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

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

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

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

A.1 Data Construction

We downloaded the entire corpus of the NDB in October 2022. Since we are interested in linking individuals to their birth year and location, we iteratively searched by birth year from 800 - 1975 and downloaded the results. We arrived at 65,477 women and 415,586 men.[1] For some individuals the exact birth year is unknown; the data then shows them twice: once in the year in which they were mentioned "erwähnt" and 120 years prior. The unique number of women (men) thus decreases to 63,124 (388,459).

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

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

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

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

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

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

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

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

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

	$\mathbb{I}[Women > 0]$			log Women			Share Women		
	(1) ADB	(2) NDB	(3) Post-NDB	(4) ADB	(5) NDB	(6) Post-NDB	(7) ADB	(8) NDB	(9) Post-NDB
Finishing school$_{it}$	0.122**	0.176***	0.100*	0.138***	0.215***	0.193**	0.020**	0.030**	0.009
	(0.049)	(0.062)	(0.056)	(0.051)	(0.069)	(0.080)	(0.009)	(0.012)	(0.010)
Unit trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
City covariates × period FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Religious covariates × period FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Educational covariates × period FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Matching on 1600 covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	7,749	7,749	7,749	7,749	7,749	7,749	7,749	7,749	7,749
Mean	0.038	0.078	0.300	0.047	0.125	0.707	0.008	0.015	0.042
Included if born before:	1800	1800	1800	1800	1800	1800	1800	1800	1800

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

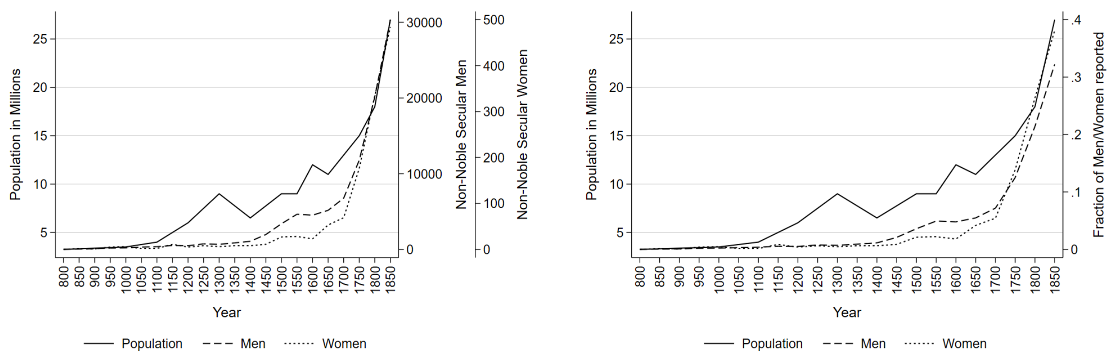
[2] The increase in shares of women noted in the ADB, NDB, and today provides further evidence of the heterogeneity with respect to importance.

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

A.2 Women's inclusion into the NDB

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

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



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

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

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

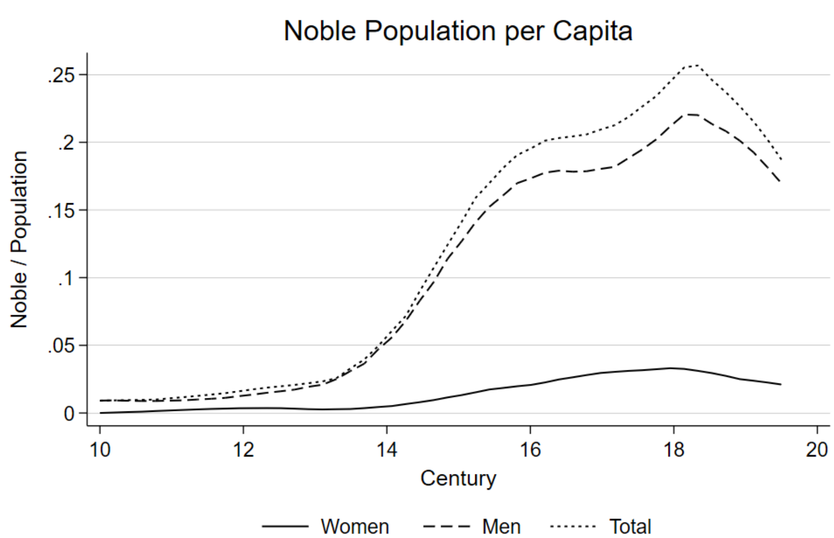
A related concern is differential reporting between cities with and cities without finishing schools in the NDB. Specifically, finishing schools may have improved record keeping on notable women rather than increased women's share among the human capital elite. We offer two arguments against this interpretation: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

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

A related concern is that the number of notables might be constant over time, yielding the placebo estimates on royals mute. Figure A.2 provides evidence against this concern as the share of nobility is increasing for both genders.[3] When using royals as a placebo we thus test whether the growth rate of nobility is different between cities.

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

Figure A.2: Noble population per capita



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

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

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

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

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

B Alternative empirical specifications and economic growth

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

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

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

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

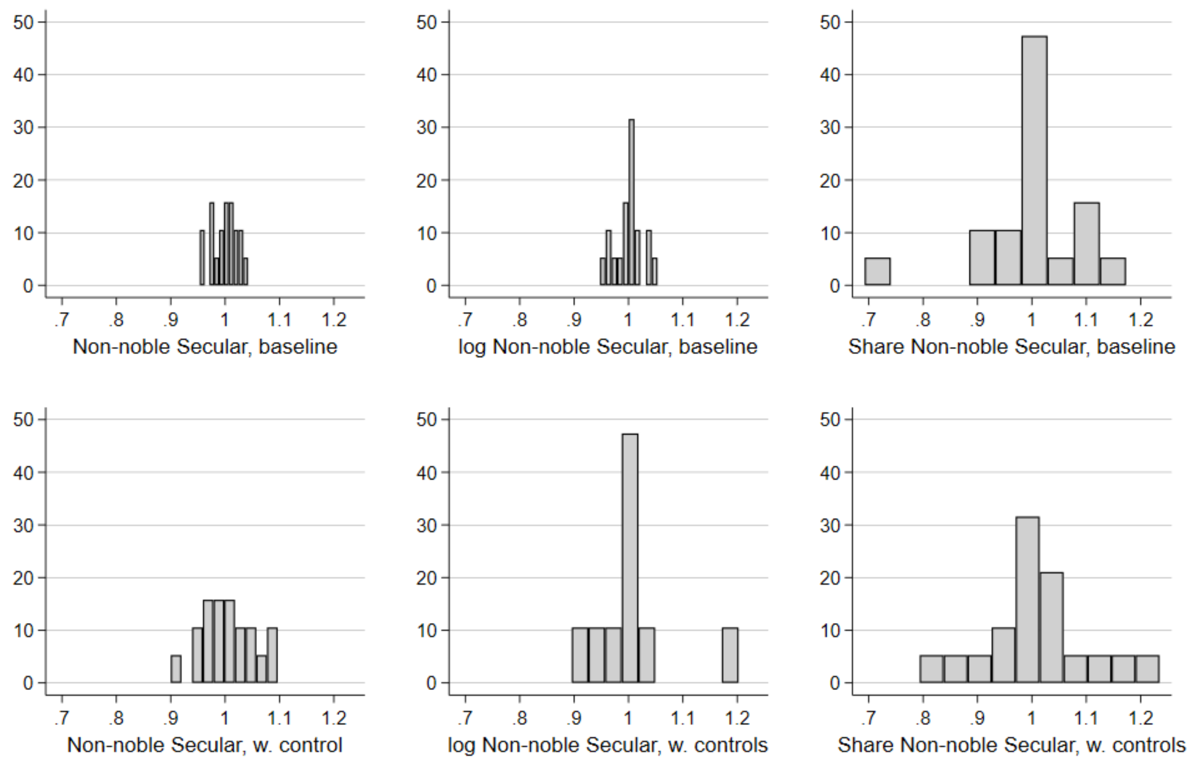
B.1 Sensitivity to dropping observations

