

# Education and Female Empowerment

Mathias Iwanowsky      Leonhard Vollmer      Johannes Wimmer \*

20th August 2019

## Abstract

A key instrument to empower oppressed groups is education. In the past however, technological and cultural changes enabled the extension of education to oppressed groups, which in turn enabled their empowerment. This paper breaks this co-dependence and asks whether education can empower one of the most oppressed groups today: Women. We construct a panel of cities and birth dates of notable women to show that the sudden appearance of catholic finishing schools in Germany, established in response to religious competition, had lasting impact on the emancipation of women. Reassuringly, we find no impact on men or economic development in general.

---

\*Corresponding author Mathias Iwanowsky. E-mail: mathias.iwanowsky@econ.lmu.de. Ludwig-Maximilians-Universität München. We thank Davide Cantoni, Fabian Waldinger, Joachim Winter, and many seminar participants at the LMU for constructive feedback. Leonhard Vollmer and Johannes Wimmer acknowledge funding through the International Doctoral Program 'Evidence-Based Economics' of the Elite Network of Bavaria (Vollmer & Wimmer).

*“One child, one teacher, one book, one pen  
can change the world.”*

— Malala Yousafzai, *Pakistani activist for  
female education and Nobel Laureate.*

## 1 Introduction

Free and unrestricted access to education is important for an individuals’ human capital accumulation (Schultz, 1961; Burgess, 2016; Albisetti, 1982), health (Breierova and Duflo, 2004; Duflo, 2012; Kabeer, 2005; Aaronson et al., 2014), and political participation (Panday, 2008; Ballington et al., 2005; Welch, 1977). In many oppressive regimes, however, access to education is restricted to an elite that ensures dominance over the interpretation of cultural rules and norms (G. Johnson, 2000). Thus, to achieve equal rights and empowerment, many policies revolve around increasing access to education for individuals from oppressed groups (e.g. Malala, SDG). In the long-run however, the evidence is unclear whether education alone can bring about social change as empowerment, economic development, and access to education co-evolved over time (Duflo, 2012).

It is thus difficult to assess the impact of education without confounding factors that jointly determine access to education and empowerment. The theoretical literature analyzing the historical strides towards gender equality has suggested two major determinants of access to education: Technological and cultural change (Galor and Weil, 1996; Kimura and Yasui, 2010; Diebolt and Perrin, 2013). While the former changes the relative wages across skills and genders, the latter increases educational attainment as a result of a society that aims to include oppressed groups (Beaman et al., 2012).<sup>1</sup> The absence of independent variation in schooling has been exacerbated by the lack of appropriate data. As the societal empowerment of women is a history of women fighting for and benefiting from access to education, comprehensive data on multiple generations of empowered women was lacking.

In this paper, we utilize a unique dataset covering the universe of notable women throughout German History to exploit variation in the sudden establishment of religious finishing schools for girls. In the aftermath of the Protestant reformation, the catholic orders of the Ursulines and Mary-Ward sisters set up finishing schools across Germany

---

<sup>1</sup>We utilize cultural change very broadly. It spans the notion of endogeneously changing norms of inclusion to the recent study by Beaman et al. (2012) that exploits reserved seat elections in India and showed that empowered women as role models erase the gender gap in educational attainment.

to protect young girls from the teachings of Protestantism. While these schools prioritized religious teachings, similar attention was dedicated to teaching mathematics and foreign languages, such that these schools constituted the only opportunity for women to acquire knowledge beyond reading and writing. As such, even if driven by ideology, these schools either helped women become critical members of society or reinforced their subservient position in society (Fink, 1992; Freire, 1972). The clear focus on religious teachings, however, implies that empowerment was not an explicitly stated goal. On the contrary, instead of responding to increasing returns to education due to technological change, the orders chose the locations in response to religious competition with Protestantism.

We exploit this sudden change in the supply of schools by constructing a balanced panel of 388 cities in Germany to which we join the birthplace and birthyear of all notable women from 800 A.D. until 1950. Using the timing of school openings to link finishing schools to the closest city in our panel, we address the potential selection of cities into establishing finishing school. The identification assumption of non-differential pre-trends is tested in an event-study design in which we estimate precise zero impacts prior to the establishment of schools. In our baseline we control for city and period fixed effects as well as linear time trends to capture all unobservable confounding factors that evolve linearly over time. We document a doubling of the probability that a historically notable woman is born in a city after a finishing school had been established.

Cities that differ economically, culturally or confessionally, also differ in the probability of establishing finishing schools. We thus interact a rich set of covariates with period fixed effects to isolate the effects of education. We capture economic differences by the presence of Jews in 1300, population, local weather conditions in 1600, and confessional battles during the 30 years war. We capture the impact of religion and religious competition by controlling for confession in 1618, bishopric seats, and distance to Wittenberg and the confessional divide of Germany. Finally, we capture educational preferences by the presence of male schools, universities, and the educational preferences of ruling houses in 1618. Combined, we flexibly control for each variable in every time-period separately and report a robust impact of education on the likelihood of notable women being born.

The main confounding hypotheses are that changing educational, cultural preferences or economic activity lead to the establishing of finishing schools. We address these hypotheses in placebo exercises. First, using the location and timing of male schools, constructed in response to increasing returns to education, we estimate a significant impact on the

probability of an notable man being born after construction of male schools. Here, the effect is composed of the effect from the changing returns to education and the impact of education itself. Thus, if changing returns to education are responsible for the increased number of notable women, we should see a significant impact of male schools on the likelihood of notable women being born. However, as neither finishing schools have an impact on men, nor male schools impact notable women, we conclude that returns to education are unlikely to be part of the estimated impact. Second, we proxy changes in culture that increase the visibility of women by using the end of witch trials in Germany. While women who spoke out were often accused of sorcery before, they could do so more freely after the threat of the stake was lifted and thus, even if recognizably remains stable across time, changing culture would increase the likelihood of appearing in our data. However, we again find no significant impact on the probability that a women is born in the city after witch trials ended. Third, we explore the impact of finishing schools on city-level construction activity and find no correlation with economic activity. In sum, these placebo exercises suggest that finishing schools indeed had a non-linear impact on notable women via education; aside from differential returns to education, culture, and economic activity.

Women used their education and became recognized as important for the cause of female rights movement that increased in momentum in the late 19th century. We exploit three measures of cross-sectional variation to proxy for local support of the women's cause in 1850, 1900 and the 20th century: Letters to the first female led newspaper in 1849–1852, recognition of female authors by women's rights activists in 1900, and the birthplace of female members of parliament in the Weimar Republic (1919-1933) and the Federal Republic (1949-2013). In this setting, unobservable factors that were previously captured by city fixed effects may prevent a direct interpretation if they are correlated with the establishment of finishing schools. We thus use an instrumental variable strategy that exploits two factors determining the location of finishing schools: Availability of space and religious competition. As the Ursuline and Mary-Ward sisters settled near previously existing monasteries, we use existing monasteries in 1300 as an instrument for the location of finishing schools between 1636–1850. We limit our sample to cities within 10 km of the religious divide in 1618 to capture religious competition that motivated the establishing of schools. Thus, a violation of the exclusion restriction would imply that monasteries in 1300 were strategically placed in areas that in 1618 were to become more religiously competitive between Protestantism and Catholicism, and thus have a separate impact on

the female rights movement.

Including the full set of controls, finishing schools increase the number of letters sent from a location to the female newspaper and the likelihood of a female author being born in that city. Using the two parliaments in German history, we find that a female member of any parliament is more likely to be born in a city with finishing schools and that finishing schools increase the share of female members of parliament being born, relative to men. Our 2SLS estimates largely confirm our OLS estimates and we thus conclude that the education provided by these finishing schools increased the probability of being a writer and activist for female rights, which had positive long-term consequences for their city of birth in terms of female activism and political participation.

We contribute to the literature by combining two strands that cover the individual impacts of education and the group level impact on empowerment. First, the literature has highlighted the prevalence of gender gaps in various settings (Blau and Kahn, 2016; Carlana, 2019; Sarsons, 2017), their historical persistence (Alesina et al., 2013; Jayachandran, 2015), and that increased educational attainment can be linked to reducing the gender gap (Bobbitt-Zeher, 2007) and political participation (Gleason, 2001; Panday, 2008). Utilizing exogenous variation, the impact of empowerment and education on health outcomes have been documented extensively (Qian, 2004; Madestam and Simeonova, 2019; Breierova and Duflo, 2004; Chou et al., 2010). In sum, while increased educational attainment increases a variety of development outcomes (Kabeer, 2005), Duflo (2012) argues that the interplay with economic development is crucial to bring about social change.

The second part of the literature on group empowerment, the literature has focus on explaining educational attainment as the consequence of technological progress (Galor and Weil, 1996; Kimura and Yasui, 2010; Diebolt and Perrin, 2013). Here, technological change affects the relative wages across skills and genders, promoting educational and occupational opportunities for women. However, these models cannot disentangle whether education has an independent effect or it is a reflection of cultural change manifesting itself in both an increased provision of education for women and female empowerment.

We contribute to these strands of the literature by exploiting an exogenous shift in the supply of girls education that is unrelated to the returns to education, culture or economic changes, and utilize data on the universe of all notable women to construct a panel of cities, schools, and birthplaces. Our results combine these strands by highlighting the long shadow cast by women attending these finishing schools on the female rights movement and modern-day political participation in Germany.

## 2 Empirical Strategy

Finishing schools are established in cities that differ along many dimensions. We isolate the impact of education on female empowerment by constructing a balanced panel of 388 cities and identify whether the timing of school opening correlates with women becoming notable for their achievements in arts, culture, or female empowerment. However, as production technologies change, increased returns to education increases the demand for education for men and women alike. Similarly, decreasing wars or natural catastrophes that disproportionately affect the male population increase the demand for female labor and thus the demand for female education. These local, often unobservable, factors increase adoption 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 education on oppressed groups. In this section, we discuss how our empirical strategy and data isolates the impact of education on empowerment from such confounding factors.

