

LEARNING TO PARTICIPATE: THE EFFECTS OF THE FIRST FEMALE TEACHERS*

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Abstract

How does opening a profession to a previously excluded group shape the next generation? From 1859, Sweden allowed women to teach in elementary schools. We digitize teacher registers and 350,000 signatures from the 1913–14 suffrage petition, linking them to full-count censuses. Exploiting the staggered timing of districts' first female hire, we show that women exposed to female teachers as schoolgirls were 23 percent more likely to sign the petition and 9 percent more likely to hold a non-domestic occupation. IV estimates using nearby women's teacher seminaries suggest early female-hiring districts also had stronger suffrage organizations and more women voting in person.

Keywords: FEMALE TEACHERS, WOMEN'S SUFFRAGE, OCCUPATIONAL MOBILITY

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1. INTRODUCTION

How do historically marginalized groups gain access to public, political, and economic life? Formal rights are one part of the answer, but they often arrive late. Long before legal equality, members of such groups may enter particular institutions, as teachers, clerks, organizers, or professionals, and so become visible examples of alternative social roles. This early representation can matter not only for those who hold the positions, but also for the next generation who grow up seeing them filled.

This paper studies this mechanism in the context of women's emancipation in Sweden. By women's emancipation, we mean their movement into political and economic life beyond the household, which we measure through participation in the suffrage movement and entry into non-domestic work. In the second half of the nineteenth century, women remained legally, politically, and economically constrained, even as industrialization, mass education, and associational life expanded the public sphere ([Amatller and Lindvall, 2024](#); [Cermeño et al., 2022](#); [García-Jimeno et al., 2022](#); [Melander, 2020](#)). Teaching was among the first public professions opened to them. After women gained access to teacher training in 1859, female teachers gradually spread through elementary schools across the country, placing educated working women in positions of authority, before universal suffrage and before most white-collar occupations were open to women.

Women's place in Swedish society grew increasingly contested over these decades. The country industrialized late but modernized quickly, through land reform from 1827, freedom of commerce in 1864, and the dissolution of the Estates in 1866, even as widespread poverty pushed roughly one million Swedes to emigrate to North America by the 1920s ([Edvinsson, 2017](#); [Karadja and Prawitz, 2019](#)). At the same time, a rising share of never-married women gave new urgency to the woman question ([Johnson and Robson, 2022](#); [Lundh, 2003](#)). The intellectual case for women's rights had been made well beyond Sweden: [Condorcet \(1790\)](#) argued, in translation, that women "necessarily have equal rights"; [Wollstonecraft \(1792\)](#) insisted that women should have power "over themselves"; and [Mill \(1869\)](#) called legal subordination by sex "wrong in itself". Such arguments helped shape the intellectual foundations of first-wave feminism ([Block, 2009](#); [Humm, 1995](#)), and at home contemporary fiction made them concrete, from [Almqvist's *Det går an*](#) on women's economic independence to [Bremer's *Hertha*](#) on women's education and legal autonomy ([Almqvist, 1839](#); [Bremer, 1856](#)). Against this background, mid-century reforms in women's education paved the way for the publicly engaged women who would form the core of the Swedish women's rights movement, culminating in the 1918–1919 suffrage reform and the first parliamentary election under

equal suffrage in 1921.¹

Our empirical setting is the entry of the first generation of women into Swedish elementary-school teaching. Until 1859 women were effectively barred from certified posts; the royal decree of that year admitted them, and the state had to build a training system for female teachers almost from scratch. Rather than open the existing seminaries to women, the government converted a few of the country's twelve diocesan teacher seminaries into all-female institutions and left the rest all-male, a split shaped by regional politics and contemporary objections to coeducation rather than by local demand for female teachers ([Folkundervisningskommittén, 1912](#)). Because newly certified women tended to take posts near the seminary that trained them, this produced sharp and plausibly arbitrary differences across regions in the supply of female teachers, so that female teachers reached some school districts decades earlier than others. Women rose from 3.6 percent of the teaching workforce in 1862 to 35.6 percent by 1910 ([Florin, 1987](#)). We exploit this staggered, supply-driven entry to identify the effect of childhood exposure to a female teacher.

Our analysis asks whether opening the teaching profession affected not only the women who entered it but also their female pupils. Such spillovers would create an emancipation multiplier: each woman entering teaching could influence many girls. We focus on women born between 1840 and 1880, who came of age during the emergence of first-wave feminism and experienced gradual expansions in their legal rights, educational opportunities, and social freedoms.

Answering the question of how exposure to female teachers shaped women's political and economic lives requires both new data and a credible empirical strategy. On the data side, we need to measure political and labor-market participation for women born in the middle and late nineteenth century, link these individuals to the school districts they grew up in, and identify the presence of female teachers during their school years. On the identification side, we need an empirical strategy that isolates the causal effect of exposure to female teachers on individual-level outcomes.

To address the first challenge, we develop a novel measure of female political participation before universal suffrage by matching 350,000 handwritten signatures from a 1913–14 mass petition organized by the National Association for Women's Suffrage, NAWS (*Landsföreningen för kvinnans politiska rösträtt, LKPR*), to the full-count 1910 census. Labor-market outcomes are measured using occupational titles in the 1880–1910 decennial censuses. We then construct a digital map of school districts and birth locations, which enables us to link individuals to their childhood school district.

¹The March 1919 municipal elections were the first nationwide elections held under equal-suffrage rules; nearly all women over age 27 could vote.

Finally, we digitize detailed teacher registers from 1876, 1882, and 1889, which include names and years of appointment for all elementary school teachers.

Combining these observations nested within school districts and birth cohorts, we use a difference-in-differences design that exploits the staggered entry of female teachers across school districts. As described above, this entry was driven largely by the plausibly arbitrary division of teacher seminaries into all-male and all-female institutions rather than by local demand.

In our empirical analysis, we show that exposure to female teachers during childhood contributed to women's political engagement and reshaped their position in the labor market. Compared with women from cohorts and localities with only male teachers, exposed women were more likely to openly support equal suffrage and more likely to enter non-domestic work. The estimated effect on petition signing is about 2.3 percentage points, corresponding to roughly a 23 percent increase relative to the baseline. The effect on non-domestic work is about 1.1 percentage points, or around 9 percent relative to the baseline. We find no corresponding effect on aggregate labor supply, indicating that female teachers did not primarily increase whether women worked, but rather shifted the type of work they entered. The labor-market effects are concentrated in professional and clerical occupations, suggesting that exposure to female teachers changed women's occupational aspirations and opportunities.

To interpret these results, we emphasize that female teachers were a novelty in patriarchal nineteenth-century Swedish society. They embodied a rejection of pre-industrial gender norms by having a respected occupation with employment protection and a guaranteed minimum wage ([Florin and Kvarnström, 2001](#); [Karlsson Sjögren, 2006](#); [Qvist, 1960](#)). For schoolgirls, exposure to a female teacher during their school years may therefore have made work outside the household, or the combination of career and marriage, appear more attainable. This role-model mechanism can be contrasted with an alternative: the productivity channel.

Both theory and empirical findings suggest that female teachers in this era may have been more effective instructors than their male counterparts ([Marklund, 2021](#)). To distinguish between these channels, we compare the effects female teachers had on girls and boys separately, using the effect on boys as a natural benchmark for the general human-capital channel. We find that boys also benefited from having a female teacher, but the productivity channel alone cannot explain the full effect on girls.

Turning to demographic mediators, we find no effect on the probability of marriage. This result may appear puzzling in light of the labor-market effects, since it was still unusual for married women to work. A conclusive analysis of marriage patterns is not possible, however, because it requires richer demographic data, in particular age at

marriage. Another aspect of marriage that we can study is equality between spouses, as measured by their age difference (Dribe and Stanfors, 2017; Roterberg and Bras, 2019). We estimate that women who had a female teacher and eventually married had slightly younger husbands, indicating more equal marriages. We also analyze effects on migration within Sweden and find no effects on moving either to a city or to Stockholm.

The analysis concludes by examining the political influence of the first female teachers. Building on Florin (1987); Rönnbäck (2004), we first document teachers' overrepresentation throughout the women's rights movement. We then use regional access to female teacher seminaries as an instrument for early female-teacher hiring. Predicted hiring is associated with more petitioning and higher female turnout in 1919, in line with the event-study evidence. The exclusion restriction is demanding, however, because seminaries may have influenced local politics through channels other than female teachers. We therefore interpret the IV estimates as suggestive corroborating evidence, not as unbiased causal effects. Reduced-form estimates relate seminary access directly to political outcomes without attributing the relationship solely to female teachers; they point in the same direction but are smaller (Appendix Table D.9, column 2).

Related Literature. We contribute most directly to the literature on the effects of female teachers on female students. de Gendre et al. (2024) provide a meta-analysis of 90 studies, documenting substantial heterogeneity in estimated same-sex teacher effects as well as evidence of publication bias; in their own cross-country analysis, they find small average effects on student performance but larger effects on job preferences, subject confidence, and subject enjoyment. Most existing work studies contemporary settings and short-run educational outcomes. We instead study a historical setting in which female teachers were a novelty, and examine long-run outcomes linked to women's emancipation. Evidence from low-income countries with more traditional gender norms (settings closer to nineteenth-century Sweden than are today's high-income countries) generally finds positive short-run effects of female teachers on girls' educational outcomes (Lee et al., 2019; Muralidharan and Sheth, 2016).

The paper closest to ours is Card et al. (2022), who study the long-run effects of female teachers on female students in 1940s rural United States. Using several fixed-effects strategies, they find that girls living near a female teacher attained more education, earned higher incomes, and lived longer. Like us, they emphasize female teachers as role models. Our setting differs in two ways. First, we study the initial entry of women into teaching, rather than a period in which women were already the majority of teachers. In their setting, 72 percent of teachers were women. Second, we focus on outcomes tied to women's political and economic lives, rather than educational

attainment and lifetime wellbeing.

We also contribute to the historical literature on the feminization of teaching. Comparative work shows that feminization was a nearly universal feature of national public-school systems, although countries and regions followed different trajectories depending on economic and political conditions (Albisetti, 1993). The Swedish experience is documented by Florin (1987, 1991). Closely related, Cappelli and Quiroga Valle (2021) study the feminization of elementary-school teaching in Italy and Spain between 1861 and 1921. Using provincial data from 69 Italian and 49 Spanish provinces, they show that areas with more female teachers also had higher girls' enrollment. This literature has primarily explained why teaching feminized and why the extent of feminization varied across places. We extend it by estimating the long-run consequences of feminization for the pupils exposed to female teachers.

A central contribution is to link the feminization of teaching to women's political mobilization. We thus contribute to a growing literature in economics and political science that uses historical political data to study first-wave feminism, the women's rights movement, and the consequences of female enfranchisement (Bühler and Sabet, 2026; Carpenter et al., 2018; Kose et al., 2021; Miller, 2008; Morgan-Collins, 2024; Teele, 2019). A distinctive feature of our setting is that we measure women's political participation before universal suffrage at the population level. This allows us to study the determinants of political mobilization before women became voters. To our knowledge, we provide the first quasi-experimental evidence linking female teachers to women's political participation and support for suffrage. In doing so, we test a hypothesis developed more descriptively in historical work on Sweden (Björkenlid, 1982; Björk, 1999; Johannesson, 1989; Rönnbäck, 2004), the United States (Leroux, 2006; Weiler, 1989), and the United Kingdom (Gorno, 2023; Oram, 2002). Related work shows that political participation could itself be transformative, both through suffrage campaigns (Morgan-Collins and Rueda, 2025) and through the acquisition of voting rights (Slotwinski and Stutzer, 2023).

We further contribute to the literature on female labor-force participation in the nineteenth and early twentieth centuries (Berger et al., 2024; Eriksson, 2015; Fernández, 2013; Goldin, 1990, 2021; Molinder, 2022). This literature emphasizes structural change, household income, marriage, fertility, and persistent gender norms. Goldin's work is foundational in documenting the long-run transformation of women's work and the constraints created by marriage, fertility, and the organization of work. Fernández (2013) develops a model in which beliefs about the returns to women's work evolve through intergenerational learning from mothers and from observing women in the mother's generation. We add early-life exposure to female role models as another determinant of women's labor-market trajectories and provide evidence on a specific channel through

which norms about women’s work could change.

Taken together, our results speak to broader research on women’s rights and the emancipation of historically marginalized groups (Althoff and Reichardt, 2024; Bühler et al., 2024; Nekoei and Sinn, 2021; Tertilt et al., 2022). This literature studies both the origins and consequences of institutions, rights, and social norms. Tertilt et al. (2022) provide a typology of women’s rights and summarize theoretical and empirical approaches to understanding their expansion. Althoff and Reichardt (2024) show how post-slavery institutions shaped the intergenerational outcomes of Black Americans. Bühler et al. (2024) show that expanding educational opportunities for women in Germany increased their representation among the human-capital elite and helped generate social activism. We contribute by providing causal evidence from an early inflection point in women’s rights on a specific institutional channel: the entry of women into a central public profession. The findings show that representation inside public institutions can matter beyond the immediate services those institutions provide. Female teachers did not only transmit human capital. They made new social roles visible to girls at a formative age, with consequences for later political participation and labor-market choices. The feminization of teaching was therefore not simply an outcome of women’s emancipation. It was also a force that helped produce it.

Outline. The remainder of the paper is organized as follows. Section 2 provides historical context, Section 3 describes the data, Section 4 presents our empirical strategy and main results, Section 5 examines the role of female teachers as political organizers, and Section 6 concludes.

2. BACKGROUND

2.1. THE EXPANSION OF PRIMARY EDUCATION

In eighteenth-century Sweden, primary education was largely provided at home by male household heads. Under the Church Law of 1686, clergy examined children in reading, writing, and numeracy, primarily to ensure that they could read and understand religious texts. By the early nineteenth century, about 85 percent of the population had some reading ability (Nilsson, 1999). Formal schooling remained limited, however. In the 1820s, as few as 10 percent of children attended schools outside the home, and before the 1840s most schools were concentrated in the Diocese of Lund in southern Sweden (Klose, 2011; Ljungberg and Nilsson, 2009).

The School Act of 1842 fundamentally transformed Swedish primary education. It required all 2,308 parishes to belong to a school district operating at least one school.

These schools taught not only reading, writing, and numeracy, but also subjects such as history, geography, and biology. Table 1 documents the rapid expansion of the system. Between 1839 and 1868, the number of schools increased from 1,516 to 6,919, while the number of teachers rose from 1,537 to 7,045. These figures include elementary-school teachers for children aged 9–14 and junior-school teachers for children aged 7–8.

Local parishes were responsible for financing primary schools, while state subsidies remained limited.² This decentralized arrangement generated substantial variation in school access and quality (Andersson and Berger, 2019). The act also established a national minimum wage for teachers and provided state funding for teacher seminaries (*folkskoleseminarier*). It required one seminary in each of the twelve diocesan capitals, except in the Diocese of Uppsala, which was to have two: one in Uppsala and one in Stockholm. Overseen by the bishop, the seminaries provided training in both spiritual and secular subjects, including teaching methods. The program initially lasted one to three semesters, but was extended to six semesters in 1862 and eight semesters in 1878.

[Table 1 about here.]

2.2. THE FEMINIZATION OF TEACHING

In the first two decades following the 1842 School Act, women were formally excluded from teaching positions unless granted state dispensation, which only a dozen women received before 1860 (Spetze, 1992; Wilmenius, 1999). However, there was a severe shortage of teachers nationwide, particularly in rural areas. On the initiative of the educational reformer Torsten Rudenschöld, the first two years of elementary schooling, which became known as *Småskolan* (literally, little school), were exempted from the requirement for a certified teacher. These minor and junior schools were swiftly staffed primarily by women starting in the late 1850s (Florin, 1987). After lively debates in parliament (Wilmenius, 1999, p.36–37), a royal decree issued in 1859 opened certified teaching roles for women for the remaining five to six years of elementary schooling. Consequently, the government had to establish teacher seminaries for women. Although coeducation was considered, only the Stockholm and Härnösand consistories supported it; all others favored separate seminaries for men and women (Folkundervisningskommittén, 1912, pp. 64–65).

In the fall of 1860, the government decided to convert the seminaries in Skara, Strängnäs, and Kalmar into female-only institutions. These cities were selected partly to ensure regional balance. However, the ongoing debate about reducing the number

²Initially, subsidies were available only to poor districts. A per capita state subsidy was introduced in 1845, and the system was substantially reformed in 1871 (Westberg, 2017).

of seminaries persisted, leading to a parliamentary agreement in 1864. Initially, the proposal suggested maintaining one male seminary in each of the five major regions, east, west, south, north, and central, with only one female seminary for all of Sweden. To appease Parliament, the government retained an additional male seminary in the south, Växjö, and transformed the seminary in Stockholm into a female-only institution by 1865. However, the female seminaries in Kalmar and Strängnäs, along with those in Visby and Västerås, were short-lived, closing down in 1866 and 1867. The Karlstad seminary, also about to close, was saved by a parliamentary initiative. In the 1870s, three new seminaries, all female, were established: in Falun and in Kalmar, which reopened, in 1875, and in Umeå in 1879 ([Folkundervisningskommittén, 1912](#)).

The overall impact of these reforms is depicted in Figure 1. In 1862, there were 113 female teachers and 3,155 male teachers, meaning that women constituted 3.6 percent of the teaching workforce. Their share rose to 24.5 percent in 1892 and 35.6 percent in 1910. Figure A.6A shows the same trend among seminary graduates.³

[Figure 1 about here.]

The minimum wage was gender-neutral and nationally determined. Although in most cities average wages were higher for men than for women, this was particularly true for married men, who often received a raise in order to support a family. In 1906, a national differentiation in minimum wages was introduced: the entry wage remained the same, but men had steeper wage progression. Employment protection and pension arrangements were also roughly equal. This was important primarily for female teachers who got married or had children ([Persson, 2008](#), p. 250).⁴

An important feature of the early primary education system was that boys and girls were educated together. Coeducation was, however, largely a necessity rather than a preferred order ([Albisetti, 1993](#)). In many school districts, large geographic distances and small cohorts made it impractical to maintain separate classes or school buildings for boys and girls. Where pupil numbers were sufficiently large, especially in cities, primary education was commonly organized into separate boys' and girls' schools ([Hamberger, 2026](#)). Female teachers therefore taught both in coeducational classes and

³Women also taught in private girls' schools (*flickskolor*). Appendix Figure A.6C documents their expansion after the 1860s. Many were founded by graduates of female seminaries or the Royal Advanced Female Teachers' Seminary, and state support began in 1873 ([Barrio de López, 2002](#)). Before 1859, female educators were mainly unmarried governesses serving affluent families (Appendix Figure A.6D).

⁴Stockholm debated whether school boards could dismiss teachers upon marriage in 1893, following an 1885 parliamentary motion and a similar proposal to the church assembly. Neither passed. Supporters argued that boards otherwise had to provide maternity substitutes. At the time, 49 of Stockholm's 297 female teachers were married ([Florin, 1987](#); [Hammarlund, 1893](#)).

in girls-only elementary schools, but rarely in boys-only schools.⁵

2.3. WOMEN'S POLITICAL PARTICIPATION BEFORE SUFFRAGE

Swedish electoral politics began in the 1860s, when the parliament of the estates was replaced with a bicameral parliament and a secular municipal governance structure was introduced (Karlsson Sjögren, 2006).

For the second chamber in parliament, voting rights were exclusively male. For municipalities, which indirectly appointed the first chamber, the electorate was made up of taxpayers, with the number of votes proportional to income. Thus, for instance, widows with their own tax bill could participate in elections as long as all other requirements were fulfilled. Participation was, however, very low in absolute numbers. The first available statistic for Stockholm is from 1880, when a total of 9 women voted.

A campaign was organized in 1887 to promote participation, mainly by providing information about eligibility. Approximately 4,000 women were eligible, and in 1887 participation rose from around 100 to 400.⁶ The campaign was organized by the Association for Married Women's Property Rights (*Föreningen för gift kvinnas äganderätt*), but was only sustained in 1888 and 1889. By 1900, turnout in Stockholm had fallen back to around 100 women, which constituted 3 percent of all votes cast and only 1.7 percent of women eligible to vote, see Figure 2C. Although turnout among eligible women was low, the minimum wealth and income requirements for voting made it hard for women to qualify. In the nation as a whole, about 4 percent of adult women could vote in municipal elections around 1900 (Kungliga Statistiska Centralbyrån, 1908).

Turnout increased substantially in Stockholm around 1907 and 1908, when participation rose from around 1,000 to 2,500, or from 6 to 21 percent. Before that, women had also taken the first step toward enfranchisement when a new law passed in 1889 allowed women to be elected to local poverty and school boards, the first time women could serve as democratically elected public servants. The reform was hotly debated and passed narrowly: 59–57 in the first chamber and 99–80 in the second.

The 1909 limited suffrage reform brought many changes to the election apparatus: a change from first-past-the-post to proportional representation (Asker, 2007), the abolition of income requirements for national elections, and a cap on the maximum number of votes per person in local elections. For women, however, little changed in their voting rights. Instead, a universal right to run for municipal and city councils was introduced. The cumulative number of distinct women who had served on a city

⁵Appendix G discusses gender-segregated elementary schools, how we classify school districts as coeducational or segregated, and the implications for the analysis.

⁶Turnout figures are from the *Statistical Yearbook of Stockholm*.

council is plotted in Figure 2A.

Electoral participation by women was therefore very limited for institutional reasons, but other forms of political action and participation were possible in civil society organizations. Historical narratives often point to a bill put forward by the conservative government in 1902 as the triggering factor of an organized suffrage movement (Rönnbäck, 2004, p. 58–59; Ruiz and Marín, 2012, p. 247). The bill proposed that men who were either married or over 40 years old should have two votes in national elections, since, in marriage, they could represent their wives, and with age came wisdom. In response, a Stockholm-based group of women founded the Society for Women's Suffrage (*Föreningen för kvinnans politiska rösträtt*). Shortly afterward, other groups of women formed local suffrage societies around the country. Finally, in 1903, these societies joined together to form the NAWS. The NAWS became the first and only national suffrage organization that, on paper, was party neutral. The NAWS was a peaceful organization, unlike the British suffragettes, whose tactics of direct action and civil disobedience they regarded as neither effective nor morally appropriate. By primarily engaging with public opinion through media, information campaigns, petitions, and courtship of political leaders, they signaled women's demand for suffrage to both the public and the elite. In 1902, full suffrage for women was still a radical proposition. Although debated in more countries, only Australia, New Zealand, the Isle of Man, the Cook Islands, and a few western states in the United States had implemented it (Teele, 2019).

The organization branched out from Stockholm rapidly and had 40 local sections and 4,000 members only two years later. By 1918, it had grown to approximately 220 local sections and 16,000 members (Björkenlid, 1982). Liberal and upper-class women dominated the organization, although it was formally open to women of all classes and political affiliations. Similar to most organizations of popular movements at that time, it had a hierarchical structure: a small female elite of about 25 well-known academics, journalists, writers, and activists guided the organization's activities (Rönnbäck, 2004, p. 69). They encouraged regular meetings across the local chapters, held plenary meetings for all local sections at least once every year, and published the journal *Rösträtt för kvinnor* (*Suffrage for Women*) more than once a month. In so doing, they helped build a robust popular movement spanning all regions of Sweden. The organization also enabled a selection of activists to spend full-time traveling, agitating, courting male politicians, and engaging with public opinion.

[Figure 2 about here.]

Petitioning for Suffrage. In 1906, the NAWS organized its first mass petition in favor of women's suffrage. The stated goal was to counter a common critique of the movement:

that women were generally uninterested in politics and did not want the vote. The initiative was modeled on the 1898 mass petition for expanded suffrage, which was signed by 301,054 men and 62,584 women. Although the 1898 petition was framed as a general expansion of suffrage, it was not understood by contemporaries as a call for gender-neutral voting rights. Mass petitioning had become a popular political strategy in the first half of the nineteenth century ([Huzzey and Miller, 2024](#)); for Sweden, see [Harvard \(2006, p. 237\)](#). The 1906 petition gathered 142,128 signatures, corresponding to 8.13 percent of women aged 18 and above.

Following the disappointing suffrage reform of 1909, which made no concession on women's voting rights, the NAWS began planning a more decisive demonstration of support ahead of the 1914 election.⁷ In the summer of 1912, plans were laid for a substantially larger petition, with explicit efforts to achieve nationwide coverage. The earlier petition had primarily reached women in regions surrounding the three largest cities, which accounted for 44 percent of signatures.

Once the campaign started, signatures were primarily collected by volunteers from local NAWS chapters during 1913–14. The NAWS also collaborated with other groups supporting women's suffrage, particularly the temperance movement and Social Democratic organizations.⁸ Petition lists were further disseminated through progressive newspapers and women's magazines. In parishes lacking organized women's groups, the NAWS often contacted female teachers or midwives to facilitate collection ([LKPR, 1914, p. 14](#)). There is also anecdotal evidence that some married women were prevented from signing if their husbands opposed the cause ([LKPR, 1914, p. 14](#)).

In total, 351,454 signatures were collected, corresponding to roughly 20 percent of women aged 18 and above in 1914. This makes it the second-largest petition in Swedish political history after the 1898 petition. Since it targeted only women, however, it reached a larger share of its intended population. The petition was submitted to parliament on 2 July 1914, with all sheets bound into 30 volumes.⁹

Each petition list contains handwritten information on the petitioner's first and last name, title, occupation, and address. The collector also recorded the county and locality where the signatures were gathered. See Figure [E.26](#) for an example.

During the First World War, although Sweden remained neutral, the suffrage issue

⁷After 1909, both Liberals and Social Democrats unequivocally supported women's suffrage ([Rönnbäck, 2004, p. 41](#)). In May 1912, the Staaff II government introduced the first government bill for women's suffrage. It passed the second chamber 140–66 but failed in the first 86–58; no Conservative supported it.

⁸The principal temperance ally was the Woman's Christian Temperance Union (*Vita bandet*). The Social Democratic women's clubs were organized nationally in 1920.

⁹These are held at the Swedish National Archives and were digitized in connection with the centennial of the first national election with universal suffrage in 2021.

became less focal. Nevertheless, the NAWS sustained high membership numbers and continued its activities. The breakthrough came in November 1918. Against the backdrop of revolution in Germany, domestic social unrest, and food shortages, Swedish conservatives increasingly feared broader societal breakdown.¹⁰ On December 17, 1918, the government and the opposition agreed to implement universal and equal suffrage for men and women, with one vote per eligible voter. In March and April 1919, men and women could participate in municipal and county elections on equal terms for the first time. In September 1921, the first parliamentary election with universal suffrage was held.

