3.

when extensively controlling for economic, religious and educational factors.

Population in 1600 interacted with period fixed effects might not adequately capture the heterogeneous growth paths of German cities.[21] By using the number of notable men born in each city and period, we capture differential growth in population, prosperity, and creativity, that might lead to the adoption of finishing schools and an increased representation of women among the human capital elite. In columns (7) – (9), we thus divide the number of notable women born by the total number of notable men and women in the period and city. If the number of notable women in our sample only increased due to a discontinuous change in population, prosperity, or creativity happening at the same time, this would increase in the number of notable men in the same category, too.[22] Relative to cities without finishing schools in which 2.6% of all notable individuals are women, the share of women among the human capital elite increased to 4.5% after the establishment of finishing schools.[23] The robust estimates suggest that finishing schools increased women's representation among the human capital elite and did not affect a city's population or its elite's size in particular.

In Panel B we consider women from the nobility in a placebo exercise. Women from the nobility should not benefit from school construction because the had access to private tutoring. At the same time, it is likely that the fertility rates between the nobility and the common people correlate as the number of noble women increases over time (Online Appendix A.2). We find robustly estimated insignificant null effects of finishing schools on the nobility throughout all specifications indicating that only commoners used the opening of schools to achieve notability.

Robustness We take the strong and robust results on non-noble secular women, and the non-existent impact on women from the nobility, as evidence that finishing schools indeed increased women's representation among the human capital elite in Germany. We conduct numerous further robustness tests in the Online-Appendix to this paper. In Appendix B, we show that our results remain qualitatively unaffected when omitting the linear time-trend, using different covariates (Table B.1), or omitting outliers (Figure B.1). In Appendix C, we gather additional evidence against data construction choices biasing our estimates: Our results remain unchanged when using alternative sets of cities (Table C.1), alternative lengths of periods (Table C.2), or an entirely different set of territories (Table C.3). The estimated effect does not vary greatly the timing of school opening (Table C.4). In Appendix D, we assess the role of demand-side factors and find no impact of finishing schools on population growth or a correlation with the arrival of the Enlightenment (18th century) and the Industrial Revolution (19th century). We dedicate Appendix

[21] While Aachen and Trier were some of the most important cities at the begin of our sample period, they have been outpaced by Munich and Berlin at the end. This pattern is not predicted by initial population size or ruling houses in the 17th century, but due to the emergence of the Prussians and Wittelsbacher lines.

[22] The number of notable men is constructed and obtained from the same source as the number of notable women.

[23] We address the possibility that people move to neighboring towns with schools, and thus spillovers are impacting our interpretation, in two tables: We increase the catchment area of each city by only using 101 cities that already existed in 800 and show the same effect sizes (Table C.1); In Table E.2 we restrict our sample to 129 cities with schools and 27 non-neighboring cities in 1300. All results are robust and indistinguishable from the baseline empirical specification.

E to show that the results are unlikely to be the result of systematic SUTVA violations. To assess whether spillovers affect our interpretation, we create 200 placebo datasets using the true spatial correlation and temporal assignment and find p-values of 0.000 for non-noble secular women, but p-values of 0.570 for royals, as expected. In Appendix F, we show that our point estimates are also robust to various weighting techniques from the recent literature on the validity of event study designs. In Appendix G, we report similar estimates from a classical differences-in-differences setting, dividing cities into those that had established a finishing school by 1850 and those that had not (Table G.1). There is no discernible pre-trend when using all treatment periods jointly (Figure G.1). We find no effect of the arrival of the Protestant Reformation in cities (Figure G.2), but consistent with a supply-side shock in the availability of education, find a significant impact when instrumenting finishing schools with monasteries that existed in 1300 (Figure G.3). We regard the robustness of our results as evidence against a mechanical relationship between finishing schools and notable women which could arise simply due to finishing schools improving record keeping of influential women or increasing the demand for teachers.

6 Economic and cultural change as confounding factors

To rightfully attribute the increase in women's representation among the human capital elite to the emergence of finishing schools, we show that changes in the returns to education, increasing economic activity, or cultural change do not predict an increase of the female human capital elite. We identify such potential confounding factors exploiting the following city- and time-specific placebo events: In Section 6.1, we explore the gender specific impacts of secondary schools to assess whether differential returns to education explain our results. In Section 6.2 we use construction activity as a proxy for economic activity; and in Section 6.3, we exploit the end of witch trials, the opening of female monasteries, the consecration of churches to a female saint, and the arrival of the Reformation, to capture gender-specific cultural changes at the local level. No placebo event predicts a subsequent increase in the number of notable women.[24] Unobservable non-linear and city-specific factors are thus unlikely to confound our finding that finishing schools increase women's representation among the human capital elite.

6.1 Returns to education

In our first placebo exercise, we assess whether finishing schools merely capture local changes to the returns to education. We exploit cross-gender variation in the availability of secondary schooling and show that the number of notable men and women is only affected by the opening of male and female schools, respectively. We thus argue that finishing schools are unlikely to reflect local changes of the returns to education.

[24]These changes are however, correlated to the establishing of finishing schools, suggesting that they are relevant cultural and educational proxies to consider.

To assess the importance of changes in the returns to education, we correlate the occurrence of non-noble secular men with the opening of male schools. Following Galor and Weil (1996), an increased demand for skilled labor increases the returns to education, which in turn increases the demand for schools. School openings thus could reflect changes in local returns to education, and thus economic prosperity would bias our estimates. If that were the case, we would observe that the opening of a male school would increase the number of notable men and women—as would the opening of female schools. The absence of such cross-gender effects would provide evidence against local returns to education biasing our estimates.

In Columns (1) - (3) of Table 2, we limit our sample to 129 cities that ever constructed a finishing school, in a window of four centuries before and three after establishing the first school.[25] Despite the reduction in sample size and the omission of educational covariates, the estimated coefficient (0.126) in this event-study design is still close to those of the fixed-effects estimation reported in Table 1. Finishing schools do, however, have no impact on the likelihood of observing notable men in our data (columns 2). In columns (3) we then construct a panel in which every city-period cell has two observations; one for women and one for men. In this setup, we are able to control for city-by-period fixed effects and gender-by-period fixed effects to estimate the impact of finishing schools on women, while non-linearly controlling for the trends in men and city characteristics at any point in time. Our results confirm the pattern observed previously as finishing schools increase the likelihood of a notable woman being born in the city.

In Columns (4) - (6) of Table 2, we turn to the impact of male schools on notable women and men. The opening of a male school in a city only increases the likelihood of observing a notable man (Column 5), but the impact on women in the same city is a precisely estimated zero (Columns 4). Repeating the panel exercise and non-linearly controlling for city characteristics confirms this pattern and suggests that male schools only had an impact on notable men in the city.

This evidence is summarized graphically in Figure 3, in which we mark the opening of a male school or finishing school, respectively, as our reference period. The validity of our point estimates is supported by the absence of pre-trends and the increase of notable women and men after the opening of finishing and male schools, respectively (top right and bottom left). If finishing schools captured local returns to education, in the same way male schools likely do, we would observe a significant increase in the number of men as well (top left). Similarly, if we observe more notable women purely because the returns to education increased, we should observe a similar increase in women when using male schools as the source of variation (bottom right). Since we observe neither, we conclude that differential returns to education are unlikely to explain the increase in the number of notable women after the opening of a finishing school.

An alternative interpretation, consistent with our discussion in Section 7, is that the curricula across male and female schools differed greatly. While male schools featured large parts of mathematics and science, finishing schools often focused on religion, language, and pedagogy. Then,

[25] We do not include educational covariates as we exploit them for variation. Similarly, matching estimates are not reported as we only use treated observations and drop non-treated from the estimation sample.

male pupils would be educated to be productive members of society, while female pupils would be given the tools to be effective writers and teachers.

6.2 Economic Growth

In the second placebo exercise, we test whether cities with a steeper growth trajectory established finishing schools earlier. Then, finishing schools merely reflect the underlying growth potential that attracted the human capital elite.

Under this alternative hypothesis, the increase in notable women born is not a response to the emergence of finishing schools, but a response to increasing income. We identify local economic activity in our panel using city-level construction data by Cantoni et al. (2018). If finishing schools are merely a manifestation of increased economic growth, the establishment of finishing schools should be a good predictor of future construction activity. However, this is not borne out in our data: even when defining a subset of growth-specific construction that excludes religious, military, and palace buildings, we find no impact of finishing schools on economic activity in Table B.2, nor in any period around the opening of finishing schools (Figure 4a).

In Figure 4b, we repeat this exercise, using population data from Bairoch et al. (1988). Again, we find no evidence for differential growth prior to the establishing of finishing schools and no significant impact of finishing schools on population afterwards.

We believe that the absence of general equilibrium effects on population and growth, in contrast to findings for more recent interventions (Duflo, 2001), is likely explained by two factors: First, finishing schools are neither primary schools that expand literacy, nor are they high schools that prepare women for, at the time nonexistent, high-skilled jobs. In fact, these schools never intended to prepare women for work outside the traditionally advocated family. Second, as husband's approvals for work was required until at least 1958, labor force participation in higher-skilled jobs remained low.

6.3 Cultural Change

In the last set of placebo exercises, we provide evidence against the premise that finishing schools are a reflection of broader cultural changes in society. To assess this alternative hypothesis, we exploit city-, time-, and gender-specific changes in culture: the end of witch trials; the opening of female monasteries; the consecration of churches to a female saint; and the arrival of the Protestant Reformation. Using event-study designs analogous to our analysis of finishing schools, we find no significant impacts on the prevalence of notable women from any of these cultural changes (Table 3 and Figure 5).

In Panel A of Table 3, we use data on the end of witch trials in Germany from Leeson and Russ (2017). Witch trials disproportionately targeted widows living a more independent life as well as midwives and female folk healers (Ehrenreich et al., 1973; Oster, 2004).[26] We thus argue that the

[26] Leeson and Russ (2017) collect data on 3,080 witch trials in 121 German cities, with the first and last trial recorded

'end of witch trials' in a city is informative of a change in local culture away from one of the most violent forms of discrimination against women. The threat of the stake forced midwives and folk healers to practice in secrecy. Then, the end of witch trials might have increased their likelihood of entering our sample. However, we see no impact of the end of witch trials on women becoming recognized for their achievements.

In Panel B of Table 3, we exploit the opening of female monasteries taken from Cantoni et al. (2018) as proxies for gender-specific cultural change. Female monasteries presented women with one of the few alternatives to "traditionally advocated marriage" (Frigo and Fernandez, 2022) and household roles. The establishment of such monasteries could thus be considered reflective of local culture becoming more accepting towards women choosing a comparatively independent lifestyle.[27] However, we do not find significant impacts of the establishment of female monasteries on the number of notable women once we add economic, religious, and educational controls.

Next, we turn to the consecration of churches to female saints in Panel C of Table 3. We utilize data by Cantoni et al. (2018) on 12,334 church construction events in Germany, and identify 1,610 events in which a church was consecrated to honor a female saint.[28] We argue that since churches could be consecrated to any saint, using a female saint might indicate a cultural shift towards the inclusion of women and thus could be correlated with a higher status of women in society. Yet, we identify a precisely estimated null effect throughout all specifications.

In Panel D of Table 3, we use the timing of the Protestant Reformation in each city as an indicator of a potential shift in the status of women. We follow Becker and Woessmann (2008, 2009) who argue that, since Martin Luther suggested that women needed to be able to read, Protestantism had a positive impact on female education.[29] We utilize data by Cantoni (2015) on the timing of the Reformation in cities, to proxy for a cultural shift towards the inclusion and primary education of women following Luther's teachings. Our findings suggest that Protestantism, and the associated potential shift in gender roles, cannot explain the increase in notable women, writers, or any subcategory.[30]

We summarize the impact of cultural changes in the event-study graphs of Figure 5: It is unlikely that gender-specific cultural change contributed to the establishment of finishing schools and the following increase in notable women. We conclude that unobserved economic or cultural change are unlikely to bias our estimates on finishing schools. Instead, it is more likely that finishing schools were established by religious orders in response to religious competition or idio-

in 1300 and 1792. Our inclusion is motivated by the fact that 76 % of witch trials were conducted before 1648 and 23.5% of women were trialed between 1627–1633; a period in which finishing schools for girls sprung up.

[27]Cantoni et al. (2018) have 414 female monasteries in Germany with the average year of foundation being 1275.

[28]The average year of consecration in the data of Cantoni et al. (2018) is 1452 in 260 cities.

[29]Note that this requirement to read was interpreted as providing basic primary schooling. Finishing schools provided secondary education that included French, arithmetic, and literature classes.

[30]We have 146 cities, 129 of which switched by the 16th century. We substantiate our finding in Table G.3 in which we use those cities in a standard differences-in-differences setup, and find weak results on non-noble secular women, but no results on writers, activists, or nobility. We use the log distance to Wittenberg as an instrument (Becker and Woessmann, 2008) and report insignificant reduced form impacts on notable women. The OLS estimates however, suffer from a pre-trend in which cities with more notable women are more likely to become Protestant.

syncratic shocks. Thus, finishing schools, conditional on fixed effects, can be interpreted as an exogenous shift in the supply of education for women.

7 Discussion

In this section, we provide qualitative evidence that link finishing schools to the rise in the female human capital elite and substantiate this assertion with an analysis of book titles published between 1840–1898. We show that the prevailing teaching doctrine focused on religion, German, and foreign languages, which enabled women to develop and exchange critical ideas as writers. With an increasing share of literate women, these authors of the '*Frauenliteratur*' effectively became leaders of social change, entering the hitherto male dominated human capital elite.

The first systematic evidence for teaching doctrines regarding girls date back to enlightenment thinkers Jean-Jacque Rousseau (1712-1778) and Heinrich Campe (1746-1818). Yet, "*both educational theorists were concerned with the legitimization of the subordination of women and the maintenance of a patriarchal gender relationship.*"[31] Women were seen as "*unable to comprehend larger and more abstract connections*" (Jonach, 1997, p. 77) and '*fulfilling the duties as a mother left no place for the individual*".[32] Schools thus focused on religious studies, German, and French or English, leaving little room in the curriculum for arithmetic, physics, or chemistry. "*In principle, only such knowledge and skills can be useful for a woman, that correspond to her special purpose. Everything that runs counter to the educational goal of women as housewives, wives and mothers was not accepted as educational content or as a female quality to be trained and is rejected as useless and/or harmful.*"(Jonach, 1997, p. 157).[33]

The parents of graduates often worked in prestigious occupations, and as "*it is not fitting for an educated man to marry an uneducated woman*" (Rousseau, 1762)[34], positive assortative matching (Becker, 1973) is a likely motive for parents to send their female children to finishing schools. This view is supported by the few biographies known that list the occupation of the student's parents, her husband, and herself: Almost all married into similar social strata than their parents and became subsequently known as writers, female rights activists, teachers, or artists (De la Roi-Frey, 2004, p. 284-286).

Thus, instead of preparing women to be successful participants of the labor force, these schools formed proficient readers and writers, fluent in foreign languages and pedagogy. This prepared its graduates to be leaders of social change amid the increasing share of literate women. While few

[31] "*Beiden Erziehungstheoretikern geht es um die Legitimation der Unterordnung der Frau und der Aufrechterhaltung eines patriarchalen Geschlechterverhältnisses.*" (Jonach, 1997, p. 196).

[32] "*Man faßte den weiblichen 'Stand' ungeachtet seiner sozialen, kulturellen und Interessensdifferenzen als 'Berufskreis' und stutzte die Vielfalt weiblicher Lebensweisen faktisch auf die Anforderung an die verheiratete Frau aus der Mittelschicht zurecht. Individuelle Bedürfnisse der Frau als Mensch werden ganz ihrer weiblichen Bestimmung untergeordnet, die geschlechtliche Pflichterfüllung ließ weiblicher Selbstverwirklungen keinen Platz mehr übrig*".(Jonach, 1997, p. 134).

[33] "*Grundsätzlich können für die Frau nur solche Kenntnisse und Fähigkeiten nützlich sein, die ihrer besonderen Bestimmung entsprechen. Alles was dem Bildungsziel der Frau, als Hausfrau, Gattin und Mutter zuwiederläuft, wird als Bildungsinhalt oder als auszubildende weibliche Eigenschaft nicht akzeptiert und als unnütz und/oder schädlich verworfen.*"

[34] "*Für einen Mann von Bildung schickt es sich also nicht, eine Frau ohne Bildung zu heiraten*" Rousseau, 1762/1993, p. 447 as cited in (Jonach, 1997, p. 77).

women outside nobility could read in the 16th century, this portion of society grew in the 18th century. When "*women started reading, the women's question arose*" (Marie von Ebner-Eschenbach, 1880) as women started to reflect on their position in society and to formulate demands for emancipation (Jonach, 1997, p. 150). These schools, in essence, prepared women to develop and exchange critical ideas as writers.

Famous authors of the '*Frauenliteratur*' such as Sophie La Roche (1771), Frederike Unger (1784), and Caroline Auguste Fischer (1801) debated a common theme; the awakening of the female mind (Daley, 2022). Authors often spoke foreign languages and imported English (La Roche) and French (Unger) lifestyles and ideas into German literacy circles. Unintentionally, finishing schools gave women the tools to become successful writers and effective social multipliers, entering the hitherto male dominated human capital elite.

Content analysis of book titles We substantiate the argument that successful writers became effective social multipliers by analyzing book titles. Our hypothesis is that female authors who act as social multipliers tailor books towards a receptive audience. While it is easy to assign some book titles in Pataky (1898) to female rights activism ("*Der Frauen Sklaventum und Freiheit*": women's enslavement and their freedom) , others such as "*Biegen oder Brechen. Die Geschichte einer Ehe*" (To bend or to break. The story of a marriage) are near impossible to assign based on the title. A great deal of other books in Pataky (1898) are translations from English or Latin; both languages taught at finishing schools.

To structure this analysis, we use a k-means clustering algorithm on 12,800 book titles published between 1840 and 1898 in Pataky (1898) and present the six groups in Figure 6. The most common group with 80% of book titles is the first, predicted by words such as "märchen" (fairy tales) or "geschichten" (stories). However, already the second group is tailored towards women: The five most common terms essentially combine to 'stories for young girls' ("*erzählungen, mädchen, jugend, für, junge*"). Group 4 contains 'for german women' ("*deutsche, für, Frauen*"), with group 5 containing 'stories for true german girls' ("*geschichte, für, wahre, deutschen, mädchen*"). In combination, groups 2, 4, and 5 gather 15% of all booktitles in the data.

The results suggest that some female writers indeed worked on topics relevant to girls at an impressionable age. Others wrote novels and dramas for women, discussing the role of women in society. Combined with the above analysis on important female works in literacy (Daley, 2022), the analysis here purports a significant role for female writers for social change.

8 Mechanism

In this section we present evidence on how access to secondary education, and an increase female human capital elite, are linked to social change. We show that finishing schools disproportionately produce writers and activists, that form networks across cities and attract other notable women. We especially focus on the group of writers, who by disseminating their ideas in places with high

human capital, act as multiplicators of social change.

8.1 Who are the women in the human capital elite?

As a first step, we analyze the critical impact of female writers on social change discussed in Section 2.2. Female writers discussed the dependent and subordinate positions of women, propagating their criticism of the status quo through books, journals, and reading events throughout Germany. As access to secondary education increases women's representation among the human capital elite, female writers would be instrumental in formulating and spreading this criticism. Indeed, our estimate in Table 4 column (3) suggest that the probability of a famous writer being born doubles after the establishment of a finishing school (8.7% to 19.7%).

Similarly, theories on leadership in social movements predict that educational capital is necessary for leading activists to arise and change society (Morris and Staggenborg, 2004). Our estimates in Table 4 column (6) suggest that 7.5% of cities with access to secondary education have a female rights activists, compared to only 1.8% of cities without.[35]

Finally, a common prediction of social change models is an increase in labor force participation of disadvantages groups. The remaining columns in Table 4 support that view, by showing that we observe more women as writers, teachers, academics, doctors, or any other occupation in cities with access to secondary education for girls.

8.2 Networks and immigration

These social leaders then utilize their networks in order to promote their critical ideas. Unfortunately, networks that exist within a classroom environment are no longer accessible to us as a researcher. We have to rely on networks between two women in the human capital elite.

We construct our measure of networks between women using the biographies of women in the *Neue Deutsche Biographie*. Here, we define a connection between two women if one is mentioned in the biographical text of the other. A network thus exists in a city if at least one local woman is connected to another notable woman.[36] The size of a city's network in period t is then defined as the sum of notable women being mentioned in the biographies of all other women born in that city in period t.

In Table 5, we analyze the impact of finishing schools on networks between notable women. We find that finishing schools increase the likelihood of observing a network and its size fourfold (Column 1). Reflecting the importance of female writers for social change, networks between female writers increase fivefold in cities with access to finishing schools, regardless of the specification (Column 6). Yet, as these events are rare, we use migration to infer on networks across cities,

[35] We define female rights activist by keyword searches of each women's CV and classification in the NDB.

[36] An example is Getrud Bäumer: She attended the finishing school in Halle and became a teacher in Magdeburg. She was introduced to Helene Lange by an older colleague and joined the *Allgemeiner Deutschen Lehrerinnenverein* in Berlin 1898. Throughout their career, Bäumer and Lange closely collaborated on promoting women's rights, in particular women's access to education.

and confirm our hypothesis.

If finishing schools facilitated the formation of, and access to, networks of like-minded women, presumably they also increased the likelihood that women migrated to the city ("pull" factor). We document migration patterns using the difference between women's places of birth and death as recorded in the *Neue Deutsche Biographie*. A total of 507 women in our data have migrated at least 10 km between birth and death. We repeat our event-study for these immigrated non-noble secular women in Figure 7. Again, we observe no pre-trends and a distinct increase in the likelihood of immigration after the opening of the first finishing school; a finding robust to including control variables (dashed line).

To identify whether finishing schools attracted notable women, or the immigration of notable women instead facilitated the foundation of finishing schools (reverse causality), we provide two pieces of evidence: First, if immigration of notable women increased the likelihood of finishing school opening, Figure 7 would show differential pre-trends. The absence of such pre-trends suggests that finishing schools had a similar effect on immigrated women as on native women, and that finishing schools are likely not a result of immigration.

Second, we build on this result and provide further support for the idea of increased networking activity using the timing of immigration, or birth, of the first notable women as our source of variation. If immigration led to the opening of finishing schools, and therewith to the formation of a female human capital elite, the first immigration event would increase probability of establishing a school. Our effect would be driven by migrating women establishing different social norms. If instead, finishing schools increased women's representation among the human capital elite, which in turn attracted notable women from other cities, we would observe that the first native notable woman appears when the first school is established. Our effect would be driven by schools changing social norms.

We explore these alternative hypotheses in Figure 8, using either the first women who migrated to a city (left Panel) or the first notable women born in a city (right Panel) as a shifter in the likelihood of observing a finishing school. Using the first migration event as the "treatment period" in the left Panel, we find no correlation with access to secondary education. In contrast, the right hand side of Figure 8 reveals that the first native-born notable woman appears at the same time the first finishing school is established.

In combination, Figure 8b and 7, suggest that access to secondary education increased the representation of native and migrated women among the human capital elite in cities.

8.3 Initial human capital and social change

Finally, we analyze the role of human capital in social change by studying migration decisions of different groups of women. Migration of notable individuals is endogenous to location preferences; an artist migrates to theaters and a scientist to universities. In Table 6 we ask what type of migration increases social change to learn about the underlying conditions that promote it. We begin by showing that there is a positive correlation between migrating non-noble secular women

and schools: cities with schools and migrated women are more likely to have future non-noble secular women (Columns 1, 4, and 7). This interaction captures the general tendency for migration across cities, which we control for in the remaining columns.

Next we differentiate two underlying conditions that promote this multiplication effect. Female politicians arguing and promoting for the integration of women into society and women's rights need an audience that can read, but, not necessarily, critically engage with the topic. On the other hands, writers who spread critical ideas via books, journals, and reading events require a high human capital environment to be effective multipliers. We contrast these groups in Columns (2) and (3) of Table 6. While we find no additional impact of active politicians over schools itself, an active writer in a city with a finishing schools triples the effect of finishing schools: Compared to 14.3% of cities without finishing schools having a non-noble secular women, 26.5% of cities with finishing schools and 53.8% of cities with finishing schools and active writers have a future non-noble secular women. The share of women among the human capital elite is almost entirely driven by active women: the interaction effect increases women's representation by 159%.

Our results suggest that finishing schools produced women that critically engaged with their status quo (writers and activists) who migrated to places in which they acted as multipliers of social change. These cities also had finishing schools, suggesting that high human capital helps activists to create a social movement.

9 Finishing schools and outcomes of social change

When Dr. Martin Luther King Jr. and Susan B. Anthony entered the political stage, they paved the way for the civil rights and suffrage movements to succeed and change society. So far, we have shown that independent women, writers, and activists for women's rights entered the human capital after finishing schools opened. These women changed society in a way that is uncorrelated to economic or cultural change at the time. In this last section, we show that the social change brought forward by the new female human capital elite also altered cities in the long run.

As finishing schools contributed to the formation of a female human capital elite in cities (Table 1), women started to live independently and become writers and activists for suffrage and women's rights. If these women changed the role of women in society, we should observe long-run changes of in cities with finishing schools. We use three direct outcomes of social change: First, we digitize all letters to the editor of the feminist newspaper "*Frauen-Zeitung*", in which women's role in society was critically discussed. Second, we use establishment and membership data of local chapters of the women's rights movement in 1909 to capture the spread of critical ideas. Third, we study whether social change increased female representation in parliaments once suffrage was achieved.

We document the link between finishing schools and our outcomes of social change, estimating the following cross-sectional regression:

$$Y_c = \alpha + \beta \cdot \text{Finishing schools}_c + \gamma_1 X_{e,c} + \gamma_1 X_{r,c} + \gamma_1 X_{s,c} + \varepsilon_c \quad (1)$$

In this cross-sectional setting, unobservable factors, previously captured by city fixed effects and linear time trends, potentially impact our interpretation. Even controlling for economic, religious and educational covariates ($\boldsymbol{X}_c$), unobservable factors could be correlated with the establishment of finishing schools and the women's rights movement. When schools were built in areas with greater appreciation of women's role in society or women's education, our point estimate would overstate the impact of finishing schools. We assess the magnitude of this potential bias using three complementary strategies, all revealing a downward bias of our baseline estimates: First, we report the bias-adjusted point estimate from a bounding exercise in the spirit of Oster (2019), comparing coefficients from a regression without any controls and restrictions to a regression with a full set of controls in areas of religious competition, not more than 10km away from the religious divide in 1619. Second, we corroborate these findings and report point estimates from an instrumental variables strategy using monasteries in 1300 and religious competition as a shifter in the likelihood of establishing finishing schools. We discuss the first-stage relationship and exclusion restriction in Appendix H. Third, we compare the effect of finishing schools using propensity score matching on all covariates in Appendix H.1. All strategies reveal, if anything, a downward bias of our point estimates.