We approach this problem using data on the birthdates and -places of the universe of important German women in the *Neue Deutsche Biographie* (NDB). It is “considered the single most relevant biographic encyclopedia of the German language” and includes biographies detailing the professions and nobility status of women (Historische Kommission der Bayerischen Akademie der Wissenschaften, 2019). We restrict our sample to 2,363 non-royal and non-religious women who were born in the modern-day boundaries of Germany after 800 A.D. and use 261 royal women throughout the paper to ensure our estimates are not affected by population growth in important cities. We link the birth places of all women to the historical presence of higher secondary girls’ schools shown in Figure 1 obtained from the “Handbook of German Education History” that cover all finishing schools constructed in 1626–1850 (Neghabian et al., 1987).<sup>2</sup> The first finishing schools were established by female orders of the catholic church to educate and “protect the women’s mind from the falsities of their time”.<sup>3</sup> Often, they were invited by ruling houses and the sisters settled in unused rooms of monasteries. While focusing on religious teachings and manners, these schools complemented their curricula with math, German and foreign languages and thus provided the only opportunity for women to

---

<sup>2</sup>We complement this data with extensive archival research to find all finishing schools that were constructed during this time frame. Due to the scarcity of non-zero events in the outcome, we consider binary outcomes, logarithms, and shares relative to men and show robustness in all specifications.

<sup>3</sup>“... 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](https://de.wikipedia.org/wiki/Erzbischöfliche_Ursulinenschule_Köln).

acquire knowledge beyond basic reading and writing skills.<sup>4</sup>

However, even if these schools were established for reasons that are likely uncorrelated with local economic demands, a causal interpretation of the impact of schools would require that all unobservable factors are also orthogonal to school construction. To address the potentially endogenous adoption of finishing schools, we thus construct a panel of cities and estimate a fixed effects equation to capture all observable and unobservable factors affecting the timing of school establishment:

$$Y_{c,t} = \beta \text{Finishing school}_{i,t} + \alpha_c \times t + \alpha_t + \sum_{t=800}^{T=1950} [X_{e,c} \times \alpha_t + X_{r,c} \times \alpha_t + X_{s,c} \times \alpha_t] + \varepsilon_c \quad (1)$$

In our baseline specification, we regress a binary outcome of whether a notable woman was born in city  $c$  and period  $t$  on an indicator of the presence of a finishing school, city  $\alpha_c$  and period  $\alpha_t$  fixed effects and include city-specific linear time trends. This baseline set of controls then captures all unobservable city-specific trends that evolve linearly over time. Since birthplaces and school places do not overlap perfectly, we utilize data from Voigtlander and Voth (2012) to construct a balanced panel of cities that existed in 1300 across 50 year periods from 800 A.D. until 1950 to which we merge the birthplace of women and the location of finishing schools. This procedure has two advantages: It does not rely on any political or geographical boundary as the matching procedure is solely based on distance and we can use the rich set of covariates from Voigtlander and Voth (2012) to capture economic, religious and educational factors flexibly.<sup>5</sup>

The first set of covariates capture the direct effects of economic characteristics that influence the decision to open finishing schools ( $X_{e,c}$ ). We proxy for the economic and financial development in 1300 using membership in the hanseatic league, Jewish settlements and pogroms against Jews from Voigtlander and Voth (2012). We complement these covariates with population data in 1600 from Bairoch et al. (1988), religious battles during the 30 years' war affecting sex ratios and local weather conditions affecting agricultural population from Leeson and Russ (2017). Combined, these covariates, measured before

---

<sup>4</sup>Protestant and city schools started to emerge as a response to the local demand of the population after 1750. We report separate results for Schools constructed in the period 1650–1750 and 1750–1850 in the Online Appendix Table A1. The schools are weakly spatially correlated (Moran's I: 0.085) and we follow two strategies to deal with spatial autocorrelation. First, we report spatially-corrected standard errors in Table A2. Second, we randomly distribute the actual number of schools build in every period across Germany and show the distribution of point estimates in Figure A2.

<sup>5</sup>Results are robust to changing the year a city existed to 800 (Table A3) and including city  $\times$  period fixed effects in a panel setting with gender  $\times$  city  $\times$  period as the level of observation (Table A7).

the construction of the first school, capture demand factors of productivity and relative wages that impact the decision to establish a finishing school.

Since almost all early girl schools were established by religious orders, the second set of covariates capture the direct effects of religious differences across cities ( $X_{r,c}$ ). To that extend, we include whether the city was a bishopric seat (Voigtlander and Voth, 2012) and distance to Wittenberg to proxy for spread of Protestantism (Becker and Woessmann, 2009; Cantoni, 2015). In addition, we calculate which parts of Germany were protestant or catholic on the eve of the 30 years' war using data from Engel et al. (1995), and include the distance to the inner-German religious boundary to capture religious competition between the major religious denominations.

Finally, we address the direct effects of differential returns to education across cities ( $X_{s,c}$ ) by determining which city had a university or provided higher male education in 1650.<sup>6</sup> In addition, we exploit the inclination of some more economically minded ruling houses to provide education and determine the ruling house of the city in 1618 using Engel et al. (1995).<sup>7</sup> Combined, male schools and universities capture local returns to education at the time of the first construction and the indicator for the ruling house ensures that we compare cities under the influence of the same ruler, and thus the same preferences for education.

We interact all covariates with year fixed effects to isolate the effects of education from all confounding factors. 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 economic, population, religious, and educational differences across time. Thus, violations can only occur from unobservable characteristics that discontinuously change at the same time a finishing school was constructed.

### 3 Results

In this section, we provide evidence that the opening of finishing schools had a direct impact on the later occurrence of notable women in our data set. We explore different functional forms of the dependent variables and include covariates that capture economic, religious and educational confounders to highlight the robustness of our estimates.

---

<sup>6</sup>Obtained from [https://de.wikipedia.org/wiki/Liste\\_der\\_Hochschulen\\_in\\_Deutschland](https://de.wikipedia.org/wiki/Liste_der_Hochschulen_in_Deutschland) and [https://de.wikipedia.org/wiki/Liste\\_der\\_ältesten\\_Schulen\\_im\\_deutschen\\_Sprachraum](https://de.wikipedia.org/wiki/Liste_der_ältesten_Schulen_im_deutschen_Sprachraum).

<sup>7</sup>A famous 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.

We start by evaluating the validity of our fixed-effects design by testing for differential pre-trends in the event-study graph of Figure 2. Here, we limit our sample to all cities in which a finishing school was eventually established and estimate the impact of the first finishing school four centuries before and two centuries after its opening. Figure 2 provides evidence in favor of our design as the impact of finishing schools is zero in all periods prior to construction and discontinuously jumps in the first period after its opening. To capture unobservable factors that impact all women in a city, we estimate the impact of finishing schools on two subgroups of women: non-noble seculars and nobility. Royal women serve as a placebo group since these women are largely famous for having married, or being born, into nobility. Thus, if the establishment of a finishing school is correlated with an unobserved change in the overall likelihood of being recorded as notable, the point estimate on royals would be significant in post periods. However, while we find no impact of finishing schools on nobility, the probability of having a non-noble secular women born in a city increases right after the school opened. This relationship remains robust when including all control variables non-linearly in the right panel of Figure 2.<sup>8</sup>

We present our main results in Table 1 for three different specifications of our dependent variable to address sparsity in our outcome. In columns (1) and (2), we regress an indicator variable of whether a notable women 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 suggests that women are 23 percentage points more likely to become notable if their city of birth had a finishing school. We interact city, religious, and educational covariates with time fixed effects in column (2) and find a smaller, but robust 0.162 (s.e. 0.032) increase. Thus, while differential returns to education as proxied by population, male schools, or universities capture some parts of the variation, finishing schools double the likelihood of observing a notable women in periods after their establishment.

In the remaining columns (3)–(6) we explore the intensive margin adjustment of notable women. Using the log number of women born in city  $c$  at period  $t$ , we find that the number of women increase by 20%, even when extensively controlling for economic, religious and educational factors. In columns (5) and (6), we address population-growth effects directly and denominate the number of notable women born by the number of notable men in the same category and period. If the number of women in our sample only increase

---

<sup>8</sup>Online Appendix Figure A1 included all cities to increase precision.

because population discontinuously changed, this would be reflected in an increase in the number of notable men in the same category.<sup>9</sup> However, as we find similarly robust estimates we argue that finishing schools increase the likelihood of becoming noted and not population per se. On average, the share of notable women in city  $c$  at period  $t$  increases by 2 percentage points ( $\approx 100\%$ ) after finishing schools were established in the city.<sup>10</sup>

In the remaining panels of Table 1, we explore different employment categories and a placebo to the baseline estimates in Panel A. First, we confirm historical evidence, that many alumni went on to become teachers and writers and show that the likelihood of choosing this profession and becoming noted for it is markedly higher after the construction of a finishing school. Second, we analyze the CV of all notable women and searched for keywords marking female rights activism. While the mean is markedly lower than in other categories, the relationship is robust and stable in two of the three specification and suggests a threefold increase in the likelihood of becoming an activist after visiting the school (Panel C, column 3). Lastly, we estimate the impact on royals women in Panel D and report a robust and insignificant zero impact of finishing schools in all specifications. Similar to demeaning the number of women by the number of men, if finishing schools are established in faster growing regions with more nobility, this placebo exercise would report a significant impact.<sup>11</sup>

We take these results, and the non-significant impact on nobility, as evidence in favor of the hypothesis that providing education increases the social status and standing of women in the long run.

## 4 Placebo estimates

In this section we address the possibility that unobservable factors that determine the establishing of finishing schools are not sufficiently captured by our controls. In this section, we aim to exploit discontinuous changes in the returns to education, culture and construction activity across Germany at different points in time, to conduct extensive placebo exercises. To isolate the immediate impact of education, we run separate event-

---

<sup>9</sup>The number of men is constructed and obtained from the same source as the number of women.