Teachers as Suffragists. The positive relationship between teachers and suffragist organizers has been noted by historians, both in Sweden (Florin, 1987; Rönnbäck, 2004) and the UK (Gorno, 2023; Oram, 2002) and the US (Leroux, 2006), but quantitative evidence is limited. In Figure 3, we present a schematic overview of how teachers were represented, not only in the suffrage organization, but also among the first elected city council members. The included reference categories show that teachers were overrepresented not only in relation to the labor force as a whole, but also among women with professional occupations. The other large category of professional women at this time was nurses and midwives. In The Biographical Dictionary of Swedish Women (SKBL), more than one third of the included women born in the nineteenth century were teachers. Different arguments for the association have been discussed. Weiler (1989) claims that it is the combination of wage labor and the fact that teachers, unlike most other female employees at this time, had jobs that could just as well have been performed by men. This made, according to Weiler, the gender injustices of the time more salient.

[Figure 3 about here.]

2.4. WOMEN'S OCCUPATIONAL STRUCTURE, 1840–1910

In the 1840s, 90 percent of the 3.3 million inhabitants lived in rural areas. Married women usually worked alongside their husbands (Tilly and Scott, 1987). Typically, only wealthy farming families allowed women to concentrate solely on homemaking. A woman inherited a farm only if her husband died, managing it until remarriage or estate settlement. In retail, wives often worked at the counter. Widows could take over legal and financial control of craftsmanship businesses, but this was rare (Qvist, 1960). Unmarried women and young farm girls had limited professional opportunities

¹⁰The relative importance of structural and institutional explanations for the changing position of the Conservative Party is discussed by Berling Åselius (2005, p. 160).

(Hinnemo, 2016). The most common form of employment was to work as a domestic servant and tend to the home.

Between 1846 and 1864, pivotal regulations in factory and craft industries allowed women to enter entrepreneurship on par with men. Teaching rights expanded as described in Section 2.2, starting with minor schools in 1853 and extending to all elementary schools by 1859. Women also gained access to lower-level government jobs in 1859. From 1863, women could work as telephone operators and office clerks at the Swedish Post and Telecom Authority. Women could in theory pursue academic degrees in all fields except law and theology by 1873, but the lack of affordable preparatory schools made this unattainable for most. Moreover, the number of female professional midwives increased significantly from the mid-nineteenth century, and the first nursing college opened in 1867. By the 1880s, women formed the majority in restaurants and cafés. Significant shifts also occurred in the industrial and craft sectors, with women becoming the majority in textile work by 1910.

These shifts can be attributed to institutional and legal reforms enacted between the 1840s and 1860s. Figure A.5 traces reforms expanding women's rights during this period. Between 1842 and 1864, 17 reforms were passed: 9 concerned women's education, while the remainder lowered barriers to entrepreneurship and professional work. This marked a clear departure from the preceding two centuries, when few such reforms were enacted.

[Figure 4 about here.]

Figure 4 shows the change in the distribution of major occupational groups for women from 1880 to 1910, two decades after the aforementioned reforms. In 1880, nearly 75 percent of working women were in the service sector, primarily as household servants. Over 90 percent of them worked as maids or domestic workers in others' homes. Production and agriculture also employed many women, though the agricultural count was likely much higher due to married women not being recorded as agricultural workers. By 1910, a notable shift from 1880 is evident: fewer women were in service roles, while more occupied professional, sales, administrative, managerial, and clerical positions.

These trends signal a growing number of women engaging in careers beyond the household, including factory production, administrative roles in workplaces or offices, and public positions such as midwifery and teaching. In short, they represent a significant shift toward women working in the non-domestic sphere (Carlsson, 1966).

3. DATA

We build a new dataset on the schooling of individuals born in Sweden between 1840 and 1880 by linking full-count teacher registers from 1876, 1882, and 1889 to full-count decennial censuses from 1880 to 1910.¹¹ Throughout, we refer to elementary schools as schools and elementary school teachers as teachers.

Teachers. Our main treatment is an annual indicator for whether a school district employed a female teacher. We construct this indicator from full-count printed teacher registers published in 1876, 1882, and 1889 (Beskow, 1882; Beskow and Schönmeier, 1876; Borgh, 1889).¹² For every teacher, we digitize the first name, year of birth, year of examination, and year of employment in the school district. The 1889 register additionally reports place of birth and place of examination. Figure 5 pairs Bothilda Andersson’s entry in this register with a photograph of Andersson and her pupils. We infer each teacher’s gender from their first name and other information contained in the registers.¹³

Table 2 presents summary statistics on school districts and teachers. From 1876 to 1889, the number of teachers rose from 3,569 to 5,420, with the share of female teachers rising from 8 percent to 21 percent. The percentage of school districts with a female teacher grew from 5.7 percent to 16.6 percent. Female teachers were about 8–10 years younger than male teachers. Their experience, measured by years since graduation,

¹¹Unlike many historical censuses, the Swedish censuses were based on continuously maintained parish registers rather than one-time enumerations by census takers. Local priests recorded births, deaths, marriages, migration, household structure, and occupations in parish books that formed the basis of Sweden’s population statistics. This administrative system produced unusually detailed and reliable demographic information, including precise birth years and parishes of birth, with little evidence of age heaping.

¹²Figures B.17, B.18, and B.19 show extracts from the registers. The 1876 and 1882 registers were compiled by officials at the Department of Ecclesiastical Affairs and the Pension Institution for Elementary School Teachers. The 1889 register was compiled by a teacher in Lekeryd parish, who collected information between 1887 and 1889 through correspondence with school districts, clergy, municipal councillors, and school inspectors. The handwritten source material is preserved at the Swedish National Archives.

¹³For the 1876 and 1882 registers, we classify first names using the share of individuals with each name who were female in the 1880 to 1910 censuses. We manually classify names for which this share is close to 0.5. For the 1882 register, we also validate our coding against a table reporting the number of female teachers in each county (Beskow, 1882, p. VI). For the 1889 register, gender can usually be inferred from the year and place of examination because, from 1860 onward, men and women were examined at separate seminaries. In the few ambiguous cases, such as teachers with abbreviated first names and no reported place of examination, we use the first name, surname, year of birth, and school-district location to identify the individual in parish registers. These registers are searchable online through ArkivDigital at <https://www.arkivdigital.net/>.

increased from 6 years in 1876 to 10 years in 1889, while male teachers' experience remained around 16 years. Female teachers' tenure in their current district rose from 3 years to 7 years, while male teachers' tenure stayed at about 12 years. Female teachers were concentrated in urban districts with population densities above 1,000 inhabitants per square kilometer in 1880, while male teachers were more common in rural areas with a density between 100 and 200. In 1889, the average distance from a teacher's school district to their alma mater was similar for women and men, at 135 and 112 km, respectively. Appendix Figures A.9, A.10, and A.11 display the geographic distribution of female teachers, indicating a concentration in cities and parishes near female teacher seminaries.

[Table 2 about here.]

Demography and Occupation. The censuses contain individual-level information on year and parish of birth, parish of residence, civil status, occupation, and household relationships. For pre-treatment demographic measures, we use SHiPS (Swedish Historical Population Statistics), CEDAR's digital interface to parish-level Tabellverket statistics from 1749 to 1859 ([Demographic Data Base, CEDAR, Umeå University, 2025](#)).

Measuring women's labor-market activity in historical censuses is difficult, especially for married women, whose work was often not recorded ([Goldin, 1990](#); [Molinder, 2022](#)). Our baseline therefore uses the census information as reported rather than attempting to correct it without strong justification. This choice reflects both our substantive interest in women's emancipation and our empirical strategy. Most working married women assisted their husbands, who were also their legal guardians. An occupational title therefore indicates that the parish priest and, more broadly, society identified a woman not only as a wife but also by her occupation. Moreover, our design identifies changes over time, so exact levels are less important if underreporting is stable across time and space. The results should consequently be interpreted as changes in the share of women identified by an occupation, rather than necessarily in the share who worked.

We examine five dimensions of women's labor-market outcomes: labor-force participation, non-domestic employment, sector, skill level, and income. We classify occupations using the Historical International Standard Classification of Occupations (HISCO), which organizes titles into seven major groups, 79 minor groups, and 1,255 unit groups ([van Leeuwen and Miles, 2002](#)).¹⁴ Appendix Table B.1 presents examples of common female occupations by sector.

Using HISCO occupation, relationship, and status codes, we define an occupational-

¹⁴Our harmonization of Swedish occupational titles into HISCO follows the Swedish Population Databases for Research ([SwedPop, 2024](#)).

choice indicator for individuals aged over 15 who are studying for, preparing to enter, currently holding, or previously held an occupation. We exclude titles defined solely by family status, such as “worker’s wife” and “housewife.” Labor-force participation is defined as having a reported occupation.

Our main outcome is non-domestic employment. As discussed in Section 2.4, the central labor-market shift for women was from domestic to non-domestic work. Among women with an occupation, we classify as non-domestic those who were neither servants nor relatives of servants. Such women typically belonged to their own household and were less directly dependent on an employer.¹⁵

Income. The 1880–1910 censuses do not report income. We therefore construct occupational income scores using a sample from the 1930 census. We also classify occupations by required skill level rather than sector. Appendix B.3 provides the exact definitions of all labor-market outcomes.

School Age. School attendance was organized locally and varied substantially. Before the School Act of 1882, districts set the starting age, subject to the requirement that children begin no later than age 9, and school duration also varied. The 1882 Act defined the statutory school-age range as the calendar year a child turned 7 through the calendar year the child turned 14. Pupils who had not met the requirements for leaving school could remain after that range, while others could leave earlier; 8.8 percent of primary-school pupils in 1882 were outside ages 7–14 (Department of Ecclesiastics, 1887). Official statistics from 1868 nevertheless use ages 7–14 as the school-age population (Carlson, 1870). The normal plan proposed two years of junior school followed by four years of elementary school, but this was not uniformly implemented. Because we do not observe individual attendance, grade progression, or teacher assignment, the empirical design uses relative cohort timing at the district level rather than a fixed age range for treatment.¹⁶

School Districts. Our empirical strategy requires assigning individuals to the school districts they attended. Because the censuses do not record school attendance or school district, we assign individuals to the district containing their parish of birth. This approach is motivated by the organization of Swedish schooling: school districts were administered by congregations accountable to parish taxpayers and responsible for maintaining attendance records (Westberg, 2017). We therefore assume that children

¹⁵Domestic servants remained subject to *Legostadgan* until 1926, which gave employers extensive authority over their servants’ freedom.

¹⁶Figure A.4 displays the 1882 primary school enrollment ratios by county. The relatively lower enrollment in cities is partly explained by wealthier families sending their children to private schools.

attended school in their district of residence. This assignment is supported by the 1880 and 1890 censuses: approximately 80 percent of children aged 9–14 lived in their birth district.¹⁷ Appendix B.1 describes how we link school districts across time to construct geographically stable school units and match them to census birth parishes.

We collapse the teacher data to the stable-school-unit level, producing a panel of teacher composition by district. The registers report each teacher’s tenure, allowing us to infer when female teachers were employed. Because the registers were published at relatively short intervals, few female teachers are likely to have both entered and left a district between consecutive register years. The main remaining gap is between 1861, the first year women could be hired without a dispensation, and the first register in 1876. We fill this gap using an unpublished source reporting the number of male and female teachers in each school district in 1861 ([Department of Ecclesiastics, 1861](#)).

Women’s Political Participation. We collect extensive data on women’s political participation in the early twentieth century. First, we digitize annual NAWS membership figures from 1902 to 1919. Second, we use digitized and transcribed records for the approximately 350,000 women who signed the nationwide suffrage petition of 1913–14.¹⁸

Using the petitioners’ addresses, we geocode each signature with contemporary and historical maps. Appendix Figures A.3 and E.28 show the spatial distribution of signatures across Sweden and within an urban area.

We link the signatories to the 1910 full-count census using the probabilistic matching procedure of [Abramitzky et al. \(2021\)](#), as described in Appendix E. The geographic granularity of the census facilitates matching: the median census district contains only 400 adult women. Our preferred specification matches 35 percent of the signatories.

From NAWS annual reports, we collect the first name, surname, gender, civil status, and occupation of each local chapter leader between 1902 and 1920.¹⁹ We also identify every woman elected to a Swedish city council between 1910 and 1919, before universal and equal suffrage in local elections was enacted in March 1919. We collect their first name, surname, civil status, and occupation from articles published in the women’s magazines *Dagny* and *Rösträtt för kvinnor* and from city council protocols held by the relevant municipal archives.²⁰ Finally, we identify these individuals’ years and parishes

¹⁷Teachers’ diaries and grade registers are the principal surviving sources for directly linking pupils to school districts, but only a small share from our study period is preserved in Swedish archives.

¹⁸The records were created through a citizen-science project that digitizes each signatory’s first name, surname, civil status, occupation, and residential address. The project is conducted with the Swedish National Archives and is available at <https://github.com/monirbounadi/petitioners>.

¹⁹The annual reports for 1904–1921 are available at <https://gupea.ub.gu.se/handle/2077/51939>. The reports for 1902–1903 are held in the NAWS archive at the Swedish National Archives.

²⁰The magazines are available at <https://www2.ub.gu.se/kvinn/digtid/>.

of birth in parish registers.²¹

4. EFFECTS ON WOMEN'S EMANCIPATION

4.1. EMPIRICAL STRATEGY

To identify how childhood exposure to a female teacher affected women's later political participation and work, we exploit staggered first hiring of female teachers across school districts. Districts hired their first woman in different years. Within a district, younger cohorts could be exposed for more of their schooling than older cohorts. Across districts, the same cohort faced different hiring dates. We therefore compare cohort differences within districts that hired a woman with the corresponding differences in districts that never did. Because first hiring is staggered and its effects may differ across cohorts and districts, we use the difference-in-differences estimator of [Callaway and Sant'Anna \(2021\)](#), which is robust to such heterogeneity, rather than two-way fixed effects.

Let Y_{istc} denote an outcome for individual i , born in district s in cohort t and observed in census c .²² Let E_s be the year district s first hired a female teacher, and set $E_s = \infty$ for districts that remained all-male through 1889. We define a district's treatment cohort as the birth cohort that turned 14 in the hiring year, $g_s \equiv E_s - 14$, and event time as

$$e \equiv t - g_s = (t + 14) - E_s.$$

The cohort with $e = 0$ turned 14 in the hiring year, and positive e identifies successively younger cohorts. A cohort thus has non-negative event time if its district hired no later than the year it turned 14, $E_s - t \leq 14$. Appendix Figure [A.2](#) shows the distribution of treatment timing.

Event time measures potential exposure, not years taught by a woman. Before 1882, districts set entry ages and school duration. Afterward, the statutory range ran from the calendar year a child turned 7 through the calendar year it turned 14, with exceptions at both ends. We do not observe attendance, grade progression, or teacher assignment. Higher e consequently indicates both earlier potential exposure and a longer female-teacher presence in the district.

We treat the first hire as a durable change. The reason is that most districts kept their female teachers. Of the 123 districts with a female teacher in 1876, 108 still had one in

²¹Our genealogical work is documented on [Wikidata.org](https://www.wikidata.org). See <https://w.wiki/A7Mz> for local NAWS leaders and <https://w.wiki/A7N8> for female city councillors.

²²The data are repeated cross-sections rather than an individual panel. This is sufficient because treatment is assigned at the school-district-by-cohort level.

1882 and 104 in 1889. Of the 144 additional districts first hiring a woman by 1882, 113 still employed women in 1889.²³

The building block is the group-time average treatment effect,

$$ATT(g, t) = \mathbb{E} \left[Y_{istc}(g) - Y_{istc}(\infty) \mid g_s = g \right], \quad (1)$$

where $Y_{istc}(g)$ is the outcome if the district's treatment cohort were g , and $Y_{istc}(\infty)$ is the outcome if the district never hired a female teacher. $ATT(g, t)$ is thus the effect on cohort t in districts whose first hire came in the year cohort g turned 14. Our estimand is the event-study effect,

$$ATT(e) = \sum_{g < \infty} \omega_g ATT(g, g + e), \quad \omega_g = \Pr(g_s = g \mid g_s < \infty), \quad (2)$$

which averages these effects at a common event time with positive weights summing to one.

We compare treated districts only with districts that never hired a female teacher. We do not use districts that had not yet hired. The reason is that, in those districts, the cohorts that just missed the first hire are imperfect controls: some remained in school past 14, and the local changes that led to hiring may already have affected them. Each $ATT(g, t)$ compares the outcome change from cohort $g - 1$, which turned 15 in the hiring year, to cohort t , with the same change in never-treated districts. We normalize this immediately preceding cohort to zero at $e = -1$, even though it may exceptionally have remained in school. Estimates for $e < 0$ are placebo tests. Estimates for $e \geq 0$ trace effects for cohorts who were successively younger at first hire.

Identification rests on two assumptions. The first is no anticipation, $Y_{istc}(g) = Y_{istc}(\infty)$ for $t < g$: cohorts that turned 14 before the first hire were unaffected by it. The second is conditional parallel trends. Absent hiring, the difference between exposed and unexposed cohorts in treated districts would have evolved as in never-treated districts with the same covariates X_i . We include birth-county indicators, allowing region-specific political and economic trends. The covariates are identical for the political and labor-market outcomes. We do not control for age. The reason is that, in the single-cross-section petition sample, age is determined by cohort and cannot be separated from it. For comparability, we also omit age from the labor-market specifications.²⁴

We estimate equation (1) with the doubly robust repeated-cross-sections estimator of [Callaway and Sant'Anna \(2021\)](#), which combines inverse probability weighting and

²³Female teachers also made up a substantial share of staff after entry. Figure A.7 shows that they accounted for roughly half of teachers in treated districts, and in about 30 percent of those districts there was one female and one male teacher.

²⁴Appendix Table D.8 estimates each census separately, so each birth cohort is observed at one age within an estimate.

an outcome regression and is consistent if either is correctly specified, then aggregate to equation (2). Two-way fixed effects would be inappropriate here. With staggered timing and heterogeneous effects, it uses already-treated units as comparisons and can assign negative weights, so its coefficients need not estimate an interpretable average effect (Baker et al., 2026). Standard errors are clustered by school district.

The historical setting supports the identifying assumptions. The 1859 reform converted some teacher seminaries to female-only institutions and left others male-only, creating a geographically uneven supply of trained women. Teacher labor markets were regional: more than half of teachers trained within 200 km of their birthplace and worked within 100 km of their alma mater (Table 2); Appendix Figures A.9, A.10, and A.11 show the resulting clustering. As discussed in Section 2, seminary locations reflected administrative and political considerations largely unrelated to women’s later political or labor-market outcomes. Candidate seminary towns also had similar pre-trends in sex ratios and marriage rates (Figure A.8).

The remaining concern is that first hiring also reflected local demand. A newly appointed vicar, for example, could both favor women teachers and change the local social environment. Our estimates would then combine teacher exposure with broader institutional change. Table 3 compares treated and control places on pre-1860 characteristics. Hiring also tracks supply-side turnover: among districts without female teachers in 1876, 7.4 percent with a male teacher eligible for retirement by 1882 under the statutory threshold of age 55 and 30 years of service hired a woman, compared with 3.8 percent of other districts (Beskow, 1882). These facts do not eliminate demand-side selection, but they support the interpretation that access to female-teacher supply was an important determinant of timing.

[Table 3 about here.]

4.2. PETITIONING AND NON-DOMESTIC WORK

The main results are presented in Figures 7 and 8, which report event-study estimates for two measures of emancipation: petitioning for women’s suffrage and having a non-domestic occupation.

[Figure 7 about here.]

[Figure 8 about here.]

We find a positive effect of having a female teacher on both outcomes. The treatment effect is 2.27 (0.53) percentage points for petitioning and 1.13 (0.35) percentage points

for non-domestic work. Both estimates are statistically significant at the 5 percent level and correspond to an approximately 23 percent increase relative to the baseline for petitioning and a 9 percent increase for non-domestic work.²⁵ The estimated pre-treatment averages include zero, and we fail to reject that the first five pre-treatment coefficients are jointly zero for either outcome; Tables 5 and C.3 report the p -values. The estimates tend to be larger for cohorts who were younger when the district first hired a female teacher. These later-cohort effects may reflect both earlier potential exposure during childhood and the longer local presence of female teachers. In Figures C.20 and C.21, we also plot coefficients from alternative aggregations by treatment timing and birth cohort separately. While the standard errors are generally too wide to rule out specific patterns, the point estimates are, on average, higher for earlier-treated school districts.

While the effect on non-domestic work is positive, we do not find an effect on total labor-force participation; Appendix Figure C.22 reports two alternative measures. The impact of female teachers on the labor market was thus to reinforce the general shift from domestic to non-domestic work, rather than to expand total labor supply.

To put these estimates in context, we multiply the ATT by the number of treated women. For petitioning, a lower-bound estimate of the treated population above age 18 is 468,000, or 24 percent.²⁶ This calculation gives $468,000 \cdot 0.0227 \approx 10,600$ additional signatures associated with childhood exposure, or 3.0 percent of all petition signatures. For non-domestic work in 1910, the corresponding figure is 4,300 women, or 1.8 percent of the female non-domestic workforce aged 20–59. The aggregate changes are modest because female teachers were few: only 1,126 were employed in 1889. The estimates per potentially exposed student are much larger.

Our estimand is the effect of attending school in a district that had hired a female teacher. Because we cannot link students to individual teachers, the ATT measures potential exposure to a woman teacher, not the effect of being taught by one. The latter is not separately identified.

The aggregate petitioning effect also requires a caveat: female teachers were themselves central organizers of the petition. Section 5 examines the quantitative importance of this organizational role.

What kinds of non-domestic occupations did the students of female teachers enter? In Figure 9, we show the female-teacher effect by economic sector. There is a positive

²⁵The baseline is the excluded cohort, which turned 15 in the year the district first hired a female teacher.

²⁶It is a lower bound because we do not know which school districts hired a female teacher after 1889. We therefore make the conservative assumption that no additional district hired women.

gradient when comparing sectors by their occupational income score, reflecting upward mobility among treated women. The estimate for clerical work is an outlier, mostly due to the low baseline. It is, however, not surprising that the effect is strongest for clerical occupations. The new clerical jobs that became available to women in, for instance, the telegraph sector were more accessible than studying to become a nurse, doctor, or teacher, but nonetheless broke with traditional gender roles. In light of the strong macro trend from agriculture to factory work during this period, it is also interesting that the effect on working-class occupations is small and not statistically significantly different from zero at conventional levels. In Table C.3, we present all estimates on additional labor-market outcomes.

[Figure 9 about here.]

4.3. MECHANISMS

The positive effect of having a female teacher could be the product of several different factors. In this section, we discuss plausible mechanisms behind the results and the empirical support for them.

Role models or better teachers. Growing up in the second half of the nineteenth century, a girl's most visible educated adults often included the parish priest, the provincial doctor, and her schoolteacher. A female teacher's presence, and the rejection of pre-industrial gender norms she embodied, could therefore make her an important role model. Qualitative studies of elementary-school teachers make the same point (Dahllöf, 1987; Johannesson, 1989).²⁷ This channel is not the only reason female teachers could have affected their pupils differently from male teachers. The main alternative is that female teachers were more effective instructors. A related concern is that hiring a woman coincided with broader school expansion; Section 4.1 discusses this source of selection.

There are theoretical arguments for why the first female teachers would have been positively selected. High-ability women were discriminated against in practically all other white-collar occupations. They were, for instance, barred from universities until 1873. However, it remained difficult for women to attain a university degree for much longer. University admission required a finishing degree (*studentexamen*), but girls were not allowed to attend preparatory schools (*läroverk*) until the 1920s. Women therefore needed substantial private educational investments, which were possible only

²⁷Backman Prytz (2017, cited in Hamberger (2026)) studies memoirs by 171 teachers from the turn of the century. Male teachers rarely mention their female pupils, whereas female teachers describe their interactions with girls in detail.

for a small minority. Teacher seminaries could therefore attract top female candidates with little competition from other professions. Empirical findings also support this hypothesis (Marklund, 2021).

One way to distinguish between the role-model mechanism and a more general human-capital mechanism is to estimate the female-teacher effect on male students. If female teachers were equally effective instructors for boys and girls but acted as role models only for girls, comparing effects by student gender would help separate the two channels. This assumption may be too strong in light of the modern literature on same-sex teacher effects (de Gendre et al., 2024). Nevertheless, comparing boys and girls with a female teacher can still shed light on the relative strength of the two mechanisms, which are by no means mutually exclusive. The effect on male students is straightforward to analyze with the same methodology as for girls, with one important caveat: the treatment definition needs to be revisited.

As discussed in Section 2, female teachers did not generally teach in boys' schools when the school district was gender divided. It is therefore important to decide whether the female-teacher effect should be estimated only for boys who likely attended a school with a female teacher, or whether it should more broadly capture attending school in a district where some teachers were female. In our preferred specification, we exclude school districts where it is challenging to determine whether boys would have been taught by female teachers.²⁸ This also makes a standard triple-difference approach less appealing.

The results for men are presented in Appendix C.2. Because the distinction between domestic and non-domestic work is not comparable for men, we focus on labor-force participation. The ATT is 1.44 (0.50) percentage points, small relative to the 80.62 percent baseline among men aged 20–59 and to the effect on women's non-domestic work. In addition, there is no comparable gradient across occupational sectors for men. The only sector with a significant treatment effect is sales.

We conclude that these results support both the human-capital and role-model mechanisms, suggesting that the two complemented each other in shaping the overall effect.

Demographic mediators. In later generations of feminist politics, the tension between family formation and career became a central theme. For first-wave feminists, however, marriage remained a central institution around which social and economic life was organized.²⁹ What changed gradually was the norm that married women should not

²⁸Table C.5 reports estimates using alternative inclusion criteria.

²⁹Suffrage leader Anna Bugge Wicksell sought to reconcile marriage with women's economic independence by reforming the institution and challenging norms against married women's work (Johnson and

work, unless compelled by material necessity.

Against this background, we estimate the effect of female teachers on two marriage-related outcomes. The first is an indicator for ever marrying, which includes women who were widowed or divorced by the time of observation. The second is the age difference between spouses. Conditional on marriage, this outcome allows us to study not only the extensive-margin decision to marry, but also the nature of the marital match. Spousal age differences are commonly used as a proxy for conjugal power dynamics (Mason, 1986; Roterling and Bras, 2019). The censuses do not report age at marriage, so we cannot study marriage timing.

