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Assortative Matching at the Top of the Distribution: Evidence from the World's Most Exclusive Marriage Market[†]

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Using novel data on peerage marriages in Britain, I find that low search costs and marriage-market segregation can generate sorting. Peers courted in the London Season, a matching technology introducing aristocratic bachelors to debutantes. When Queen Victoria went into mourning for her husband, the Season was interrupted (1861–1863), raising search costs and reducing market segregation. I exploit exogenous variation in women's probability to marry during the interruption from their age in 1861. The interruption increased peer–commoner intermarriage by 40 percent and reduced sorting along landed wealth by 30 percent. Eventually, this reduced peers' political power and affected public policy in late nineteenth-century England. (*JEL* C78, D83, J12, J16, N33)

> It is a truth universally acknowledged, that a single man in possession of a good fortune, must be in want of a wife.
>
> —Jane Austen, *Pride and Prejudice*

In OECD countries, most people tend to marry those who have a similar education, income, or social status (Chen, Förster, and Llena-Nozal 2013). Besides preferences for others like ourselves, one important determinant of marital sorting is the matching technology: every relationship not only reflects whom we choose but also depends on whom we meet. For example, 60 percent of all married couples in the United States met in settings where entry is restricted to similar others: at college, at work, at a social club, etc. (Laumann et al. 1994). A robust prediction of marriage

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models is that search restricted to such settings strengthens marital sorting.[1] In turn, assortative matching can have important implications for inequality and income redistribution,[2] particularly at the top of the wealth distribution (Piketty and Saez 2006). Historically, marital sorting helped elites to consolidate their political power (Puga and Trefler 2014). Therefore, any matching technology that affects search costs (e.g., online dating or students' clubs at college) may well lead to greater sorting and, hence, to greater political and economic inequality.

Testing this prediction is challenging. One reason is that, in modern marriage markets, it is virtually impossible to isolate a particular matching technology from other settings where courtship takes place. Recent empirical work has used speed dating (Fisman et al. 2008) or dating websites (Hitsch, Hortaçsu, and Ariely 2010). The results are largely at odds with the theory—these matching technologies do not seem to strengthen sorting, even when they reduce search costs and affect the choice sets of potential mates. This discrepancy, however, may stem from the fact that dating is very different from marriage. In most cases, dating does not reflect the long-term partnership formation at the core of search and matching theory. An additional difficulty is that because many changes to the matching technology are recent, the long-term implications for political and economic inequality are yet to be assessed.

In this paper, I use a unique historical setting to isolate the effect of the matching technology on marital sorting and to evaluate some of its political-economy implications in the long run. In the nineteenth century, from Easter to August of each year, a string of social events was held in London to help the peerage's offspring[3] to meet and court—the "London Season." Courtship in noble circles was largely restricted to London; in most cases, the only place where a young aristocrat could speak with a girl was at a ball during the Season. Guests were carefully selected according to social status, and the high cost involved in participating even excluded peers if they were pressed for money. In economic terms, the Season reduced search costs for partners and restricted the choice set of potential mates. Crucially, the Season was interrupted by a major, unanticipated, exogenous shock: the deaths, in the same year, of Queen Victoria's mother and husband. As the queen went into mourning, royal balls in the Season were canceled for three consecutive years (1861–1863). During this period, preferences for spouses did not change, but nobles were exposed to larger search costs and lower market segmentation. I use this large shock to identify the effects of the Season on marital sorting and its long-term economic implications. Specifically, I combine archival and published sources to construct two novel datasets—one measuring attendance to the Season, another of all aristocratic marriages in nineteenth-century Britain. This allows me to evaluate the effects of the Season on marital sorting by title and landed wealth. To gauge the political-economy implications of marital sorting, I link my datasets to the biographies of peers elected to the House of Commons and to local data on state-education provision.

[1] Burdett and Coles (1997); Eeckhout (1999); Bloch and Ryder (2000); Shimer and Smith (2000); Adachi (2003); Atakan (2006); Jacquet and Tan (2007).

[2] Kremer (1997); Cancian and Reed (1998); Fernández and Rogerson (2001); Fernández, Guner, and Knowles (2005); Greenwood et al. (2014); Eika, Mogstad, and Zafar (2019).

[3] The peerage refers to the British aristocracy. More details are in Section I.

My first contribution is to estimate a strong, plausibly causal link between search frictions and marital sorting. I exploit the interruption of the Season (1861–1863) as a quasi-experiment that raised search costs and reduced market segmentation for some cohorts. Since the decision to marry during the interruption is potentially endogenous, I compare cohorts who had different risks to marry in 1861–1863 based on their age. Specifically, I consider a baseline sample of peers' daughters aged 15 to 35 in 1861. My treatment variable is a woman's *synthetic* probability to marry in 1861–1863, which is based on her age in 1861–1863 and on the probability to marry at each age in "normal times." For example, the synthetic probability for a woman aged 19–22 in 1861–1863 is equal to the percentage of women who married aged 19–22 in a benchmark cohort married before 1861.[4] My main estimates show that women with a high synthetic probability to marry in 1861–1863 were more likely to marry a commoner and less likely to marry a peer's heir. To evaluate sorting by landed wealth, I restrict the sample to matrimonies for which both spouses' family landholdings are recorded. Women with a high synthetic probability to marry in 1861–1863 sorted less by family landholdings and married husbands from poorer families. To quantify the magnitude of these effects, I estimate an IV model where I instrument a woman's decision to marry during the interruption with her synthetic probability to marry in 1861–1863. I find that women who (exogenously) married during the interruption were 40 percent more likely to marry a commoner, 30 percent less likely to marry a peer's heir, and married husbands 44 percentile ranks poorer in terms of family landholdings. Finally, I present nonparametric estimates: chi-squared tests of association reveal that higher-titled women married higher-titled husbands only when the Season was operative—sorting by title resembles random matching for cohorts exposed to the interruption. Similarly, Kolmogorov-Smirnov tests show that the interruption reduced sorting by family landholdings. Altogether, these results show that the matching technology embedded in the Season—by reducing search costs and segregating the marriage market—crucially determined sorting.

My second contribution is to show that marriage played an important role in consolidating the peerage as a political elite. To do so, I examine elections of Members of Parliament (MP) at the House of Commons for 27 general elections and 97 by-elections in the late nineteenth century. I show that a woman's marriage to a commoner reduced her blood relatives' probability to be elected MP in the following years. Specifically, I estimate an IV model where I instrument a woman's probability to marry a commoner with her synthetic probability to marry during the Season's interruption. I find that, *after* a woman's marriage to a commoner, her brothers were 50 percent less likely to be elected MP, and, together, they served 18 fewer years than the brothers of women who married in the peerage. The loss of political power was local: mostly constituencies near the family seat were affected. Not only brothers but also the family heads a decade after the interruption (in the 1870s) were affected. I also discuss historical evidence on the mechanisms behind these effects. After a woman's marriage to a commoner, her birth family had to mobilize considerable capital to sustain her. This limited their ability to control MP elections

[4] I use women born in 1815–1830 as benchmark cohort and show that results are robust to using alternative benchmark cohorts.

by distributing favors, rents, and jobs among the local electorate. Marrying a commoner also reduced a family's social prestige (Allen 2009) and a woman's ability to act as "power broker" on behalf of her blood relatives (Atkins 1990). Altogether, the evidence strongly suggests a negative relationship between within-landed-elite marriages and how contested MP elections were in late nineteenth-century England. In contrast, the Season's interruption increased a woman's probability to marry a commoner, which, in turn, reduced her family's political power. Finally, I show that this had important economic consequences: families who lost political power could not effectively oppose the introduction of state education in the 1870s—a policy otherwise subject to capture by local landowners (Stephens 1998). I use data from Goñi (2021b) on wealth taxes set by 943 local school boards in 1872–1878. IV estimates show that taxes were higher near the family seats in which a woman married a commoner (and the family lost political power) than near the family seats in which a woman married in the peerage (and the family retained political power).

The empirical setting I examine offers a number of advantages. First, Victoria's mourning generated as good as random assignment into the Season. Prince Albert's death and Victoria's long mourning were unexpected (Ellis 1977; Hobhouse 1983). Furthermore, there was high pressure to marry young: if a girl was not engaged two or three Seasons after "coming out" into society, she was written off as a failure (Davidoff 1973, 52). Hence, women at risk of marriage in 1861 could not simply postpone the search for a husband until the uncertain date when the Season would resume. Second, the short nature of the interruption allows me to disentangle search costs from preferences, as the latter unlikely changed in the short run. Third, no centralized market emerged during the interruption. Neither could the Season be replaced by arranging marriages, as these were not socially acceptable at the time (Davidoff 1973, 49). Meeting was required, and the evidence suggests that in 1861–1863, meetings took place in local marriage markets, where search costs were large. Fourth, this setting allows me to open the "black box" of the matching technology. In modern marriage markets, we can only guess who is on the market and who meets whom. By contrast, the participants in the Season were well defined, e.g., it was announced who was newly on the marriage market each year. Fifth, there is a wealth of data to evaluate many dimensions of marital sorting, peers' political power, and local public goods' provision. Sixth, in contrast to studies that use modern-day data, this historical case study allows me to identify long-term implications of marital sorting.