<sup>10</sup>To address the possibility that people move to neighboring towns with schools, and thus spillovers are impacting the interpretation, we validate our results in two tables. In Table A3 we use the 101 cities that existed in 800 and in Table A4 we restrict our sample to 129 cities with schools and 27 non-neighboring cities in 1300.

<sup>11</sup>Controlling for construction activity does not impact our results (Table A5) and is not predicted by school construction (Figure A3).

study regressions in narrow windows around the opening of finishing schools and male schools. We argue that male schools are constructed in response to technical change and thus reflect the combined impact of and the returns to education. While we show a significant impact of male schools on notable men, we report insignificant impacts of male schools on the number of notable women and vice versa. We conclude that the estimates on finishing schools are unaffected by differential returns to education, changing culture or economic development and argue instead that our estimates capture the impact of education on empowerment identified by the unexpected expansion of educational opportunities for women.

In the first panel of Table 2, we assess the importance of changes in the returns to education by utilizing the occurrence of notable men, teachers and royals in our data and the construction of male schools. If finishing schools were a result of increased returns to education in the city, this would be reflected in an increase in the number of notable men as well. Similarly, since male schools were constructed as a response to returns to education, the estimated point estimate on men is a combined effect of returns to education and education itself. In this Table, we limit our sample to 129 cities that ever constructed a finishing school in the first row, and 186 cities that established a male school in the second row, in a window of four centuries before and two after establishing the first school.

In the first column we estimate the impact of finishing schools on notable women in odd and notable men in even columns. Despite the reducing in sample size, the estimated impact in this event-study design is close to the fixed-effects estimation.<sup>12</sup> Finishing schools, however, have no impact on the likelihood of observing notable men in our data. In the second column we turn to the impact of male schools on notable women and men. While the opening of male schools significantly increases the likelihood of observing a notable men, the impact on women is a precise zero.<sup>13</sup>

Combined, we take this as evidence that local returns to education cannot explain the impact on notable women we see in the data. If the opening of finishing schools coincides with increased 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. Similarly, if we observe women purely because the returns to education increased, we should observe a similar increase in women when using male schools as the source of variations. Since

---

<sup>12</sup>Results using logarithms are reported in Online Appendix Table A6.

<sup>13</sup>In Table A7, we find the same point estimates when we instead 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  $\times$  time fixed effects and men  $\times$  time fixed effects and estimate the impact of finishing schools on women, while non-linearly controlling for the trends in men and any city characteristic at any point in time.

we observe neither, we conclude that differential returns to education are not causing the impacts of women becoming notable in society.

In Panel B of Table 2, we turn to the impact of cultural change. Here, we use the end of witch trials in Germany to capture changes in culture towards women and their standing in society. Prior to the end of witch trials, women who openly shared their knowledge ran the risk of being accused of sorcery. Since the ending of these trials allowed women to be recognized as knowledgeable, their status in society increased. Thus, even if the quality or importance of women remains stable over time, changing the observability of these traits implies that women are mechanically more likely to enter our sample. However, we see no impact of the end of witch trials on women becoming recognized in our biographies.

In Panel C of Table 2, we use city-level construction data by Cantoni et al. (2019) to proxy for economic development. Here, even when defining a subgroup of growth specific construction that excludes religious, military, and palaces, we find no impact of finishing schools on economic activity in this Table, nor in any period around the construction of finishing schools (Figure A3).

In combination, the results suggest that finishing schools were not a direct response to changes in the returns to education, cultural change increasing a women's standing in society, or economic growth. We argue that it is more likely that finishing schools were established by religious orders to educate women to recognize the falsities of their times and thus are uncorrelated with changes in economic returns or culture, but a response to religious competition in Germany.<sup>14</sup> While focusing on religious teachings and manners, equal amounts of time was spent on french, math, and german literacy, providing the only opportunity for women to acquire skills beyond basic reading and writing.

## 5 Long Term Outcomes

In this section, we study how the exposure to french, math, and german literacy classes in finishing schools changed the perception of women and their societal status. In doing so we focus on political activism and political participation in parliaments at the cross-sectional level and utilize an ordinary least squares estimation with extensive controls, complemented by an instrumental variables strategy.

In this cross-sectional setting, unobservable factors, previously captured by city fixed

---

<sup>14</sup>We assess the sensitivity of our results to demanded schools after the industrial revolution in Germany in Online Appendix Table A1 where we split schools into early (supply) and late (demand) schools and show no differential impact.

effects and linear time trends, prevent a causal interpretation of our main explanatory variable if these factors are correlated with the establishment of finishing schools. Contemporaneous sources suggest that the upcoming of foreign religious orders from Belgium, Italy, and England dedicated to the education of women in the early 17th century is correlated with the emergence of Protestantism. These orders intended to protect the women's mind of the wrongful thoughts of the time, and had strong emphasis on educating women in foreign languages and math. At the emergence of these orders, rulers sought their help in the hope of an alternative to fighting Protestantism on the battlefield and settled these orders in old monasteries in lands under their rule.

Our instrument thus combines these determinants of the location of finishing schools. We combine data on the number of monasteries that existed in 1300 with religious competition at the inner-German religious divide. We argue that our instrument is unrelated to religious competition since 1300 is more than two centuries before the reformation and three centuries before the religious divide. In addition, since these monasteries are not opened by women, it is unlikely that they represent areas with more positive views of women. We instrument the number of finishing schools in city  $c$  in 1850 with the number of old monasteries in 1300, limiting our sample to cities within 10 km of the religious divide as constructed from Engel et al. (1995). Our instrumental variables strategy then increases the comparability of cities and hence does not compare Berlin to Munich, but rather Hanover to Hildesheim.<sup>15</sup> The compliers in our setup are cities with monasteries in which female orders moved in to establish finishing schools. Any violation of the exclusion restriction is then coming from monasteries at the end of the 13th century having an impact on women's contemporaneous recognition as important, other than through finishing schools, population, and all other economic, religious, and educational controls that we include in all specifications.

We present our results in Table 3. In the first panel we digitize the source location of all letters to the editor published in the first feminist newspaper dedicated to the cause of women's rights. More finishing schools in city  $c$  increase the likelihood of women sending a letter by 0.122 (s.e. 0.037) percentage points, or fourfold. In column (2) we instrument the number of finishing schools in 1850 with the number of monasteries in 1300 within 10 km of the religious divide. The F-Stat of 8.955 suggests a relevant instrument and the IV estimate increases to twice the size of the OLS.<sup>16</sup> Similar magnitudes are found for the

---

<sup>15</sup>Berlin had 8 finishing schools, none of which are catholic compared to Munich's 7 catholic schools. Hannover had no catholic school while Hildesheim had one.

<sup>16</sup>Covariates mainly affect the precision of the first stage as the point estimate is not altered, but the F-Stat

log number of letters from a given city in columns (3) and (4). Since literacy was common among the city population, we interpret these letters to main feminist newspapers as the first sign that women started to question their status as second class citizens and the discriminatory practices against them.

In the second Panel of Table 3, we address two issues at once. We utilize the *Lexikon deutscher Frauen der Feder* (Pataky, 1898) which constitutes an encyclopedia of all important female writers of the second half of the 19th century as recognized by contemporaries. First, using a contemporaneous source, rather than the NDB, we capture the writers perceived importance to the women's rights movement and not our modern-day perception. Second, we capture a snapshot of influential female writers at a time when women started to organize in teachers unions. Inspired by writings from female writers, both the *Allgemeiner Deutscher Frauenverein* and the *Allgemeiner Deutscher Lehrerinnenverein* had strong focus on improving educational opportunities for finishing in all levels of society. The estimates in Panel B reveal that the number of finishing schools in city  $c$  is indeed positively correlated with the birthplaces of female writers in Pataky (1898). Using our 2SLS estimate we show that the number of schools in 1850 in city  $c$  had a positive and significant impact on the number of female writers important for the female rights movement born in the same city.

In the third and fourth Panel of Table 3 we collect the birthplace of all female members of parliament in the Weimarer Republic (1919-1933) and modern day Germany (1949-2013).<sup>17</sup> In an egalitarian society, controlling for population, finishing schools should have no impact on the likelihood or number of female politicians born in a city. We do however report strong and significant impacts of finishing schools in 1850 on the indicator and number of female politicians in all parliaments since conception of the parliamentary democracy in Germany.<sup>18</sup> The 2SLS estimate in column (4) suggests that each school increases the number of female politicians by 78%.<sup>19</sup> However, the relative impact of finishing has decreased to about 70% of the relevant mean, highlighting the progress that

---

decreases (Appendix Table A8). We explore different timing and bandwidths of the instrument in Appendix Table A9 and Figures A4-A5.

<sup>17</sup>Since we do not use the electorate of a female politician but her birthplace to link to our cities our estimates do not represent the preference of her electorate.

<sup>18</sup>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, Ober-Taunuskreis, Landshut, and Dresden. Estimates increase without the largest 10 percent of the sample in 1600.

<sup>19</sup>The results are robust to controlling for whether a town ever had, or the number of, male members of parliaments and the share of female member of parliaments (Table A10).

has been made in the last century.