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

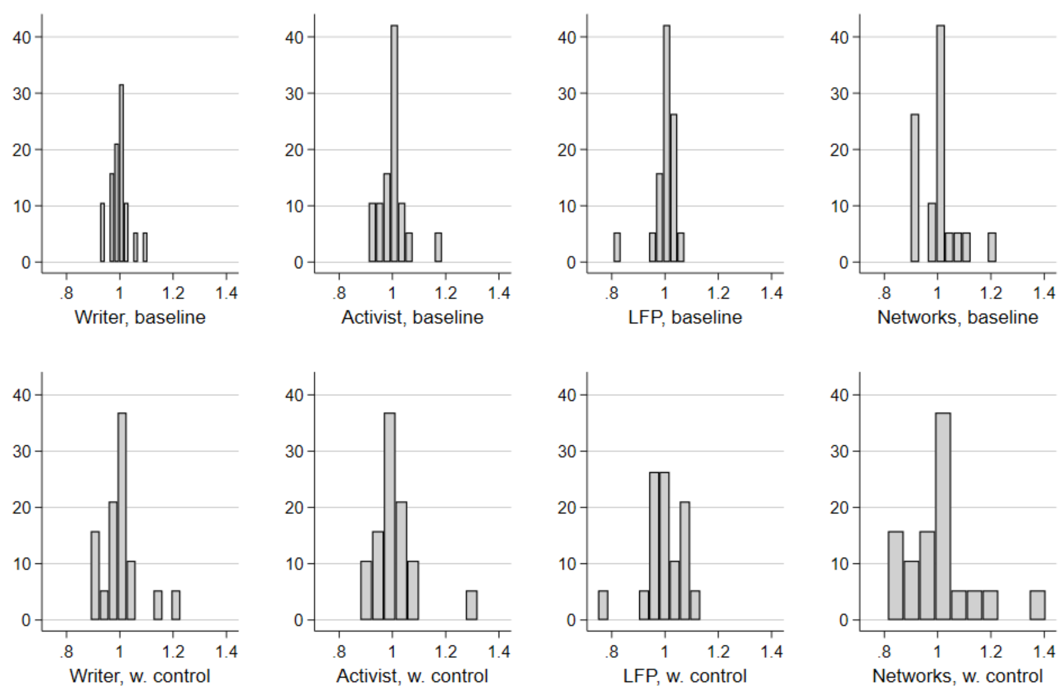
In Figure B.1 and B.2, we present all outcomes in all specifications corresponding to Tables 1, 4, 5 and 7. The x-axis measures the ratio between a restricted estimate when a set of cities is dropped and the original estimate from the corresponding table. If the restricted estimate remains unchanged,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

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

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



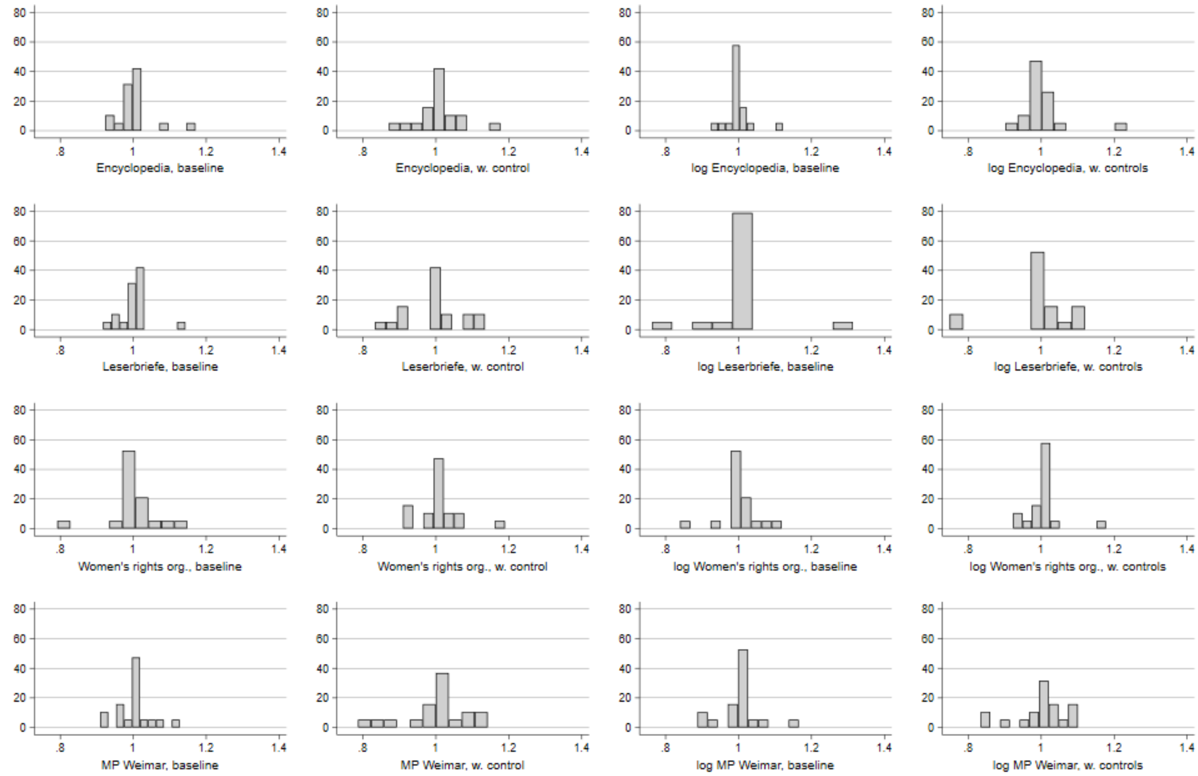
(a) Main outcome: Female representation among the human capital elite.