The second type of demographic mediator is internal migration. Opportunities for both political engagement and work outside the household were better in cities than in the countryside. This was also a period of rapid urbanization, with consequences for female labor-force participation. Berger et al. (2024) find strong positive effects on work for women who moved to Stockholm in the same period and context that we study, and their results also show a tradeoff between marriage and work. We thus estimate the effect on moving to any urban area and, separately, to Stockholm.

The results are presented in Table 4, and we find no effect on either marriage rates or migration. However, we find that married women who had a female teacher were, on average, married to husbands who were 0.3 years younger.³⁰

[Table 4 about here.]

Another potential mediator is emigration to the US, which was very common, especially toward the end of the study period. While we do not have access to birth parish-by-birth year data on emigration, Table 3 shows no cross-sectional differences in aggregate emigration flows between parishes that hired a female teacher and those that did not.

Heterogeneity. Next, we examine treatment-effect heterogeneity along observable characteristics. These comparisons help identify which groups benefited most and shed light on the mechanisms behind the effects. We focus on two dimensions: place of birth, distinguishing between urban and rural areas, and socioeconomic background. There are theoretical reasons to expect both dimensions to interact with the treatment effect. Girls from rural or less advantaged backgrounds may have benefited more from

Robson, 2022).

³⁰The outcome variable is calculated by subtracting the wife's age from the husband's. Thus, a positive age difference means that the husband is older than the wife. The conclusions are qualitatively the same when instead using the absolute age difference as the dependent variable.

exposure to a female teacher, since the marginal impact of an educated female role model would be larger in environments with fewer other such influences. At the same time, these girls faced stronger social and economic constraints than urban middle-class girls when seeking to enter politics or non-domestic work.

We classify birth parishes as urban or rural using SWEDPOP. Measuring childhood socioeconomic status is more difficult. Lacking direct information on a woman's childhood household, we proxy for its socioeconomic status using her surname.³¹ Using surnames for women may seem undesirable because spouses commonly adopted the husband's surname. The censuses, however, typically include wives' maiden names. The surname typology distinguishes patronymic from non-patronymic names; patronymic names indicate lower socioeconomic status on average.³²

Table 5 reports the heterogeneity results for petition signing. In addition to the subgroup splits discussed above, columns (2) and (3) address potential differences in exposure to the petition campaign itself. Although the baseline specification compares women within birth locations, some women may not have had an opportunity to sign because the petition never reached their area. In column (2), we therefore exclude parishes where no one signed the petition. In column (3), we add fixed effects for 1910 census district, comparing women living in the same local area at the time of the petition. The estimated effect is slightly smaller in these specifications, but remains statistically significant.

The remaining columns show that the effect is larger for women born in rural areas. By socioeconomic background, however, the effect is larger for higher-SES women, as measured by surname type.

[Table 5 about here.]

Table 6 reports the heterogeneity results for non-domestic work. Here, the pattern for urban versus rural districts reverses. However, relative to the baseline mean, the effect sizes are not very different between urban and rural districts. For socioeconomic status, the effect is again stronger for women from higher-SES backgrounds.

[Table 6 about here.]

³¹Dalman (2022) show that specific surname types predict socioeconomic status in nineteenth- and early twentieth-century Sweden.

³²Appendix B.2 describes the method and validates it against occupation-based social status from the SOCP scheme (Putte and Miles, 2005).

5. TEACHERS AS POLITICAL ORGANIZERS

5.1. EMPIRICAL STRATEGY

Beyond their primary role as teachers, many testimonies describe female teachers playing an integral role in organizing the early feminist movement before universal suffrage. An analysis of letters from leading women's suffragists shows that professional and intellectual women, especially teachers, were expected to form the core of the movement (Florin, 2006, p. 126). In the 1913–14 petition campaign, the NAWS primarily contacted female teachers in parishes where they lacked organized members in order to spread the petition (LKPR, 1914, p. 14).

There is also substantial overlap between the skills required for teaching and those required for political organizing. Moreover, the centralized seminary system for teacher training, rather than an apprenticeship model for professional education, created dense networks among female teachers through shared educational experiences.

The presence of female teachers, as opposed to only male teachers, in the city or parish is therefore expected to have shaped the political culture beyond the intergenerational channel operating through students. Measures of the political culture include the strength of the NAWS, petitioning intensity at the parish level rather than the individual level, and turnout in the first election after universal suffrage. All of these outcomes help us understand the broader impact of female teachers, but cannot be estimated using the difference-in-differences strategy employed for individual-level outcomes.

The regional distribution of male- and female-only teacher seminaries motivates a spatial instrumental-variables analysis. We estimate

$$\text{First stage} \quad D_s = \pi_X + \pi_{1X}Z_s + \xi_s \quad (3)$$

$$\text{Second stage:} \quad Y_s = \alpha_X + \beta D_s + \varepsilon_s \quad (4)$$

where the second-stage equation captures the effect of hiring a female teacher before 1889, $D_s = \mathbb{1}(\text{Female Teacher})$, on political outcomes Y_s in school district s around the peak of the women's suffrage movement. The endogeneity problem is addressed in the first stage, where we use the gender of the nearest teacher seminary to predict whether a school district hired a woman. This strategy uses cross-sectional variation between school districts that hired female teachers, with the instrument $Z_s = \mathbb{1}(\text{Close to female seminary})_s$ providing the identifying variation. In this strategy, treated school districts are those with a long history of female teachers. π_X and α_X are fully saturated indicators for the covariates X_s , and π_{1X} is the separate first-stage effect for each value of X_s . Under standard IV assumptions of relevance, exogeneity, exclusion,

and monotonicity, β identifies a weighted average of covariate-specific local average treatment effects (LATE) (Angrist and Imbens, 1995, Theorem 3). We use the size of the school district in 1876 as the only covariate in order to strengthen the relevance of the instrument.

Seminary gender was historically predetermined and strongly predicts early female-teacher hiring. The design is therefore informative about whether seminary-induced differences in access to female teachers covary with later political outcomes. A causal interpretation additionally requires the exclusion restriction: proximity to a female rather than male seminary must affect politics only through female-teacher hiring. This is demanding because seminaries may have influenced local politics through other channels. Restricting the sample to districts near seminary towns absorbs the general effect of proximity to a regional center, but does not resolve every alternative channel. We therefore treat the IV estimates as complementary, suggestive evidence rather than unbiased causal effects.

Instrument relevance is strengthened by the algorithm used to select the neighborhood around each seminary town included in the analysis. This procedure is described in more detail in Appendix F.

5.2. POLITICAL MOBILIZATION AND VOTING

Table 7 presents the 15-km specification for seven z-standardized outcomes. Column (1) measures the persistence of female teachers, column (2) petitioning intensity, and columns (3)–(4) the strength of the local suffrage organization. Columns (5)–(7) use precinct-level turnout in the March 1919 municipal election, the first in which women voted on equal terms with men. The election also indirectly affected the composition of parliament’s upper house.³³ The records distinguish votes cast in person from proxy votes. Proxy voting had previously been limited to one other voter, but the suffrage bill extended it to legal spouses. Suffrage leaders urged women to vote in person rather than let a husband or other man cast their ballot.³⁴ Women cast 62 percent of their votes, but only 20 percent of these in person. Thus, 12 percent of eligible women voted in person, compared with 53 percent of men.

Across outcomes, predicted early female-teacher hiring is associated with greater centrality in the suffrage movement and stronger political engagement among women. The

³³This is, to our knowledge, the first quantitative analysis of female turnout in this election. Existing work usually begins with the 1921 parliamentary election. The data come from unpublished Statistics Sweden material held at the Swedish National Archives (SE/RA/420401/31/02/H 1 B/1); see Statistiska centralbyrån (1920) for the election.

³⁴Florin and Kvarnström (2001) notes that suffragists regarded abstention or proxy voting as a betrayal of female solidarity.

direction of these relationships agrees with the individual-level event-study estimates.

[Table 7 about here.]

The coefficients are generally large. They are also local to places near regional centers with teacher seminaries.

The 15-km sample is small, raising finite-sample concerns for 2SLS. Appendix D uses a larger radius that still passes a weak-instrument test and estimators with better small-sample properties. The coefficients shrink, and some voting estimates lose significance. The overidentification test also rejects instrument exogeneity for several political outcomes in the 15-km sample, though only one rejection remains with the larger sample. These sensitivities reinforce our suggestive interpretation of the IV evidence.

6. CONCLUSION

This paper asks whether women's entry into elementary-school teaching advanced women's emancipation in nineteenth-century Sweden. Teaching was among the first public professions open to women, and female teachers' staggered arrival after 1859 made a respected form of female employment visible to girls before universal suffrage and broad access to white-collar work. We link teacher registers, full-count censuses, and the 1913–14 suffrage petition to women's childhood school districts, then compare cohorts differentially exposed to female teachers. Women in exposed cohorts were 2.27 percentage points more likely to sign the petition, or 23 percent relative to the baseline, and 1.13 percentage points more likely to hold a non-domestic occupation, or 9 percent. The latter reflects movement from domestic service into professional and clerical work, not higher total labor-force participation.

The evidence is consistent with both instruction and example. Small gains in boys' labor-force participation may reflect improved teaching quality; the distinct effects on women's political engagement and non-domestic work point to an additional role-model channel. Female teachers were also prominent in the suffrage movement, and seminary-based spatial estimates provide complementary evidence linking earlier access to stronger suffrage organizations and higher female political participation.

Representation in a central public institution can therefore affect more than those who enter it. Opening teaching expanded women's employment opportunities and exposed girls to a new social role at a formative age, with lasting political and economic effects. The feminization of teaching was both a consequence of emancipation and a force advancing it.

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FIGURES

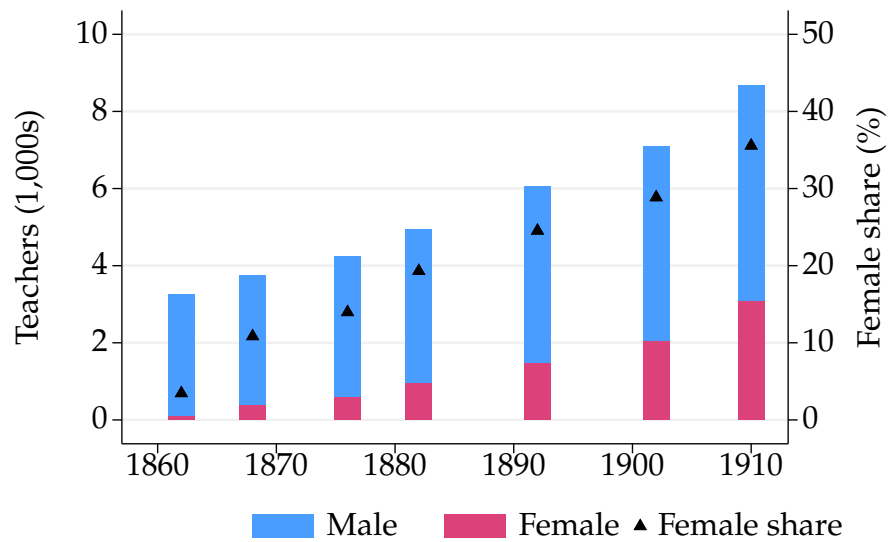


FIGURE 1. TEACHERS IN ELEMENTARY SCHOOLS BY GENDER, 1862–1910

Notes: Bars report male and female elementary-school teachers (left axis, thousands); triangles report the female share of teachers (right axis), 1862–1910. Data from [Carlson \(1870\)](#); [Department of Ecclesiastics \(1862, 1887, 1900, 1905, 1912\)](#).

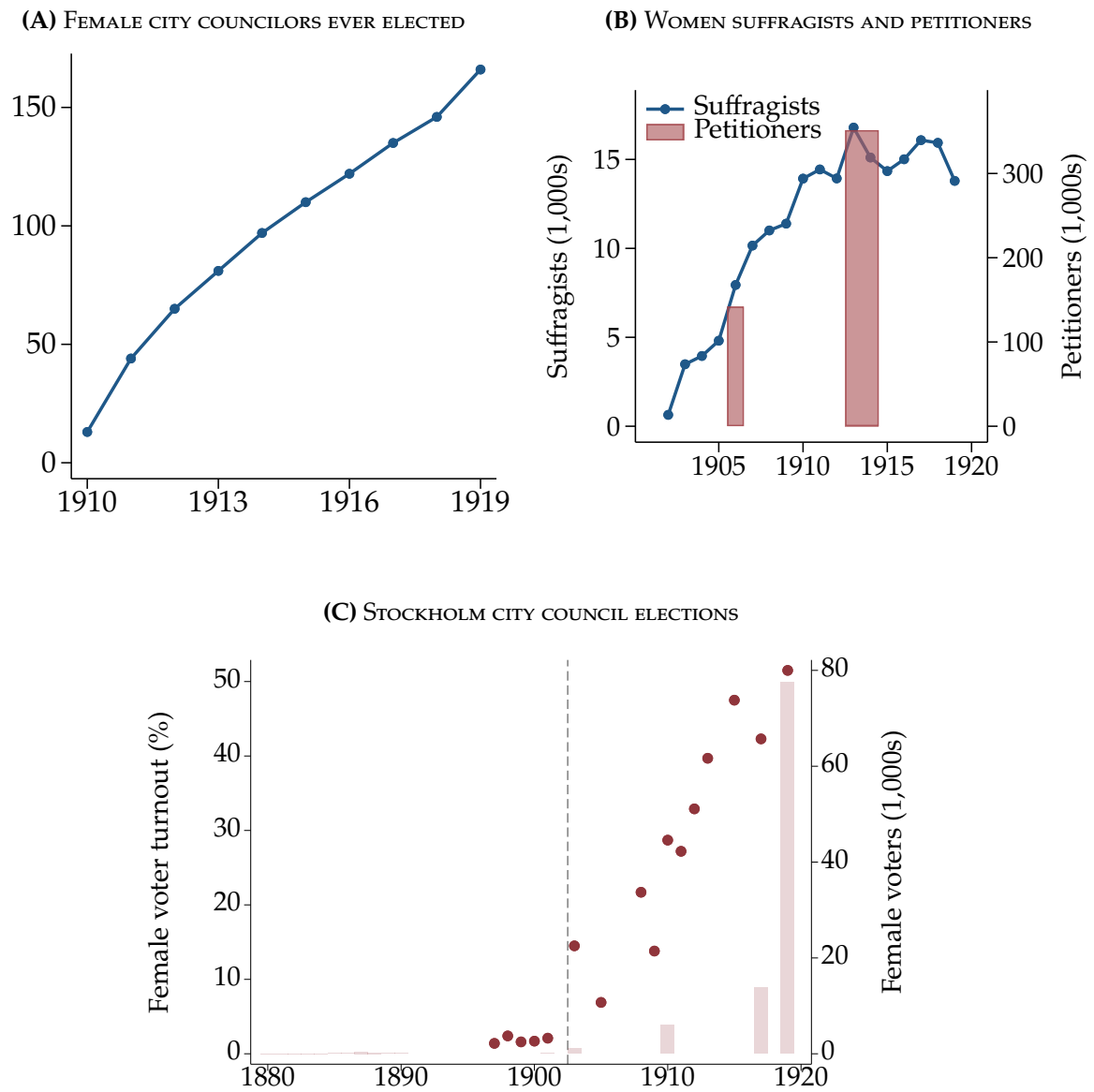


FIGURE 2. WOMEN'S POLITICAL PARTICIPATION BEFORE SUFFRAGE

Notes: Panel A reports the cumulative number of women elected to Swedish city councils, 1910–1919. Panel B plots National Association for Women's Suffrage membership (line, left axis) and signatures on the 1906 and 1913–14 petitions (bars, right axis). Panel C reports female turnout (left axis) and female voters (right axis) in Stockholm City Council elections; the dashed line marks the founding of Stockholm's first suffrage association in 1902. Data from the authors' dataset, NAWS annual reports, *Landsföreningen för kvinnans politiska rösträtt* (1906); LKPR (1903, 1914), and Karlsson Sjögren (2008).

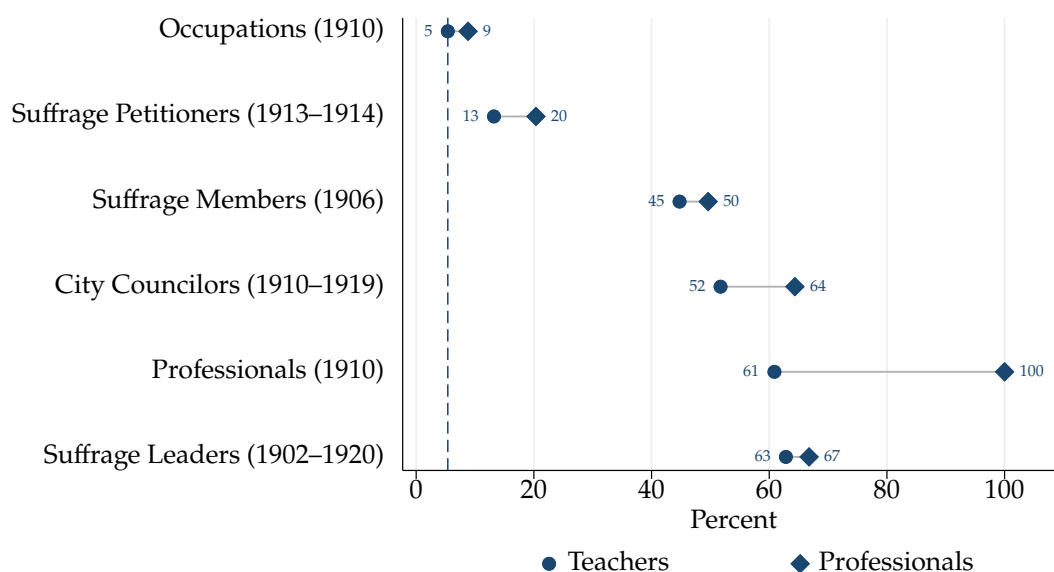


FIGURE 3. TEACHERS AND PROFESSIONALS ACROSS SELECTED GROUPS OF WOMEN, CA. 1910

Notes: Bars report the shares of teachers and professionals among selected groups of women with recorded occupations. The dashed line gives the teacher share among all employed women in the 1910 census. Professionals include teachers and other professional and technical occupations. Data from the 1910 census ([SwedPop, 2024](#)), the authors' datasets, and the 1906 NAWs annual report ([Landsföreningen för kvinnans politiska rösträtt, 1906](#)).

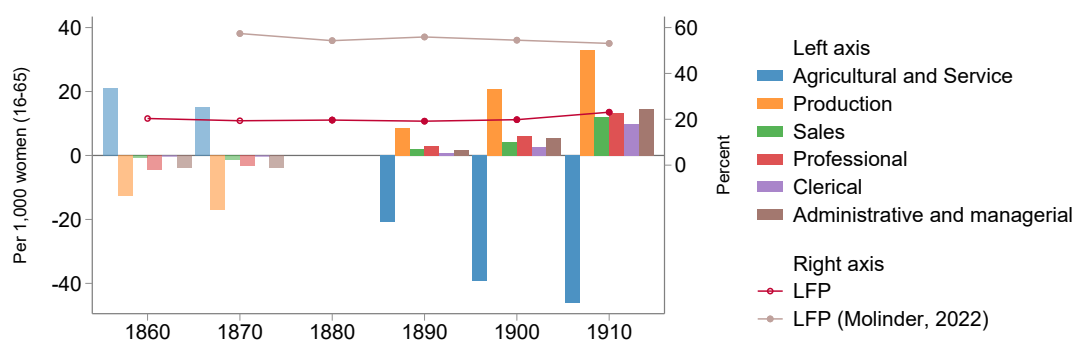


FIGURE 4. OCCUPATIONAL STRUCTURE OF WOMEN, 1860–1910

Notes: Bars show changes in female employment relative to 1880 across six sectors. Lines show labor-force participation among women aged 15–75. Data from the 1860–1870 censuses ([Statistiska Central-Byrån, 1865, 1874](#)), the 1880–1910 censuses ([SwedPop, 2024](#)), and [Molinder \(2022\)](#).

(A) ANDERSSON IN THE 1889 TEACHER REGISTER

Malmöhus län.																	
1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18
Socknarnes		Bostädernas				Personliga uppgifter.											
namn.	folk-mängd.	(by-)namn.	afstånd i kilom. fr. kyrka.	postst.	Tjenstemännens namn.	Födelse-			Folksk.-ex.		Organ.-ex.		Kant.-ex.		Ort. Tjänstgör. Tillsädesår.		
						år.	datum.	ort.	år.	ort.	år.	ort.	år.	ort.			
Landskrona	11738	Staden ²⁾	—	—	Andersson, Nils Johan, 1:o lär.	55	10/2	Ramdala	79	Vexjö					83	5	
		"	—	—	Palmqvist, Karl August	39	21/12	Eke	69	Linköping					82	6	
		"	—	—	Ahlqvist, Ola	40	8/4	Asmundtorp	71	Vexjö					82	6	
		"	—	—	Johansson, Per Ludvig	48	18/12	Hudeno	71	Göteborg					83	14	
		"	—	—	Wallqvist, Anders	61	17/5	Valleberga	81	Lund					86	2	
		"	—	—	Dahlgren, Jöns	63	21/2	Skabersjö	83	d:o					87	1	
		"	—	—	Berg, Augusta Johanna	59	7/5	Kristianstad	82	Umeå					84	4	
		"	—	—	Lundgren, Emma Charl. Vilhelmina	63	12/5	Malmö	83	Kalmar					85	3	
		"	—	—	Söderström, Jenny	62	10/5	Fritsla	85	Skara					87	1	
		"	—	—	Johnsson, Klara	65	10/1	Lund	86	Kalmar					e. o.		
		"	—	—	Andersson, Bothilda	61	14/2	Sjörup	86	d:o					e. o.		
		"	—	—	Ahlgren, Ebba	63	30/7	Lund	84	d:o					e. o.		
		Borstahusen (Bisköping)	2,5	2,5	Holmstedt, Nils Magnus	60	7/2	d:o	81	Lund					82	6	
		Staden	—	—	Norén, Amalia Mathilda	56	2/5	Broby	80	Falun					85	3	
		"	—	—	Lövén, Nils Eberhard	46	12/1	Lund			79	Stockholm	79	Stockholm	88	1	
S:t Ibb	985	Tuna	2,5	3	Rosengqvist, Peter Magnus	44	6/6	Fryeled	69	Vexjö					71	17	
Qvistofta	1424	Qvistofta	2	2	Palmqvist, Peter August	59	22/5	Billeberga	79	Lund					81	7	
		Katslösa	2,6	4	Engström, Olof	57	22/6	Gudmundtorp	77	d:o					79	9	
		Qvistofta	0	2,6	Tholander, Ola	45	7/6	Mörarp	66	d:o			74	Lund	79	9	
Glumslöf	918	Glumslöf	0	5,2	Liljedahl, Jonas C.	32	7/6	Asmundtorp	53	d:o					56	32	
Herslöf	1476	Herslöf	0	1	Ljungberg, Per M. *	37	25/5	Örkened	57	d:o					65	23	
			—	—		50	1/6		70	d:o					80	8	

(B) ANDERSSON WITH HER PUPILS



FIGURE 5. FROM THE TEACHER REGISTER TO THE CLASSROOM

Notes: Panel A highlights Bothilda Andersson's entry in the 1889 teacher register. It records that she was born in Sjörup on February 14, 1861, completed her teacher examination in Kalmar in 1886, and taught in Landskrona. Panel B shows Andersson with her pupils; the red circle identifies her. Photograph from Landskrona Museum, B13343.

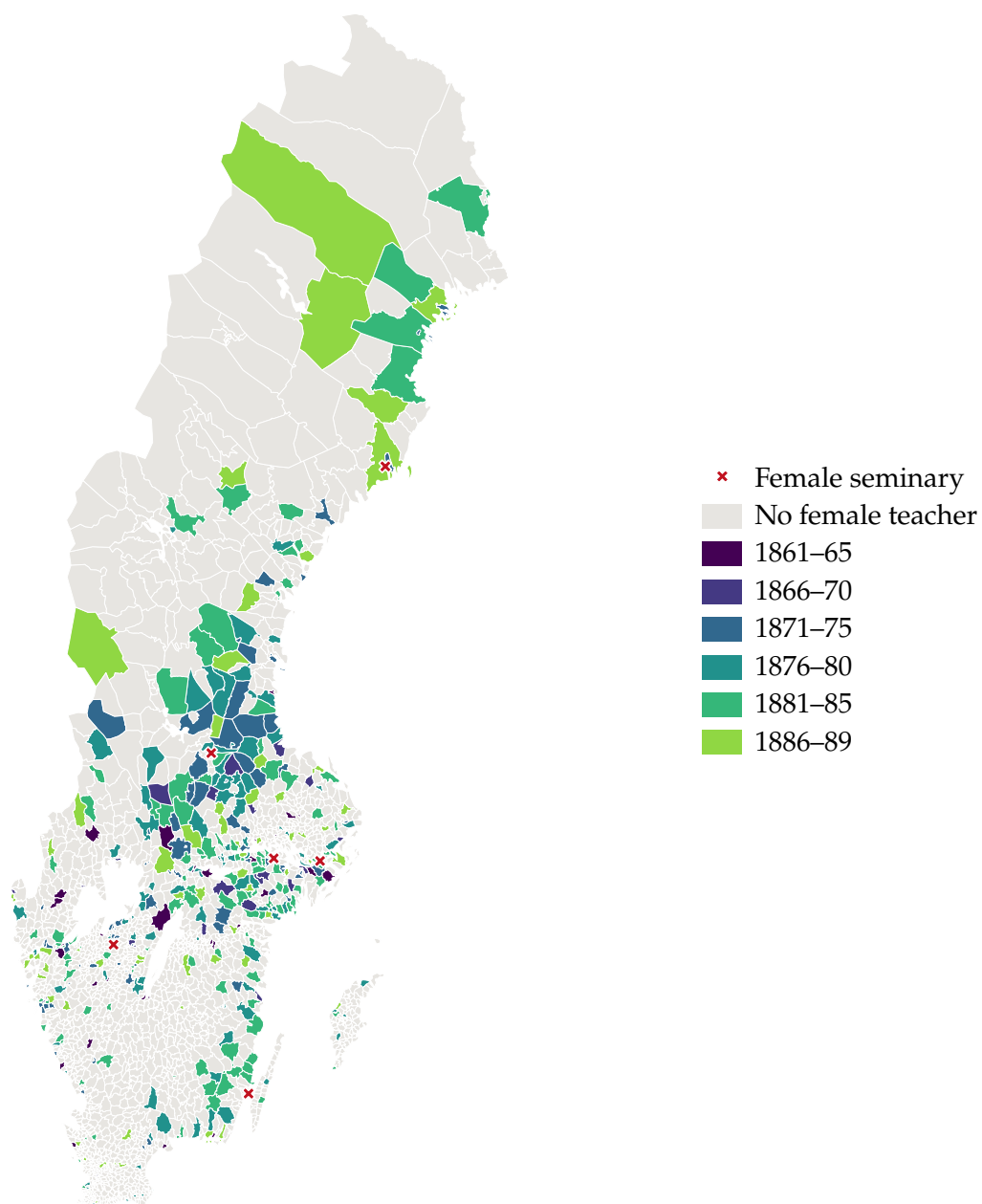


FIGURE 6. MAPPING THE FIRST FEMALE TEACHERS

Notes: Colors indicate when each school district first employed a female teacher; gray districts had not done so by 1889. Red crosses mark female teacher seminaries active by 1889. Treatment timing combines the 1876, 1882, and 1889 teacher registers with an unpublished 1861 school-district tabulation by the Department of Ecclesiastics ([Department of Ecclesiastics, 1861](#)).