The historical literature on finishing schools suggests that religious competition was one determinant of the location of early finishing schools (Lewejohann, 2014). Yet, religious competition may exhibit a direct effect on our measures, even when controlling for the distance to the religious boundary. Thus, we limit our sample to cities within 10km of the borders marking the religious divide in 1619, i.e. to regions where religious competition was particularly pronounced in the early phases of finishing school openings. Limiting our sample to cities within 10km of the religious divide also enhances the comparability of cities. For instance, rather than comparing Berlin to Munich (600km due south), our strategy compares the neighboring cities of Hanover and Hildesheim.

We present our results linking finishing schools with the emergence of the women's rights movement in the late 19th century and with political representation of women throughout the 20th century in Table 7. We start by replicating our panel results in a cross-sectional setting using a lexicon of female writers from a different source (Panel A). We then examine the link between historical finishing schools in 1850 and the dissemination of critical ideas of women's role in society to the general public (Panel B), and the institutionalization of the women's rights movement by founding local chapters and recruiting female members (Panels C). We then turn to female representation in parliaments after women achieved the right to both vote and stand for parliament in 1919 (Panels D). [37]

[37] In Appendix I, we directly correlate the number of non-noble secular women in 1800 with political activity at the turn of the century and provide IV estimates using schools as an instrument for the number of non-noble secular women in each city. The elasticities between the number of non-noble secular women and writers, letters to the editor, or membership in a women's rights organization are greater than one, again suggesting that these women represent a

Encyclopedia of female writers We replicate our finding that finishing schools increase the number of writers in the first Panel of Table 7. Here, we exploit an encyclopedia that contains a cross-section of female writers active between 1840 and 1898, and recognized as important by their contemporaries (Pataky, 1898). In Table 7 column (1), we estimate a bivariate regression without any controls or restrictions, and report a 17 percentage point greater likelihood that a female writer was born in a city with a finishing school until 1850 (Panel A, Column 1). However, selection on unobservables remains a threat in this cross-sectional analysis.

To assess the potential severity of selection on unobservables, we report the bias-adjusted point estimate from a restricted estimation in column (2) and report estimates from an instrumental variables strategy. First, we include all previously defined controls and limit the sample to areas that, had been religiously competitive at the beginning of the Thirty Years' War in 1618. We estimate a similar point estimate of 0.180 (s.e. 0.039), a 138% increase over the likelihood of a female writer being born in cities without finishing schools (0.130 in this sample). The bias-adjusted point estimate and the IV estimates using monasteries in 1300 are of similar magnitude (0.185 and 0.176). In columns (3) and (4) of Table 7, we repeat this exercise with the number of recognized female writers. Again, the bias-adjusted point estimate confirms the OLS point estimate and suggests a 33% increase in the number of female writers, while the IV estimate is larger.[38] Despite using different data, the relative increase in the cross-section compared to cities without finishing schools (138%, Table 7, Panel A, Column 2) is similar to the point estimates in the panel setting (148%, Table 1, Panel C, Column 2).

Dissemination of ideas The first outcome of social change we consider is the dissemination of critical ideas. We digitize all letters to the editor of the first feminist newspaper in Germany, "*Frauen-Zeitung*" (1849-52), in Panel B. We use the place of residence of all letters and link this to the pesence of finishing schools in the nearest city. In Table 7 column (2), we estimate a bivariate regression with all controls and restrictions, documenting an increase in the likelihood of sending a letter of 0.122 (s.e. 0.037), an threefold increase over the mean. Only 3.8% of cities without finishing schools by 1650 sent letters to the "*Frauen-Zeitung*", compared to 16% of cities with finishing schools. We interpret this increase as evidence that critical ideas are more common in cities with finishing schools. While bias-adjusted point estimates reveal a similar magnitude (0.132), IV estimates reveal a downward bias of the OLS (0.249).

Women's rights associations Next, we turn to studying the institutionalization of the German women's rights movement as an outcome of social change. We digitize novel data on local chapters of women's rights associations from the Imperial Statistical Office (Kaiserliches Statistisches Amt, 1909). This source provides detailed establishment and membership data on more than 1,200 local chapters in 1909, irrespective of their political affiliation. The average local chapter in our data-

larger fraction of society.

[38] We use the transformation $\log(y+1)$ in columns (3) and (4). Due to the sparsity of our outcome data, we refer to columns (1) and (2) for inference. We explain our instrumental variables strategy in Appendix H in great detail.

set was established in 1898 and counted approximately 1600 members. This source also allows us to differentiate between different types of associations, e.g. female suffrage association and associations dedicated to improving women's educational opportunities.

We exploit this unique micro data in Panel C of Table 7. Controlling for covariates in column (2), we find that an additional finishing school by 1850 increases the likelihood that a city has any local women's rights association by 14 percentage points (Panel C), equivalent to a 50 percent increase over the mean in cities without finishing schools. The number of members organized in these local chapter even doubles (Column 4).[39] Again, while bias-adjusted point estimates reveal a similar magnitude (0.132 and 1.021), IV estimates reveal a downward bias of the OLS (0.378 and 2.868).

Female representation in parliament Finally, we study female representation in parliaments as an outcome of social change. As women represented a larger share of the human capital elite in cities (Table 1), they started to live independently and become writers and activists for women's rights, equal access to education and female suffrage. Thus, ence suffrage was achieved, this larger representation of women among the human capital elite should have translated into greater female political representation in parliaments.

We explore this hypothesis in Panel D of Table 7. To measure political representation, we collect the place of birth of all female members of parliament in the Weimar Republic (1919-1933, Panel D).[40] We report positive and significant coefficients when regressing an indicator for and the number of female politicians in all parliamentary elections since 1919 on the number of finishing schools in 1850.[41]

10 Conclusion

We set out to determine whether access to education contributes to the arrival of social change at the example of the women's rights movement in Germany. Following the literature on social movements (Wood and Tilly, 2012; Markoff, 2015) and the history of successful movements (Dr. Martin Luther King Jr for the civil rights movement or Susan B. Anthony for the suffrage movement) we argue that social change often starts from educated leaders who challenge the status quo. But for social change to emerge, leaders require a critical mass of followers that join to form a social movement.

We study the importance of one form of educational institutions in bringing about social change,

[39] These findings carry over when focusing on organziations dedicated to improving access to education: Cities with finishing schools are three times as likely to have such a women's rights organziation.

[40] Germany uses a list-based electoral system in which voters voted for the list of a party. Thus, female representation on this list is more likely driven by the woman's preference to be nominated, than by her electorate's preference, as it would be in a system where voters directly choose their representative.

[41] The findings are robust to estimating the impact in every period separately or jointly. The findings are not driven by large cities as the top 5 cities with the most finishing schools are Munich, Berlin, Obertaunuskreis, Landshut, and Dresden. Estimates increase without the largest 10 percent of the sample in 1600.

using the example of the arrival of finishing schools and the women's rights movement in Germany. In this setting, newly collected panel and cross-sectional data allows us to draw out the effect of education on the success of social movements: First, after cities established finishing schools, women started to represent a larger share of the political, intellectual, and economic elite ("human capital elite"), forming an activist nucleus of independent women, writers and activists for women's rights. These women contribute to a changing role of women in society as measured by critical discussions in newspapers, the emergence of women's rights associations, and female representation in parliament.

Using a wide range of empirical specifications our paper highlights the role of education in contributing to the emergence of a female human capital elite from which early activists for social change can emerge. Further, our empirical results suggest that a world without educational institutions but significant economic and cultural changes would not see the level or pace of social change we observe throughout history.

Taken together, our findings indicate that educational institutions, which foster the exchange of critical ideas and provide the space to form networks, can function as important catalysts for the formation of a human capital elite critically engaging with its status quo. Yet, education does not only benefit those receiving it; to the contrary, society as a whole can benefit when committed activists fight for and bring about social change.

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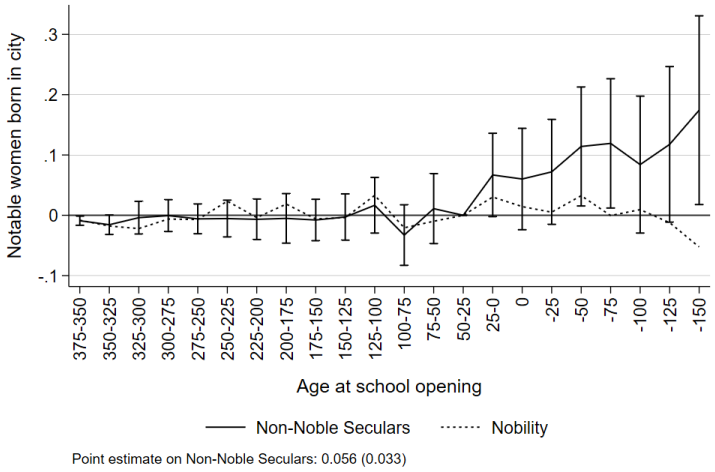
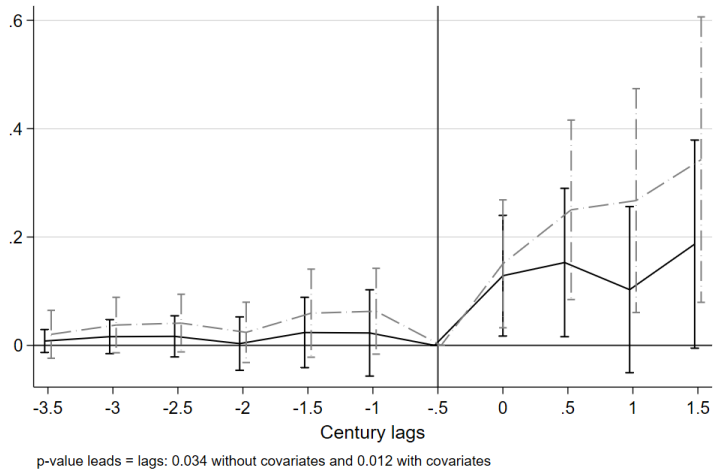
Figure 1: Event-Study: Impact of finishing school establishment on notable women



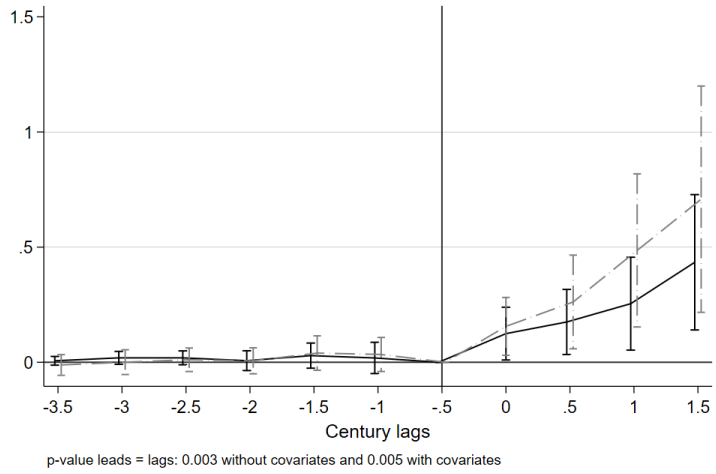
(a) Indicator function: Notable woman born in city

Event study results for *non-noble secular* women and women from the *nobility*. For each woman, we normalize the opening of the finishing school in her city of birth relative to her year of birth. A woman that was ten at the time of the finishing school is coded as the group (25-0), and thus has the chance of attending the school. A women that is born 50 years after the construction of the finishing school is coded as '-50'. The outcome is an indicator equal to one if a notable woman was born in a given city and period 90%-confidence intervals shown only for non-noble secular, the impact of nobility is indistinguishable form zero in all periods and specifications. City and fifty-year period fixed effects included.

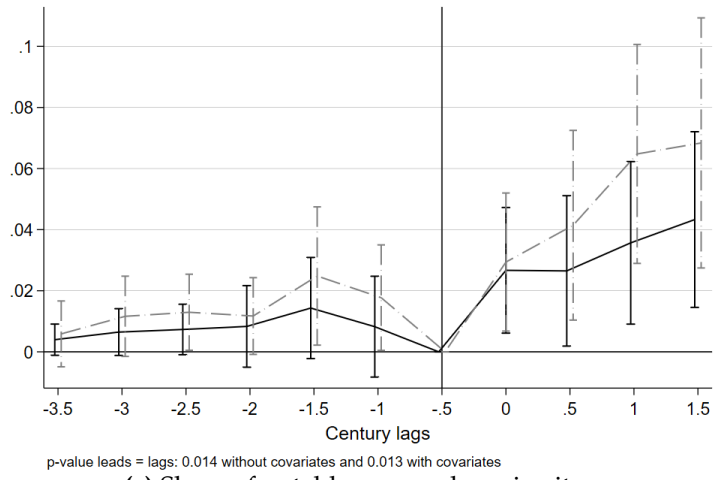
Figure 2: Event-Study: Impact of finishing school establishment on notable women



(a) Indicator function: Notable woman born in city



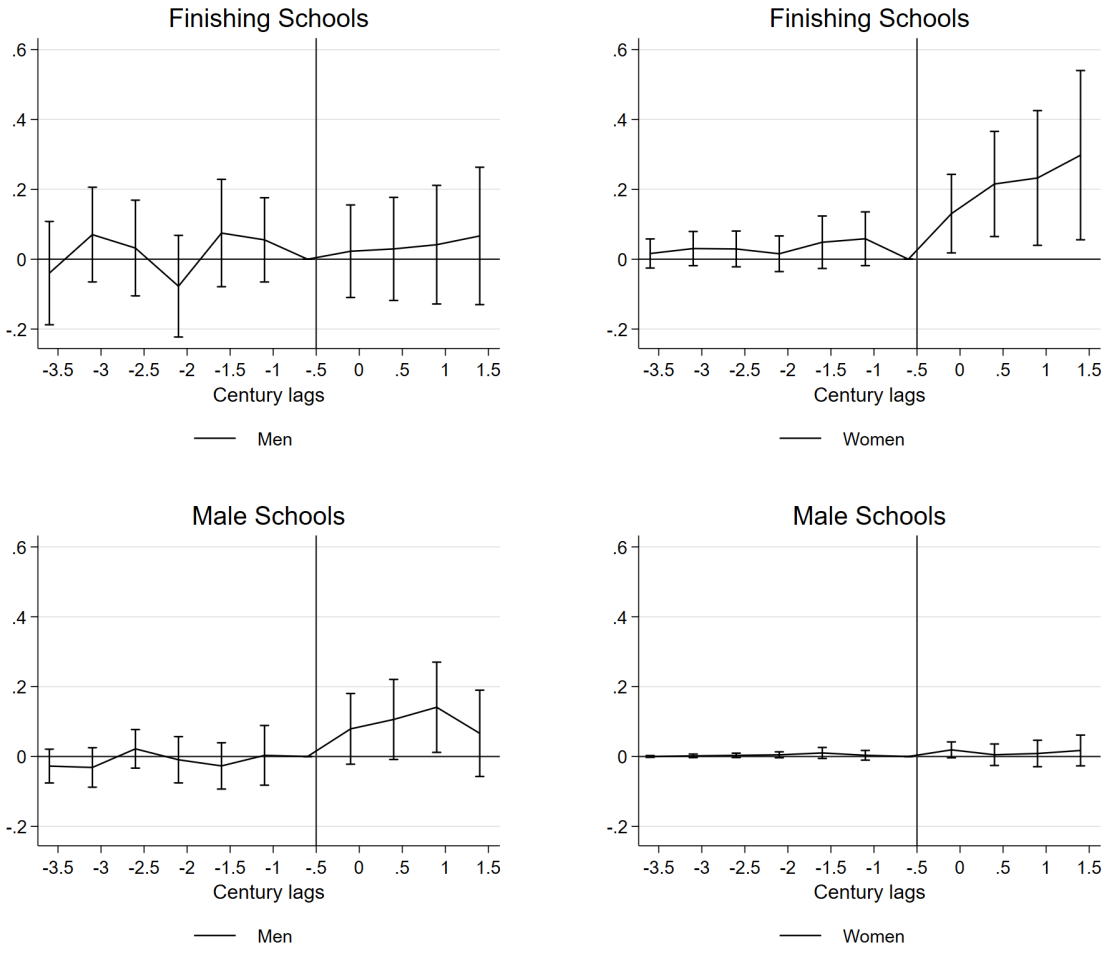
(b) Logarithm of the number of notable women born in city



(c) Share of notable women born in city

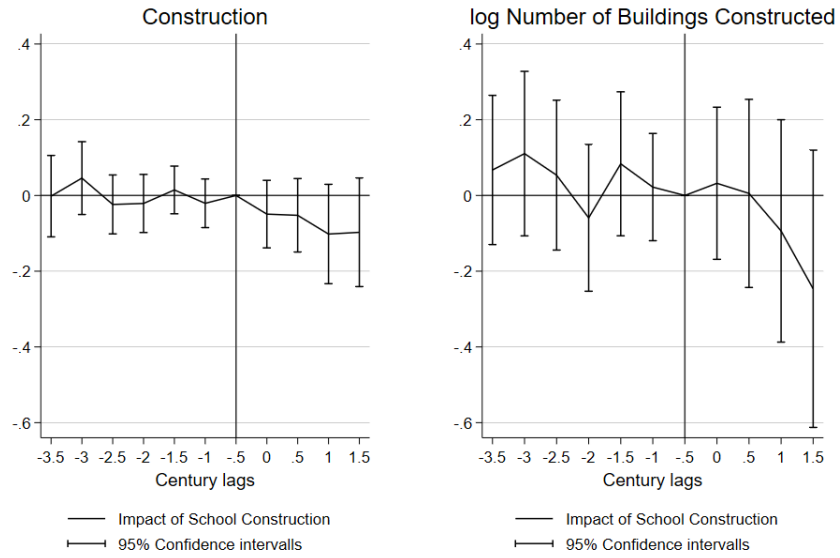
Event study results for *non-noble secular* women creating 50-year periods before and after the period of school construction '0'. In Figure a, the outcome is an indicator equal to one if a notable woman from the respective group was born in a given city and period. Figure b uses the natural logarithm of number of women born plus one. Figure c denotes the number of notable women by the number of notable individuals of all genders. Zero is the normalized time of opening of the first finishing schools in the city. The vertical line marks the reference period, which we choose to be 50 years prior to establishment of the school. City and period fixed effects included in all figures; The grey dashed line denotes estimates with covariates included. 95%-confidence intervals shown. Both lines are shifted for increased visibility, but estimated at the same period in time. Alternative approaches discussed in Section F.

Figure 3: Cross-gender impact of male and female schools

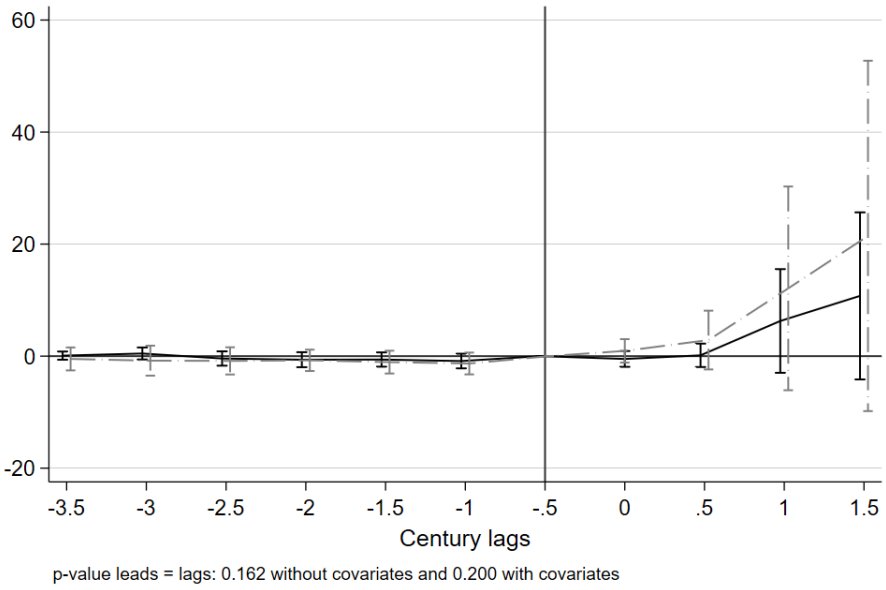


The impacts of male schools and finishing schools on notable women and notable men. The outcome in the two Panels on the left (right) is an indicator equal to one if a notable man (woman) was born in a given city and period. Zero is the normalized time of opening of the first gender-specific schools in the city. We take as comparison the 50-year period prior to the opening to ensure a clean control group that does not include women and men born before the opening of the first school. The vertical line marks the reference period, which we choose to be 50 years prior to establishment of the school. All figures include full economical and religious controls; educational controls are omitted. 95%-confidence intervals reported.

Figure 4: Impact of finishing schools on economic growth



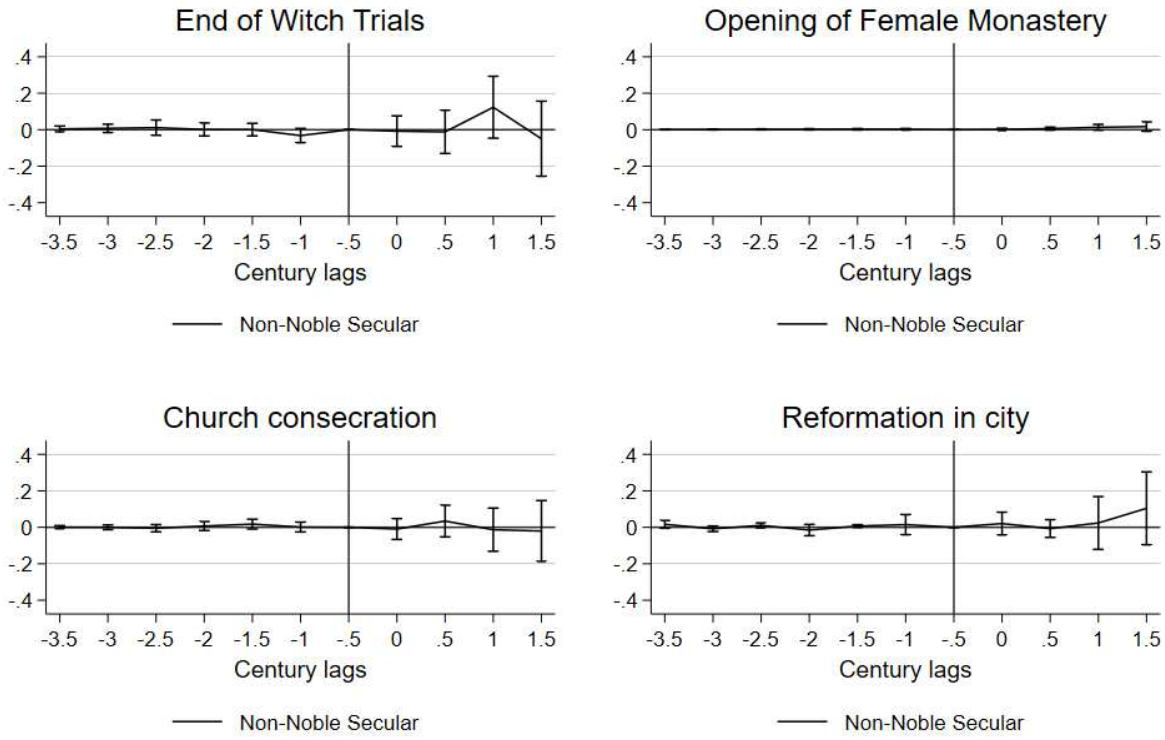
(a) Construction activity



(b) Population

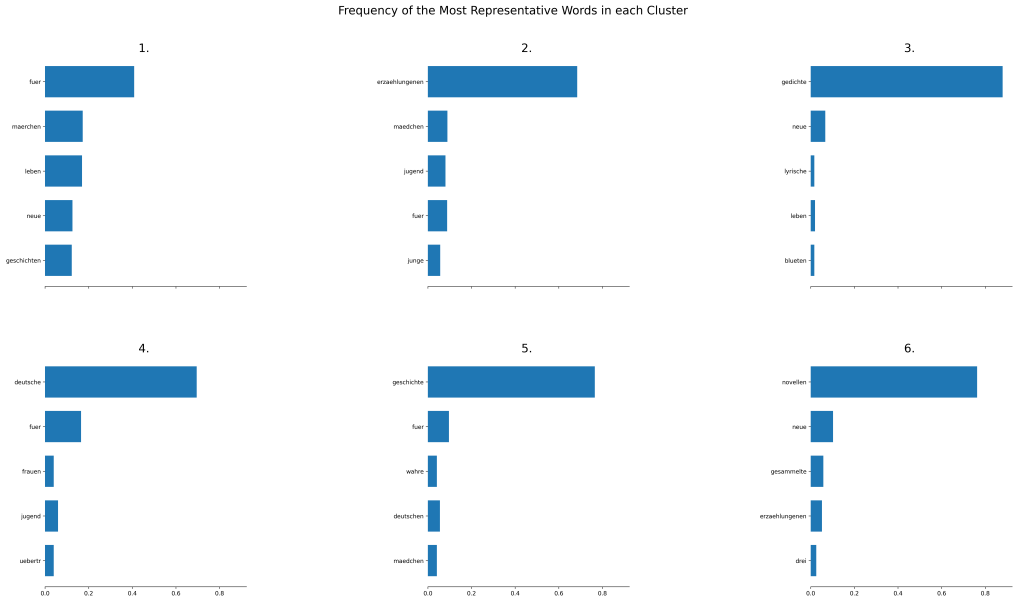
Top Panel a: The correlation between finishing schools and building construction. The outcome in the left panel is an indicator variable capturing construction activity in a given city and period, while the outcome in the right panel is the log number of buildings constructed plus one. All covariates from Table 1 column (2) included in both Figures. Bottom Panel b: The outcome is city size as recorded by Bairoch et al. (1988). All covariates from Table 1 column (2) included in the right figure. Both lines are shifted for increased visibility, but estimated at the same period in time. Zero is the normalized time of opening of the first finishing schools in the city. The vertical line marks the reference period, which we choose to be 50 years prior to establishment of the school. 95%-confidence intervals reported.