## References

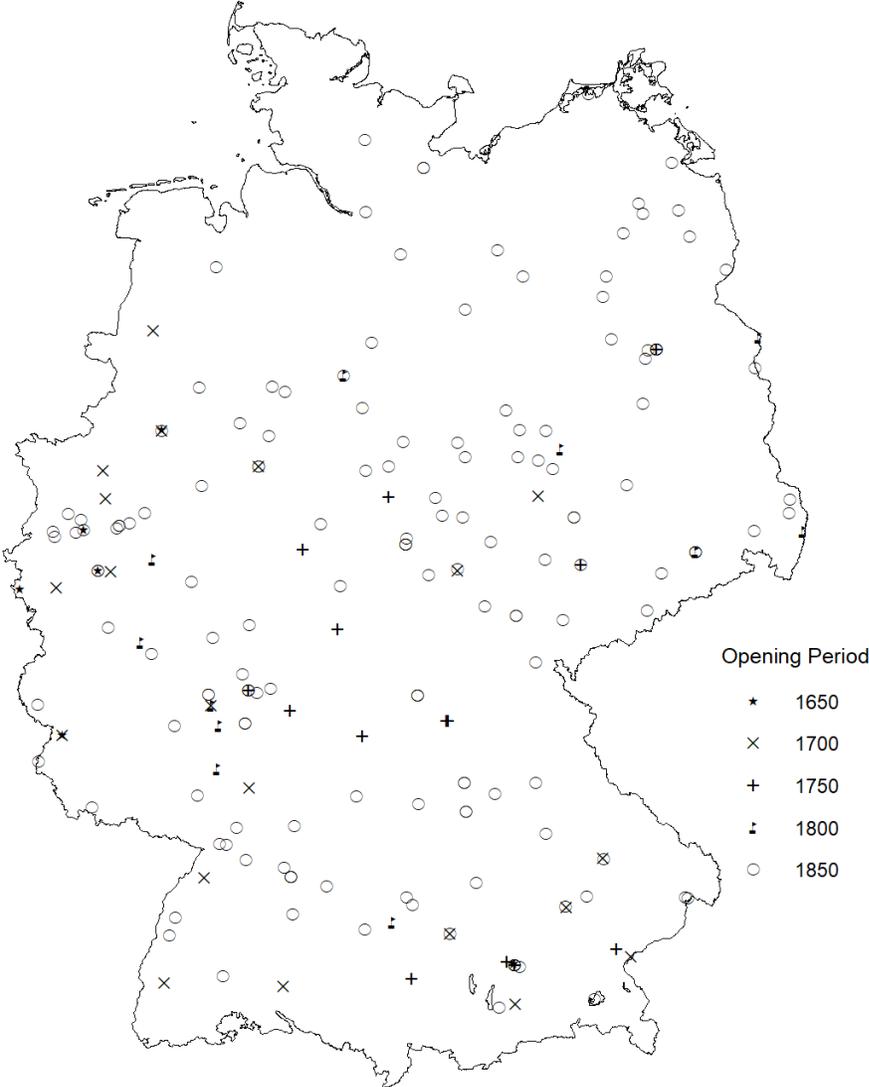
- Aaronson, D., F. Lange, and B. Mazumder (2014). Fertility transitions along the extensive and intensive margins. *American Economic Review* 104(11), 3701–3724.
- Albisetti, J. C. (1982). Could separate be equal? helene lange and women's education in imperial germany. *History of Education Quarterly* 22(3), 301–317.
- Alesina, A., P. Giuliano, and N. Nunn (2013). On the Origins of Gender Roles: Women and the Plough. *The Quarterly Journal of Economics* 128(2), 469–530.
- Bairoch, P., J. Batou, and C. Pierre (1988). *Population des villes européennes de 800 à 1850: banque de données et analyse sommaire des résultats (la)*. Librairie Droz.
- Ballington, J., A. Karam, I. I. for Democracy, and E. Assistance (2005). *Women in Parliament: Beyond Numbers*. Handbook series. International IDEA.
- Beaman, L., E. Duflo, R. Pande, and P. Topalova (2012). Female leadership raises aspirations and educational attainment for girls: A policy experiment in india. *Science* 335(6068), 582–586.
- Becker, S. O. and L. Woessmann (2009). Was Weber wrong? A human capital theory of Protestant economic history. *The Quarterly Journal of Economics* 124(2), 531–596.
- Blau, F. D. and L. M. Kahn (2016, January). The gender wage gap: Extent, trends, and explanations. Working Paper 21913, National Bureau of Economic Research.
- Bobbitt-Zeher, D. (2007). The gender income gap and the role of education. *Sociology of Education* 80(1), 1–22.
- Breierova, L. and E. Duflo (2004, May). The impact of education on fertility and child mortality: Do fathers really matter less than mothers? Working Paper 10513, National Bureau of Economic Research.
- Burgess, S. (2016, April). Human Capital and Education: The State of the Art in the Economics of Education. IZA Discussion Papers 9885, Institute of Labor Economics (IZA).
- Cantoni, D. (2015). The economic effects of the protestant reformation: Testing the weber hypothesis in the german lands. *Journal of the European Economic Association* 13(4), 561–598.

- Cantoni, D., C. Mohr, and M. Weigand (2019). The rise of fiscal capacity. *Working Paper*.
- Carlana, M. (2019). Implicit stereotypes: Evidence from teachers' gender bias. *Quarterly Journal of Economics*.
- Chou, S.-Y., J.-T. Liu, M. Grossman, and T. Joyce (2010, January). Parental education and child health: Evidence from a natural experiment in taiwan. *American Economic Journal: Applied Economics* 2(1), 33–61.
- Diebolt, C. and F. Perrin (2013, May). From stagnation to sustained growth: The role of female empowerment. *American Economic Review* 103(3), 545–49.
- Duflo, E. (2012). Women empowerment and economic development. *Journal of Economic Literature* 50(4), 1051–1079.
- Engel, J., E. W. Zeeden, and E. W. Zeeden (Eds.) (1995). *Großer historischer Weltatlas*. München: Bayer. Schulbuch-Verl.
- Fink, M. (1992). *Women and education in Latin America: knowledge, power, and change*, Chapter Women and popular education in Latin America, pp. 171–193. Boulder, Colorado: Lynne Rienner Publishers.
- Freire, P. (1972). *Pedagogy of the Oppressed*. Middlessex: Penguin Books.
- G. Johnson, A. (2000, 01). *Privilege, power, and difference*.
- Galor, O. and D. N. Weil (1996). The Gender Gap, Fertility, and Growth. *American Economic Review* 86(3), 374–387.
- Gleason, S. (2001). Female political participation and health in india. *The ANNALS of the American Academy of Political and Social Science* 573(1), 105–126.
- Historische Kommisson der Bayerischen Akademie der Wissenschaften (2019). *Deutsche Biographie*.
- Hsiang, S. M., M. Burke, and E. Miguel (2013). Quantifying the influence of climate on human conflict. *Science* 341(6151).
- Jayachandran, S. (2015). The roots of gender inequality in developing countries. *Annual Review of Economics* 7(1), 63–88.

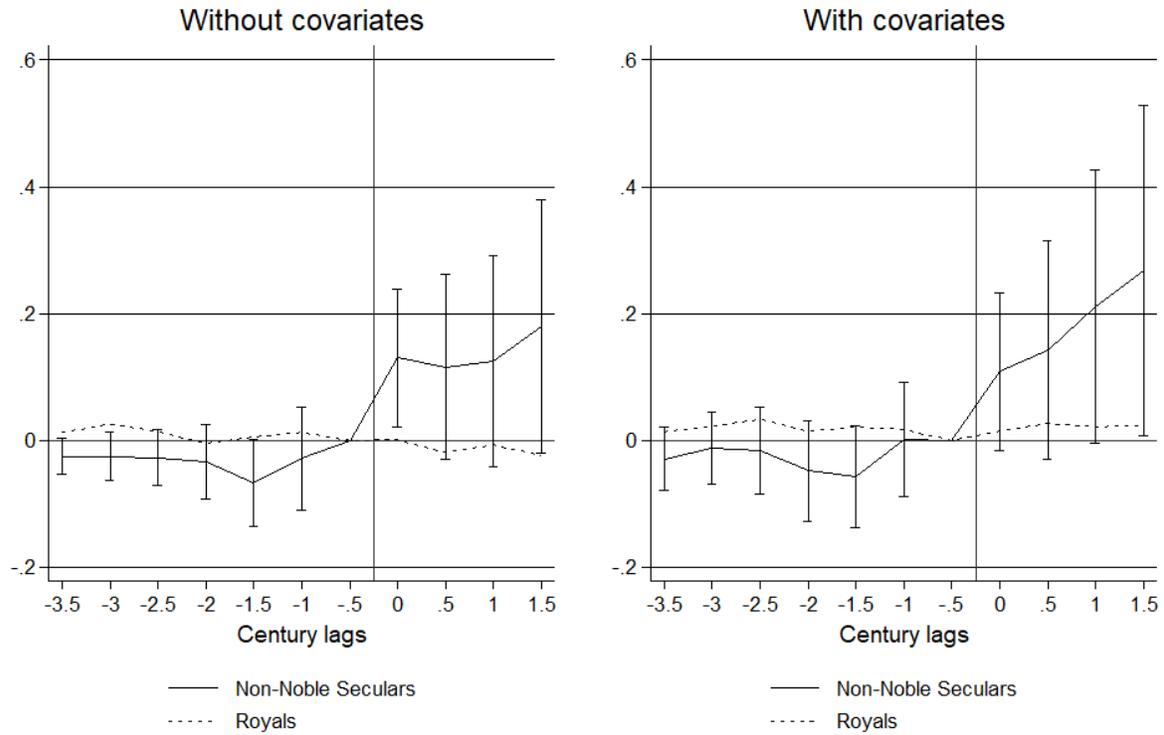
- Kabeer, N. (2005). Gender equality and women's empowerment: A critical analysis of the third millennium development goal 1. *Gender & Development* 13(1), 13–24.
- Kimura, M. and D. Yasui (2010, 12). The galor-weil gender-gap model revisited: From home to market. *Journal of Economic Growth* 15, 323–351.
- Leeson, P. T. and J. W. Russ (2017). Witch trials. *The Economic Journal* 128(613), 2066–2105.
- Madestam, A. and E. Simeonova (2019). Gender empowerment in 19th century sweden: Women's economic empowerment and the intergenerational transmission of human capital.
- Neghabian, G., L. Ziob, and B. Zymek (1987). Sozialgeschichte und Statistik des Mädchenschulwesens in den deutschen Staaten 1800-1945. In H. Titze (Ed.), *Datenhandbuch zur deutschen Bildungsgeschichte*. Vandenhoeck & Ruprecht.
- Panday, P. K. (2008). Representation without participation: Quotas for women in bangladesh. *International Political Science Review* 29(4), 489–512.
- Pataky, S. (1898). *Lexikon deutscher Frauen der Feder: Eine Zusammenstellung der seit dem Jahre 1840 erschienenen Werke weiblicher Autoren, nebst Biographien der lebenden und einem Verzeichnis der Pseudonyme*. Berlin: Pataky, Sophia.
- Qian, N. (2004). Missing women and the price of tea in china.
- Sarsons, H. (2017). Gender differences in recognition for group work.
- Schultz, T. W. (1961). Investment in human capital. *The American Economic Review* 51(1), 1–17.
- Voigtlander, N. and H.-J. Voth (2012, 07). Persecution Perpetuated: The Medieval Origins of Anti-Semitic Violence in Nazi Germany\*. *The Quarterly Journal of Economics* 127(3), 1339–1392.
- Welch, S. (1977). Women as political animals? a test of some explanations for male-female political participation differences. *American Journal of Political Science* 21(4), 711–730.

