Education and the Women's Rights Movement

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25th October 2021

Abstract

Does education facilitate the emergence of social activists, and thus, social movements? We study the impact of the opening of secondary schools for women (finishing schools) in Germany starting in the 1600s. We assemble a city-level panel of the political, intellectual, and economic elite throughout history, and find that finishing schools double the share of women among the human capital elite. In cities with finishing schools, women started to organize in women's rights associations, demanding suffrage and equal access to education. We find no evidence for differential returns to education affecting our results, as the staggered introduction of male schools only impacts men entering the human capital elite, but not women. Conversely, finishing schools only affect women, but not men. Several other city-specific indicators of economic and gender-specific cultural change are unrelated to the increasing representation of women among the human capital elite. Educational institutions thus function as catalysts for social movements, distinct from economic and cultural change.

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1 Introduction

What determines the emergence and success of social movements? Historically successful movements often passed three key milestones in their development (Wood and Tilly, 2012; Markoff, 2015; Della Porta and Mattoni, 2015): (i) a small number of dedicated activists develop critical ideas that challenge the status quo and begin forming networks. (ii) These activists then spread these ideas using available mass media and (iii) institutionalize their movement. From Dr. Martin Luther King Jr. to Susan B. Anthony, from Nelson Mandela to V. I. Lenin, such leaders are often considerably more educated than their peers. While their education is arguably crucial in a movement's emergence, the arrival of educational opportunities often coevolves with economic development and culture (Duflo, 2012; Goldin, 2006; Morris and Staggenborg, 2004). Thus, it remains unclear whether increasing educational attainment can bring about societal change by facilitating the emergence and success of social activists and movements.

In this paper, we isolate the role of education in the emergence of social movements by studying the women's rights movement in Germany, and its relation to the expansion of educational opportunities for women. By 1919, German women achieved suffrage following the growing influence of women's rights associations (Schraut, 2019). By 1909, these associations were present in more than 320 cities, with the women teachers' association alone organizing more than 23,000 female teachers. Much like women's rights movements in other countries at the time, early members utilized female-led newspapers (e.g. *Frauen-Zeitung*, 1849–1852) to expand public support for their cause beyond their own demographic of educated teachers, writers, and artists.

In many cases, these early leaders obtained their education at Germany's first institutions providing secondary education and teacher training to women: so-called finishing schools (*Höhere Töchterschulen*). Finishing schools only admitted women and were present in more than 170 cities by 1850. The first finishing schools in Germany were opened by foreign Catholic orders dedicated to female education: Ursuline nuns (Aachen, 1626) and the Congregation of Jesus (Munich, 1627). Despite focusing on religious teachings and manners, these nuns also critically engaged with the ecclesiastical and social discrimination against women and supported the educational and sociopolitical principles of the Enlightenment in the early 1800s (Conrad, 1996). Religious finishing schools complemented their curriculum with instructions in foreign languages and arithmetic. In short, they represented the only possibility for women to obtain secondary education or the necessary qualifications to work and live independently as teachers.[1]

Against this background, we leverage the timing of finishing school establishment as a positive shock to the availability of education for women. We highlight the role of education at three milestones in the history of the women's rights movement. First, in a panel of cities and notable individuals, we find that women started to represent a larger share of the political, intellectual, and economic elite ("human capital elite") after cities established finishing schools. Second, wo-

[1] As Albisetti (1988, p. xiv) hypothesizes, "*the formal and informal curricula of these schools, when compared to those of the classical Gymnasien attended by boys from the same social groups, could stimulate in young girls an early awareness of, and a protest against, their 'second-class citizenship' rather than a submissive conformity to the 'German ideal of womanhood'.*"

men from these cities also sent a disproportionate share of editorial letters to the first feminist newspaper in the mid-19th century. Third, cities with historical finishing schools had more, and larger, women's rights organizations by the beginning of the 20th century. We argue that finishing schools facilitated the exchange of critical ideas about women's role in society and the formation of networks; thus contributing to the rise of a female human capital elite from which the nucleus of the women's rights movement emerged. Crucially, these pioneering women disseminated critical ideas among a wider public and founded local chapters to convert their movement into a successful societal force.

For the first milestone, an increased representation of women among the human capital elite, we use variation in the availability of secondary education arising from the opening date and city of 255 finishing schools established between 1626–1850 (Neghabian et al., 2005) and a measure of human capital in every city and period from the *Neue Deutsche Biographie*. This biographical collection reports the places of birth and occupation for more than 150,000 individuals born between 800 AD and today. Its editors only included individuals in a high position of responsibility who impacted the general societal course. Thus, these data provide the most comprehensive historical account of Germany's political, intellectual, and economic elite.

In an event-study design with city and period fixed effects, we find that the share of women among the human capital elite rose from 1.8% prior to the opening of schools, to 4% within 50 years. Notably, the share of unmarried women also increased from 2.2% to 3.6%, indicating that finishing schools improved women's opportunities to live independently and be recognized for their achievements.

Cities that establish finishing schools may differ on a wide range of characteristics. 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 cities would exhibit different trends prior to school establishment. However, we find no evidence for differential pre-trends in women entering the human capital elite. Our findings are robust to including city and period fixed effects, linear time trends, and flexibly controlling for a rich set of predetermined educational, economic, and religious covariates separately in each period.

We argue that the opening of finishing schools is the result of idiosyncratic supply-side decisions by Catholic orders or rulers, and not driven by local demand for education. We capture local demand for educational facilities in three ways: First, population data from Bairoch et al. (1988) exhibits no differential growth in population prior or post school construction in cities. In addition, we standardize our main variable by the total number of notable individuals born, thus controlling for the size of the elite in every city and period. We also use women from the nobility, a demographic educated by private tutors, as a placebo to capture potentially different population growth rates. We find no evidence that population trends confound our estimates. Second, finishing schools also increase the likelihood of observing notable women in periods that precede the German Industrial Revolution (second half of the 19th century), speaking against economic demand for education. Finally, to test whether demand for education driven by cultural change

preceeded or coincided with finishing school establishment, we use a compendium of Enlightenment journals and articles (1688-1815) featuring information on 317 journals and 260,000 articles (Akademie der Wissenschaften, 2018). Finishing schools are not correlated with the number of journals or the number of articles on women in periods prior to their establishment; nor does the inclusion of these variables change our point estimates. These findings are inconsistent with the notion that cultural changes drove local demand for education. Finishing schools are more likely the result of supply-side factors, idiosyncratic choices that led Catholic orders or rulers to establish a finishing school in a city.

If cities establish finishing schools in response to changes in (local) attitudes towards women, we would wrongly attribute the effect of social change to the expansion of education. Thus, 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.

Finally, we show that our results are robust to applying different weighting techniques (Callaway and Sant'Anna, 2021; de Chaisemartin and D'Haultfœuille, 2020) and the procedures outlined in Baker et al. (2021). 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 then additionally instrument our treatment group using monasteries constructed before 1300 coupled with religious competition near the religious divide. Throughout all specifications, we find no differential pre-trends, but a significant increase in women entering the human capital elite after the first finishing school was constructed. These findings carry over when analyzing every treatment period separately: even finishing schools established in the 19th century, when women were already more common among the human capital elite, significantly increase women's representation among the human capital elite.

The increased representation of women among the human capital elite is concentrated in key demographics at the core of Germany's women's rights movement: The share of female teachers and writers increased from 1.9 to 3.6% after the opening of a finishing school, relative to men in the same category. Further, using their biographies to identify activists fighting for equal rights and women's suffrage, we show that the likelihood of an activist being born in a city increased

from 1.6% to 6.9%.

This activist nucleus started to form networks early on. We find that after the opening of finishing schools, the probability that a notable woman is mentioned in another woman's biography from the same city increased threefold.[2] To show that these networks increased human capital representation, we identify 500 women who migrated during their lifetime. While cities do not differentially attract women before the establishment of finishing schools, women start migrating to cities in which a native notable woman has already established a network.[3]

The second historical milestone of the German women's rights movement, spreading their ideas using available mass media, began after the leaders of successful social movements had developed critical ideas and formed an early network. We document this historical milestone by linking the presence of finishing schools to letters to the editor of the first feminist newspaper (*Frauen-Zeitung*, 1849–1852) in a cross-sectional analysis. Compared to cities without finishing schools, cities with finishing schools 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.

The third historical milestone of the women's rights movement we study is its institutionalization. Local chapters of the German women's rights movement sprung up from 1848, with the first organization specifically targeting female education being founded in the 1880s. Yet by 1909, only 37% of cities without finishing schools established a women's rights organization, compared to 78% of cities with finishing schools in 1850. This difference is even more pronounced for educational organizations, at 5% and 29% respectively; these organizations have an order of magnitude more members when located in a city with finishing schools.

In these cross-sectional results, unobserved differences between cities, previously captured by fixed effects, might reemerge and bias our estimates. We thus always control for economic, religious, and educational covariates to mitigate the threat from differential attitudes towards women. In addition, bias-adjusted point estimates (Oster, 2019), estimates from an instrumental variables strategy (using monasteries in 1300 coupled with religious competition as an instrument), as well as estimates from a propensity score matching show a robust and stable impact of finishing schools on all cross-sectional outcomes.

In sum, 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 emergence of a group of leading activists. Using newspapers to disseminate critical ideas and founding local chapters to institutionalize their movement, these leading activists turned an initially upper-class movement into a broad societal force. In the first democratically elected parliament of the

[2]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.

[3]These migrating women are a subset that - in our main results - are assigned to their cities of birth. We only assign them to their city of death to identify whether finishing schools were a pull factor in their migration decision. Our results are not the result of a violation of SUTVA, and are robust to excluding these women, excluding neighboring cities, and choosing a larger unit of observation (Appendix E).

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.

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; Goldin, 1990, 2006; Nekoei and Sinn, 2021). First, by disentangling the availability of secondary education from other cultural and societal changes, we show that education was a key driver behind the women's rights movement and the increasing status of women in society. Second, and at a more general level, our results indicate that the positive effects of education are not limited to students themselves. In our case, women from various backgrounds benefited from extending education to an initially limited number of women. In that way, our paper also informs a large development literature studying the effects of interventions targeted at reducing gender inequality in education (Chattopadhyay and Duflo, 2004; Beaman et al., 2009). By providing evidence on the effects of secondary education for women from the historical case of Germany, our paper highlights the potential long-run benefits of such interventions for society at large.

This paper also complements a recent literature in economics which has highlighted the importance of civic leadership (Dippel and Heblich, 2021) and technology (Zhuravskaya et al., 2020; Garcia-Jimeno et al., 2020; Melander, 2020) in promoting the success of existing social movements. 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 the women's rights movement from before its very beginning until it reached key milestones, such as 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 (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, 2019; 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.

Finally, we contribute to an open debate on the role of Germany's bourgeois women's rights movement (Schraut, 2019). Evans (1980) ascribes improvements in women's empowerment largely

to the working-class women's movement associated with the rise of the Social Democratic Party of Germany (SPD). He argues that Germany's bourgeois women's rights movement neither played a relevant part in the struggle for suffrage nor in improving women's educational opportunities. Wolff (2018) argues strongly against this view and emphasizes the importance of the "association and print media structures built since the 1860s" in carrying the demand for women's suffrage into society at large. Our results on finishing schools support the view that the bourgeois women's movement played an important part in improving women's role in society, two hundred years before the formation of the first political parties.[4]

The paper is structured as follows: In Section 2, we discuss the historical link between finishing schools and the women's rights movement. 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 discuss the long-term results on the dissemination of critical ideas, the organization of the women's rights movement, and modern-day representation in parliaments in Section 8.

2 Historical Background

We begin by illustrating the links between the women's rights movement in the late 19th century 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 in feminist newspapers and founded the women's rights movement. 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 Reforma-

[4] Albisetti (1982) disagrees with Evans' assertion regarding the relative importance of the working class movement, arguing for the bourgeois associations' efficacy in gradually persuading government officials to improve women's educational opportunities. While the socialist movement, and in particular the SPD undoubtedly played an important role, quantitatively assessing the relative importance of either movement lies beyond the scope of this paper.

tion, 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). In response, 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). By and large, city governments and Prussian rulers only became active in the field of female secondary education in the 19th century.[5]

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.

2.2 The German women's rights movement

Starting in 1848, early women's rights activists around Louise Otto-Peters publicly demanded equal access to education, equal occupational opportunities and the right to vote (Berndt, 2019; Gerhard, 1990; Nagelschmidt and Ludwig, 1996). Similar in spirit to the agenda of contemporary women's rights movements in the US or Great Britain, they particularly emphasized the necessity of obtaining equal access to education as a key enabling factor for securing the other two central demands, the right to vote and equal occupational opportunities (Schötz, 2019).

Initially, only women from the upper class formed the nucleus of the German women's rights movement. To gain broader support and turn the movement into a societal force, early women's rights activists pursued two complementary strategies: the dissemination of critical ideas about women's role in society and an institutionalization of the movement (Berndt, 2019; Gerhard, 1990; Nagelschmidt and Ludwig, 1996). First, the movement started to publish a newspaper in 1849, "*Frauen-Zeitung*", to disseminate critical ideas about the role of women in society among interested women and the general public alike; "*Frauen-Zeitung*" remained the main relay of the Ger-

[5]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.

man women's rights movement until World War I (Schötz, 2019).[6] Second, to coordinate its members, the movement started to establish associations with an increasing number of local chapters throughout Germany.

The first of these women's rights associations, "*Allgemeiner Deutscher Frauenverein*" (German Association of Female Citizens), was founded in Leipzig in 1865 and soon organized more than 20,000 women in 48 local chapters (Kaiserliches Statistisches Amt, 1909). An important part of the local chapters' activity was to file petitions to (state) governments: they demanded the equality of women and men in the civil code (1876), the admission of women to universities (1876), and the improvement of the quality of teacher training for women (1887) (Schraut, 2019). Reflecting the central importance of teachers, the "*Allgemeiner Deutscher Lehrerinnenverein*" (German Association of Female Teachers), founded in 1890 to advocate for equal access to education for women and adequate training for female teachers, quickly grew to a membership of more than 23,000 teachers spread across 108 local chapters by 1909.

In total, more than one million women joined women's rights associations by 1909 (Kaiserliches Statistisches Amt, 1909, p. 17); many also joining political parties when the ban on female entry was lifted in 1908 (Evans, 1980). 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 actively fought for women's rights in one of more than 1,200 women's rights associations in Germany.

2.3 Finishing schools and the women's rights movement

Several accounts by historians and the biographies of leading women's rights activists, such as the teacher Helene Lange, indicate the importance of finishing schools for the emergence of the women's rights movement in Germany (Albisetti, 1988; Ringer, 1987; Schaser, 2000; Schötz, 2019). Based on these accounts, we discuss two mechanisms that link the establishment of finishing schools to the emergence of the women's rights movement: access to critical ideas about women's role in society, and reduced cost to form and access networks of like-minded peers. In this way, finishing schools provided the "foundations upon which the whole breadth and force of the women's movement were to depend" (Strachey, 1928, p. 124, as quoted in Albisetti, 1988, p. xiii).

First, despite their general focus on religious piety, Ursuline nuns and Mary Ward sisters also critically engaged with the ecclesiastical and social discrimination against women and demanded the "spiritual" recognition of the equality of the sexes. They also actively supported the educational and socio-political principles of the Enlightenment in the early 19th century and amended their religious teachings with secular subjects such as arithmetic and foreign languages.[7] Knowledge of English and French allowed women to access the critical writings of early feminist

[6] "*Frauen-Zeitung*" (translated: Women's Newspaper) was renamed "*Neue Bahnen*" (translated: New Ways) after it was banned by the Prussian government. However, the editorial staff and the ideological orientation remained.

[7] Authors' translation, adapted from Conrad (1996) p. 256 and p. 262. We test whether the Enlightenment period confounds our interpretation in Online Appendix D.

thinkers (e.g. Olympe de Gouge), which influenced the formation of the women's rights movement in Germany (Hauch, 2019). Their ideas likely stimulated a critical questioning of women's role in society among the young women and teachers at finishing schools, especially when contrasting their opportunities with those afforded to their male counterparts (Albisetti, 1988).

Second, finishing schools reduced the costs to form and access networks of like-minded women. In contrast to life outside schools, students at finishing schools lived together without the supervision of their families, being taught by female teachers who pursued an independent lifestyle unthinkable outside the teaching profession. This provided young women at a formative stage in life with access to a network of students and teachers which could strengthen opposition to their status as second-class citizens (Albisetti, 1988; Ringer, 1987). Finishing schools thus facilitated the exchange of ideas between teachers and fueled the rapid spread of local women's rights associations across Germany, as illustrated by the more than 23,000 teachers active in the "*Allgemeiner Deutscher Lehrerinnenverein*" (German Association of Female Teachers) in 1909.

More than any other profession, female teachers at finishing schools shaped the direction and force of the women's rights movement in Germany by influencing the lives of generations of women. This does not stand in contrast to the achievements of the working-class women's movement (Evans 1980), but complements the views of (Albisetti, 1988, p. 249f, 303) and (Wolff, 2018, p. 19) who emphasize the importance of the "association and print media structures built since the 1860s" in carrying the demand for women's suffrage into society at large.[8]

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 and institutionalized their movement, 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.

[8]Our findings are consistent with the idea that both the bourgeois and the working-class women's movement made important contributions to improving women's opportunities in general and suffrage. Both, female leaders of the SPD such as Clara Zetkin and leaders of the 'radical wing' of the bourgeois women's movement such as Anita Augspurg, Minna Cauer, Lida Gustava Heymann, Gertrud Bäumner, either studied, received teacher training or taught at a finishing school at one point in their life.

Biographies of Notable Women We obtain detailed microdata data on the universe of notable German women and men for the period 800 to 1950 CE from the "*Neue Deutsche Biographie*" (NDB) 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).[9] 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. We link 2,363 non-noble secular women to cities of birth within in the modern-day boundaries of Germany after 800 CE, as well as 261 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 2,624 women, 32% became notable for being an artist, 21% for being a writer, 10% for being born into nobility, and 6% each for being an academic or a politician (Table 1). 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 woman was born in a given city and period who became notable later in life, (ii) the log number of notable women, (iii) and the share of notable women among all notable 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 traditional female finishing schools constructed between 1626–1850 and their location as shown in Figure 2 (Neghabian et al., 2005).[10] 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 orders of the Catholic church who, following an invitation by

[9] "Those personalities are to be included whose deeds and works reflect the development of German history in science, art, trade, and commerce; in short in every branch of political, intellectual and economic life." (Bayerische Akademie der Wissenschaften. Historische Kommission, 1953, p. VII-VIII). 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).

[10] 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. Since such schools are more likely to appear in the later years of our dataset, we divide the data into 'Early Schools' prior to 1750, and 'Late Schools' post 1750 in Table C.5. We find no differential impact, and thus no evidence for a bias arising from the omission of these temporary existing schools.

the ruling houses, settled near existing monasteries to educate and "protect the women's mind from the falsities of their time".[11] 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 (Figure 2).[12]

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.[13] 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.[14] 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 secondary education in promoting the emergence of a female human capital elite which later formed the nucleus of the German women's rights movement. Our data allow us to descriptively assess at 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 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 ("40-20").

[11] "*...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.

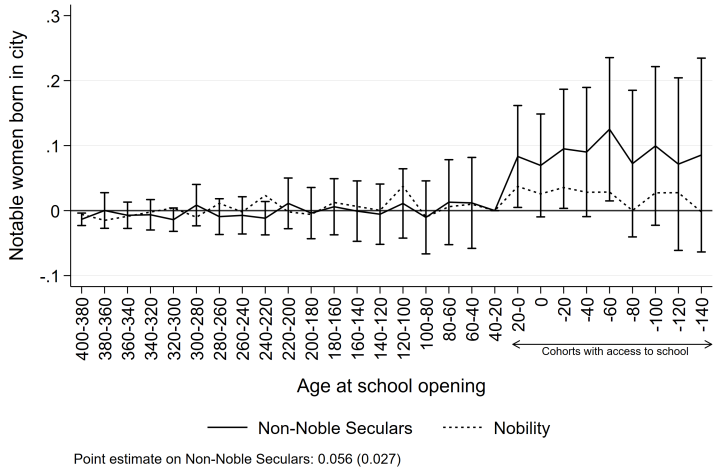
[12] Some later schools might have been a response to local demands of the population. We report the same results for when using schools constructed in the period 1650–1750 or 1750–1850 in the Table C.5. We also report no differential pre-trends and similar sized point estimates for every treatment period in Figure G.2 and 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.

[13] The 'extended sample' of Voigtländer and Voth (2012) include 1,428 'towns and cities', 739 of which were mentioned before 1300. Many of these 'towns and cities' are close to a major city. For example, Voigtländer and Voth (2012) link three 'towns and cities' to Aachen: AACHEN L, town_id 1,3,4, mentioned in 930, 1118, and 870 CE who are close to the original city of Aachen (AACHEN S, town_id 5, mentioned in 400 CE). We use the latter as our reference city if it lies in 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 3).