Figure 5: Impact of cultural change on notable women



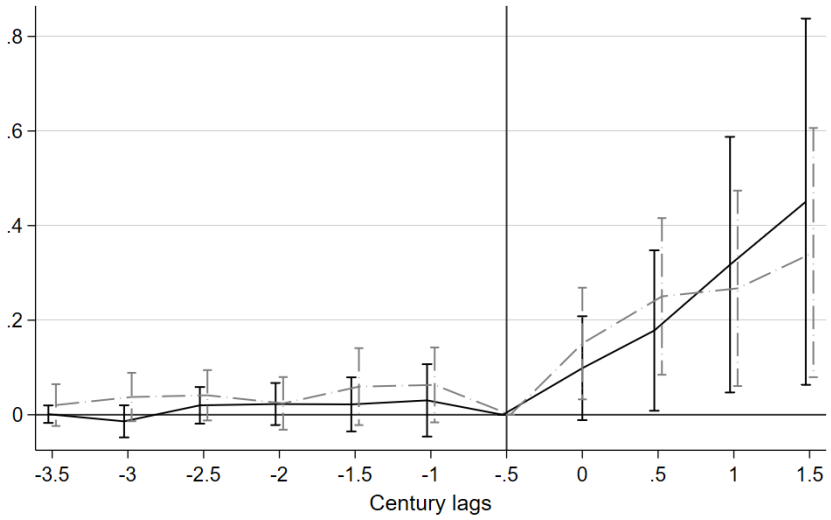
The correlation between notable women and cultural change. The outcome in all panels is an indicator equal to one if a non-noble secular woman was born in a given city and period. The vertical line marks the reference period, which we choose to be 50 years prior to the respective event. Economic and education controls included in the all figures. Religious controls are omitted when identifying the impact of Reformation. 95%-confidence intervals reported.

Figure 6: Word frequency plot for the book titles in (Pataky, 1898).



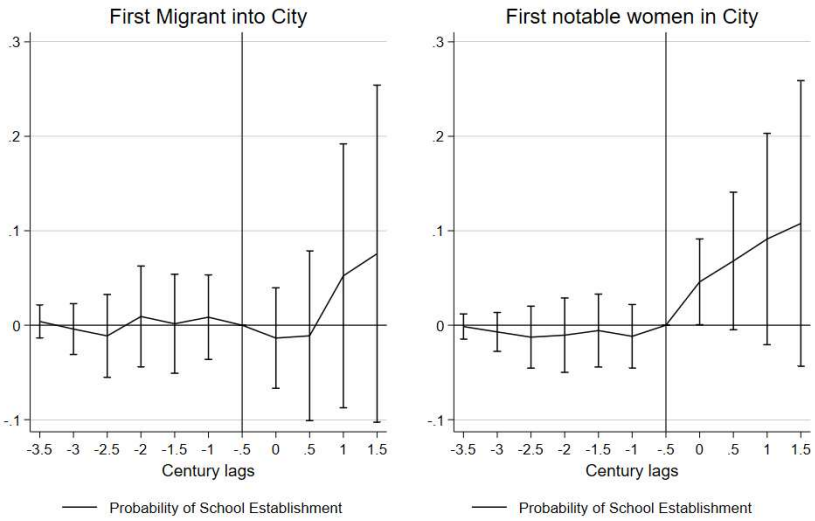
Fractions of speeches containing the word on the y-axis is given on the x-axis. Not shown is the frequency of each group: the most frequent group is 1 (80.7%), 2 (13%), and 3 (2.7%) of all book titles.

Figure 7: Impact of finishing school establishment on migrated women



Main results for women who migrated to the city with finishing schools, focusing in cities that ever established a school. Zero is the normalized time of opening of the first finishing schools in the city. The vertical line marks the reference period, which we choose to be 50 years prior to establishment of the school. Full economic, religious, and educational controls added in the grey dashed line. Both lines are shifted for increased visibility, but estimated at the same period in time. 95%-confidence intervals reported.

Figure 8: Impact of native and migrated women on subsequent notable women



The impact of the first notable female migrant on the birth of "native" notable women in a city is shown in the left Panel. Conversely, the right Panel shows the impact of the first "native" notable woman born in a city on the migration of notable women into the city. Zero is the normalized time of either the first migrated notable woman (left) or the first notable woman born in a city (right). Correspondingly, the outcome in the left panel is an indicator equal to one if a notable woman was born in a given city and period, while the outcome in the right panel is an indicator equal to one if at least one notable woman migrated to a city in a given period. The vertical line marks the reference period, which we choose to be 50 years prior to the respective event. Full controls included in both Figures. 95%-confidence intervals reported.

Table 1: Female representation among the human capital elite

	$\mathbb{I}[Women > 0]$			log Women			Share Women		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Non-Noble Seculars									
Finishing school$_{it}$	0.202***	0.134***	0.157***	0.301***	0.148***	0.165***	0.012**	0.012**	0.019***
	(0.029)	(0.031)	(0.038)	(0.050)	(0.040)	(0.047)	(0.005)	(0.006)	(0.006)
Mean, untreated	0.145	0.144	0.143	0.246	0.245	0.244	0.026	0.026	0.026
Panel B: Nobility									
Finishing school$_{it}$	0.024	0.005	-0.014	0.043*	0.017	0.000	0.002	-0.002	-0.010
	(0.021)	(0.024)	(0.028)	(0.022)	(0.022)	(0.024)	(0.007)	(0.009)	(0.010)
Mean, untreated	0.062	0.062	0.061	0.077	0.077	0.075	0.023	0.023	0.023
Unit trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
City covariates × period FE		Yes	Yes		Yes	Yes		Yes	Yes
Religious covariates × period FE		Yes	Yes		Yes	Yes		Yes	Yes
Educational covariates × period FE		Yes	Yes		Yes	Yes		Yes	Yes
Matching on 1600 covariates			Yes			Yes			Yes
Observations	9,312	9,240	8,856	9,312	9,240	8,856	9,312	9,240	8,856

Main results using a fixed-effects estimation and all cities in all periods. All regressions include a full set of city and period fixed effects. We consider three types of dependent variables to capture the extensive and intensive margin of the birth of notable women. $\mathbb{I}[Women > 0]$ is an indicator equal to one if a city had at least one notable woman born in this period. 'log Women' constitutes the natural logarithm of the number of women born plus one. 'Share Women' denotes the number of women by the number of men and women in the same category. We regress the number of non-noble secular women and women from the nobility born in a city, as defined in the top row, on our measure of access to secondary education. Columns (1), (4), and (7) constitute the baseline and include city and period fixed effects as well as city specific linear trends. In columns (2), (4), and (6) we interact city controls with period fixed effects to capture variation from economic, religious, and educational differences. We include the following controls measured in the 13th century: whether the city was a Hanseatic League or bishopric city and whether it had a Jewish presence and a pogrom. Additionally, we include the following controls from 1600: distance to Wittenberg, confessional battle in the vicinity, distance to the religious divide and Catholicism to capture religious differences. In addition, we control for the average temperature in 1650 to capture differential agricultural productivity, and hence income. City-level population in 1600 is included to capture different population effects and pre-existing male schools, universities in 1650, and the ruling houses are included to capture differential educational preferences. All covariates are interacted with period fixed effects. In columns (3), (6), and (9), we match cities on pre-1650 covariates to compare cities with the same pre-treatment characteristics. We include 86 match-specific fixed effects interacted with period fixed effects to confine the variation to within pre-treatment similar groups of cities. Standard errors clustered by city shown in parentheses. $* \ p < 0.10$, $** \ p < 0.05$, $*** \ p < 0.01$

Table 2: Placebo estimates on the importance of finishing schools:
Differential returns to education

	Access to female education			*Access to male education*		
	(1)	(2)	(3)	(4)	(5)	(6)
	Women	Men	Panel	Women	Men	Panel
Girl school	0.126**	-0.002				
	(0.049)	(0.072)				
Girl school X women			0.109**			
			(0.054)			
Boy school				0.006	0.095**	
				(0.008)	(0.038)	
Boy school X Men						0.071***
						(0.027)
City covariates × period FE	Yes	Yes		Yes	Yes	
Religious covariates × period FE	Yes	Yes		Yes	Yes	
City × period FE			Yes			Yes
Men × period FE			Yes			Yes
Adj. R2	0.415	0.424	0.496	0.376	0.253	0.384
Observations	1,421	1,421	2,996	2,172	2,172	4,582

Testing the differential impact of girl and boy schools on girls and boys, respectively. Columns 1, 2, 4, and 5 include city and period fixed effects, as well as economic and religious covariates interacted with period fixed effects. Columns 3 and 6 constitute a panel specification with city × time fixed effects in a window of four centuries before and two centuries after the establishing of a finishing school in city c (column 3) or male schools (Columns 6). All regressions include a full set of city and period fixed effects. The outcomes are indicators for the birth of notable women or men. We only use the binary indicator to ensure sufficient variation in all periods. Here, we construct a panel in which every city × period cell has two observations; one for women and one for men. This allows us to control for city × time fixed effects and period fixed effects of the opposite gender and estimate the impact of finishing schools on women, while non-linearly controlling for the trends in men and time-dependent city fixed effects. We regress either the opening of finishing schools, or the opening of finishing schools interacted with women (men) on an indicator equal to one if a city had at least one notable women (man) born in this period $\mathbb{I}[> 0]$. Standard errors clustered by city shown in parentheses. $* \ p < 0.10$, $** \ p < 0.05$, $*** \ p < 0.01$

Table 3: Placebo estimates on the importance of finishing schools:
Changing culture

	$\mathbb{I}[$*Non-Noble Secular* $> 0]$		log *Non-Noble Secular*		Share *Non-Noble Secular*	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: End of witch trials						
End of Witch Trial$_{it}$	0.014	0.009	0.024	-0.001	0.005	0.003
	(0.025)	(0.038)	(0.021)	(0.031)	(0.006)	(0.012)
Religious covariates × period FE		Yes		Yes		Yes
Panel B: Creation of a female monastery						
Female monastery opens$_{it}$	0.002	0.002	0.001	0.001	0.000	0.000
	(0.003)	(0.002)	(0.002)	(0.002)	(0.000)	(0.000)
Religious covariates × period FE		Yes		Yes		Yes
Panel C: Church consecration to a female Saint						
Consecration to a female saint$_{it}$	0.000	0.004	-0.003	0.008	0.007	0.009
	(0.026)	(0.029)	(0.021)	(0.023)	(0.010)	(0.009)
Religious covariates × period FE		Yes		Yes		Yes
Panel D: Reformation happening in city						
Reformation in City$_{it}$	0.023	-0.009	0.008	-0.012	0.008	0.000
	(0.025)	(0.017)	(0.019)	(0.013)	(0.005)	(0.004)
Religious covariates × period FE						
City covariates × period FE		Yes		Yes		Yes
Educational covariates × period FE		Yes		Yes		Yes

Results using a fixed-effects estimation in a window of four centuries before and two centuries after the end of witch trials in city c (Panel A), the creation of a female monastery (Panel B), a church consecration to a female Saint after 1650 (Panel C), and the arrival of the Protestant reformation in a city (Panel D). All outcomes are indicators equal to one if a notable woman from the respective group was born in a given city and period. All regressions include a full set of city and period fixed effects. Cities that ever had witch trials: 112; cities with a female monastery: 221; cities with a female church consecration: 152; cities that turned Protestant: 146. We include covariates as defined in Table 1 columns (2), (4), and (6) where indicated. We omit religious covariates in Panel D, as our ruler, distance to the religious divide and being Catholic in 1619 define whether a city becomes Protestant. Differences-in-Differences estimates confirm this picture and are presented in Table G.3. Standard errors clustered by city shown in parentheses. $* \ p < 0.10$, $** \ p < 0.05$, $*** \ p < 0.01$

Table 4: Occupations of women in the human capital elite

	Writers			Activists			Labor Force Participation		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Finishing school$_{it}$	0.152***	0.086***	0.110***	0.083***	0.061***	0.053***	0.180***	0.100***	0.123***
	(0.028)	(0.028)	(0.037)	(0.019)	(0.019)	(0.019)	(0.032)	(0.033)	(0.041)
Mean, untreated	0.088	0.088	0.087	0.018	0.018	0.018	0.140	0.139	0.137
Unit trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
City covariates × period FE		Yes	Yes		Yes	Yes		Yes	Yes
Religious covariates × period FE		Yes	Yes		Yes	Yes		Yes	Yes
Educational covariates × period FE		Yes	Yes		Yes	Yes		Yes	Yes
Matching on 1600 covariates			Yes			Yes			Yes
Observations	9,312	9,240	8,856	9,312	9,240	8,856	9,312	9,240	8,856

All regressions include a full set of city and period fixed effects. $\mathbb{I}[Women > 0]$ is an indicator equal to one if a city had at least one notable woman born in this period. We only use the binary indicator to ensure sufficient variation in all periods and occupational groups. We regress the incidence of writers, activists, and women active in the labor market on our measure of access to secondary education. Columns (1), (4), and (7) constitute the baseline and include city and period fixed effects as well as city specific linear trends. We include covariates as defined in Table 1 columns (2), (4), and (6) where indicated. In columns (3), (6), and (9), we additionally match cities on pre-1650 covariates to compare cities with the same pre-treatment characteristics. We include 86 match-specific fixed effects interacted with period fixed effects to confine the variation to within pre-treatment similar groups of cities. Standard errors clustered by city shown in parentheses. $* \ p < 0.10$, $** \ p < 0.05$, $*** \ p < 0.01$

Table 5: Fixed-effects results on the importance of finishing schools: Network formation across cities

	with Non-Noble Secular			*with Writers*		
	(1)	(2)	(3)	(4)	(5)	(6)
Finishing school$_{it}$	0.039***	0.023*	0.016	0.032***	0.025**	0.025**
	(0.013)	(0.013)	(0.013)	(0.010)	(0.011)	(0.012)
Mean, untreated	0.008	0.008	0.009	0.004	0.004	0.004
Unit trend	Yes	Yes	Yes	Yes	Yes	Yes
City covariates × period FE		Yes	Yes		Yes	Yes
Religious covariates × period FE		Yes	Yes		Yes	Yes
Educational covariates × period FE		Yes	Yes		Yes	Yes
Observations			Yes			Yes
Matching on 1600 covariates	9,312	9,240	8,856	9,312	9,240	8,856

All regressions include a full set of city and period fixed effects. We consider two types of dependent variables to capture the extensive and intensive margin of connections among notable women (Panel A) and writers (Panel B). $\mathbb{I}[Connections > 0]$ is an indicator equal to one if a city had at least one connected women born in this period. 'log Connection' constitutes the natural logarithm of the number of women with connections plus one. Columns (1) and (4) constitute the baseline and include city and period fixed effects as well as city specific linear trends. We include covariates as defined in Table 1 where indicated. In Columns (3) and (6) we match cities on pre-1650 covariates to compare cities with the same pre-treatment characteristics. We include 86 match-specific fixed effects interacted with period fixed effects to confine the variation to within pre-treatment similar groups of cities. All covariates are interacted with period fixed effects. Standard errors clustered by city shown in parentheses. $* \ p < 0.10$, $** \ p < 0.05$, $*** \ p < 0.01$

Table 6: Fixed-effects results on the importance of finishing schools:
Multiplicator effects

	$\mathbb{1}$[*Non-Noble Secular* > 0]			log *Non-Noble Secular*			Share *Non-Noble Secular*		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Finishing school$_{it}$	0.134***	0.134***	0.122***	0.086*	0.079	0.071	0.010	0.010	0.008
	(0.040)	(0.040)	(0.040)	(0.048)	(0.048)	(0.050)	(0.008)	(0.008)	(0.008)
School × Active Non-Noble Secular	0.049	0.044	-0.137*	0.190**	0.123	-0.047	0.023*	0.024*	-0.004
	(0.062)	(0.065)	(0.081)	(0.085)	(0.078)	(0.097)	(0.013)	(0.014)	(0.015)
School × Active Politician		0.010			0.244			-0.005	
		(0.089)			(0.155)			(0.016)	
School × Active Writer			0.273***			0.349***			0.040**
			(0.082)			(0.123)			(0.016)
Unit trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
City covariates × period FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Religious covariates × period FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Educational covariates × period FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Matching on 1600 covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	8,856	8,856	8,856	8,856	8,856	8,856	8,856	8,856	8,856
Mean dependent variable	0.143	0.143	0.143	0.244	0.244	0.244	0.026	0.026	0.026
Mean active writers		0.086	0.178		0.086	0.178		0.086	0.178

All regressions include a full set of city and period fixed effects. We consider three types of women that migrated to a city: Any non-noble secular (second row), writers (third row), and politicians (fourth row). We define migrated women as women who are not born in the same city historians have noted their activity in. We include city and period fixed effects as well as city specific linear trends in all columns, as well as interact city controls with period fixed effects to capture variation from economic, religious, and educational differences. We also match cities on pre-1650 covariates to compare cities with the same pre-treatment characteristics throughout all specifications. We include 86 match-specific fixed effects interacted with period fixed effects to confine the variation to within pre-treatment similar groups of cities. All covariates are interacted with period fixed effects. Standard errors clustered by city shown in parentheses. $* \ p < 0.10$, $** \ p < 0.05$, $*** \ p < 0.01$

Table 7: Long-term impact of finishing schools on the women's rights movement and political representation

	$\mathbb{I}[>0]$		log Number	
	(1)	(2)	(3)	(4)
Panel A: Encyclopedia of female writers				
Finishing schools	0.171***	0.180***	0.358***	0.329***
	(0.027)	(0.039)	(0.039)	(0.051)
R-squared	0.148	0.374	0.406	0.576
Mean, untreated	0.208	0.130	0.274	0.137
Bias-Adjusted β		0.185		0.300
IV estimate using Monasteries in 1300		0.174		0.479
Panel B: Leserbriefe, Frauenzeitung, 1849–1852				
Finishing schools	0.100***	0.122***	0.192***	0.241**
	(0.017)	(0.037)	(0.051)	(0.097)
R-squared	0.121	0.370	0.151	0.353
Mean, untreated	0.062	0.038	0.104	0.061
Bias-Adjusted β		0.132		0.266
IV estimate using Monasteries in 1300		0.246		0.412
Panel C: Women's rights organizations				
Finishing schools	0.158***	0.126**	1.083***	0.937***
	(0.025)	(0.049)	(0.136)	(0.271)
R-squared	0.129	0.354	0.204	0.410
Mean, untreated	0.189	0.153	37.799	34.160
Bias-Adjusted β		0.111		0.850
IV estimate using Monasteries in 1300		0.264		1.674
Panel D: Member Parliament, 1919–1933				
Finishing schools	0.103***	0.101***	0.133***	0.105***
	(0.017)	(0.034)	(0.027)	(0.035)
R-squared	0.107	0.418	0.195	0.472
Mean, untreated	0.066	0.038	0.073	0.053
Bias-Adjusted β		0.100		0.091
IV estimate using Monasteries in 1300		0.155		0.217
City Covariates		Yes		Yes
Religious covariates		Yes		Yes
Educational covariates		Yes		Yes
Observations	388	183	388	183
Bandwidth		10		10

Cross-sectional results using all observations in odd columns and sample limited to 10 km of the religious boundary in 1619 in even columns. In each Panel we regress an indicator variable for the existence and the natural logarithm plus one of the number of instances on the number of finishing schools. In Panel A, we estimate whether finishing schools increase the likelihood of important women being born in cities in a cross-section, replicating our main results. In Panel B, we estimate whether finishing schools increase the likelihood and number of letters written from city c to the first active feminist newspaper in Germany. In Panel C, we analyze whether finishing schools increase the likelihood and member count of local chapters of the women's rights organizations in city c. In Panel D, we estimate the impact of finishing schools on the likelihood and number of female members of parliament from their birthplace c. We include covariates as defined in Table 1 columns (2), (4), and (6) where indicated and limit the sample to within 10km of Germany's denominational divide in 1619 to capture areas with stronger religious competition in columns (2) and (4). *Bias-Adjusted* β follows the procedure laid out in Oster (2019) assuming $R^{max} = 1.3\tilde{R}$ and $\delta = 1$. *IV estimate using Monasteries in 1300* instruments finishing schools using existing monasteries in 1300. The F-Test on the first stage is 9.75. All estimates are significant. We discuss the first-stage relationship and the exclusion restriction in great detail in Online Appendix H. Standard errors clustered by city shown in parentheses. $* \ p < 0.10$, $** \ p < 0.05$, $*** \ p < 0.01$

For Online Publication: Appendix
25th May 2023

This appendix provides additional evidence supporting our main hypothesis that finishing schools contributed to the emergence of the German women's rights movement. We cover the following topics:

A Record keeping in the *Neue Deutsche Biographie (NDB)*

Our main results show an increase in the representation of women among the human capital elite – as measured by notable women recorded in the NDB – following the establishment of finishing schools. For further information on the Neue Deutsche Biographie (NDB) and its construction, we refer to the excellent online appendix to "Public Goods Institutions, Human Capital, and Growth: Evidence from German History", by Jeremiah Dittmar and Ralf Meisenzahl in The Review of Economic Studies, Volume 87, Issue 2, March 2020 (DOI: https://doi.org/10.1093/restud/rdz002).

In this Appendix we focus on two aspects of the NDB: Data construction of our final outcome and whether this increased representation of women is driven by changes in reporting.

A.1 Data Construction

We downloaded the entire corpus of the NDB in October 2022. Since we are interested in linking individuals to their birth year and location, we iteratively searched by birth year from 800 - 1975 and downloaded the results. We arrived at 65,477 women and 415,586 men.[42] For some individuals the exact birth year is unknown; the data then shows them twice: once in the year in which they were mentioned "erwähnt" and 120 years prior. The unique number of women (men) thus decreases to 63,124 (388,459).

In the second step we cleaned birth years and death years to arrive at a reasonable age distribution. We constructed life expectancy per century based on individuals that we observe, and use it to construct birth years for people with known death years or last mentioned years. Following this procedure we arrive at 62,783 women (383,582 men) with known birth years with an average birth year of 1868 (1822). The average age for 36,262 women (296,756 men) is 66 (68).

In the next step we complement the recorded latitude and longitude for birthplaces with geocoded locations of their recorded birthplaces and drop all observations without a birthplace. Our final selection step involves only selecting individuals that were born within the modern day boundaries of Germany. This results in 20,226 women and 114,891 men.

Following the 100 years of women's right to vote in Germany, the NDB was significantly expanded. This expansion includes less known and notable individuals that were not part of the original Allgemeine Deutsche Biographie (ADB) (1871) or the NDB (1951). We thus repeat the above step for women that were previously selected and arrive at 10,633 women, of which 9,449 were unique, 9,111 have a birth year ($\bar{y}$ =1810), 8,085 have full information on age ($\bar{y}$ =69), 4,474 have information on birth places, and 2,732 were born in modern day Germany. We apply the same procedures to men.

We can use these three periods, women in the ADB, women in the NDB, and women added later, to hypothesize about the relative importance of women for social change. A women born in 1800, that is included in the ADB is likely more important and known for her contributions to

[42] We added data checks to ensure that we indeed arrive at 100% of the entire data by comparing the results for each birth year with the unique entries that we downloaded.

German culture than the same women born in 1800, but only included recently. We record 142 women (4,435 men) with full birth years and birth places in Germany in the ADB, versus 2,732 (43,574 men) in the NDB and 18,865 (114,891 men) later. When we condition on being born before 1800, we record 89 in the ADB, 541 in the NDB, and 3,327 later. [43]

We present this sensitivity analysis in Table A.1, conditioning on being born before 1800. Comparing the indicator of whether a women was born in a city, only 3.8% of cities without finishing schools record a notable women in the ADB, but 16% of cities with finishing schools did (+321%). The relative effect sizes is smaller for the NDB at 225% (7.8% to 25,4%), but drastically smaller for the most recent sample (30% in Column 3). A similar ordering can be seen when considering shares of women relative to men in columns (7)–(9): While access to secondary education increases the share of women from 0.8% to 2.8% in the ADB (1.5% to 4.5% in the NDB), the share of women in the latest sample (but born before 1800) only increases from 4.2% to 5.1%.

We use the NDB with all years in our main result for two reasons. First, it is the definitive biographical dictionary of economic, cultural, and political figures in Germany (Hockerts, 2008; Dittmar and Meisenzahl, 2020). Second, it builds on the ADB and increases its sample size, while retaining the original focus on notable individuals. This allows us to estimate longer post-treatment effects and enable us to differentiate between occupations. Our choice is supported by similar relative effect sizes between the ADB and NDB. We decide against using the ADB, or restricting our sample to women born before 1850 or 1800, because we believe that while the effect sizes indicate considerable heterogeneity in "importance", we have no qualitative evidence to support that claim.

Table A.1: ADB, NDB, and later:
Varying the inclusion date

	$\mathbb{I}[Women > 0]$			log Women			Share Women		
	(1) ADB	(2) NDB	(3) Post-NDB	(4) ADB	(5) NDB	(6) Post-NDB	(7) ADB	(8) NDB	(9) Post-NDB
Finishing school$_{it}$	0.122**	0.176***	0.100*	0.138***	0.215***	0.193**	0.020**	0.030**	0.009
	(0.049)	(0.062)	(0.056)	(0.051)	(0.069)	(0.080)	(0.009)	(0.012)	(0.010)
Unit trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
City covariates × period FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Religious covariates × period FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Educational covariates × period FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Matching on 1600 covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	7,749	7,749	7,749	7,749	7,749	7,749	7,749	7,749	7,749
Mean	0.038	0.078	0.300	0.047	0.125	0.707	0.008	0.015	0.042
Included if born before:	1,800	1,800	1,800	1,800	1,800	1,800	1,800	1,800	1,800

Results using a fixed-effects estimation and all cities in all periods, varying the inclusion dates. Columns (1), (4), and (7) only include women that were noted in the first bibliography, the ADB by 1871. Columns (2), (5), and (8) only include women in the ADB and NDB (1951). Columns (3), (6), and (9) include women that were added up until 2022. We consider three types of dependent variables to capture the extensive and intensive margin of the birth of notable women. $\mathbb{I}[Women > 0]$ is an indicator equal to one if a city had at least one notable woman born in this period. 'log Women' constitutes the natural logarithm of the number of women born plus one. 'Share Women' denotes the number of women by the number of men and women in the same category. We include city and period fixed effects as well as city specific linear trends in all columns, as well as interact city controls with period fixed effects to capture variation from economic, religious, and educational differences. We also match cities on pre-1650 covariates to compare cities with the same pre-treatment characteristics throughout all specifications. We include 86 match-specific fixed effects interacted with period fixed effects to confine the variation to within pre-treatment similar groups of cities. Standard errors clustered by city shown in parentheses. $* p < 0.10, ** p < 0.05, *** p < 0.01$

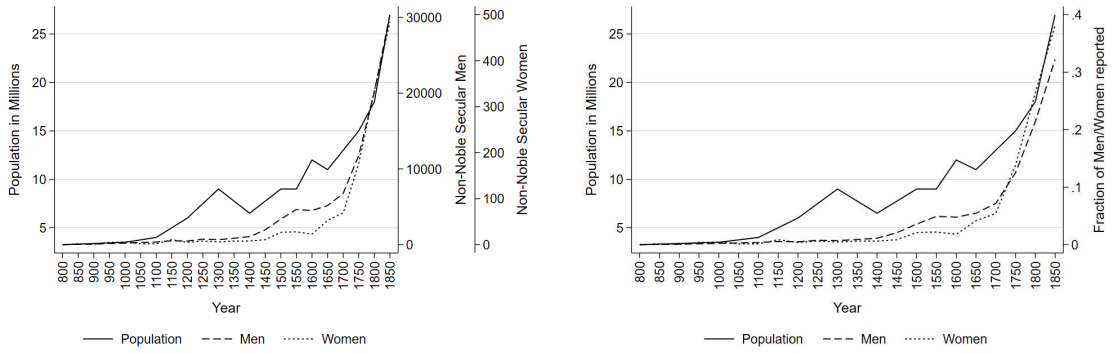
[43] The increase in shares of women noted in the ADB, NDB, and today provides further evidence of the heterogeneity with respect to importance.