This paper can be seen as integrating two literatures. Starting with the seminal works of Gale and Shapley (1962) and Becker (1973), a large literature has studied the determinants of assortative matching. A classic prediction is that a matching technology that reduces search costs and limits people's choice set increases sorting.[5] Surprisingly, these well-accepted theoretical insights lack clear-cut empirical support. Hitsch, Hortaçsu, and Ariely (2010, 162) show that sorting patterns in a dating website "absent of search frictions" differ little from those of people searching off-line. Similarly, Lee (2016) suggests that lower search costs online and search

[5] See references listed in footnote 1.

filters do not affect sorting. Fisman et al. (2008) find few interracial matches in speed dating, even though this matching technology facilitates encounters between people of different ethnic groups. Contrary, Belot and Francesconi (2013) find that meeting opportunities dominate preferences in speed dating. These mixed results may be explained because dating does not always reflect the long-term partnership formation at the core of search theory. Several studies suggest that meeting opportunities matter for marriage. For example, Abramitzky, Delavande, and Vasconcelos (2011) shows that male scarcity in post-WWI France reduced marital sorting.[6] Here, I analyze a matching technology that, like online or speed dating, reduces search costs but where outcomes are marriages. Another paper that looks at marriages in a low–search cost environment is Banerjee et al. (2013). Their aim, however, is to estimate the strength of same-caste preferences in India. To the extent of my knowledge, my paper is the first to provide causal evidence that a matching technology that reduces search costs and restricts the choice set of mates generates greater sorting in marriages. Hence, my first contribution is to reconcile the empirical evidence on matching technologies with the predictions of search theory.[7]

The second literature that motivates this paper studies the persistence of elites and institutional capture. Several studies show that elites persist by opposing inclusive institutions (Sokoloff and Engerman 2000; Acemoglu 2008; Galor, Moav, and Vollrath 2009; Allen 2009). Others have argued that household decisions such as inheritance (Bertocchi 2006) or marriage (Puga and Trefler 2014; Cruz, Labonne, and Querubín, 2017; Marcassa, Pouyet, and Trégouët 2020) were crucial to consolidate elites. My contribution is to link these two strands of the literature. I show empirically that marital sorting reinforced peers' political power, which led to the distortion of inclusive institutions such as state education. This suggests that marriage was crucial for institutional capture in late nineteenth-century England. The case study of the peerage is interesting for understanding inequality and elite persistence.[8] The peerage was likely the most exclusive elite ever to exist. Compared to the continental aristocracy, peers were fewer, richer, and remained in power longer (Cannadine 1990). I present an institution—the London Season—seldom considered by economists[9] and show that it helped sustain the peerage as an unusually small, exclusive, and rich elite.

The article proceeds as follows. Sections I and II present the historical background and the data. Section III establishes a plausibly causal link between search frictions and marital sorting. Section IV investigates the implications of sorting for the peerage's political power and its impact on public policy. Section V concludes.

[6] Laumann et al. (1994); Nielsen and Svarer (2009); Kaufmann, Messner, and Solis (2013) also emphasize meeting opportunities. Differently, Bruze (2011) finds that actors sort by education even if they do not meet their partners at school.

[7] My paper also relates to the matching market design literature (Roth and Sotomayor 1990).

[8] For studies linking marital sorting and inequality in modern settings, see footnote 2.

[9] Doepke and Zilibotti (2008) use the Season to illustrate the upper classes' taste for leisure. More generally, Allen (2009) argues that the extravagant aristocratic lifestyle was a sunk investment that allowed the peerage to effectively rule England from circa 1550 to 1880.

I. Historical Background

This section describes the Season and who was considered a suitable match as well as some contractual consequences of marriages in nineteenth-century Britain.

A. *The London Season*

The Season arose in the seventeenth century, when peers (and their families) started to move to London every year from February to August to attend Parliament (Davidoff 1973). In the nineteenth century, the Season developed into "the largest marriage market in the world," providing a string of balls and social events where the right sort of people met (Aiello 2010).[10] The reason was that

> arranged marriages were no longer acceptable so that individual choice must be carefully regulated to ensure exclusion of undesirable partners. Under such a system it was vital that only potentially suitable people should mix. To meet these ends, balls and dances became the particular place for a girl to be introduced into Society. (Davidoff 1973, 49)

Figure 1 illustrates the Season's calendar and main events. It plots 4,000 movements into and out of London by Season participants, as reported in the *Morning Post* in 1841. At the beginning of the year, most were out of town. Later in January, Parliament convened, and members of the aristocracy from all over the country moved from their family seats to London.[11] On April 20, the Queen joined in, and the first debutante was presented at court. Court presentations were yearly public announcements of who was newly on the marriage market. Afterward, many events designed to introduce bachelors to debutantes took place. It was the most crowded time of the year in London: on May 15—the day of the royal ball at Buckingham—over 800 families were in London for the Season. Many ladies met their future husbands at royal parties, which were described as "mating" rituals (Inwood 1998). The Season was over by August 12, when peers moved back to their country seats for the shooting season. Lucy, daughter of the fourth Baron Lyttelton, followed this calendar in 1859. Her diary describes the trip to London from her Worcestershire family seat (May 18), the "very memorable day" of her court presentation (June 11), and the royal ball at Buckingham (June 29).

Who attended the Season? Since it coincided with Parliament meetings, the Season was attended by the families of members of the two Houses of Parliament. Specifically, by all hereditary peers who sat in the House of Lords (Lords), and by those elected to the House of Commons (MPs). Peerage families without political connections also participated in the Season (Sheppard 1977, 89–93) as well as members of the landed gentry.[12] By centralizing the marriage market in

[10] Similar arrangements in continental Europe, e.g., the Rallye in Paris, never eclipsed the Season. Continental nobilities were too large for such meeting to be possible and did not engage in annual migrations to the capital, as their parliaments did not meet as regularly as in Britain.

[11] Sheppard (1977, 90) notes the "peripatetic existence of the 'Fashionable World'," suggesting that coming from a remote family seat was not a major impediment to attend the Season.

[12] The gentry was a social class consisting on landowners. Socially, they were below the peerage.

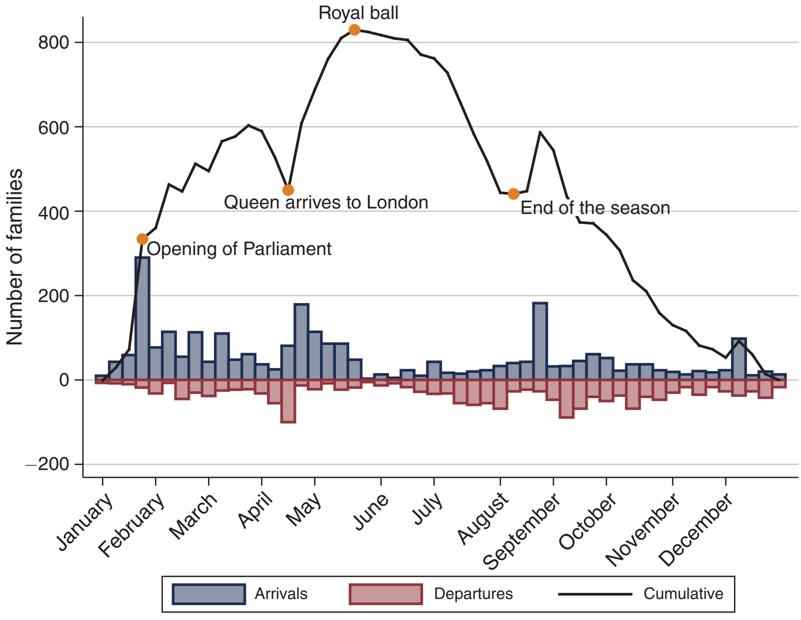

FIGURE 1. THE SEASON IN 1841

Notes: This figure plots 4,000 movements into and out of London of families participating in the Season of 1841, as reported in the *Morning Post*. The solid line shows arrivals minus departures. Since departures are underreported, Sheppard multiplies them by 1.6.

Source: Sheppard (1977)

London, the Season brought together aristocrats whose family seats were dispersed around the country. In addition, guests to private balls in the Season were carefully selected based on status (Davidoff 1973). Masked balls ceased because they were gate-crashed by commoners (Ellenberger 1990, 636). Similarly, court presentations were restricted to women sponsored by someone who had been presented before. In 1841–1850, for example, 88 percent of women presented were from the peerage or the gentry (Ellenberger 1990, table 1). The expenses incurred in the Season also restricted access. Few could afford to rent a house in Grosvenor Square or organize a ball for hundreds of guests (Sheppard 1971).[13]

The decline of the Season is linked to that of the peerage. Land values dropped in the late 1870s,[14] and peers' estates could no longer support their opulent lifestyle (Cannadine 1990). After that, many events in the Season became public, and commoners were presented at court, including American *nouveau riche* like Consuelo Vanderbilt (Ellenberger 1990). It was the death of the Season.