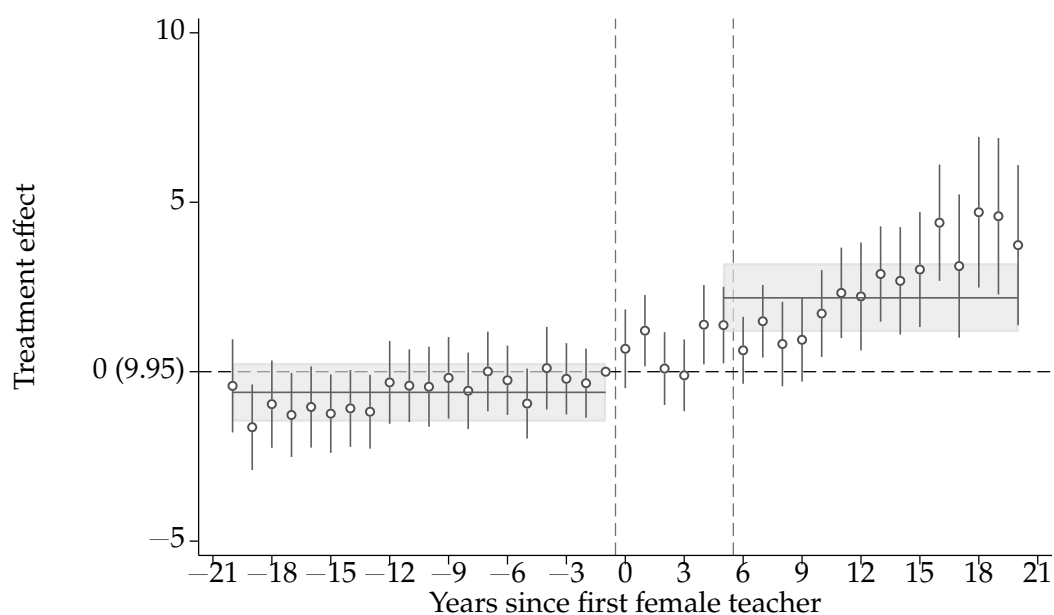


FIGURE 7. EFFECTS ON PETITIONING FOR WOMEN'S SUFFRAGE

Notes: Points and vertical bars report event-study estimates and 95 percent confidence intervals from equation (2). The outcome equals 100 for petition signers and 0 for matched non-signers. Event time indexes cohorts relative to the cohort aged 14 when the district first hired a female teacher; positive values denote younger cohorts. The parenthetical beside 0 gives the reference-cohort mean at event time -1. Horizontal lines and shaded bands report average estimates and 95 percent confidence intervals for event times -20 to -1 and 5 to 20. Sample includes women born 1840–1880 and observed in 1910. Estimates use the doubly robust estimator of [Callaway and Sant'Anna \(2021\)](#), never-treated districts, and birth-county controls. Standard errors clustered by school district.

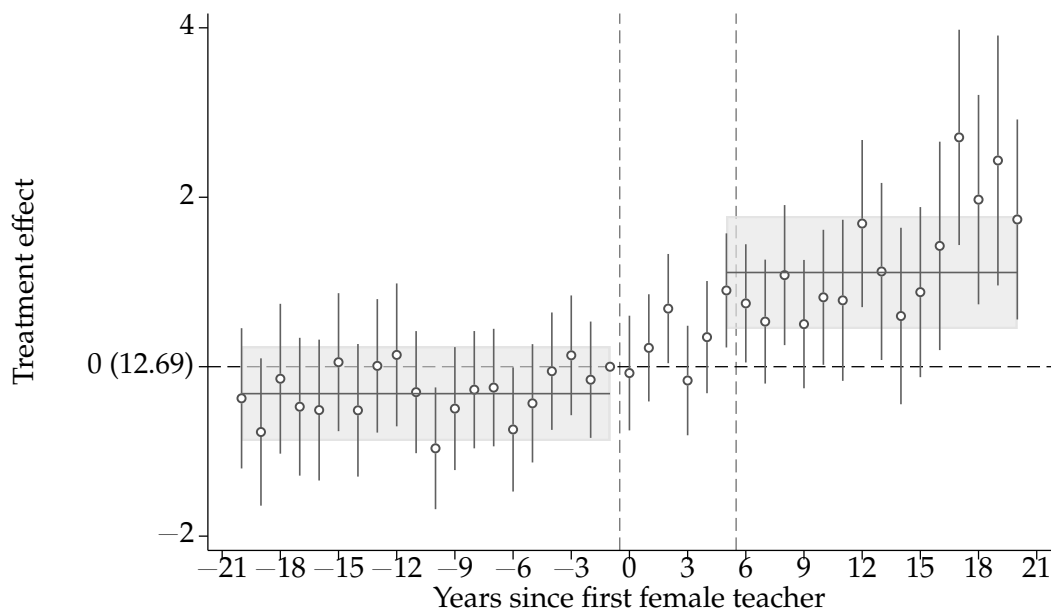


FIGURE 8. EFFECTS ON NON-DOMESTIC WORK

Notes: Points and vertical bars report event-study estimates and 95 percent confidence intervals from equation (2). The outcome equals 100 for a non-domestic occupation and 0 otherwise. Event time indexes cohorts relative to the cohort aged 14 when the district first hired a female teacher; positive values denote younger cohorts. The parenthetical beside 0 gives the reference-cohort mean at event time -1 . Horizontal lines and shaded bands report average estimates and 95 percent confidence intervals for event times -20 to -1 and 5 to 20 . Sample includes women born 1840–1880 and observed at ages 20–59 in the 1880–1910 censuses. Estimates use the doubly robust estimator of [Callaway and Sant’Anna \(2021\)](#), never-treated districts, and birth-county controls. Standard errors clustered by school district.

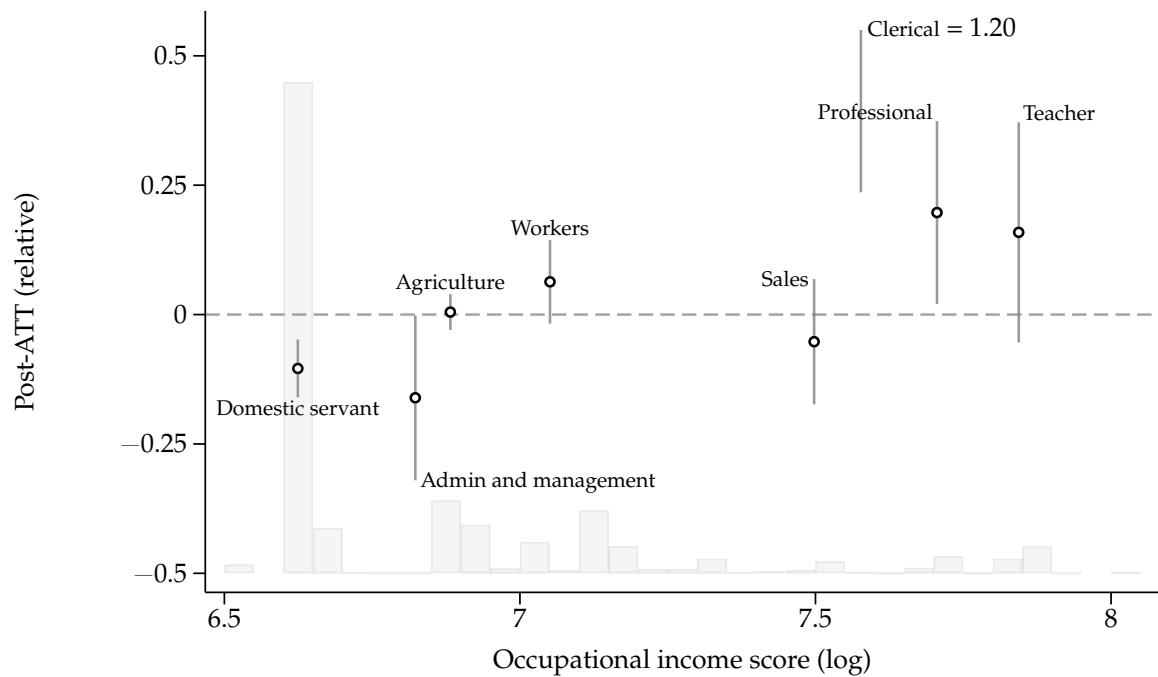


FIGURE 9. EFFECTS BY OCCUPATIONAL SECTOR AND INCOME SCORE

Notes: Points report average effects at event times 6–20 on employment in each occupational group, relative to the group’s reference-cohort mean; vertical bars give 95 percent confidence intervals. Clerical work is a separate group, but its relative estimate (1.20) lies outside the displayed range because its reference mean is small. Horizontal position gives the log occupational income score from the 1930 census; background bars show its distribution among women with occupations. Sales and agriculture are adjusted for census under-reporting. Estimates use the main doubly robust specification. Standard errors clustered by school district.

TABLES

TABLE 1—THE RISE OF PRIMARY EDUCATION, 1812–1910

Year	Enrolment ratio, %	School year in days	Teachers	Schools	Teachers per school
1812	5.4	36			
1814				957	
1839			1,537	1,516	1.01
1843	21.2	60			
1847				2,785	
1868	64.9	89	7,045	6,919	1.02
1890	72.9	122	13,508	10,563	1.28
1910	75.3	166	21,585	14,894	1.45

Notes: Enrollment ratios cover children aged 7–14. From 1858, school and teacher counts include elementary schools (*folkskolor*) for ages 9–14 and junior schools (*småskolor*) for ages 7–8. Data from [Carlson \(1870\)](#) and [Westberg \(2017\)](#).

TABLE 2—SUMMARY STATISTICS: SCHOOL DISTRICTS AND TEACHERS

	Registry year		
	1876	1882	1889
<i>Panel A. School districts</i>			
School districts (stable)	2223	2223	2223
School districts (in registry)	2276	2351	2400
Schools	4519	5020	5451
Teachers	3569	4220	5420
Female teachers	286	560	1127
Female teachers (%)	8.01	13.27	20.79
Vacant positions (%)	4.93	5.30	4.54
Teachers per school district	1.69	2.00	2.55
Female teachers per school district	0.13	0.26	0.50
School districts with female teachers (%)	5.53	11.34	17.05
School districts with only female teachers (%)	0.54	1.30	1.12
<i>Panel B. Female teachers</i>			
Age	31.70 (7.20)	32.61 (7.25)	33.89 (8.11)
Years since graduation	6.13 (3.90)	7.88 (4.72)	10.00 (5.88)
Years in current position	3.33 (3.34)	5.01 (3.96)	7.51 (5.54)
School district population density (per km ²)	1539.15 (1788.59)	1333.53 (1739.31)	1688.67 (1883.52)
Distance from birthplace to teacher seminary of graduation (km)			219.67 (145.86)
Distance from teacher seminary of graduation to school district (km)			133.66 (138.59)
Distance from birthplace to school district (km)			254.12 (165.17)
<i>Panel C. Male teachers</i>			
Age	40.20 (10.61)	38.75 (10.10)	39.86 (10.67)
Years since graduation	16.87 (9.94)	15.38 (10.25)	16.41 (10.61)
Years in current position	12.56 (10.17)	10.69 (9.30)	12.04 (9.21)
School district population density (per km ²)	123.06 (534.60)	130.29 (528.25)	169.92 (632.94)
Distance from birthplace to teacher seminary of graduation (km)			108.53 (112.96)
Distance from teacher seminary of graduation to school district (km)			111.69 (101.98)
Distance from birthplace to school district (km)			178.57 (145.57)

Notes: Table covers 2,223 stable school districts in the 1876, 1882, and 1889 teacher registers described in Section 3. School counts are from Statistics Sweden (BiSOS P), and population density uses the 1880 census. Female-teacher shares exclude vacant positions. Panels B and C report teacher-level means with standard deviations in parentheses.

TABLE 3—BALANCE

Variable	Mean (SD)		Normalized difference	Distinct observations
<i>Panel A. School districts</i>				
	Treated	Never treated		
Sex ratio, 1859	1.07 (0.09)	1.05 (0.07)	0.25	1,854
Marriages per 1,000, 1859	8.06 (2.74)	8.21 (3.43)	−0.05	1,853
Female net migration per 1,000, 1859	−0.25 (17.60)	−3.73 (20.39)	0.18	1,645
Emigration per capita, 1867–1879	0.05 (0.05)	0.05 (0.05)	0.01	2,188
<i>Panel B. Seminary cities</i>				
	Female	Male		
Girls’ schools, 1860	1.75 (2.87)	1.00 (1.00)	0.35	13
Newspapers, 1860	2.75 (2.87)	2.00 (1.22)	0.34	13
Population growth, 1840–1860	0.37 (0.03)	0.47 (0.15)	−0.91	13
<i>Panel C. Dioceses</i>				
	Female	Male		
Bishops supporting coeducation, 1858	0.25 (0.50)	0.11 (0.33)	0.33	13
Fixed schools per parish, 1839	0.87 (1.10)	0.32 (0.34)	0.67	13
Excluding Stockholm	0.32 (0.21)	0.32 (0.34)	0.00	12

Notes: Table covers characteristics measured before the first female-teacher hires. Panel A compares treated with never-treated school districts; Panels B and C compare locations assigned female- and male-only seminaries in 1861. Cells report means with standard deviations in parentheses. The normalized difference is the first-group mean minus the second-group mean, divided by their pooled standard deviation. Sex ratio is women per man. School-district measures except emigration use the latest pre-treatment SHiPS observation, typically 1859.

TABLE 4—EFFECTS ON DEMOGRAPHIC MEDIATORS

	Marriage		Migration	
	(1) Married	(2) Δ Age	(3) Urban	(4) Sthlm
ATT	0.524 (0.570)	−0.326*** (0.119)	−0.814 (0.593)	−0.219 (0.348)
Included	All	Currently married	Born rural	→ From Sthlm
Baseline mean	60.46	−3.38	19.22	8.43
Pre-trend p-value	0.34	0.52	0.80	0.59
Observations	3,553,063	1,996,485	3,186,667	3,492,059

Notes: Columns report separate estimations for the indicated outcomes; binary outcomes are multiplied by 100. *Married* includes married, widowed, and divorced women. *Δ Age* is the husband's age minus the wife's. *Urban* denotes birth in a rural parish and residence in an urban area; *Sthlm* denotes birth outside Stockholm and residence in Stockholm. ATT averages event times 6 and above. The pre-trend p-value tests whether the first five pre-treatment estimates are jointly zero. Standard errors clustered by school district. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE 5—EFFECTS ON PETITIONING: TREATMENT-EFFECT HETEROGENEITY

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
ATT	2.270*** (0.525)	2.034*** (0.572)	1.760*** (0.606)	0.421 (1.346)	1.726*** (0.552)	1.035 (0.678)	3.511*** (0.745)
Included	All	Signatures > 0	All	Born urban	Born rural	Patronymic	Non-patronymic
Baseline mean	10.03	11.13	10.03	15.37	8.38	9.37	10.70
Pre-trend p-value	0.45	0.57	0.70	0.13	0.49	0.09	0.92
Birth County FE	✓	✓	✓	✓	✓	✓	✓
Census District FE			✓				
Observations	824,308	676,307	824,308	84,453	739,855	513,098	311,210

Notes: The outcome equals 100 for matched petition signers and 0 for matched non-signers. Matching uses municipality blocks and a Jaro–Winkler cutoff of 0.9; Appendix E gives details and Tables D.6 and D.7 vary the cutoff. *Included* identifies each estimation sample. The baseline mean is the reference-cohort mean at event time -1 ; ATT averages event times 6 and above. The pre-trend p-value tests whether the first five pre-treatment estimates are jointly zero. Standard errors clustered by school district. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE 6—EFFECTS ON NON-DOMESTIC WORK: TREATMENT-EFFECT HETEROGENEITY

	(1)	(2)	(3)	(4)	(5)
ATT	1.130*** (0.349)	2.037* (1.093)	0.727 (0.457)	0.867** (0.398)	1.430*** (0.533)
Included	All	Born urban	Born rural	Patronymic	Non Patronymic
Baseline mean	12.69	19.31	9.97	12.68	12.70
Pre-trend p-value	0.14	0.06	0.81	0.41	0.40
Observations	3,553,063	366,396	3,186,667	1,962,109	1,590,954

Notes: The outcome equals 100 for a non-domestic occupation and 0 otherwise. *Included* identifies each estimation sample. The baseline mean is the reference-cohort mean at event time -1 ; ATT averages event times 6 and above. The pre-trend p-value tests whether the first five pre-treatment estimates are jointly zero. Standard errors clustered by school district. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE 7—EFFECTS ON POLITICAL MOBILIZATION

	Suffrage organisation				Voting 1919		
	(1) Teachers	(2) Petitioning	(3) Extensive	(4) Intensive	(5) In person	(6) Via husband	(7) Turnout gap
β^{2SLS}	2.713*** (0.911)	1.731*** (0.653)	1.311*** (0.495)	2.346*** (0.905)	1.576*** (0.534)	-1.293** (0.552)	-1.827*** (0.587)
σ	4.566	13.470	0.004	0.005	10.207	11.190	14.665
Hansen J	0.877	1.000	0.050	0.075	0.014	0.004	0.024
Observations	151	151	151	151	145	145	145

Notes: Outcomes are teachers per 1,000 inhabitants in 1910 (column 1); the share of adult women signing the 1913–14 petition, winsorized at the 99th percentile (column 2); the geographic reach and membership-weighted intensity of suffrage chapters in 1913 (columns 3–4); women’s in-person voting and voting through husbands, in percent (columns 5–6); and the male–female gap in in-person voting (column 7). Appendix B.3 defines the suffrage measures. All outcomes are z-standardized; σ reports their full-sample standard deviations. Hansen J reports the overidentification-test p-value. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

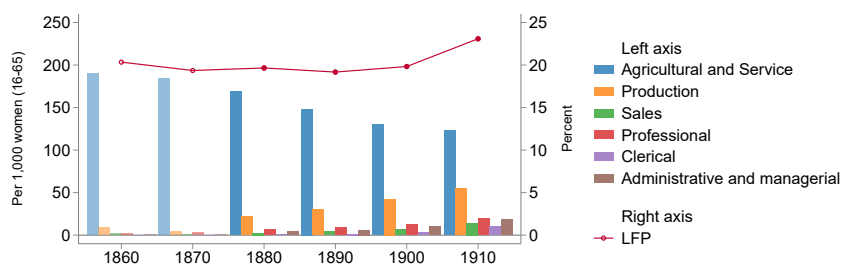
APPENDIX

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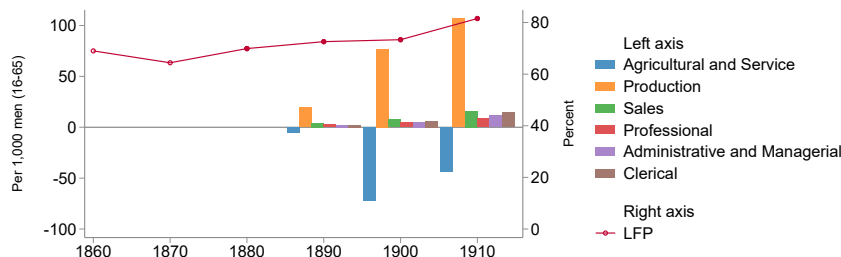
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A. APPENDIX FIGURES

(A) WOMEN (IN LEVELS)



(B) MEN



(C) MEN (IN LEVELS)

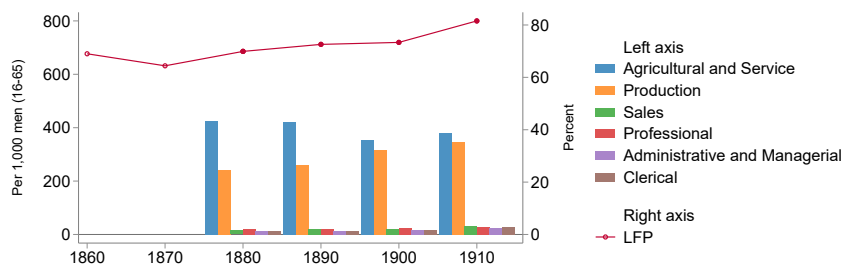


FIGURE A.1. OCCUPATIONAL STRUCTURE, 1880–1910

Notes: Bars show changes in employment relative to 1880 across six sectors; lines show labor-force participation among women and men aged 15–75. Data from the 1860–1870 censuses ([Statistiska Central-Byrån, 1865, 1874](#)), the 1880–1910 censuses ([SwedPop, 2024](#)), and [Molinder \(2022\)](#).

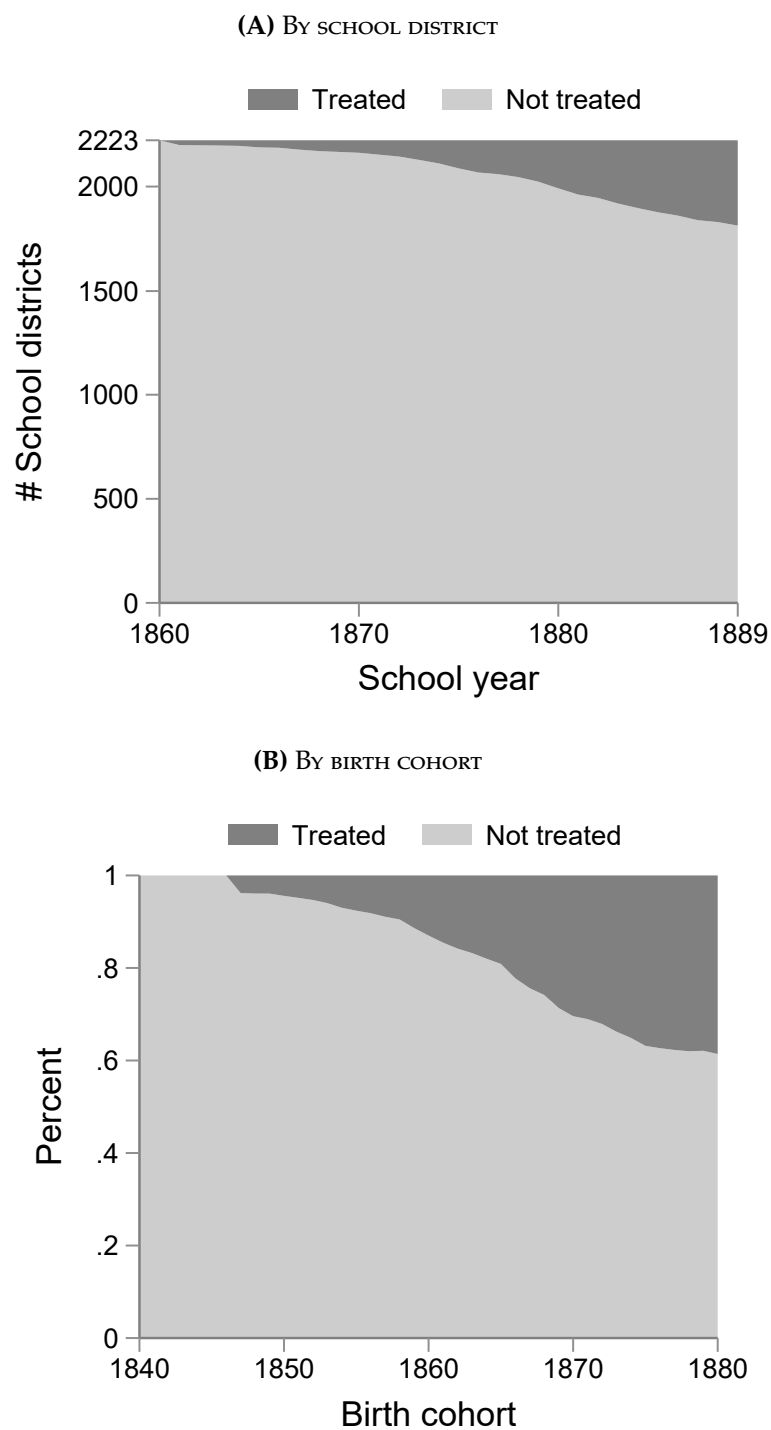


FIGURE A.2. TREATMENT TIMING AND COHORT EXPOSURE

Notes: Panel A reports the staggered arrival of first female teachers across school districts. Panel B reports the share of each birth cohort exposed during primary school.