Instead we take the strong, robust, and consistent relationship across samples and inclusion dates as further evidence that access to secondary education matters for women's representation among the human capital elite.

A.2 Women's inclusion into the NDB

If women's inclusion in the NDB increased disproportionately over time, estimates of the impact of finishing schools might be confounded by the general effect of time. In Figure A.1, we provide direct evidence against this concern: the recording of notable women and men in the NDB followed the same time trend, which is, moreover, in line with general population growth. This motivates our use of the *share* of notable women among all notable women and men as dependent variable and our interpretation of the data in the main text.

Figure A.1: Number of women and men in the NDB relative to total population



(a) Women and men in our data. **(b)** Share of non-noble secular women and men.

The left panel depicts the population of Germany in its modern boundaries (solid line), the number of notable men (right axis, dashed line) and the number of notable women born in each period (right axis, dotted line). All lines follow the same trend, suggesting that our estimated impacts are not driven by a change in reporting. The right panel again depicts the population of Germany in its modern boundaries as well as the share of all *non-noble secular* women (men) among all notable women (men) born in each period. This indicates that also in the subcategory of *non-noble secular* individuals the NDB exhibits no differential time trends in reporting between women and men.

In Figure A.1, we compare the trends of total population in Germany based on McEvedy and Jones (1978) to the trends in the number of men and women recorded in the NDB. While the levels are different, all time series follow the same trend over time suggesting no change in reporting that could affect our data. The right panel in Figure in A.1 shows that also the fraction of non-noble secular women among all women in our data increased similar to the increase among notable men: women's non-noble secular shares went up from 10% to 80% with the men's increase being 35% to 90%. Again, the pattern closely follows population, so that calculating the share of women born in each city and period, relative to all notable women and men in that city and period, provides a good measure of the human capital elite as it explicitly controls for trends.

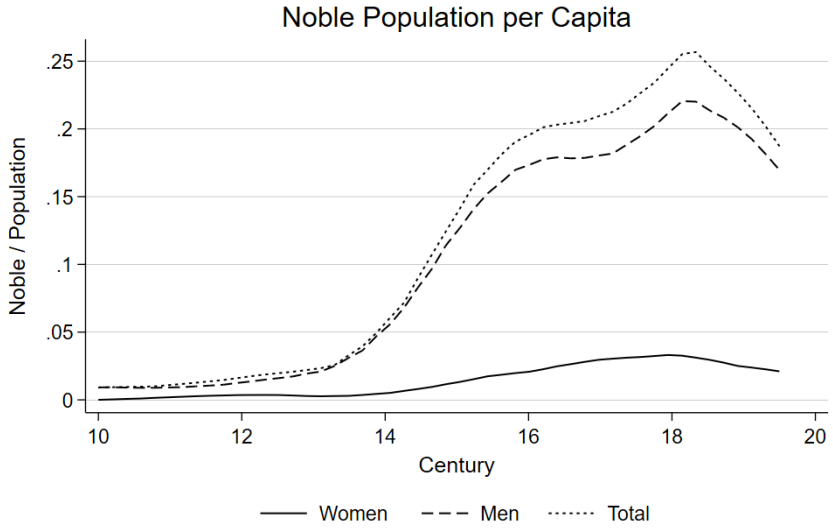
A related concern is differential reporting between cities with and cities without finishing schools in the NDB. Specifically, finishing schools may have improved record keeping on notable women rather than increased women's share among the human capital elite. We offer two arguments against this interpretation: first, as shown in Figure 2 in the main text, we find no impact of finishing schools on notable women from the nobility; if finishing schools merely improved record keeping on notable women, one might reasonably expect this to manifest also in an increased representation of women from the nobility.

Second, if finishing schools merely improved record keeping in the NDB, this ought to show up in differential pre-trends, as a purported record-keeping effect would presumably also extend to the women who contributed to the founding of finishing schools. However, as shown in Figure 2 and as emphasized in Appendix F we find strong evidence against differential pre-trends.

A related concern is that the number of notables might be constant over time, yielding the placebo estimates on royals mute. Figure A.2 provides evidence against this concern as the share of nobility is increasing for both genders.[44] When using royals as a placebo we thus test whether the growth rate of nobility is different between cities.

Our implicit assumption is that population growth rates across nobles and non-nobles are correlated. If the nobility has a replacement rate of two, implying a constant number of nobles, and non-noble people have a replacement rate of four, implying population growth, the placebo does not capture population growth. If however, the replacement rates of nobles is always half, or double that of the non-noble people, they are correlated, and serve as a proxy for population growth.

Figure A.2: Noble population per capita



Increase in the frequency of noble individuals per 1,000 inhabitants in the NDB.

[44]The Human Record Database https://humanrecord.org/ (Nekoei and Sinn, 2021) also highlights an increase in the rise of self-made women.

Table A.2: Summary statistics: Finishing schools and notable women

	Cities		
	Without finishing schools (N=259)	With finishing schools (N=129)	Percent of sample
Data: Female finishing schools in Germany			
Finishing schools	0	1.6	
Data: Neue Deutsche Biographie			
	Non-Noble Secular (NNS)		
Total	516	1656	0.795
Academic	66	274	0.124
Artist	235	887	0.411
Founders	18	42	0.022
Medicine	22	79	0.037
Not assigned	8	35	0.016
Occupations	129	353	0.176
Politics	89	223	0.114
Sports	2	10	0.004
Teacher	81	239	0.117
Writer	233	629	0.316
	Activists (also NNS)		
Activists	31	74	0.038
	Nobility		
Royals, Wifes, Relatives	138	308	0.163
	Nuns		
Religion	51	89	0.051
	Data: (Bairoch et al., 1988)		
Population (1600)	2.3	5.2	
Population (1850)	8.0	15.1	

The first row reports the average number of schools in cities without historical finishing schools (259) and with historical finishing schools (129). The average number of finishing schools in cities with schools is 1.62, with 85 cities having one school, 29 cities having two schools, and 15 cities having three or more schools. The subsequent rows detail the absolute number of notable women in each sub-group and their share of the total. Women can belong to multiple groups. The last row indicates the average city size in thousands. Cities that have a finishing school by 1850 are nearly twice the size in 1600. While this relationship is very similar for women from the Nobility (2.2) and Nuns (1.7), Non-Noble Secular women (writers) are 3.6 (2.8) times more likely to appear in cities with finishing schools. Cities with finishing schools are on average 2.2 times larger than cities without in 1600, a distance that shrank to 1.9 in 1850. We control for the difference in population by interacting 'Population in 1600' with period fixed effects in all regression with control variables. We show that differential population growth is not an outcome of finishing school construction (Figure D.1). NNS, Nobility, and Nuns do not add up to 100% as 26 women are recorded as being noble and a nun.

B Alternative empirical specifications and economic growth

We continue by documenting the robustness of our results presented in Table 1 in the main text. To this end, we start by the most basic two-way fixed effect design, only including period and city fixed effects in column (1) of Table B.1. In the four subsequent columns we individually add and remove a city-specific trend as well as city, educational, and religious covariates. As expected, the largest drop originates from city covariates, and specifically controlling for population. These covariates are responsible for almost the entire difference between the baseline and full specifications. This effect is largely an extensive margin effect, as when we drop all cities without population figures in 1600, we do not observe a change in the point estimates. The city-specific trend, while changing the point estimate significantly between columns (1) and (2), does not affect the point estimates when already controlling for covariates (columns 6 vs 7). We thus conclude that our estimates do not rely on the inclusion of city-specific trends or a specific specification.

Table B.1: Fixed-effects results on the importance of finishing schools:
Sensitivity to covariates

	Baseline	with trends	with covariates				Full
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Non-Noble Seculars, $\mathbb{I}[Women > 0]$							
Finishing school$_{it}$	0.266***	0.202***	0.154***	0.267***	0.240***	0.160***	0.134***
	(0.028)	(0.029)	(0.028)	(0.029)	(0.029)	(0.029)	(0.031)
Panel B: Non-Noble Seculars, log Women							
Finishing school$_{it}$	0.398***	0.301***	0.183***	0.397***	0.353***	0.197***	0.148***
	(0.056)	(0.050)	(0.040)	(0.056)	(0.054)	(0.040)	(0.040)
Panel C: Non-Noble Seculars, Share Women							
Finishing school$_{it}$	0.021***	0.012**	0.016***	0.021***	0.020***	0.016***	0.012**
	(0.004)	(0.005)	(0.005)	(0.004)	(0.005)	(0.005)	(0.006)
Unit trend		Yes					Yes
City covariates × period FE			Yes			Yes	Yes
Educational covariates × period FE				Yes		Yes	Yes
Religious covariates × period FE					Yes	Yes	Yes
Observations	9,312	9,312	9,312	9,288	9,264	9,240	9,240

Main results using a fixed-effects estimation and all cities in all periods. We consider three types of dependent variables to capture the extensive and intensive margin of the birth of notable women. $\mathbb{I}[Women > 0]$ is an indicator equal to one if a city had at least one notable woman born in this period. 'log Women' constitutes the natural logarithm of the number of women born plus one. 'Share Women' denotes the number of women by the number of men and women in the same category. We regress the number of non-noble secular women, and teachers and writers born in a city, as defined in each panel, on our finishing school variable. Column (1) denotes a primitive baseline, only including time and city fixed effects. Column (2) adds linear time trends to ascertain their impact on the point estimate. In columns (3)-(6), we add various covariates interacted with period fixed effects, first individually then jointly, without the linear time trends. We include the following controls measured in the 13th century: Whether the city was a Hanseatic League or bishopric city and whether it had a Jewish presence and a pogrom. Additionally, we include the following controls from 1600: distance to Wittenberg, confessional battle in the vicinity, distance to the religious divide between Protestantism and Catholicism to capture religious differences. In addition, we control for the average temperature in 1650 to capture differential agricultural productivity, and hence income. City-level population in 1600 is included to capture different population effects and pre-existing male schools, universities in 1650, and the ruling houses are included to capture differential educational preferences. All covariates are interacted with period fixed effects. In column (7), we then add linear time trends to show that linear time-trends do not impact the precision of our estimates. Standard errors clustered by city shown in parentheses. $* \ p < 0.10$, $** \ p < 0.05$, $*** \ p < 0.01$

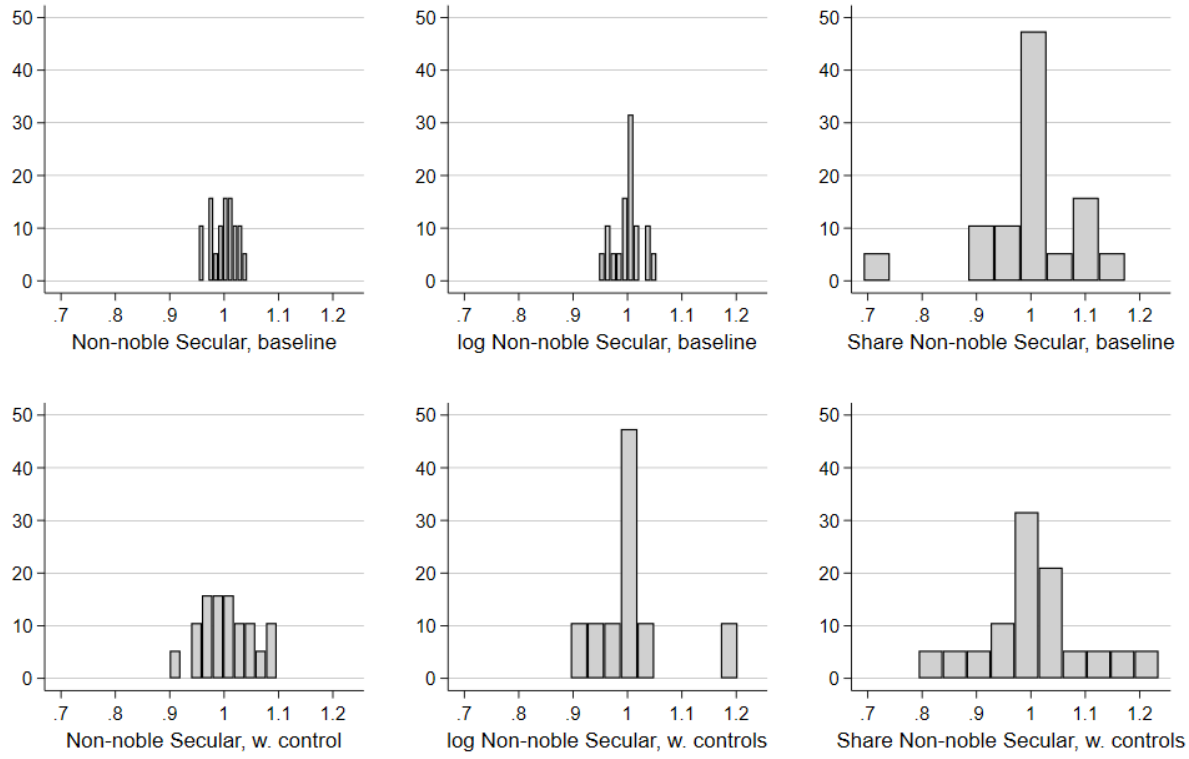
B.1 Sensitivity to dropping observations

In a recent paper Broderick et al. (2020) stressed the importance of assessing the validity of results by analyzing their robustness to outliers. We implement this robustness test as follows: we drop entire sets of cities belonging to one ruling house rather than dropping individual cities (1 out of 388). With this procedure, we drop on average 18 cities, with the two largest sets of cities being ruled by the Catholic clergy (114) and the House of Hohenzollern (52). Since these two sets of cities also capture the distinction between Catholic and Protestant cities almost perfectly, the results of this analysis also document that our findings are not driven by cities from either denomination alone.

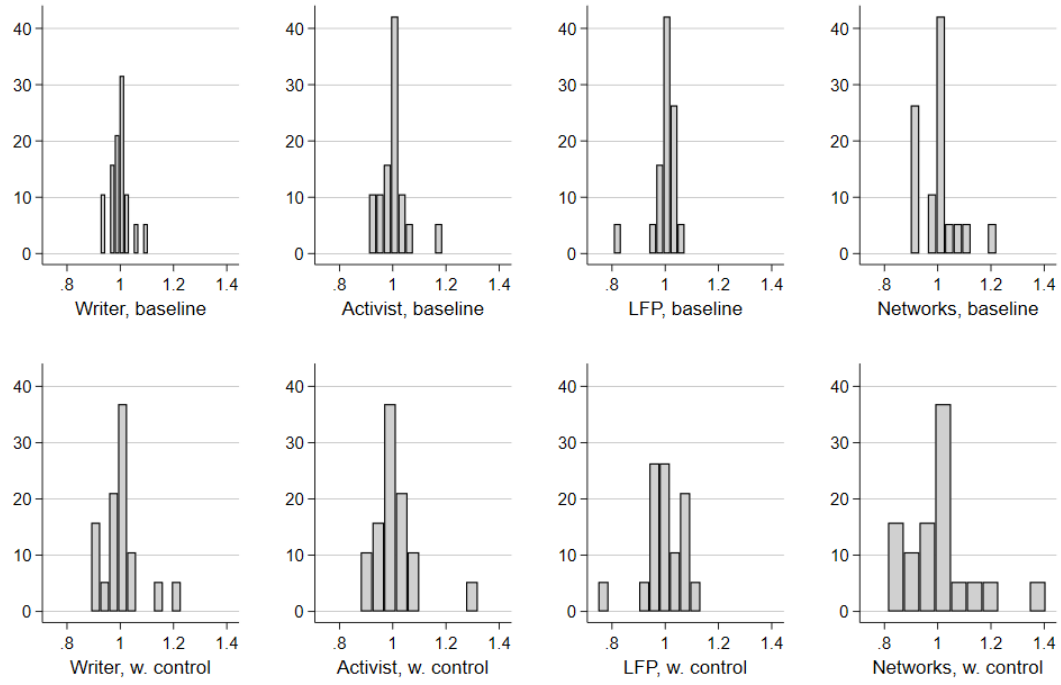
In Figure B.1 and B.2, we present all outcomes in all specifications corresponding to Tables 1, 4, 5 and 7. The x-axis measures the ratio between a restricted estimate when a set of cities is dropped and the original estimate from the corresponding table. If the restricted estimate remains unchanged, this ratio is one. It is 1.5 if the restricted estimate is 50% larger than the original, and 0.5 if the restricted estimate is 50% smaller than the original. We do this for 22 sets of cities belonging to different rulers and find a minimum of 0.8 and a maximum of 1.3 for the share of non-noble secular in the panel setting. These figures suggest that our panel estimates are highly robust to potential outliers as they only vary within 30% of the original effect size. The corresponding numbers for the cross-sectional regressions are 0.7 (for the log number of educational women's rights associations, with controls) and 1.6 (for the members of parliament 1949-2017, with controls).

Overall, the density plots reveal a stable pattern around the estimated mean, suggesting that our results are not driven by individual cities or sample selection.

Figure B.1: Senstivity to dropping sets of cities:
Panel outcomes



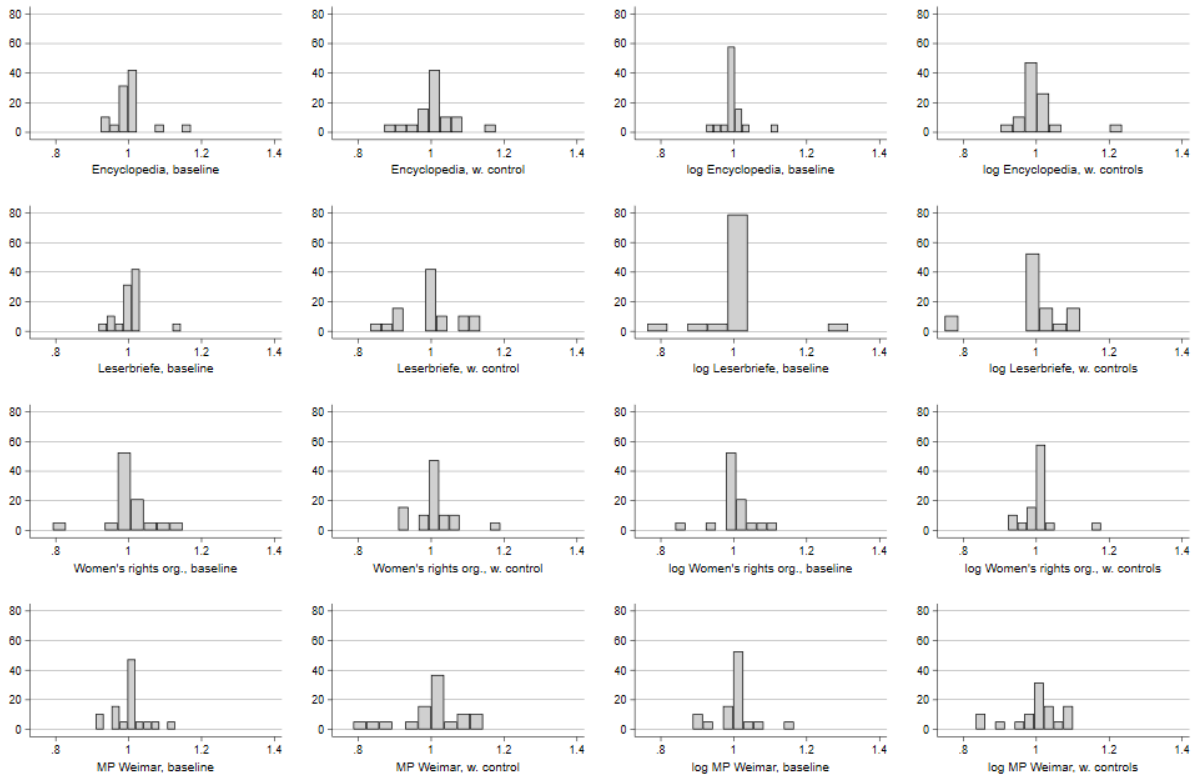
(a) Main outcome: Female representation among the human capital elite.



(b) Occupations of women.

The x-axis measures the ratio between the restricted point estimate when dropping one of 22 sets of cities and the corresponding original estimate in Table 1. This ratio is one, if the restricted estimate is unchanged, 1.5 if the restricted estimate is 50% larger than the original, and 0.5 if the restricted estimate is 50% smaller than the original. The sum of all bars is 100%.

Figure B.2: Senstivity to dropping sets of cities:
Long-run outcomes



The x-axis measures the ratio between the restricted point estimate when dropping one of 22 sets of cities and the corresponding original estimate in Table 7. This ratio is one, if the restricted estimate is unchanged, 1.5 if the restricted estimate is 50% larger than the original, and 0.5 if the restricted estimate is 50% smaller than the original. We present all outcomes (in rows) in all specifications (columns) corresponding to Table 7. The sum of all bars is 100%. 'WRO' in the third row stands for 'Women's rights organisation'. 'MP' stands for Member of Parliament'.

B.2 The role of economic growth: flexibly controlling for construction

We address the possibility that our city covariates do not adequately capture economic growth by including construction data from Cantoni et al. (2021). School construction does not predict general construction activity (Table B.2) and neither using the construction activity in 1650 (prior to the establishment of the first finishing school), nor the potentially endogeneous time-varying construction activity data change the point estimates significantly (Table B.3). We thus conclude our identification is robust to including or excluding different sets cities, city-specific trends, or economic activity.

Table B.2: Placebo estimates on the importance of finishing schools: Construction Activity

	$\mathbb{I}[>0]$		*Number*		*log*	
	(1)	(2)	(3)	(4)	(5)	(6)
	Any	Growth	Any	Growth	Any	Growth
Finishing school$_{it}$	-0.043	-0.017	1.805	0.939	0.034	0.133
	(0.034)	(0.066)	(1.236)	(0.644)	(0.108)	(0.111)
City covariates × period FE	Yes	Yes	Yes	Yes	Yes	Yes
Religious covariates × period FE	Yes	Yes	Yes	Yes	Yes	Yes
Educational covariates × period FE	Yes	Yes	Yes	Yes	Yes	Yes

Main results using a fixed-effects estimation in a window of four centuries before and two centuries after the establishment of a finishing school in city c (N= 1,421). All regressions include a full set of city and period fixed effects. We include full religious and educational covariates as defined in Table 1 columns (2), (4), and (6) in all regressions. As outcomes we consider all construction activity ("Any") in odd columns as well as growth-related construction activity ("Growth") in even columns, which excludes religious, military and palace buildings. In addition, we consider three transformations of these outcomes, namely indicators for building construction (columns 1 and 2), the raw number of buildings constructed (columns 3 and 4) and the log number of buildings constructed (columns 5 and 6). Standard errors clustered by city shown in parentheses. $* \ p < 0.10$, $** \ p < 0.05$, $*** \ p < 0.01$

Table B.3: Fixed-effects results on the importance of finishing schools - Controlling for construction

	$\mathbb{I}[Women > 0]$		log Women		Share Women	
	(1)	(2)	(3)	(4)	(5)	(6)
Finishing school$_{it}$	0.165***	0.155***	0.174***	0.167***	0.019***	0.017**
	(0.035)	(0.037)	(0.047)	(0.046)	(0.007)	(0.007)
Mean, untreated	0.143	0.142	0.131	0.130	0.026	0.025
Unit trend	Yes	Yes	Yes	Yes	Yes	Yes
City covariates × period FE	Yes	Yes	Yes	Yes	Yes	Yes
Religious covariates × period FE	Yes	Yes	Yes	Yes	Yes	Yes
Educational covariates × period FE	Yes	Yes	Yes	Yes	Yes	Yes
Matching on 1600 covariates	Yes	Yes	Yes	Yes	Yes	Yes
Construction in 1650 × period FE	Yes		Yes		Yes	
Construction in every period × period FE		Yes		Yes		Yes
Observations	8,640	8,737	8,640	8,737	8,640	8,737

We consider three types of dependent variables to capture the extensive and intensive margin of the birth of notable women. $\mathbb{I}[Women > 0]$ is an indicator equal to one if a city had at least one notable woman born in this period. 'log Women' constitutes the natural logarithm of the number of women born plus one. 'Share Women' denotes the number of women by the number of men and women in the same category. All columns control for city and period fixed effects as well as city-specific linear trends in addition to interacting city controls with period fixed effects to capture variation from economic, religious, and educational differences. Standard errors clustered by city shown in parentheses. $* \ p < 0.10$, $** \ p < 0.05$, $*** \ p < 0.01$

B.3 The role of Population: flexibly controlling for population and alternative data

Finally, we address the possibility that our city population data is too coarse. The most widely used data by Bairoch et al. (1988) has often been critized by its inclusion critera (a city must have at least 5000 inhabitants in any moment of time), but remains the de-facto standard. Recently, Buringh (2021) complemented this data, enabling us to have a more accurate picture of population at the time of the first construction of a finishing school.

While our original data only featured 122 cities with population in 1600 (average 8,352), the newer data has information on 186 cities (average 6,849). The results in Table B.4 are robust to this more extensive data.

Another concern with respect to population is that population size deterministically predicts school construction, and that thus controlling for initial levels of population might not adequately capture such a step function. The results are, however, robust to including endogenous per period population linearly or interacted with period fixed effects (Results not shown).