[13] For example, the Duke of Northumberland spent £20,000 in the 1840 Season (Sheppard 1971), the equivalent to $2,000,000 today (Nye 2019).

[14] This was due to the nationwide fall in grain prices after the opening up of the American prairies to cultivation and the development of steamships (Cannadine 1990).

B. *Choice of a Husband*

Who was considered a suitable match in the Season? Typically, marriages were not love matches but were based on eligibility. According to Davidoff (1973, 50), "[marriage was] not so much an alliance between the sexes as an important social definition; serious for a man but imperative for a girl. It was part of her duty to enlarge her sphere of influence through marriage."

Women and men aimed to preserve their social status through marriage. That is, peers' daughters aspired to marry peers' sons and vice versa. The peerage is divided into five titles: the most desired partners were dukes, marquesses, and earls, followed by viscounts and barons. Next came the gentry (baronets and knights), who were considered commoners. Titleless commoners were the least desirable. Marrying a partner of a very different status was frowned upon (Perkin 2002, 61). Yet marrying in the peerage was not trivial. In 1900, only 0.03 percent of people in Britain were an aristocrat; in Europe, it was 1 percent (Beckett 1986, 35–40). The Season facilitated encounters within this small group, leading to a high endogamy rate: in 1851–1875, 50 percent of peers' daughters married within this 0.03 percent group.

Family landholdings also played an important role in marriage decisions. Stone (1979, 87) argues that the aristocracy feared an "alliance with a family of lower estate or degree than one's own." The strict settlement—a contract regulating peers' inheritances—encouraged women to marry into families with large landholdings. In detail, settlements established a widowhood pension (jointure), a yearly payment (allowance), and capital sums (portions) for the heir's wife and children (Habakkuk 1994, 2). These payments were raised from the family landholdings and hence, were proportional to its size. That is, marrying an heir to large landholdings meant marrying a husband with a large income as well as a large jointure, allowance, and portion for the wife (and for her children). Marrying his brother was also desirable: even though he was not the heir, the strict settlement established allowances and portions for all the heir's siblings.[15] For example, portions of £10,000 to £30,000 were common for the heirs' brothers and sisters, who typically received similar allowances (Thompson 1963, 70; Perkin 2002, 67). In contrast, the income of an heir (or the allowance of a non-heir) to small landholdings may not have met the wife's needs. To compensate, her birth family typically provided her considerable portions, allowances, pin money, etc., diverting resources away from other purposes (see Section IVA). This compensation was agreed upon the marriage (Habakkuk 1994). The first Earl of Lytton gave a sense of the magnitude of the diverted resources: in 1864, he reckoned that a yearly income of £1,500 was the absolute minimum for a married couple (Perkin 2002, 68). This is the equivalent of $170,600 today (Nye 2019).

Although some marriages involved dowries, a systematic record does not exist. Hence, my analysis focuses on marriage outcomes rather than on the corresponding prices. The tested prediction—that search costs reduce sorting—is robust to allowing transfers between spouses (Shimer and Smith 2000; Atakan 2006). Omitting

[15] Similarly, men aimed to marry women entitled to a larger allowance and portion.

prices from the analysis is also justified because title and land reflect social prestige, which was not transferable through dowries or bride-prices (Davidoff 1973).

An important feature of the courting process was the pressure to marry young. Women had two to three Seasons to become engaged. If they failed, they were considered "on the shelf" (Davidoff 1973, 52). Figure 2 confirms that social norms circumscribed courting to young ages. In 1851–1875, a woman's "market value"—measured as her probability to marry an heir—declined after age 22. Importantly, women of higher status could not delay their marriage longer: around age 22, the market value of dukes', marquesses', and earls' daughters equalized to that of the lower-ranked barons' and viscounts' daughters. Consequently, courtship was an intense process. Although it is unknown how many proposals women received before accepting one, anecdotal evidence suggests that courting involved many interactions. For example, Lady Nevill attended "50 balls, 60 parties, 30 dinners and 25 breakfasts" in her first Season (Nevill 1920). In each ball, she was supposed to meet various suitors, as decorum rules discouraged dancing more than three times with the same suitor (Davidoff 1973, 49). Once a proposal was accepted, engagements lasted around six months. Marriage manuals explicitly discouraged long engagements. In general, marriages took place at the end of the Season.

II. Data

This section describes the datasets that I constructed for this paper. Online Appendix A provides detailed summary statistics.

A. *Attendance to the Season*

The British National Archives keep the original invitations issued to parties at Buckingham and St. James's Palace during the Season.[16] The records cover the period 1851–1875 and consist of circa 5,000 invitations per year. Based on these archival documents, I created a computerized dataset with all the invitations issued in 1851–1875 and the number of attendees at each party. Figure 3 plots the number of attendees over time. The chart reveals a huge disruption to the Season between 1861 and 1863 as a result of Queen Victoria's mourning for the deaths of her mother and her husband. Moreover, in 1851 attendance rates were unusually high as a result of the Great Exhibition.[17]

Royal parties were central to the Season (Davidoff 1973, 25). However, many events took place in private houses. How well does this data reflect general trends in the marriage market? Anecdotal evidence suggests that fewer balls were organized in private houses during Queen Victoria's mourning. While in normal years almost all Grosvenor Square residents hosted and attended private balls (Pullar 1978), in 1862 over a third of the residents did not participate in any private ball

[16] The National Archives (LC): LC 6/31-55, LC 6/127-156, and LC 6/157-164.

[17] The Great Exhibition was the first in a series of nineteenth-century World's Fairs.

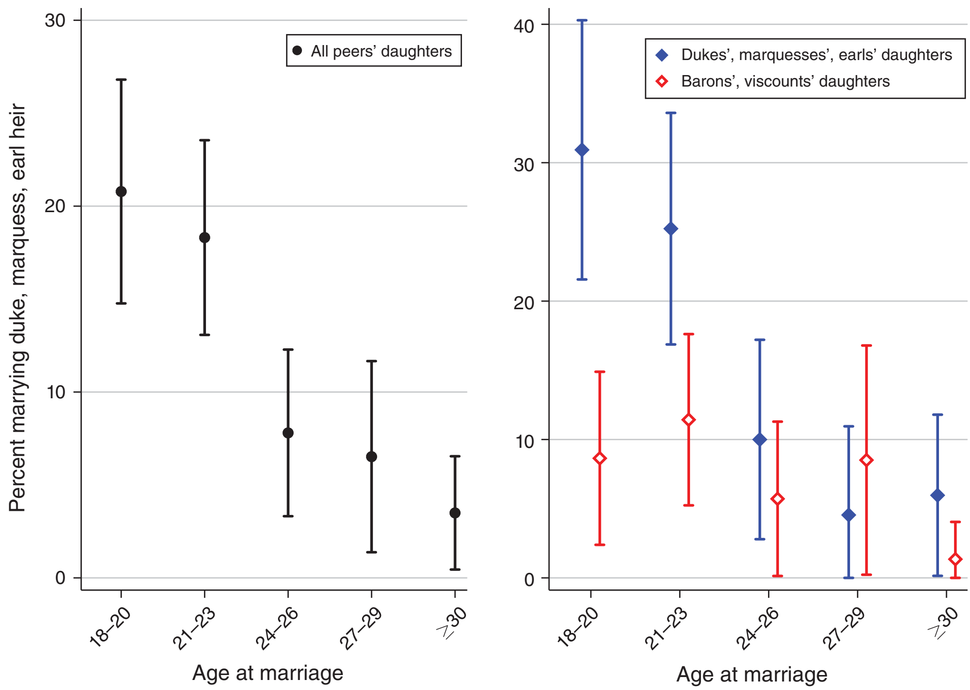

Figure 2. Pressure to Marry Young

Note: The sample is all 796 peers' daughters first marrying in 1851–1875.

(*Morning Post*, cited in Wilkins 2011, 10–11).[18] In addition, Figure 3 includes an alternative measure of the Season: the debutantes presented at court, i.e., the numbers announced to the marriage market (Ellenberger 1990). Trends are remarkably similar: when royal parties were well attended, more debutantes were presented. In 1862, both royal parties and court presentations were canceled.

B. *Peerage Marriages*

I assemble a novel dataset of peerage marriages that contains measures of sorting by title and landed wealth, and the geographical origin of peerage families. To do so, I use three data sources, two of which I have newly computerized. In detail, I complement Hollingsworth's Genealogical Data on the Peerage (2001) with geo-referenced data on landholdings (Bateman 1883) and family seats (Burke 1826).