[14] 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 'y' was in territory 'x'. As our results remain qualitatively unchanged, we argue that sample selection does not introduce a bias in our setting.

A ten year old women in the same year however ("20-0"), had the chance to attend a finishing school and is thus 10 percentage points more likely to appear in our data, than her thirty-year old counter part.

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 (20-0), and thus has the chance of attending the school. A women that is born 60 years after the construction of the finishing school is coded as '-60'. The outcome is an indicator equal to one if a notable woman was born in a given city and period 95%-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.

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 the framework constant and conduct all our exercises in. 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.

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.

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.

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

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

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

[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 differential pre-trends or heterogeneous treatment effects across treatment periods (Figure G.2 and Table G.2).

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). Combined, 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

[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 the religious competition, pushed for female education to win over the minds of women.

across all genders at the time the first finishing schools were established in Germany.[18]

We interact all covariates with period fixed effects to isolate the effects of finishing schools from these confounding factors.[19] 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 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 3.[20] 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. In Figure 3, we provide evidence in favor of our identification assumption as finishing schools have a precisely estimated zero impact in all periods prior to opening. We estimate the impact of finishing schools on two subgroups of women: non-noble secular women (solid line) and the nobility (dashed line). We use women from the nobility as a placebo group and separate them from the remaining notable women, since they likely had access to private tutoring and thus should not be affected by the opening of finishing schools.[21] If the establishment of finishing schools is correlated with an unobserved change in the overall likelihood of being recorded as notable (e.g. population growth or local political change), the point estimate on nobility would be significant in post periods. However, while we find no impact of finishing schools on women from the nobility, the probability of a non-noble secular woman being born in the city and becoming notable later in life increases immediately after the first school opened. This relationship remains robust when including all control variables non-linearly in the right Panel of Figure 3a.

In the remaining Panels of Figure 3, we document the absence of pre-trends when using the number of women born (Figure 3b) and the share of women among all notable individuals born in the same city and period (Figure 3c). We observe a significant treatment effect in the first period after opening that is slightly increasing in the rights panels when controlling for covariates.

[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 3 captures this variation and the point estimates are not statistically different from our baseline.

[19] We explore heterogeneity along all covariates and find no heterogeneous impact or effect on our main coefficient.

[20] 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.

[21] We separate this group not to discredit the efforts and successes of many noble women advocating women's rights, but merely to reflect historical differences in the provision of secondary education.

If this slight increase is driven by cohort-specific treatment effects, our Two-Way-Fixed-Effects estimator (TWFE) might produce biased 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 3). Second, we provide an assessment of pre-trends by treatment cohort (Figure G.2) and 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 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 Finishing schools and 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. Using detailed occupational and biographical data, we provide additional evidence that finishing schools contributed to women entering the human capital elite as teachers and activists. These women later formed the core demographic of the women's rights movement, spreading their ideas in the *Frauen-Zeitung*, and organize in women's rights organizations throughout the country.

We present our main results in Table 2, 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) and (2), 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 23-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 interact economic, religious, and educational covariates with period fixed effects in column (2). The point estimate of 0.164 (16%, s.e. 0.033) suggests a stable 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.[22]

In the remaining columns (3)–(6) 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 20%, even when extensively controlling for economic, religious and educational factors.[23]

Population in 1650 interacted with period fixed effects might not adequately capture the heterogeneous growth paths of German cities.[24] 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 (5) and (6), we thus divide the number of notable women born by the total number of notable men and women in the same category and period. 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.[25] Relative to cities without finishing schools in which 1.8% of all notable individuals are women, the share of women among the human capital elite increased to 4% after the establishment of finishing schools.[26] 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.

[22]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.4.

[23]Using the logarithm of a variable with a large amount of zeros is problematic as the $\log(y+1)$ transformation might introduce a bias. We are aware of this and thus refer to columns (1) and (2) as our preferred specification and report all Figures using the binary definition (columns 1 and 2) as the outcome variable.

[24]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.

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

[26]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.

Similar to other countries (Goldin, 2006), the majority of notable women were unmarried and independent. The share of unmarried women, relative to all unmarried men and women, increases from 2.2% to 3.6% after the opening of finishing schools (Table 2, Panel B, Column 6). While it is possible that measurement error in the data biases this point estimate, the measurement error would have to be correlated to finishing school opening to bias the point estimate upwards. Our results thus support the notion that finishing schools facilitated the emergence of a greater number of women pursuing a more independent lifestyle, free from the constraints of marriage in a patriarchal society.

In the remaining Panels (C)–(E) of Table 2 we explore the effects of finishing schools on different subcategories of notable women based on their professions and the placebo group, women from the nobility. First, we confirm historical accounts arguing that many students went on to become teachers and writers by showing that the likelihood of a female teacher or writer being born and recorded in our data is substantially higher after the opening of a finishing school. Second, we analyze the biographies of all notable women and use keywords to identify women's rights activism.[27] While we record markedly fewer women than in other categories, the relationship is robust and stable in all specifications and suggests a threefold increase in the likelihood of observing an activist after the opening of a finishing school (Panel D, column (2)).

Finally, we estimate the impact on the subgroup of noble women in Panel E. Again, we treat the nobility as a placebo group since the likelihood of being recorded in the *Neue Deutsche Biographie* should not benefit from the establishment of a finishing school. This subgroup captures overall trends in population growth which should equally affect all notable individuals of either category. In line with our argument that the relationship between finishing schools and women's representation among the human capital elite is not mechanically driven by population growth, we find robustly estimated insignificant null effects of finishing schools on the nobility throughout all specifications.[28]

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) or alternative lengths of periods (Table C.2). The estimated effect does not vary greatly by occupation (Table C.3) or the timing of school opening (Table C.5). In Appendix D, we assess the role of demand-side factors and find no impact of finishing schools on population

[27] The top five keywords are (in order): "Frauenrecht" (Women's rights), "Frauenbewegung" (Women's movement), "Frauenverein" (Women's clubs), "Emanzipation" (emancipation), and "Feministin" (feminist). The share of women is constructed using the number of male politicians as a proxy for the politically active male population.

[28] Controlling for construction activity does not impact our results (Table B.3) and is not predicted by school establishment (Figure 5a).

growth or a correlation with the arrival of the Enlightenment (18th century) and the Industrial Revolution (19th century). We dedicate Appendix 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 all outcomes except activists (p-value: 0.020). 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) or when separately identifying pre-trends by school opening period (Figure G.2). We find no effect of the arrival of the Protestant Reformation in cities (Figure G.3), 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.4). 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 Placebo exercises

To rightfully attribute the increase in women's representation among the human capital elite to the emergence of finishing schools, we discuss whether changes in the returns to education, culture, or economic activity predict a similar increase. To identify such potential confouding factors, we exploit the following city- and time-specific placebo events: In Section 6.1, we use the opening of secondary schools for men to capture an increase in the overall returns to education. 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.[29] 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.

[29] 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, unmarried men, and male teachers and writers, with the opening of male schools. Following Galor and Weil (1996), we interpret schools for men as an endogenous response to increased returns to education following an increased demand for skilled labor. As such, the estimated effect of male schools on the occurrence of notable men is a combination of (i) increased returns to education and (ii) education itself. By the same token, if female finishing schools were also a result of increased returns to education common to both genders, we would expect to see an increase in the number of notable men in response to the establishment of finishing schools.[30]

In Panel A of Table 3, we limit our sample to 129 cities that ever constructed a finishing school, in a window of four centuries before and two after establishing the first school. In columns (1), (4), and (7) we estimate the impact of finishing schools on notable women, unmarried women and teachers and writers. Despite the reduction in sample size and the omission of educational covariates, the estimated coefficients in this event-study design are close to those of the fixed-effects estimation reported in Table 2. Finishing schools do, however, have no impact on the likelihood of observing notable men in our data (columns (2), (5), and (8)). In columns (3), (6), and (9), we 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 the second Panel of Table 3, we turn to the impact of male schools on notable women and men. The opening of a male school in a city increases the likelihood of observing a notable man (Columns (2), (5) and (8)), but the impact on women in the same city is a precisely estimated zero (Columns (1), (4), and (7)). 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 4, 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.

[30] In support of this argument we find that in cities that had both finishing and male schools, the male school was always constructed before the finishing school.

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 4, nor in any period around the opening of finishing schools (Figure 5a).

In Figure 5b, 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.

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 5 and Figure 6).

In Panel A of Table 5, 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).[31] We thus argue that the '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 5, 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, 2021) and household roles. The establishment of such monasteries could thus be considered reflective

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

of local culture becoming more accepting towards women choosing a comparatively independent lifestyle.[32] 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 5. 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.[33] 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 5, 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.[34] 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, teachers, or any subcategory.[35]

We summarize the impact of cultural changes in the event-study graphs of Figure 6: 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 idiosyncratic shocks. Thus, finishing schools, conditional on fixed effects, can be interpreted as an exogenous shift in the supply of education for women.

7 Mechanism

The historical literature on finishing schools (Albisetti, 1988) and the women's rights movement (Schraut, 2019) suggest two complementary mechanisms that link the establishment of finishing schools to an emerging nucleus of the women's rights movement: access to critical ideas about women's role in society, and reduced cost of forming and accessing networks of like-minded peers. We interpret our results thus far as critical ideas about women's role in society taking hold in cities with finishing schools, as more unmarried women entered the human capital elite as teachers,

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

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

[34] 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.

[35] 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 teachers, 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.

writers, and women's rights activists. In this Section, we shed light on the second mechanism: finishing schools reducing the cost of forming and accessing networks of like-minded women. We document that the establishment of finishing schools positively impacted the emergence and size of networks between notable women and increased the immigration of notable women, further contributing to network formation.

7.1 Networks between notable women

We construct our measure of networks between women by analyzing 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, and the younger was at least 16 years old when the older women died. 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 6, 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 (Panel A). The estimated effect, however, predictably varies by the type of network constructed: In stark contrast to networks between non-noble secular (Panel B) or politically active women (Panel C), connections between religious or noble networks are unaffected by establishing a finishing school (Panel D). The results on networks between notable women echo our main results: finishing schools increase networks only for politically active women, but not for the placebo group of the nobility.

7.2 Immigration of notable women

We provide further evidence on the formation of networks using the immigration of notable women. 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 (left Panel); a finding robust to including control variables (right Panel).

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

[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.

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 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 increases immigration. If, however, 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 the number of notable women born in a city.

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 future notable women being born. Using the first migration event as the "treatment period" in the left Panel, we report no impact on future non-noble secular women being born. In contrast, the right hand side of Figure 8 reveals that the first native-born notable woman induces a strong increase in immigration of other notable women from elsewhere.

Our results thus indicate that finishing schools increased women's representation among the human capital elite: women became teachers, writers and early activists, indicating that critical ideas about women's role in society took hold in cities with finishing schools. These women would eventually form networks with other women from the human capital elite and attracted other like-minded women from other cities. These early networks laid the foundation for the further dissemination of critical ideas and the institutionalization of the women's rights movement.

8 Finishing schools and the women's rights movement

When Dr. Martin Luther King Jr. and Susan B. Anthony spread their ideas and institutionalized their movement, they provided the social acceptance required for the civil rights and suffrage movements to succeed. German activists from the early phase of the women's rights movement pursued similar strategies to gain broader public appeal and turn their movement into a societal force (Berndt, 2019; Nagelschmidt and Ludwig, 1996; Schraut, 2019). We measure the dissemination of critical ideas by digitizing all letters to the editor of the feminist newspaper "*Frauen-Zeitung*", in which women's role in society was critically discussed. To capture the increasing institutionalization, we use establishment and membership data of local chapters of the women's rights movement in 1909. Lastly, we provide evidence that finishing schools, via accumulating human capital, disseminating critical ideas, and institutionalizing the movement, increased female representation in parliaments once suffrage was achieved.

8.1 Empirical approach

We document the link between finishing schools and the success of the women's rights movement in a cross-sectional setting. Specifically, we show that cities c with historical exposure to finishing schools send more letters to the *Frauen-Zeitung* and have more local chapters of the women's rights movement in 1909. In doing so, we estimate cross-sectional regression using specifications of the following type:

$$Y_c = \alpha + \beta \cdot \text{finishing schools}_c + \gamma \boldsymbol{X}_c + \varepsilon_c \tag{1}$$

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: 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, in Appendix I 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. Third, we compare the effect of finishing schools using propensity score matching on all covariates in Appendix I.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 examining 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 A), and the institutionalization of the women's rights movement by founding local chapters and recruiting female members (Panels B and C). We then turn to an important outcome of the women's rights movement, female representation in parliaments after women achieved the right to

both vote and stand for parliament in 1919 (Panels D and E).

8.2 Dissemination of ideas

To measure dissemination of critical ideas, we digitize all letters to the editor of the first feminist newspaper in Germany, "*Frauen-Zeitung*" (1849-52), in Panel A. 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 (1), we estimate a bivariate regression without controls and restrictions, documenting an increase in the likelihood of sending a letter of 0.100 (s.e. 0.017), an 150% increase over the mean. Only 6.2% of cities without finishing schools by 1650 sent letters to the "*Frauen-Zeitung*", compared to 16.2% of cities with finishing schools. We interpret this increase as evidence that critical ideas are more common in cities with finishing schools.

To assess the potential severity of selection on unobservables, we report the bias-adjusted point estimate from a restricted estimation in column (2). Here, we include all previously defined controls and limit the sample to areas that, 200 years prior to the foundation of the "*Frauen-Zeitung*", had been religiously competitive. We estimate a similar point estimate of 0.122 (s.e. 0.037), a fourfold increase over the likelihood of sending a letter in cities without a finishing schools (0.038 in this sample). The bias-adjusted point estimate is of a similar magnitude than the baseline (0.132). In columns (3) and (4) of Table 7, we repeat this exercise with the number of letters sent. Again, the bias-adjusted point estimate confirms the OLS point estimate and suggests a 24% increase in the number of letters sent to the "*Frauen-Zeitung*".[37]

8.3 Organization of the movement

Next, we turn to studying the institutionalization of the German women's rights movement. To measure the institutionalization of networks in the second half of the 19th and the early 20th century, 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. The average local chapter in our dataset 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 Panels B and 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 B), equivalent to a 50 percent increase over the mean in cities without finishing schools. Especially associations dedicated to promoting equal access to education for women exhibited stronger public support: if cities had established finishing schools by 1850, the number of members in these organizations

[37] 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 only record 242 letters from 40 cities, with five cities sending over half the letters.

exceeded that in cities without schools by 50% (Panel C, column 4).[38]

8.4 Female representation in parliament

Our results suggest that critical ideas took hold in cities with finishing schools, leading to more members in women's rights organizations than in cities without finishing schools. First, the increasing representation of women among the human capital elite (Table 2) contributed to the creation of networks between cities that attracted other notable women (Figure 8). Second, these women were up to three times more likely to disseminate their critical ideas using the first female led newspaper, the "*Frauen-Zeitung*", as an outlet (Table 7, Panel A). Finally, they organized into women's rights groups (Table 7, Panel B) and jointly lobbied for the core demands of the women's rights movement: Equal access to education and female suffrage.

Thus, by educating young women and teachers, finishing schools contributed to the formation of a human capital elite that ultimately succeeded in achieving suffrage in 1919. Once 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 Panels D and E 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) and the Federal Republic of Germany (1949-2019, Panel E).[39] 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.[40]

While during the Weimar Republic, only 4% of cities without finishing schools sent women to parliament, this figure rose to 53% in the Federal Republic of Germany (Panel D, column 2). In contrast, cities with historical finishing schools were 10 percentage points more likely to have sent women to parliament, equivalent to a 250% increase during the Weimar Republic and a 25% increase during the Federal Republic. Panel D and E thus highlight cities' historical advantage as 'early movers' towards a more gender equal society, gained by the establishment of finishing schools more than 300 years earlier.[41]

[38] In Appendix J, we directly correlate the number of non-noble secular women in 1850 with political activity at the turn of the century: a 10% increase in the number of notable women increases political activity by 15%.

[39] 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.

[40] 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.

[41] We explore such a "early movers" hypothesis in more detail in Appendix Table I.2. Here, a city with 50 more years of exposure to finishing schools would imply 14% more letters, twice the number of women's rights organizations and 23% more women in parliament today.

9 Conclusion

We set out to determine conditions for the emergence and success of social movements 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 identify three key milestones. First, future leaders are educated and develop critical ideas. Second, these leaders disseminate their ideas using available mass media. Third, leaders institutionalize their movement as their ideas take root in society.

We study the importance of one form of educational institutions at these three milestones, 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 at every step of their development. 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 the women's rights movement. Second, women born in such cities also sent a disproportionate share of editorial letters to female-led newspapers, important platforms for early women's rights activism. Third, cities with historical finishing schools hosted more and larger women's rights organizations, key forces in the advancement of women's empowerment.

Using a wide range of empirical specifications our paper highlights the role of education in contributing to the emergence and success of the German women's rights movement. 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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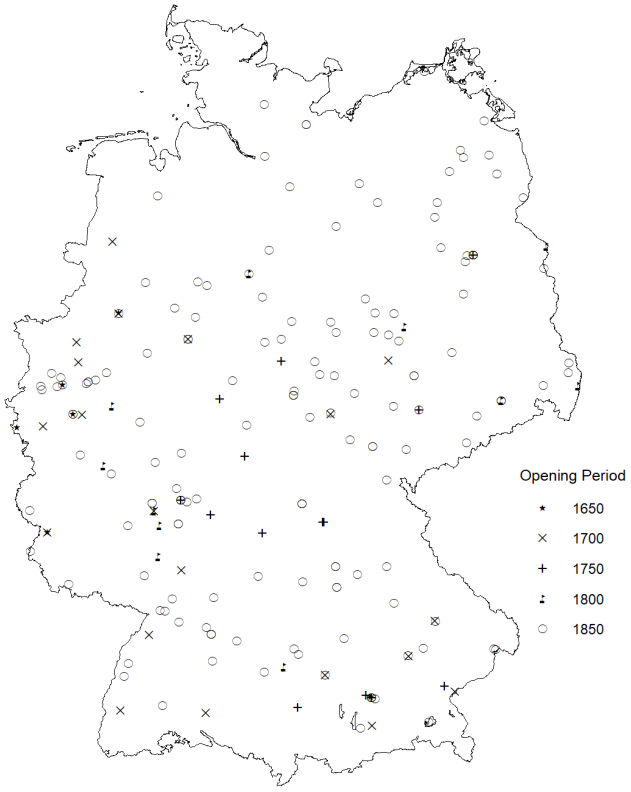
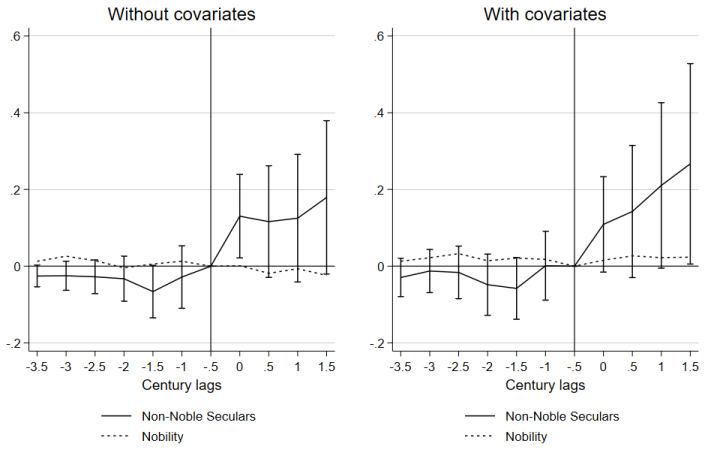
Figures and Tables

Figure 2: School opening years in Germany

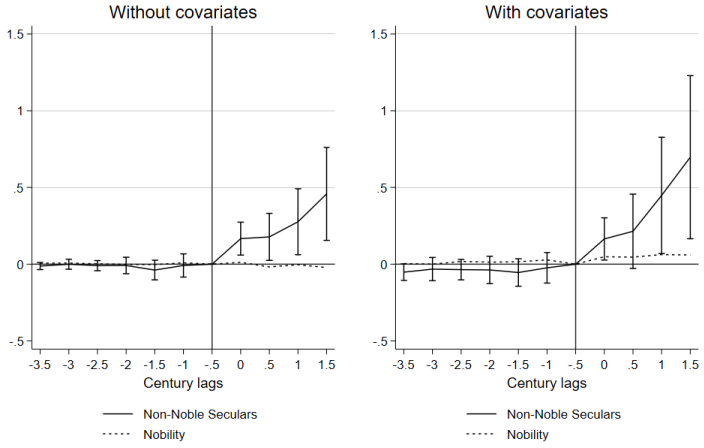


This figure shows the location of finishing schools by their opening period. In Figure C.1, we additionally show the variation, including cities and the number of notable women. We depict finishing schools by opening period and religious denomination in Figure K.1.