Table B.4: Fixed-effects results on the importance of finishing schools: Different population data

	$\mathbb{I}[Women > 0]$		log Women		Share Women	
	(1)	(2)	(3)	(4)	(5)	(6)
Finishing school$_{it}$	0.157***	0.146***	0.165***	0.143***	0.019***	0.019***
	(0.038)	(0.037)	(0.047)	(0.050)	(0.006)	(0.007)
Mean, untreated	0.143	0.143	0.130	0.130	0.026	0.026
Unit trend	Yes	Yes	Yes	Yes	Yes	Yes
City covariates × period FE	Yes	Yes	Yes	Yes	Yes	Yes
Religious covariates × period FE	Yes	Yes	Yes	Yes	Yes	Yes
Educational covariates × period FE	Yes	Yes	Yes	Yes	Yes	Yes
Matching on 1600 covariates	Yes	Yes	Yes	Yes	Yes	Yes
log Bairoch population in 1600 × period FE	Yes		Yes		Yes	
log Buringh population in 1600 × period FE		Yes		Yes		Yes
Observations	8,856	8,856	8,856	8,856	8,856	8,856

Results using a fixed-effects estimation and all cities in all periods, varying the population data to capture economic growth. Columns (1), (3), and (5) use city-level population in 1600 from Bairoch et al. (1988), the remaining columns population data in 1600 from Buringh (2021). We consider three types of dependent variables to capture the extensive and intensive margin of the birth of notable women. $\mathbb{I}[Women > 0]$ is an indicator equal to one if a city had at least one notable woman born in this period. 'log Women' constitutes the natural logarithm of the number of women born plus one. 'Share Women' denotes the number of women by the number of men and women in the same category. We regress the number of non-noble secular women and women from the nobility born in a city, as defined in the top row, on our measure of access to secondary education. All columns include city and period fixed effects as well as city specific linear trends. We interact city controls with period fixed effects to capture variation from economic, religious, and educational differences. We include the following controls measured in the 13th century: whether the city was a Hanseatic League or bishopric city and whether it had a Jewish presence and a pogrom. Additionally, we include the following controls from 1600: distance to Wittenberg, confessional battle in the vicinity, distance to the religious divide and Catholicism to capture religious differences. In addition, we control for the average temperature in 1650 to capture differential agricultural productivity, and hence income. Pre-existing male schools, universities in 1650, and the ruling houses are included to capture differential educational preferences. All covariates are interacted with period fixed effects. We also match cities on pre-1650 covariates to compare cities with the same pre-treatment characteristics. We include 86 match-specific fixed effects interacted with period fixed effects to confine the variation to within pre-treatment similar groups of cities. Standard errors clustered by city shown in parentheses. $* \ p < 0.10$, $** \ p < 0.05$, $*** \ p < 0.01$

C Dataset construction choices and timing of school establishment

In this Appendix, we discuss the construction of the Thiessen Polygons around each city that existed in 1300 A.D. as taken from from Voigtländer and Voth (2012) and show that the results are robust to only using cities that existed in 800 A.D. (Appendix C.1). As the cities in Voigtländer and Voth (2012) might have oversampled Jewish cities, we instead use the territories and rulers in 1619 as our baseline and reproduce the main findings of the paper and conclude that neither dataset construction nor sample selection introduced a bias in our estimates (Appendix C.2). We then highlight the impact of different school establishment periods (Appendix C.3).

C.1 Structure of the data

We take the city-level data by Voigtländer and Voth (2012) as a starting point and construct Thiessen Polygons around the center of each city in their dataset. Thiessen Polygons are constructed such that every village or town inside the polygon around city i is closer to city i than to any other city $j \neq i$. Figure C.1 shows the resulting polygons alongside the location of finishing schools and the number of notable women born within each area. By construction, the city lies in the center of its polygon.

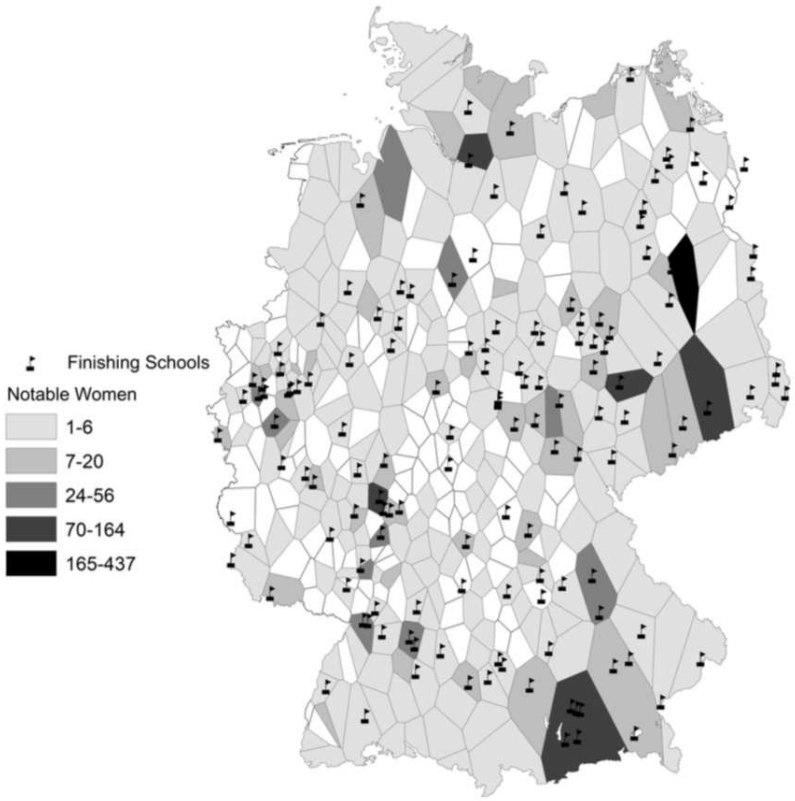
We use this data structure and the set of cities used by Voigtländer and Voth (2012) to include their rich city-level covariates and to avoid relying on county boundaries. From the entire set of cities in Voigtländer and Voth (2012), we only select those cities that are mentioned before 1300 and are the oldest town within a county. For example: Aachen has four recorded 'cities' in Voigtländer and Voth (2012): town_id 1, mentioned in 830, 13.45 km from Aachen; town_id 3, mentioned in 1118, 10.74 km from Aachen; town_id 4, mentioned in 870, 5.12 km from Aachen; and Aachen itself (town_id 5, mentioned in 400). Since these other cities are likely suburbs or dependent on Aachen's existence, we use the location of Aachen and merge all variables to Aachen. This has the advantage that our estimates are not biased by a potential rural-urban bias when including suburbs. We arrive at 388 cities by only using the oldest city within each Landkreis (town_id 5) that lies in present-day Germany.

As the NDB starts recording notable individuals born from the year 800 onwards, using cities with recorded population levels by 800 is a natural alternative, which, however, reduces the sample of cities to 101. In Table C.1 we document that results for both choices (1300 vs. 800) are similar, but imprecise, across the outcomes.

The next choice concerns the length of periods. We choose to assign notable individuals to 50-year periods based on their year of birth. There are two reasons for our 50-year period choice: First, by choosing 50-years, we ensure that on average a woman that is born in this period either did or did not have access to a finishing school. Second, the scarce number of women recorded in the NDB prior to the 15th century implies a trade-off between statistical power and assignment accuracy. If we used every birth year separately, and thus matched schools most precisely, we would end up with no variation within most city x birth-year cells. Thus, to increase power, we

rely on 50-year periods, and show robustness to using 25 year intervals in Table C.2. Again, our point estimates remain unaffected.

Figure C.1: Thiessen Polygons, finishing schools and notable women



This figure shows our unit of observation, Thiessen polygons created around cities included in the data by Voigtländer and Voth (2012). By construction, the cities lie in the center of each Thiessen polygon. For simplicity we continue to refer to our unit of observation as "city". The figure also shows the location of finishing schools as well as the number of notable women born in each city.

Table C.1: Fixed-effects results on the importance of finishing schools: Changing the Unit of observation to cities that existed in 800

	$\mathbb{I}[Women > 0]$		log Women		Share Women	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Non-Noble Seculars						
Finishing school$_{it}$	0.206***	0.211***	0.355***	0.239***	0.019***	0.026**
	(0.047)	(0.057)	(0.091)	(0.083)	(0.007)	(0.011)
Mean, untreated	0.177	0.165	0.354	0.314	0.023	0.022
Panel B: Nobility						
Finishing school$_{it}$	0.056	0.035	0.095**	0.038	0.006	-0.012
	(0.038)	(0.055)	(0.041)	(0.051)	(0.012)	(0.018)
Mean, untreated	0.144	0.129	0.203	0.175	0.043	0.040
Unit trend	Yes	Yes	Yes	Yes	Yes	Yes
City covariates × period FE		Yes		Yes		Yes
Religious covariates × period FE		Yes		Yes		Yes
Educational covariates × period FE		Yes		Yes		Yes
Observations	2,424	2,232	2,424	2,232	2,424	2,232

Instead of 1300, we consider all cities that exist in 800. This severely reduces the number of cities from 388 to 101. We consider three types of dependent variables to capture the extensive and intensive margin of the birth of notable women. $\mathbb{I}[Women > 0]$ is an indicator equal to one if a city had at least one notable woman born in this period. 'log Women' constitutes the natural logarithm of the number of women born plus one. 'Share Women' denotes the number of women by the number of men and women in the same category. Columns (1), (3), and (5) constitute the baseline and include city and period fixed effects as well as city-specific linear trends. In columns (2), (4), and (6) we interact city controls with period fixed effects to capture variation from economic, religious, and educational differences. Standard errors clustered by city shown in parentheses. $* \ p < 0.10$, $** \ p < 0.05$, $*** \ p < 0.01$

Table C.2: Fixed-effects results on the importance of finishing schools: Changing the Unit of observation to 25 year intervals

	$\mathbb{I}[Women > 0]$		log Women		Share Women	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Non-Noble Seculars						
Finishing school$_{it}$	0.155***	0.083***	0.206***	0.088***	0.012***	0.008*
	(0.023)	(0.022)	(0.038)	(0.030)	(0.004)	(0.004)
Mean, untreated	0.099	0.098	0.142	0.141	0.021	0.021
Panel B: Nobility						
Finishing school$_{it}$	0.026**	0.013	0.030**	0.012	0.003	-0.000
	(0.013)	(0.016)	(0.014)	(0.014)	(0.004)	(0.006)
Mean, untreated	0.035	0.035	0.042	0.042	0.015	0.015
Unit trend	Yes	Yes	Yes	Yes	Yes	Yes
City covariates × period FE		Yes		Yes		Yes
Religious covariates × period FE		Yes		Yes		Yes
Educational covariates × period FE		Yes		Yes		Yes
Observations	18,236	18,095	18,236	18,095	18,236	18,095

Instead of 50 year periods that clearly separate different generations, we consider 25 year generations instead. We consider three types of dependent variables to capture the extensive and intensive margin of the birth of notable women. $\mathbb{I}[Women > 0]$ is an indicator equal to one if a city had at least one notable woman born in this period. 'log Women' constitutes the natural logarithm of the number of women born plus one. 'Share Women' denotes the number of women by the number of men and women in the same category. Columns (1), (3), and (5) constitute the baseline and include city and period fixed effects as well as city-specific linear trends. In columns (2), (4), and (6) we interact city controls with period fixed effects to capture variation from economic, religious, and educational differences. Standard errors clustered by city shown in parentheses. $* \ p < 0.10$, $** \ p < 0.05$, $*** \ p < 0.01$

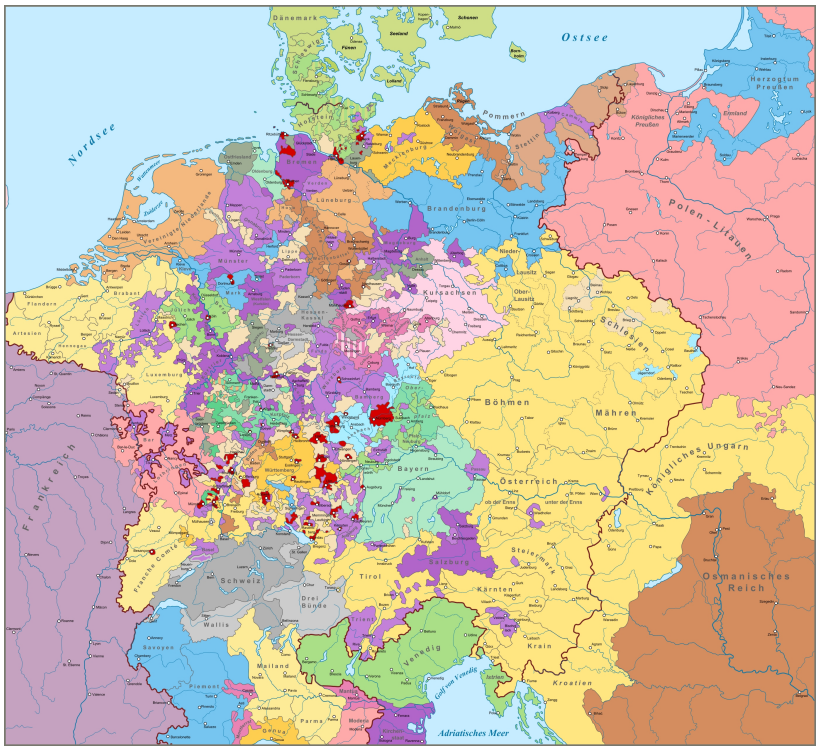
C.2 Sample selection: Using a different starting point for the analysis

In our baseline data, we created a balanced panel for each city in Voigtländer and Voth (2012) using Thiessen Polygons as a starting point (Figure C.1). This procedure has the advantage that it does not rely on any administrative boundary, past or present, and any covariate from Voigtländer and Voth (2012) can easily be used. However, as the original focus of this paper was on Jewish pogroms, the original data might have oversampled cities with black death and pogroms. In this section, we thus show robustness to using an alternative baseline source to create a balanced panel: the territories of Germany in 1619.

In Figure C.2, we depict the territories of 21 different rulers, 91 ecclesiastical cities, 96 free cities and 57 imperial cities in Germany on the eve of the Thirty Years' war. We then use these administrative boundaries to create a balanced panel from 800 until 1950. The implicit assumption here is that people migrate disporporitonately within a rulers territory and only rarely migrate between competing territories. We avoid this assumption using the Voigtländer and Voth (2012) cities in combination with Thiessen polygons.

The results in Table C.3, however, confirm our initial results. We conclude that choosing the cities from Voigtländer and Voth (2012) to create Thiessen polygons did not introduce a bias into our setting.

Figure C.2: German territorial belongings and rulers in 1619



This figure shows the territories of rulers, ecclesiastical cities, free cities, and imperial cities in 1619, which we use as a baseline for the results in this section.

Table C.3: Fixed-effects results on the importance of finishing schools: Changing the Sample to territories in 1619

	$\mathbb{I}[Women > 0]$		log Women		Share Women	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Non-Noble Seculars						
Finishing school$_{it}$	0.383***	0.145***	0.582***	0.187***	-0.032	0.024*
	(0.034)	(0.027)	(0.080)	(0.052)	(0.043)	(0.014)
Mean, untreated	0.073	0.073	0.129	0.120	0.016	0.017
Panel B: Nobility						
Finishing school$_{it}$	0.024	-0.007	0.081**	0.003	-0.004	-0.009
	(0.038)	(0.040)	(0.034)	(0.030)	(0.011)	(0.010)
Mean, untreated	0.041	0.036	0.056	0.046	0.013	0.012
Unit trend	Yes	Yes	Yes	Yes	Yes	Yes
City covariates $\times$ period FE		Yes		Yes		Yes
Religious covariates $\times$ period FE		Yes		Yes		Yes
Educational covariates $\times$ period FE		Yes		Yes		Yes
Observations	6,360	6,216	6,360	6,216	6,360	6,216

Instead of using the cities in Voigtländer and Voth (2012), we use the territories as show in Figure C.2. We consider three types of dependent variables to capture the extensive and intensive margin of the birth of notable women. $\mathbb{I}[Women > 0]$ is an indicator equal to one if a city had at least one notable woman born in this period. 'log Women' constitutes the natural logarithm of the number of women born plus one. 'Share Women' denotes the number of women by the number of men and women in the same category. Columns (1), (3), and (5) constitute the baseline and include city and period fixed effects as well as city-specific linear trends. In columns (2), (4), and (6) we interact city controls with period fixed effects to capture variation from economic, religious, and educational differences. Standard errors clustered by city shown in parentheses. $* \ p < 0.10$, $** \ p < 0.05$, $*** \ p < 0.01$

C.3 Using the exact opening time of finishing schools

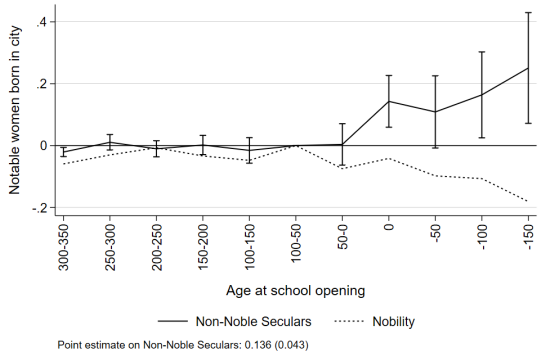
In our baseline data, we created a balanced panel for each city to include never-treated cities and covariates. This decision is in line with the recent literature on event-study validity, as discussed in Appendix F. In the resulting panel, we merged individuals to the closest of 50-year periods in cities. That is, if an individual is born in 1640, we merge her to the City's 1650 period, regardless of treatment status. In that setting, we have cities that switch into treatment, as well as pure-control cities in every period and can compare the three groups.

However, an event-study usually uses the exact timing to estimate the treatment effect. Ignoring never-treated cities, our data allows for such a fine-grained distinction. In this Appendix, we normalize the time period for every city to zero at the exact time the first school was opened. That is, if the first school opens in 1626 for the city of Aachen, we create city-specific period lags of arbitrary length. Yet, there are two problems associated with this: First, we are unable to merge control cities to this framework, and thus the comparison is strictly within treated cities only. Second, the choice of omitted period is not innocuous: Women that are born 10 years prior to the opening of a Finishing schools still benefit from its construction, while not having had any say in its establishing. We thus need to normalize at an earlier period at which women could not have benefited from the future presence of finishing schools. While these considerations average out at 50-year intervals, they matter greatly at smaller intervals.

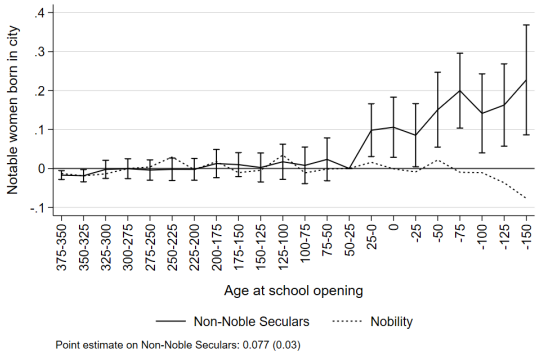
In Figure C.3, we use the opening time of the first finishing school in our 129 cities with schools and create various lags around it. In all Panels, we aim to reference the estimates to a previous generation of women who could no longer benefit from education: parents. In Panel a, c and e, we create 50-year lag windows around each school and omit women born between 100 and 50 years before. In Panles b, d, and f, we create 25 year periods and omit women born between 25 and 50 years before. Both figures show the exact same pattern: We find no evidence for a pre-trend in any specification, a significant uptick after the opening, and point estimates that are not statistically different from our baseline.

Yet, as we discuss in Appendix F, the inclusion of never-treated cities allows for a clean comparison between treatment and control, as well as a classical differences-in-differences setup (Appendix G). These benefits, along with the possibility to merge covariates and the unchanged point estimates, motivate our choice to match women and schools to a balanced panel of cities, instead of using this exact-timing setup.

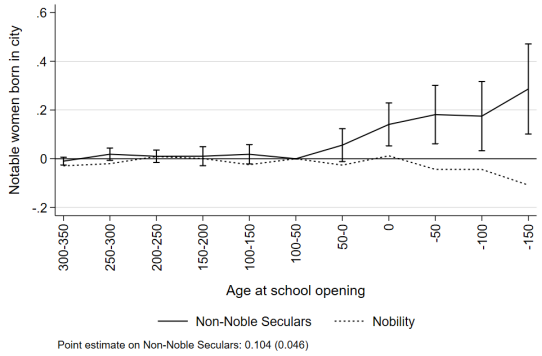
Figure C.3: Event-Study: Impact of finishing school establishment on notable women



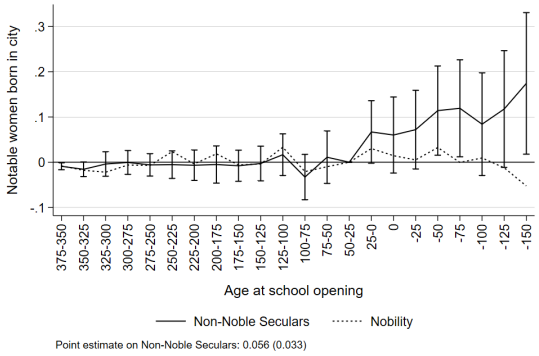
(a) 50 year periods, 100 year fixed effects



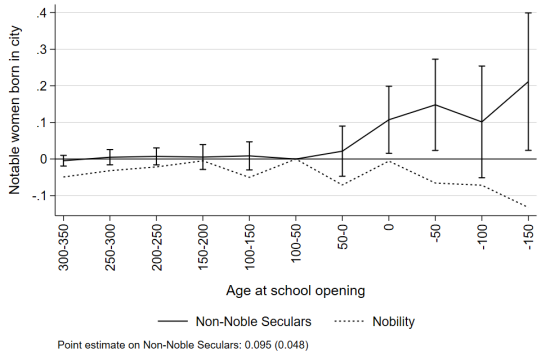
(b) 25 year periods, 100 year fixed effects



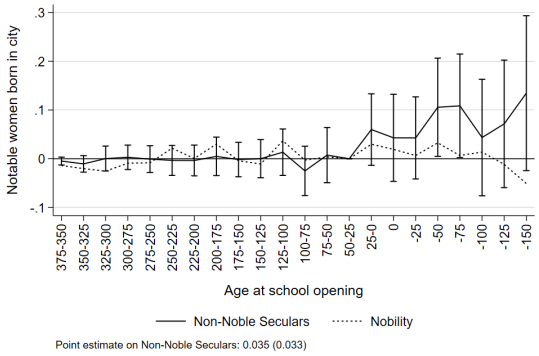
(c) 50 year periods, 50 year fixed effects



(d) 25 year periods, 50 year fixed effects



(e) 50 year periods, 25 year fixed effects



(f) 25 year periods, 25 year fixed effects

Event study graphs using the exact timing of the first finishing school in every city to create 50-year periods (left) and 25-year periods (right). We include century-period fixed effects in Figure C.3a-b, 50-year period fixed effects in Figure C.3c-d, and 25-year fixed effects in Figure C.3e-f. Results are robust to using year-fixed effects instead.

C.4 Timing of school construction

When taking historical accounts at face value, the establishment of *early* finishing schools by foreign Catholic women's orders constituted a shift in the supply of women's education as opposed to a local shift in the demand for education.

In this Appendix, we assess the severity of a potential bias in our estimates that would arise if the establishment of the *later* finishing schools in our data were largely driven by increasing demand for women's education. If the *later* schools (constructed between 1800 and 1850, i.e. after the fall of the Holy-Roman-Empire) accounted for all the impact on women's representation among the human-capital elite, this would call into question our interpretation that the establishment of finishing schools constituted a supply-side shift. However, our results largely remain robust when only using schools constructed before 1800 in the odd columns of Table C.4. In addition, the point estimates on *early* and *late* schools are not statistically different from each other in most specifications.

Moreover, in Table C.5 we compare the impact of the first versus the second school constructed in a city and show that most of the impact indeed comes from the first established school. This suggests that indeed the first, arguably exogenous school opening, is responsible for the increase in the share of women among the human capital elite of German cities. This finding is confirmed in the differences-in-differences setting, where all periods produce similar estimates (Table G.2).

Table C.4: Fixed-effects results on the importance of finishing schools:
Early vs Late Schools

	$\mathbb{I}[Women > 0]$		log Women		Share Women	
	(1) Early	(2) Late	(3) Early	(4) Late	(5) Early	(6) Late
Panel A: Non-Noble Seculars						
Finishing school$_{it}$	0.117***	0.159***	0.263**	0.188***	0.027***	0.010
	(0.043)	(0.040)	(0.108)	(0.048)	(0.009)	(0.007)
Mean, untreated	0.142	0.145	0.130	0.132	0.026	0.026
Panel B: Nobility						
Finishing school$_{it}$	-0.014	0.023	0.043	0.027	-0.016	0.002
	(0.043)	(0.029)	(0.048)	(0.025)	(0.016)	(0.011)
Mean, untreated	0.055	0.061	0.043	0.047	0.022	0.023
Unit trend	Yes	Yes	Yes	Yes	Yes	Yes
City covariates $\times$ period FE	Yes	Yes	Yes	Yes	Yes	Yes
Religious covariates $\times$ period FE	Yes	Yes	Yes	Yes	Yes	Yes
Educational covariates $\times$ period FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	6,984	8,400	6,984	8,400	6,984	8,400

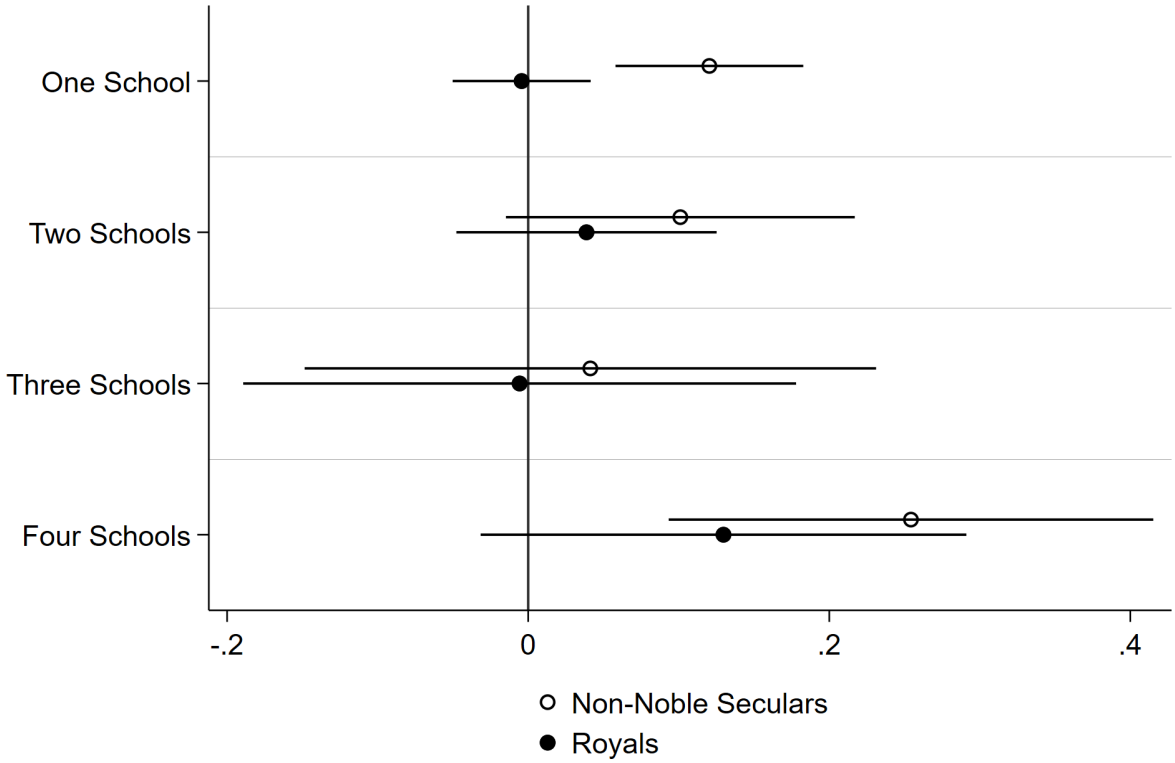
Main results comparing schools constructed between 1650–1750 (*early*) and 1800–1850 (*late*) to assess the sensitivity of our results to schools whose establishment is potentially influenced by rising demand for women's education in the context of the Industrial Revolution in Germany. We consider three types of dependent variables to capture the extensive and intensive margin of the birth of notable women. $\mathbb{I}[Women > 0]$ is an indicator equal to one if a city had at least one notable woman born in this period. 'log Women' constitutes the natural logarithm of the number of women born plus one. 'Share Women' denotes the number of women by the number of men and women in the same category. We regress the number of non-noble secular women and women from the nobility on our measure of access to secondary education. In all columns we interact city controls with period fixed effects to capture variation from economic, religious, and educational differences. Standard errors clustered by city shown in parentheses. $* \ p < 0.10$, $** \ p < 0.05$, $*** \ p < 0.01$

Table C.5: Fixed-effects results on the importance of finishing schools:
Comparing the impact of the first to the second school

	$\mathbb{I}[Women > 0]$		log Women		Share Women	
	(1)	(2)	(3)	(4)	(5)	(6)
Finishing school$_{it}$	0.157***	0.146***	0.165***	0.143***	0.019***	0.019***
	(0.038)	(0.037)	(0.047)	(0.050)	(0.006)	(0.007)
Mean, untreated	0.143	0.143	0.130	0.130	0.026	0.026
Unit trend	Yes	Yes	Yes	Yes	Yes	Yes
City covariates $\times$ period FE	Yes	Yes	Yes	Yes	Yes	Yes
Religious covariates $\times$ period FE	Yes	Yes	Yes	Yes	Yes	Yes
Educational covariates $\times$ period FE	Yes	Yes	Yes	Yes	Yes	Yes
Matching on 1600 covariates	Yes	Yes	Yes	Yes	Yes	Yes
log Bairoch population in 1600 $\times$ period FE	Yes		Yes		Yes	
log Buringh population in 1600 $\times$ period FE		Yes		Yes		Yes
Observations	8,856	8,856	8,856	8,856	8,856	8,856

We consider three types of dependent variables to capture the extensive and intensive margin of the birth of notable women. $\mathbb{I}[Women > 0]$ is an indicator equal to one if a city had at least one notable woman born in this period. 'log Women' constitutes the natural logarithm of the number of women born plus one. 'Share Women' denotes the number of women by the number of men and women in the same category. We regress the number of non-noble secular women and women from the nobility on our measure of access to secondary education. In all columns we interact city controls with period fixed effects to capture variation from economic, religious, and educational differences. Standard errors clustered by city shown in parentheses. $* \ p < 0.10$, $** \ p < 0.05$, $*** \ p < 0.01$

Figure C.4: The impact of multiple schools



The cumulative impact of cities having one, two, three, or more school in the fixed effect estimation on the occurrence of notable women. The outcome is an indicator equal to one if a notable woman from the respective group was born in a given city and period. All covariates from Table 1 column (2) included.