[18] For example, in 1862 the *Morning Post* reported only two private balls for the week of July 14–20. In 1858, seven balls took place the corresponding week (July 11–17). One possibility is that hosting a ball while the Queen mourned would be seen as disrespectful.

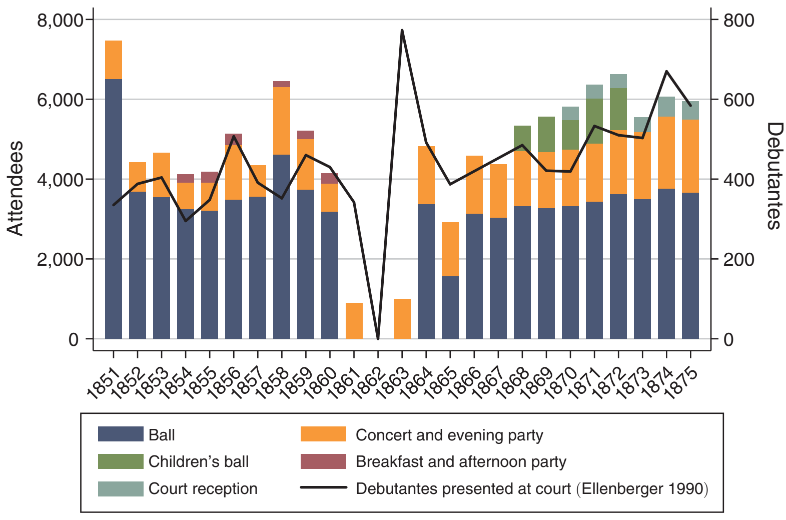

Figure 3. Attendees at Royal Parties, by Type of Event

Note: The data comprise circa 5,000 yearly invitations to royal parties during the Season (1851–1875).

Hollingsworth Dataset.—This genealogical dataset covers the marriages of all peers who died in 1603–1938 and of their offspring.[19] Hollingsworth (1964) constructed this dataset from peerage records, the chronicles of the family histories of the British aristocracy.[20] The data comprise around 26,000 individuals. My baseline sample is 644 women aged 15 to 35 in 1861 who ever married.[21] For each spouse, I know the title and the title of the highest-ranked parent. Titles are grouped in five categories: (1) duke, marquess, or earl; (2) baron or viscount; (3) baronet; (4) knight; and (5) commoner. I use this information to measure the rate of peer–commoner intermarriage, whether women married an heir, and sorting by title. The dataset also lists if a title is an English, Scottish, or Irish peerage; birth order (Gobbi and Goñi 2021); the number of children; and spouses' date of birth, marriage, and death.

Landholdings.—I computerized new data of peers' family landholdings based on Bateman (1883). The book lists the great landowners in Britain and Ireland by 1876. It includes all owners of at least 3,000 acres and 1,300 owners of 2,000 acres. I digitized the total acreage owned by families who appear both in Bateman's book and the Hollingsworth dataset.[22] Henceforth, a woman's family landholdings refers to the acreage owned by her birth family (idem for men). I focus on acreage because

[19] Note that the Hollingsworth (2001) dataset excludes the gentry. Hence, I focus on the peerage—the layer for which marriage had the highest stakes.

[20] The data were redigitized by the Cambridge Group for the History of Population and Social Structure in 2001. I am grateful to them for sharing the dataset.

[21] I exclude second marriages, women married to foreigners, and members of the royal family. When I evaluate celibacy rates, the sample is 765 because it includes women who never married.

[22] I constructed the dataset in three steps. First, I digitized all 596 men who appear both in Bateman and in Hollingsworth's dataset. Second, I coded 353 of their wives' birth families. This number is lower because some coded men did not marry or married landless commoners. From these two steps, I found both spouses' family

the information reported is more reliable than that on land rents (Bateman 1883). To measure sorting by landholdings, I consider the difference between spouses' family landholdings. Out of a potential sample of 644 women, I found the family landholdings for 324 women and their husbands. Eighty-one percent of the lost observations correspond to women marrying landless commoners. The remaining 19 percent are women whose families (or whose husbands' families) owned fewer than 2,000 acres and hence, were not listed in Bateman (1883).

Family Seats.—The data on family seats are from Burke (1826). This *Heraldic Dictionary* lists the seats of peerage families 34 years before the interruption of the Season. That is, the seats where women in my baseline sample lived before marrying. I geo-referenced 694 seats for 498 peerage families (some families owned more than one seat). These seats spread over Britain and Ireland (online Appendix Figure A6). I then link every individual in the Hollingsworth dataset to the seats owned by her/his birth family (henceforth, family seats). This gives me the family seat of 484 out of 644 women in the baseline sample (75 percent) and 260 out of 324 women with both spouses' family landholdings (80 percent).[23] I use the location of family seats to (i) assess a family's local political power, (ii) evaluate education provision around their seats, and (iii) control for distance to London. For these exercises, the sample is restricted to women with a recorded family seat.

C. *Political Power and Education Provision*

To investigate the implications of the Season for political power and education provision, I use two additional sources. Here I describe them briefly; more details are provided in the respective sections and in online appendixes A4 and A5.

First, I construct a new dataset on elections of Members of Parliament for the House of Commons. To do so, I use thepeerage.com, a website that provides biographies for all members of the peerage. The biographies state whether an individual was elected MP, the constituency, and the terms served. I hand-collected 674 biographies of the fathers and the brothers of women in my baseline sample. Of a potential sample of 279 women with a seat in England,[24] I found the biographies of all their fathers and brothers (except for 9 women who did not have any brothers). I also collect the biographies of those who where family heads in the 1870s, when state education was introduced in England. I use regular expressions to identify whether and when an individual was elected MP, his constituency, whether he was elected in the family seat's county, and how many years he served.[25] My dataset spans 27 general elections and 97 by-elections between 1776 and 1910 and covers 205 different constituencies (see Figure A7 in the online Appendix for details).[26]

landholdings for 227 couples in my baseline sample. Third, I searched the remaining spouses in the baseline sample, finding 97 additional couples.

[23] For the sample used to evaluate celibacy rates, I match 565 out of 765 women (74 percent).

[24] The sample is restricted to England because only there I have education provision data.

[25] thepeerage.com also lists other political posts, but, unlike for MPs, the date of appointment is not provided systematically. This information is crucial to examine whether a family's political power was reduced after a woman's marriage to a commoner (see Section IVA).

[26] In this sample, 20 percent of peers in the House of Lords had sons seating in the House of Commons.

The second source I use is the Reports of the Committee of Council on Education, digitized by Goñi (2021b). The reports list wealth taxes set by each school board in England (1872–1878). I use data on all 943 school boards in a 10-mile radius of 387 family seats in England, i.e., the family seats of women in the baseline sample. Since some families owned several seats, the number of seats is larger than the number of women. I measure local education provision with the average tax rate in this ten-mile radius. On average, wealth was taxed at 2.3 percent.

III. Empirical Analysis

In this section, I establish a causal link between search costs, market segmentation, and sorting. To do so, I exploit the interruption of the Season in 1861–1863 as a quasi-experiment that raised search costs and reduced market segmentation.

A. *Identification: The Interruption of the Season*

On March 16, 1861, Queen Victoria's mother died. Victoria was grief-stricken, and her husband, Prince Albert, took over most of her duties (Hobhouse 1983). On December 14, Albert died too. As a result of these two losses, Victoria avoided public appearances as much as she could. From 1861 to 1863, most royal parties during the Season were canceled (Figure 3). In 1862, the Queen suspended all court presentations, i.e., did not announce who was newly on the marriage market.

My identification strategy exploits the fact that the interruption of the Season (1861–1863) raised search costs and reduced marriage market segmentation. Concomitantly, nobles' preferences for spouses were stable. Since the decision to marry in 1861–1863 might be endogenous, I identify the effect of search costs and market segmentation by comparing female cohorts who had different risks of marrying during the interruption based on their age. Next, I define the sample and treatment and provide evidence supporting the identifying assumptions.[27]

Sample and Treatment.—My baseline sample is all peers' daughters aged 15 to 35 in 1861 who ever married. This includes women who could potentially have married during the interruption, although with different risks, determined by their age. My treatment variable is a woman's *synthetic* probability to marry during the interruption of the Season (1861–1863). This is based on her age in 1861–1863 and on the percentage of women marrying at each age in "normal times" (i.e., before the interruption). For example, consider a woman aged 20 at the start of the interruption, 21 in 1862, and 22 in 1863. Her synthetic probability to marry during the interruption will be the probability of marrying at ages 20, 21, and 22 in normal times. Formally, the synthetic probability (T_t) is

$$T_t = p(t) + p(t+1) + p(t+2), \tag{1}$$

[27] Admittedly, I estimate the effect of a transitory disruption of the Season; the estimated effects would be different if the participants thought that the Season would never resume again.