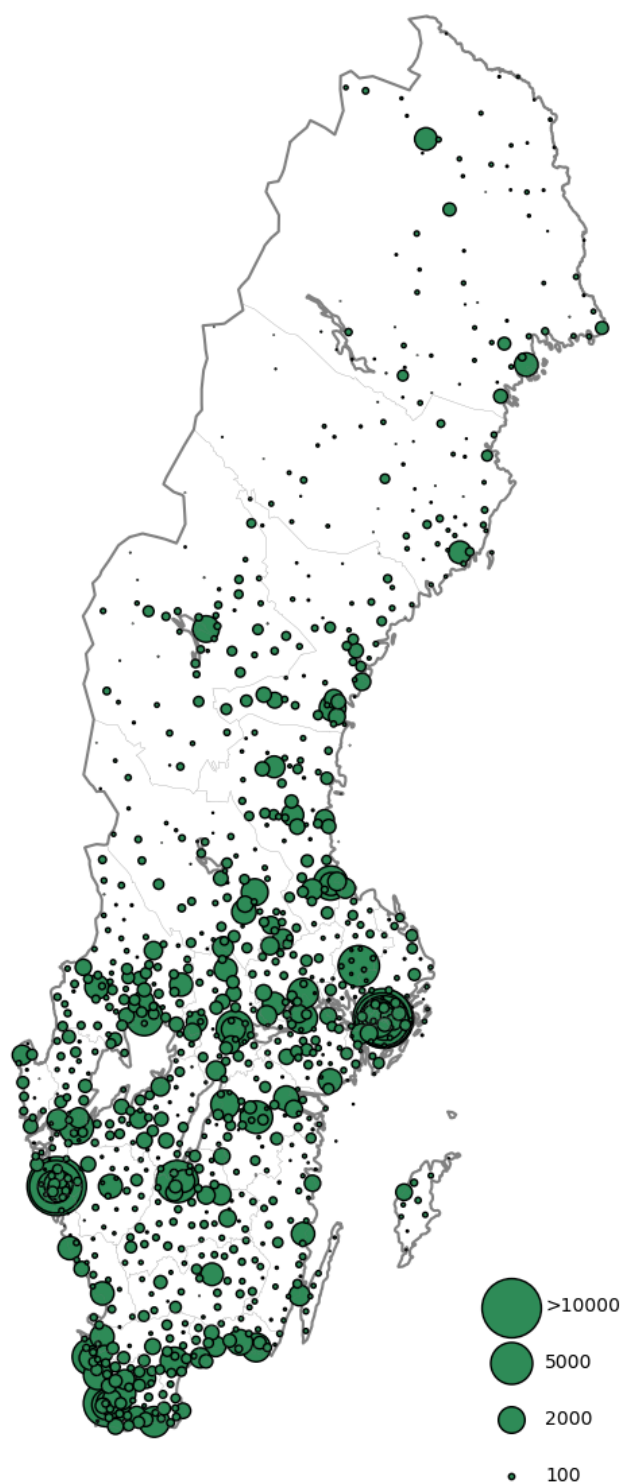


FIGURE A.3. MAPPING SUFFRAGE PETITION SIGNATORIES, 1913–14

Notes: Petition signatories are grouped into 1,000 K-means nearest-neighbor clusters. Circle size is proportional to signatories; boundaries are 1910 counties.

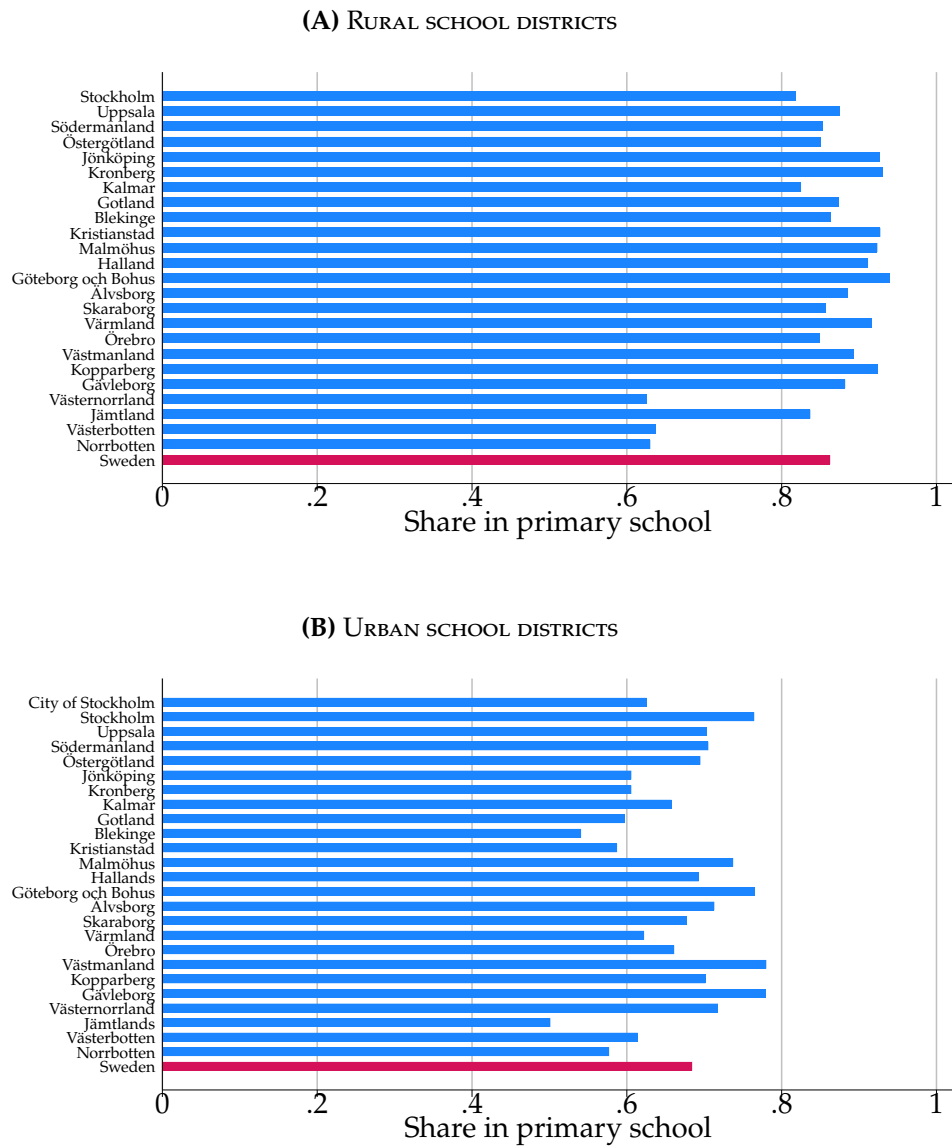


FIGURE A.4. SCHOOL ENROLLMENT RATIOS BY COUNTY FOR AGES 7–14, 1882

Notes: Panels report the share of children aged 7–14 enrolled in primary school by county and district type in 1882. Primary schools include *folkskolor*, *mindre folkskolor*, and *småskolor*; higher public, private, and home schooling are excluded. Data from [Department of Ecclesiastics \(1887, Table 7\)](#).

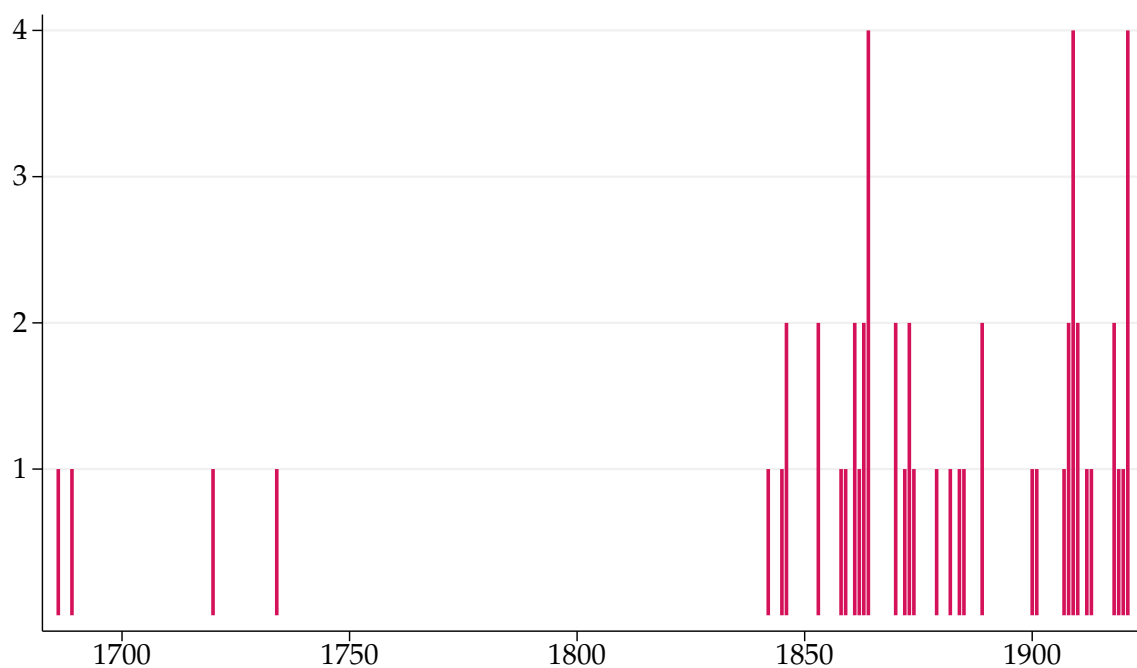


FIGURE A.5. CRITICAL MOMENTS IN WOMEN’S RIGHTS, SWEDEN, 1686–1921

Notes: Bars count Swedish women’s-rights events by year. Nine of the 17 reforms in 1842–1864 concerned women’s education. Data from [KvinnSam \(2019\)](#), [Du Rietz \(2013\)](#), [Manns \(1987\)](#), and [Wikipedia \(2026\)](#).

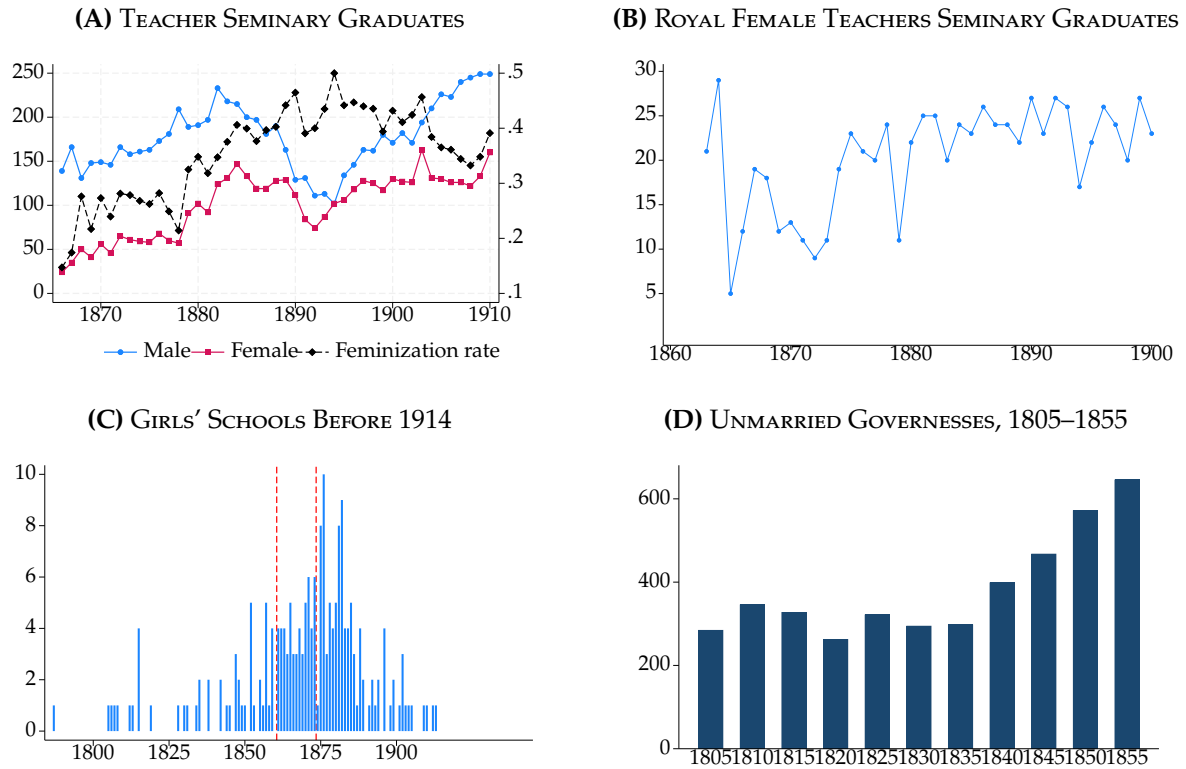


FIGURE A.6. BACKGROUND TO THE FEMINIZATION OF TEACHING

Notes: Panels report teacher-seminary graduates (A), Royal Female Teachers Seminary graduates (B), girls' schools before 1914 (C), and unmarried governesses in 1805–1855 (D). Data from [Folkundervisningskommittén \(1912\)](#), Archive Acc2000/30 (Kungliga Högre lärarinneseminariet), [Heckscher \(1914\)](#), and [Statistics Sweden \(1949\)](#), respectively.

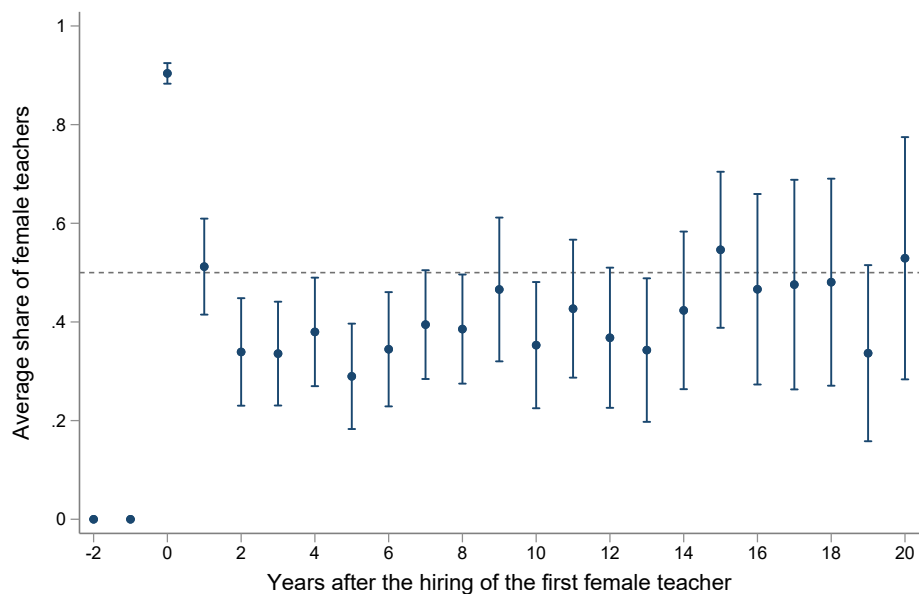


FIGURE A.7. AVERAGE SHARE OF FEMALE TEACHERS IN DISTRICTS HIRING WOMEN BEFORE 1890

Notes: Points report the average female-teacher share by years since first hiring among districts that hired a woman before 1890. Data from the teacher registers.

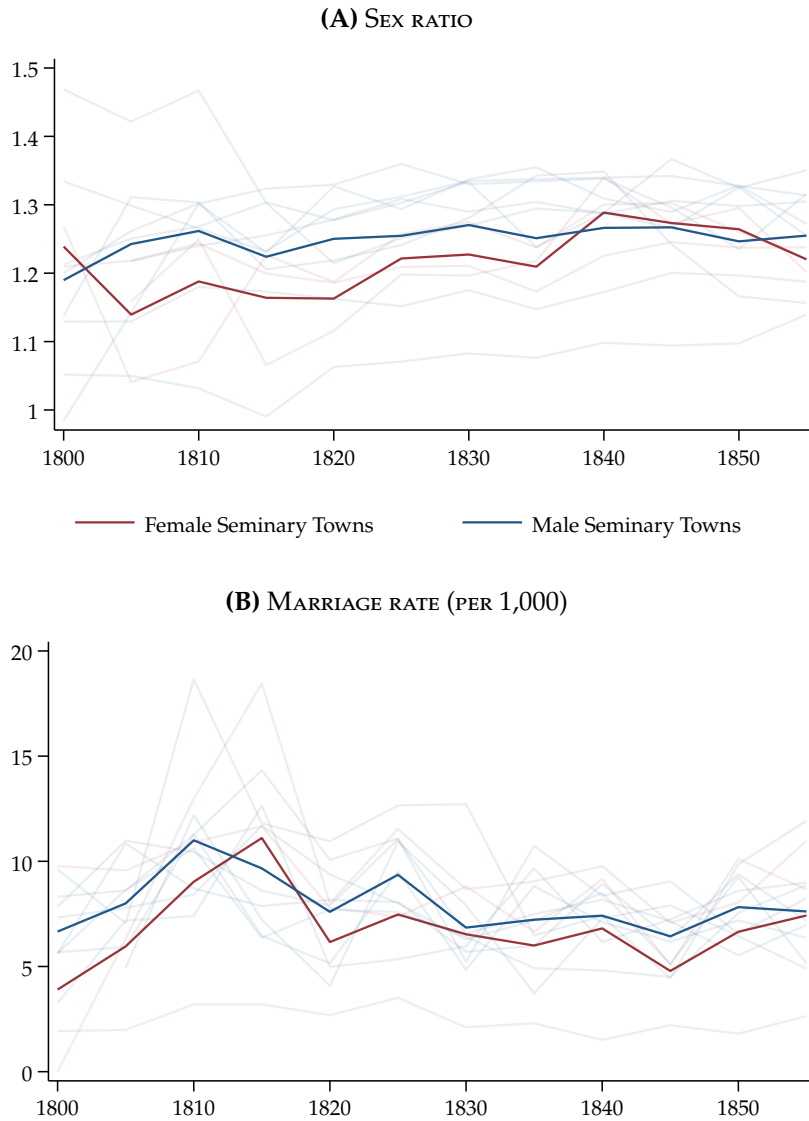


FIGURE A.8. CITY DEMOGRAPHICS BY TEACHER SEMINARY GENDER BEFORE 1859

Notes: Panel A reports women per man and Panel B marriages per 1,000 inhabitants in seminary cities before 1859. Seminary gender follows the 1861 classification. Data from SHiPS ([Demographic Data Base](#), CEDAR, Umeå University, 2025).

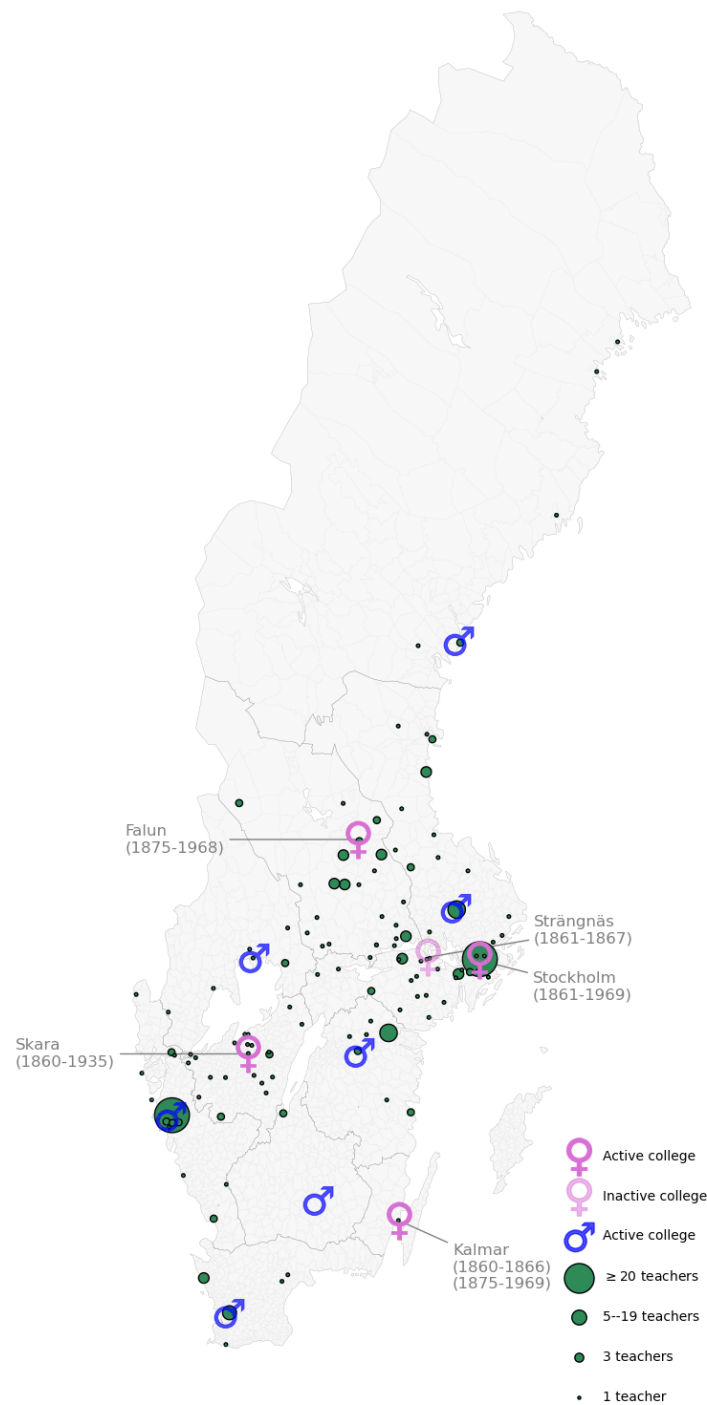


FIGURE A.9. FEMALE TEACHERS, 1876

Notes: Points mark school districts in the 1876 register; marker size is proportional to female teachers. Triangles mark seminaries operating in 1876. Data from the 1876 teacher register.

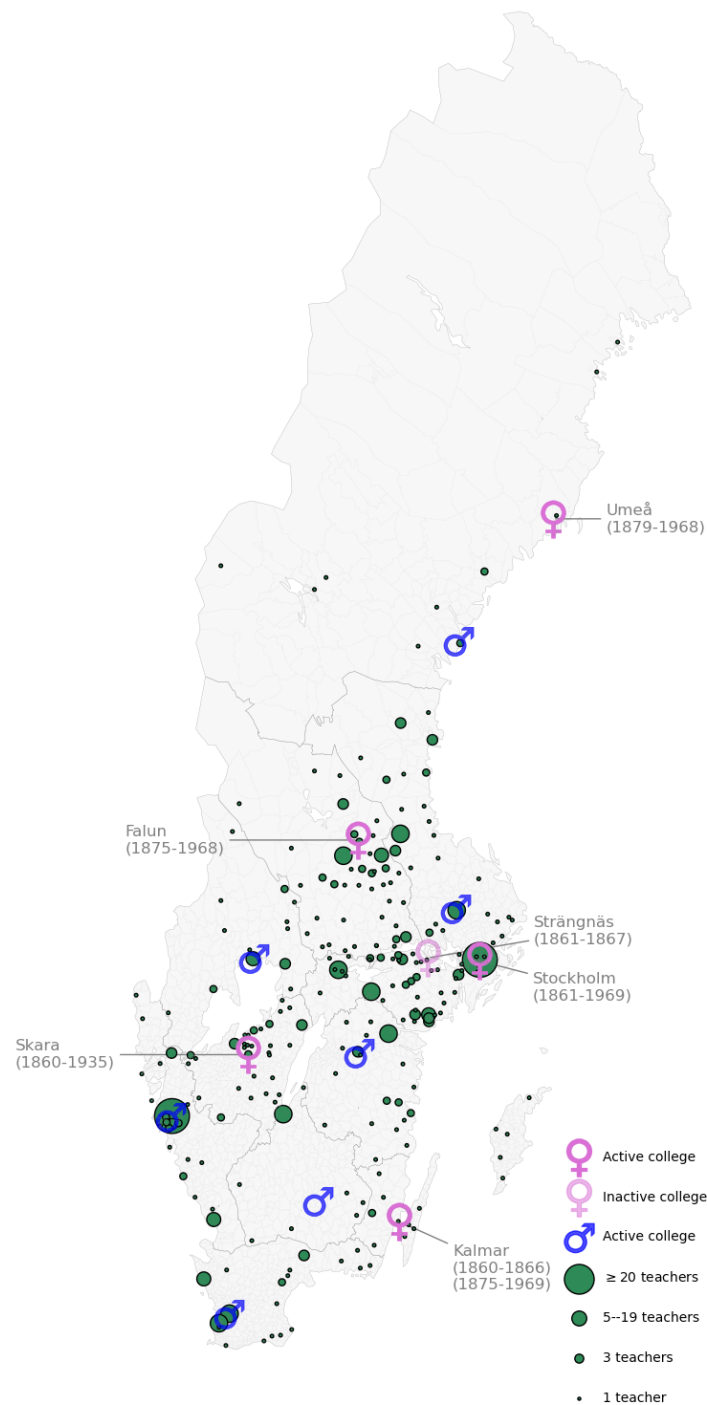


FIGURE A.10. FEMALE TEACHERS, 1882

Notes: Points mark school districts in the 1882 register; marker size is proportional to female teachers. Triangles mark seminaries operating in 1882. Data from the 1882 teacher register.

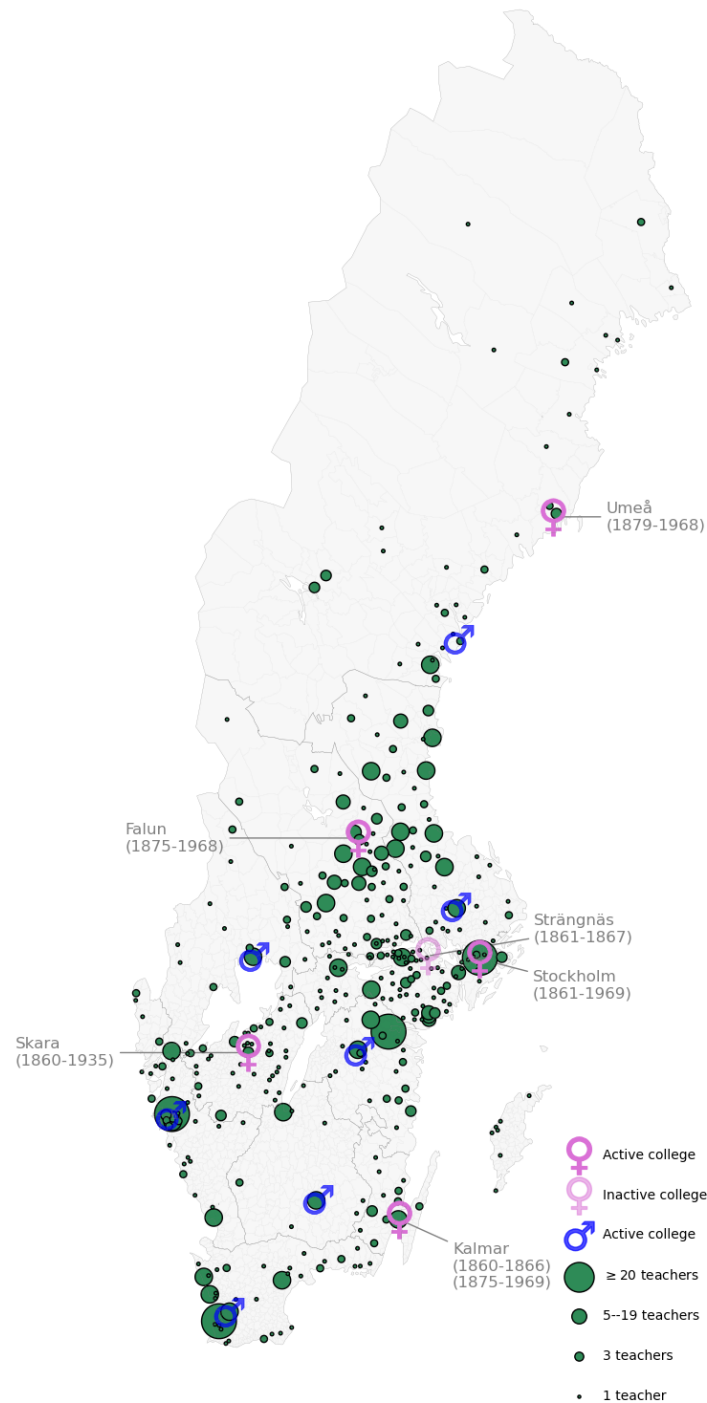


FIGURE A.11. FEMALE TEACHERS, 1889

Notes: Points mark school districts in the 1889 register; marker size is proportional to female teachers. Triangles mark seminaries operating in 1889. Data from the 1889 teacher register.