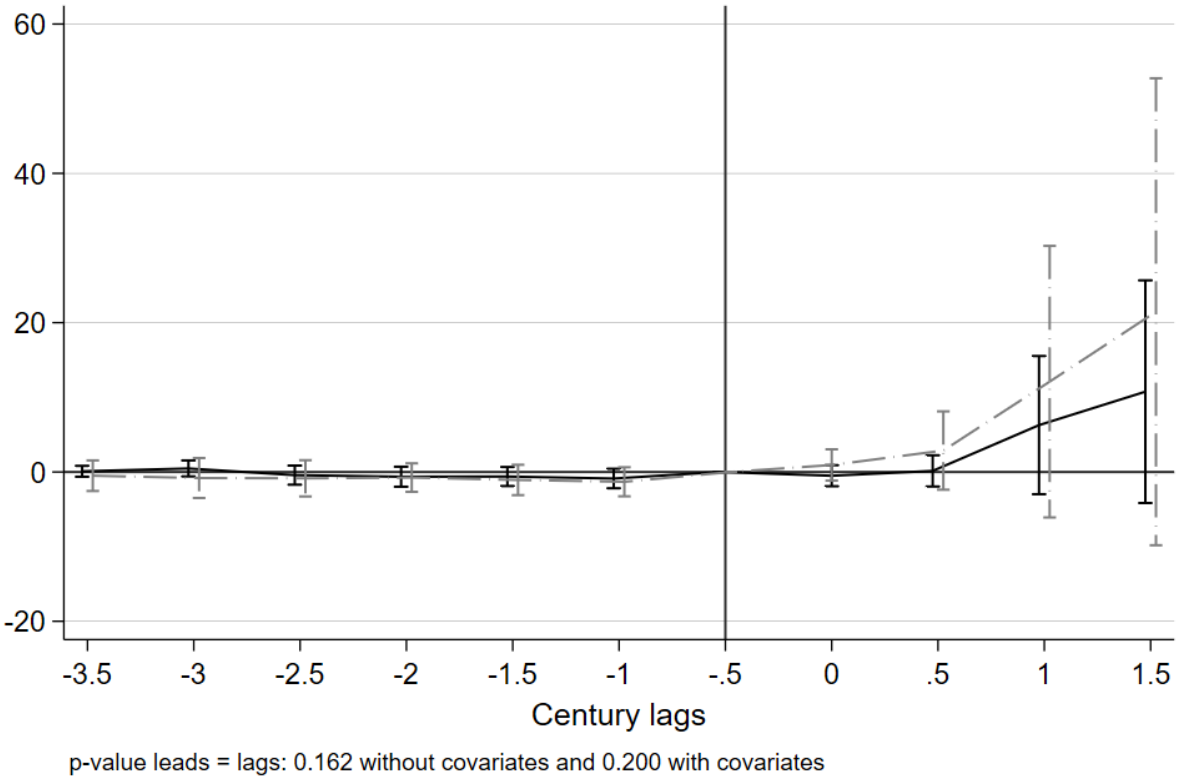
D The role of demand-side factors

In this Appendix, we address potential confounding factors arising from demand-side factors. We show that: (i) finishing school construction is uncorrelated to population growth, (ii) the rise of the Enlightenment in the early 18th century does not affect our point estimates, and (iii) argue that the Industrial Revolution in Germany only started in the beginning of the 19th century, well after the establishment of the first finishing schools. We conclude that demand-side factors are unlikely to have affected the establishing of finishing schools. Finishing schools are likely the result of idiosyncratic decisions by religious orders and rulers.

D.1 Population growth

In a first exercise, we utilize a panel of population for German cities by Bairoch et al. (1988) and show that neither before nor after finishing school construction, cities did differ in their population growth trends. If anything, population seems to grow more than 100 years after the construction of finishing schools.

Figure D.1: The impact of finishing schools on population



The outcome is city size as recorded by Bairoch et al. (1988). All covariates from Table 1 column (2) included.

D.2 Cultural demand: German Enlightenment (18th-19th century)

In a second exercise, we consider the possibility that finishing school construction only captures the arrival of the Enlightenment ideas in German cities of the early 17th century. We use data on 317 Enlightenment journals published during 1688–1815 in 99 cities of Germany from Akademie der Wissenschaften (2018).[45] Most of these journals were published in the late 18th to early 19th century (median: 1789) for a short period of time (median: 4 years). Out of these 317 journals, only two cover women's topics explicitely ("Iris : Vierteljahresschrift für Frauenzimmer"; "Journal für deutsche Frauen : von deutschen Frauen geschrieben"). While this compendium includes more than 260,000 articles, only 580 articles (0.3%) were categorized into articles on women's rights (31), on female education (210), or other topics for women (389).

While this data has a panel dimension, we abstain from assessing pre-trends in this setting for two reasons. First, the time dimension is too short to allow for a meaningful assessment of pre-trends, and second, 1688 is post school construction for some of the cities in our sample. We provide a cross-sectional assessment of pre-trends in Table D.1 instead. In columns 1–3, we regress future finishing school establishment in the next period on women's representation in the human capital elite (1), the number of Enlightenment journals (2), and the number of articles on or for women published in this city and period. As expected, none of the point estimates are significant.[46] In columns 4–6, we repeat this exercise, but instead regress these outcomes on whether in this period a finishing school was established in this city and period. While our indicator for women's representation among the human capital elite is highly significant, our estimates on Enlightenment journals and articles suggest no significant correlation with finishing school construction.

There are two important distinctions between these journals and the "Frauen-Zeitung" we use in Table 7. First, we only record the city of the publisher of the Enlightenment journals, not the actual readers. So instead of demand for these journals, we only observe where these journals are produced. In contrast, the letters to editor in the "Frauen-Zeitung" enable us to measure demand for content related to women's rights in every city. Second, the "Frauen-Zeitung" is a magazine dedicated to the women's cause, and thus read by people who support women's rights; and not only general Enlightenment ideas. Thus, the "Frauen-Zeitung" is a clear outcome of the finishing schools, whereas Enlightenment journals potentially represent a confounding factor.

In Table D.2, we include the number of journals active in every city, as well as the number of publications related to women, as a potentially endogeneous control. If finishing schools only act as a proxy for the arrival of the Enlightenment, we would expect the point estimate on finishing schools to drop significantly. We do observe no change in point estimates in any specification.

[45] https://gelehrte-journale.de/faechersystematik. Last accessed: 2021-10-21.

[46] Our control group of cities consists of cities that never established a finishing school, their outcomes measured in 1800.

Table D.1: The impact of finishing schools on Enlightenment journals

	Period prior to finishing school establishment			Period of finishing school establishment		
	Non-Noble Secular Women (1)	# Enlighten-ment Journals (2)	# Articles on/for women (3)	Non-Noble Secular Women (4)	# Enlighten-ment Journals (5)	# Articles on/for women (6)
Finishing school	-0.005	-0.119	0.170	0.189***	-0.228	0.237
	(0.049)	(0.173)	(0.201)	(0.058)	(0.208)	(0.279)
City covariates	Yes	Yes	Yes	Yes	Yes	Yes
Religious covariates	Yes	Yes	Yes	Yes	Yes	Yes
Educational covariates	Yes	Yes	Yes	Yes	Yes	Yes
Observations	385	385	385	385	385	385
Mean dep. var.	0.057	0.039	0.144	0.106	0.038	0.238

Assessing the impact of finishing school establishment on non-noble secular women and Enlightenment outcomes. Columns 1-3 assess the impact of finishing schools in the period immediately prior to their establishment; columns 4-6 in the period of their establishment. We consider three outcome variables: 'Non-Noble Secular Women' is an indicator equal to one if a city had at least one notable woman born in this period. '# Enlightenment Journals' is the number of journals published in city and period. '# Articles on/for women' is the number of articles in these journals that relate to women. We regress these outcomes, as defined in the top row, on our finishing school variable. In all columns we include covariates as defined in Table 1. The regression is a pooled OLS in which the treatment sample is defined by the timing of finishing school construction. The control sample is defined as all cities that never establish a finishing school, in all periods in which a finishing school was constructed in another city. We thus control for period fixed effects to compare *within* period, cities that establish a finishing school to cities that never establish a finishing school, measured in 1800. Standard errors clustered by city shown in parentheses. $* \ p < 0.10$, $** \ p < 0.05$, $*** \ p < 0.01$

Table D.2: Fixed-effects results on the importance of finishing schools: controlling for Enlightenment journals

	$\mathbb{I}[Women > 0]$		log Women		Share Women	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Non-Noble Seculars						
Finishing school$_{it}$	0.055***	0.162***	0.155***	0.173***	0.016**	0.020***
	(0.019)	(0.036)	(0.045)	(0.046)	(0.007)	(0.007)
Mean, untreated	0.142	0.142	0.240	0.241	0.025	0.025
Unit trend	Yes	Yes	Yes	Yes	Yes	Yes
City covariates × period FE	Yes	Yes	Yes	Yes	Yes	Yes
Religious covariates × period FE	Yes	Yes	Yes	Yes	Yes	Yes
Educational covariates × period FE	Yes	Yes	Yes	Yes	Yes	Yes
# Enlightenment:	Journals	Articles	Journals	Articles	Journals	Articles
Matching on 1600 covariates	Yes	Yes	Yes	Yes	Yes	Yes
Observations	8,851	8,845	8,851	8,845	8,851	8,845

Main results using finishing schools construction in each city, controlling for the number of Enlightenment journals and articles related to women in every period (even columns). We consider three types of dependent variables to capture the extensive and intensive margin of the birth of notable women. $\mathbb{I}[Women > 0]$ is an indicator equal to one if a city had at least one notable woman born in this period. 'log Women' constitutes the natural logarithm of the number of women born plus one. 'Share Women' denotes the number of women by the number of men and women in the same category. In all columns we interact city controls with period fixed effects to capture variation from economic, religious, and educational differences. We match cities on pre-1650 covariates to compare cities with the same pre-treatment characteristics. We include 86 match-specific fixed effects interacted with period fixed effects to confine the variation to within pre-treatment similar groups of cities. Standard errors clustered by city shown in parentheses. $* \ p < 0.10$, $** \ p < 0.05$, $*** \ p < 0.01$

Finally, we test in the cross-section whether access to education increases the likelihood of observing Enlightenment Journals. Although possible, we abstain from running a panel regression because of the non-existent pre-school data on Enlightenment Journals (18th-Century).

In line with our hypothesis that access to secondary education increases the average human capital as well as women's representation among the human capital elite, we find positive correlations between the number of finishing schools on the existence and number of Enlightenment journals in Table D.3. Cities are three times as likely to have an Enlightenmentjournal in 1800, if the city had established a finishing school earlier (4.6% to 14.9%)

Table D.3: Long-term impact of finishing schools on Enlightenment Journals

	$\mathbb{I}[>0]$		log Number	
	(1)	(2)	(3)	(4)
Panel A: Enlightenment Journals				
Finishing School	0.167***	0.103*	0.256***	0.143*
	(0.028)	(0.061)	(0.059)	(0.079)
R-squared	0.107	0.356	0.131	0.328
Mean, untreated	0.073	0.046	0.212	0.084
Bias-Adjusted β		0.076		0.090
IV estimate using Monasteries in 1300		0.206		0.075
City Covariates		Yes		Yes
Religious covariates		Yes		Yes
Educational covariates		Yes		Yes
Observations	388	183	388	183
Bandwidth		10		10

Cross-sectional results using all observations in odd columns and sample limited to 10 km of the religious boundary in 1619 in even columns. In each Panel we regress an indicator variable for the existence and the natural logarithm plus one of the number of instances on the number of finishing schools. In Panel A, we estimate whether finishing schools increase the likelihood and number of Enlightenment Journals from city c. We include covariates as defined in Table 1 columns (2), (4), and (6) where indicated and limit the sample to within 10km of Germany's denominational divide in 1619 to capture areas with stronger religious competition in columns (2) and (4). *Bias-Adjusted* β follows the procedure laid out in Oster (2019) assuming $R^{max} = 1.3\tilde{R}$ and $\delta = 1$. *IV estimate using Monasteries in 1300* instruments finishing schools using existing monasteries in 1300. The F-Test on the first stage is 9.75. All estimates are significant. We discuss the first-stage relationship and the exclusion restriction in great detail in Online Appendix H. Standard errors clustered by city shown in parentheses. $* \ p < 0.10$, $** \ p < 0.05$, $*** \ p < 0.01$

D.3 Economic demand for educated workers: German Industrial Revolution (19th century)

Finally, we consider the role of economic demand for education fueled by the Industrial Revolution. It is generally accepted that the early Industrial Revolution in Germany started in 1815 (Tilly and Kopsidis, 2020), but only took off after the March revolution of 1848. Throughout Germany, guilds often had strong regulations and protections before the Industrial Revolution - often also banning women from the trade (Ogilvie, 2004; Hoogenboom et al., 2018). When guilds became less powerful, early attempts at industrialization started towards the end of the 18th century: the first mechanical cotton spinning mill was installed in 1784; the first steam engine in 1785. Yet, coal production as a measure of the true beginning of the Industrial Revolution only skyrocketed after 1850, after the end of our finishing school data.

We split school construction into two time periods: The first, 1650-1750 captures a period in which industrial demand did not exist, and a second, 1800-1850, in which the industrial demand of the early Industrical Revolution might have increased the establishing of finishing schools. However, as we observe no differential impact across these two time periods (Table D.4), we conclude that economic demand fueled by the Early Industrial Revolution is unlikely to affect our interpretation.

Table D.4: Fixed-effects results on the importance of finishing schools:
The role of the Industrial Revolution

	$\mathbb{I}[Women > 0]$		log Women		Share Women	
	(1) Pre Industrial Revolution ($\leq$ 1750)	(2) Early Industrial Revolution ($\geq$ 1800)	(3) Pre Industrial Revolution ($\leq$ 1750)	(4) Early Industrial Revolution ($\geq$ 1800)	(5) Pre Industrial Revolution ($\leq$ 1750)	(6) Early Industrial Revolution ($\geq$ 1800)
Panel A: Non-Noble Seculars						
Finishing school$_{it}$	0.117***	0.159***	0.263**	0.188***	0.027***	0.010
	(0.043)	(0.040)	(0.108)	(0.048)	(0.009)	(0.007)
Mean, untreated	0.142	0.145	0.130	0.132	0.026	0.026
Unit trend	Yes	Yes	Yes	Yes	Yes	Yes
City covariates $\times$ period FE	Yes	Yes	Yes	Yes	Yes	Yes
Religious covariates $\times$ period FE	Yes	Yes	Yes	Yes	Yes	Yes
Educational covariates $\times$ period FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	6,984	8,400	6,984	8,400	6,984	8,400

Main results comparing schools constructed between 1650–1750 and 1800–1850 to assess the sensitivity of our results to schools whose establishment is potentially influenced by rising demand for women's education in the context of the Industrial Revolution in Germany. We consider three types of dependent variables to capture the extensive and intensive margin of the birth of notable women. $\mathbb{I}[Women > 0]$ is an indicator equal to one if a city had at least one notable woman born in this period. 'log Women' constitutes the natural logarithm of the number of women born plus one. 'Share Women' denotes the number of women by the number of men and women in the same category. In all columns we interact city controls with period fixed effects to capture variation from economic, religious, and educational differences. Standard errors clustered by city shown in parentheses. $* p < 0.10$, $** p < 0.05$, $*** p < 0.01$

E Spatial dependence and SUTVA

In this Appendix, we address the potential threat of spatial correlation, possible violations of the Stable Unit Treatment Value Assumption (SUTVA), and discuss spatial noise (Kelly, 2020).

We show that standard errors accounting for spatial correlation are slightly smaller than cluster-robust standard errors at the city level (Table E.1). To address potential violations of SUTVA, we exclude all cities that border a city with finishing schools in Table E.2. If migration from cities without finishing schools to cities with such schools drove our findings, an increase in the 'cost of migration' by increasing control cities' distance to the next school city should result in significantly smaller estimates. As expected , we find no evidence that migration impacts our point estimates.

A recent literature has focused on how estimates indicating persistent effects of past events on more recent outcomes can be driven by spatial noise (Kelly, 2020). To address the potential severity arising from this line of thought, we report a low Moran's I of 0.002 with a p-value of 0.156. In addition, we conduct an exercise where we randomly distribute schools across Germany in each period, holding the number of schools opened in that period constant. Once opened, a school remains for the remainder of time. The results in Figure E.1 reveal that our results are clear outliers in this distribution.

Taken together, the results presented in this Appendix suggest that our estimates are unlikely to be driven by spatial dependence and potential violations of SUTVA.

Table E.1: Fixed-effects results on the importance of finishing schools:
Standard errors corrected for spatial dependence

	$\mathbb{I}[Women > 0]$		log Women		Share Women	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Non-Noble Seculars						
Finishing school$_{it}$	0.202***	0.157***	0.301***	0.114***	0.012**	0.019***
	(0.028)	(0.027)	(0.032)	(0.023)	(0.005)	(0.005)
Mean, untreated	0.145	0.143	0.132	0.130	0.026	0.026
Unit trend	Yes	Yes	Yes	Yes	Yes	Yes
City covariates × period FE		Yes		Yes		Yes
Religious covariates × period FE		Yes		Yes		Yes
Educational covariates × period FE		Yes		Yes		Yes
Matching on 1600 covariates		Yes		Yes		Yes
Observations	9,312	8,856	9,312	8,856	9,312	8,856

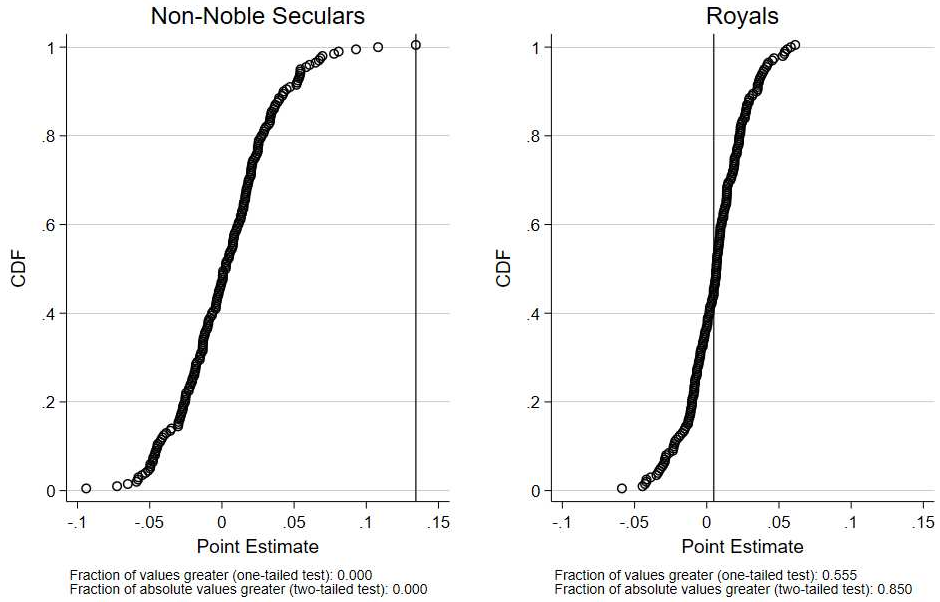
Main results using a fixed-effects estimation and all cities in all periods, with standard errors corrected for spatial dependence within 100km as in Hsiang et al. (2013). We consider three types of dependent variables to capture the extensive and intensive margin of the birth of notable women. $\mathbb{I}[Women > 0]$ is an indicator equal to one if a city had at least one notable woman born in this period. 'log Women' constitutes the natural logarithm of the number of women born plus one. 'Share Women' denotes the number of women by the number of men and women in the same category. Columns (1), (3), and (5) constitute the baseline and include city and period fixed effects as well as city-specific linear trends. In columns (2), (4), and (6) we interact city controls and match fixed effects with period fixed effects and to capture variation from economic, religious, and educational differences. Spatially corrected standard errors shown in parenthesis. $* \ p < 0.10$, $** \ p < 0.05$, $*** \ p < 0.01$

Table E.2: Fixed-effects results on the importance of finishing schools: Comparing towns with schools to non-neighboring towns without schools

	$\mathbb{I}[Women > 0]$		log Women		Share Women	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Non-Noble Seculars						
Finishing school$_{it}$	0.134***	0.147***	0.148***	0.107**	0.012**	0.015*
	(0.031)	(0.042)	(0.040)	(0.053)	(0.006)	(0.008)
Mean, untreated	0.144	0.142	0.131	0.131	0.026	0.025
Unit trend	Yes	Yes	Yes	Yes	Yes	Yes
City covariates × period FE	Yes	Yes	Yes	Yes	Yes	Yes
Religious covariates × period FE	Yes	Yes	Yes	Yes	Yes	Yes
Educational covariates × period FE	Yes	Yes	Yes	Yes	Yes	Yes
Non-Spillover sample		Yes		Yes		Yes
Observations	9,240	3,696	9,240	3,696	9,240	3,696

Main results using a fixed-effects estimation and either cities with finishing schools or non-neighboring cities without schools to address spatial spillovers. We consider three types of dependent variables to capture the extensive and intensive margin of the birth of notable women. $\mathbb{I}[Women > 0]$ is an indicator equal to one if a city had at least one notable woman born in this period. 'log Women' constitutes the natural logarithm of the number of women born plus one. 'Share Women' denotes the number of women by the number of men and women in the same category. All columns control for city and period fixed effects as well as city-specific linear trends in addition to interacting city controls with period fixed effects to capture variation from economic, religious, and educational differences. Standard errors clustered by city shown in parentheses. $* \ p < 0.10$, $** \ p < 0.05$, $*** \ p < 0.01$

Figure E.1: Placebo estimates: Distributing Schools across Germany and centuries



Each figure reports the point estimates from 200 randomization exercises that proceed as follows: We use the number of schools in every period and randomly distribute them across Germany. This is repeated for every period and used as a new explanatory variable in a regression with full controls. The outcome is an indicator equal to one if a city had at least one notable woman from the respective occupational group born in this period. The vertical line marks the baseline estimate in Table 1 column (2).

F Recent advances in event-study designs: DID with multiple time periods or heterogeneous treatment effects

There has been a rich recent debate in the literature on how to interpret the average treatment effect on the treated in event-study designs. Following these developments, Baker et al. (2021) argue that "staggered treatment timing and treatment effect heterogeneity, either accross groups or over time, leads to biased Two-Way-Fixed-Effects DID [TWFE] estimates for the ATT", and propose three methods to assess the severity of this bias. First, show the event-study graph without controls (Figure 2). Second, implement the methods by de Chaisemartin and D'Haultfœuille (2020) to assess whether any treatment group receives negative weights, and the method by Callaway and Sant'Anna (2021) to assess whether treatment heterogeneity by treatment period bias our estimates. Third, show the implied weights following Goodman-Bacon (2020), showing that the main effect is derived from the comparison treatment versus control (Figure F.1). All methods provide no evidence of different pre-trends and provide similar point estimates, highlighting the validity of our empirical approach.

First, we show that heterogeneous treatment effects do not bias our estimates. Implementing the suggested methods, we estimate an average treatment effect on the treated (ATT) of 0.157 (s.e. 0.040) for the method of Callaway and Sant'Anna (2021) and 0.159 (s.e. 0.039) using the method by de Chaisemartin and D'Haultfœuille (2020). These estimates are very close to our baseline ATT in Figure 2 (0.157, s.e. 0.038). We find no evidence of negative treatment weights affecting our results in two tests: neither the method by de Chaisemartin and D'Haultfœuille (2020), nor plotting the residuals from a regression of our treatment variable on time and unit fixed effects reveal negative weights or treatment effects for any group. In our setting this result is unsurprising: School opening would have to create *fewer* notable women to create negative weights.