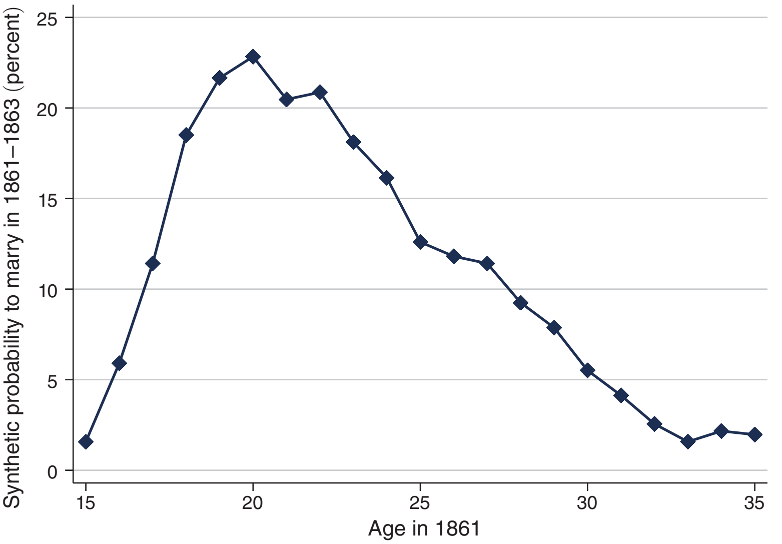

Figure 4. Synthetic Probability to Marry during the Season's Interruption (1861–1863)

Notes: For a woman aged $t \in \{15, 16, \ldots, 35\}$ in 1861, the synthetic probability to marry during the three-year interruption of the Season is $T_t = p(t) + p(t+1) + p(t+3)$, where $p(t)$ is the probability to marry at age t in "normal times." All $p(t)$ are computed from a benchmark cohort of daughters born in 1815–1830, excluding those marrying before 1861 and dying before age 30.

where t, $t+1$, and $t+2$ index a woman's age in 1861, 1862, and 1863, respectively, and $p(t)$ is the percentage of women married at age t in normal times. All $p(t)$ are computed from a benchmark cohort that was marriageable before the interruption but is close to the baseline sample such that they are comparable. Specifically, I use peers' daughters born in 1815–1830,[28] excluding those who married after 1861 or died before age 30.[29] In Section IIIE, I show that the results are robust to using alternative benchmark cohorts. Finally, note that T_t captures the probability *at birth*—or at the start of the courting process—to marry during the interruption. Hence, it is independent of an individual's (endogenous) marriage decisions.[30]

Because social norms circumscribed courting to young ages, small age gaps led to large differences in treatment. Figure 4 illustrates this. The cohorts most exposed to the interruption were women aged 19 to 22 in 1861. Their synthetic probability is above 20 percent. In other words, based on marriage behavior in normal times, one in five women in these cohorts was expected to marry during the interruption. Women aged 18 or 23 in 1861 have lower but considerable synthetic probabilities. The synthetic probability rapidly declines after age 24. Nine out of 10 women aged 25 or above were not expected to marry during the interruption, likely because they

[28] I choose this 15-year cohort as, on average, they married 15 years before the base sample.

[29] I exclude those who died before 30 such that $p(t)$ reflects the marriage probabilities of those who participated in the marriage market.

[30] In my setting, T is preferable to the hazard rate, which measures marriage probabilities in 1861–1863 *conditional* on remaining single—a potentially endogenous decision (online Appendix B5).

were already married. Similarly, very young cohorts (aged 17 or less in 1861) were expected to marry after the Season resumed.

Identifying Assumptions.—The identifying assumptions are that cohorts with different treatment levels are otherwise identical, that social norms circumscribed courting to young ages, that no centralized market replaced the Season, and that Victoria's mourning was the only shock to the marriage market in 1861–1863.

The first assumption would be violated if women could select their treatment level, i.e., their synthetic probability to marry in 1861–1863. Note, however, that this variable is independent of a woman's endogenous marriage decisions. It depends only on her age, and hence, selection is not an issue—nobody can choose her age.[31]

As for the second assumption, social norms circumscribed courting to young ages (Section I). This did not change during the interruption. First, the synthetic probability to marry in 1861–1863 (which is based on previous cohorts) predicts well the actual probability to marry in 1861–1863 (panel B of Table 3). Second, women did not postpone the search for a husband: the average age at marriage was the same during the three-year interruption, three years before, and three years after (Table 1).[32] Third, if some women had postponed marriage (or had anticipated the interruption), the pool of married women would be altered. Table 1 shows that this was not the case. The share of dukes', marquesses', and earls' daughters marrying during the interruption is identical to that in the three years before or the three years after. Women came from a family in a similar percentile of the landholdings' distribution. The share of families in the English peerage, life expectancy, and birth order also do not vary substantially. Fourth, it is unlikely that women (or men) anticipated marriages or waited for the Season to resume because the timing and duration of the interruption were unpredictable. Nobody expected Prince Albert to die in 1861: he was only 42 and took on government duties until 1 month before his death.[33] Even doctors failed to diagnose him with cancer, the cause of his death (Hobhouse 1983, 150). The peerage was also surprised by the length of Victoria's mourning. They complained that "the Queen came less and less to London, and the palace was more and more deserted" (Ellis 1977, 361).

The third identifying assumption is that no centralized market replaced the Season in 1861–1863. There is no mention of such a market in historical records. As discussed in Section IIA, private balls did not replace the canceled court presentations and royal balls. The evidence also suggests that, although Parliament met during the Season's interruption, the families of Lords and MPs did not move to London with them. That is, they did not engage in an alternative, reduced version of the London Season. For example, in 1863, the family of the sixth Earl Bathurst did not move to London with him, even though he was an MP and his sister, aged 26, was single (Wilkins 2011, 10). More generally, the 1841 and 1861 censuses were conducted in

[31] Given that the peerage was small (Beckett 1986), that the Season centralized information, and that peerage records published birth dates, it is unlikely that women lied about their age.

[32] Similarly, online Appendix B6 shows that women who, based on their age, were at risk of marriage during the interruption did not marry at an older age than women not exposed to the interruption.

[33] For example, on November 8, 1861, Union forces intercepted the British RMS *Trent*. Albert intervened to soften the diplomatic response, lowering the threat of war (Hobhouse 1983, 154).

Table 1—Marriage Market before, during, and after the Season's Interruption

	Interrup. 1861–1863 (1)	Before 1858–1860 (2)	Diff. (1)−(2) (3)	After 1864–1866 (4)	Diff. (1)−(4) (5)
Panel A. Wife's characteristics					
Age at first marriage	24.7 (0.6)	24.8 (0.7)	−0.1 (0.9)	24.5 (0.7)	0.2 (0.9)
Life expectancy	69.0 (1.9)	64.5 (2.1)	4.5 (2.8)	69.5 (1.9)	−0.5 (2.7)
Birth order (excluding heirs)	3.7 (0.3)	3.9 (0.3)	−0.2 (0.4)	4.0 (0.3)	−0.3 (0.4)
Duke/Earl/Marquess daughter	0.5 (0.1)	0.5 (0.0)	0.0 (0.1)	0.5 (0.1)	0.0 (0.1)
Peerage of England	0.6 (0.0)	0.5 (0.0)	0.1 (0.1)	0.6 (0.1)	0.0 (0.1)
Family acres (percentile)	47.4 (3.2)	52.6 (3.1)	−5.2 (4.5)	50.6 (3.2)	−3.3 (4.6)
Panel B. Cohort characteristics					
Female cohort size (18–24)	261 (3.1)	261 (2.0)	0 (3.7)	262 (3.4)	−1.7 (4.6)

Notes: Panel A is for all 286 peers' daughters first marrying in 1858–1866. "Duke/Earl/Marquess" and "Peerage of England" are proportions. Panel B shows year-averages for the number of peers' daughters aged 18–24. Standard errors are in parentheses.

months in which Parliament met regularly and report whether residences in London were occupied by a family or by one individual. By focusing on the West End, where many MPs and Lords rented a house while Parliament met, one can assess if their families moved with them to London. In 1841, most did: 87 percent of West End's houses were occupied by a family. In contrast, in 1861, only a third were occupied by a family (Wilkins 2011, 7).

In addition, the Season could not be simply replaced by arranged marriages, as these were not acceptable by the nineteenth century (Davidoff 1973, 49). Meeting was required, and the evidence suggests that meetings took place in local marriage markets: Figure 5 shows that, before and after the interruption, spouses came from family seats separated by 160 miles. In contrast, in 1862 couples came from 60 miles closer. Compared to the Season, local markets were shallow and search costs large—meeting a sizable pool of suitors would involve visits to several seats.