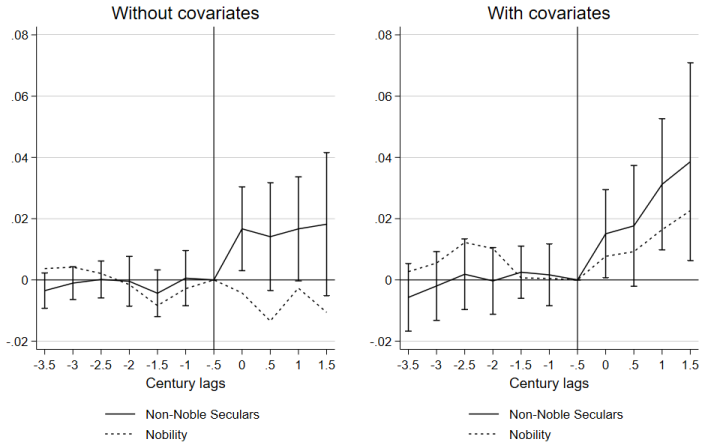
Figure 3: Event-Study: Impact of finishing school establishment on notable women



(a) Indicator function: Notable woman born in city



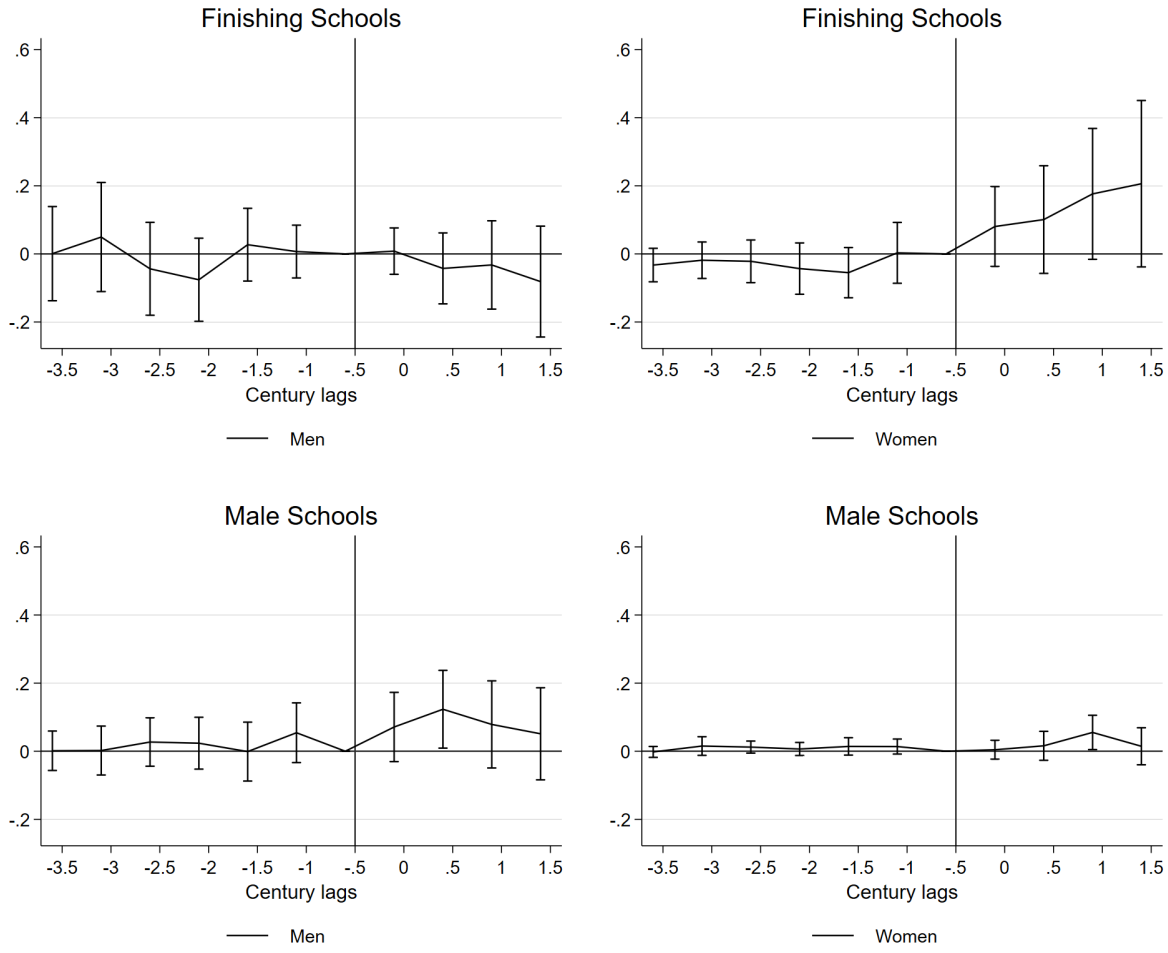
(b) Logarithm of the number of notable women born in city



(c) Share of notable women born in city

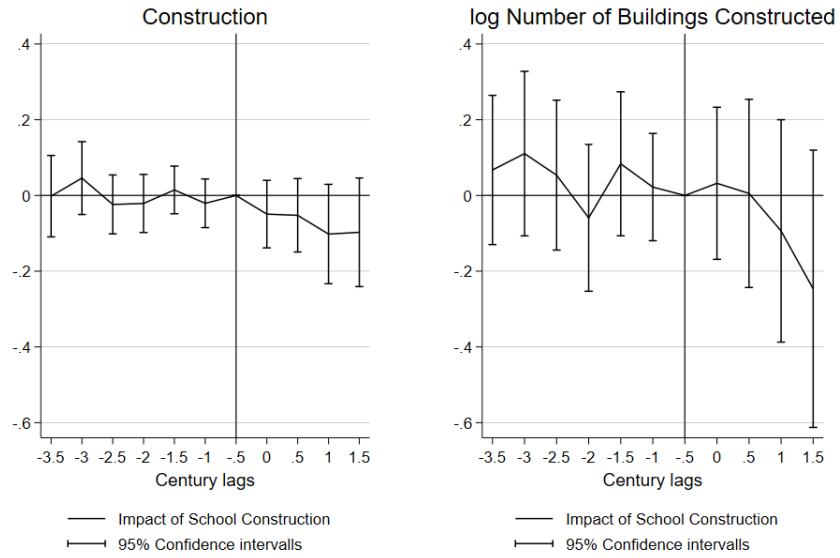
Event study results for *non-noble secular* women and women from the *nobility*. In Figure a, the outcome is an indicator equal to one if a notable woman from the respective group was born in a given city and period. Figure b uses the natural logarithm of number of women born plus one. Figure c denotes 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 the left figure and full economic, religious, and educational controls added in the right. 95%-confidence intervals shown only for non-noble secular, the impact of nobility is indistinguishable form zero in all periods and specifications. Alternative approaches are discussed in Section F.

Figure 4: Cross-gender impact of male and female schools

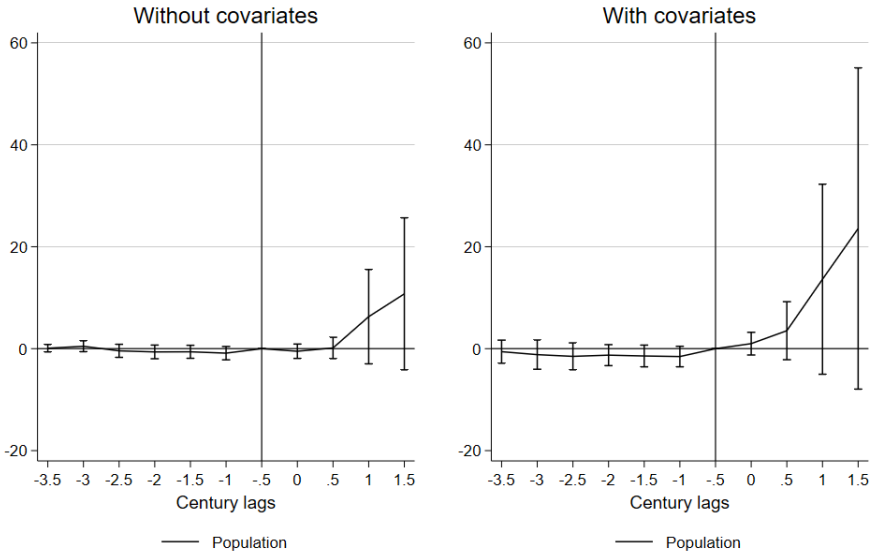


The impacts of male schools and finishing schools on notable women and notable men. The outcome in the two Panels on the left (right) is an indicator equal to one if a notable man (woman) was born in a given city and period. Zero is the normalized time of opening of the first gender-specific schools 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 5: Impact of finishing schools on economic growth



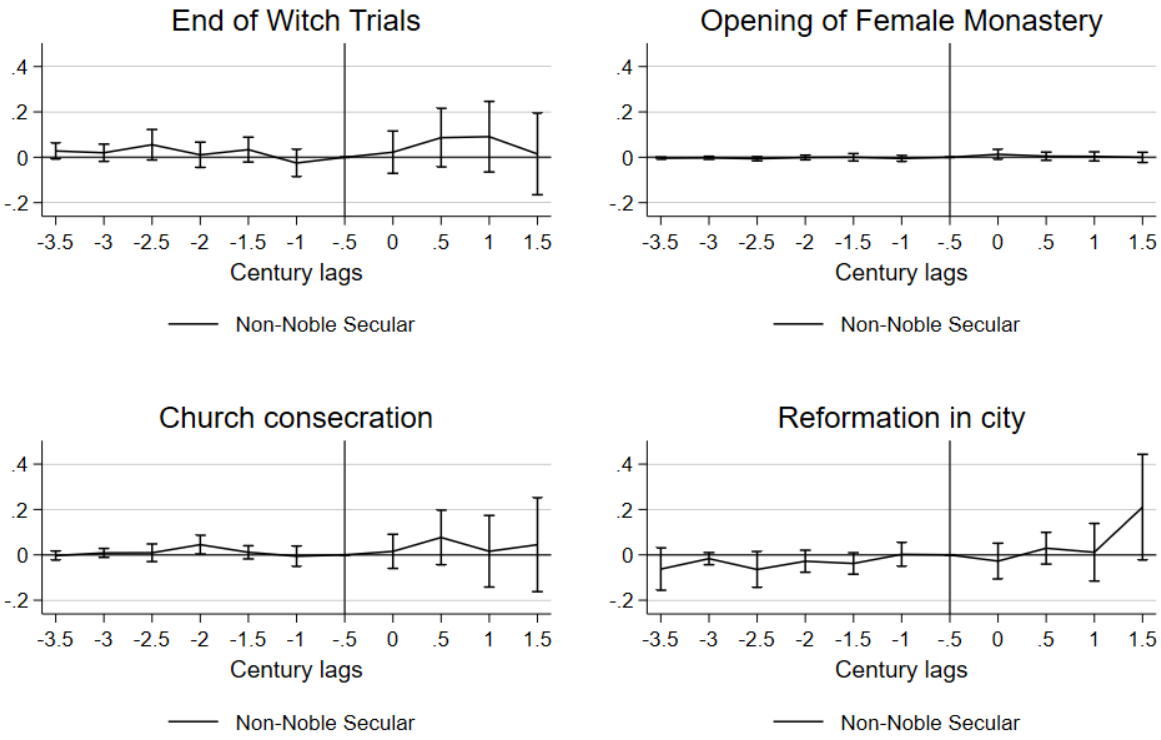
(a) Construction activity



(b) Population

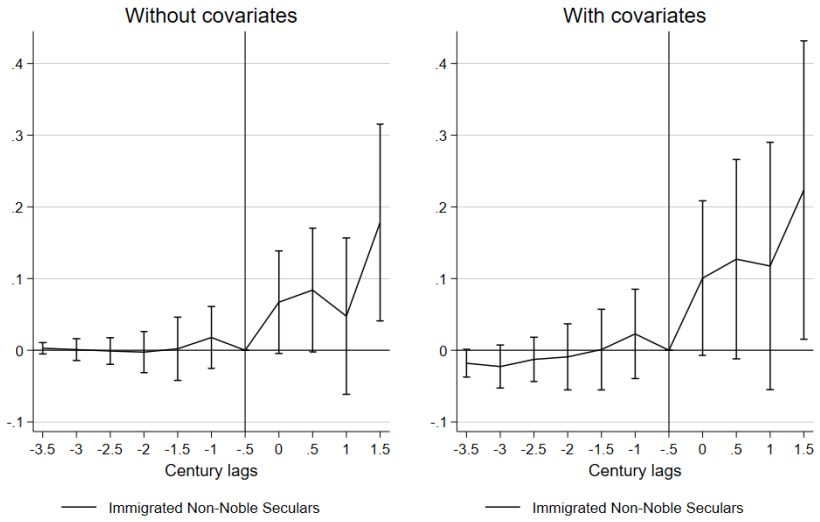
Top Panel a: The correlation between finishing schools and building construction. The outcome in the left panel is an indicator variable capturing construction activity in a given city and period, while the outcome in the right panel is the log number of buildings constructed plus one. All covariates from Table 2 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 2 column (2) included in the right figure. 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 6: Impact of cultural change on notable women



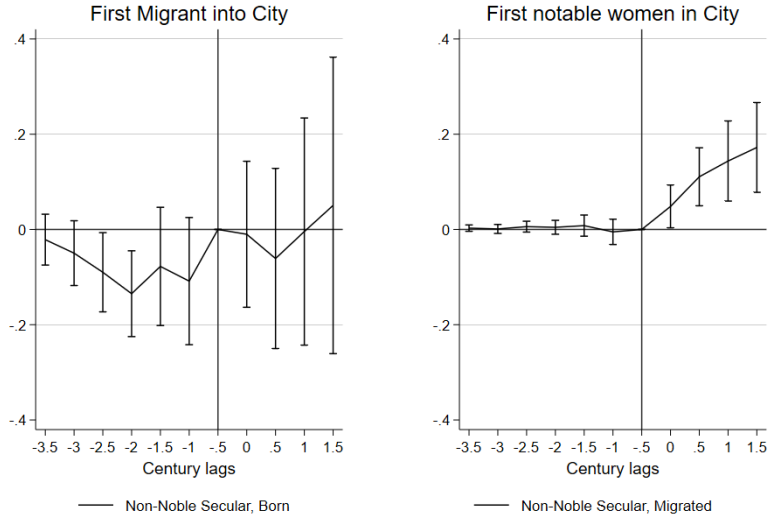
The correlation between notable women and cultural change. The outcome in all panels is an indicator equal to one if a non-noble secular woman was born in a given city and period. The vertical line marks the reference period, which we choose to be 50 years prior to the respective event. Economic and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

Figure 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 right Panel. Point estimates reported in Table H.1. 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: 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	1.620	
Data: Neue Deutsche Biographie			
	Non-Noble Secular (NNS)		
Academic	33	131	0.063
Artists	139	712	0.324
Founders	2	9	0.004
Medicine	17	56	0.028
Not assigned	45	146	0.073
Occupations	39	136	0.067
Politics	43	122	0.063
Sports	0	5	0.002
	Teachers and Writers (also NNS)		
Teacher	27	59	0.033
Writers, Publishers	146	416	0.214
	Activists (also NNS)		
Activists	36	94	0.050
	Unmarried women		
Unmarried	492	1666	0.822
	Nobility		
Royals, Wifes, Relatives	91	170	0.099
	Nuns		
Religion	25	55	0.030
	Population (Bairoch, 1988)		
Population in 1600	5.3	10.4	

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. Activists and unmarried women are separately coded and could belong to all other groups as well. 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 (1.9) and Nuns (2.2), Non-Noble Secular (unmarried) women are 3.6 (3.3) times more likely to appear in cities with finishing schools. We control for the difference in population by interacting 'Population in 1600' with period fixed effects in all regression with control variables.

Table 2: Fixed-effects results on the importance of finishing schools

	$\mathbb{I}[Women > 0]$		log Women		Share Women	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Non-Noble Seculars						
Finishing school$_{it}$	0.230***	0.164***	0.355***	0.204***	0.019***	0.021***
	(0.029)	(0.033)	(0.053)	(0.045)	(0.004)	(0.005)
Mean, untreated	0.150	0.149	0.272	0.272	0.018	0.018
Panel B: Unmarried women						
Finishing school$_{it}$	0.194***	0.147***	0.302***	0.173***	0.011**	0.014**
	(0.030)	(0.034)	(0.049)	(0.043)	(0.005)	(0.006)
Mean, untreated	0.155	0.153	0.275	0.274	0.022	0.022
Panel C: Teachers & Writers						
Finishing school$_{it}$	0.151***	0.104***	0.174***	0.103***	0.019***	0.017***
	(0.027)	(0.026)	(0.034)	(0.029)	(0.006)	(0.006)
Mean, untreated	0.076	0.075	0.096	0.096	0.019	0.019
Panel D: Activists						
Finishing school$_{it}$	0.076***	0.053***	0.064***	0.043***	0.013***	0.011**
	(0.018)	(0.018)	(0.017)	(0.015)	(0.004)	(0.005)
Mean, untreated	0.016	0.016	0.018	0.018	0.005	0.005
Panel E: Nobility						
Finishing school$_{it}$	-0.018	-0.013	-0.009	-0.007	-0.002	-0.002
	(0.016)	(0.017)	(0.015)	(0.018)	(0.008)	(0.009)
Mean, untreated	0.039	0.038	0.050	0.050	0.018	0.018
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	9,312	9,240	9,312	9,240	9,312	9,240

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, except for Activists, where we use the number of male politicians instead. We regress the number of non-noble secular women, teachers and writers, and women from the nobility born in a city, as defined in the top row, on our finishing school variable. 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. 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. 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: Differential returns to education

	Non-Noble Secular			*Unmarried*			*Teachers & Writers*		
	(1) Female	(2) Male	(3) Panel	(4) Female	(5) Male	(6) Panel	(7) Female	(8) Male	(9) Panel
Panel A: Impact of Finishing Schools									
Finishing school$_{it}$	0.096*	-0.002		0.087*	0.003		0.115**	-0.081	
	(0.054)	(0.039)		(0.052)	(0.037)		(0.049)	(0.061)	
Finishing school$_{it}$ × women			0.145**			0.100*			0.123*
			(0.059)			(0.058)			(0.066)
Panel B: Impact of Male Schools									
Male school$_{it}$	0.005	0.066		0.015	0.012		0.000	0.075**	
	(0.012)	(0.040)		(0.021)	(0.041)		(0.005)	(0.034)	
Male school$_{it}$ × men			0.088**			0.072**			0.110***
			(0.038)			(0.036)			(0.030)
City covariates × period FE	Yes	Yes		Yes	Yes		Yes	Yes	
Religious covariates × period FE	Yes	Yes		Yes	Yes		Yes	Yes	
City × period FE			Yes			Yes			Yes
Gender × period FE			Yes			Yes			Yes

Testing the a panel specification with city × time fixed effects in a window of four centuries before and two centuries after the establishing of finishing schools (N=1,421) or male schools (N=2,161). All regressions include a full set of city and period fixed effects. The outcomes are indicators for the birth of notable women or men. In columns (1), (4), and (7), we estimate the impact of finishing schools on women in the sample of cities that ever established a finishing school. In columns (2), (3), and (8) we estimate the impact of finishing schools on men in the sample of cities that ever established a finishing school. In columns (3), (6), and (9) 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 $\mathbb{I}[> 0]$. We include full economic and religious covariates as defined in Table 2 columns (2), (4), and (6) in all regressions. Due to colinearity with our the 'Male school'-treatment variable, we exclude the educational controls. Standard errors clustered by city shown in parentheses. $* \ p < 0.10$, $**$ $p < 0.05$, $* * * \ p < 0.01$

Table 4: Placebo estimates on the importance of finishing schools: Construction Activity

	$\mathbb{I}[> 0]$		*Number*		*log*	
	(1) Any	(2) Growth	(3) Any	(4) Growth	(5) Any	(6) 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 (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 2 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 5: Placebo estimates on the importance of finishing schools:
Changing culture

	Non-Noble Secular		*Unmarried women*		*Teachers & Writers*		*Royals*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: End of witch trials								
End of Witch Trial$_{it}$	0.002	0.052	0.059*	0.062	0.014	0.005	0.031	-0.016
	(0.028)	(0.040)	(0.031)	(0.044)	(0.020)	(0.025)	(0.025)	(0.030)
Religious covariates × period FE		Yes		Yes		Yes		Yes
Panel B: Creation of a female monastery								
Female monastery opens$_{it}$	0.020**	0.012	0.027**	0.018	0.000	-0.004	-0.001	-0.006
	(0.009)	(0.008)	(0.012)	(0.012)	(0.003)	(0.004)	(0.007)	(0.010)
Religious covariates × period FE		Yes		Yes		Yes		Yes
Panel C: Church consecration to a female Saint								
Consecration to a female saint$_{it}$	0.047	0.031	0.019	-0.005	0.040*	0.041	-0.007	0.006
	(0.031)	(0.036)	(0.039)	(0.043)	(0.021)	(0.026)	(0.033)	(0.034)
Religious covariates × period FE		Yes		Yes		Yes		Yes
Panel D: Reformation happening in city								
Reformation in City$_{it}$	0.017	-0.028	0.069**	0.020	0.015	-0.009	0.030	0.036
	(0.025)	(0.019)	(0.027)	(0.033)	(0.015)	(0.016)	(0.033)	(0.041)
Religious covariates × period FE								
City covariates × period FE		Yes		Yes		Yes		Yes
Educational covariates × period FE		Yes		Yes		Yes		Yes