(b) Occupations of women.

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

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



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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

C Dataset construction choices and timing of school establishment

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

C.1 Structure of the data

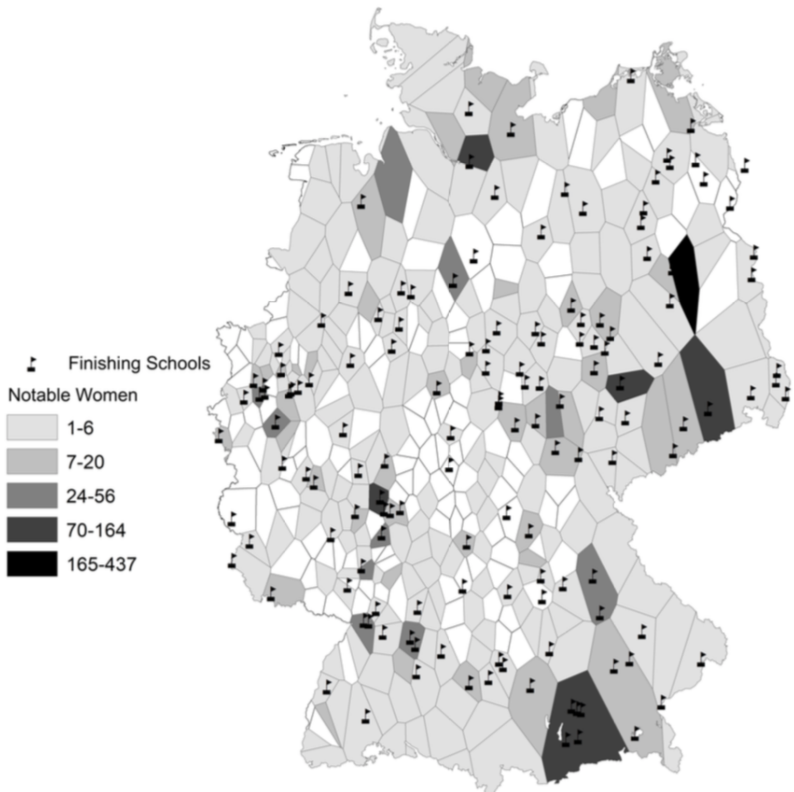
We take the city-level data by Voigtländer and Voth (2012) as a starting point and construct Thiessen Polygons around the center of each city in their dataset. Thiessen Polygons are constructed such that every village or town inside the polygon around city i is closer to city i than to any other city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

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

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

The next choice concerns the length of periods. We choose to assign notable individuals to 50-year periods based on their year of birth. There are two reasons for our 50-year period choice: First, by choosing 50-years, we ensure that on average a woman that is born in this period either did or did not have access to a finishing school. Second, the scarce number of women recorded in the NDB prior to the 15th century implies a trade-off between statistical power and assignment accuracy. If we used every birth year separately, and thus matched schools most precisely, we would end up with no variation within most city x birth-year cells. Thus, to increase power, we rely on 50-year periods, and show robustness to using 25 year intervals in Table C.2. Again, our point estimates remain unaffected.

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



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

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

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

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

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

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

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

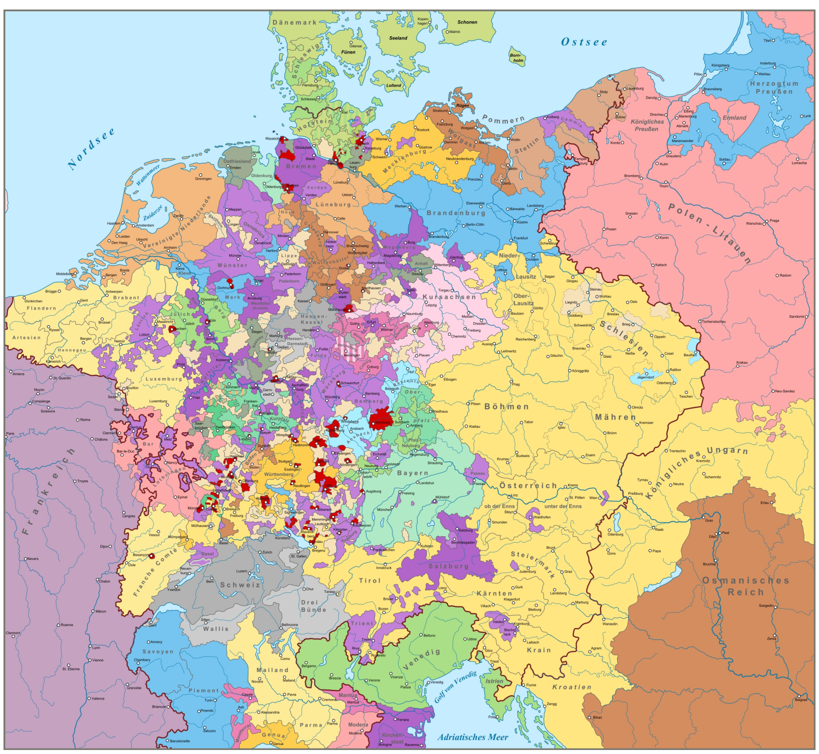
C.2 Sample selection: Using a different starting point for the analysis

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

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

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

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



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

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

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

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

C.3 Using the exact opening time of finishing schools

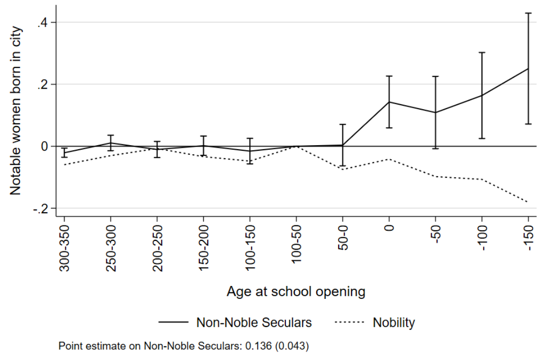
In our baseline data, we created a balanced panel for each city to include never-treated cities and covariates. This decision is in line with the recent literature on event-study validity, as discussed in Appendix F. In the resulting panel, we merged individuals to the closest of 50-year periods in cities.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

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

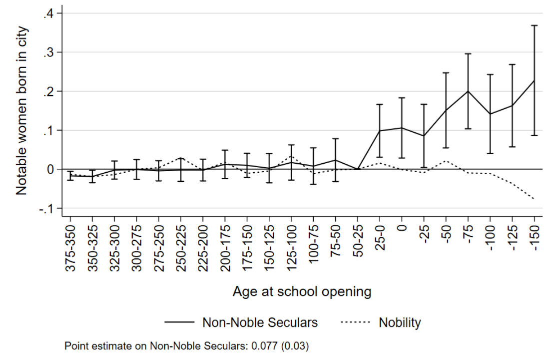
In Figure C.3, we use the opening time of the first finishing school in our 129 cities with schools and create various lags around it. In all Panels, we aim to reference the estimates to a previous generation of women who could no longer benefit from education: parents. In Panel a, c and e, we create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