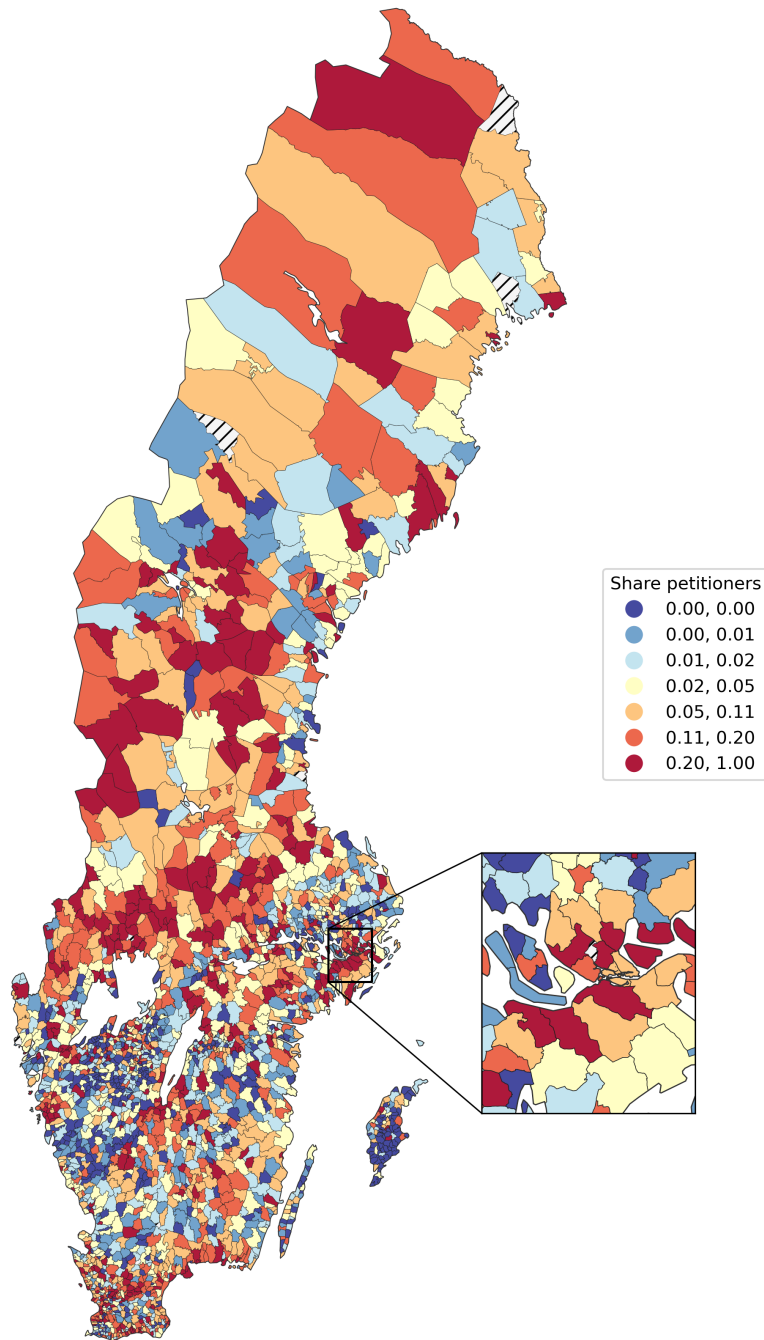


FIGURE A.12. PETITION SIGNATURES AS A SHARE OF ADULT WOMEN

Notes: Colors give petition signers as a share of adult women in each census district. Red denotes higher and blue lower petitioning intensity; districts are grouped into seven equal-sized bins.

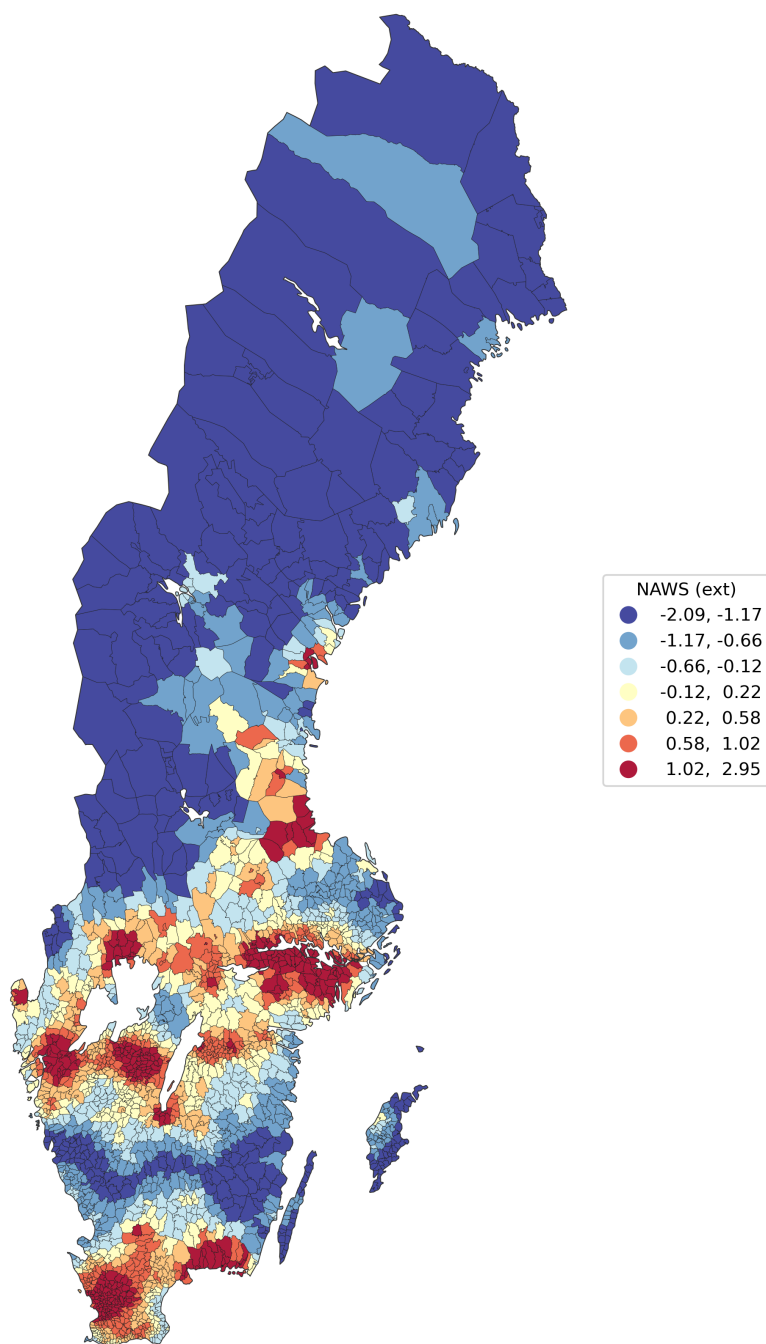


FIGURE A.13. GEOGRAPHIC REACH OF THE SUFFRAGE MOVEMENT, 1913

Notes: Colors map the geographic reach of National Association for Women's Suffrage chapters in 1913. Appendix B.3 defines the measure.

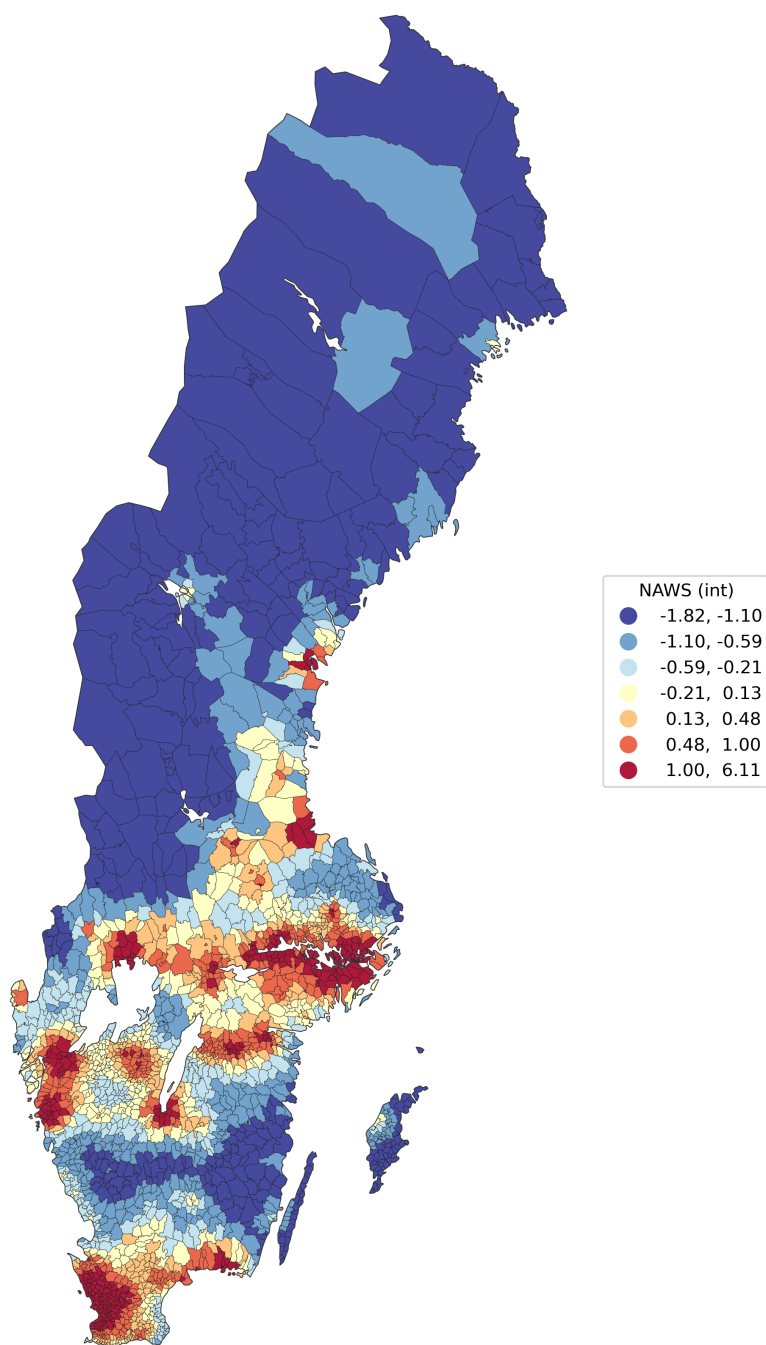


FIGURE A.14. LOCAL MEMBERSHIP IN THE SUFFRAGE MOVEMENT, 1913

Notes: Colors map local suffrage-movement membership in 1913. Appendix B.3 defines the measure.

B. DATA APPENDIX

B.1 Construction of Stable School Districts

To facilitate record linkage and combine data across geographic units, we construct a digital map of nineteenth-century school districts. Although school districts often coincided with other administrative boundaries, such as census districts, municipalities, and parishes, there were many exceptions to a one-to-one match between these boundaries and school districts. Over time, school districts and other administrative boundaries were merged or subdivided, sometimes in parallel and sometimes not.

We start from shapefiles of nineteenth- and twentieth-century Swedish administrative boundaries ([Junkka, 2018](#); [Swedish National Archives, 2008](#)). Using digitized historical maps from the Swedish Land Survey, we manually construct vector geometries for all school districts in 1876, 1882, and 1889, drawing on teacher registers to identify which parishes belong to each district. We then use these maps to define stable school districts that satisfy three criteria:

1. Their polygons cover all of Sweden, are mutually exclusive, and have boundaries that remain fixed over time.
2. Each school district observed in 1876, 1882, or 1889 maps into exactly one stable school district.
3. Individuals born in the same place between 1840 and 1880 are assigned to the same stable school district, even when historical boundaries changed.

Two examples illustrate how we construct these stable school districts:

- In 1876, the City of Gothenburg formed a single school district. In 1882 and 1889, however, it was subdivided into parish-based school districts. We therefore treat the entire City of Gothenburg as one stable school district.
- The parishes *Södra Fågelås* and *Norra Fågelås* in Skaraborg County were separate school districts in 1876, 1882, and 1889. In the 1880, 1890, and 1900 censuses, however, they belonged to the same census district, so individuals born in either parish were recorded simply as born in *Fågelås* Parish. They appear separately only in the 1910 census. We therefore do not treat *Södra Fågelås* and *Norra Fågelås* as separate stable school districts. Instead, we use their union, *Fågelås*, as a single stable school district.

B.2 Surname Types and Socioeconomic Status

Individuals' surnames from the censuses are classified using a rule-based procedure. We first clean, lower-case, and split raw names into components. Patronymics are identified using standard Swedish endings, including -son, -dotter, and -d:r. Elite surnames are identified by matching cleaned surnames to a curated list of noble families from historical registers, excluding patronymic forms. We also flag Latinized (-ius/-eus), Greek-style (-ander), and French-influenced (-ell/-ér/-én) endings, following linguistic criteria used in prior work (Clark, 2012). Each individual is then assigned to one of three mutually exclusive groups: patronymic surnames, elite surnames, or other non-patronymic surnames.

Figure B.15 shows the distribution of surname types across five social groups that previous research has identified as relevant for studying nineteenth-century living conditions.

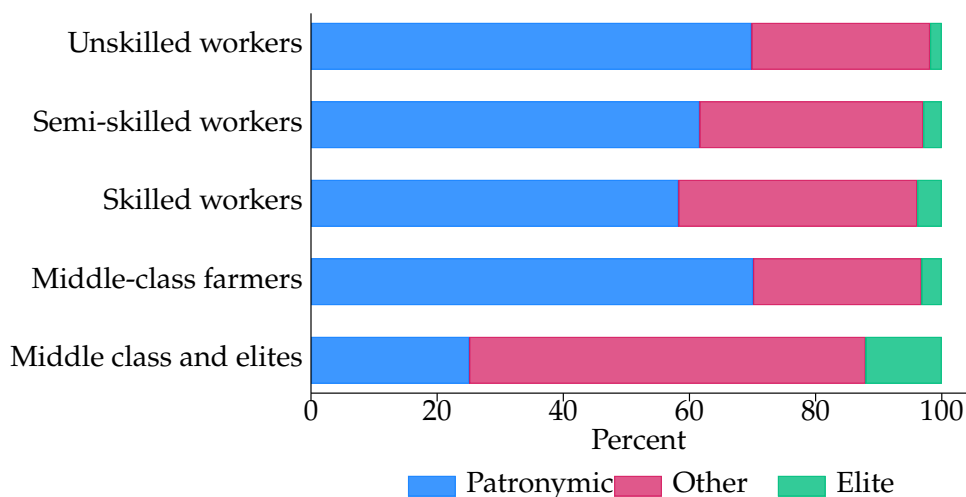


FIGURE B.15. SURNAME-TYPE PREVALENCE BY SOCIOECONOMIC GROUP

Notes: Bars report surname-type shares within socioeconomic groups defined from occupations using the SOCPO scheme (Putte and Miles, 2005). Elite surnames include noble, French-influenced, Latinized, and Greek-style names. Sample includes men and women in all four censuses with a surname and a classifiable occupation or title.

B.3 Variable Definitions

Unless otherwise specified, binary variables are coded as 0 and 100 rather than 0 and 1 to ease interpretation.

Census District. In each census (1880, 1890, 1900, and 1910), all households are assigned to a census district, which can be either territorial or non-territorial.³⁵ To the best of our ability, we match all non-territorial districts to territorial units. These clusters of districts form the basis for our census-district definition.

School District. See Appendix B.1.

Seminary-Area Characteristics. Table 3 classifies seminary cities by the gender assigned to their seminary in 1861. Bishop support records whether the diocesan consistory supported admitting women to existing seminaries in the 1858 consultation. Only Stockholm and Härnösand did so, one in each seminary group; the remaining consistories favored separate institutions (Folkundervisningskommittén, 1912, pp. 64–65). Fixed schools per parish are measured in 1839.

Urban. We use the urban and rural parish definitions from SwedPop (2024).

Petitioning. For all women born in 1895 or earlier, petitioning is coded as 100, 0, or missing based on the matching procedure described in Appendix E.

Petitioning Intensity. Petitioning intensity is the number of petition signatures geocoded to coordinates inside a 1910 census district, divided by the number of women born before 1895 in that district. The variable is then winsorized at the 99th percentile.

Labor-Market Position. Labor-market position is measured for all individuals in the censuses using HISCO codes and household position. When an individual has multiple valid HISCO codes, we select the code with the lowest value, which typically indicates higher status. For individuals without any title in the census, all labor-market variables are coded as 0.

We also apply two broad adjustments following Molinder (2022). First, we classify women married to working proprietors as having the same occupation as their husbands. In our sample, this adds roughly 5,000 women to the labor force in 1880 and 13,500 in 1910. Second, and quantitatively more important, we classify wives and adult daughters of small farmers as working in agriculture. This adds roughly 300,000 women to the labor force in each census.

The remaining sector where Molinder (2022) document substantial census under-reporting is industrial homework, predominantly in textile manufacturing. By 1912, when the first reliable estimates are available, as many as 34,400 women were employed in this sector. Under-reporting in the census is estimated to be nearly 100 percent.

³⁵Examples of non-territorial districts include ethnic, military, and prison districts.

Because there are no reliable individual-level census proxies for this work, we do not correct for it.

These adjustments are not consequential for our main finding, since both corrected occupations are considered domestic. However, they matter for estimating effects on total labor-force participation and on employment across sectors. Our baseline uses the corrected variables.

TABLE B.1—COMMON FEMALE OCCUPATIONS BY HISCO SECTOR

Sector of Work	Three Most Common Occupations
Professionals	Teacher, Midwife, Nurse
Administration and Management	Housekeeper, Manager, Sales Manager
Clerical	Office Clerk, Telephone Switchboard Operator, Cashier
Sales	Retail Salesperson, Working Proprietor, Street Vendor
Service	House Servant, Personal Maid, Charworker
Agricultural	General Farmer, Subsistence Farmer, Farm-Worker
Production	Hand and Machine Sewer, Worker, Day-Labourer

Notes: Entries are the three most common five-digit HISCO occupations among women across all censuses.

Occupation by Skill Level. We use an open-source translation from HISCO to HIS-CLASS, which classifies occupations into four skill groups: unskilled, low-skilled, medium-skilled, and high-skilled ([Cedar Foundation, 2026](#)).

Occupational Income Score. The occupational income score is constructed from a sample of the 1930 census, where income is reported. We restrict the sample to individuals aged 20–65 with positive reported income. Because occupational titles in 1930 have not been HISCO-standardized, we match titles to HISCO codes using fuzzy string matching. We then calculate gender-specific average incomes by three-digit HISCO occupation. To reduce noise in small occupation cells, we shrink these means toward their corresponding two-digit HISCO parent mean using an empirical Bayes adjustment.

Patronymic Surname. See Appendix [B.2](#).

Suffrage Organizations. As a continuous measure of NAWS presence, we calculate each school district’s centrality relative to all NAWS chapters in 1913:

$$\text{Centrality}_s \equiv \frac{\sum_i w_i \exp[-\lambda d(c_s, n_i)]}{\sum_i w_i},$$

where c_s is the centroid of school district s , n_i is the location of chapter i , and w_i is its weight. We set λ to the inverse of the median nearest-neighbor distance between chapters, $\lambda \approx 1/(17 \text{ km})$. With uniform weights, $w_i = 1$, the measure captures distance-weighted

proximity to chapters. We also weight chapters by the square root of membership, $w_i = \sqrt{N_i}$, where N_i denotes chapter membership, giving greater weight to larger chapters while imposing diminishing returns to size.

In-Person Turnout 1919. In-person turnout is the number of voters in a precinct who voted in person, divided by the number of eligible voters. This measure is constructed separately for men and women.

Voted via Husband 1919. Voted via husband is the number of female voters in a precinct whose ballot was cast by their husband, divided by the number of eligible female voters.

Turnout Gap 1919. The turnout gap is the difference between male and female in-person turnout.

TABLE B.2—DEFINITIONS OF LABOR-MARKET OUTCOMES

Outcome	HISCO	Relation	Position
Labor-force participation		–9, 21, 22	
Labor-force participation, adjusted*		–9, 21, 22	
Non-domestic occupation		–9, 21, 22	Is not a servant
Professionals [†]	[0–1]xxxx	–9, 21, 22	
Teachers	13xxx	–9, 21, 22	
Admin. and management	2xxxx	–9, 21, 22	
Clerical work	3xxxx	–9, 21, 22	
Sales [‡]	4xxxx	–9, 21, 22	
Service	5xxxx	–9, 21, 22	
Domestic servant	5xxxx	–9, 21, 22	Is a servant
Agriculture [§]	6xxxx	–9, 21, 22	
Workers	[7–9]xxxx	–9, 21, 22	

Notes: Empty cells include all values. In HISCO patterns, x is a wildcard and brackets give allowed digits; [7–9]xxxx, for example, includes codes beginning with 7, 8, or 9. Relation –9 assigns an occupation only to the individual; 21 and 22 denote future or former occupations. *Includes the sales and agriculture adjustments in [‡] and [§]. [†]Professionals comprise HISCO groups 0–1 and the few women without an occupation whose status codes 41 or 42 denote academic titles. [‡]Also includes women married to working proprietors (HISCO 41xxx). [§]Also includes women over 15 without a HISCO code who live in an agricultural household and are married to or related to its head; agricultural households have a head in HISCO 6[0–2]xxx.

B.4 Sample Extracts from Historical Data

[illegible]

FIGURE B.16. EXTRACT FROM THE 1861 TEACHER TABULATION

Notes: Department of Ecclesiastics tabulation of teachers and schools by district. These 1861 counts provide the first pre-registry measure used to date female-teacher employment (Department of Ecclesiastics, 1861).

Blekinge län. Lunds stift.		Födelseår.	Examenår.	Amiralitet i hans befordr. in utställning såsom lärare	Debitations- i Pensions-år.	Postadress.
Församling.	Namn.					
Asarums	Lundén, Karl	27	46	49	49	600 Carlshamn.
	Svensson, Nils	29	50	50	61	600
	Petersson, Jonas	30	51	53	53	600
	Rydén, Johan Arvid	33	57	58	58	600
	Sellergren, Peter	33	58	62	62	600
	Olsson, Oscar	51	75	76	76	600
Augerums	Tegnér, Reinhold	38	59	65	65	600 Carlskrona.
	Rundqvist, Nils Hansson	43	63	73	73	600
Backaryds	Melander, Anders Larsson	33	50	52	52	600 Ronneby.
	Linder, Johannes	41	68	70	70	600
Carlshamns	Lindén, Sven Johan	19	51	51	60	600 Carlshamn.
	Herrlin, Åke Persson	44	65	68	68	600
	Rydén, Caspar	44	65	72	72	600
Carlskrona Amiralitets-	Wollin, Paulus Johan Christian	38	61	64	64	600 Carlskrona.
	Hansson, Bengt	44	69	72	72	600
	Hansson, Karl Herman	49	72	74	74	600
	Hannell, Johannes	33	51	54	76	600
Carlskrona stads-	Genström, Johan Alfred	38	59	61	61	700 Carlskrona.
	Carlsson, Peter Magnus	30	50	63	63	700
	Sjögren, Nils	31	54	63	72	700
	Engbladh, Johan Lorentz	46	71	72	72	700
	Tjensten otillsatt	—	—	—	—	700
Christanopels	Ekström, Måns August	40	60	68	68	600 Carlskrona.
	Tegnander, Johan	32	54	67	67	600
	Zeberg, Samuel Pontus	39	72	72	75	600
	Melin, Carl Victor	50	74	74	76	600
Edestads	Petersson, Hans Persson	28	46	47	48	600 Ronneby.
Eringsboda	Colliander, Johan	25	53	54	54	600 Eringsboda.
	Rosenqvist, Carl Ludvig	28	50	51	51	600

FIGURE B.17. EXTRACT FROM THE 1876 TEACHER REGISTER

Notes: Page from the 1876 teacher register used to construct the teacher data.

Notes: Page from the 1882 teacher register used to construct the teacher data.

Notes: Page from the 1882 teacher register used to construct the teacher data.

[illegible]

Notes: Pages from the 1889 teacher register used to construct the teacher data.