Main results using a fixed-effects estimation in a window of four centuries before and two centuries after the end of witch trials (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 2 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 6: Fixed-effects results on the importance of finishing schools:
Network formation within cities

	$\mathbb{I}[Connections > 0]$		log Connections	
	(1)	(2)	(3)	(4)
Panel A: Any network in city				
Finishing school$_{it}$	0.060***	0.043***	0.069***	0.052**
	(0.016)	(0.016)	(0.021)	(0.021)
Mean, untreated	0.015	0.015	0.020	0.020
Panel B: Network between non-noble secular women				
Finishing school$_{it}$	0.060***	0.043***	0.067***	0.052***
	(0.016)	(0.016)	(0.021)	(0.020)
Mean, untreated	0.012	0.012	0.016	0.016
Panel C: Network between politically active women				
Finishing school$_{it}$	0.016**	0.012	0.018**	0.015*
	(0.007)	(0.009)	(0.008)	(0.009)
Mean, untreated	0.003	0.003	0.003	0.003
Panel D: Network between religious women				
Finishing school$_{it}$	0.006	0.005	0.005	0.004
	(0.005)	(0.007)	(0.004)	(0.005)
Mean, untreated	0.004	0.004	0.004	0.004
Unit trend	Yes	Yes	Yes	Yes
City covariates × period FE		Yes		Yes
Religious covariates × period FE		Yes		Yes
Educational covariates × period FE		Yes		Yes
Observations	9,312	9,240	9,312	9,240

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 two types of dependent variables to capture the extensive and intensive margin of connections among notable women. $\mathbb{I}[Connections > 0]$ 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. We regress the number of connections between any women, non-noble secular women, politically active women, and religious women, as defined in the top row of each Panel, on our finishing school variable. Columns (1) and (3) constitute the baseline and include city and period fixed effects as well as city specific linear trends. In columns (2) and (4) 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. 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: 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
Panel B: All women's rights organizations				
Finishing schools	0.150***	0.137***	1.419***	1.157***
	(0.027)	(0.050)	(0.179)	(0.306)
R-squared	0.101	0.362	0.211	0.483
Mean, untreated	0.367	0.275	444.355	155.802
Bias-Adjusted β		0.132		1.021
Panel C: Women's rights organizations to promote equal access to education				
Finishing schools	0.128***	0.074**	0.779***	0.496**
	(0.017)	(0.036)	(0.112)	(0.217)
R-squared	0.165	0.399	0.198	0.426
Mean, untreated	0.046	0.038	12.973	13.023
Bias-Adjusted β		0.046		0.337
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
Panel E: Member Parliament, 1949–2019				
Finishing schools	0.099***	0.091*	0.312***	0.268***
	(0.020)	(0.047)	(0.036)	(0.071)
R-squared	0.048	0.282	0.203	0.402
Mean, untreated	0.556	0.527	1.170	1.031
Bias-Adjusted β		0.088		0.241
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 letters written from city c to the first active feminist newspaper in Germany. In Panel B, 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 C, we limit the dependent variable from Panel B to only include women's rights organizations in city c that are explicitly dedicated to promoting equal access to education. 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 all German postwar parliaments until 2019. We include covariates as defined in Table 2 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 $\delta = 1$. Standard errors clustered by city shown in parentheses. $* \ p < 0.10$, $** \ p < 0.05$, $*** \ p < 0.01$

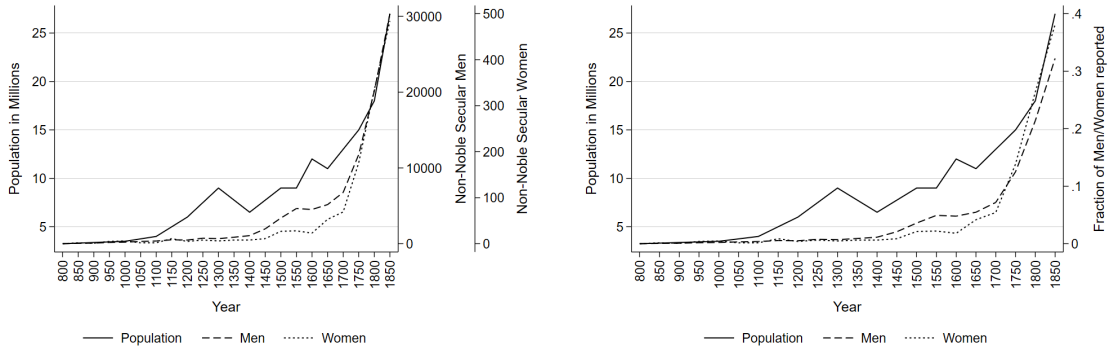
For Online Publication: Appendix
25th October 2021

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. In this Appendix we explore whether this increased representation of women is driven by changes in reporting. 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 3 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 3 and as emphasized in Appendix F we find strong evidence against differential pre-trends.

B Alternative empirical specifications and economic growth

We continue by documenting the robustness of our results presented in Table 2 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.

In a final step, we try to identify pairs of cities that only differ in the presence of finishing schools. Instead of classical matching procedures, which are usually done in cross-sectional settings, we employ increasingly parsimonious fixed effects to create smaller and smaller 'cells' for cities in Table B.2. We start with the full-specification including city-specific trends and all covariates interacted with period fixed effects. In column (2), we include fixed effects grouping cities into 3,244 cells according to their similarity regarding population, membership in the Hanseatic League, occurrence of anti-Jewish pogroms and religious battles within a given period. In columns (3) and (4) we slowly add similar cells for religious and educational covariates, before exactly matching on educational and economic covariates resulting in 6,580 different cells for cities to fall into. The results remain robust throughout the entire set of specifications.

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}	0.300***	0.230***	0.177***	0.298***	0.274***	0.181***	0.164***
	(0.029)	(0.029)	(0.030)	(0.030)	(0.031)	(0.032)	(0.033)
Panel B: Non-Noble Seculars, log Women							
Finishing school_{it}	0.464***	0.355***	0.235***	0.460***	0.423***	0.246***	0.204***
	(0.063)	(0.053)	(0.046)	(0.063)	(0.063)	(0.048)	(0.045)
Panel C: Non-Noble Seculars, Share Women							
Finishing school_{it}	0.022***	0.019***	0.019***	0.022***	0.023***	0.021***	0.021***
	(0.004)	(0.004)	(0.005)	(0.004)	(0.004)	(0.005)	(0.005)
Panel D: Unmarried women, $\mathbb{I}[Women > 0]$							
Finishing school_{it}	0.276***	0.194***	0.167***	0.272***	0.260***	0.173***	0.147***
	(0.029)	(0.030)	(0.031)	(0.029)	(0.031)	(0.032)	(0.034)
Panel E: Unmarried women, log Women							
Finishing school_{it}	0.422***	0.302***	0.215***	0.415***	0.388***	0.226***	0.173***
	(0.060)	(0.049)	(0.045)	(0.061)	(0.060)	(0.047)	(0.043)
Panel F: Unmarried women, Share Women							
Finishing school_{it}	0.015***	0.011**	0.015***	0.015***	0.017***	0.016***	0.014**
	(0.004)	(0.005)	(0.005)	(0.004)	(0.004)	(0.005)	(0.006)
Panel G: Teachers & Writers, $\mathbb{I}[Women > 0]$							
Finishing school_{it}	0.196***	0.151***	0.111***	0.196***	0.176***	0.117***	0.104***
	(0.026)	(0.027)	(0.024)	(0.026)	(0.026)	(0.024)	(0.026)
Panel H: Teachers & Writers, log Women							
Finishing school_{it}	0.220***	0.174***	0.116***	0.220***	0.194***	0.120***	0.103***
	(0.037)	(0.034)	(0.027)	(0.037)	(0.035)	(0.028)	(0.029)
Panel I: Teachers & Writers, Share Women							
Finishing school_{it}	0.024***	0.019***	0.018***	0.024***	0.023***	0.019***	0.017***
	(0.005)	(0.006)	(0.005)	(0.005)	(0.005)	(0.005)	(0.006)
Panel J: Activists, $\mathbb{I}[Women > 0]$							
Finishing school_{it}	0.077***	0.076***	0.044***	0.077***	0.078***	0.051***	0.053***
	(0.017)	(0.018)	(0.016)	(0.017)	(0.017)	(0.017)	(0.018)
Panel K: Activists, log Women							
Finishing school_{it}	0.066***	0.064***	0.038***	0.067***	0.066***	0.043***	0.043***
	(0.017)	(0.017)	(0.014)	(0.018)	(0.017)	(0.015)	(0.015)
Panel L: Activists, Share Women							
Finishing school_{it}	0.011***	0.013***	0.008**	0.011***	0.012***	0.009**	0.011**
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.005)
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, except for Activists, where use the number of male politicians. 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 the absolute 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. $* \ p < 0.10$, $** \ p < 0.05$, $*** \ p < 0.01$

Table B.2: Fixed-effects results on the importance of finishing schools - Exactly matching on covariates in 1600

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: $\mathbb{I}[Women > 0]$						
Finishing school$_{it}$	0.164***	0.203***	0.214***	0.159***	0.164***	0.171***
	(0.033)	(0.038)	(0.040)	(0.045)	(0.050)	(0.047)
Panel B: log Women						
Finishing school$_{it}$	0.204***	0.224***	0.238***	0.163***	0.167***	0.175***
	(0.045)	(0.047)	(0.050)	(0.055)	(0.059)	(0.058)
Panel C: Share Women						
Finishing school$_{it}$	0.021***	0.021***	0.021***	0.014**	0.015*	0.015**
	(0.005)	(0.007)	(0.007)	(0.007)	(0.008)	(0.007)
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
Exact match on economic covariates		Yes	Yes	Yes	Yes	Yes
Exact match on religious covariates			Yes	Yes	Yes	Yes
Exact match on educational covariates				Yes	Yes	Yes
Exact match on educational and economic covariates					Yes	Yes
Exact match on educational and religious covariates						Yes
Observations	9,312	9,312	9,312	9,312	9,312	9,312
Number of Fixed Effects	1,300	3,244	3,484	5,284	6,580	5,956

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 include city and period fixed effects as well as city-specific linear trends. In column (1) 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 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. In column (2) we interact all economic covariates with each other to compare cities within one Population-Hanse-Jewish-Pogrom-Battle-Period cell. In column (3) we additionally interact all religious covariates with each other to compare cities within one Population-bishop-Catholic-Period cell. In column (4) we additionally interact all religious covariates with each other to compare cities within one Population-Male school-University-Ruler-Period cell. In column (5) we additionally control for the interaction of (2) and (3). In column (6) we additionally control for the interaction of (2) and (4). All covariates are interacted with period fixed effects. Significance levels are $* \ p < 0.10$, $** \ p < 0.05$, $*** \ p < 0.01$

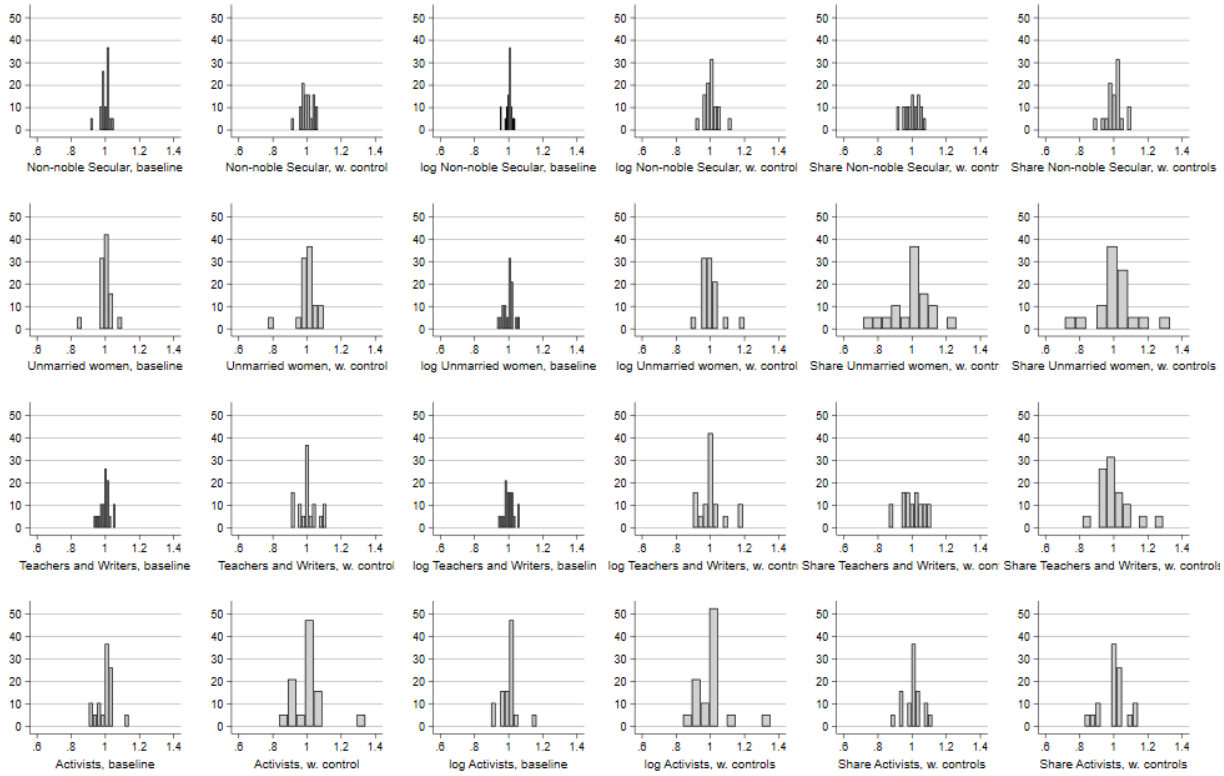
B.1 Sensitivity to dropping observations

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 rows) in all specifications (columns) corresponding to Tables 2 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 0.7 (for the share of unmarried women) and a maximum of 1.3 (for the log number of activists) 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).

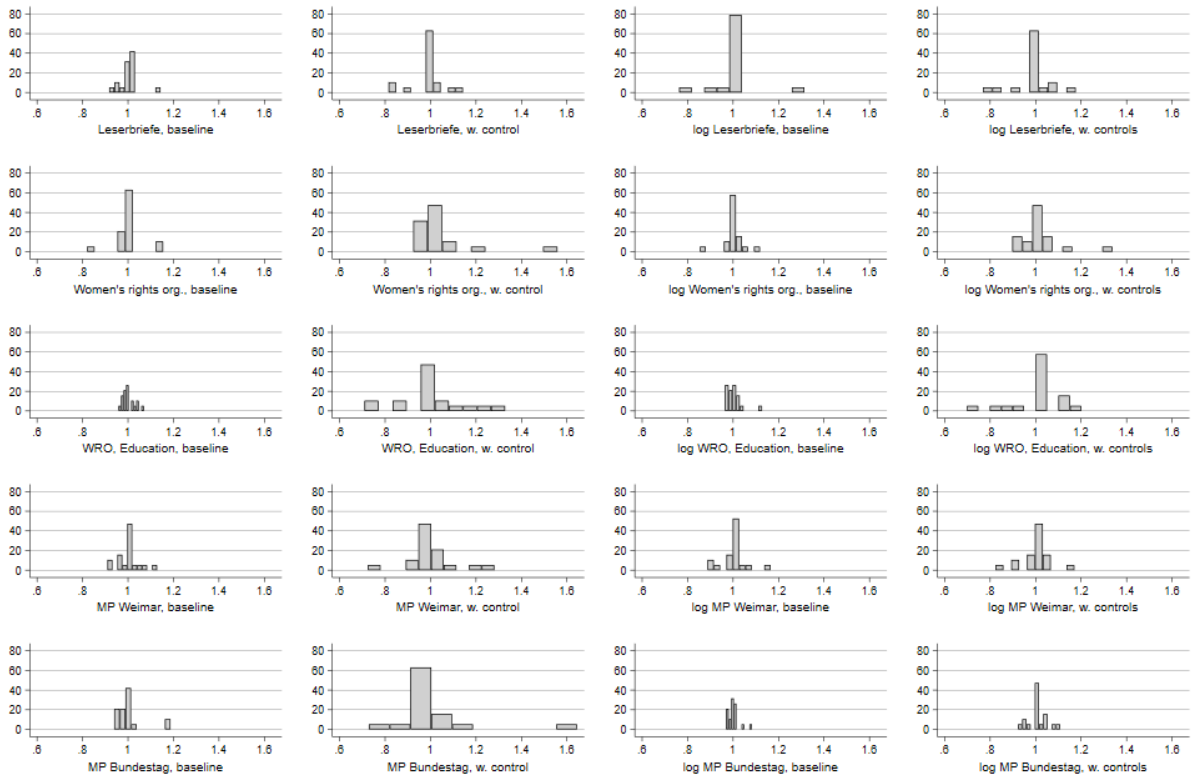
Overall, the density plots reveal a stable pattern around the estimated mean, suggesting that our results are not driven by individual cities or sample selection.

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



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 2. 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 2. 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

Finally, we address the possibility that our city covariates do not adequately capture economic growth by including construction data from Cantoni et al. (2021). 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, as shown in 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.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)
Panel A: Non-Noble Seculars						
Finishing school$_{it}$	0.161***	0.169***	0.208***	0.214***	0.020***	0.021***
	(0.034)	(0.034)	(0.045)	(0.045)	(0.006)	(0.005)
Mean, untreated	0.148	0.147	0.138	0.137	0.018	0.018
Panel B: Unmarried women						
Finishing school$_{it}$	0.150***	0.149***	0.182***	0.183***	0.014**	0.014**
	(0.035)	(0.035)	(0.042)	(0.042)	(0.006)	(0.006)
Mean, untreated	0.152	0.152	0.142	0.141	0.022	0.022
Panel C: Teachers & Writers						
Finishing school$_{it}$	0.104***	0.109***	0.106***	0.112***	0.018***	0.019***
	(0.027)	(0.027)	(0.027)	(0.028)	(0.006)	(0.006)
Mean, untreated	0.075	0.075	0.059	0.059	0.019	0.019
Panel D: Activists						
Finishing school$_{it}$	0.065***	0.053***	0.049***	0.044***	0.015***	0.012**
	(0.018)	(0.018)	(0.015)	(0.016)	(0.005)	(0.005)
Mean, untreated	0.016	0.016	0.012	0.012	0.005	0.005
Panel E: Nobility						
Finishing school$_{it}$	-0.017	-0.015	-0.007	-0.006	-0.002	-0.002
	(0.017)	(0.016)	(0.017)	(0.015)	(0.009)	(0.008)
Mean, untreated	0.038	0.037	0.030	0.030	0.017	0.017
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
Construction in 1650 × period FE	Yes		Yes		Yes	
Construction in every period × period FE		Yes		Yes		Yes
Observations	9,096	9,144	9,096	9,144	9,096	9,144

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, except for Activists, where we use the number of male politicians. We regress the number of non-noble secular women, teachers and writers, and noble women born in a city, as defined in the top row, on our finishing school variable. 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. 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. Significance levels are $* \ p < 0.10$, $** \ p < 0.05$, $*** \ p < 0.01$

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.