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

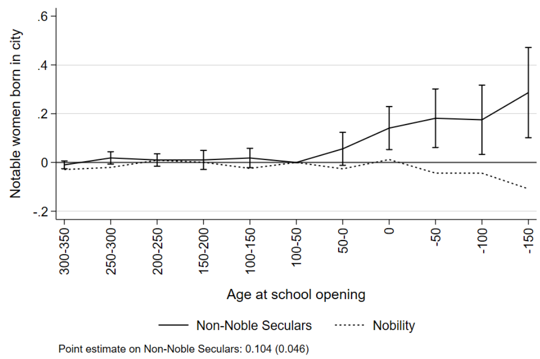
Figure C.3: Event-Study: Impact of finishing school establishment on notable women



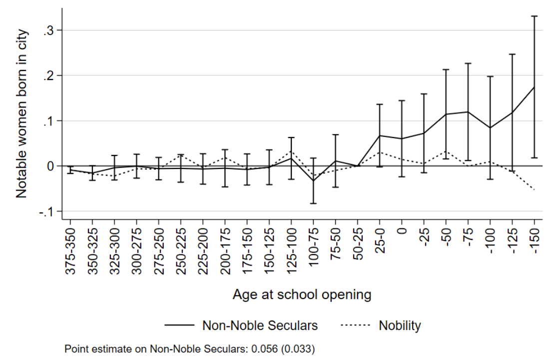
(a) 50 year periods, 100 year fixed effects



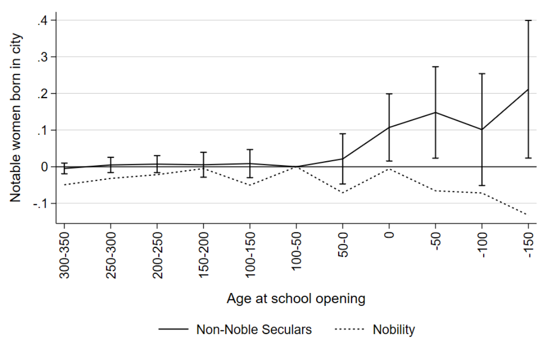
(b) 25 year periods, 100 year fixed effects



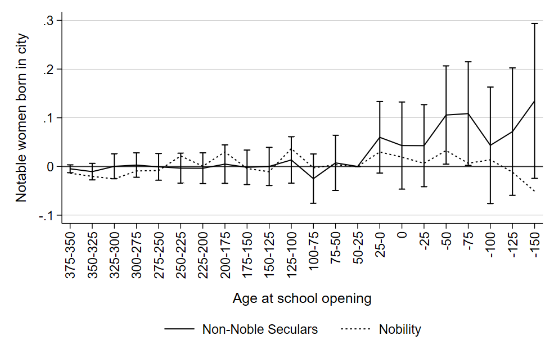
(c) 50 year periods, 50 year fixed effects



(d) 25 year periods, 50 year fixed effects



(e) 50 year periods, 25 year fixed effects



(f) 25 year periods, 25 year fixed effects

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

C.4 Timing of school construction

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

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

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

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

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

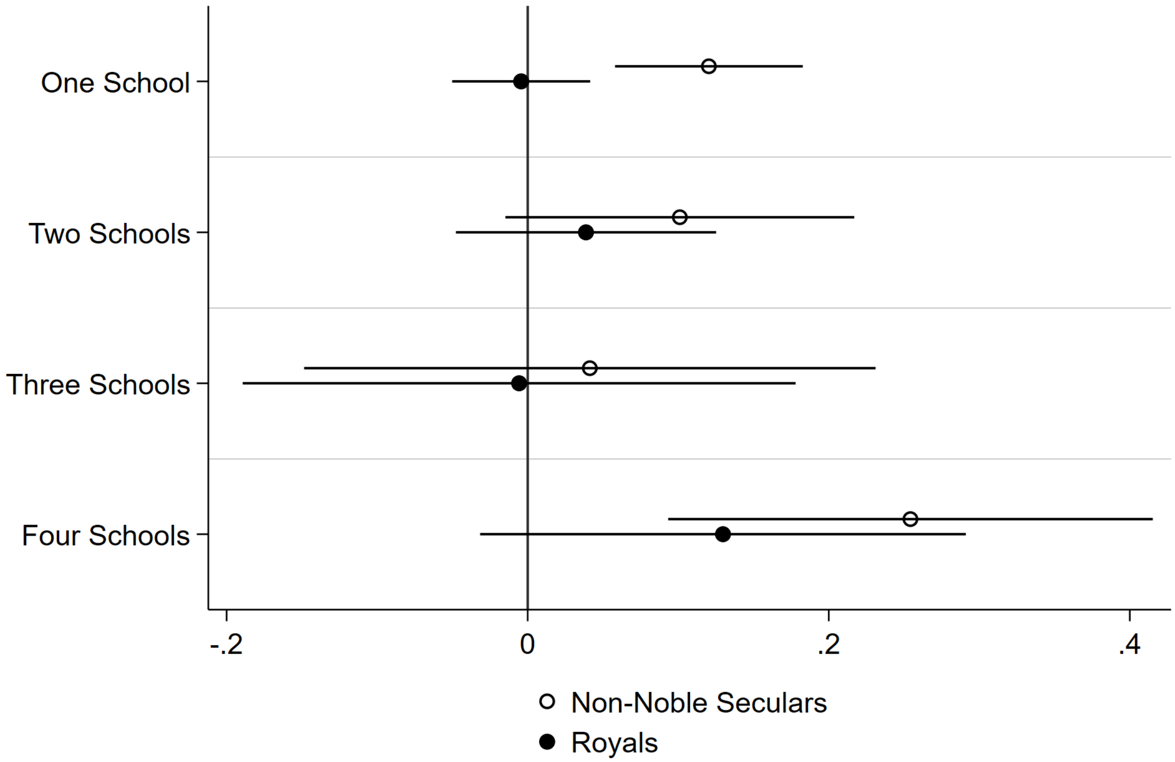
Main results comparing schools constructed between 1650–1750 (*early*) and 1800–1850 (*late*) to assess the sensitivity of our results to schools whose establishment is potentially influenced by rising demand for women's education in the context of the Industrial Revolution in Germany. We consider three 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. We regress the number of non-noble secular women and women from the nobility on our measure of access to secondary education. In all columns we interact city controls with period fixed effects to capture variation from economic, religious, and educational differences. Standard errors clustered by city shown in parentheses. $* \ p < 0.10$, $** \ p < 0.05$, $*** \ p < 0.01$