The final assumption is that the Season's interruption is the only shock to the marriage market. Although Albert's death coincides with the outbreak of the American Civil War, the marriage market was not affected. The American economy and the British textile sector suffered, but peers were not big investors in either (Rubinstein 1977, 115; Ventura and Voth 2015, 11). Furthermore, the textile crisis would make industrialists (commoners) less attractive suitors—which goes against my results. In addition, the peerage was not subject to any demographic shock in 1861–1863: the size of the cohort was not unusual (Table 1).

B. *Main Estimates*

To estimate the effect of the Season's interruption (1861–1863) on marital sorting, I use the following econometric specification:

$$\Pr\left(y_{i,t} = 1 \mid \mathbf{X}_{i,t}\right) = \Phi\left(\alpha + \beta T_t + \mathbf{X}_{i,t}'\delta\right), \tag{2}$$

where i indexes women; $t \in \{15, 16, \ldots, 35\}$ their age in 1861; and the treatment, T_t, is the synthetic probability to marry during the interruption (1861–1863). As

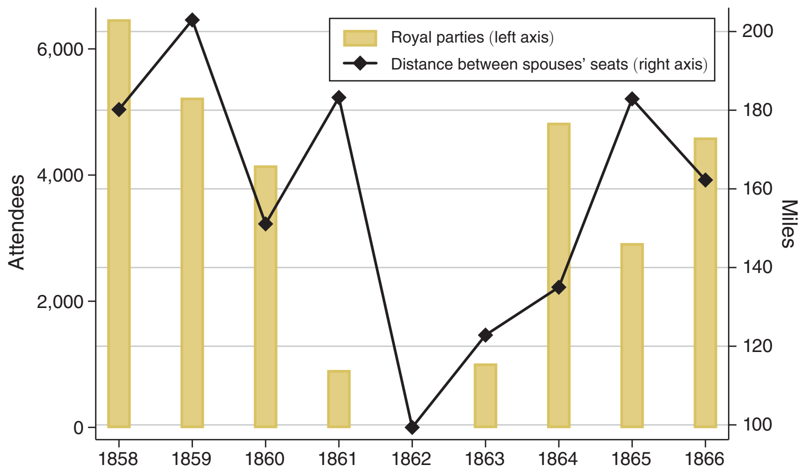

Figure 5. The Interruption of the Season and Distance between Spouses' Seats

Note: The sample is 68 marriages where *both* spouses' family seats are in Burke (1826).

described above, T_t is based on the percentage of women marrying at a given age in normal times. $y_{i,t}$ is a discrete outcome (e.g., married a commoner), and Φ is the CDF of the standard normal distribution. For continuous outcomes, I estimate

$$Y_{i,t} = A + BT_t + \mathbf{X}'_{i,t}\Delta + \epsilon_{i,t}, \tag{3}$$

where $Y_{i,t}$ is the difference between spouses' family landholdings. The coefficients of interest, β and B, capture the effect of the interruption of the Season. The vector **X** includes alternative predictors of marriage outcomes: family title, women's birth order, peerage of origin, and distance from family seat to London. Given that the treatment variable varies across birth cohorts, I cluster standard errors by birth year. I also report p-values from the bootstrap-t procedure (Cameron, Gelbach, and Miller 2008) to account for the small number of clusters ($G = 21$).[34]

Table 2 reports estimates of equations (2) and (3) for the baseline sample, i.e., peers' daughters aged 15–35 in 1861 who ever married.[35] The interruption of the Season reduced marital sorting. Specifically, women who—based on marriage behavior in "normal times"—were at risk of marriage in 1861–1863 were more likely to marry a commoner (column 1). This is consistent with the hypothesis that the Season was crucial to prevent peer–commoner intermarriage. The interruption also reduced the probability to marry an heir (column 2). Given that in Britain only heirs inherited titles, this was an important margin of marriage quality. To get a sense of the magnitudes, consider two cohorts separated by a small age gap: women aged 22 and 25 in 1861. In the absence of the marriage market disruption, we would expect them to end up marrying similar husbands. However, the synthetic

[34] The preferred cluster resampling scheme in Cameron, Gelbach, and Miller (2008), wild bootstrap, is only suited for linear regressions. For my probit estimates in equation (2), I use the pairs resampling.

[35] I exclude women marrying foreigners and members of the royal family.

Table 2—The Season's Interruption and Marriage Outcomes, Probit, and OLS Estimation

			Spouses' landholdings (rank percentile)			
	Married a commoner (1)	Married an heir (2)	Difference (absolute value) (3)	Difference (husband−wife) (4)	Married down (5)	Never married (6)
Panel A. Baseline						
Treatment[a]	0.005	−0.004	0.524	−0.516	0.009	0.002
	(0.002)	(0.002)	(0.196)	(0.225)	(0.003)	(0.002)
	[0.043]	[0.033]	[0.029]	[0.043]	[0.010]	[0.227]
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	644	644	324	324	324	765
Percent correct	66	74	—	—	64	77
Mean of dep var.	0.65	0.26	29	−3	0.53	0.23
Panel B. Controlling for distance to London						
Treatment[a]	0.006	−0.005	0.512	−0.537	0.009	0.002
	(0.002)	(0.002)	(0.213)	(0.221)	(0.003)	(0.002)
	[0.039]	[0.021]	[0.037]	[0.059]	[0.044]	[0.282]
Distance	0.0002	−0.0001	0.028	−0.039	0.0005	−0.0003
	(0.0002)	(0.0002)	(0.015)	(0.015)	(0.0002)	(0.0001)
	[0.502]	[0.834]	[0.088]	[0.012]	[0.074]	[0.030]
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	484	484	260	260	260	565
Percent correct	62	73	—	—	65	79
Mean of dep var.	0.62	0.27	30	−5	0.55	0.22
Model	Probit	Probit	OLS	OLS	Probit	Probit

Notes: This table reports estimates of equations (2) and (3). The baseline sample is all peers' daughters aged 15–35 in 1861 who ever married, excluding second marriages, women married to foreigners, and royals. Columns 3 to 5 evaluate sorting by landholdings and hence, mechanically exclude women for which Bateman (1883) does not list both spouses' family landholdings. Column 6 considers 765 peers' daughters aged 15–35 in 1861, including those who never married and excluding those who died before age 35. Controls are indicators for dukes'/marquesses'/earls' daughters and for English peerages, and birth order excluding heirs. Panel B also includes the distance between the family seat and London. Hence, it restricts the sample to women with a recorded family seat. Standard errors clustered by birth year in parentheses and p-values from the bootstrap-t procedure (Cameron, Gelbach, and Miller 2008) are in squared brackets.

[a]Synthetic probability (percent) to marry during interruption, based on marriage probabilities in normal years.

probability to marry in 1861–1863 was one standard deviation larger for women aged 22 in 1861. As a result, they were 5 percent more likely to marry a commoner and 10 percent less likely to marry an heir than women aged 25 in 1861.

The interruption of the Season also reduced sorting by landed wealth. Columns 3 to 5 restrict the sample to marriages for which Bateman (1883) lists both spouses' family landholdings. This selected sample allows me to test whether the interruption also affected sorting within the landed elite, that is, at the very top of the distribution. Compared to the baseline sample, some covariates are different, e.g., there are more dukes', marquesses', and earls' daughters. That said, the synthetic probability and the actual proportion of women married in 1861–1863 are similar across samples (online Appendix A6). In other words, this sample and the baseline sample were similarly exposed to the interruption.

In column 3, I evaluate the difference between spouses' family landholdings. Specifically, the dependent variable is the difference between spouses' percentile rank in acres, in absolute value. A value of zero indicates that both spouses' families are in the same percentile of the distribution; larger values indicate less sorting. I find that increasing the risk to marry in 1861–1863 by one standard deviation (7.3 pp) increases the difference in spouses' family landholdings by 4 percentile ranks. Given the sample averages, this corresponds to a 31 percent reduction in sorting.[36] This reduction in sorting should take the form of women marrying down. Relative to men, women were pressured to marry younger and hence, were more adversely affected by the three-year interruption of the Season. Columns 4 and 5 confirm this hypothesis: increasing a woman's synthetic probability to marry in 1861–1863 by one standard deviation is associated to marrying a husband 4 percentile ranks poorer and increases the probability to marry down by 12 percent. This finding is not specific to Victorian Britain. Low (2017) shows that women's pressure to marry young (due to a depreciation of their reproductive capital) may carry economic losses in modern marriage markets.

In column 6, I examine marital rates. I consider women aged 15–35 in 1861, whether they married or not. To avoid counting women who died at an early age as celibate, I exclude those dying before age 35. Results show that women at risk of marriage during the interruption were more likely to never marry, although the effect is not statistically different from zero.

Finally, panel B reports estimates controlling for the distance between a woman's family seat and London. Hence, the sample is restricted to women with family seats recorded in Burke (1826). This covariate is important because attending the Season may have been more costly for women living further away. Although families typically rented a house in London for the entire Season, some may have stayed in their country seats and traveled there for specific events. This covariate is significantly associated with sorting by family landholdings: women living further from London sorted less by landholdings (column 3) and married down more (columns 4 and 5). That said, the main estimates are robust. After controlling for the distance to London, I find that women at risk of marriage during the interruption were more likely to marry a commoner, less likely to marry an heir, sorted less by landholdings, and married poorer husbands within the landed elite. The magnitude of the effects is comparable to those in panel A.