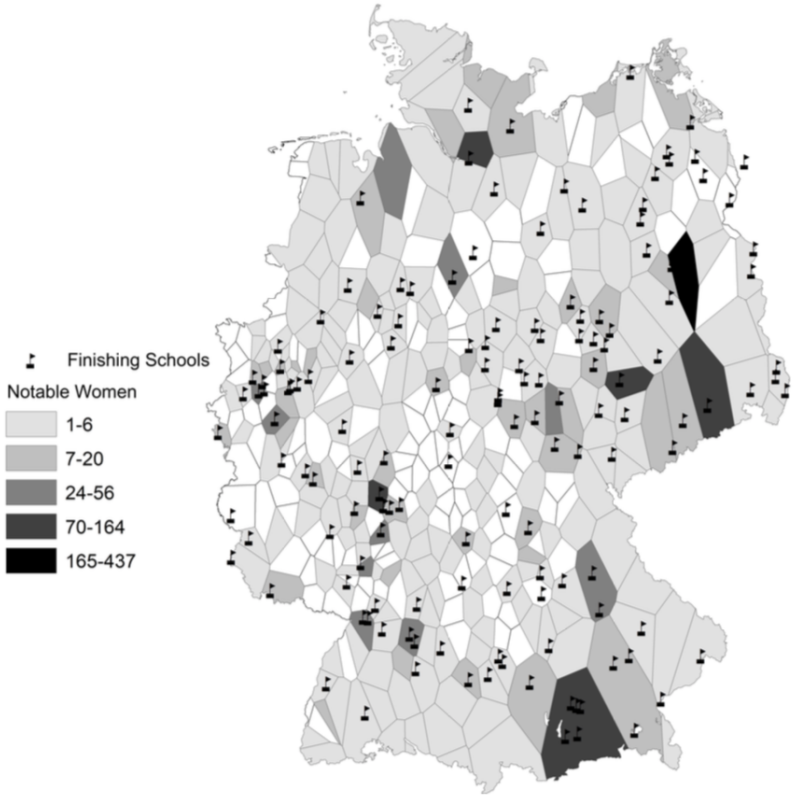
As the NDB starts recording notable individuals born from the year 800 onwards, using cities with recorded population levels by 800 is a natural alternative, which, however, reduces the sample of cities to 101. In Table C.1 we document that results for both choices (1300 vs. 800) are similar across all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

The final choice concerns the classification of notable women into different (occupational) groups: *Non-Noble Seculars, Unmarried, Teachers & Writers, Activists, and the Nobility*. We grouped women together to ensure enough variation within every city-period-occupation-cell. In Table C.3, we show the consistent impact across most occupational groups. In addition to our baseline results, we show that finishing schools increase the share of unmarried women (Panel A), artists (Panel D), writers (Panel E), politicians (Panel G), academics (H), but not the share of nuns (Panel J). This evidence, especially the impact on academics, artists, and writers, reinforces the notion that finishing schools increased the share of women among the human capital elite.

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$_{it}$	0.251***	0.230***	0.465***	0.356***	0.016***	0.017*
	(0.049)	(0.064)	(0.098)	(0.100)	(0.005)	(0.009)
Mean, untreated	0.201	0.180	0.214	0.189	0.020	0.018
Panel B: Unmarried women						
Finishing school$_{it}$	0.134***	0.142**	0.356***	0.272***	0.007	0.010
	(0.048)	(0.067)	(0.086)	(0.097)	(0.007)	(0.009)
Mean, untreated	0.242	0.226	0.252	0.227	0.024	0.023
Panel B: Teachers & Writers						
Finishing school$_{it}$	0.183***	0.154**	0.257***	0.179**	0.019**	0.016
	(0.048)	(0.062)	(0.067)	(0.072)	(0.008)	(0.011)
Mean, untreated	0.103	0.090	0.091	0.076	0.019	0.016
Panel C: Activists						
Finishing school$_{it}$	0.104***	0.077*	0.100***	0.058	0.016**	0.016*
	(0.032)	(0.046)	(0.031)	(0.039)	(0.006)	(0.009)
Mean, untreated	0.029	0.026	0.023	0.020	0.005	0.005
Panel D: Nobility						
Finishing school$_{it}$	-0.018	-0.056	-0.001	-0.036	0.002	-0.033
	(0.037)	(0.044)	(0.039)	(0.043)	(0.019)	(0.023)
Mean, untreated	0.105	0.098	0.092	0.083	0.045	0.041
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	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, except for Activists, where we use the number of male politicians. We regress the number of non-noble secular women, teachers and writers, and noble women born in a city, as defined in the top row, on our finishing school variable. 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. 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. Significance levels are $* \ p < 0.10$, $** \ p < 0.05$, $*** \ p < 0.01$

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

	$\mathbb{I}[Women > 0]$		log Women		Share Women	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Non-Noble Seculars						
Finishing school$_{it}$	0.149***	0.096***	0.212***	0.110***	0.016***	0.014***
	(0.021)	(0.021)	(0.038)	(0.030)	(0.003)	(0.004)
Mean, untreated	0.094	0.093	0.142	0.142	0.015	0.015
Panel B: Unmarried women						
Finishing school$_{it}$	0.124***	0.088***	0.182***	0.097***	0.011***	0.012***
	(0.020)	(0.022)	(0.034)	(0.028)	(0.003)	(0.004)
Mean, untreated	0.097	0.097	0.143	0.142	0.017	0.017
Panel C: Teachers & Writers						
Finishing school$_{it}$	0.088***	0.059***	0.096***	0.057***	0.015***	0.013***
	(0.017)	(0.016)	(0.022)	(0.019)	(0.004)	(0.004)
Mean, untreated	0.044	0.043	0.050	0.050	0.013	0.013
Panel D: Activists						
Finishing school$_{it}$	0.042***	0.030***	0.034***	0.023***	0.008***	0.007**
	(0.011)	(0.011)	(0.010)	(0.009)	(0.003)	(0.003)
Mean, untreated	0.009	0.009	0.009	0.009	0.003	0.003
Panel E: Royals						
Finishing school$_{it}$	-0.014*	-0.009	-0.007	-0.005	-0.003	-0.002
	(0.008)	(0.009)	(0.008)	(0.010)	(0.004)	(0.005)
Mean, untreated	0.021	0.021	0.025	0.025	0.010	0.010
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	18,624	18,480	18,624	18,480	18,624	18,480

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, except for Activists, where we use the number of male politicians. We regress the number of non-noble secular women, teachers and writers, and noble women born in a city, as defined in the top row, on our finishing school variable. 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. 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. Significance levels are $* \ p < 0.10$, $** \ p < 0.05$, $*** \ p < 0.01$

Table C.3: Fixed-effects results on the importance of finishing schools - All occupations

	$\mathbb{I}[Women > 0]$		log Women		Share Women	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Unmarried women						
Finishing school$_{it}$	0.194***	0.147***	0.302***	0.173***	0.011**	0.014**
	(0.030)	(0.034)	(0.049)	(0.043)	(0.005)	(0.006)
Mean, untreated	0.155	0.153	0.275	0.274	0.022	0.022
Panel B: Non-Royal women						
Finishing school$_{it}$	0.224***	0.164***	0.350***	0.201***	0.018***	0.018***
	(0.030)	(0.034)	(0.053)	(0.045)	(0.004)	(0.005)
Mean, untreated	0.156	0.154	0.285	0.284	0.018	0.018
Panel C: Occupation						
Finishing school$_{it}$	0.055***	0.025	0.058***	0.021	0.004	0.004
	(0.017)	(0.018)	(0.018)	(0.016)	(0.003)	(0.004)
Mean, untreated	0.019	0.019	0.020	0.020	0.004	0.004
Panel D: Artists						
Finishing school$_{it}$	0.137***	0.062**	0.187***	0.071**	0.027***	0.016**
	(0.027)	(0.028)	(0.043)	(0.033)	(0.007)	(0.007)
Mean, untreated	0.056	0.056	0.085	0.085	0.013	0.013
Panel E: Writers						
Finishing school$_{it}$	0.147***	0.099***	0.159***	0.096***	0.023***	0.020***
	(0.025)	(0.025)	(0.032)	(0.027)	(0.006)	(0.006)
Mean, untreated	0.067	0.067	0.084	0.083	0.020	0.020
Panel F: Doctors						
Finishing school$_{it}$	0.021*	-0.003	0.020**	-0.003	0.003	-0.000
	(0.011)	(0.011)	(0.009)	(0.009)	(0.003)	(0.003)
Mean, untreated	0.009	0.009	0.009	0.009	0.003	0.003
Panel G: Politicians						
Finishing school$_{it}$	0.058***	0.025	0.054***	0.018	0.011**	0.007
	(0.017)	(0.018)	(0.016)	(0.015)	(0.004)	(0.005)
Mean, untreated	0.018	0.018	0.020	0.020	0.005	0.005
Panel H: Academics						
Finishing school$_{it}$	0.080***	0.056***	0.069***	0.037***	0.009***	0.009**
	(0.015)	(0.016)	(0.014)	(0.014)	(0.003)	(0.004)
Mean, untreated	0.014	0.014	0.016	0.016	0.003	0.003
Panel I: Teachers						
Finishing school$_{it}$	0.041***	0.018	0.036***	0.014	0.006*	0.005
	(0.014)	(0.012)	(0.012)	(0.010)	(0.003)	(0.003)
Mean, untreated	0.011	0.011	0.012	0.012	0.003	0.003
Panel J: Nunns						
Finishing school$_{it}$	0.001	0.002	0.001	0.000	-0.002	-0.000
	(0.011)	(0.013)	(0.008)	(0.009)	(0.003)	(0.004)
Mean, untreated	0.012	0.012	0.012	0.013	0.004	0.004
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	9,312	9,240	9,312	9,240	9,312	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, except for Activists, where we use the number of male politicians. We regress the number of non-noble secular women, teachers and writers, and noble women born in a city, as defined in the top row, on our finishing school variable. 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. 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. Standard errors clustered by city shown in parentheses. $* \ p < 0.10$, $** \ p < 0.05$, $*** \ p < 0.01$

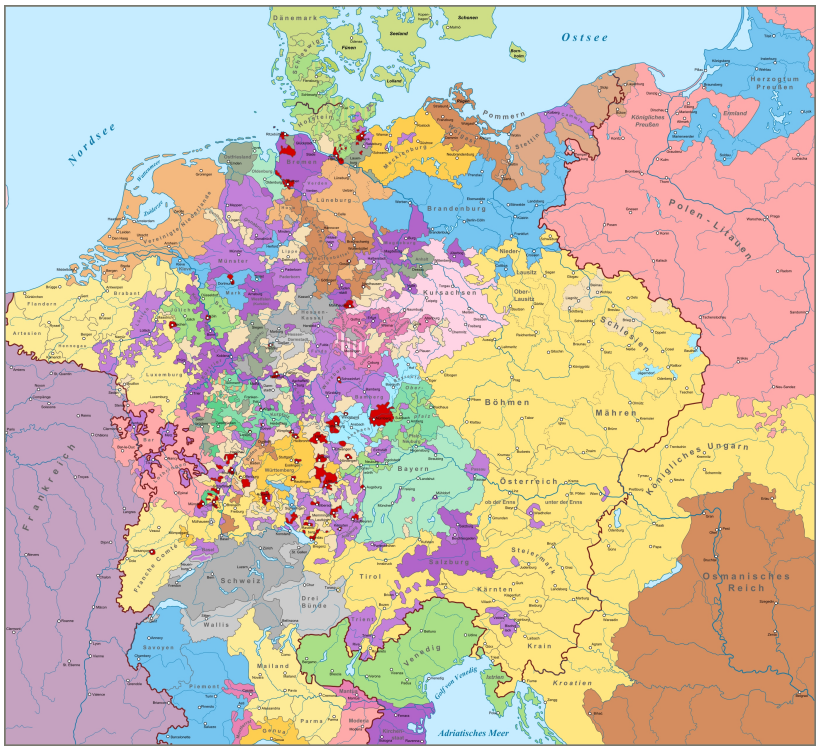
C.2 Sample selection: Using a different starting point for the analysis

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.

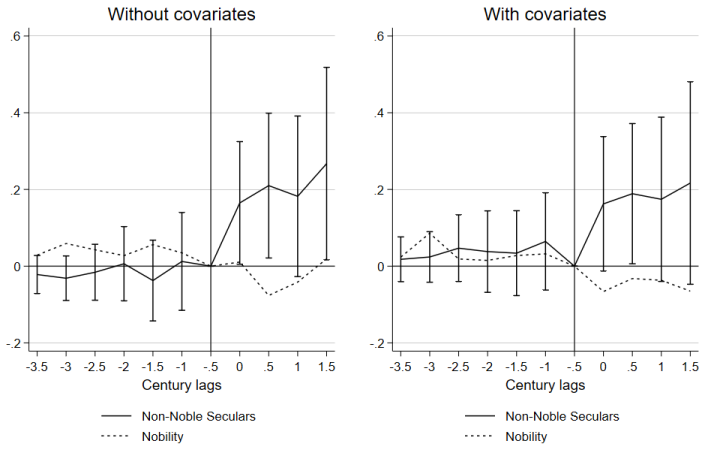
The event-study results in Figure C.3 and the fixed effects results in Table C.4, however, confirm our initial results. We conclude that choosing the cities from Voigtländer and Voth (2012) to create Thiessen polygons did not introduce a bias into our setting.

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

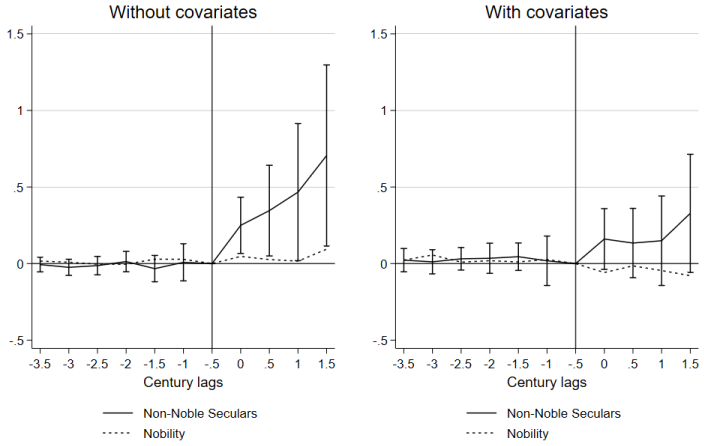


This figure shows the territories of rulers, ecclesiastical cities, free cities, and imperial cities in 1619, which we use as a baseline for the results in this section.

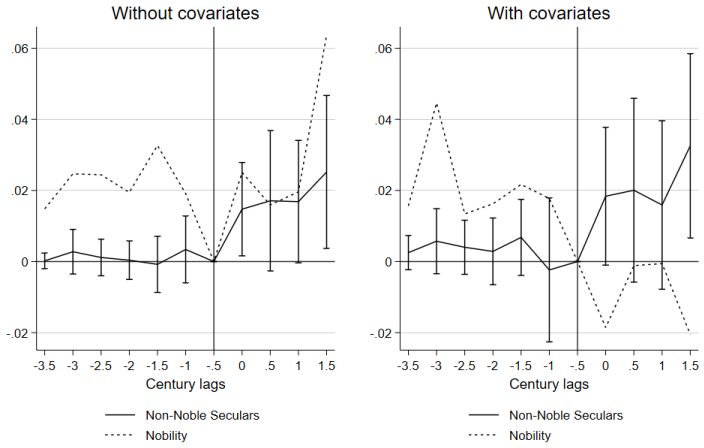
Figure C.3: Event-Study: Impact of finishing school establishment on notable women



(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 and women from the *nobility*. 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 the left figure and full economic, religious, and educational controls added in the right. 95%-confidence intervals shown only for non-noble secular, the impact of nobility is indistinguishable form zero in all periods and specifications.

Table C.4: 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$_{it}$	0.376***	0.188***	0.728***	0.270***	0.024***	0.017***
	(0.039)	(0.046)	(0.096)	(0.065)	(0.004)	(0.006)
Mean, untreated	0.081	0.078	0.141	0.128	0.011	0.011
Panel B: Unmarried women						
Finishing school$_{it}$	0.283***	0.154***	0.603***	0.244***	0.017***	0.018***
	(0.040)	(0.045)	(0.081)	(0.070)	(0.005)	(0.007)
Mean, untreated	0.086	0.082	0.157	0.134	0.013	0.013
Panel C: Teachers & Writers						
Finishing school$_{it}$	0.283***	0.097**	0.381***	0.105***	0.033***	0.018**
	(0.039)	(0.038)	(0.065)	(0.036)	(0.007)	(0.008)
Mean, untreated	0.037	0.035	0.057	0.048	0.008	0.008
Panel D: Activists						
Finishing school$_{it}$	0.151***	0.060**	0.146***	0.057**	0.021***	0.014**
	(0.029)	(0.028)	(0.032)	(0.023)	(0.005)	(0.007)
Mean, untreated	0.007	0.006	0.008	0.006	0.002	0.002
Panel E: Nobility						
Finishing school$_{it}$	-0.070**	-0.052*	-0.035	-0.044*	-0.009	-0.018
	(0.029)	(0.028)	(0.025)	(0.024)	(0.012)	(0.012)
Mean, untreated	0.029	0.025	0.043	0.031	0.012	0.011
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, except for Activists, where we use the number of male politicians. We regress the number of non-noble secular women, teachers and writers, and noble women born in a city, as defined in the top row, on our finishing school variable. 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. 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. Significance levels are $* \ p < 0.10$, $** \ p < 0.05$, $*** \ p < 0.01$

C.3 Using the exact opening time of finishing schools

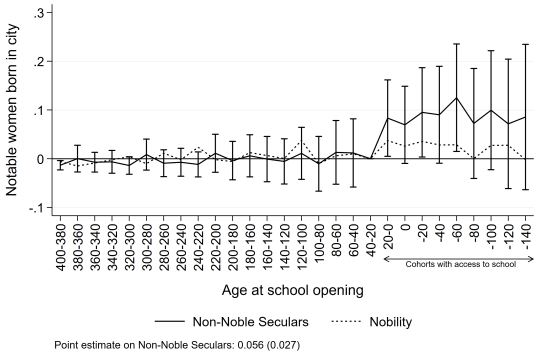
In our baseline data, we created a balanced panel for each city to include never-treated cities and covariates. This decision is in line with the recent literature on event-study validity, as discussed in Appendix F. In the resulting panel, we merged individuals to the closest of 50-year periods in cities. 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.

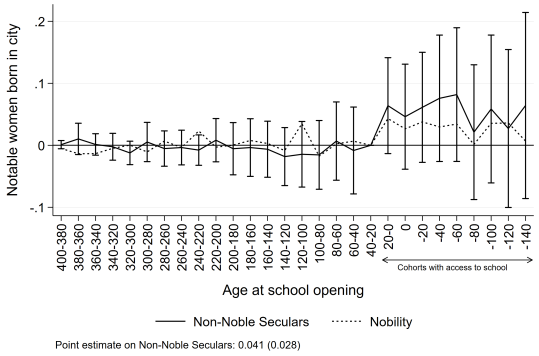
In Figure C.4, we use the opening time of the first finishing school in our 129 cities with schools and create various lags around it. In all Panels, we aim to reference the estimates to a previous generation of women who could no longer benefit from education: parents. In Panel a and b), we create 20-year lag windows around each school and omit women born between 20 and 39 years and 25 and 50 years before in the 25-year Panel c and d. 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.

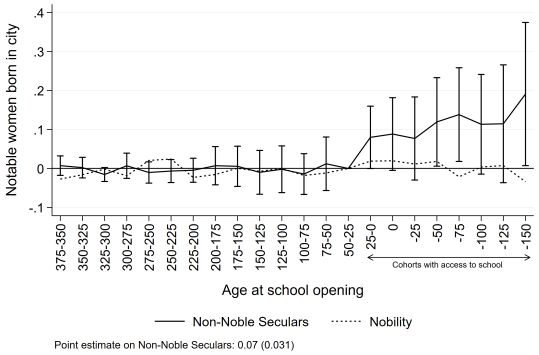
Figure C.4: Event-Study: Impact of finishing school establishment on notable women



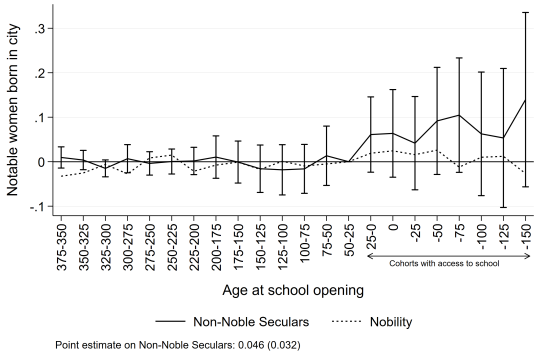
(a) 20 year periods, 50 year fixed effects



(b) 20 year periods, 20 year fixed effects



(c) 25 year periods, 50 year fixed effects



(d) 25 year periods, 25 year fixed effects

Event study graphs using the exact timing of the first finishing school in every city to create 20-year periods (C.4a-b) and 25-year periods (C.4c-d). We include fifty-year period fixed effects in Figures C.4a and c for commonality and comparability across figures; showing that period fixed effects only increase standard errors in Figures C.4b and d. Results are robust to using year-fixed effects that include ≥ 645 fixed effects for every year.

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.5. In addition, the point estimates on *early* and *late* schools are not statistically different from each other in most specifications.

Moreover, in Table C.6 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. Combined with the impact of multiple schools shown in Figure C.5, 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 (Figure G.2 and Table G.2).