Another way to assess the validity of our approach is by estimating the implied weight of each treatment period. In a classical event study design where one focuses on cities that ever establish treatment, late treatment cities are the implied control cities for early treatment cities. (Goodman-Bacon, 2020). Then, TWFE estimates are a weighted sum of individual treatment effects estimated for every city and period. Since these weights can be negative, inference can be affected. Using the approach suggested by Goodman-Bacon (2020), we show in Table F.1 that the weight of the effect comes from the comparison between treated and never-treated. This result is confirmed in Figure F.1, where the DID estimate is almost exclusively derived from the differences between cities without and with finishing schools, thus validating our approach.

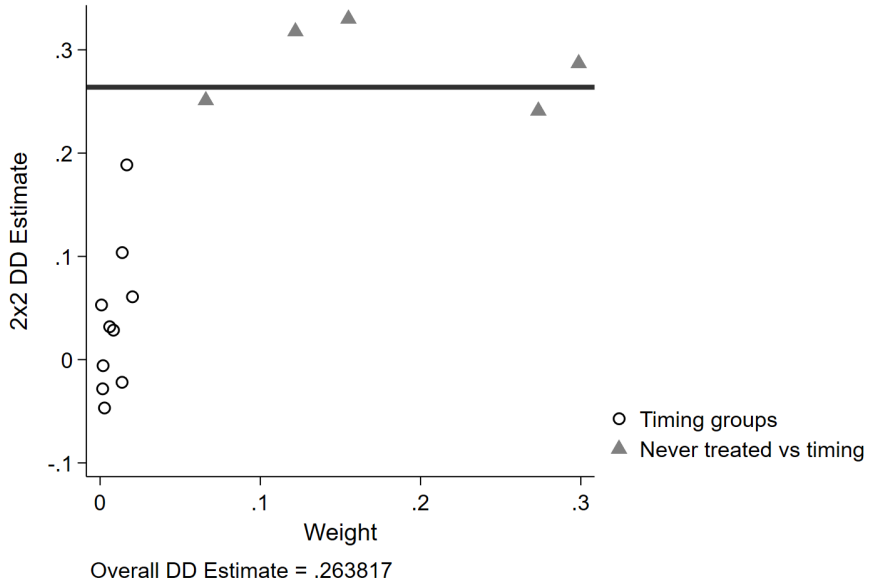
Figure F.1 suggests that the point estimate in our TWFE estimation stems from the difference between never-treated cities and cities with finishing schools. We thus provide additional evidence for the parallel trends assumption including all cities. In our main Figure 2, we show parallel trends in the set of cities that ever established finishing schools. In Figure F.3, we complement this evidence by including cities that never established a finishing school. The results speak in favor

of the parallel trends assumption: When controlling extensively for economic, religious and educational covariates, the estimated leads are centered around zero and show no difference between cities with and without finishing schools.

Table F.1: Goodman-Bacon (2020) decomposition of differences-in-differences estimation with variation in treatment timing

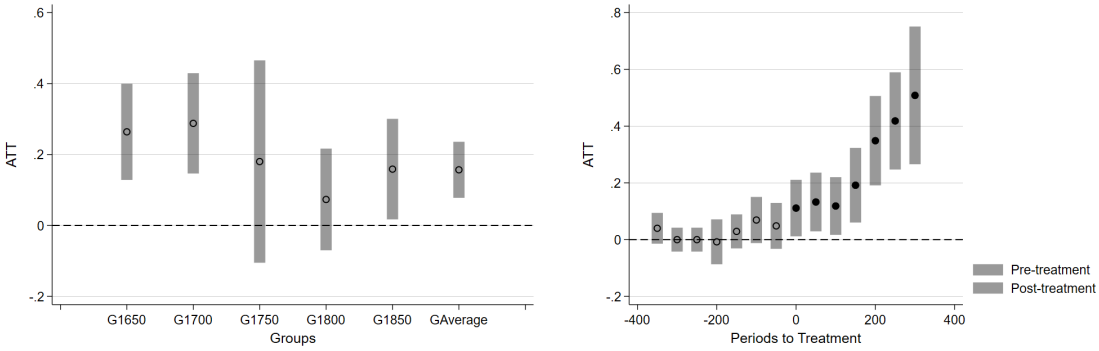
	$\mathbb{I}[Women > 0]$	
	Weight	Av. DID Est.
Timing Groups	0.085	0.093
Treatment vs Never treated	0.915	0.281
Differences-in-differences estimate:		0.264

Figure F.1: Goodman-Bacon (2020) decomposition of differences-in-differences estimation with variation in treatment timing



Showing the implied weights against the treatment effect when using the indicator $\mathbb{I}[Women > 0]$. The treatment effect is amost entirely estimated from the comparison of treated cities to non-treated cities.

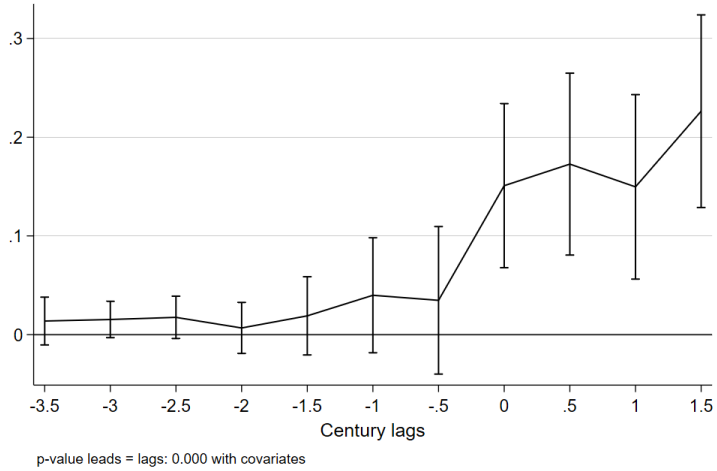
Figure F.2: Callaway and Sant'Anna (2021) Aggregation of treatment effects



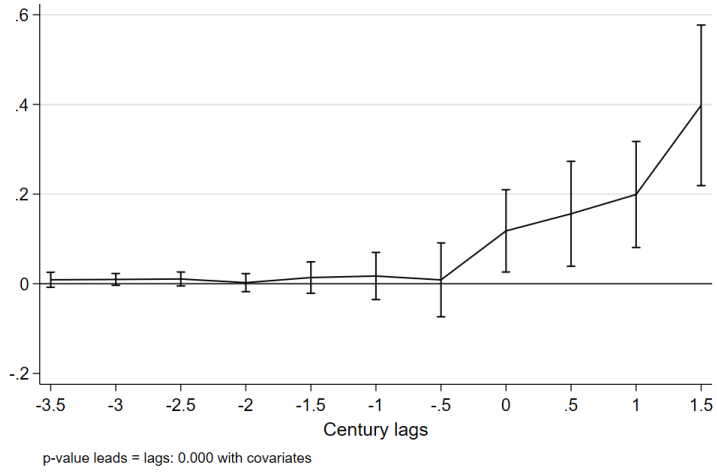
(a) Group specific ATT

(b) Event specific ATT

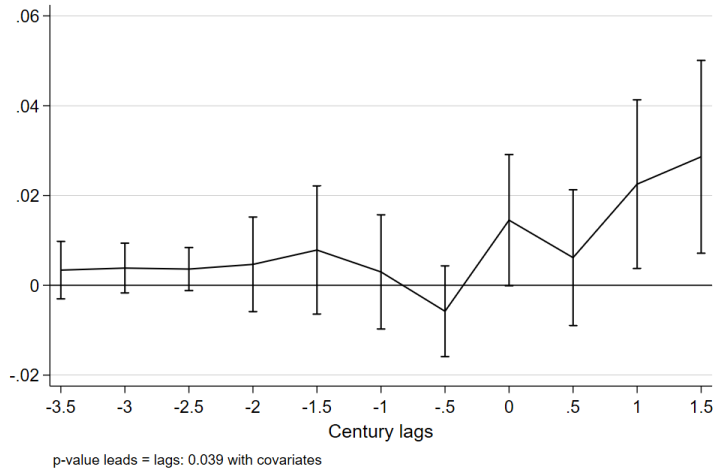
Figure F.3: Event-Study: Impact of finishing school establishment on notable women



(a) Indicator funtion: Notable woman born in city



(b) Logarithm of the number of notable women born in city



(c) Share of notable women born in city

Additional results for *non-noble secular* women, including all control cities. The outcome is an indicator equal to one if a notable woman from the respective group was born in a given city and period. In contrast to Figure 2 in the main text, here we also include cities which never established finishing schools to improve precision. Zero is the normalized time of establishment of finishing schools in the city; -4 is the omitted period and includes all never-treated cities. To capture differences between cities with and without finishing schools, we extensively control for city characteristics in all panels. 95%-confidence intervals reported.

G Standard differences-in-differences estimates and possible instruments in the panel setting

In this Appendix, we show results from a standard differences-in-differences estimator, comparing cities without finishing schools (control group) with cities that establish a finishing school by 1850 (treatment group) to complement our assessment of pre-trends in the event-study setting and assess whether specific periods impact the estimates disproportionately. We then continue and analyze whether the diffusion of Protestantism threatens the interpretation of our findings (Becker and Woessmann, 2009). We conclude this Appendix with a complementary empirical strategy using monasteries established before 1300 as an instrument for finishing schools. We document local average treatment effects that are very similar to the main results presented in the paper.

G.1 Standard differences-in-differences

We start by splitting our sample into cities that established finishing schools by 1850 and cities which did not and compare women's representation among the human capital elite in these two sets of cities before and after 1650, the period in which the first finishing school was founded. While this strategy allows for a more standard analysis of pre-trends than an event-study strategy, it also combines many treament periods into one, and thus likely underestimates the true impact. In Figure G.1, we document the absence of significant pre-trends for both the extensive margin (establishing a school) and the intensive margin (number of schools). Yet, both panels reveal an increase in women's representation among the human capital elite in the periods after the first finishing school was established (1626). The differences-in-differences estimators are reported in Table G.1 for both margins. First, the point estimates are very similar to the baseline results reported in Table 1 and are stable across specifications. Second, the point estimates on the intensive and extensive margin do not differ in most cases.

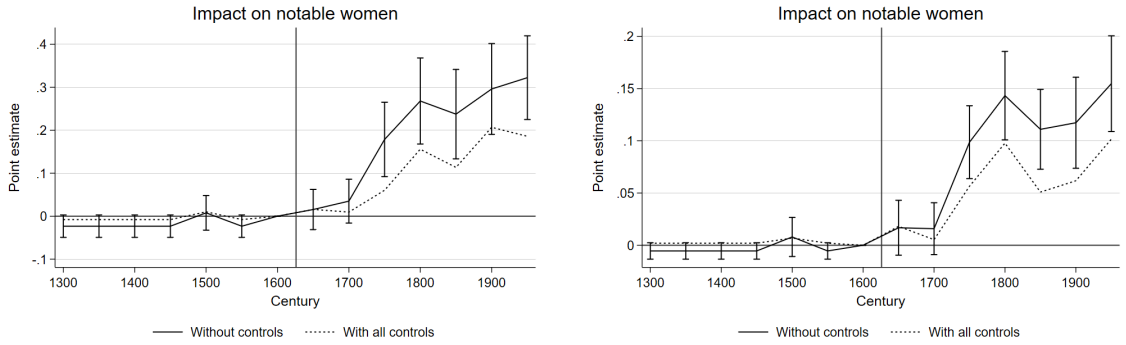
We continue and present DID-estimates for each period. The results in Table G.2 reveal no significant differences across school opening years. Here, we jointly estimate all treatment periods as compared to cities that never establish schools and find similar impacts across all types of schools. The only insignificant period is 1750, in which only three schools were established. Yet, even here the point estimate is statistically indistinguishable from the other periods.

We take this as evidence that our conclusion that finishing schools increase the share of women among the human capital elite is not driven by the functional form, identification strategy, or any period in particular. Also, while one could reasonably assume that the lack of variation in the outcome in the periods leading up to 1650 makes a pre-trend assessment problematic, the pre-trends are also insignificant in periods with more outcome variation such as the years 1600-1800 for the cities that establish finishing schools only in the 1850 period.

The effects in Table G.2 also indicate that the main effect in our baseline estimate is not driven by unobserved characteristics of the set of cities ever receiving finishing schools, which generally affect women's representation among the human capital elite in these cities after 1600. The tem-

poral correspondence between the establishment of finishing schools and the timing of the effects (and the absence of pre-trends) certainly cannot alleviate all concerns about the potential endogeneity of the timing of school opening; however, it clearly points to an important nexus between the opening of finishing schools and the subsequent increase in women's representation among the human capital elite.

Figure G.1: Differences-in-differences estimation: Comparing cities with and without finishing schools over time



(a) Any finishing school **(b)** Number of schools

These graphs split the data into cities that ever establish at least one finishing school and those without and compare those before and after 1650. The outcome is an indicator equal to one if a notable woman was born in a given city and period. The left Panel reports the point estimates from the interaction between period fixed effects and whether the city ever established a finishing school $\in \{0,1\}$. The right Panel reports the point estimates from the interaction between period fixed effects and the number of schools that have been established in this city by 1850 $\in \{0,1,2,3,4,5,8,10\}$. The omitted period is 1600, the period before the first schools were opened. Estimates without (solid line) and with (dashed line) all controls all indicate no pre-trends and an increase in the likelihood of women becoming notable only after the opening of the first school. While the left Panel can be interpreted as the extensive margin of finishing schools: "Whether cities were different before", the right Panel represents "how different these cities were before".

Table G.1: Differences-in-Differences Estimation:
Establishing finishing schools in cities

	$\mathbb{I}[Women > 0]$			log Women			Share Women		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Non-Noble Seculars									
Finishing school × Post 1650	0.162***	0.082***	0.077***	0.220***	0.091***	0.087***	0.014***	0.009**	0.007
	(0.020)	(0.019)	(0.021)	(0.033)	(0.024)	(0.028)	(0.004)	(0.004)	(0.005)
# Finishing schools × Post 1650	0.078***	0.044***	0.041***	0.145***	0.105***	0.116***	0.007***	0.005***	0.005***
	(0.011)	(0.009)	(0.010)	(0.021)	(0.023)	(0.031)	(0.001)	(0.002)	(0.002)
Panel B: Nobility									
Finishing school × Post 1650	0.025	0.003	0.008	0.037**	0.015	0.019	0.001	-0.004	-0.003
	(0.018)	(0.022)	(0.032)	(0.017)	(0.019)	(0.029)	(0.006)	(0.008)	(0.010)
# Finishing schools × Post 1650	0.019**	0.010	0.013	0.042***	0.039***	0.051***	0.000	-0.003	-0.004
	(0.008)	(0.009)	(0.011)	(0.010)	(0.011)	(0.015)	(0.003)	(0.003)	(0.004)
Unit trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
City covariates × period FE		Yes	Yes		Yes	Yes		Yes	Yes
Religious covariates × period FE		Yes	Yes		Yes	Yes		Yes	Yes
Educational covariates × period FE		Yes	Yes		Yes	Yes		Yes	Yes
Matching on 1600 covariates			Yes			Yes			Yes
Observations	9,312	9,240	8,856	9,312	9,240	8,856	9,312	9,240	8,856

Results using a 'standard' differences-in-differences setup. We divide the data according to whether a city had a finishing school in 1850 (first row of each Panel) to capture the extensive margin of establishing a school. In the second row of each Panel, we use the same division, but use the number of schools to capture the intensive margin of establishing a schools. We then interact these with a post 1650 indicator to capture the DID estimator. We consider three types of dependent variables to capture the extensive and intensive margin of the birth of notable women. $\mathbb{I}[Women > 0]$ is an indicator equal to one if a city had at least one notable woman born in this period. 'log Women' constitutes the natural logarithm of the number of women born plus one. 'Share Women' denotes the number of women by the number of men and women in the same category. We regress the number of non-noble secular women and noble women born in a city, as defined in the top row, on our finishing school variable. Columns (1), (4), and (7) constitute the baseline and include city and period fixed effects as well as city-specific linear trends. In the remaining columns we control for city and period fixed effects as well as city-specific linear trends in addition to interacting city controls with period fixed effects to capture variation from economic, religious, and educational differences. We match cities on pre-1650 covariates to compare cities with the same pre-treatment characteristics. We include 86 match-specific fixed effects interacted with period fixed effects to confine the variation to within pre-treatment similar groups of cities. Standard errors clustered by city shown in parentheses. $* \ p < 0.10$, $** \ p < 0.05$, $*** \ p < 0.01$

Table G.2: Differences-in-Differences Estimation:
Establishing finishing schools in different periods

	$\mathbb{I}[Women > 0]$			log Women			Share Women		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Non-Noble Seculars									
Finishing school by 1650 × post 1650	0.220***	0.134**	0.132*	0.329***	0.172**	0.132	0.025***	0.021*	0.026**
	(0.048)	(0.056)	(0.074)	(0.087)	(0.082)	(0.099)	(0.009)	(0.011)	(0.012)
Finishing school by 1700 × post 1700	0.230***	0.140**	0.123	0.281***	0.136*	0.059	0.027***	0.028***	0.027*
	(0.073)	(0.055)	(0.090)	(0.087)	(0.072)	(0.119)	(0.008)	(0.009)	(0.014)
Finishing school by 1750 × post 1750	0.185	0.102	0.209	0.777*	0.630*	1.002**	0.024	0.023	0.043*
	(0.131)	(0.104)	(0.150)	(0.445)	(0.375)	(0.476)	(0.020)	(0.024)	(0.025)
Finishing school by 1800 × post 1800	0.209***	0.142***	0.179***	0.354***	0.188**	0.202*	0.013*	0.015*	0.020*
	(0.049)	(0.051)	(0.064)	(0.095)	(0.082)	(0.109)	(0.008)	(0.009)	(0.012)
Finishing school by 1850 × post 1850	0.183***	0.132***	0.147***	0.157***	0.040	0.050	-0.000	0.003	0.010
	(0.048)	(0.047)	(0.053)	(0.053)	(0.061)	(0.072)	(0.008)	(0.008)	(0.009)
Unit trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
City covariates × period FE		Yes	Yes		Yes	Yes		Yes	Yes
Religious covariates × period FE		Yes	Yes		Yes	Yes		Yes	Yes
Educational covariates × period FE		Yes	Yes		Yes	Yes		Yes	Yes
Matching on 1600 covariates			Yes			Yes			Yes
Observations	9,312	9,240	8,856	9,312	9,240	8,856	9,312	9,240	8,856

Results using a 'standard' differences-in-differences setup. We divide the data according to whether a city had a finishing school in the indicated year and interact this variable with a post year indicator to capture the DID estimator. All coefficiencts are jointly estimated. We consider three types of dependent variables to capture the extensive and intensive margin of the birth of notable women. $\mathbb{I}[Women > 0]$ is an indicator equal to one if a city had at least one notable woman born in this period. 'log Women' constitutes the natural logarithm of the number of women born plus one. 'Share Women' denotes the number of women by the number of men and women in the same category. We regress the number of non-noble secular women and noble women born in a city, as defined in the top row, on our finishing school variable. Columns (1), (4), and (7) constitute the baseline and include city and period fixed effects as well as city-specific linear trends. In the remaining columns we control for city and period fixed effects as well as city-specific linear trends in addition to interacting city controls with period fixed effects to capture variation from economic, religious, and educational differences. We match cities on pre-1650 covariates to compare cities with the same pre-treatment characteristics. We include 86 match-specific fixed effects interacted with period fixed effects to confine the variation to within pre-treatment similar groups of cities. Standard errors clustered by city shown in parentheses. $* \ p < 0.10$, $** \ p < 0.05$, $*** \ p < 0.01$

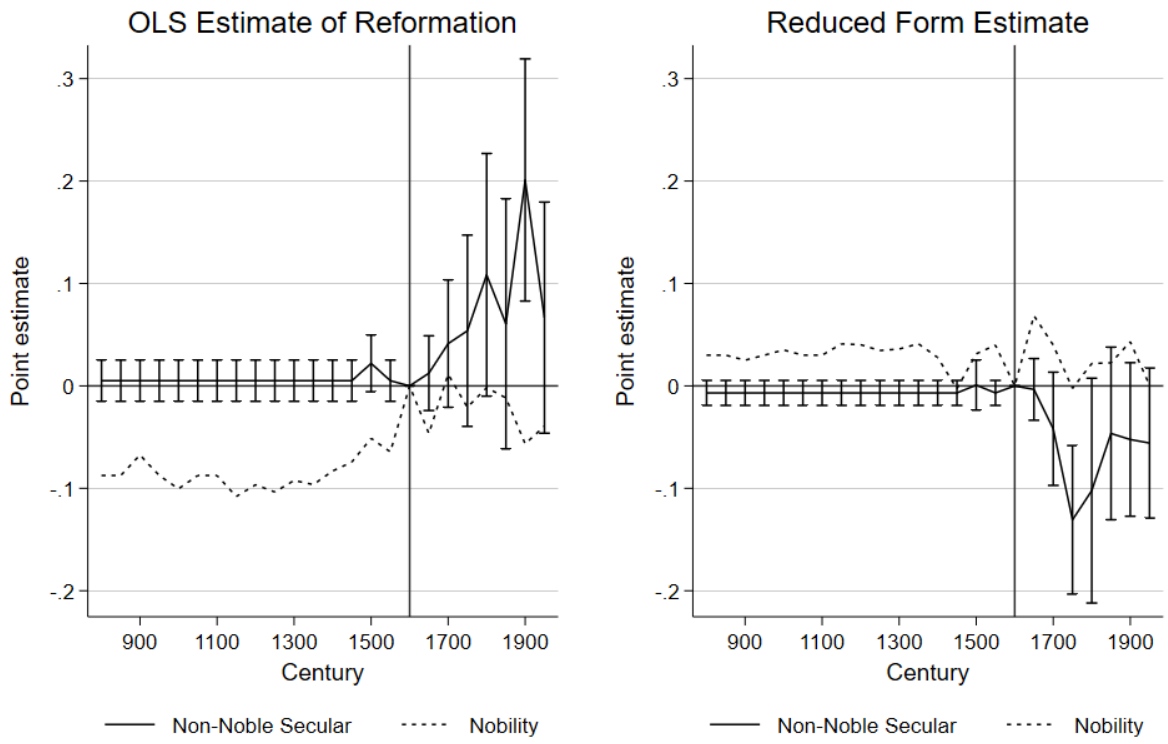
G.2 Protestantism as a confounding factor

Next, we turn to the diffusion of Protestantism as a potential confounding factor. Martin Luther advocated the education of women to enable their independent study of the bible (Becker and Woessmann, 2009). It is important to note, however, that he only argued for primary education (particularly reading), and not the secondary education and teacher training provided by finishing schools. We thus do not expect a significant impact of the Protestant Reformation on women's representation among the human capital elite. In order to obtain a causal estimate that is not confounded by the potentially endogeneous decision to adopt Protestantism, we also provide estimates using an instrumental variables strategy based on a city's distance to Wittenberg, the Reformation's epicenter.

We assess the impact of the Protestant Reformation on women's representation among the human capital elite in Figure G.2. In the right-hand Panel, we report estimates from an OLS regression of an indicator whether a notable woman was born in a given city and period on an indicator for whether a certain city adopted Protestantism by 1650. The lead-lag estimates suggest no consistently significant and positive effect of the Protestant Reformation on women's representation among the human capital elite until 1900. In the right hand Panel, we report estimates from a reduced form exercise where we replace the indicator for having adopted Protestantism by 1650 with the distance to Wittenberg, the city from which Protestantism spread across Germany. Again, we find no consistent positive effect on notable women. Taken together, Figure G.2 suggests that our main results on the nexus between finishing schools and women's increasing representation among the human capital elite are unlikely to merely reflect the effects of the Protestant Reformation. The differences-in-differences estimates (odd columns) and reduced form estimates (even columns) in Table G.3 confirm this pattern as they do not reveal a significant impact of the Reformation on women among the human capital elite.[47]

[47] We also find no evidence of a heterogeneous effect of the Reformation on the number of notable women.

Figure G.2: Using the Protestant Reformation as explanatory variation



Estimating the impact of switching to Protestantism and the reduced form impact of the log distance to Wittenberg across all time periods in our data for non-noble secular women and women from the nobility. The outcome is an indicator equal to one if a notable woman from the respective group was born in a given city and period. We exclude religious controls in all estimations. 95%-confidence intervals shown only for non-noble secular, the impact of nobility is indistinguishable form zero in all periods and specifications.