# Figures and Tables

Figure 1: School opening years in Germany



**Figure 2: Main results: Impact of finishing school construction on notable women**



Main results for *Non-Noble Seculars* and *Royals* 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. City and period fixed effects included in the left figure and full controls included in the right. 95%-confidence intervals reported.

**Table 1: Fixed-effects results on the importance of finishing schools**

	$\mathbb{I}[\text{Women} > 0]$		log Women		Share Women	
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Panel A: Non-Noble Seculars</i>						
Finishing school <sub>it</sub>	0.230*** (0.029)	0.165*** (0.034)	0.355*** (0.053)	0.204*** (0.045)	0.019*** (0.004)	0.021*** (0.005)
Mean, untreated	0.150	0.149	0.140	0.139	0.018	0.018
<i>Panel B: Teachers &amp; Writers</i>						
Finishing school <sub>it</sub>	0.151*** (0.027)	0.104*** (0.026)	0.174*** (0.034)	0.104*** (0.029)	0.019*** (0.006)	0.017*** (0.006)
Mean, untreated	0.076	0.075	0.060	0.059	0.019	0.019
<i>Panel C: Activists</i>						
Finishing school <sub>it</sub>	0.076*** (0.018)	0.053*** (0.018)	0.064*** (0.017)	0.043*** (0.015)	0.001 (0.001)	0.000 (0.001)
Mean, untreated	0.016	0.016	0.012	0.012	0.001	0.001
<i>Panel D: Royals</i>						
Finishing school <sub>it</sub>	-0.018 (0.016)	-0.013 (0.017)	-0.009 (0.015)	-0.007 (0.018)	-0.002 (0.008)	-0.002 (0.009)
Mean, untreated	0.039	0.038	0.031	0.031	0.018	0.018
Unit trend	Yes	Yes	Yes	Yes	Yes	Yes
City covariates × time FE		Yes		Yes		Yes
Religious covariates × time FE		Yes		Yes		Yes
Educational covariates × time 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.  $\mathbb{I}[\text{Women} > 0]$  is an indicator equal to one if a city had at least one notable women 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 in the same category, except for Activists, where we denote it by the number of non-noble secular men. We regress the number of non-noble secular women, teachers and writers, and royal 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 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 time fixed effects. Standard errors clustered by city shown in parentheses. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

**Table 2: Placebo estimates on the importance of finishing schools**

	<i>Non-Noble Secular</i>		<i>Teachers &amp; Writers</i>		<i>Royals</i>	
	(1)	(2)	(3)	(4)	(5)	(6)
	Female	Male	Female	Male	Female	Male
<i>Panel A: Returns to Education</i>						
Finishing school <sub>it</sub>	0.096*	-0.002	0.116**	-0.081	0.008	-0.050
	(0.054)	(0.039)	(0.050)	(0.061)	(0.034)	(0.057)
Male school <sub>it</sub>	0.015	0.068*	0.011	0.072**	-0.004	0.037
	(0.013)	(0.039)	(0.008)	(0.033)	(0.018)	(0.032)
<i>Panel B: Cultural Change</i>						
End of Witch Trial <sub>it</sub>	0.052	0.006	0.000	-0.049	-0.008	-0.015
	(0.038)	(0.071)	(0.027)	(0.075)	(0.032)	(0.047)
<i>Panel C: Construction Activity</i>						
	I[> 0]		Number		log	
	Any	Growth	Any	Growth	Any	Growth
Finishing school <sub>it</sub>	-0.035	-0.019	1.290	0.679	0.033	0.093
	(0.031)	(0.063)	(1.078)	(0.583)	(0.097)	(0.104)
City covariates × time FE	Yes	Yes	Yes	Yes	Yes	Yes
Religious covariates × time FE	Yes	Yes	Yes	Yes	Yes	Yes

Main results using a fixed-effects estimation in four centuries before and two centuries after the establishing of the row variable, only in cities who eventually established the row variable. This reduces the sample to 1,421 observations for finishing schools, 2,094 for male schools, and 1,344 for the end of witch trials in Panel B. We regress each row variable on an indicator equal to one if a city had at least one notable women/man born in this period  $I[> 0]$ . We regress the number of non-noble secular women, teachers and writers, and royal women born in a city, as defined in the top row, on our finishing school variable. We include covariates as defined in Table 1 columns (2), (4), and (6) in all regressions, except for educational variables. A panel version of this Panel A with city × time fixed effects is available in the Online Appendix Table A7. In Panel C, we regress the opening of a finishing school on the indicator, the raw number, and the log number of construction activity in odd columns, and growth specific construction in even columns. Growth specific constructions excludes religious, military, and palaces. Standard errors clustered by city shown in parentheses. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

**Table 3: Long-term impact of finishing schools on political outcomes**

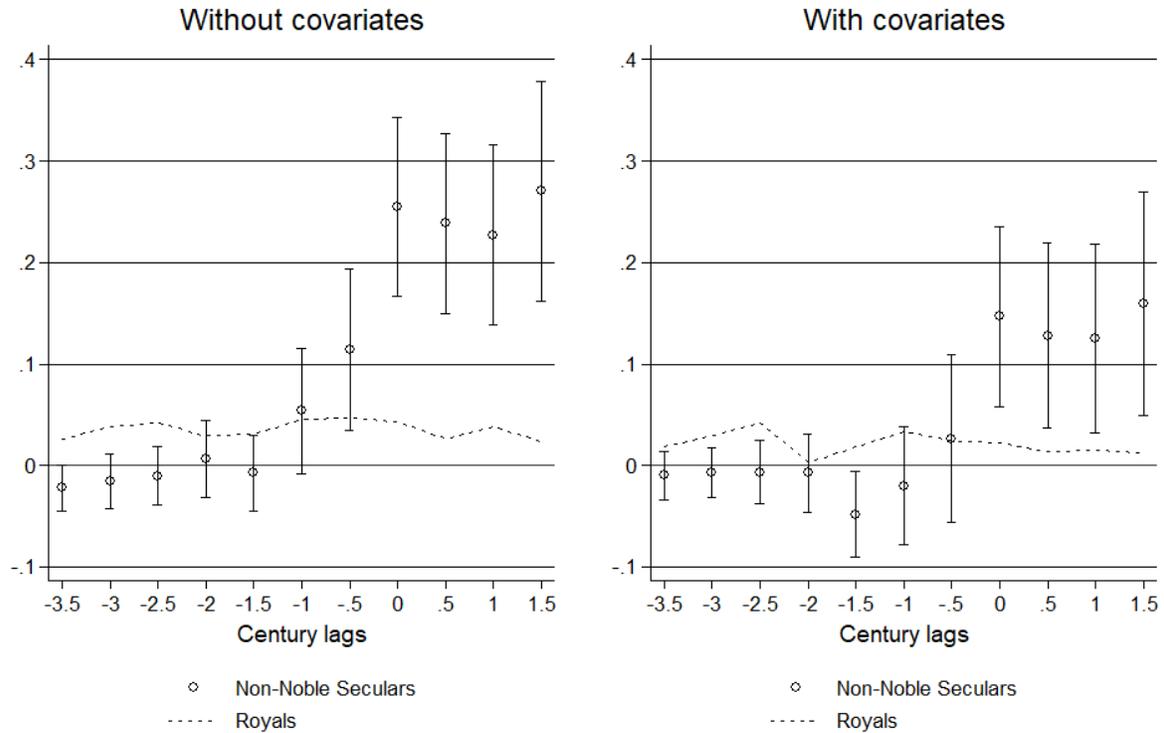
	I[> 0]		log Number	
	(1) OLS	(2) 2SLS	(3) OLS	(4) 2SLS
<i>Panel A: Leserbriefe, Frauenzeitung, 1849–1852</i>				
Finishing schools	0.122*** (0.037)	0.249** (0.098)	0.240** (0.097)	0.411*** (0.158)
Mean, untreated	0.038	0.038	0.061	0.061
<i>Panel B: Zeno: Female Writers, 1899</i>				
Finishing schools	0.181*** (0.039)	0.176 (0.133)	0.329*** (0.051)	0.491*** (0.141)
Mean, untreated	0.130	0.130	0.137	0.137
<i>Panel C: Member Parliament, 1919–1933</i>				
Finishing schools	0.101*** (0.034)	0.164* (0.093)	0.105*** (0.035)	0.226** (0.090)
Mean, untreated	0.038	0.038	0.053	0.053
<i>Panel D: Member Parliament, 1949–2013</i>				
Finishing schools	0.136*** (0.040)	0.303* (0.169)	0.317*** (0.082)	0.695** (0.289)
Mean, untreated	0.244	0.244	0.855	0.855
City Covariates	Yes	Yes	Yes	Yes
Religious covariates	Yes	Yes	Yes	Yes
Educational covariates	Yes	Yes	Yes	Yes
Observations	183	183	183	183
Bandwidth	10	10	10	10
F-Stat first stage		8.941		8.941

Main results using an ordinary least squares (OLS) in odd columns and a two-stage least-squares estimation (2SLS) in even columns. We instrument the number of finishing Schools in 1850 in city  $c$  with the number of monasteries in 1300 in city  $c$ , comparing cities within 10 km of the inner-German religious divide to proxy religious competition and capture similar cities. Estimates robust to using log number of schools and log number of monasteries in 1300. 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 the 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 produce more contemporaneously acknowledged writers who were famous for their activism for the female rights movement in city  $c$ . In Panel C we estimate the impact of girl schools on the likelihood and number of female members of parliament from their birthplace  $c$ . In Panel D we repeat the exercise for female members of parliament in the German parliament until 2013. We include covariates as defined in Table 1 columns (2), (4), and (6) in all regressions. Standard errors clustered by city shown in parentheses. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

# Online Appendix - Not for Publication

20th August 2019

Figure A1: Results including all cities: Impact of finishing school construction on notable women



Additional results for *Non-Noble Seculars* and *Royals* including all cities to improve precision. Zero is the normalized time of establishment of finishing schools in the city. We include a zero point estimate to have every estimate in relation to the untreated control. The lead 50 years prior can be explained by girls who were born some years prior to the opening of a finishing school, but are still eligible to attend these schools later in life. When extensively controlling for city characteristics in the right figure, this lead is insignificant. City and period fixed effects included in the left figure and full controls included in the right. 95%-confidence intervals reported.