Table C.5: 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$_{it}$	0.095**	0.185***	0.279***	0.246***	0.016**	0.023***
	(0.041)	(0.044)	(0.100)	(0.057)	(0.007)	(0.007)
Mean, untreated	0.147	0.148	0.137	0.138	0.019	0.018
Panel B: Unmarried women						
Finishing school$_{it}$	0.050	0.180***	0.231**	0.217***	0.004	0.018***
	(0.046)	(0.044)	(0.092)	(0.053)	(0.010)	(0.007)
Mean, untreated	0.148	0.152	0.137	0.141	0.022	0.022
Panel C: Teachers & Writers						
Finishing school$_{it}$	0.095**	0.129***	0.166**	0.124***	0.011	0.022***
	(0.041)	(0.032)	(0.081)	(0.032)	(0.008)	(0.007)
Mean, untreated	0.074	0.074	0.058	0.059	0.019	0.019
Panel D: Activists						
Finishing school$_{it}$	0.053*	0.066***	0.070	0.051***	0.004	0.014**
	(0.029)	(0.022)	(0.043)	(0.018)	(0.004)	(0.006)
Mean, untreated	0.018	0.016	0.013	0.012	0.006	0.005
Panel E: Nobility						
Finishing school$_{it}$	-0.022	-0.014	0.004	-0.012	-0.002	-0.003
	(0.039)	(0.019)	(0.035)	(0.016)	(0.018)	(0.009)
Mean, untreated	0.031	0.037	0.024	0.030	0.015	0.017
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
Observations	6,984	8,400	6,984	8,400	6,984	8,400

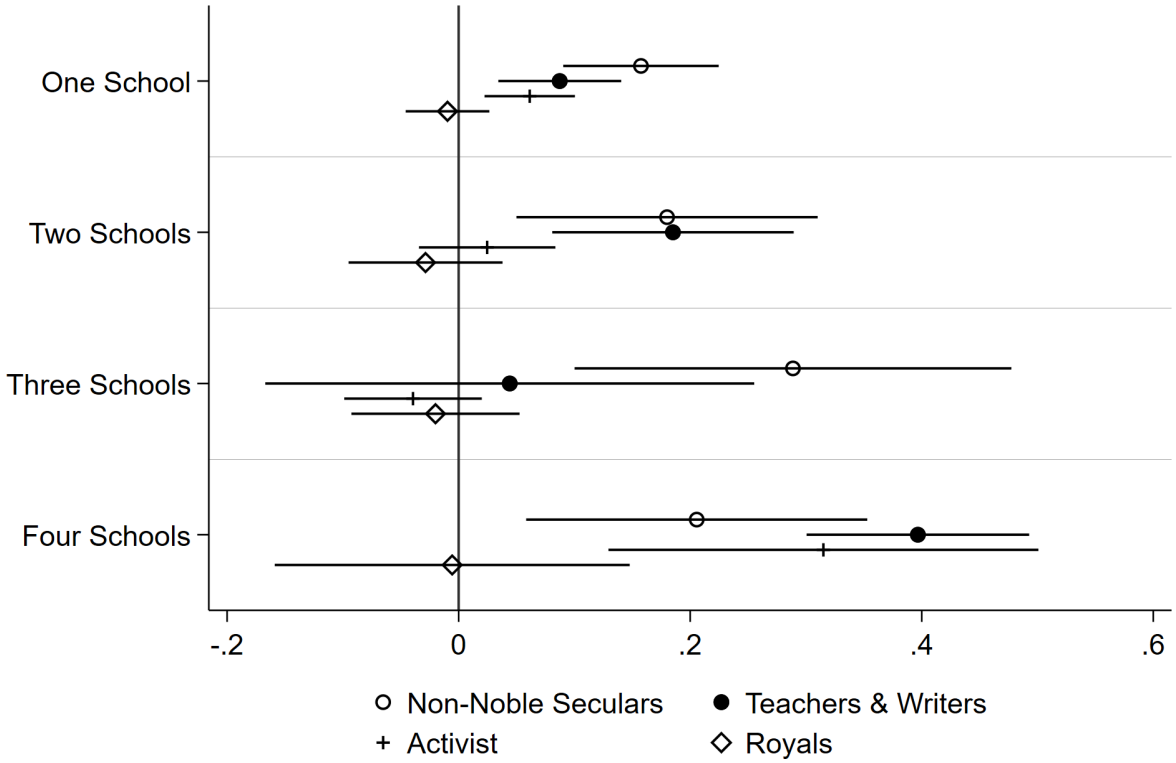
Main results comparing schools constructed between 1650–1750 (*early*) and 1800–1850 (*late*) to assess the sensitivity of our results to schools whose establishment is potentially influenced by rising demand for women's education in the context of the Industrial Revolution in Germany. We consider three 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, except for Activists, where we use the number of male politicians. We regress the number of non-noble secular women, teachers and writers, and noble women born in a city, as defined in the top row, on our finishing school variable. In all columns 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 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. Significance levels are $* \ p < 0.10$, $** \ p < 0.05$, $*** \ p < 0.01$

Table C.6: 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)
Panel A: Non-Noble Seculars						
First finishing school$_{it}$	0.164***	0.156***	0.204***	0.147***	0.021***	0.020***
	(0.033)	(0.034)	(0.045)	(0.044)	(0.005)	(0.006)
Second finishing school$_{it}$		0.040		0.279**		0.003
		(0.058)		(0.109)		(0.008)
Mean, untreated	0.149	0.149	0.139	0.139	0.018	0.018
Panel B: Unmarried women						
First finishing school$_{it}$	0.147***	0.154***	0.173***	0.137***	0.014**	0.016**
	(0.034)	(0.035)	(0.043)	(0.043)	(0.006)	(0.006)
Second finishing school$_{it}$		-0.039		0.180*		-0.008
		(0.056)		(0.097)		(0.007)
Mean, untreated	0.153	0.153	0.143	0.143	0.022	0.022
Panel C: Teachers & Writers						
First finishing school$_{it}$	0.104***	0.082***	0.103***	0.065**	0.017***	0.015**
	(0.026)	(0.027)	(0.029)	(0.027)	(0.006)	(0.006)
Second finishing school$_{it}$		0.110**		0.191**		0.010
		(0.047)		(0.077)		(0.011)
Mean, untreated	0.075	0.075	0.059	0.059	0.019	0.019
Panel D: Activists						
First finishing school$_{it}$	0.053***	0.053***	0.043***	0.039**	0.011**	0.015***
	(0.018)	(0.020)	(0.015)	(0.016)	(0.005)	(0.005)
Second finishing school$_{it}$		-0.004		0.019		-0.017***
		(0.036)		(0.037)		(0.006)
Mean, untreated	0.016	0.016	0.012	0.012	0.005	0.005
Panel E: Nobility						
First finishing school$_{it}$	-0.013	-0.010	-0.007	-0.001	-0.002	-0.000
	(0.017)	(0.018)	(0.018)	(0.018)	(0.009)	(0.009)
Second finishing school$_{it}$		-0.015		-0.027		-0.008
		(0.035)		(0.026)		(0.014)
Mean, untreated	0.038	0.038	0.031	0.031	0.018	0.018
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
Observations	9,240	9,240	9,240	9,240	9,240	9,240

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, except for Activists, where we use the number of male politicians. We regress the number of non-noble secular women, teachers and writers, and noble women born in a city, as defined in the top row, on our finishing school variable. 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. 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. Significance levels are $* \ p < 0.10$, $** \ p < 0.05$, $*** \ p < 0.01$

Figure C.5: 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 2 column (2) included.

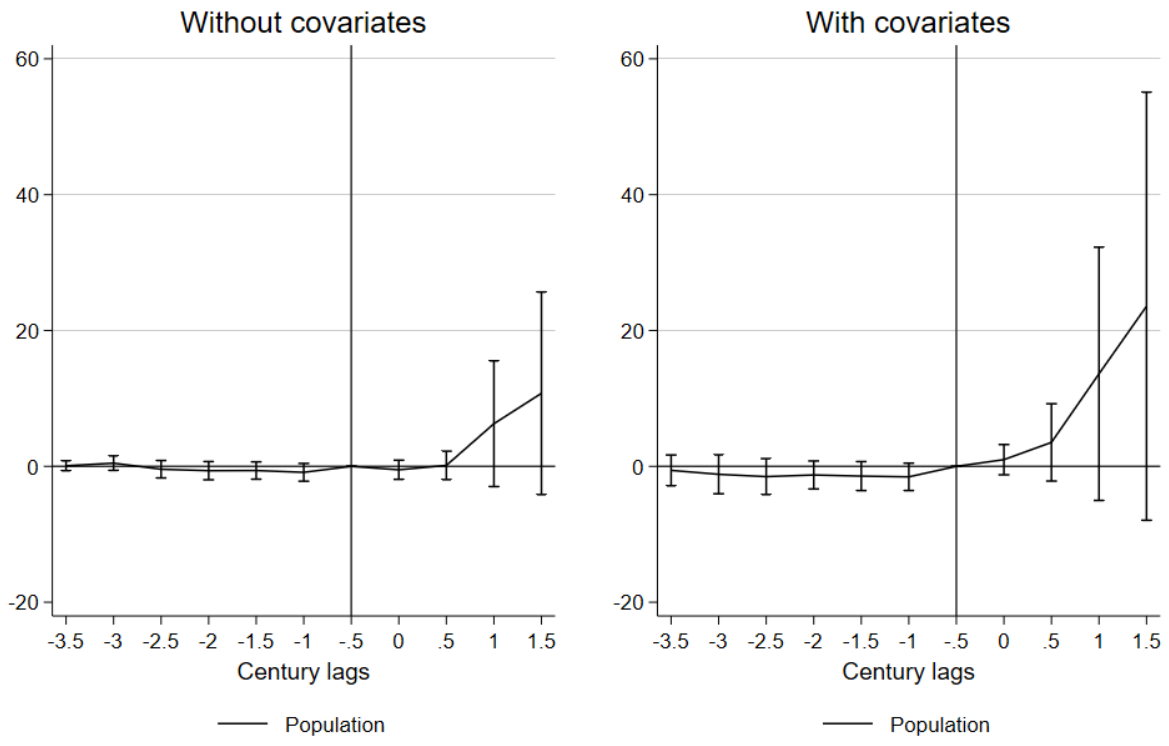
D The role of demand-side factors

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 2 column (2) included in the right figure.

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).[42] 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. 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.

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

Table D.1: Fixed-effects results on the importance of finishing schools - controlling for Enlightenment journals

	Period prior to finishing school establishment			Period of finishing school establishment		
	Non-Noble Secular Women (1)	# Enlightenment Journals (2)	# Articles on/for women (3)	Non-Noble Secular Women (4)	# Enlightenment Journals (5)	# Articles on/for women (6)
Finishing school	0.061	0.095	-0.047	0.200***	0.127	0.971
	(0.057)	(0.065)	(0.160)	(0.068)	(0.110)	(0.963)
City covariates	Yes	Yes	Yes	Yes	Yes	Yes
Religious covariates	Yes	Yes	Yes	Yes	Yes	Yes
Educational covariates	Yes	Yes	Yes	Yes	Yes	Yes
Period fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	717	717	717	717	717	717
Bandwidth	10	10	10	10	10	10
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 2 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 in this period to cities that never establish a finishing school. The sample of 717 thus contains 185 cities within 10 kilometers of the religious divide: 52 of which are treated and counted once (in the period of finishing school construction), and 133 cities untreated cities that are counted for each period in which a finishing school was constructed (1650, 1700, 1750, 1800, 1850). Period fixed effects then ensure a comparison *within each period*. Significance levels are $* \ p < 0.10$, $** \ p < 0.05$, $*** \ p < 0.01$

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$_{it}$	0.164***	0.163***	0.204***	0.195***	0.021***	0.021***
	(0.033)	(0.034)	(0.045)	(0.044)	(0.005)	(0.005)
Mean, untreated	0.149	0.147	0.272	0.263	0.018	0.018
Panel B: Unmarried women						
Finishing school$_{it}$	0.147***	0.147***	0.173***	0.168***	0.014**	0.014**
	(0.034)	(0.034)	(0.043)	(0.042)	(0.006)	(0.006)
Mean, untreated	0.153	0.151	0.274	0.268	0.022	0.021
Panel C: Teachers & Writers						
Finishing school$_{it}$	0.104***	0.102***	0.103***	0.095***	0.017***	0.017***
	(0.026)	(0.025)	(0.029)	(0.027)	(0.006)	(0.006)
Mean, untreated	0.075	0.073	0.096	0.092	0.019	0.019
Panel D: Activists						
Finishing school$_{it}$	0.053***	0.048***	0.043***	0.040***	0.011**	0.011**
	(0.018)	(0.018)	(0.015)	(0.015)	(0.005)	(0.005)
Mean, untreated	0.016	0.016	0.018	0.017	0.005	0.005
Panel E: Nobility						
Finishing school$_{it}$	-0.013	-0.020	-0.007	-0.019	-0.002	-0.005
	(0.017)	(0.016)	(0.018)	(0.014)	(0.009)	(0.008)
Mean, untreated	0.038	0.039	0.050	0.050	0.018	0.018
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
# Enlightenment journals and articles		Yes		Yes		Yes
Observations	9,240	9,226	9,240	9,226	9,240	9,226

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, except for Activists, where we use the number of male politicians. We regress the number of non-noble secular women, teachers and writers, and noble women born in a city, as defined in the top row, on our finishing school variable. In all columns 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 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. Significance levels are $* \ p < 0.10$, $** \ p < 0.05$, $*** \ p < 0.01$

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.3), we conclude that economic demand fueled by the Early Industrial Revolution is unlikely to affect our interpretation.

Table D.3: 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$_{it}$	0.095**	0.185***	0.279***	0.246***	0.016**	0.023***
	(0.041)	(0.044)	(0.100)	(0.057)	(0.007)	(0.007)
Mean, untreated	0.147	0.148	0.137	0.138	0.019	0.018
Panel B: Unmarried women						
Finishing school$_{it}$	0.050	0.180***	0.231**	0.217***	0.004	0.018***
	(0.046)	(0.044)	(0.092)	(0.053)	(0.010)	(0.007)
Mean, untreated	0.148	0.152	0.137	0.141	0.022	0.022
Panel C: Teachers & Writers						
Finishing school$_{it}$	0.095**	0.129***	0.166**	0.124***	0.011	0.022***
	(0.041)	(0.032)	(0.081)	(0.032)	(0.008)	(0.007)
Mean, untreated	0.074	0.074	0.058	0.059	0.019	0.019
Panel D: Activists						
Finishing school$_{it}$	0.053*	0.066***	0.070	0.051***	0.004	0.014**
	(0.029)	(0.022)	(0.043)	(0.018)	(0.004)	(0.006)
Mean, untreated	0.018	0.016	0.013	0.012	0.006	0.005
Panel E: Nobility						
Finishing school$_{it}$	-0.022	-0.014	0.004	-0.012	-0.002	-0.003
	(0.039)	(0.019)	(0.035)	(0.016)	(0.018)	(0.009)
Mean, untreated	0.031	0.037	0.024	0.030	0.015	0.017
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, except for Activists, where we use the number of male politicians. We regress the number of non-noble secular women, teachers and writers, and noble women born in a city, as defined in the top row, on our finishing school variable. In all columns 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 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. Significance levels are $*$ $p < 0.10$, $**$ $p < 0.05$, $***$ $p < 0.01$

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 constant. The results in Figure E.1 reveal that our results are clear outliers in this distribution, with the largest fraction of absolute values greater than our estimate at a mere 0.02 (for the results on Activists).

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$_{it}$	0.230***	0.164***	0.355***	0.204***	0.019***	0.021***
	(0.026)	(0.028)	(0.033)	(0.030)	(0.004)	(0.004)
Mean, untreated	0.150	0.149	0.140	0.139	0.018	0.018
Panel B: Unmarried women						
Finishing school$_{it}$	0.194***	0.147***	0.302***	0.173***	0.011**	0.014***
	(0.028)	(0.029)	(0.033)	(0.029)	(0.004)	(0.005)
Mean, untreated	0.155	0.153	0.144	0.143	0.022	0.022
Panel C: Teachers & Writers						
Finishing school$_{it}$	0.151***	0.104***	0.174***	0.103***	0.019***	0.017***
	(0.019)	(0.020)	(0.018)	(0.020)	(0.005)	(0.005)
Mean, untreated	0.076	0.075	0.060	0.059	0.019	0.019
Panel D: Activists						
Finishing school$_{it}$	0.076***	0.053***	0.064***	0.043***	0.013***	0.011***
	(0.015)	(0.014)	(0.012)	(0.010)	(0.004)	(0.004)
Mean, untreated	0.016	0.016	0.012	0.012	0.005	0.005
Panel E: Nobility						
Finishing school$_{it}$	-0.018	-0.013	-0.009	-0.007	-0.002	-0.002
	(0.014)	(0.015)	(0.012)	(0.013)	(0.006)	(0.007)
Mean, untreated	0.039	0.038	0.031	0.031	0.018	0.018
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	9,312	9,240	9,312	9,240	9,312	9,240

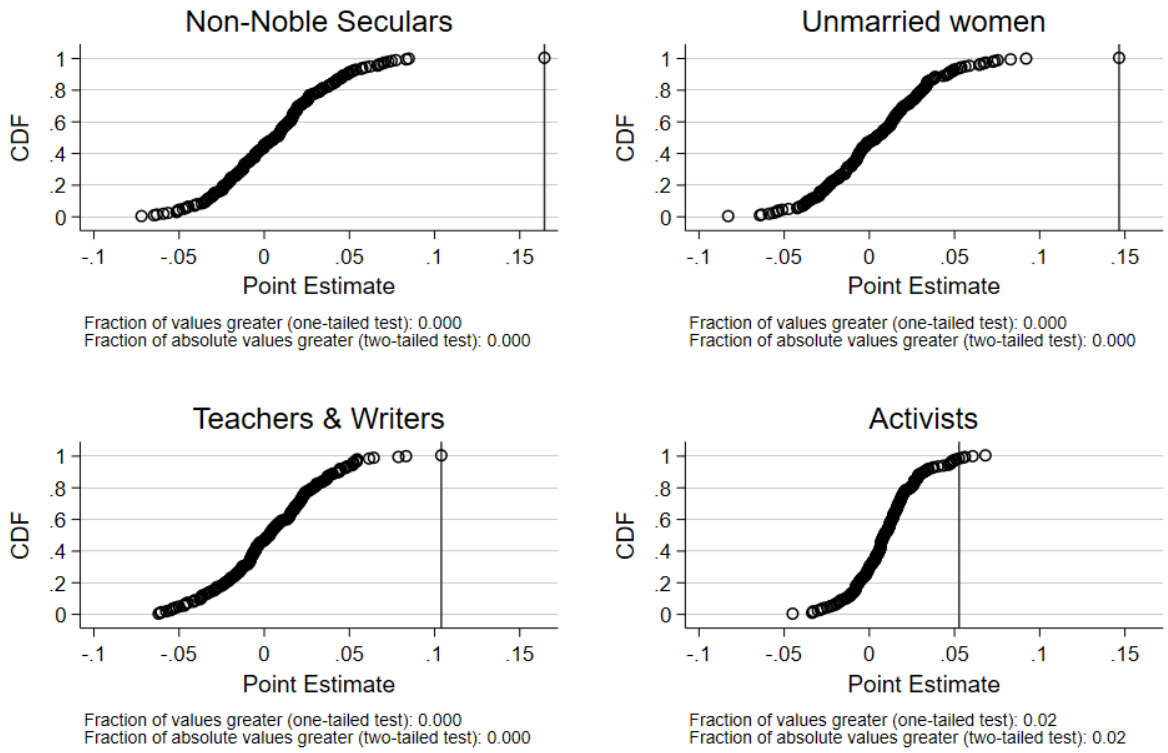
Main results using a fixed-effects estimation and all cities in all periods, with standard errors corrected for spatial dependence within 100km as in Hsiang et al. (2013). We consider three types of dependent variables to capture the extensive and intensive margin of the birth of notable women. $\mathbb{I}[Women > 0]$ 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, except for Activists, where we use the number of male politicians. We regress the number of non-noble secular women, teachers and writers, and noble women born in a city, as defined in the top row, on our finishing school variable. 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. 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. Significance levels are $* \ p < 0.10$, $** \ p < 0.05$, $*** \ p < 0.01$

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$_{it}$	0.164***	0.167***	0.204***	0.156***	0.018**	0.021***
	(0.033)	(0.047)	(0.045)	(0.055)	(0.007)	(0.005)
Mean, untreated	0.149	0.164	0.139	0.158	0.017	0.016
Panel B: Unmarried women						
Finishing school$_{it}$	0.147***	0.140***	0.173***	0.122**	0.011	0.014**
	(0.034)	(0.049)	(0.043)	(0.056)	(0.008)	(0.006)
Mean, untreated	0.153	0.180	0.143	0.176	0.021	0.021
Panel C: Teachers & Writers						
Finishing school$_{it}$	0.104***	0.108**	0.103***	0.090**	0.023**	0.017***
	(0.026)	(0.044)	(0.029)	(0.043)	(0.009)	(0.006)
Mean, untreated	0.075	0.085	0.059	0.066	0.017	0.017
Panel D: Activists						
Finishing school$_{it}$	0.053***	0.039	0.043***	0.026	0.012*	0.011**
	(0.018)	(0.026)	(0.015)	(0.019)	(0.006)	(0.005)
Mean, untreated	0.016	0.011	0.012	0.008	0.003	0.003
Panel E: Nobility						
Finishing school$_{it}$	-0.013	-0.010	-0.007	0.007	0.001	-0.002
	(0.017)	(0.028)	(0.018)	(0.034)	(0.015)	(0.009)
Mean, untreated	0.038	0.068	0.031	0.056	0.029	0.029
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
Non-Spillover sample		Yes		Yes		Yes
Observations	9,240	3,696	9,240	3,696	3,696	9,240

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, except for Activists, where we use the number of male politicians. We regress the number of non-noble secular women, teachers and writers, and noble women born in a city, as defined in the top row, on our finishing school variable. 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. 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. Significance levels are $* \ p < 0.10$, $** \ p < 0.05$, $*** \ p < 0.01$

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 2 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 3) and by treatment group (Figure G.2). Second, implement the methods by de Chaisemartin and D'Haultfœuille (2020) to assess whether heterogeneous treatment effects, 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 0.284 (s.e. 0.054) for the method of Callaway and Sant'Anna (2021) and 0.146 (s.e. 0.052) using the method by de Chaisemartin and D'Haultfœuille (2020). These estimates are very close to our baseline ATT in Figure 3 (0.146, s.e. 0.049).