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

	$\mathbb{I}[Women > 0]$		log Women		Share Women	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Non-Noble Seculars						
First finishing school$_{it}$	0.134***	0.131***	0.148***	0.102**	0.012**	0.013**
	(0.031)	(0.032)	(0.040)	(0.041)	(0.006)	(0.006)
Second finishing school$_{it}$		0.014		0.228**		-0.003
		(0.047)		(0.110)		(0.008)
Mean, untreated	0.144	0.144	0.131	0.131	0.026	0.026
Panel B: Nobility						
First finishing school$_{it}$	0.005	-0.002	0.017	0.002	-0.002	-0.002
	(0.024)	(0.024)	(0.022)	(0.022)	(0.009)	(0.009)
Second finishing school$_{it}$		0.033		0.071*		0.002
		(0.039)		(0.040)		(0.014)
Mean, untreated	0.062	0.062	0.048	0.048	0.023	0.023
Unit trend	Yes	Yes	Yes	Yes	Yes	Yes
City covariates $\times$ period FE	Yes	Yes	Yes	Yes	Yes	Yes
Religious covariates $\times$ period FE	Yes	Yes	Yes	Yes	Yes	Yes
Educational covariates $\times$ period FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	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. We regress the number of non-noble secular women and women from the nobility on our measure of access to secondary education. In all columns we interact city controls with period fixed effects to capture variation from economic, religious, and educational differences. Standard errors clustered by city shown in parentheses. $* \ p < 0.10$, $** \ p < 0.05$, $*** \ p < 0.01$

Figure C.4: The impact of multiple schools



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

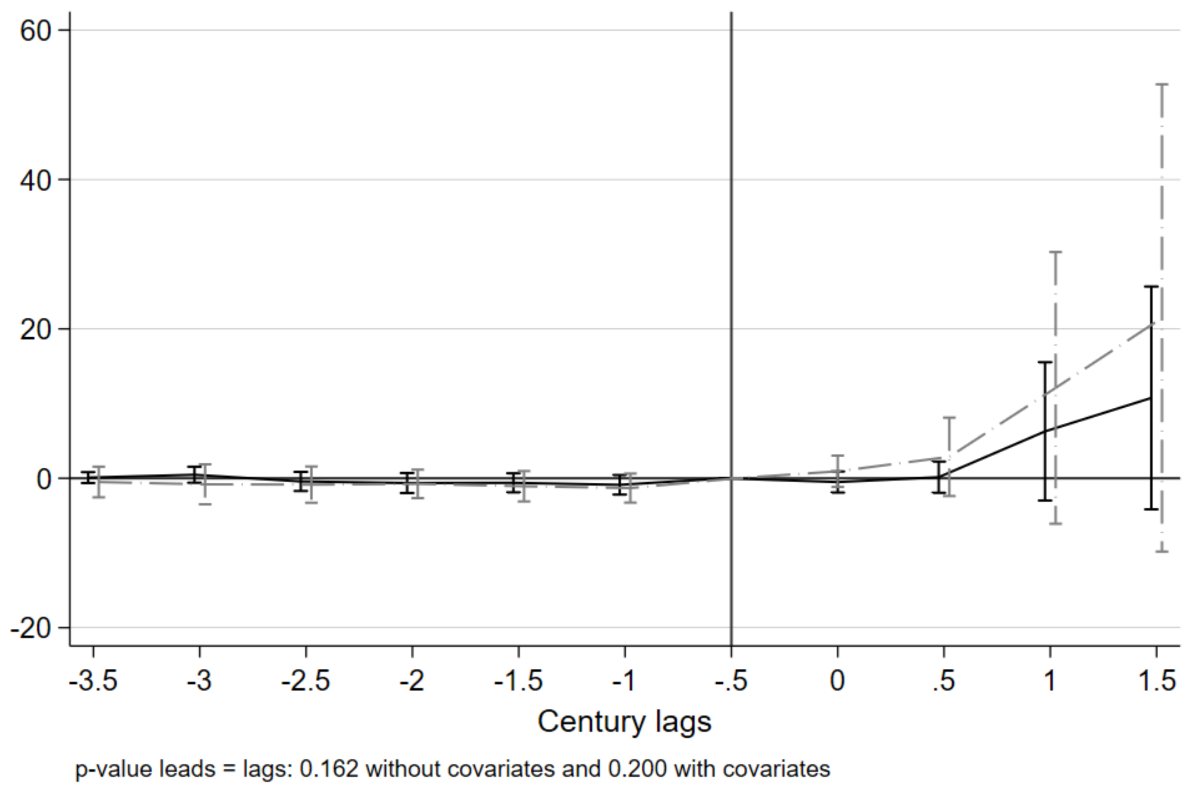
D The role of demand-side factors

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

D.1 Population growth

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

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



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

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

In a second exercise, we consider the possibility that finishing school construction only captures the arrival of the Enlightenment ideas in German cities of the early 17th century. We use data on 317 Enlightenment journals published during 1688–1815 in 99 cities of Germany from Akademie der Wissenschaften (2018).[4] 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.[5] 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.

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

	$\mathbb{I}[Women > 0]$		log Women		Share Women	
	(1) Pre Industrial Revolution ($\leq$ 1750)	(2) Early Industrial Revolution ($\geq$ 1800)	(3) Pre Industrial Revolution ($\leq$ 1750)	(4) Early Industrial Revolution ($\geq$ 1800)	(5) Pre Industrial Revolution ($\leq$ 1750)	(6) Early Industrial Revolution ($\geq$ 1800)
Panel A: Non-Noble Seculars						
Finishing school$_{it}$	0.117***	0.159***	0.263**	0.188***	0.027***	0.010
	(0.043)	(0.040)	(0.108)	(0.048)	(0.009)	(0.007)
Mean, untreated	0.142	0.145	0.130	0.132	0.026	0.026
Unit trend	Yes	Yes	Yes	Yes	Yes	Yes
City covariates × 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 and 1800–1850 to assess the sensitivity of our results to schools whose establishment is potentially influenced by rising demand for women's education in the context of the Industrial Revolution in Germany. We consider three types of dependent variables to capture the extensive and intensive margin of the birth of notable women. $\mathbb{I}[Women > 0]$ is an indicator equal to one if a city had at least one notable woman born in this period. 'log Women' constitutes the natural logarithm of the number of women born plus one. 'Share Women' denotes the number of women by the number of men and women in the same category. In all columns we interact city controls with period fixed effects to capture variation from economic, religious, and educational differences. Standard errors clustered by city shown in parentheses. $* p < 0.10$, $** p < 0.05$, $*** p < 0.01$