Table G.3: Differences-in-Differences Estimation:
Switch to Protestantism as a cultural shock to the role of women in society

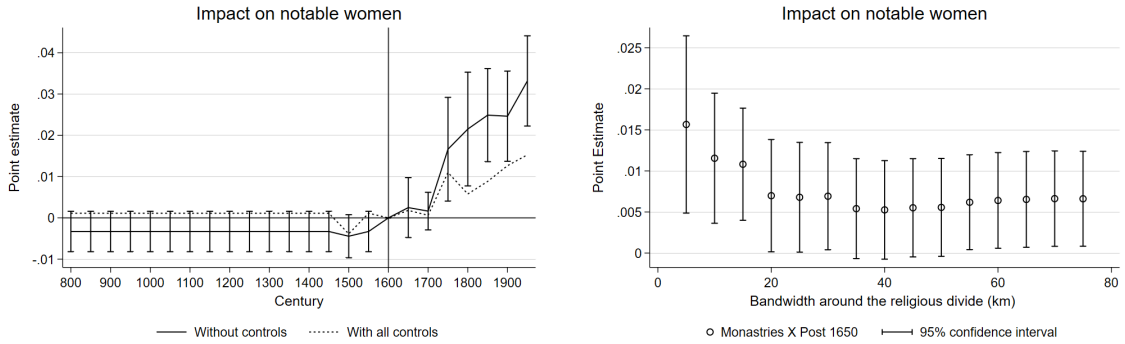
	$\mathbb{I}[Women > 0]$		log Women		Share Women	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Non-Noble Seculars						
Reformation in City × Post 1650	0.057***		0.057**		0.003	
	(0.020)		(0.027)		(0.004)	
log Distance to Wittenberg × Post 1650		-0.056***		-0.053*		-0.003
		(0.016)		(0.029)		(0.003)
Panel B: Nobility						
Reformation in City × Post 1650	0.031		0.027		0.008	
	(0.024)		(0.021)		(0.008)	
log Distance to Wittenberg × Post 1650		0.011		0.007		0.006
		(0.014)		(0.013)		(0.005)
Unit trend	Yes	Yes	Yes	Yes	Yes	Yes
City covariates × period FE	Yes	Yes	Yes	Yes	Yes	Yes
Educational covariates × period FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	9,288	9,288	9,288	9,288	9,288	9,288
F-Test		7.602		7.602		7.602

Odd columns show results of a differences-in-differences estimation using an indicator variable whether a city has adopted Protestantism by 1650. Even columns show results of a reduced form exercise using log distance to Wittenberg as a proxy for whether a city switched to Protestantism. The first stage regression of switching to Protestantism on log Distance to Wittenberg has an F-Stat of 16.23. We consider three types of dependent variables to capture the extensive and intensive margin of the birth of notable women. $\mathbb{I}[Women > 0]$ is an indicator equal to one if a city had at least one notable woman born in this period. 'log Women' constitutes the natural logarithm of the number of women born plus one. 'Share Women' denotes the number of women by the number of men and women in the same category. We regress the number of non-noble secular women and noble women born in a city, as defined in the top row, on our finishing school variable. Columns (1), (4), and (7) constitute the baseline and include city and period fixed effects as well as city-specific linear trends. In the remaining columns we control for city and period fixed effects as well as city-specific linear trends in addition to interacting city controls with period fixed effects to capture variation from economic, religious, and educational differences. Standard errors clustered by city shown in parentheses. $* \ p < 0.10$, $** \ p < 0.05$, $*** \ p < 0.01$

G.3 Monasteries as an instrument

Finally, we discuss a potential instrument for the establishment of finishing schools. From historical accounts we know that most of the early finishing schools were founded by Catholic nuns (Albisetti, 1988). These nuns were often invited by rulers of German states and settled in available space in and around existing monasteries. We use monasteries that were established by 1300, more than 300 years prior to the opening of the first finishing school, as an instrument for finishing school establishment. With this instrument we exploit variation in the supply of buildings which could be converted to (or expanded to include) finishing schools at fairly low cost. By additionally limiting our analysis to cities in close vicinity to the inner-German denominational divide between Protestants and Catholics as of 1619, we hold religious competition constant. Thus, we estimate effects net of any direct impact of religious competition which the historical literature on finishing schools suggests as is a determinant of finishing school establishment (Lewejohann, 2014). The key identification assumption is then that the number of monasteries established by 1300 in areas which were to become religiously competitive around the year 1600 only affects women's representation among the human capital elite via the construction of finishing schools. Figure G.3 summarizes our findings. Using monasteries as an instrument provides reliable reduced form estimates that suggest a relevant instrument that is independent of the chosen bandwidth around the religious divide.

Figure G.3: Reduced Form Estimates: Using monasteries in 1300 as an instrument



Estimating the reduced form impact of monasteries in 1300 on Non-Noble Secular women across all time periods in our data within 10km of the religious divide (left). The outcome is an indicator equal to one if a notable woman was born in a given city and period. Estimates with and without all controls all indicate no pre-trends and an increase in the likelihood of women becoming notable after the opening of the first school in 1626. Sensitivity of the point estimate comparing pre- and post-treatment periods to various bandwidths shown in the right figure. All controls included.

H Specification and robustness in the cross-sectional setting

In this Appendix, we want to highlight that our cross-sectional setting is robust to using an instrumental variables estimation, to estimating effects of a city's length of exposure to finishing schools, and to matching on observables.

First, we discuss a potential instrument for the establishment of finishing schools. From historical accounts we know that most of the early finishing schools were founded by Catholic nuns (Albisetti, 1988). These nuns were often invited by rulers of German states and settled in available space in and around existing monasteries. We use monasteries that were established by 1300, more than 300 years prior to the opening of the first finishing school, as an instrument for finishing school establishment. With this instrument we exploit variation in the supply of buildings which could be converted to (or expanded to include) finishing schools at fairly low cost. By additionally limiting our analysis to cities in close vicinity to the inner-German denominational divide between Protestants and Catholics as of 1619, we can hold religious competition constant and thus estimate effects net of any direct impact of religious competition which the historical literature on finishing schools suggests is a determinant of finishing school establishment (Lewejohann, 2014). The key identification assumption is then that the number of monasteries established by 1300 in areas which were to become religiously competitive around the year 1600 only affects women's representation among the human capital elite via the construction of finishing schools.

In Table H.1, we show that indeed using the number of monasteries existing in 1300 as an instrument for the number of finishing schools in 1850 produces consistent estimates throughout all outcomes and main specifications (columns 1 and 4). Changing the cutoff year for pre-existing monasteries closer to 1648, the end of the Thirty Years' War, produces similarly sized estimates, yet smaller F-statistics (columns 2, 3, 5, and 6).

Finally, we estimate effects of a city's length of exposure to finishing schools (instead of the absolute number of finishing schools). In Table H.2, we show that changing the independent variable to years since first opening produces very similar results in a wide range of specifications. Here, we define '0' as having no school in 1850, and progressively move back in time to '224', indicating the school was build in 1626. In Table H.2 we thus investigate whether more time elapsed since the establishment of the first finishing school in city – and thereby a greater representation of women among the human capital elite – is associated with stronger support of the women's rights movement.

At a mean of 20 years of exposure to finishing schools, increasing the number of years by 10% (2 years), increases the number of letters to *Frauenzeitung* by 0.56%, the number of women's rights associations by 5% and the number of female members of parliament by 0.25% and 0.95% respectively. Or to put it differently, had a city opened a finishing school in 1800 (instead of never) and thus had 50 years more exposure to such a school, this would imply a 250% increase in exposure compared to the mean of 20 years. This city would have sent 14% more letters, hosted twice the number of women's rights organizations, and sent 24% more women to postwar parliaments. These are sizable effects, for a relatively small change in exposure.

Table H.1: Long-term impact of finishing schools on political outcomes:
IV estimates using different timings of the Monastery instrument

	$\mathbb{I}[>0]$			log Number		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Encyclopedia of female writers						
Finishing schools	0.174	0.177	0.074	0.479***	0.478***	0.363***
	(0.131)	(0.148)	(0.127)	(0.137)	(0.152)	(0.131)
Mean, untreated	0.130	0.130	0.130	0.137	0.137	0.137
Panel B: Leserbriefe, Frauenzeitung, 1849–1852						
Finishing schools	0.246**	0.269**	0.291**	0.412***	0.488***	0.442**
	(0.096)	(0.103)	(0.117)	(0.156)	(0.182)	(0.187)
Mean, untreated	0.038	0.038	0.038	0.061	0.061	0.061
Panel C: Women's rights organizations						
Finishing schools	0.264	0.239	0.204	1.674*	1.468	1.493*
	(0.181)	(0.187)	(0.180)	(0.929)	(0.964)	(0.881)
Mean, untreated	0.153	0.153	0.153	34.160	34.160	34.160
Panel D: Member Parliament, 1919–1933						
Finishing schools	0.155*	0.109	0.125	0.217**	0.179**	0.213**
	(0.088)	(0.086)	(0.100)	(0.086)	(0.089)	(0.089)
Mean, untreated	0.038	0.038	0.038	0.053	0.053	0.053
City Covariates	Yes	Yes	Yes	Yes	Yes	Yes
Religious covariates	Yes	Yes	Yes	Yes	Yes	Yes
Educational covariates	Yes	Yes	Yes	Yes	Yes	Yes
Observations	183	183	183	183	183	183
Bandwidth	10	10	10	10	10	10
Monastery Year	1300	1500	1648	1300	1500	1648
F-Stat first stage	9.750	8.070	9.424	9.750	8.070	9.424

Results using a two-stage least-squares estimation (2SLS) in all columns, changing the monastery date to 1300 (Columns (1) and (4)), 1500 (Columns (2) and (5)) and 1648 (Columns (3) and (6)). We instrument the number of finishing schools in 1850 in city c with the number of monasteries in city c, comparing cities within 10 km of the inner-German religious divide to proxy religious competition and capture similar cities. In each Panel we regress an indicator variable for the existence and the natural logarithm plus one of the number of instances on the number of finishing schools. In Panel A we estimate whether finishing schools increase the likelihood of important women being born in cities in a cross-section, replicating our main results. In Panel B we estimate whether finishing schools increase the likelihood and number of letters written from city c to the first active feminist newspaper in Germany. In Panel C we analyze whether finishing schools increase the likelihood and member count of local chapters of the women's rights organizations in city c. In Panel D we estimate the impact of finishing schools on the likelihood and number of female members of parliament from their birthplace c. In Panel E we repeat the exercise for female members of parliament in the German parliament until 2019. We include all covariates as defined in equation (1) in all regressions. Standard errors clustered by city shown in parentheses. $* \ p < 0.10$, $** \ p < 0.05$, $*** \ p < 0.01$

Table H.2: Long-term impact of finishing schools on political outcomes:
Years of Schooling

	$\mathbb{I}[>0]$				log Number			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Encyclopedia of female writers								
Years since first opening	0.002***	0.002**			0.003***	0.003***		
	(0.001)	(0.001)			(0.001)	(0.001)		
log Years since first opening			0.077***	0.071***			0.098***	0.092***
			(0.017)	(0.024)			(0.020)	(0.025)
Mean, untreated	0.210	0.130	0.210	0.130	0.276	0.137	0.276	0.137
Panel B: Leserbriefe, Frauenzeitung, 1849–1852								
Years since first opening	0.001*	0.001			0.002**	0.001		
	(0.000)	(0.001)			(0.001)	(0.001)		
log Years since first opening			0.024**	0.025*			0.056***	0.061*
			(0.011)	(0.015)			(0.021)	(0.035)
Mean, untreated	0.062	0.038	0.062	0.038	0.105	0.061	0.105	0.061
Panel C: Women's rights organizations								
Years since first opening	0.002***	0.001			0.012***	0.010**		
	(0.001)	(0.001)			(0.003)	(0.005)		
log Years since first opening			0.067***	0.068***			0.402***	0.431***
			(0.016)	(0.023)			(0.086)	(0.129)
Mean, untreated	0.187	0.153	0.187	0.153	37.977	34.160	37.977	34.160
Panel D: Member Parliament, 1919–1933								
Years since first opening	0.001**	0.001			0.001***	0.001*		
	(0.000)	(0.001)			(0.000)	(0.001)		
log Years since first opening			0.023**	0.022			0.025**	0.020*
			(0.011)	(0.014)			(0.010)	(0.011)
Mean, untreated	0.066	0.038	0.066	0.038	0.074	0.053	0.074	0.053
City Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Religious covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Educational covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	385	183	385	183	385	183	385	183
Bandwidth	400	10	400	10	400	10	400	10

Results using a baseline regression with the full sets of controls in all regressions, and comparing coefficients to the limited sample within 10 km of the German religious divide in even columns. In each Panel we regress an indicator variable for the time since and the natural logarithm plus one of the the years since the opening of the first schools or its natural logarithm plus one. Cities without schools in 1850 are coded as having zero years of school. Time is related to 1850, such that Aachen, with the first established school in 1626, has 224 years of schooling. In Panel A we estimate whether finishing schools increase the likelihood of important women being born in cities in a cross-section, replicating our main results. In Panel B we estimate whether finishing schools increase the likelihood and number of letters written from city c to the first active feminist newspaper in Germany. In Panel C we analyze whether finishing schools increase the likelihood and member count of local chapters of the women's rights organizations in city c. In Panel D we estimate the impact of finishing schools on the likelihood and number of female members of parliament from their birthplace c. In Panel E we repeat the exercise for female members of parliament in the German parliament until 2019. We include all covariates as defined in equation (1) in all regressions. Standard errors clustered by city shown in parentheses. $* \ p < 0.10$, $** \ p < 0.05$, $*** \ p < 0.01$

H.1 Comparison to propensity score matching

As a final step, we show robustness of our results to matching each city to its closest counterparts based on observable characteristics. The point estimates in columns (3) and (6) are not statistically different from the OLS (columns 1 and 4) or the sample of cities that lie within 10 km of the religious divide (columns 2 and 5). In addition, the matched sample shows no signs of imbalances across all covariates (Table H.4, column 6).

Table H.3: Long-term impact of finishing schools on political outcomes:
Comparison to Matching estimators

	$\mathbb{I}[>0]$			log Number		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Encyclopedia of female writers						
Finishing schools	0.120***	0.180***	0.110***	0.310***	0.329***	0.325***
	(0.028)	(0.039)	(0.031)	(0.040)	(0.051)	(0.054)
Panel B: Leserbriefe, Frauenzeitung, 1849–1852						
Finishing schools	0.095***	0.122***	0.114***	0.183***	0.241**	0.185***
	(0.020)	(0.037)	(0.018)	(0.055)	(0.097)	(0.061)
Panel C: Women's rights organizations						
Finishing schools	0.090***	0.126**	0.107***	0.723***	0.937***	0.839***
	(0.025)	(0.049)	(0.034)	(0.133)	(0.271)	(0.189)
Panel D: Member Parliament, 1919–1933						
Finishing schools	0.067***	0.101***	0.045**	0.100***	0.105***	0.089**
	(0.018)	(0.034)	(0.022)	(0.029)	(0.035)	(0.038)
City Covariates	Yes	Yes	Yes	Yes	Yes	Yes
Religious covariates	Yes	Yes	Yes	Yes	Yes	Yes
Educational covariates	Yes	Yes	Yes	Yes	Yes	Yes
Propensity score matching			Yes			Yes
Observations	385	183	369	385	183	369
Bandwidth		10			10	

Results using a baseline regression with the full sets of controls in all regressions (columns 1 and 4), comparing coefficients to the sample limited to 10km of the religigous boundary in 1619 (columns 2 and 5), as well as to a propensity score matching (columns 3 and 6). In Panel A we estimate whether finishing schools increase the likelihood of important women being born in cities in a cross-section, replicating our main results. In Panel B we estimate whether finishing schools increase the likelihood and number of letters written from city c to the first active feminist newspaper in Germany. In Panel C we analyze whether finishing schools increase the likelihood and member count of local chapters of the women's rights organizations in city c. In Panel D we estimate the impact of finishing schools on the likelihood and number of female members of parliament from their birthplace c. In Panel E we repeat the exercise for female members of parliament in the German parliament until 2019. We include all covariates as defined in equation (1) in all regressions. Standard errors clustered by city shown in parentheses. $*$ $p < 0.10$, $**$ $p < 0.05$, $***$ $p < 0.01$

Table H.4: Outcomes and covariates prior to first finishing school (1600)

	Unmatched sample			Matched sample		
	β	s.e.	p-value	β	s.e.	p-value
Panel A: Outcome: Share of the human capital elite in 1600 (prior to first finishing school):						
Share non-noble secular women	0.004	0.003	0.090	-0.000	0.000	1.000
Share noble women	0.003	0.007	0.636	0.004	0.011	0.676
Panel B: Control variables as recored in 1600 (prior to first finishing school):						
log(Population)	0.917	0.115	0.000	0.070	0.069	0.311
log(Distance Wittenberg)	-0.207	0.081	0.010	0.088	0.072	0.223
log(Distance religious divide)	0.379	0.177	0.032	-0.128	0.204	0.530
Temperature in Spring	0.032	0.101	0.753	0.106	0.130	0.418
Temperature in Summer	0.142	0.136	0.296	-0.010	0.167	0.950
Temperature in Fall	-0.013	0.093	0.893	0.068	0.117	0.559
Temperature in Winter	-0.199	0.109	0.068	0.188	0.137	0.172
Hanse city	0.101	0.036	0.005	-0.023	0.038	0.543
Bishop seat	0.097	0.031	0.002	0.001	0.026	0.976
Jewish settlement	0.187	0.053	0.000	0.002	0.069	0.982
Progrom	0.087	0.053	0.105	0.019	0.072	0.789
Battle during 30-years war	0.175	0.047	0.000	-0.009	0.051	0.867
Boy school	0.020	0.033	0.552	0.006	0.044	0.883
University	0.019	0.021	0.357	-0.011	0.024	0.646
Catholic region	0.040	0.051	0.431	-0.011	0.052	0.835

This table presents the balance test on covariates in 1600, based on the regression $X_c = \alpha + \beta \cdot \text{Girl School}_{c,1850} + \varepsilon_c$. The unmatches sample contains all cities in 1600, whereas the matched sample only contains the nearest neighbor for each treatment city. While cities with schools are closer to Wittenberg, further away from the religious divide and have larger population in 1600, these differences disappear when matching cities to their nearest neighbor.

I Impact of notable women in 1800 on social change

In this Appendix, we directly ask what is the correlation between an additional non-noble secular women in 1800 and the changing role of women in society in the century that marked the greatest change to the role of women in society.

To this end, we estimate the following equation in Table I.1:

$$Y_{c,1900} = \alpha + \beta \cdot \log(\text{Number Non-Noble Seculars+1})_{c,1800} + \gamma \boldsymbol{X}_c + \varepsilon_c \tag{2}$$

Recognizing the endogeneity concerns associated with this equation, we nevertheless present estimates for their interpretability: a 1% increase in the number of notable women in a city is associated with a 0.4% increase in the number of female writers, 0.4% increase in correspondence (Panel B, column 6), a 1.4% increase in women's rights associations membership (Panel C, Column 6), and a 0.2% increase in the number of female members of parliament during the Weimar Republic (Panel D, column 6).

We conduct two exercises to judge the reliabilty of these correlations. First, we present point estimates with (odd columns) and without (even columns) controls. The estimates largely remain unchanged. Second, we instrument the number of notable women by the number of Schools in 1800. However, as the exclusion restriction, schools only affect political outcomes through their impact on notable women, might fail, we take these estimates with a caution. All 2SLS estimates are significant and larger than the OLS estimates with a strong first stage of 13.8: a 1% increase in the number of notable women in each city is associated with a 1.5% increase in the number of female writers (Panel A, column 8), a 1% increase in correspondence (Panel B, column 8), a 3.3% increase in women's rights associations (Panel C, column 8), and a 0.6% increase in the number of female members of parliament during the Weimar Republic (Panel D, column 8).

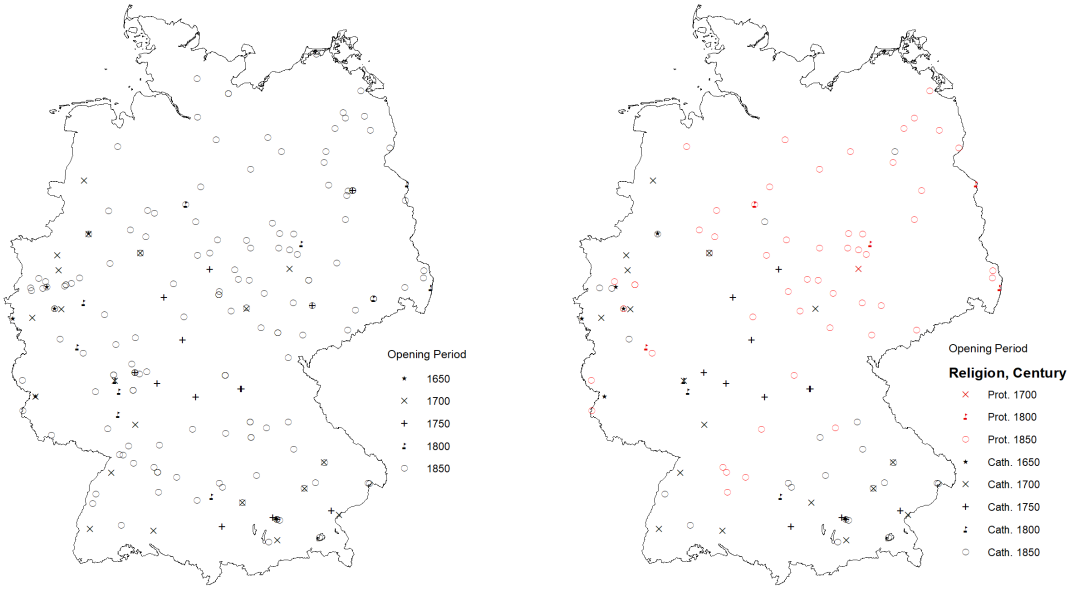
Both extensively controlling for confounding factors and instrumenting non-noble secular women by historical finishing schools suggest that a larger representation of women among the human capital elite increases women's political activity. Yet, as neither finishing schools nor non-noble secular women are likely randomly allocated to German cities in 1800, these estimates represent an informative correlation.

Table I.1: Impact of notable women in 1800 on social change

	$\mathbb{I}[>0]$				log Number			
	(1) OLS	(2) OLS	(3) IV	(4) IV	(5) OLS	(6) OLS	(7) IV	(8) IV
Panel A: Encyclopedia of female writers								
log(Number Non-Noble Seculars)	0.295***	0.132***	0.636***	0.527***	0.637***	0.456***	1.297***	1.413***
	(0.039)	(0.050)	(0.124)	(0.187)	(0.093)	(0.102)	(0.160)	(0.259)
Panel B: Leserbriefe, Frauenzeitung, 1849–1852								
log(Number Non-Noble Seculars)	0.248***	0.218***	0.376***	0.483***	0.445***	0.374***	0.734***	1.003***
	(0.039)	(0.039)	(0.082)	(0.135)	(0.098)	(0.091)	(0.182)	(0.312)
Panel C: Women's rights organizations								
log(Number Non-Noble Seculars)	0.350***	0.198***	0.571***	0.359***	2.204***	1.341***	3.977***	3.089***
	(0.039)	(0.049)	(0.103)	(0.137)	(0.210)	(0.259)	(0.589)	(0.733)
Panel D: Member Parliament, 1919–1933								
log(Number Non-Noble Seculars)	0.214***	0.111**	0.394***	0.343***	0.276***	0.179***	0.518***	0.542***
	(0.041)	(0.047)	(0.081)	(0.117)	(0.062)	(0.068)	(0.088)	(0.114)
City Covariates		Yes		Yes		Yes		Yes
Religious covariates		Yes		Yes		Yes		Yes
Educational covariates		Yes		Yes		Yes		Yes
Observations	388	385	388	385	388	385	388	385
First stage F-statistic			29.046	16.253			29.046	16.253

In each Panel we regress an indicator variable for the existence and the natural logarithm plus one of the number of instances on the number of women in each city in 1850. In columns (1), (2), (5) and (6), we use the log number of notable women born in 1800 as a predictor of social change. In the remaining columns, we instrument the log number of notable women in city c with the number existing schools in 1800. In Panel A we estimate whether finishing schools increase the likelihood of important women being born in cities in a cross-section, replicating our main results. In Panel B we estimate whether finishing schools increase the likelihood and number of letters written from city c to the first active feminist newspaper in Germany. In Panel C we analyze whether finishing schools increase the likelihood and member count of local chapters of the women's rights organizations in city c. In Panel D we estimate the impact of finishing schools on the likelihood and number of female members of parliament from their birthplace c. We include all covariates as defined in equation (1) in all regressions. Standard errors clustered by city shown in parentheses. $* \ p < 0.10$, $** \ p < 0.05$, $*** \ p < 0.01$

J Additional history on finishing schools



(a) School opening years in Germany, by religious denomination

(b) School opening years in Germany, by religious denomination

The left figure shows the location of finishing schools by their opening period. In Figure C.1, we additionally show the variation, including cities and the number of notable women. The right map shows the geographic distribution Catholic and Protestant schools in Germany. Differences between the left and right Figure are explained by privately funded schools without a religious denomination not shown in the right. The first schools were exclusively Catholic, as the first Protestant school opened in 1698. The first city-funded school opened in 1800, and the growth in school construction in the period 1800-1850 is likely driven by the downfall of the holy-roman empire (800-1806) freeing up resources from previous inner-german conflicts. More than 100 schools were build between 1825 and 1850 alone, most of them in Prussia relying on Bavarian female teachers. Dividing into early and late periods (Table C.4) or treatment periods (Table G.2) suggest no differential treatment effect along the timing dimension.

J.1 Pupils and Parents at finishing schools

In this section, we qualitatively address the role of parents and pupils. For a group of schools founded by private groups of individuals between 1838 and 1897, Roi-Frey notes the most common occupations of its founders and its pupil's parents as 'civil servants', 'merchant', or 'industrialist'. (De la Roi-Frey, 2004, p. 222). For the school of Burghausen, the most common parental occupation of the 61 pupils in 1897/98 in Burghausen are distributed as follows: 12 landowners or managers, 10 owners of shops, 8 illegitimate children and 7 farmers. We take this as evidence that parent's education did not differ across schools and their occupation did not change significantly over time. Parents predominantly had a high-education background.

There are two motives for parents to send their female children to these schools. First, parents wanted to increase the marriage chances of their female offspring, as the traits learned would be valuable in the higher strata of society. "It is not fitting for an educated man to marry an uneducated woman, and consequently also not from a social sphere where none is to be expected" (Rousseau, 1762).[48] Alternatively, parents wanted to afford their female offspring similar opportunities that had been afforded to them. The dedication of some parents to fund their own schools could be interpreted in this direction. It is however impossible for us to differentiate these two motives for parents.

What is clear, however, is that average education plays a role in both motives. Either are educated men marrying 'educated' women, or are educated fathers sending their children to receive similar 'education' than them. Average education among men, interacted with educational opportunities for women, contributed to the rise of a female human capital elite that changed society.

The positive assortative matching between marriage partners is also documented on page 284-285 of De la Roi-Frey (2004). Here, she discusses the pupils' biographies of four selected schools translated and shortened in Table J.1. Three pieces of information stand out. First, the schools are founded by two queens, a priest (Höheres Töchterinstitut, Ellwangen), and the parents of the first seven children of the families Abel, Flatt, Schnurrer and Schott. Second, the occupations of the fathers and future husbands are of equal or better status, potentially indicating upward mobility of their female offspring. Third, when the biography of these children is known, most became mothers (15), but some also became writers, female rights activists and teachers.

Table J.1: Biographies of pupils

	Julie von May Institut, Tübingen 1798 (1)	*Höheres Töchterinstitut, Ellwangen 1838* (2)	*Königin-Katharina-Stift, Stuttgart 1856* (3)	*Königin-Olga-Stift, Stuttgart 1858* (4)
Number of pupils	7	17	20	19
Fathers' occupation	professor (3), civil servant (3), ephorus (1)	procurator, merchant (3), civil servant (7), priest (2), baron	doctor (4), colonel, priest, professor, director, merchant , civil servant (2), writer (2), singer, industrialist	writer, merchant, prime minister (3), publisher, lawyer (3), minister, principle, priest (3)
Husbands' occupation	prelate (2), merchant, civil servant, professor	minister of state, professor	engineer, merchant, priest, industrialist (2)	merchant, museum director, prelate
Occupation of pupil	unknown	mother (5), writer and female rights activist, canoness	mother (7), teacher at a finishing school, singer, pianist	mother (3), painter, governess

Authors' translation from De la Roi-Frey (2004) pages 284-286. The names of the schools are not translated to highlight that two of them were founded by queens.

[48] *"Für einen Mann von Bildung schickt es sich also nicht, eine Frau ohne Bildung zu heiraten, und folglich auch nicht aus einem Stande wo man keine erwarten darf."* Rousseau, 1762/1993, p. 447 as cited in (Jonach, 1997, p. 77).