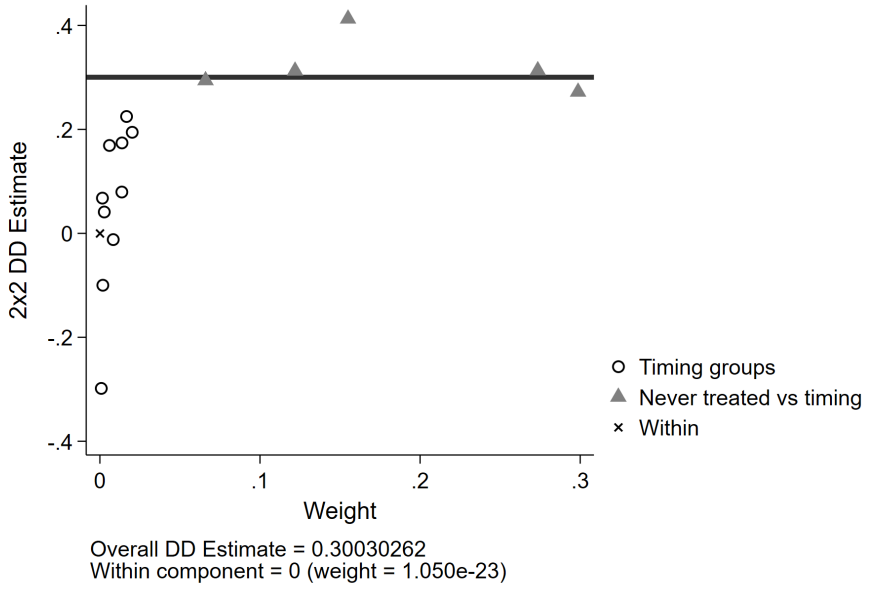
Another way to assess the validity of our approach is by estimating the implied weight of each treatment period. In a classical event study design where one focuses on cities that ever establish treatment, late treatment cities are the implied control cities for early treatment cities. (Goodman-Bacon, 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 3, we show parallel trends in the set of cities that ever established finishing schools. In Figure F.2, 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

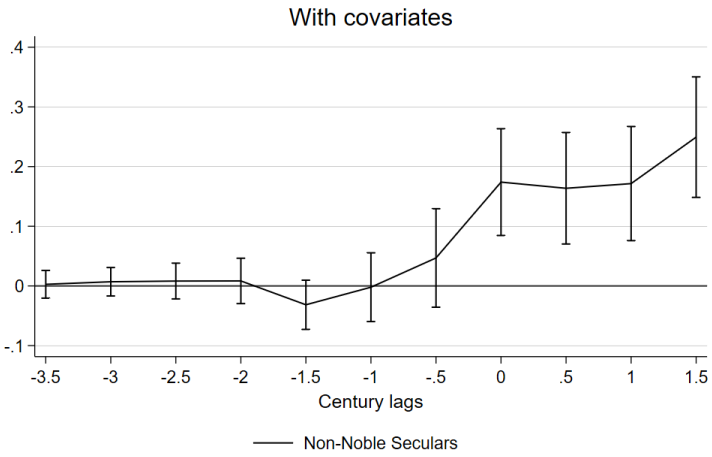
	$\mathbb{I}[Women > 0]$		log Women		Share Women	
	Weight	Av. DID Est.	Weight	Av. DID Est.	Weight	Av. DID Est.
Earlier Treatment vs. Later Control	0.071	0.160	0.071	0.227	0.071	0.015
Later Treatment vs. Earlier Control	0.013	0.028	0.013	-0.171	0.013	0.007
Treatment vs Never treated	0.915	0.315	0.915	0.492	0.915	0.023
Differences-in-differences estimate:		0.300		0.464		0.022

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

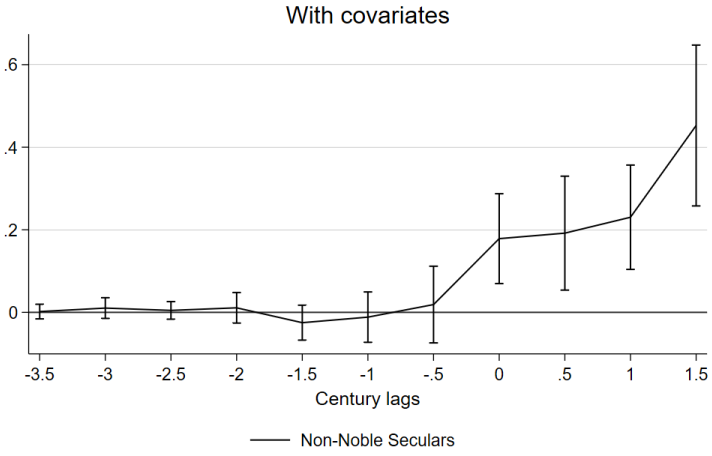


Showing the implied weights against the treatment effect when using the indicator $\mathbb{I}[Women > 0]$. The treatment effect is amost entirely estimated from the comparison of treated cities to non-treated cities.

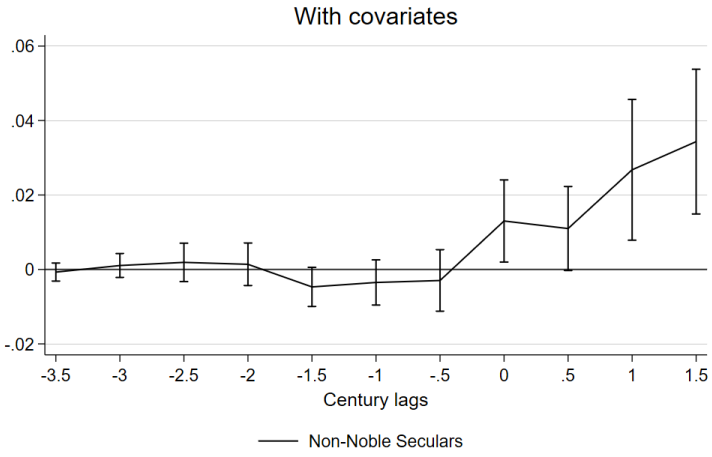
Figure F.2: Event-Study: Impact of finishing school establishment on notable women



(a) Indicator funtion: Notable woman born in citv



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



(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 3 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

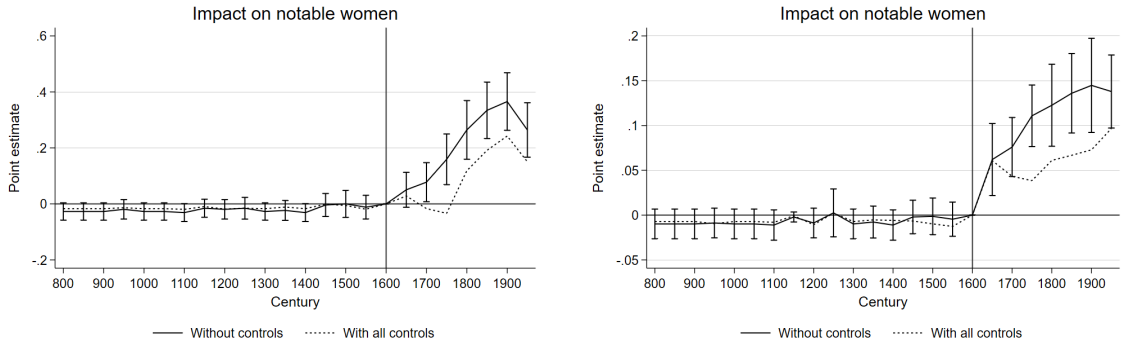
We start by splitting our sample into cities that established finishing schools by 1850 and cities which did not and compare women's representation among the human capital elite in these two sets of cities before and after 1650, the period in which the first finishing school was founded. While this strategy 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). Point estimates are reported in Table G.1 for both margins. First, the point estimates are very similar to the baseline results reported in Table 2 and are stable across specifications. Second, the point estimates on the intensive and extensive margin do not differ in most cases.

We continue and analyze the pre-trends for each treatment period separately in Figure G.2. Again, we see no differential pre-trend in any pre-treatment period and significant impacts of schools only after the schools have been established. The results are somewhat stronger for the first and last schools, yet reveal no differential DID-estimate in Table G.2. 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 Figure 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 temporal 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

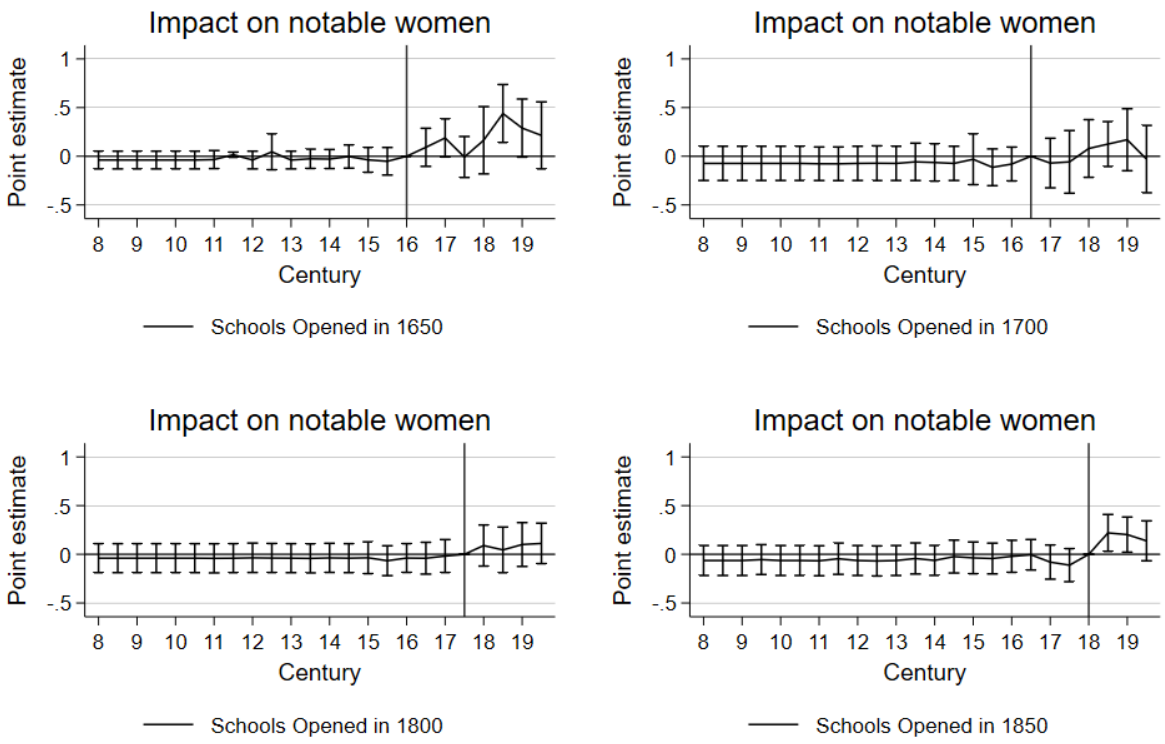
These graphs split the data into cities that ever establish at least one finishing school and those without and compare those before and after 1650. The outcome is an indicator equal to one if a notable woman was born in a given city and period. The left Panel reports the point estimates from the interaction 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)
Panel A: Non-Noble Seculars						
Finishing school × Post 1650	0.182***	0.073***	0.264***	0.101***	0.010***	0.005*
	(0.024)	(0.022)	(0.041)	(0.030)	(0.003)	(0.003)
# Finishing schools × Post 1650	0.103***	0.063***	0.192***	0.148***	0.006***	0.004***
	(0.009)	(0.011)	(0.025)	(0.031)	(0.001)	(0.001)
Panel B: Unmarried women						
Finishing school × Post 1650	0.131***	0.044*	0.219***	0.079**	0.001	-0.001
	(0.024)	(0.025)	(0.038)	(0.032)	(0.004)	(0.004)
# Finishing schools × Post 1650	0.069***	0.033***	0.164***	0.126***	0.000	-0.001
	(0.013)	(0.012)	(0.023)	(0.028)	(0.001)	(0.002)
Panel C: Teachers & Writers						
Finishing school × Post 1650	0.113***	0.043***	0.122***	0.046***	0.014***	0.007*
	(0.018)	(0.016)	(0.022)	(0.016)	(0.003)	(0.004)
# Finishing schools × Post 1650	0.064***	0.039***	0.092***	0.073***	0.007***	0.004*
	(0.010)	(0.011)	(0.018)	(0.022)	(0.002)	(0.002)
Panel D: Activists						
Finishing school × Post 1650	0.036***	0.017*	0.032***	0.015*	0.004*	0.001
	(0.009)	(0.010)	(0.009)	(0.008)	(0.002)	(0.002)
# Finishing schools × Post 1650	0.027***	0.021***	0.028**	0.025**	0.002*	0.000
	(0.007)	(0.007)	(0.011)	(0.012)	(0.001)	(0.001)
Panel E: Nobility						
Finishing school × Post 1650	-0.012	-0.020	-0.000	-0.006	-0.002	-0.007
	(0.016)	(0.017)	(0.014)	(0.015)	(0.007)	(0.008)
# Finishing schools × Post 1650	0.004	0.003	0.015*	0.016*	0.006	0.005
	(0.008)	(0.009)	(0.009)	(0.009)	(0.004)	(0.005)
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	9,312	9,240	9,312	9,240	9,312	9,240

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, except for Activists, where we use the number of male politicians. We regress the number of non-noble secular women, teachers and writers, and noble women born in a city, as defined in the top row, on our finishing school variable. 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 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: confessional battle in the vicinity. 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 to capture differential educational preferences. All covariates are interacted with period fixed effects. Standard errors clustered by city shown in parentheses. $* \ p < 0.10, ** \ p < 0.05, *** \ p < 0.01$

Figure G.2: Parallel Trends Analysis: Lead-Lag figure by treatment cohort.



Each figure represents the lead-lag graph for the indicated treatment group relative to the never-treated control group. The outcome is an indicator equal to one if a notable woman was born in a given city and period. No controls included. 95% confidence intervals reported.

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)
Panel A: Non-Noble Seculars						
Finishing school by 1650 × post 1650	0.294***	0.190***	0.415***	0.228**	0.023***	0.021**
	(0.060)	(0.058)	(0.114)	(0.105)	(0.008)	(0.008)
Finishing school by 1700 × post 1700	0.248***	0.138**	0.350***	0.180**	0.014***	0.014**
	(0.076)	(0.061)	(0.105)	(0.084)	(0.005)	(0.006)
Finishing school by 1750 × post 1750	0.159*	0.069	0.855*	0.699*	0.025*	0.024
	(0.083)	(0.072)	(0.437)	(0.366)	(0.015)	(0.017)
Finishing school by 1800 × post 1800	0.195***	0.134**	0.347***	0.190**	0.015**	0.019**
	(0.047)	(0.052)	(0.099)	(0.087)	(0.007)	(0.008)
Finishing school by 1850 × post 1850	0.249***	0.203***	0.248***	0.137*	0.023***	0.023***
	(0.050)	(0.052)	(0.067)	(0.074)	(0.009)	(0.009)
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	9,312	9,240	9,312	9,240	9,312	9,240

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, as defined in the top row, on our finishing school variable. 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 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: confessional battle in the vicinity. 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 to capture differential educational preferences. All covariates are interacted with period fixed effects. Standard errors clustered by city shown in parentheses. $* \ p < 0.10$, $** \ p < 0.05$, $*** \ p < 0.01$

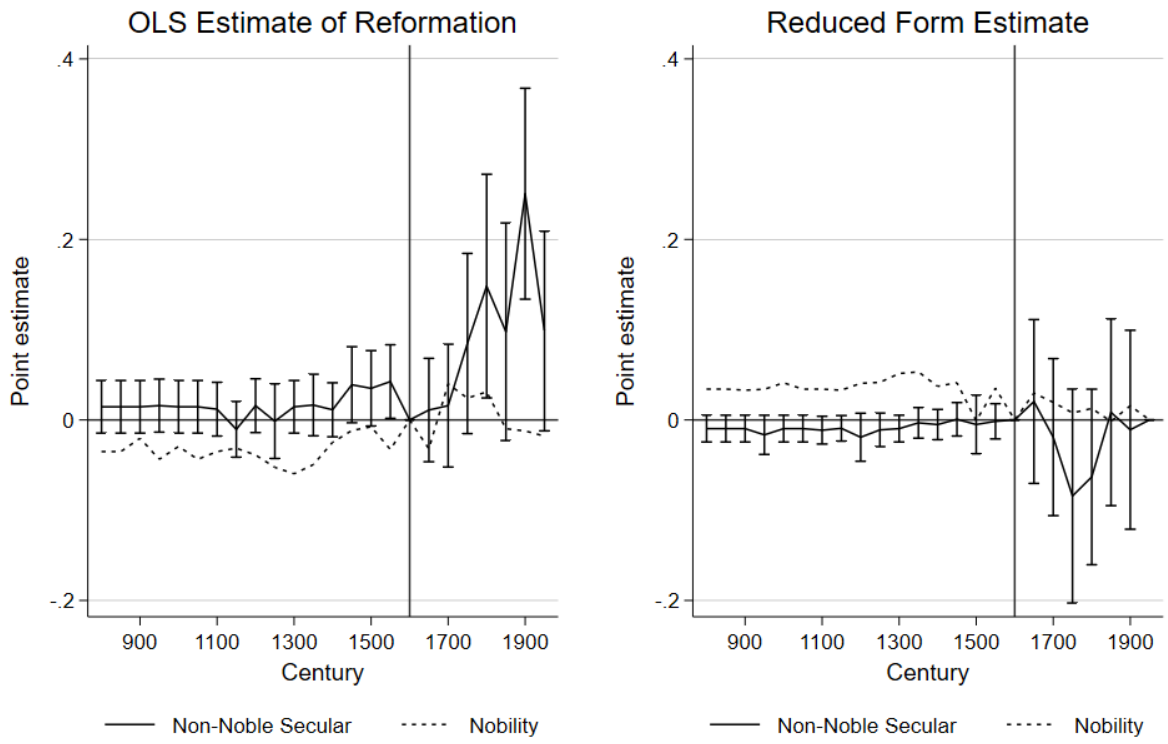
G.2 Protestantism as a confounding factor

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.3. 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.3 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.[43]

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

Figure G.3: 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

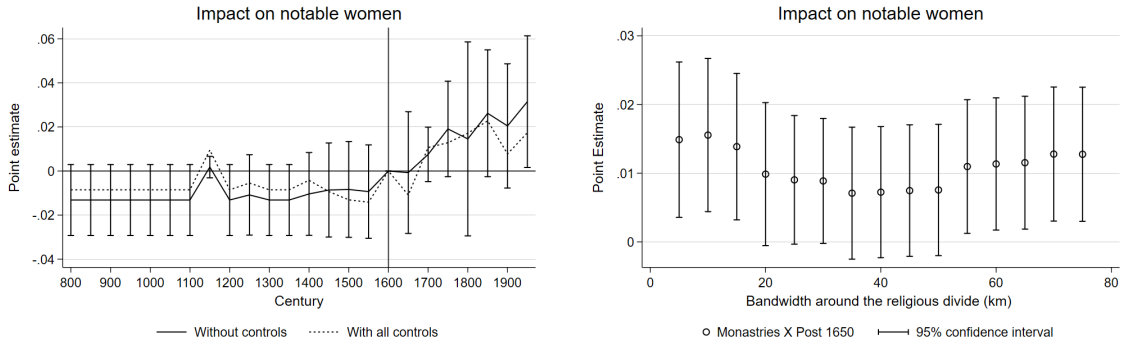
	$\mathbb{I}[Women > 0]$		log Women		Share Women	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Non-Noble Seculars						
Reformation in City × post 1600	0.056**		0.068*		0.003	
	(0.023)		(0.035)		(0.003)	
log Distance to Wittenberg × post 1600		-0.041*		-0.046		-0.003
		(0.022)		(0.039)		(0.003)
Panel B: Unmarried women						
Reformation in City × post 1600	0.083***		0.088**		0.003	
	(0.027)		(0.037)		(0.004)	
log Distance to Wittenberg × post 1600		-0.009		-0.023		-0.001
		(0.028)		(0.040)		(0.003)
Panel C: Teachers & Writers						
Reformation in City × post 1600	0.030*		0.028		0.004	
	(0.018)		(0.018)		(0.004)	
log Distance to Wittenberg × post 1600		-0.032		-0.031		-0.004
		(0.022)		(0.024)		(0.003)
Panel D: Activists						
Reformation in City × post 1600	0.014		0.010		0.001	
	(0.010)		(0.007)		(0.003)	
log Distance to Wittenberg × post 1600		-0.005		-0.005		-0.000
		(0.008)		(0.008)		(0.002)
Panel E: Nobility						
Reformation in City × post 1600	0.026		0.026		0.012	
	(0.022)		(0.019)		(0.010)	
log Distance to Wittenberg × post 1600		0.017		0.010		0.005
		(0.012)		(0.010)		(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		16.227		16.227		16.227

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, except for Activists, where we use the number of male politicians. We regress the number of non-noble secular women, teachers and writers, and women from the nobility born in a city, as defined in the top row, on our finishing school variable. 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 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: confessional battle in the vicinity. 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 to capture differential educational preferences. All covariates are interacted with period fixed effects. Standard errors clustered by city shown in parentheses. $* \ p < 0.10$, $** \ p < 0.05$, $*** \ p < 0.01$

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 an important 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.4 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.4: 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 Accumulation and role-model hypothesis

In this Appendix, we discuss whether finishing schools served as a pull factor motivating women from the human capital elite to migrate into a city. In contrast to the rest of the paper, where we link notable women to cities based on their place of *birth*, for this exercise, we leverage information on notable women's place of *death* to measure whether finishing schools attracted notable women from elsewhere. We thus investigate whether finishing schools contributed to a local accumulation of notable women, potentially via the mechanism that local notable women served as role-models in attracting others. In Table H.1 we show that upon the establishment of the first finishing school in a city, more women from the human capital elite born in other cities moved to the city with the newly established finishing school. It is important to note that our rich data on notable women's places of birth and places of death allow us to distinguish the in-migration of notable women born elsewhere from spillover effects, which we discuss in Appendix E of this appendix. Our data also allow us to document that finishing schools attracted the in-migration of women from the human capital elite to these cities while ruling out that finishing schools were established in response to the in-migration of women from the human capital elite as evidenced by the clear absence of differential pre-trends in Figure 7 in the main text.