E Spatial dependence and SUTVA

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

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

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

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

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

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

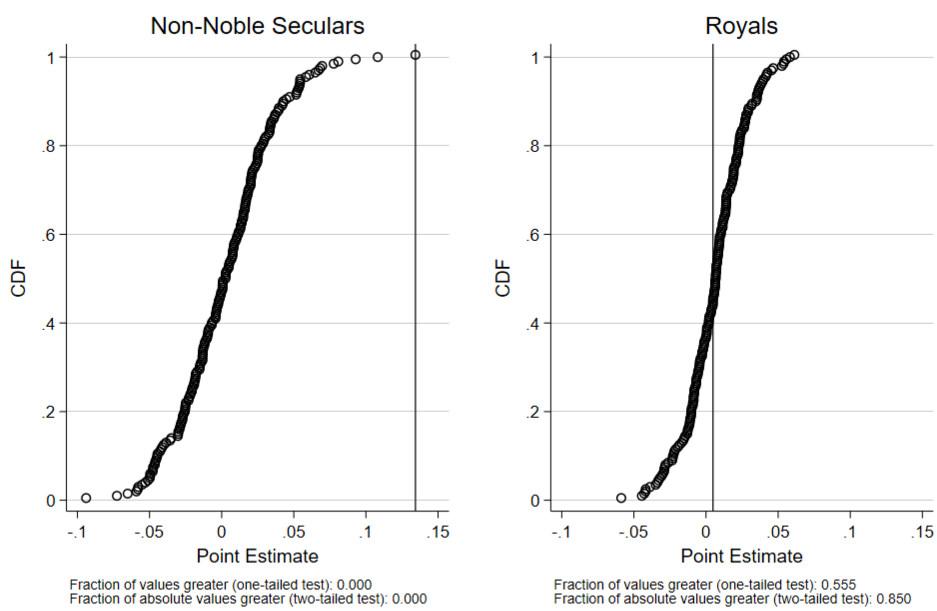
Main results using a fixed-effects estimation and all cities in all periods, with standard errors corrected for spatial dependence within 100km as in Hsiang et al. (2013). We consider three types of dependent variables to capture the extensive and intensive margin of the birth of notable women. $\mathbb{I}[Women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

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

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

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

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



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

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

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

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

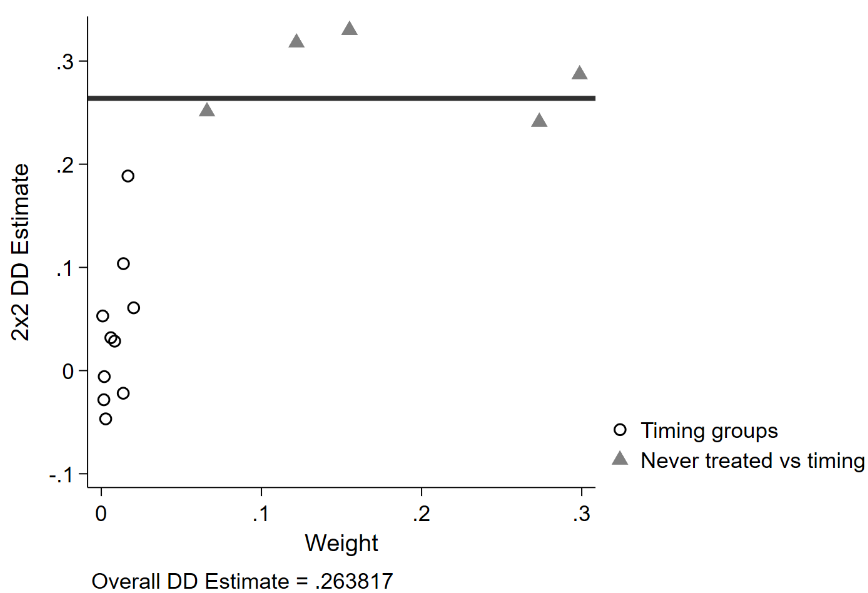
Another way to assess the validity of our approach is by estimating the implied weight of each treatment period. In a classical event study design where one focuses on cities that ever establish treatment, late treatment cities are the implied control cities for early treatment cities. (Goodman-Bacon,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

Figure F.1 suggests that the point estimate in our TWFE estimation stems from the difference between never-treated cities and cities with finishing schools. We thus provide additional evidence for the parallel trends assumption including all cities in Figure F.2 using the methods suggested by Callaway and Sant'Anna (2021). The left panel suggests that the average treatment effect is nearly homogeneous across groups, the only outlier being the treatment group 1800. Homogeneous treatment effects are a prerequisite for satisfying the parallel trends assumption. The right panel applies their method to the inclusion of all cities and shows perfectly flat trends prior to treatment (Periods to Treatment<0), with an immediate increase in the periods thereafter. In our main Figure 2, we show parallel trends in the set of cities that ever established finishing schools. In Figure F.3, we complement this evidence by including cities that never established a finishing school. The results speak in favor of the parallel trends assumption: When controlling extensively for economic, religious and educational covariates, the estimated leads are centered around zero and show no difference between cities with and without finishing schools.

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

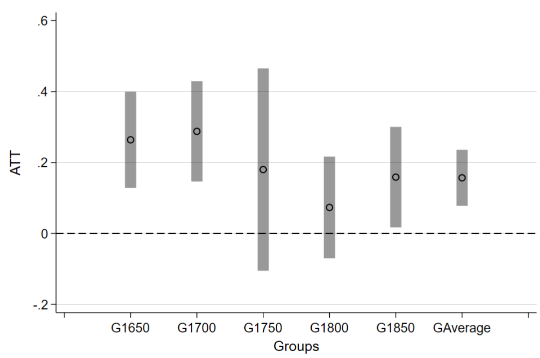
	$\mathbb{I}[Women > 0]$	
	Weight	Av. DID Est.
Timing Groups	0.085	0.093
Treatment vs Never treated	0.915	0.281
Difference-in-differences estimate:		0.264

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

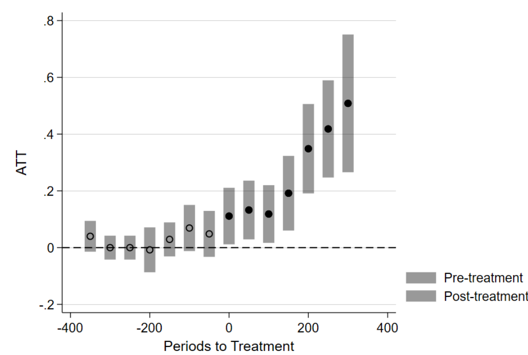


Showing the implied weights against the treatment effect when using the indicator $\mathbb{I}[Women > 0]$. The treatment effect is amost entirely estimated from the comparison of treated cities to non-treated cities.