Next, I perform placebo tests. I consider 40 cohorts who were on the marriage market $x \in \{10, 11, \ldots, 50\}$ years before the interruption of the Season (1861–1863). Specifically, each placebo sample and treatment is defined analogously to the baseline case but x years before.[37] Then I estimate the effect of a placebo interruption between $1861 - x$ and $1863 - x$ on the probability of marrying a commoner. Figure 6 presents the results. The placebo estimates are close to zero and, in most cases, significantly different from the baseline estimate. Only 1 out of the 40

[36] Under strict male primogeniture, the first-born son would inherit all the family's estates and its associated incomes. In this case, marrying a husband from a family with few landholdings would only matter if he was the heir. However, the peerage's inheritance system granted younger brothers (and sisters) portions and yearly allowances proportional to the family estates' size (see Section IB). Hence, marrying a non-heir from a poorer family implied an economic loss.

[37] Each placebo sample are women aged 15–35 in $1861 - x$ who ever married (excluding those in the baseline sample). The placebo treatment is the synthetic probability to marry from $1861 - x$ to $1863 - x$, based on the probability to marry at each age of women born from $1815 - x$ to $1830 - x$.

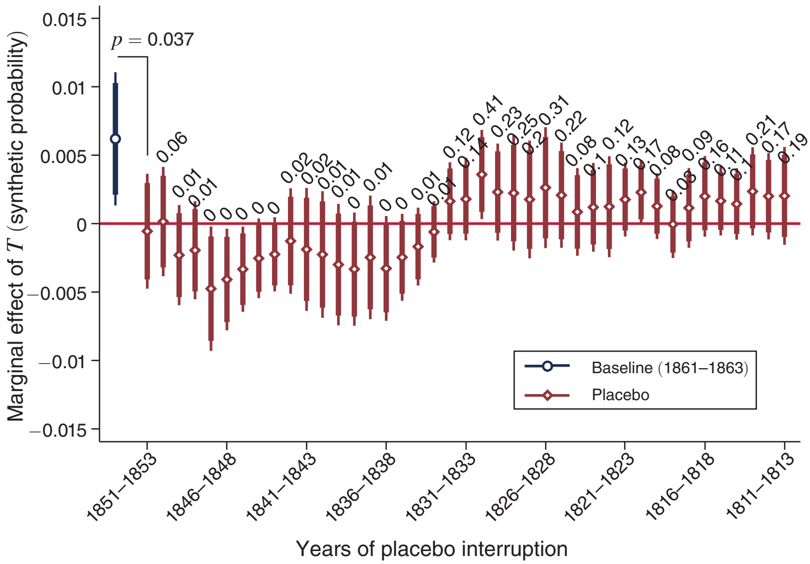

FIGURE 6. PLACEBO TESTS. DEPENDENT VARIABLE: MARRIED A COMMONER

Notes: This figure presents marginal effects and 95 percent confidence intervals from equation (2). Each placebo test considers a sample and a treatment (T) defined analogously to the baseline case but $x \in \{10, 11, \ldots 50\}$ years before. The placebo sample are women aged 15–35 in $1861 - x$ who ever married (excluding those in the baseline sample); $T_t^{placebo}$ is the synthetic probability to marry during a placebo interruption between $1861 - x$ and $1863 - x$, based on the percent of women married at each age in a previous cohort, i.e., women born between $1815 - x$ and $1830 - x$. All estimates include the full set of controls and the distance between family seat and London. On top of each confidence interval, I report p-values for a test of equality of coefficients (baseline versus placebo).

placebo tests reports a positive, marginally significant estimate, and its magnitude is only half of the baseline estimate (see the placebo interruption in 1830–1832). In other words, marriage outcomes were distorted only when the Season was actually interrupted. This suggests that my baseline specification captures the effect of the interruption and not any confounding factor correlated with age.

So far, I assumed that cohorts with a similar synthetic probability to marry in 1861–1863 (e.g., women aged 19 and 22 in 1861) respond similarly to the Season's interruption. Next, I estimate a flexible specification where I include fixed effects for a woman's age in 1861 instead of the treatment, T_t. This allows each age cohort to respond differently to the interruption of the Season. Formally, I estimate

$$\Pr\left(y_{i,t} = 1 \,|\, \mathbf{X}_{i,t}\right) = \Phi\left(\mu_t + \mathbf{X}_{i,t}'\delta\right), \tag{4}$$

where μ_t are fixed effects for a woman's age in 1861; $t \in \{15, 16, \ldots, 35\}$; and $y_{i,t}$ indicates marrying a commoner.

Figure 7 presents the results graphically. The reference cohort is women aged 16 in 1861. They were not at risk of marriage during the Season's interruption—their synthetic probability to marry in 1861–1863 is only around 5 percent. Likely, this

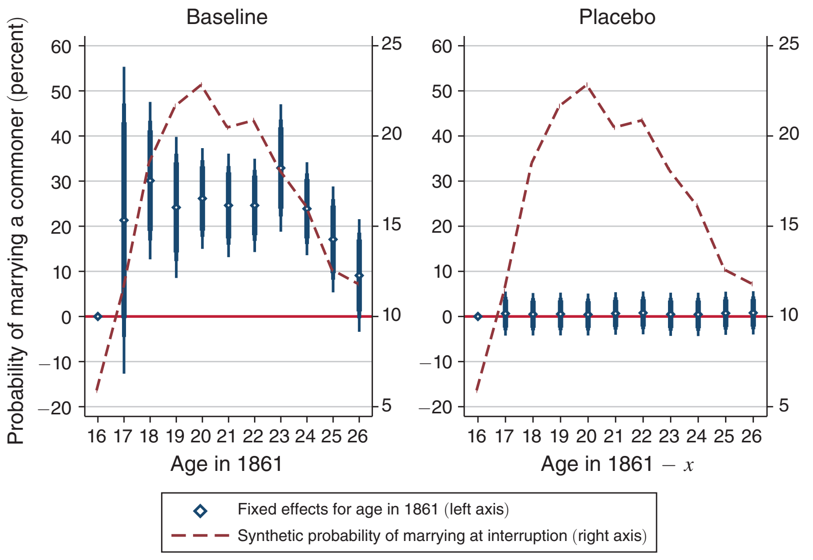

Figure 7. Peer–Commoner Intermarriage, Probit Estimation with Age Dummies

Notes: The left panel shows marginal effects and 95 percent confidence intervals for a set of dummies indicating a woman's age in 1861, μ_t, in equation (4). The baseline sample are women aged 15 to 35 in 1861. The right panel plots the corresponding estimates for a placebo test: I pool 40 cohorts of women aged 15 to 35 in 1861 $- x$, where $x \in \{10, 11, \ldots 50\}$, excluding women in both samples. All samples include women who ever married and exclude second marriages, women married to foreigners, and royals. The dashed line shows the synthetic probability to marry during the interruption, T.

young cohort were presented at court (i.e., announced to the marriage market) after the Season resumed. Relative to them, older women have a 25 to 30 percent higher probability to marry a commoner. The effect declines for those aged 24 and older and is only marginally significant for women aged 26 in 1861. The latter, who likely married before the interruption, also have a low synthetic probability (around 10 percent). Overall, the figure suggests that peer–commoner intermarriage was more prevalent for cohorts with a high synthetic probability to marry during the interruption. Finally, the right panel shows estimates from equation (4) on the (pooled) placebo samples described above, i.e., women on the marriage market $x \in \{10, \ldots, 50\}$ years before the interruption. For them, there are no visible cohort effects: the coefficients are tightly estimated around zero for all age groups. Again, this suggests that a woman's birth cohort only affected her probability to marry a commoner around the interruption of the Season.

In sum, this section shows that a matching technology with low search costs and market segmentation generates sorting. The Season announced who was on the market, created multiple settings for the opposite sexes to meet, and segregated the rich from the poor. Women at risk of marriage when this matching technology was operative sorted more than women at risk of marriage when it was interrupted.

C. *IV Estimates*

So far, my estimation strategy resembles a reduced-form IV. The synthetic probability to marry in 1861–1863 captures exogenous variation in the actual probability to marry in 1861–1863 and is regressed directly against marriage outcomes. Here, I estimate the full-IV model. This is interesting in its own right, especially to quantify the aggregate effect of the Season's interruption. Formally, I treat the decision to marry during the three-year interruption, M, as endogenous and instrument it with T, the synthetic probability to marry in 1861–1863. In the second stage, I regress the (instrumented) M on the marriage outcomes described above.