**Table A1: Fixed-effects results on the importance of finishing schools - Early vs Late Schools**

	$\mathbb{I}[\text{Women} > 0]$		log Women		Share Women	
	(1) Early	(2) Late	(3) Early	(4) Late	(5) Early	(6) Late
<i>Panel A: Non-Noble Seculars</i>						
Finishing school <sub>it</sub>	0.095** (0.041)	0.185*** (0.044)	0.278*** (0.100)	0.246*** (0.058)	0.016** (0.007)	0.023*** (0.007)
Mean, untreated	0.147	0.148	0.137	0.138	0.019	0.018
<i>Panel B: Teachers &amp; Writers</i>						
Finishing school <sub>it</sub>	0.095** (0.041)	0.129*** (0.032)	0.166** (0.081)	0.124*** (0.032)	0.011 (0.008)	0.022*** (0.007)
Mean, untreated	0.074	0.074	0.058	0.059	0.019	0.019
<i>Panel C: Activists</i>						
Finishing school <sub>it</sub>	0.053* (0.029)	0.066*** (0.022)	0.070 (0.043)	0.051*** (0.018)	0.000 (0.001)	0.000 (0.001)
Mean, untreated	0.018	0.016	0.013	0.012	0.002	0.001
<i>Panel D: Royals</i>						
Finishing school <sub>it</sub>	-0.021 (0.039)	-0.014 (0.019)	0.004 (0.035)	-0.012 (0.016)	-0.002 (0.018)	-0.003 (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 × time FE	Yes	Yes	Yes	Yes	Yes	Yes
Religious covariates × time FE	Yes	Yes	Yes	Yes	Yes	Yes
Educational covariates × time 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 demanded schools after 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.  $\mathbb{I}[\text{Women} > 0]$  is an indicator equal to one if a city had at least one notable women 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 in the same category, except for Activists, where we denote it by the number of non-noble secular men. We regress the number of non-noble secular women, teachers and writers, and royal 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 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 time fixed effects. Significance levels are \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

**Table A2: Fixed-effects results on the importance of finishing schools - Standard errors corrected for spatial dependence**

	$\mathbb{I}[\text{Women} > 0]$		log Women		Share Women	
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Panel A: Non-Noble Seculars</i>						
Finishing school <sub>it</sub>	0.230*** (0.026)	0.165*** (0.028)	0.355*** (0.033)	0.204*** (0.030)	0.019*** (0.004)	0.021*** (0.004)
Mean, untreated	0.150	0.149	0.140	0.139	0.018	0.018
<i>Panel B: Teachers &amp; Writers</i>						
Finishing school <sub>it</sub>	0.151*** (0.019)	0.104*** (0.020)	0.174*** (0.018)	0.104*** (0.020)	0.019*** (0.005)	0.017*** (0.005)
Mean, untreated	0.076	0.075	0.060	0.059	0.019	0.019
<i>Panel C: Activists</i>						
Finishing school <sub>it</sub>	0.076*** (0.015)	0.053*** (0.014)	0.064*** (0.012)	0.043*** (0.010)	0.001 (0.001)	0.000 (0.001)
Mean, untreated	0.016	0.016	0.012	0.012	0.001	0.001
<i>Panel D: Royals</i>						
Finishing school <sub>it</sub>	-0.018 (0.014)	-0.013 (0.015)	-0.009 (0.012)	-0.007 (0.013)	-0.002 (0.006)	-0.002 (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 × time FE		Yes		Yes		Yes
Religious covariates × time FE		Yes		Yes		Yes
Educational covariates × time 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.  $\mathbb{I}[\text{Women} > 0]$  is an indicator equal to one if a city had at least one notable women 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 in the same category, except for Activists, where we denote it by the number of non-noble secular men. We regress the number of non-noble secular women, teachers and writers, and royal 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 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 time fixed effects. Significance levels are \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

**Table A3: Fixed-effects results on the importance of finishing schools - Changing the Unit of observation to cities that existed in 800**

	$\mathbb{I}[\text{Women} > 0]$		log Women		Share Women	
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Panel A: Non-Noble Seculars</i>						
Finishing school <sub>it</sub>	0.251*** (0.049)	0.231*** (0.064)	0.465*** (0.098)	0.357*** (0.100)	0.016*** (0.005)	0.017* (0.009)
Mean, untreated	0.201	0.180	0.214	0.189	0.020	0.018
<i>Panel B: Teachers &amp; Writers</i>						
Finishing school <sub>it</sub>	0.183*** (0.048)	0.154** (0.063)	0.257*** (0.067)	0.180** (0.072)	0.019** (0.008)	0.016 (0.011)
Mean, untreated	0.103	0.090	0.091	0.076	0.019	0.016
<i>Panel C: Activists</i>						
Finishing school <sub>it</sub>	0.104*** (0.032)	0.077* (0.046)	0.100*** (0.031)	0.058 (0.039)	0.001 (0.001)	0.000 (0.001)
Mean, untreated	0.029	0.026	0.023	0.020	0.001	0.001
<i>Panel D: Royals</i>						
Finishing school <sub>it</sub>	-0.018 (0.037)	-0.057 (0.044)	-0.001 (0.039)	-0.036 (0.043)	0.002 (0.019)	-0.034 (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 x time FE		Yes		Yes		Yes
Religious covariates x time FE		Yes		Yes		Yes
Educational covariates x time 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.  $\mathbb{I}[\text{Women} > 0]$  is an indicator equal to one if a city had at least one notable women 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 in the same category, except for Activists, where we denote it by the number of non-noble secular men. We regress the number of non-noble secular women, teachers and writers, and royal 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 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 time fixed effects. Significance levels are \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

**Table A4: Fixed-effects results on the importance of finishing schools - Comparing towns with schools to non-neighboring towns without schools**

	$\mathbb{I}[\text{Women} > 0]$		log Women		Share Women	
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Panel A: Non-Noble Seculars</i>						
Finishing school <sub>it</sub>	0.165*** (0.034)	0.167*** (0.047)	0.204*** (0.045)	0.156*** (0.055)	0.018** (0.007)	0.021*** (0.005)
Mean, untreated	0.149	0.164	0.139	0.158	0.017	0.016
<i>Panel B: Teachers &amp; Writers</i>						
Finishing school <sub>it</sub>	0.104*** (0.026)	0.108** (0.044)	0.104*** (0.029)	0.091** (0.043)	0.023** (0.009)	0.017*** (0.006)
Mean, untreated	0.075	0.085	0.059	0.066	0.017	0.017
<i>Panel C: Activists</i>						
Finishing school <sub>it</sub>	0.053*** (0.018)	0.039 (0.026)	0.043*** (0.015)	0.026 (0.019)	0.001 (0.001)	0.000 (0.001)
Mean, untreated	0.016	0.011	0.012	0.008	0.001	0.001
<i>Panel D: Royals</i>						
Finishing school <sub>it</sub>	-0.013 (0.017)	-0.010 (0.028)	-0.007 (0.018)	0.007 (0.034)	0.001 (0.015)	-0.002 (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 × time FE	Yes	Yes	Yes	Yes	Yes	Yes
Religious covariates × time FE	Yes	Yes	Yes	Yes	Yes	Yes
Educational covariates × time 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