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

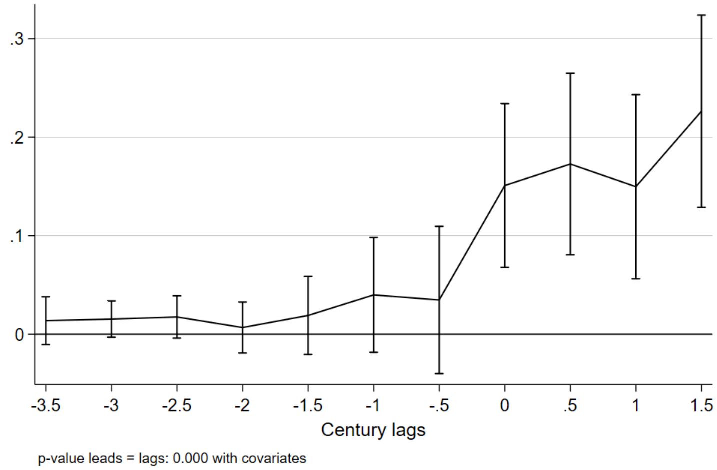


(a) Group specific ATT

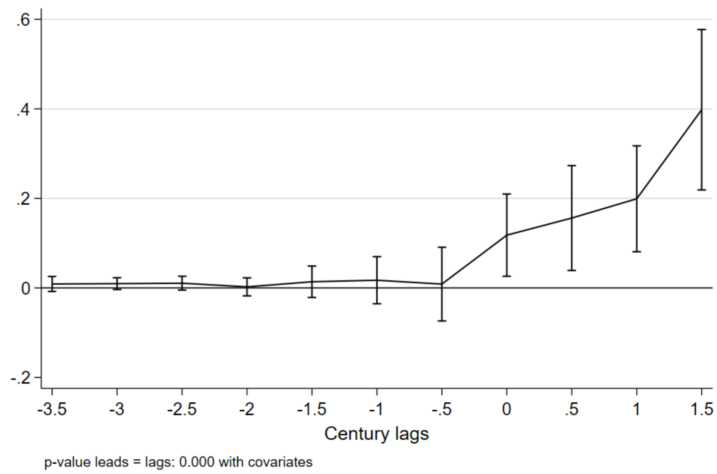


(b) Event specific ATT

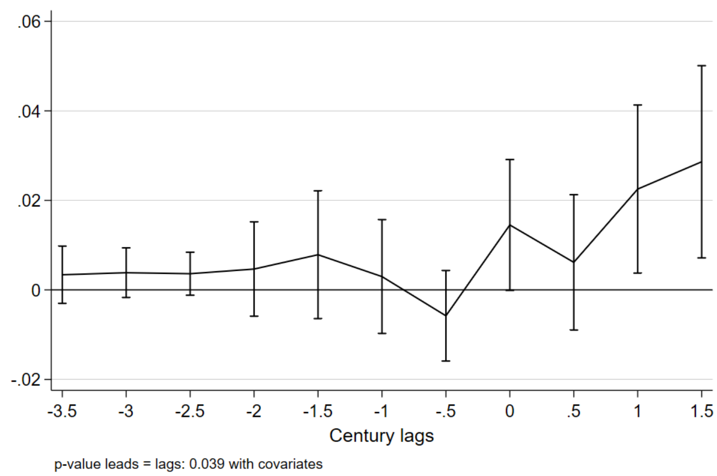
Figure F.3: Event-Study: Impact of finishing school establishment on notable women



(a) Indicator funtion: Notable woman born in city



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



(c) Share of notable women born in city

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

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

In this Appendix, we show results from a standard difference-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 difference-in-differences

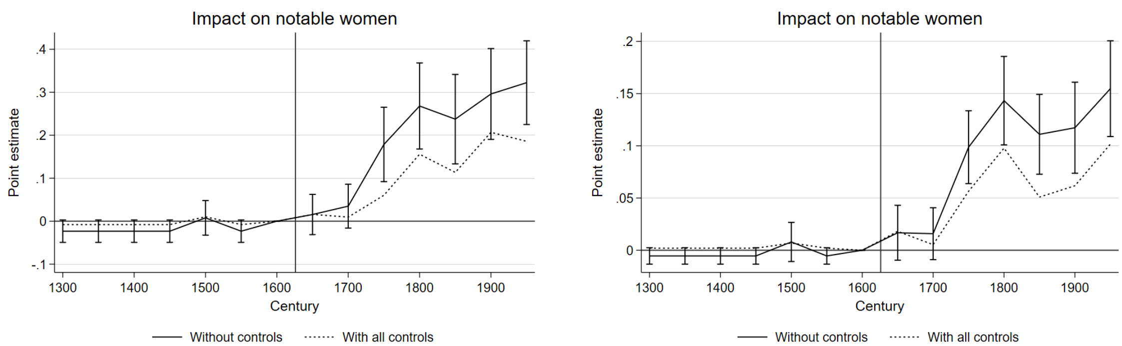
We start by splitting our sample into cities that established finishing schools by 1850 and cities which did not and compare women's representation among the human capital elite in these two sets of cities before and after 1650, the period in which the first finishing school was founded. While this strategy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

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

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

The effects in Table G.2 also indicate that the main effect in our baseline estimate is not driven by unobserved characteristics of the set of cities ever receiving finishing schools, which generally affect women's representation among the human capital elite in these cities after 1600. The 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: Difference-in-differences estimation: Comparing cities with and without finishing schools over time



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

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

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

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

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

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

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

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

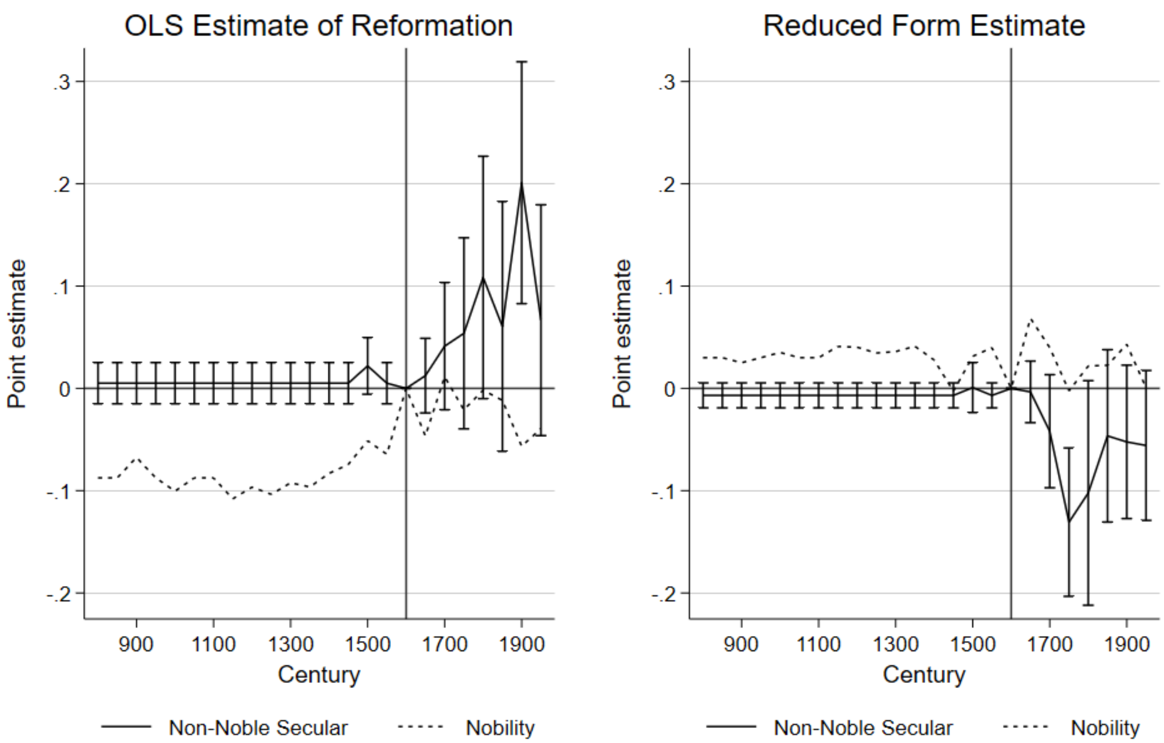
G.2 Protestantism as a confounding factor