C. ADDITIONAL RESULTS

C.1 Effects of Emancipation

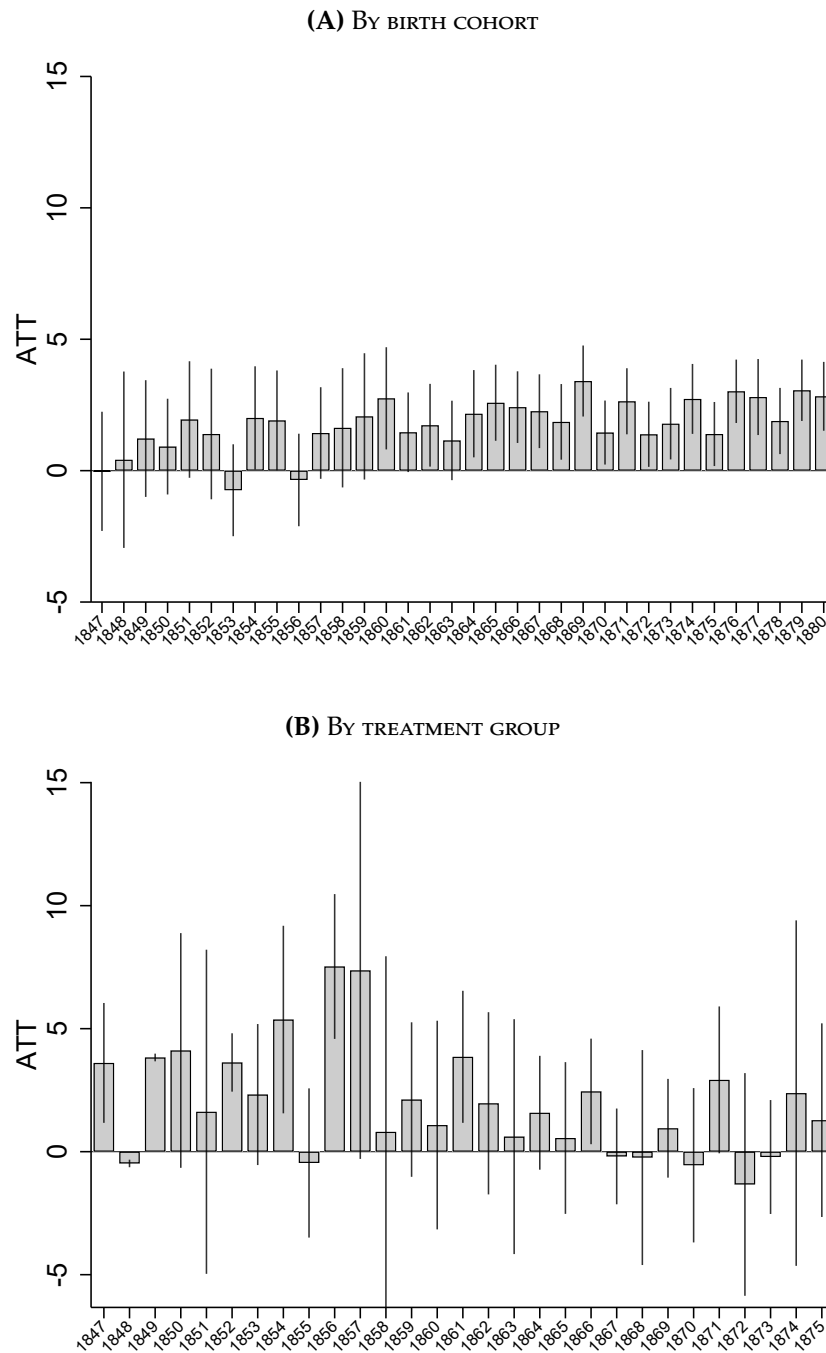


FIGURE C.20. EFFECTS ON PETITIONING: ALTERNATIVE AGGREGATION

Notes: Panel A aggregates petitioning effects by birth cohort; Panel B aggregates them by the year a district first hired a female teacher. Bars report estimates and spikes 90 percent confidence intervals.

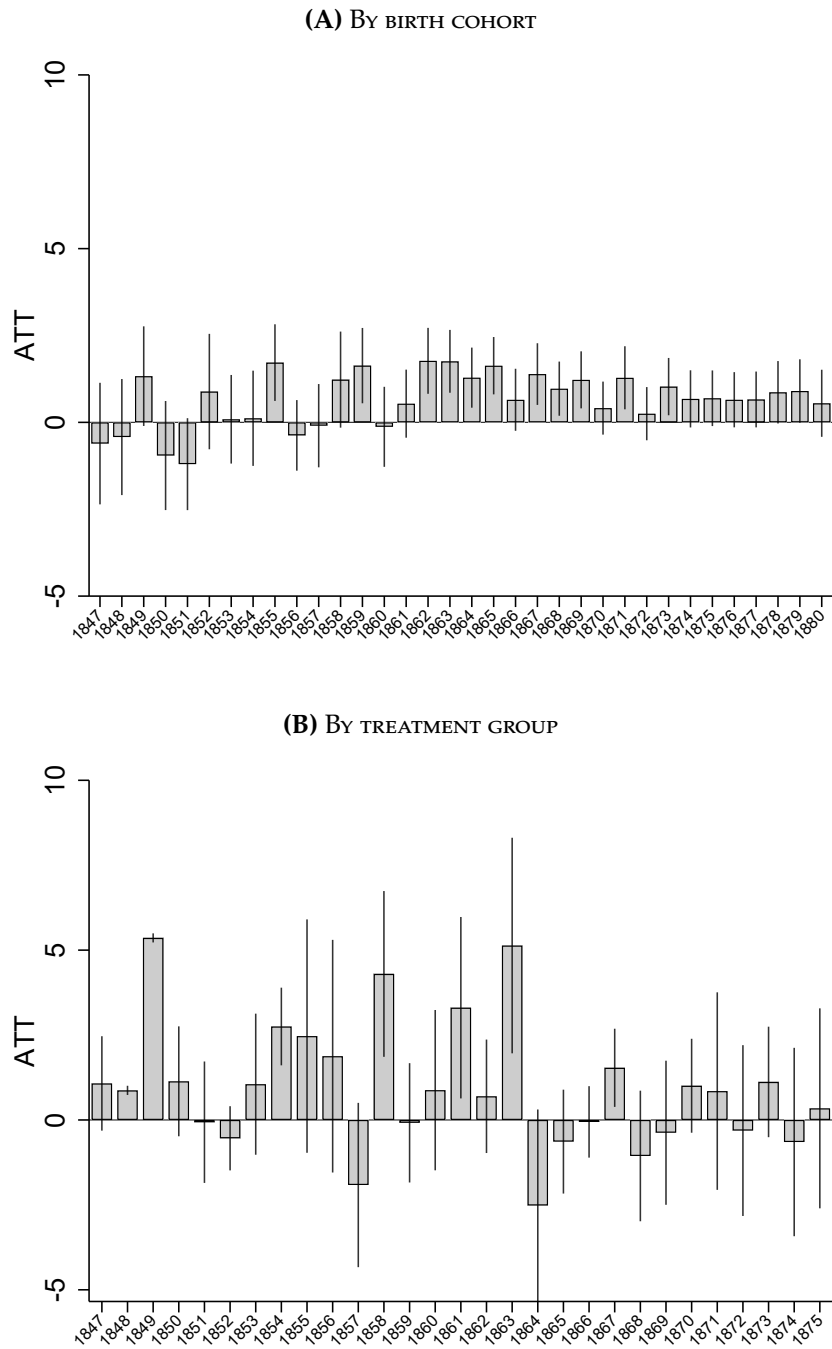


FIGURE C.21. EFFECTS ON NON-DOMESTIC WORK: ALTERNATIVE AGGREGATION

Notes: Panel A aggregates non-domestic-work effects by birth cohort; Panel B aggregates them by the year a district first hired a female teacher. Bars report estimates and spikes 90 percent confidence intervals.

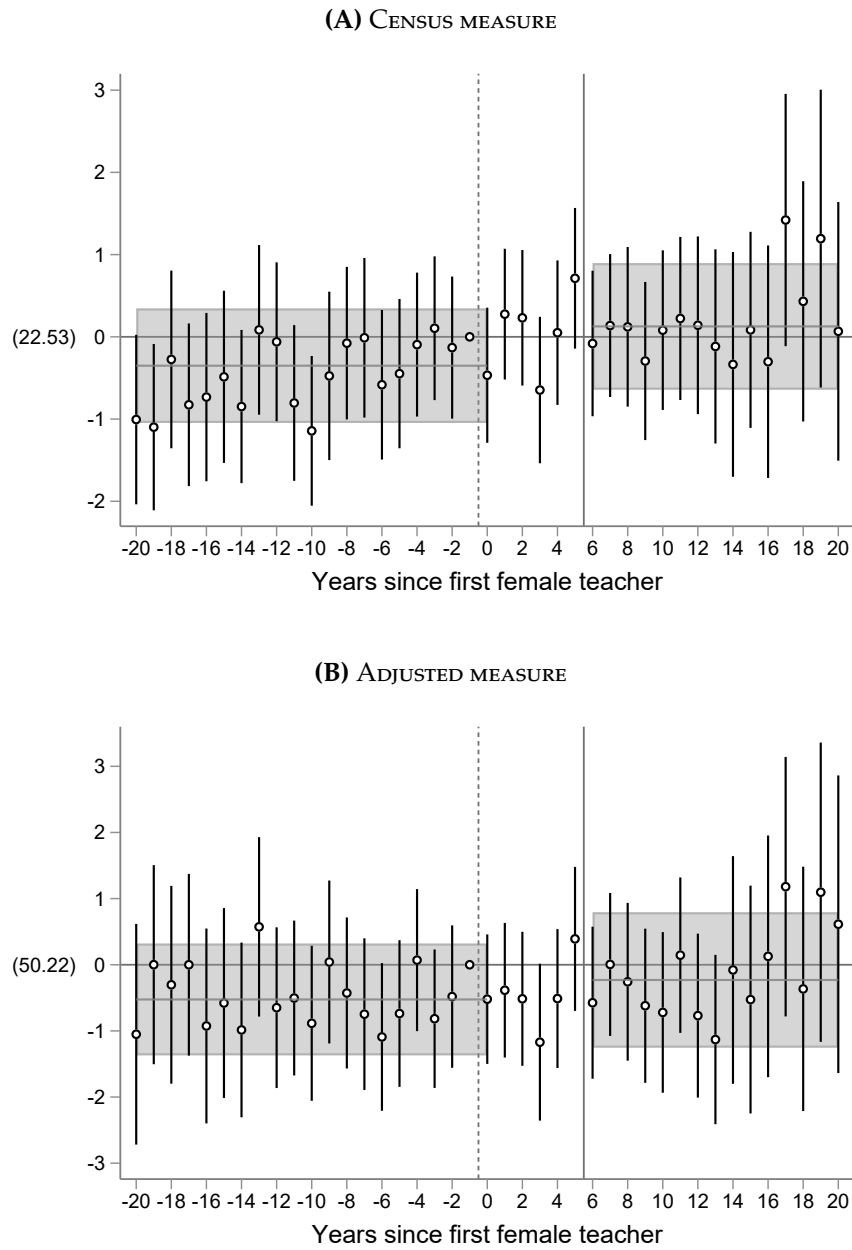


FIGURE C.22. EFFECTS ON FEMALE LABOR-FORCE PARTICIPATION

Notes: Panel A uses census-recorded labor-force participation; Panel B uses the alternative broad employment measure. Specification and inference follow Figure 8.

TABLE C.3—EFFECTS ON LABOR-MARKET OUTCOMES

	ATT	Std. Err.	Control mean	Pre-trends p-value
Non-domestic work				
Non-domestic work	1.130***	0.349	12.69	0.143
Labor-Force Participation				
Adjusted ^a	−0.230	0.523	50.22	0.253
As in census	0.127	0.393	22.53	0.625
Sectors of Work				
Professionals	0.344**	0.158	1.75	0.153
Administration and management	−0.240**	0.121	1.49	0.693
Clerical work	0.481**	0.197	0.40	0.648
Sales ^a	−0.134	0.158	2.56	0.462
Sales	0.186**	0.094	0.98	0.291
Service	−1.055***	0.395	12.79	0.996
Domestic servant ^b	−0.984***	0.269	9.45	0.984
Agriculture ^a	0.130	0.478	27.04	0.145
Agriculture	0.144	0.102	0.93	0.084
Workers	0.266	0.173	4.20	0.447
Teacher ^c	0.195	0.133	1.23	0.196
Skill level				
Unskilled	−0.755	0.468	38.75	0.334
Low skilled	0.256	0.294	6.12	0.693
Medium skilled	0.317	0.196	4.28	0.685
High skilled	−0.201*	0.111	0.77	0.273
Income				
Log occ. income score	0.012	0.009	6.88	0.821
Log occ. income score + 1	0.016	0.027	1.55	0.548

Notes: Sample includes women born 1840–1880 and observed at ages 20–59. Appendix B.3 defines the outcomes. ATT averages event times 6 and above; baseline mean is the reference-cohort mean at event time −1. The pre-trend p-value tests whether the first five pre-treatment estimates are jointly zero. Standard errors clustered by school district. ^a Sales and agriculture are adjusted for census under-reporting. ^b Subgroup of *Service*. ^c Subgroup of *Professionals*.

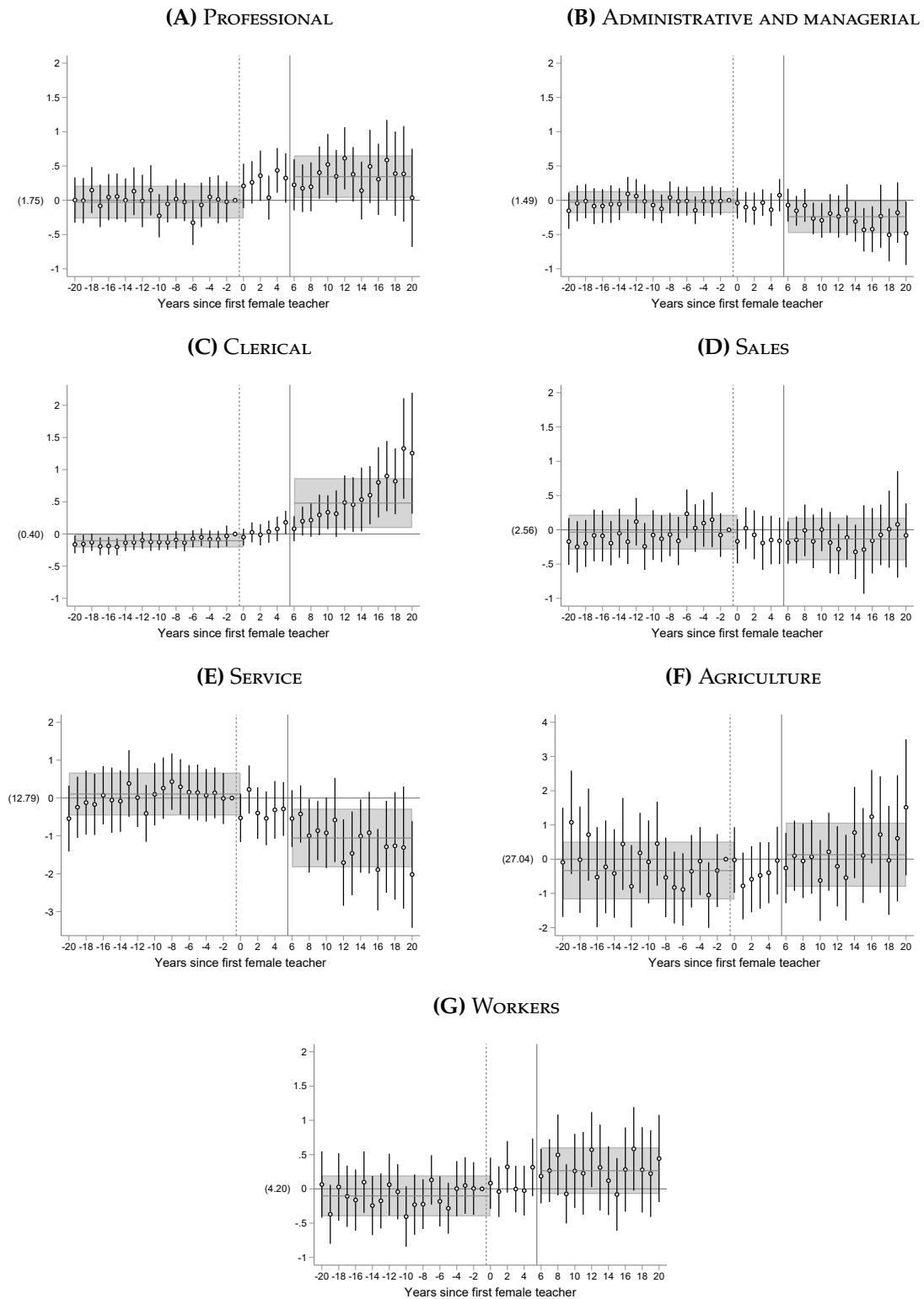


FIGURE C.23. EFFECTS ON OCCUPATIONS BY SECTOR

Notes: Panels report event-study estimates by major occupational sector. Specification, sample, and inference follow Figure 8; Appendix B.3 defines the outcomes.

C.2 Female Teacher Effect on Male Students

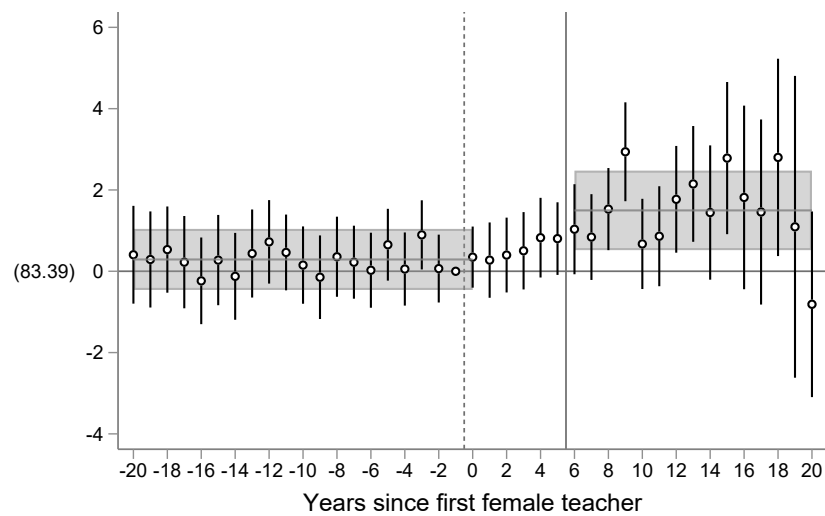


FIGURE C.24. EFFECTS ON MALE LABOR-FORCE PARTICIPATION

Notes: Event-study estimates use men in coeducational districts, where boys could have been exposed to a female teacher. Specification and inference otherwise follow Figure 8.

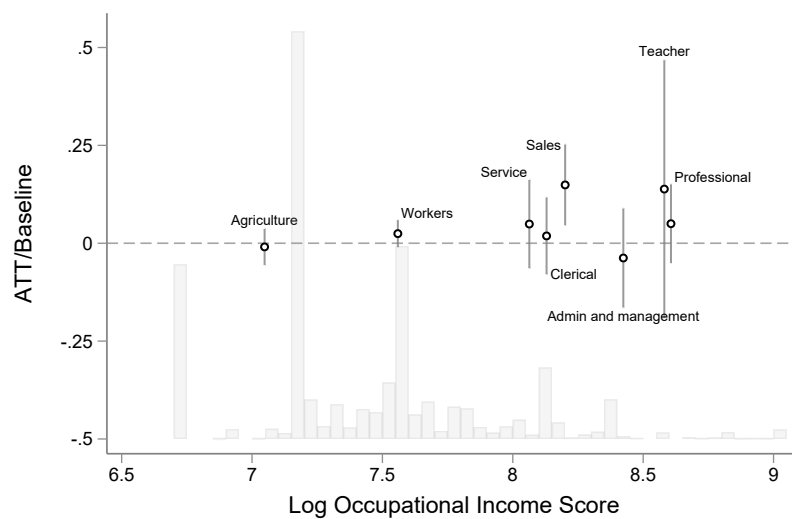


FIGURE C.25. EFFECTS BY OCCUPATIONAL SECTOR AND INCOME SCORE FOR MEN

Notes: Points report average treatment effects relative to the baseline mean for male employment in seven sectors and teaching. Horizontal position gives the log 1930 occupational income score; background bars show its distribution among men with occupations. Standard errors clustered by school district.

TABLE C.4—EFFECTS ON LABOR-MARKET OUTCOMES FOR MEN

	ATT	Std. Err.	Control mean	Pre-trends p-value
Labor Force Participaion	1.497***	0.499	80.79	0.096
Sectors of Work				
Professionals	0.188	0.193	1.99	0.802
Administration and management	−0.100	0.170	2.08	0.525
Clerical work	0.053	0.142	1.42	0.717
Sales	0.439 * **	0.155	1.77	0.321
Service	0.240	0.282	4.22	0.192
Agriculture	−0.263	0.662	34.46	0.660
Workers	0.939	0.664	34.85	0.660
Teacher	0.081	0.098	0.49	0.640
Skill level				
Unskilled	−0.916	0.616	27.58	0.753
Low skilled	0.475	0.523	19.14	0.512
Medium skilled	1.897 * **	0.607	31.18	0.775
High skilled	0.042	0.143	1.40	0.697
Income				
log Occ Income Score	0.017 * *	0.008	7.40	0.167
log Occ Income Score + 1	0.121 * **	0.037	5.96	0.081

Notes: Sample includes men born 1840–1880 and observed at ages 20–59. Appendix B.3 defines the outcomes. ATT averages event times 6 and above; baseline mean is the reference-cohort mean at event time −1. The pre-trend p-value tests whether the first five pre-treatment estimates are jointly zero. Standard errors clustered by school district. ^a Sales and agriculture are adjusted for census under-reporting. ^b Subgroup of *Service*. ^c Subgroup of *Professionals*.

TABLE C.5—EFFECTS ON MEN'S LABOR-FORCE PARTICIPATION: ROBUSTNESS CHECKS

	(1)	(2)	(3)
ATT	1.442*** (0.498)	1.955*** (0.616)	1.325*** (0.502)
Included	Only co-ed	Boys' schools treated	Boys' schools control
Baseline mean	80.79	83.39	80.79
Pre-trend p-value	0.13	0.04	0.10
Observations	3,026,690	3,325,215	3,325,215

Notes: Column 1 reports baseline estimates; columns 2–3 vary the treatment rule for districts with gender-separate schools. ATT averages event times 6 and above; baseline mean is the reference-cohort mean at event time −1. The pre-trend p-value tests whether the first five pre-treatment estimates are jointly zero. Standard errors clustered by school district.

D. ROBUSTNESS CHECKS

The robustness checks vary the three design choices most likely to affect the estimates. Tables [D.6](#) and [D.7](#) re-estimate petitioning effects at lower and higher Jaro–Winkler cutoffs. Table [D.8](#) varies the labor-market sample, comparison group, controls, and treatment definition. Table [D.9](#) reports reduced-form and alternative IV estimators, while Tables [D.10](#) and [D.11](#) use a wider teacher-seminary radius. The main results are similar across these alternatives.

TABLE D.6—EFFECTS ON PETITIONING: HIGHER-COVERAGE MATCHING SAMPLE

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
ATT	2.042*** (0.568)	1.830*** (0.609)	1.522** (0.634)	-0.667 (1.410)	1.677*** (0.602)	0.775 (0.692)	3.311*** (0.839)
Included	All	Signatures > 0	All	Born urban	Born rural	Patronymic	Non-patronymic
Baseline mean	11.38	12.45	11.38	17.74	9.41	10.29	12.50
Pre-trend p-value	0.34	0.47	0.75	0.13	0.49	0.13	0.69
Birth County FE	✓	✓	✓	✓	✓	✓	✓
Census District FE			✓				
Observations	819,446	688,336	819,446	83,968	735,478	509,571	309,875

Notes: The outcome equals 100 for matched petition signers and 0 for matched non-signers. Matching uses municipality blocks and a Jaro–Winkler cutoff of 0.85; Appendix E gives details. *Included* identifies each estimation sample. Baseline mean is the reference-cohort mean at event time -1 ; ATT averages event times 6 and above. The pre-trend p-value tests whether the first five pre-treatment estimates are jointly zero.

TABLE D.7—EFFECTS ON PETITIONING: HIGHER-PRECISION MATCHING SAMPLE

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
ATT	1.986*** (0.494)	1.761*** (0.554)	1.430*** (0.554)	0.244 (0.918)	1.418*** (0.494)	0.868 (0.631)	3.147*** (0.702)
Included	All	Signatures > 0	All	Born urban	Born rural	Patronymic	Non-patronymic
Baseline mean	8.13	9.27	8.13	12.51	6.78	8.08	8.18
Pre-trend p-value	0.40	0.55	0.47	0.22	0.55	0.09	0.60
Birth County FE	✓	✓	✓	✓	✓	✓	✓
Census District FE			✓				
Observations	835,673	659,488	835,673	86,017	749,656	520,761	314,912

Notes: The outcome equals 100 for matched petition signers and 0 for matched non-signers. Matching uses municipality blocks and a Jaro–Winkler cutoff of 0.95; Appendix E gives details. *Included* identifies each estimation sample. Baseline mean is the reference-cohort mean at event time -1 ; ATT averages event times 6 and above. The pre-trend p-value tests whether the first five pre-treatment estimates are jointly zero.

TABLE D.8—EFFECTS ON NON-DOMESTIC WORK: ROBUSTNESS CHECKS

	(1) Baseline	(2) 1	(3) 2	(4) 3a	(5) 3b	(6) 3c	(7) 4	(8) 5	(9) 6a	(10) 6b	(11) 6c
ATT	1.130*** (0.349)	1.221*** (0.420)	1.268*** (0.387)	1.245* (0.689)	1.029* (0.559)	1.045* (0.536)	1.134*** (0.349)	1.722** (0.798)	1.337*** (0.345)	1.189*** (0.403)	0.738** (0.358)
Baseline mean	12.69	14.10	12.69	11.33	12.67	14.86	12.69	12.69	12.27	12.52	12.13
Pre-trend p-value	0.14	0.13	0.14	0.69	0.21	0.94	0.17	0.43	0.84	0.87	0.25
Observations	3,553,063	2,490,141	3,206,806	907,299	1,149,000	861,218	3,553,063	3,553,063	3,553,063	3,553,063	3,553,063

Notes: Column 1 reports the baseline. Remaining columns exclude women aged 20–29; exclude cohorts born after 1875, who would have left school after the teacher panel ends in 1889; estimate separately by census year; use not-yet-treated districts; control for urban background; or define exposure assuming girls left school at age 13, 12, or 11 rather than 14.