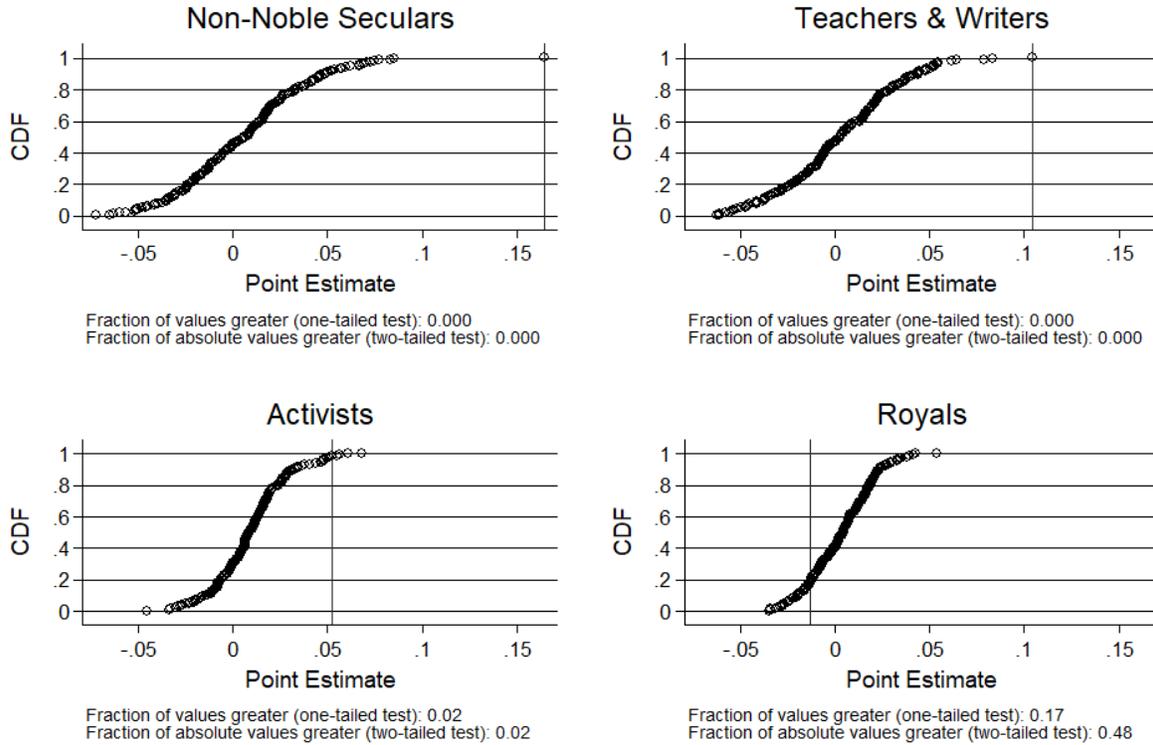
Main results using a fixed-effects estimation and either cities with finishing schools or non-neighboring cities without schools to address spatial spillovers. We consider three types of dependent variables to capture the extensive and intensive margin of the birth of notable women.  $\mathbb{I}[\text{Women} > 0]$  is an indicator equal to one if a city had at least one notable women 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 in the same category, except for Activists, where we denote it by the number of non-noble secular men. We regress the number of non-noble secular women, teachers and writers, and royal 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 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 time fixed effects. Significance levels are \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

**Table A5: Fixed-effects results on the importance of finishing schools - Controlling flexibly for construction**

	$\mathbb{I}[\text{Women} > 0]$		log Women		Share Women	
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Panel A: Non-Noble Seculars</i>						
Finishing school <sub>it</sub>	0.161*** (0.034)	0.169*** (0.034)	0.208*** (0.045)	0.214*** (0.045)	0.020*** (0.006)	0.021*** (0.005)
Mean, untreated	0.148	0.147	0.138	0.137	0.018	0.018
<i>Panel B: Teachers &amp; Writers</i>						
Finishing school <sub>it</sub>	0.104*** (0.027)	0.109*** (0.027)	0.106*** (0.027)	0.112*** (0.028)	0.018*** (0.006)	0.019*** (0.006)
Mean, untreated	0.075	0.075	0.059	0.059	0.019	0.019
<i>Panel C: Activists</i>						
Finishing school <sub>it</sub>	0.065*** (0.018)	0.053*** (0.018)	0.049*** (0.015)	0.044*** (0.015)	0.001 (0.001)	0.000 (0.001)
Mean, untreated	0.016	0.016	0.012	0.012	0.001	0.001
<i>Panel D: Royals</i>						
Finishing school <sub>it</sub>	-0.017 (0.017)	-0.015 (0.016)	-0.007 (0.017)	-0.006 (0.016)	-0.002 (0.009)	-0.002 (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 × time FE	Yes	Yes	Yes	Yes	Yes	Yes
Religious covariates × time FE	Yes	Yes	Yes	Yes	Yes	Yes
Educational covariates × time FE	Yes	Yes	Yes	Yes	Yes	Yes
Construction in 1650 × time FE	Yes		Yes		Yes	
Construction in every period × time FE		Yes		Yes		Yes
Observations	9,096	9,144	9,096	9,144	9,096	9,144

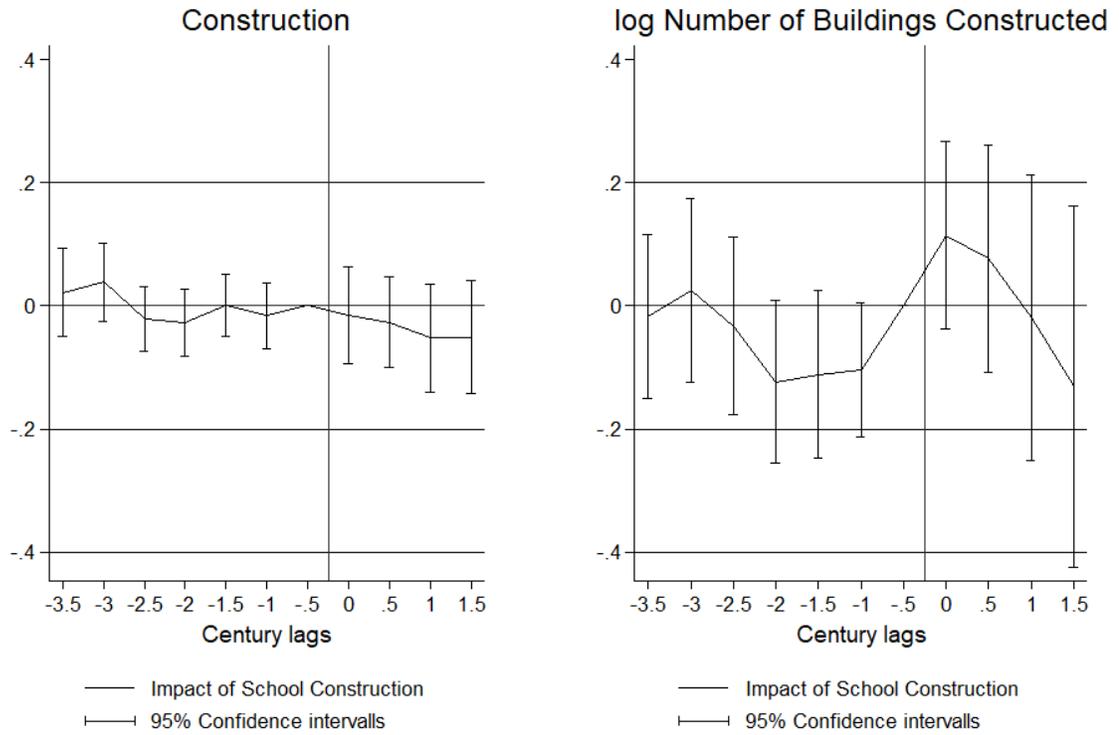
We consider three types of dependent variables to capture the extensive and intensive margin of the birth of notable women.  $\mathbb{I}[\text{Women} > 0]$  is an indicator equal to one if a city had at least one notable women born in this period. 'log Women' constitutes the natural logarithm of the number of women born plus one. 'Share Women' denotes the number of women by the number of men in the same category, except for Activists, where we denote it by the number of non-noble secular men. We regress the number of non-noble secular women, teachers and writers, and royal 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 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 time fixed effects. Significance levels are \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

**Figure A2: 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 schools across a map of Germany. This is repeated for every period and used as a new explanatory variable in a regression with full controls. The vertical line marks the baseline estimate in Table 1 column (2). The large p-values for royals are in line with our expectations of a null-effect on royal women.

**Figure A3: Impact of finishing schools on construction activity**



The correlation between finishing schools and building construction. 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 controls included in the right. 95%-confidence intervals reported.

**Table A6: Placebo estimates on the importance of finishing schools - log outcomes**

	<i>Non-Noble Secular</i>		<i>Teachers &amp; Writers</i>		<i>Royals</i>	
	(1) Female	(2) Male	(3) Female	(4) Male	(5) Female	(6) Male
<i>Panel A: Returns to Education</i>						
Finishing school <sub>it</sub>	0.143** (0.063)	0.033 (0.104)	0.131*** (0.045)	-0.039 (0.101)	0.035 (0.041)	-0.016 (0.052)
Male school <sub>it</sub>	0.017 (0.012)	0.191*** (0.055)	0.011* (0.006)	0.138*** (0.041)	0.003 (0.014)	0.035 (0.030)
<i>Panel B: Cultural Change</i>						
End of Witch Trial <sub>it</sub>	0.023 (0.034)	0.128 (0.094)	-0.008 (0.020)	0.008 (0.098)	-0.006 (0.023)	-0.002 (0.037)
City covariates × time FE	Yes	Yes	Yes	Yes	Yes	Yes
Religious covariates × time FE	Yes	Yes	Yes	Yes	Yes	Yes

Main results using a fixed-effects estimation in four centuries before and two centuries after the establishing of the row variable, only in cities who eventually established the row variable. This reduces the sample to 1,421 observations for finishing schools, 2,094 for male schools, and 1,344 for the end of witch trials in Panel C. We regress each row variable on the log number of women/men in that category plus one. We include all covariates as defined in Table 1 columns (2), (4), and (6) in all regressions, except for educational variables. Standard errors clustered by city shown in parentheses. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

**Table A7: Importance of finishing schools - City  $\times$  period fixed effects**

	<i>Non-Noble Secular</i>			<i>Teachers &amp; Writers</i>		
	(1) Female	(2) Male	(3) Panel	(4) Male	(5) Female	(6) Panel
Finishing school	0.096*	-0.002		0.116**	-0.081	
	(0.054)	(0.039)		(0.050)	(0.061)	
Finishing school $\times$ women			0.139**			0.138**
			(0.058)			(0.064)
City covariates $\times$ time FE	Yes	Yes		Yes	Yes	
Religious covariates $\times$ time FE	Yes	Yes		Yes	Yes	
City $\times$ time FE			Yes			Yes
Men $\times$ time FE			Yes			Yes
Adj. R2	0.465	0.448	0.535	0.309	0.427	0.446
Observations	1,421	1,421	2,996	1,421	1,421	2,996