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

We assess the impact of the Protestant Reformation on women's representation among the human capital elite in Figure G.2. In the right-hand Panel, we report estimates from an OLS regression of an indicator whether a notable woman was born in a given city and period on an indicator for whether a certain city adopted Protestantism by 1650. The lead-lag estimates suggest no consistently significant and positive effect of the Protestant Reformation on women's representation among the human capital elite until 1900. In the right hand Panel, we report estimates from a reduced form exercise where we replace the indicator for having adopted Protestantism by 1650 with the distance to Wittenberg, the city from which Protestantism spread across Germany. Again, we find no consistent positive effect on notable women. Taken together, Figure G.2 suggests that our main results on the nexus between finishing schools and women's increasing representation among the human capital elite are unlikely to merely reflect the effects of the Protestant Reformation. The difference-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.[6]

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

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



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

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

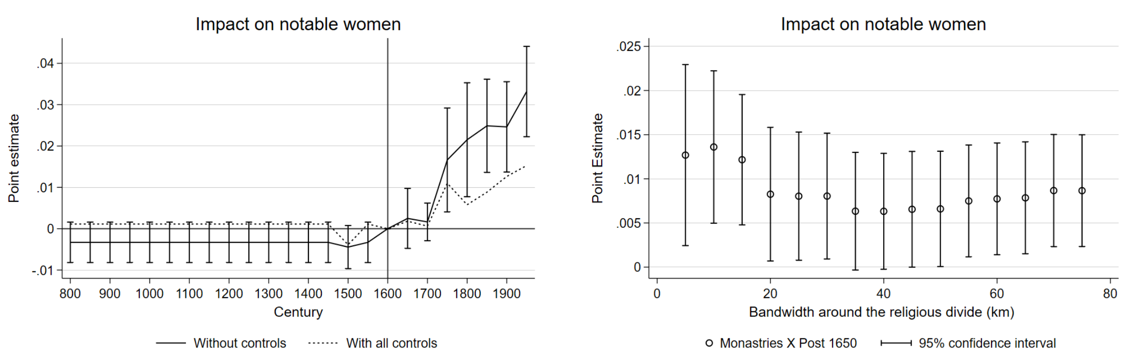
	$\mathbb{I}[Women > 0]$		log Women		Share Women	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Non-Noble Seculars						
Reformation in City × Post 1650	0.057***		0.057**		0.003	
	(0.020)		(0.027)		(0.004)	
log Distance to Wittenberg × Post 1650		-0.056***		-0.053*		-0.003
		(0.016)		(0.029)		(0.003)
Panel B: Nobility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

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

G.3 Monasteries as an instrument

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

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



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

H Specification and robustness in the cross-sectional setting

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

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

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

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

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

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

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

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

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

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

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

H.1 Comparison to propensity score matching

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

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

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

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

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

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

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

I Impact of notable women in 1800 on social change

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

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

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

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

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

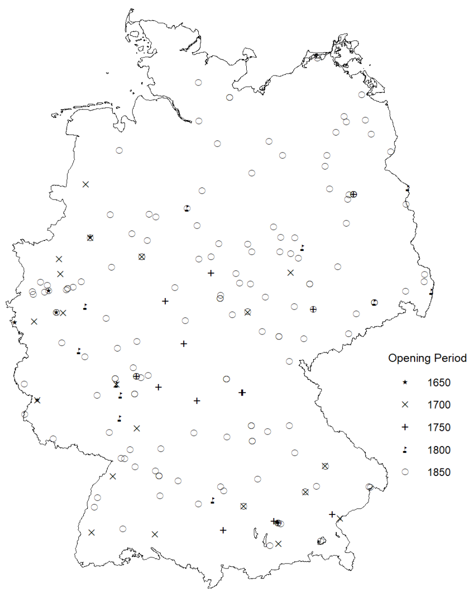
Both extensively controlling for confounding factors and instrumenting non-noble secular women by historical finishing schools suggest that a larger representation of women among the human capital elite increases women's political activity. Yet, as neither finishing schools nor non-noble secular women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

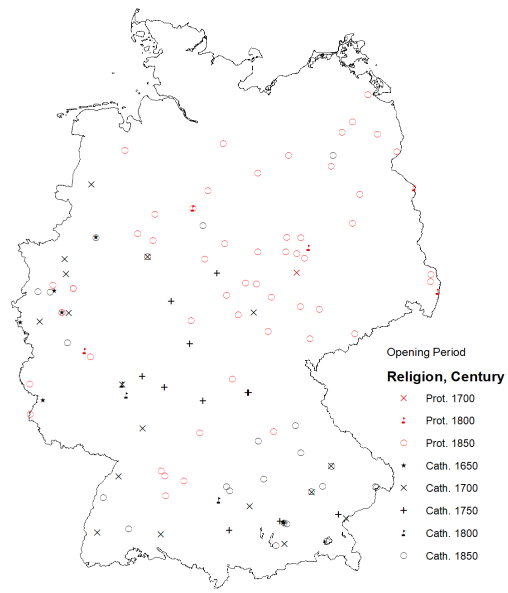
Table I.1: Impact of notable women in 1800 on social change

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

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

J Additional history on finishing schools





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

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

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

J.1 Pupils and Parents at finishing schools

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

There are two motives for parents to send their female children to these schools. First, parents wanted to increase the marriage chances of their female offspring, as the traits learned would be valuable in the higher strata of society. “It is not fitting for an educated man to marry an uneducated woman, and consequently also not from a social sphere where none is to be expected” (Rousseau, 1762).[7] Alternatively, parents wanted to afford their female offspring similar opportunities that had been afforded to them. The dedication of some parents to fund their own schools could be interpreted in this direction. It is however impossible for us to differentiate these two motives for parents.

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

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

Table J.1: Biographies of pupils

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

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

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

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