A further concern is that most of the positive effect of finishing schools on the in-migration of women from the human capital elite might be mechanical since finishing schools were primary employers for notable women. We test for this in the second Panel of Table H.1: we find that once we add our control variables and thus adequately control for initial differences between cities, we see no significant effect of finishing schools on the number of notable teachers who migrated to a city with a finishing school. This suggests that a potential mechanical effect for teachers alone cannot account for the main effect shown in the first Panel of Table H.1.

Taken together, the evidence presented in this Appendix suggests that finishing schools indeed served as a pull factor which attracted notable women born elsewhere.

Table H.1: Testing role-model and accumulation hypotheses

	$\mathbb{I}[Women > 0]$		log Women	
	(1)	(2)	(3)	(4)
Panel A: Immigration of Non-Noble Seculars				
Finishing school$_{it}$	0.114***	0.059**	0.134***	0.049*
	(0.023)	(0.024)	(0.033)	(0.028)
Mean, untreated	0.042	0.042	0.034	0.034
Panel B: Immigration of Teachers & Writers				
Finishing school$_{it}$	0.049***	0.016	0.052**	0.015
	(0.018)	(0.019)	(0.022)	(0.019)
Mean, untreated	0.020	0.020	0.016	0.016
Unit trend	Yes	Yes	Yes	Yes
City covariates $\times$ period FE		Yes		Yes
Religious covariates $\times$ period FE		Yes		Yes
Educational covariates $\times$ period FE		Yes		Yes
Observations	9,312	9,240	9,312	9,240

Results using a fixed-effects estimation and all cities in all periods. We consider two 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 who migrated to this city in this period. 'log Women' constitutes the natural logarithm of the number of migrated women plus one. We regress the number of non-noble secular women, and teachers and writers born in a city, as defined in the top row, on our finishing school variable. 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. 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. Standard errors clustered by city shown in parentheses. $* \ p < 0.10$, $** \ p < 0.05$, $*** \ p < 0.01$

I 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 as an important 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 I.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 I.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 I.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 I.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: Leserbriefe, Frauenzeitung, 1849–1852						
Finishing schools	0.249**	0.274**	0.297**	0.412***	0.492***	0.444**
	(0.098)	(0.108)	(0.121)	(0.158)	(0.187)	(0.192)
Mean, untreated	0.038	0.038	0.038	0.061	0.061	0.061
Panel B: All women's rights organizations						
Finishing schools	0.378*	0.378	0.258	2.868*	2.844	2.308
	(0.223)	(0.241)	(0.219)	(1.680)	(1.835)	(1.678)
Mean, untreated	0.275	0.275	0.275	155.802	155.802	155.802
Panel C: Women's rights organizations to promote equal access to education						
Finishing schools	0.333**	0.340*	0.393**	2.099**	2.123**	2.504**
	(0.159)	(0.178)	(0.178)	(0.851)	(0.940)	(0.966)
Mean, untreated	0.038	0.038	0.038	13.023	13.023	13.023
Panel D: Member Parliament, 1919–1933						
Finishing schools	0.164*	0.122	0.137	0.227**	0.193**	0.226**
	(0.093)	(0.090)	(0.104)	(0.090)	(0.093)	(0.093)
Mean, untreated	0.038	0.038	0.038	0.053	0.053	0.053
Panel E: Member Parliament, 1949–2019						
Finishing schools	0.237	0.236	0.179	0.471**	0.480*	0.524*
	(0.174)	(0.189)	(0.192)	(0.223)	(0.247)	(0.269)
Mean, untreated	0.527	0.527	0.527	1.031	1.031	1.031
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	8.906	7.177	8.435	8.906	7.177	8.435

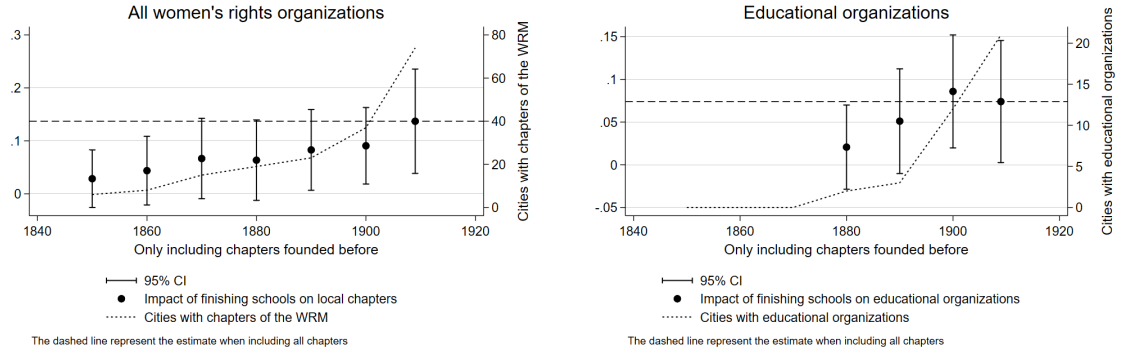
Results using a two-stage least-squares estimation (2SLS) in all columns, changing the monastery date to 1300 (Columns (1) and (4)), 1500 (Columns (2) and (5)) and 1648 (Columns (3) and (6)). We instrument the number of finishing schools in 1850 in city c with the number of monasteries in city c, comparing 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 and number of letters written from city c to the first active women's only magazine in Germany. In Panel B 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 C we limit the dependent variable from Panel B to only include women's rights organizations in city c that are explicitly dedicated to promoting equal access to education. 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 Table 2 columns (2), (4), and (6) in all regressions. Standard errors clustered by city shown in parentheses. $* \ p < 0.10$, $** \ p < 0.05$, $*** \ p < 0.01$

Table I.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: 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 B: All women's rights organizations								
Years since first opening	0.002***	0.002***			0.015***	0.015***		
	(0.000)	(0.001)			(0.003)	(0.005)		
log Years since first opening			0.072***	0.096***			0.535***	0.634***
			(0.015)	(0.023)			(0.098)	(0.148)
Mean, untreated	0.366	0.275	0.366	0.275	447.696	155.802	447.696	155.802
Panel C: Women's rights organizations to promote equal access to education								
Years since first opening	0.001*	0.000			0.005*	0.003		
	(0.001)	(0.001)			(0.003)	(0.004)		
log Years since first opening			0.029**	0.017			0.169***	0.100
			(0.011)	(0.015)			(0.062)	(0.081)
Mean, untreated	0.047	0.038	0.047	0.038	13.074	13.023	13.074	13.023
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
Panel E: Member Parliament, 1949–2019								
Years since first opening	0.001	0.001*			0.002*	0.004**		
	(0.001)	(0.001)			(0.001)	(0.002)		
log Years since first opening			0.044***	0.063***			0.095***	0.137***
			(0.015)	(0.022)			(0.025)	(0.036)
Mean, untreated	0.556	0.527	0.556	0.527	1.163	1.031	1.163	1.031
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 and number of letters written from city c to the first active women's only magazine in Germany. In Panel B 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 C we limit the dependent variable from Panel B to only include women's rights organizations in city c that are explicitly dedicated to promoting equal access to education. 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 Table 2 in all regressions. Standard errors clustered by city shown in parentheses. $* \ p < 0.10$, $** \ p < 0.05$, $*** \ p < 0.01$

Figure I.1: The impact of finishing schools on chapters of the Women's Rights Movement - Time varying effects



The left graph shows the impact of finishing schools on whether a local chapter of the women's rights movement was founded in a city by 1850, 1860, 1870, 1880, 1890, 1900, and 1909. The right graph shows the same impact on local chapters promoting equal access to education for women. Only 27% of cities without finishing schools report having a women's rights organization by 1909, compared to 65% of cities with finishing schools. The numbers for educational organizations are 3.8% and 26% respectively. All controls included and sample reduced to within 10km of the religious divide.

I.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 I.4, column 6).

Table I.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: Leserbriefe, Frauenzeitung, 1849–1852						
Finishing schools	0.095***	0.122***	0.144***	0.183***	0.241**	0.209***
	(0.020)	(0.037)	(0.018)	(0.055)	(0.097)	(0.076)
Panel B: All women's rights organizations						
Finishing schools	0.064**	0.137***	-0.003	0.800***	1.157***	0.532***
	(0.027)	(0.050)	(0.023)	(0.160)	(0.306)	(0.194)
Panel C: Women's rights organizations to promote equal access to education						
Finishing schools	0.083***	0.074**	0.055*	0.549***	0.496**	0.510***
	(0.017)	(0.036)	(0.029)	(0.113)	(0.217)	(0.194)
Panel D: Member Parliament, 1919–1933						
Finishing schools	0.067***	0.101***	0.042	0.100***	0.105***	0.107**
	(0.018)	(0.034)	(0.027)	(0.029)	(0.035)	(0.049)
Panel E: Member Parliament, 1949–2019						
Finishing schools	0.060**	0.091*	0.012	0.246***	0.268***	0.280***
	(0.024)	(0.047)	(0.025)	(0.040)	(0.071)	(0.055)
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	318	385	183	318
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 and number of letters written from city c to the first active women's only magazine in Germany. In Panel B 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 C we limit the dependent variable from Panel B to only include women's rights organizations in city c that are explicitly dedicated to promoting equal access to education. 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 Table 2 in all regressions. Standard errors clustered by city shown in parentheses. $* \ p < 0.10$, $** \ p < 0.05$, $*** \ p < 0.01$

Table I.4: Balance in the matched Sample

	Unmatched sample			Matched sample		
	β	s.e.	p-value	β	s.e.	p-value
log(Distance Wittenberg)	-0.080	0.040	0.046	0.084	0.079	0.291
log(Distance religious divide)	0.214	0.065	0.001	0.029	0.098	0.769
log(Population in 1650)	0.422	0.058	0.000	0.047	0.034	0.167
Temperature in Spring 1650	0.011	0.041	0.783	0.011	0.068	0.871
Temperature in Summer 1650	0.079	0.048	0.097	0.010	0.075	0.892
Temperature in Fall 1650	-0.002	0.036	0.947	-0.022	0.052	0.668
Temperature in Winter 1650	-0.119	0.048	0.014	-0.078	0.065	0.227
Hanse city	0.044	0.020	0.031	-0.016	0.039	0.689
Bishop seat	0.036	0.017	0.033	-0.030	0.022	0.184
Jewish settlement	0.081	0.025	0.001	0.021	0.039	0.598
Progrom	0.044	0.023	0.057	0.036	0.039	0.350
Battle during 30-years war	0.062	0.021	0.003	-0.049	0.049	0.314
Boy school in 1605	0.018	0.017	0.279	0.036	0.030	0.221
University in 1650	0.005	0.008	0.557	-0.004	0.011	0.701
Catholic region	0.012	0.023	0.597	0.014	0.042	0.746

This table presents the balance test on covariates in 1650, based on the regression $X_c = \alpha + \beta \cdot \text{Schools}_{c,1850} + \varepsilon_c$. The unmatches sample contains all cities in 1650, whereas the matched sample selects a 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 1650, these differences disappear when matching cities to their nearest neighbor.

J Impact of notable women in 1850 on local political activity

In this Appendix, we directly ask what is the correlation between an additional non-noble secular women in 1850 and subsequent political activity in the next 100 years. To this end, we estimate the following equation in Table J.1:

$$Y_c = \alpha + \beta \cdot \log(\text{Number Non-Noble Seculars+1})_{c,1850} + \gamma \boldsymbol{X}_c + \varepsilon_c \tag{2}$$

Recognizing the endogeneity concerns associated with this equation, we nevertheless present estimates for their interpretability: a 10% increase in the number of notable women in a city is associated with a 2% increase in correspondence (Panel A), a 15% increase in women's rights associations (Panel B&C), and a 2% (4.6%) increase in the number of female members of parliament during the Weimar Republic (Federal Republic).

We conduct two exercises to judge the reliabilty of these correlations. First, we present point estimates with (odd columns) and without (even columns) controls, limited to 10 km of the religious boundary. The estimates remain stable throughout all specifications. Second, we instrument the number of notable women by the number of fexisting monastries in 1300 and provide the 2SLS coefficient, the p-value and F-statistic below the OLS estimates. However, as the exclusion restriction, monastries only affect political outcomes through their impact on finishing schools' impact on notable women, is likely to 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 14: a 10% increase in the number of notable women in each city is associated with a 8% increase in correspondence (Panel A), a 40% increase in women's rights associations for education (Panel C), and a 4% increase in the number of female members of parliament during the Weimar Republic (Panel D).[44]

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 1850, these estimates represent an informative correlation.

[44] A similar exercise using finishing schools as an instrument can be conducted. It yields qualitatively similar results with a stronger first stage of 22.

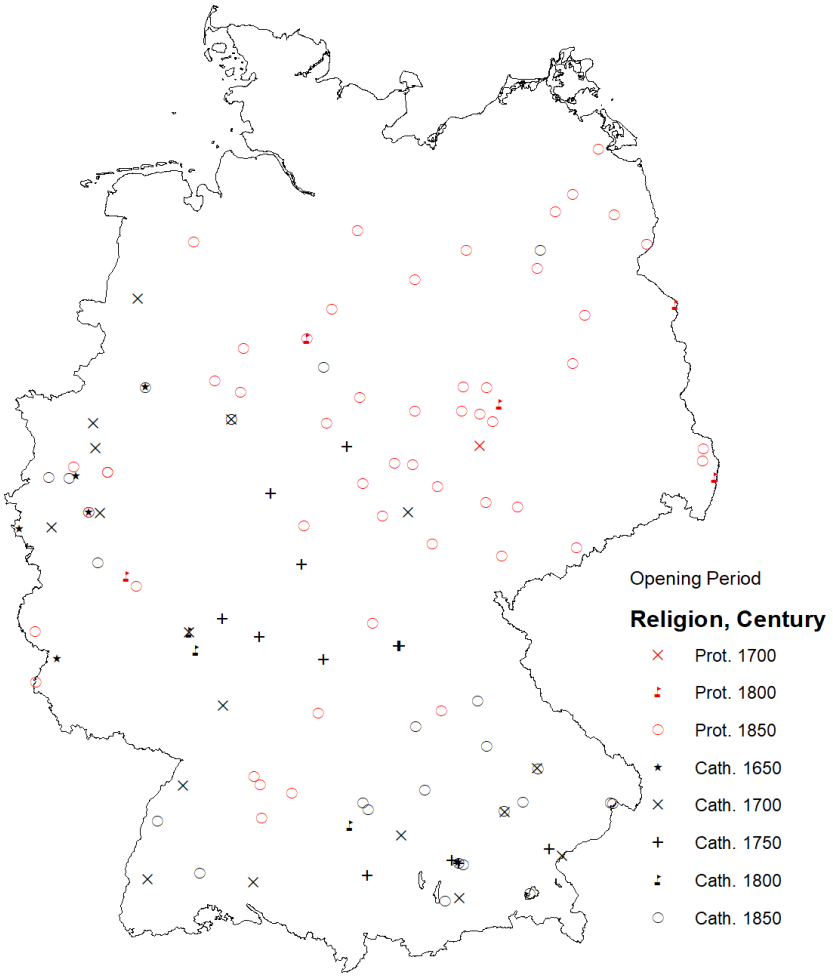
Table J.1: Impact of notable women in 1850 on political activity of the Women's Rights Movement

	$\mathbb{I}[>0]$		log Number	
	(1)	(2)	(3)	(4)
Panel A: Leserbriefe, Frauenzeitung, 1849–1852				
log(Number Non-Noble Seculars)	0.221***	0.190***	0.381***	0.227***
	(0.028)	(0.058)	(0.078)	(0.076)
Implied 2SLS coefficient	0.246	0.483	0.285	0.800
P-value 2SLS coefficient	0.005	0.013	0.038	0.018
First stage F-statistic	29.640	14.916	29.640	14.916
Panel B: All women's rights organizations				
log(Number Non-Noble Seculars)	0.262***	0.133*	2.598***	1.511***
	(0.030)	(0.076)	(0.178)	(0.488)
Implied 2SLS coefficient	0.695	0.734	5.737	5.563
P-value 2SLS coefficient	0.000	0.074	0.000	0.052
First stage F-statistic	29.640	14.916	29.640	14.916
Panel C: Women's rights organizations to promote equal access to education				
log(Number Non-Noble Seculars)	0.300***	0.263***	1.813***	1.516***
	(0.025)	(0.055)	(0.144)	(0.303)
Implied 2SLS coefficient	0.531	0.646	2.840	4.073
P-value 2SLS coefficient	0.000	0.000	0.000	0.000
First stage F-statistic	29.640	14.916	29.640	14.916
Panel D: Member Parliament, 1919–1933				
log(Number Non-Noble Seculars)	0.206***	0.204***	0.257***	0.198***
	(0.030)	(0.050)	(0.044)	(0.049)
Implied 2SLS coefficient	0.422	0.319	0.461	0.440
P-value 2SLS coefficient	0.000	0.036	0.000	0.004
First stage F-statistic	29.640	14.916	29.640	14.916
Panel E: Member Parliament, 1949–2019				
log(Number Non-Noble Seculars)	0.197***	0.199***	0.610***	0.469***
	(0.027)	(0.070)	(0.051)	(0.109)
Implied 2SLS coefficient	0.418	0.460	0.935	0.914
P-value 2SLS coefficient	0.000	0.120	0.000	0.005
First stage F-statistic	29.640	14.916	29.640	14.916
City Covariates		Yes		Yes
Religious covariates		Yes		Yes
Educational covariates		Yes		Yes
Observations	388	183	388	183
Bandwidth		10		10

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. We also report the point estimate, p-value and F-statistic from an 2SLS regression below the OLS coefficient for convenience. Here, we instrument the log number of notable women in city c with the number existing monastries in 1300, comparing cities within 10 km of the inner-German religious divide in odd columns to proxy religious competition and capture similar cities. In Panel A we estimate whether finishing schools increase the likelihood and number of letters written from city c to the first active women's only magazine in Germany. In Panel B 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 C we limit the dependent variable from Panel B to only include women's rights organizations in city c that are explicitly dedicated to promoting equal access to education. 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 Table 2 columns (2), (4), and (6) in all regressions. Standard errors clustered by city shown in parentheses. $* \ p < 0.10$, $** \ p < 0.05$, $*** \ p < 0.01$

K Additional history on finishing schools

Figure K.1: School opening years in Germany



This map shows the geographic distribution Catholic and Protestant schools in Germany. 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.5) or treatment periods (Table G.2) suggest no differential treatment effect along the timing dimension.