Testing the a panel specification with city  $\times$  time fixed effects four centuries before and two centuries after the establishing of finishing schools. In columns (1) and (4) we estimate the impact of finishing schools on women in the sample of cities that ever established a finishing school. In columns (2) and (5) we estimate the impact of finishing schools on men in the sample of cities that ever established a finishing school. In columns (3) and (6) we construct a panel in which every city  $\times$  period cell has two observations; one for women and one for men. This allows us to control for city  $\times$  time fixed effects and men  $\times$  time fixed effects 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 each row variable on an indicator equal to one if a city had at least one notable women/man born in this period  $\mathbb{I}[\gt 0]$ . We include all covariates as defined in Table 1 columns (2), (4), and (6) in all regressions, except for educational variables. Standard errors clustered by city shown in parentheses. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

**Table A8: Long-term impact of finishing schools on political outcomes - sensitivity to covariates**

	I[> 0]				log Number			
	(1) OLS	(2) 2SLS	(3) OLS	(4) 2SLS	(5) OLS	(6) 2SLS	(7) OLS	(8) 2SLS
<i>Panel A: Leserbriefe, Frauenzeitung, 1849–1852</i>								
Finishing schools	0.121*** (0.036)	0.168*** (0.064)	0.122*** (0.037)	0.249** (0.098)	0.221*** (0.074)	0.232** (0.103)	0.240** (0.097)	0.411*** (0.158)
Mean, untreated	0.045	0.045	0.038	0.038	0.068	0.068	0.061	0.061
<i>Panel B: Zeno: Female Writers, 1899</i>								
Finishing schools	0.232*** (0.027)	0.281*** (0.079)	0.181*** (0.039)	0.176 (0.133)	0.371*** (0.044)	0.472*** (0.081)	0.329*** (0.051)	0.491*** (0.141)
Mean, untreated	0.128	0.128	0.130	0.130	0.135	0.135	0.137	0.137
<i>Panel C: Member Parliament, 1919–1933</i>								
Finishing schools	0.149*** (0.035)	0.273*** (0.055)	0.101*** (0.034)	0.164* (0.093)	0.156*** (0.042)	0.302*** (0.058)	0.105*** (0.035)	0.226** (0.090)
Mean, untreated	0.038	0.038	0.038	0.038	0.053	0.053	0.053	0.053
<i>Panel D: Member Parliament, 1949–2013</i>								
Finishing schools	0.197*** (0.036)	0.338*** (0.089)	0.136*** (0.040)	0.303* (0.169)	0.410*** (0.068)	0.680*** (0.133)	0.317*** (0.082)	0.695** (0.289)
Mean, untreated	0.241	0.241	0.244	0.244	0.842	0.842	0.855	0.855
City Covariates			Yes	Yes			Yes	Yes
Religious covariates			Yes	Yes			Yes	Yes
Educational covariates			Yes	Yes			Yes	Yes
Observations	185	185	183	183	185	185	183	183
Bandwidth	10	10	10	10	10	10	10	10
F-Stat first stage		41.161		8.941		41.161		8.941

Results using an ordinary least squares (OLS) in odd columns and a two-stage least-squares estimation (2SLS) in even columns, with and without covariates. We instrument the number of finishing schools in 1850 in city  $c$  with the number of monasteries in 1300 in city  $c$ , comparing cities within 10 km of the inner-German religious divide to proxy religious competition and capture similar cities. Estimates robust to using log number of schools and log number of monasteries in 1300. 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 the 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 produce more contemporaneously acknowledged writers who were famous for their activism for the female rights movement in city  $c$ . In Panel C we estimate the impact of girl schools on the likelihood and number of female members of parliament from their birthplace  $c$ . In Panel D we repeat the exercise for female members of parliament in the German parliament until 2013. We include all covariates as defined in Table 1 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 A9: Long-term impact of finishing schools on political outcomes - IV estimates using different timings of the Monastery instrument**

	I[> 0]			log Number		
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Panel A: Leserbriefe, Frauenzeitung, 1849–1852</i>						
Finishing schools	0.249** (0.098)	0.274** (0.107)	0.296** (0.121)	0.411*** (0.158)	0.491*** (0.186)	0.444** (0.192)
Mean, untreated	0.038	0.038	0.038	0.061	0.061	0.061
<i>Panel B: Zeno: Female Writers, 1899</i>						
Finishing schools	0.176 (0.133)	0.181 (0.151)	0.073 (0.127)	0.491*** (0.141)	0.497*** (0.158)	0.376*** (0.134)
Mean, untreated	0.130	0.130	0.130	0.137	0.137	0.137
<i>Panel C: Member Parliament, 1919–1933</i>						
Finishing schools	0.164* (0.093)	0.121 (0.090)	0.136 (0.104)	0.226** (0.090)	0.192** (0.093)	0.225** (0.093)
Mean, untreated	0.038	0.038	0.038	0.053	0.053	0.053
<i>Panel D: Member Parliament, 1949–2013</i>						
Finishing schools	0.303* (0.169)	0.323* (0.187)	0.259 (0.188)	0.695** (0.289)	0.756** (0.338)	0.658* (0.338)
Mean, untreated	0.244	0.244	0.244	0.855	0.855	0.855
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	8.941	7.231	8.447	8.941	7.231	8.447

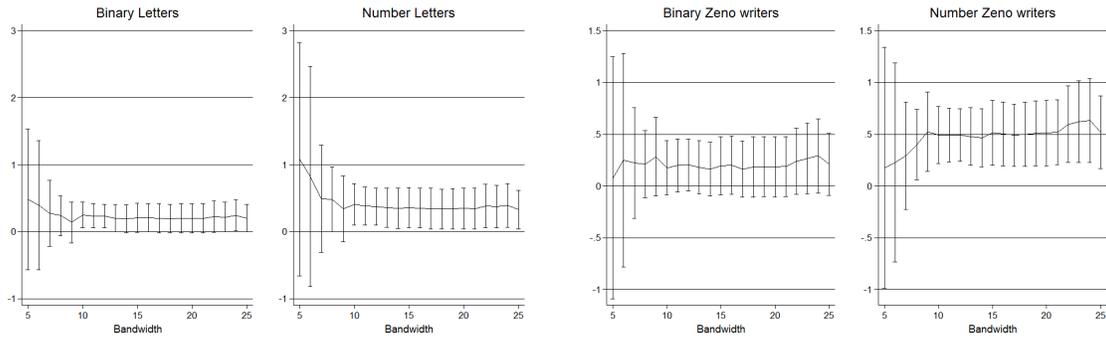
Results using a two-stage least-squares estimation (2SLS) in all columns, changing the monastery date to 1300 (Columns (1) and (4)), 1500 (Columns (2) and (5)) and 1648 (Columns (3) and (6)). We instrument the number of finishing schools in 1850 in city  $c$  with the number of monasteries in city  $c$ , 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 the 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 produce more contemporaneously acknowledged writers who were famous for their activism for the female rights movement in city  $c$ . In Panel C we estimate the impact of girl schools on the likelihood and number of female members of parliament from their birthplace  $c$ . In Panel D we repeat the exercise for female members of parliament in the German parliament until 2013. We include all covariates as defined in Table 1 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 A10: Long-term impact of finishing schools on political outcomes - Controlling for men as members of parliament**

	$\mathbb{I}[\gt 0]$		log Number		Female Share	
	(1) OLS	(2) 2SLS	(3) OLS	(4) 2SLS	(5) OLS	(6) 2SLS
<i>Member Parliament, 1949–2013</i>						
Finishing schools	0.134*** (0.040)	0.291* (0.165)	0.242*** (0.087)	0.599* (0.320)	0.028* (0.016)	0.062 (0.042)
Male MP from town	0.136 (0.106)	0.118 (0.110)				
# Male MP from Town			0.195*** (0.059)	0.114 (0.097)		
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
Mean, untreated	0.244	0.244	0.855	0.855	0.051	0.051
F-Stat first stage		8.718		6.484		8.941

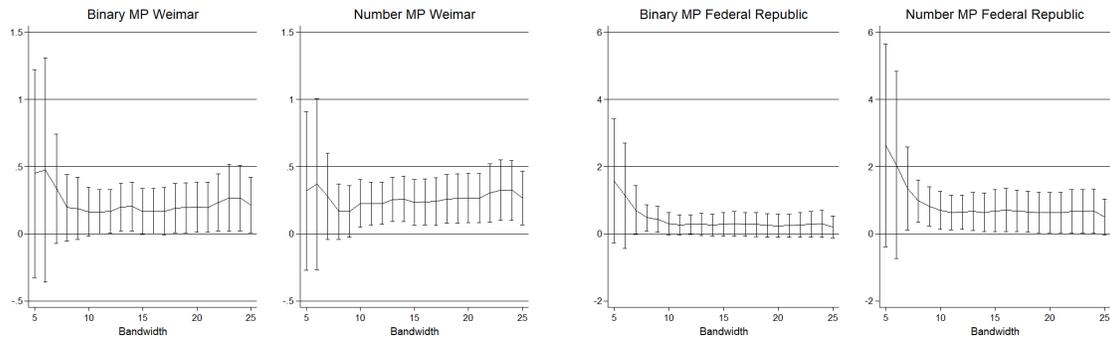
Results using a two-stage least-squares estimation (2SLS) in all columns, changing the monastery date to 1300 (Columns (1) and (4)), 1500 (Columns (2) and (5)) and 1648 (Columns (3) and (6)). We instrument the number of finishing schools in 1850 in city  $c$  with the number of monasteries in city  $c$ , 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 the 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 produce more contemporaneously acknowledged writers who were famous for their activism for the female rights movement in city  $c$ . In Panel C we estimate the impact of girl schools on the likelihood and number of female members of parliament from their birthplace  $c$ . In Panel D we repeat the exercise for female members of parliament in the German parliament until 2013. We include all covariates as defined in Table 1 columns (2), (4), and (6) in all regressions. Standard errors clustered by city shown in parentheses. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

**Figure A4: Bandwidth for letters to the editors and female writers**



These figures report the sensitivity of the 2SLS estimates in different bandwidths around the religious divide. The increased standard errors are the result of a weaker first stage.

**Figure A5: Bandwidth for Members of Parliament**



These figures report the sensitivity of the 2SLS estimates in different bandwidths around the religious divide. The increased standard errors are the result of a weaker first stage.