TABLE D.9—EFFECTS OF FEMALE TEACHERS ON POLITICAL MOBILIZATION: ALTERNATIVE ESTIMATORS

	(1)	(2)	(3)	(4)	(5)
	Saturated	RF	Just identified	LIML	Abadie κ
Teachers					
β^{2SLS}	2.713*** (0.911)	0.709** (0.312)	2.363** (0.942)	2.738*** (0.925)	2.363** (0.933)
Petition intensity					
β^{2SLS}	1.731*** (0.653)	0.503* (0.275)	1.677** (0.854)	1.732*** (0.655)	1.672** (0.844)
NAWS Local Chapter					
β^{2SLS}	1.311*** (0.495)	0.632*** (0.123)	2.106*** (0.762)	1.634** (0.650)	2.081*** (0.725)
NAWS Membership					
β^{2SLS}	2.346*** (0.905)	0.458 (0.304)	1.528 (0.932)	3.480** (1.451)	1.614* (0.863)
Voting in person 1919					
β^{2SLS}	1.576*** (0.534)	0.157 (0.261)	0.532 (0.818)	1.731*** (0.630)	0.677 (0.726)
Voting through husband 1919					
β^{2SLS}	-1.293** (0.552)	-0.036 (0.250)	-0.121 (0.815)	-1.425** (0.676)	-0.284 (0.710)
Male-female turnout gap 1919					
β^{2SLS}	-1.827*** (0.587)	-0.229 (0.284)	-0.777 (0.885)	-2.036*** (0.684)	-0.923 (0.794)

Notes: Column 1 reports the baseline saturated IV; column 2 the reduced form; column 3 a just-identified IV without instrument-covariate interactions; column 4 limited-information maximum likelihood; and column 5 the normalized Abadie κ estimator (Słoczyński et al., 2025). Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE D.10—EFFECTS OF FEMALE TEACHERS ON POLITICAL MOBILIZATION: ALTERNATIVE INSTRUMENT RADIUS

	Suffrage organisation				Voting 1919		
	(1) Teachers	(2) Petitioning	(3) Extensive	(4) Intensive	(5) In person	(6) Via husband	(7) Turnout gap
β^{2SLS}	1.679*** (0.470)	0.680* (0.392)	0.876** (0.346)	1.498*** (0.572)	0.508* (0.286)	-0.127 (0.380)	-0.468 (0.337)
σ	4.566	13.470	0.004	0.005	10.207	11.190	14.665
Hansen J	0.785	0.700	0.000	0.564	0.397	0.108	0.237
Observations	601	601	601	601	592	592	592

Notes: Outcomes are teachers per 1,000 inhabitants in 1910 (column 1); petitioning, winsorized at the 99th percentile (column 2); the geographic reach and membership-weighted intensity of suffrage chapters in 1913 (columns 3–4); women’s in-person voting and voting through husbands (columns 5–6); and the male–female in-person voting gap (column 7). Appendix B.3 defines the suffrage measures. All outcomes are z-standardized; σ reports full-sample standard deviations. Hansen J reports the overidentification-test p-value. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE D.11—EFFECTS OF FEMALE TEACHERS ON POLITICAL MOBILIZATION: ALTERNATIVE ESTIMATORS AND INSTRUMENT RADIUS

	(1) Saturated	(2) RF	(3) Just identified	(4) LIML	(5) Abadie κ
Teachers					
β^{2SLS}	1.679*** (0.470)	0.250** (0.109)	1.973** (0.864)	1.694*** (0.477)	1.876*** (0.663)
Petition intensity					
β^{2SLS}	0.680* (0.392)	0.127 (0.103)	1.004 (0.795)	0.687* (0.409)	0.913 (0.613)
NAWS Local Chapter					
β^{2SLS}	0.876** (0.346)	0.516*** (0.077)	4.069*** (1.154)	1.576** (0.654)	3.151*** (0.721)
NAWS Membership					
β^{2SLS}	1.498*** (0.572)	0.156 (0.107)	1.230 (0.813)	1.657** (0.653)	1.268* (0.698)
Voting in person 1919					
β^{2SLS}	0.508* (0.286)	-0.079 (0.092)	-0.626 (0.771)	0.511* (0.296)	-0.328 (0.560)
Voting through husband 1919					
β^{2SLS}	-0.127 (0.380)	0.118 (0.093)	0.932 (0.795)	-0.113 (0.403)	0.603 (0.586)
Male-female turnout gap 1919					
β^{2SLS}	-0.468 (0.337)	0.115 (0.098)	0.911 (0.850)	-0.463 (0.357)	0.544 (0.612)

Notes: Column 1 reports the baseline saturated IV; column 2 the reduced form; column 3 a just-identified IV without instrument–covariate interactions; column 4 LIML; and column 5 the normalized Abadie κ estimator (Śloczyński et al., 2025). Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

E. PETITION DATA AND CENSUS LINKAGE

E.1 Transcription

We use version 0.1 of *Swedish Women's Suffrage Petitioners, 1913–14*, a transcribed and harmonized release of the nationwide petition ([Bounadi and Folkestad, 2026](#)). The National Archives of Sweden digitized the archival volumes through *I demokratins namn: kvinnorna som krävde rösträtt* on FromThePage. The citizen-science project, launched in 2022, has involved about 370 contributors. The release contains 18,733 petition lists and 350,092 petitioner entries after review removes duplicate scans, blank pages, and crossed-out or non-person entries. Figure [E.27](#) shows the transcription platform.

E.2 Geocoding

Most petitioners reported an address, and each sheet records the locality where signatures were collected. We georeference the 3,000 sheet-level localities, typically a city or parish, using contemporary and historical maps and the *Geografiskt-Statistiskt handlexikon öfver Sverige* (1880).

We then standardize individual addresses and geocode them to modern locations using the Google Maps API. For each sheet, we compare these locations with the sheet-level locality. When the median distance exceeds 10 km, approximately the 90th percentile, we use the sheet location for all signatures on that sheet. This rule limits the influence of failed automated geocoding.

E.3 Census Linkage

We link signatures to the 1910 census with probabilistic record linkage following [Abramitzky et al. \(2021\)](#). The linkage classifies women born in 1895 or earlier as signers, non-signers, or ambiguous matches, connecting political participation to childhood exposure to female teachers.

We first restrict comparisons to geographic blocks based on the geocoded address. Within each block, we score full-name similarity with Jaro–Winkler distance, giving equal weight to first and last names.³⁶ We estimate the procedure separately using census districts, municipalities, and counties as blocks.

Three features of the data make linkage difficult. First, the census predates the petition by four years, so names and addresses can change. Population registers from

³⁶For candidate records 1 and 2, let f_1 and f_2 denote first names and e_1 and e_2 surnames. Their score is the average of the two Jaro–Winkler similarities: $\frac{1}{2}\{jws(f_1, f_2) + jws(e_1, e_2)\}$.

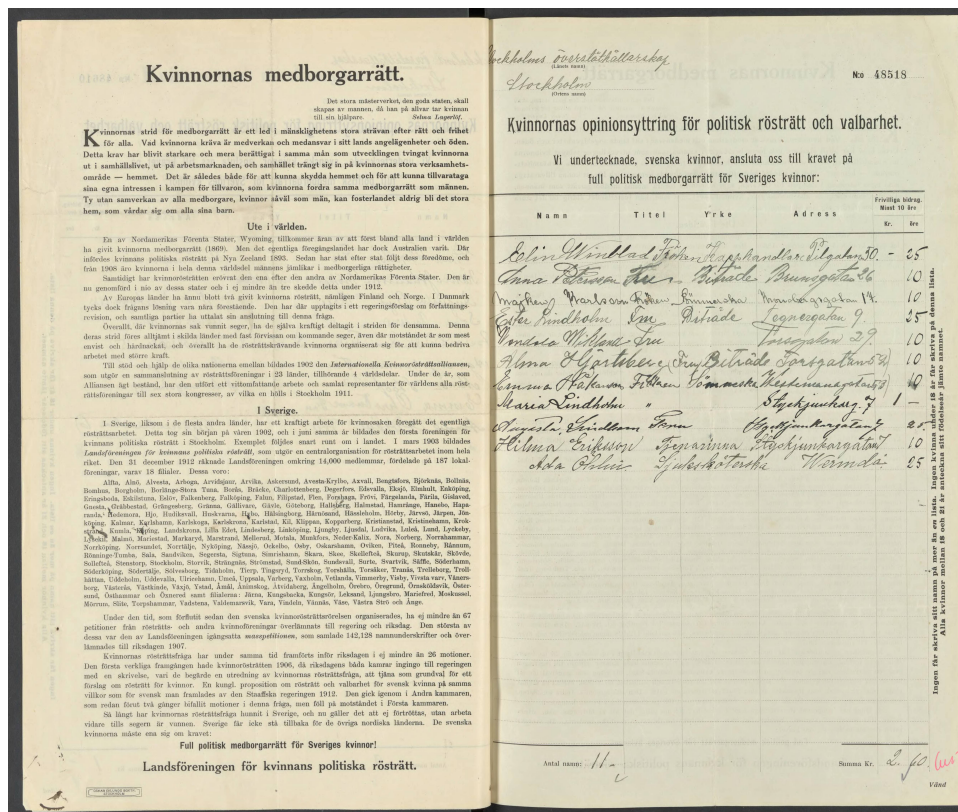


FIGURE E.26. EXAMPLE SIGNATURE SHEET FROM THE 1913–14 SUFFRAGE PETITION

Notes: Reproduction of a signature sheet from the 1913–14 women's suffrage petition.



FIGURE E.27. PETITION SIGNATURE SHEET IN THE OPEN TRANSCRIPTION PLATFORM

Notes: Screenshot of the crowdsourced *FromThePage* platform used to transcribe the 1913–14 suffrage petition. The sheet shown is the same as in Figure E.26.

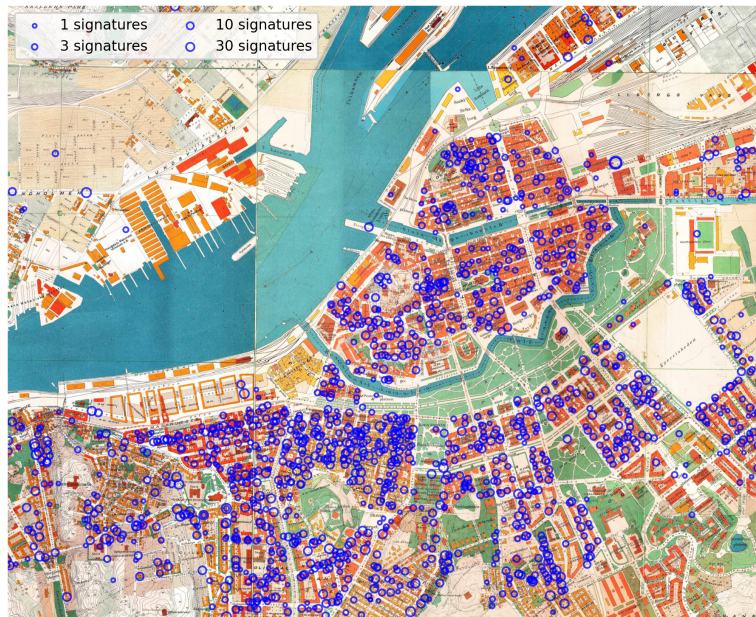


FIGURE E.28. EXAMPLE OF PETITION GEOCODING IN GOTHENBURG

Notes: Points mark petitioners in central Gothenburg; Appendix E describes the geocoding. The basemap reproduces *Jubileumskartan 1923* from the City Planning Office in Gothenburg.

1913 or 1914 would be preferable, but no comparable population-wide source exists. Second, errors in the transcribed or geocoded address can place a signature in the wrong block. Third, signatures often use a woman's preferred name, whereas the census records official names, including middle names and, for married women, both married and maiden surnames.

For example, the petition records the Gothenburg suffrage leader as *Frigga Carlberg*, while the census records *Anna Fredrika Carlberg Lundgren*; the highest-scoring name combination has a Jaro–Winkler score of 0.88. Common names create a separate problem: 229 women named *Anna Andersson* lived in Norrköpings Sankt Olai parish, 17 of whom appear in the geocoded petition. We retain ambiguous cases rather than force a unique match. A unique name is not required, however, when the available petition signatures can be assigned unambiguously to the corresponding census records, as for the two *Ada Andersson* records in Backa.

Without a hand-verified ground truth, the data cannot determine a single objectively optimal blocking rule or similarity cutoff. We therefore evaluate the linkage over a transparent range of choices and report results under alternative thresholds.

E.4 Evaluation of Linkage Quality

We vary the Jaro–Winkler cutoff from 0.75 to 0.99 in increments of 0.01. Larger geographic blocks reduce sensitivity to geocoding error but make names less distinctive,

increasing ambiguity. Figure E.29 reports, for each blocking rule and cutoff, the shares classified as matched, ambiguous, and unmatched; the horizontal line marks the observed petition participation rate.

The figure also reports the precision–coverage trade-off. Precision is the share of matched women with the same civil status in the census and petition; civil status is not used in the match.³⁷

$$\text{precision} = \frac{\text{same civil status}}{\text{same civil status} + \text{different civil status}}.$$

Marriage between 1910 and 1914 prevents precision from reaching one even under perfect linkage. The dotted line in Figure E.29 gives the precision expected under random matching. We summarize the trade-off with an F1-type score,

$$F_1 = 2 \cdot \frac{\text{precision} \cdot \text{coverage}}{\text{precision} + \text{coverage}},$$

where coverage is the share of petition signatures linked unambiguously to a census record.

The procedure outperforms random matching at all cutoffs. Municipal blocking provides the best precision–coverage trade-off; county blocks are too large, while census-district blocks have lower precision. Coverage never exceeds roughly 50 percent because many names are not unique within a geographic block. Lowering the cutoff consequently produces more ambiguous, rather than more unambiguous, matches.

E.5 Selecting a Baseline

Our baseline uses municipality blocking and a Jaro–Winkler cutoff of 0.9. Municipality blocks offer the best precision–coverage trade-off: census-district blocks are more sensitive to address error, while county blocks leave too many women with indistinguishable names. The 0.9 cutoff is conservative enough to limit false matches while retaining substantial coverage. We report results at 0.85 and 0.95 to show that the findings do not depend on this choice. Table E.12 summarizes the resulting match categories and their demographic and labor-market characteristics.

³⁷Civil status is reported for 84 percent of petitioners. Among these, 62 percent were or had been married.

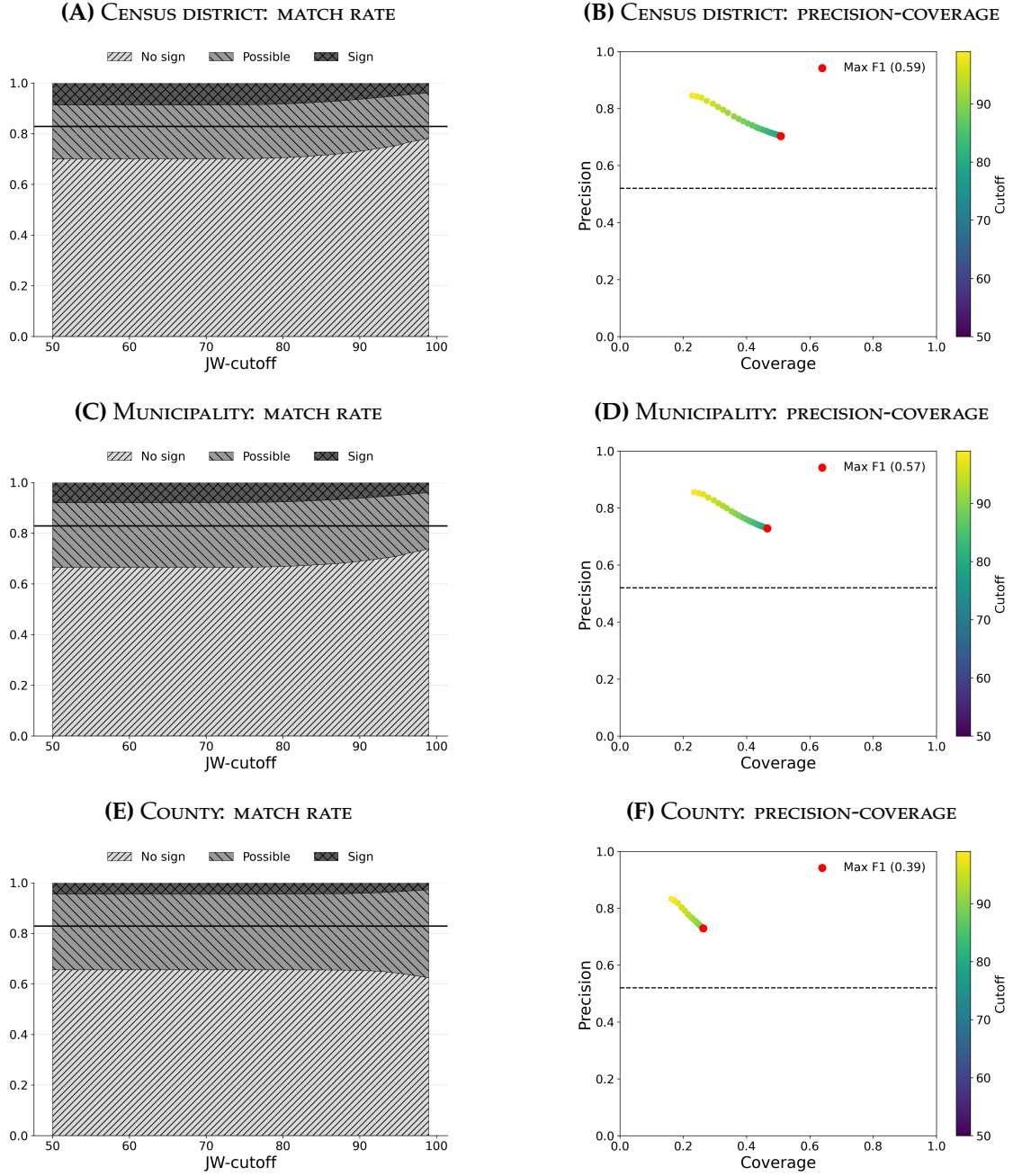


FIGURE E.29. EVALUATION OF PETITION–CENSUS LINKAGE QUALITY

Notes: Rows compare census-district, municipality, and county blocking. Left panels report match classifications across Jaro–Winkler cutoffs; right panels report the precision–coverage trade-off. Horizontal lines mark the observed petition participation rate.

TABLE E.12—SUMMARY STATISTICS BY PETITION-MATCHING STATUS

	Petition-matching status			Normalized difference		
	Signed	Non-signer	Ambiguous	Signed vs. non-signer	Non-signer vs. ambiguous	Signed vs. ambiguous
Childhood district employed a female teacher (%)	30.90 (46.21)	14.52 (35.23)	26.91 (44.35)	0.40	−0.31	0.09
Age	39.02 (14.85)	45.72 (19.23)	43.11 (16.55)	−0.39	0.15	−0.26
Urban residence (%)	43.40 (49.56)	15.39 (36.08)	49.87 (50.00)	0.65	−0.79	−0.13
Born in an urban parish (%)	27.16 (44.48)	10.35 (30.46)	23.20 (42.21)	0.44	−0.35	0.09
Children in household (%)	49.03 (49.99)	44.77 (49.73)	49.61 (50.00)	0.09	−0.10	−0.01
Married (%)	50.78 (49.99)	43.87 (49.62)	51.24 (49.98)	0.14	−0.15	−0.01
Unmarried (%)	42.86 (49.49)	43.31 (49.55)	39.52 (48.89)	−0.01	0.08	0.07
Widowed (%)	6.05 (23.84)	12.58 (33.16)	8.87 (28.43)	−0.23	0.12	−0.11
Husband-wife age difference	−2.91 (5.99)	−2.80 (6.44)	−2.40 (6.13)	−0.02	−0.06	−0.08
Non-domestic occupation (%)	20.89 (40.66)	14.23 (34.93)	20.10 (40.08)	0.18	−0.16	0.02
Domestic servant (%)	4.31 (20.30)	6.76 (25.10)	6.13 (23.99)	−0.11	0.03	−0.08
Teacher (%)	2.92 (16.84)	0.99 (9.90)	0.82 (9.01)	0.14	0.02	0.16
Professional (%)	4.18 (20.01)	1.58 (12.48)	1.58 (12.48)	0.16	−0.00	0.16
Administrative, clerical, or sales (%)	6.51 (24.67)	3.30 (17.85)	5.24 (22.28)	0.15	−0.10	0.05
Service or production worker (%)	14.36 (35.07)	16.06 (36.72)	19.35 (39.51)	−0.05	−0.09	−0.13
Patronymic surname (%)	38.35 (48.63)	51.21 (49.99)	62.24 (48.48)	−0.26	−0.22	−0.49
Elite surname (%)	3.49 (18.35)	2.13 (14.45)	1.16 (10.70)	0.08	0.08	0.16
Observations	120,844	1,346,342	482,796			

Notes: Sample comprises women born by 1895 in the 1910 census. Columns 1–3 distinguish matched signers, matched non-signers, and ambiguous matches. Cells report means with standard deviations in parentheses; binary variables are percentages. Age is measured in 1914. Normalized differences divide the difference in group means by the square root of the mean group variance.

F. INSTRUMENT SELECTION

Define the instrument as

$$Z_s(r) = 1\{\text{the sole seminary city within } r \text{ km of } s \text{ is female-only}\}. \quad (5)$$

At each radius, the estimation sample includes only districts with exactly one seminary city inside it; those near a male-only seminary form the zero group.

The radius r involves a trade-off: it must be small enough to support the exogeneity assumption, yet large enough to yield a strong first stage and limit weak-instrument bias. We select r following the weak-instrument test of [Andrews et al. \(2019\)](#), designed for settings with multiple instruments and heteroskedastic errors.³⁸

We estimate the effective F-statistic for the first stage of Equation 3 over a grid of r from 10 to 100 km in 1 km steps. Figure F.30 plots the results against the critical values for 5 percent and 10 percent maximal IV bias. A radius of 15 km is the preferred choice: it produces a strong first stage while restricting the sample to districts at a moderate distance. A radius of 36 km is a viable alternative, enlarging the sample while still passing the weak-instrument test.

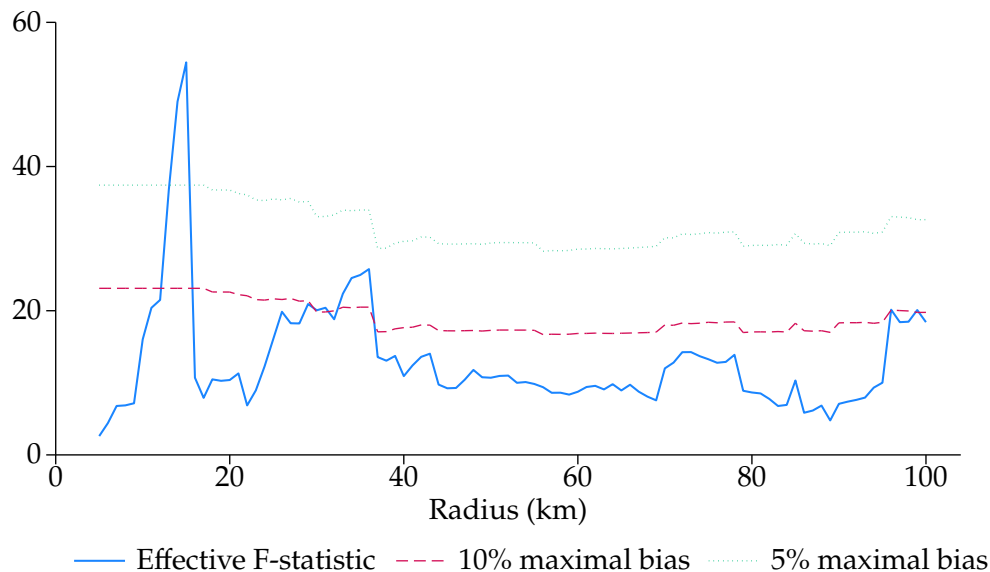


FIGURE F.30. INSTRUMENT RADIUS SELECTION

Notes: The solid line reports the effective first-stage F-statistic across candidate radii. Dashed and dotted lines give the critical values for 10 and 5 percent maximal IV bias, calculated with weakivtest ([Pflueger and Wang, 2013](#)).

³⁸Unlike the canonical test based on the Cragg–Donald F-statistic, this test uses the Effective F-statistic and the critical values of [Montiel Olea and Pflueger \(2013\)](#). The Effective F-statistic is compared to a critical value reflecting the tolerated maximal IV bias.

G. COEDUCATION AND GENDER SEGREGATION

[Hamberger \(2026\)](#) conducts the first systematic review of gender segregation in the nineteenth-century primary school system. School districts organized education as they chose. Separate girls' and boys' schools were an urban practice: between 1862 and 1891, just over half of administrative cities ran a segregated system ([Hamberger, 2026](#), Figure 1). Segregation rose with city size, and every major city divided its schools by gender.

We extend this analysis to teachers, asking whether female teachers in urban districts taught boys. We use the 1889 teacher register, which records each school's gender profile for a subset of urban districts, and supplement it with the historical literature for the largest cities.

Large Cities. Stockholm let parishes choose between coeducation and segregation until it created an integrated district in 1862 ([Bergman, 1897](#)). An 1876 regulation mandated coeducation in the first three grades but went unenforced. Female teachers worked mainly in girls' schools and in mixed classes of younger pupils.

Malmö segregated its schools, consistent with [Hamberger \(2026, Appendix 2\)](#).³⁹ Matching school names to the teacher register, we find almost no female teachers in boys' schools and 92 percent in girls' schools.

Gothenburg and Norrköping segregated throughout the century. We lack direct evidence on where their female teachers worked, but they almost certainly followed the common pattern.

Smaller Cities. Fifteen cities ran a single school and were therefore coeducational. Our 1889 data classify the remaining 73:

- Thirty-seven cities have separately named boys' and girls' schools; these cities have more schools overall.
- Women were 7 percent of teachers in boys' schools.
- Women were 96 percent of teachers in girls' schools.

By 1889, men and women split the teaching workforce roughly evenly in nearly every major city; Karlskrona, which hired few women, is the exception. This balance is what segregation produces when male teachers staff boys' schools and female teachers staff girls' schools.

³⁹Some school names identify pupil gender directly, such as *Västra folkskola* (the Western school), which from 1865 admitted only girls while boys attended *Östra folkskola*. Archival records confirm the split ([Malmö City Archives, 1844/1962](#)).

Gender-segregated schooling was thus the norm in Swedish cities, especially medium-sized and large ones. Female teachers in these districts rarely taught boys, and when they did, taught only the youngest. The cities for which we observe both teacher gender and school gender profile in 1889 confirm this.

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