The identifying assumptions are that the instrument is relevant and that the exclusion restriction is satisfied. First-stage estimates support the first assumption. As for the exclusion restriction, note that the instrument, i.e., the synthetic probability to marry in 1861–1863, is based on marriage behavior in a previous cohort. Hence, it is independent of women's (endogenous) marriage decisions. Moreover, Section IIIA shows that the instrument could not affect marriage outcomes through channels other than the Season's interruption.

Panel B of Table 3 presents first-stage results. As before, I use my baseline sample: peers' daughters aged 15 to 35 in 1861 who ever married. Increasing the synthetic probability to marry in 1861–1863 by 1 percentage point is associated with an increase in the actual probability to marry in 1861–1863 by 1 percentage point. This shows that social norms and the pressure to marry young did not change during the interruption: the probability to marry at a given age in a previous cohort (the instrument) predicts well the timing of marriages around the interruption. The F-stat is large for the full sample (columns 1 and 3) but falls when the sample is restricted to 484 women with recorded family seats (even columns) and to 324 marriages with data on spouses' family landholdings (columns 5 to 10). To address this, I report p-values based on Moreira (2003) conditional likelihood ratio (CLR).

Panel A reports second-stage results. The three-year interruption of the Season had a large aggregate impact: it increased peer–commoner intermarriage dramatically and reduced sorting within the landed elite. Women who (exogenously) married during the interruption of the Season were 39–54 pp more likely to marry a commoner and 33–45 pp less likely to marry an heir than women marrying before and after the interruption (columns 1 to 4). Comparing estimates to sample means suggests that the rate of peer–commoner intermarriage would have been 60–87 percent larger in the absence of the Season. In addition, marrying during the interruption increased the difference in spouses' landholdings by 45–58 percentile ranks (columns 5 and 6). Women married husbands 44–61 percentile ranks poorer (columns 7 and 8). This corresponds to marrying a husband on the twentieth instead of the eightieth percentile (5,000 versus 31,000 acres). Overall, the probability to marry down was 58–71 pp higher for women marrying during the interruption.

D. *Nonparametric Estimation*

Here I show that the interruption not only increased peer–commoner intermarriage but also disrupted sorting across different nobility titles. Similarly, I show

Table 3—The Season's Interruption and Marriage Outcomes, IV Estimation

	Married a commoner		Married an heir	
	(1)	(2)	(3)	(4)
Panel A. Second stage				
Married in 1861–1863	0.39	0.54	−0.33	−0.45
	(0.14)	(0.15)	(0.13)	(0.15)
Dependent variable mean	0.65	0.62	0.26	0.27
	Dependent variable: Married during interruption (1861–1863)			
Panel B. First stage				
Treatment[a]	0.01	0.01	0.01	0.01
	(0.00)	(0.00)	(0.00)	(0.00)
Controls	Yes	Yes	Yes	Yes
Distance London	No	Yes	No	Yes
Observations	644	484	644	484
F first-stage	18.3	11.6	18.3	11.6
Model	IV probit		IV probit	

	Spouses' landholdings					
	Difference (absolute value)		Difference (husband−wife)		Married down	
	(5)	(6)	(7)	(8)	(9)	(10)
Panel A. Second stage						
Married in 1861–1863	45.3	58.5	−44.6	−61.4	0.58	0.71
	(17.4)	(23.4)	(22.0)	(30.0)	(0.12)	(0.16)
	[0.04]	[0.06]	[0.02]	[0.02]	[0.00]	[0.02]
Dependent variable mean	29	30	−3	−5	0.53	0.55
	Dependent variable: married during interruption (1861–1863)					
Panel B. First stage						
Treatment[a]	0.01	0.01	0.01	0.01	0.01	0.01
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Distance London	No	Yes	No	Yes	No	Yes
Observations	324	260	324	260	324	260
F first-stage	9.2	4.5	9.2	4.5	9.2	4.5
Model	IV (liml)		IV (liml)		IV probit	

Notes: The baseline sample is all peers' daughters aged 15–35 in 1861 who ever married, excluding second marriages, women married to foreigners, and royals. Columns 5 to 10 evaluate sorting by landholdings and hence, mechanically exclude women for which Bateman (1883) does not list both spouses' family landholdings. Controls are defined in Table 2. Even columns also include the distance between the family seat and London and hence, restrict the sample to women with a recorded family seat. Standard errors clustered by birth year are in parentheses. When the first-stage F-stat is below 10, I report p-values based on Moreira's (2003) conditional likelihood ratio (CLR) in brackets.

[a]Synthetic probability (percent) to marry during Season interruption, based on marriage probabilities in "normal times."

that sorting by landholdings was distorted at various moments of the distribution beyond the mean. To do so, I use nonparametric methods based on contingency tables and Kolmogorov-Smirnov distribution tests. In detail, I consider my baseline sample (women aged 15–35 in 1861) and compare a high- versus a low-treatment cohort. The high-treatment cohort are women with a synthetic probability to marry during the interruption above 20 percent. This corresponds to the top quintile: the 20 percent of women with the highest synthetic probability to marry in 1861–1863.[38] Conversely, the low-treatment cohort are women below the top quintile.

I begin by showing that the interruption disrupted sorting across different titles in the nobility. Specifically, I construct a contingency table of wife's and husband's title for the high- and low-treatment cohorts (Table 4). The wife's title is arrayed across rows i. I consider barons' and viscounts' daughters (henceforth, BV's daughters) versus dukes', marquesses', and earls' daughters (henceforth, DME's daughters).[39] The latter are the highest ranks of the peerage. Their husbands' titles are arrayed across columns j. Each cell reports observed frequencies (O) and expected frequencies under random matching (E). Expected frequencies are $E_{i,j} = \frac{n_i \times n_j}{N}$, where n_i is the number of counts in the ith row, n_j is the number of counts on the jth column, and N is the total number of counts in the table.

Table 4 shows that when the Season ran smoothly, marital sorting was stronger. Consider the low-treatment cohort in panel A: expected frequencies are similar for DME's and BV's daughters.[40] That is, under random matching, they should marry similarly. In practice, DME's daughters married more peers' heirs and fewer commoners than the lower-ranked BV's daughters. Sorting patterns are different for the high-treatment cohort, that is, those most exposed to the interruption of the Season: observed and expected frequencies are similar for both DME's and BV's daughters. In other words, their marriages resemble random matching.[41]

Next, Table 5 compares sorting patterns across cohorts using chi-squared tests of association. First, it presents Pearson's chi-squared test of association (χ^2), which evaluates whether spouses sorted by title. Specifically, the null hypothesis is that marriages were random with respect to title. The test-statistic is

$$\chi^2 = \sum_{i=1}^{r}\sum_{j=1}^{c}\frac{\left(O_{i,j} - E_{i,j}\right)^2}{E_{i,j}}, \tag{5}$$

where $O_{i,j}$ and $E_{i,j}$ are the observed and expected frequencies in cell $\{i,j\}$, r is the number of rows, and c is the number of columns.

For women in the low-treatment cohort, the test rejects the null hypothesis that marriages were randomly set. In contrast, for the high-treatment cohort, the chi-square statistic is 7 times lower, and the null of random matching cannot be rejected. Column 3 confirms that the chi-square statistics are significantly different between the high- and low-treatment cohort. In other words, when the Season worked smoothly, women sorted by title; when the Season was interrupted, marriage resembles random matching. This result is not a by-product of the smaller sample in the high-treatment cohort. For a 2-by-4 contingency table, the Pearson's chi-squared

[38] Specifically, the high-treatment cohort are women aged 19 to 22 in 1861 (see Figure 4).

[39] The Hollingsworth dataset groups titles into DME versus BV to distinguish high versus low peerage titles. In online Appendix B3, I construct contingency tables with five rows, i.e., dukes', marquesses', earls', viscounts', and barons' daughters. Results are robust.

[40] Note that to calculate the expected frequencies for marriages with commoners ($E_{1,1}$ and $E_{2,1}$), I only consider commoners who married into the peerage.

[41] Random matching predicts 51 percent low-treatment DMEs to marry commoners. In practice, only 42 percent did so. In the high-treatment cohort, we expect 53 and observe 49 percent of such marriages.

Table 4—Contingency Tables

			Husband's rank at age 15				
			Commoner	Gentry	Peer's son	Peer's heir	Observations
Panel A. Low-treatment cohorts ($T <$ eightieth percentile)[a]							
Wife	Baron/Viscount's daughter	O	161	34	20	49	264
		E	135.8	32.8	25.2	70.2	
	Duke/Earl/Marquess's daughter	O	108	31	30	90	259
		E	133.2	32.2	24.8	68.8	
	Observations		269	65	50	139	523
Panel B. High-treatment cohorts ($T \geq$ eightieth percentile)[a]							
Wife	Baron/Viscount's daughter	O	34	11	5	10	60
		E	31.7	8.9	5.5	13.9	
	Duke/Earl/Marquess's daughter	O	30	7	6	18	61
		E	32.3	9.1	5.5	14.1	
	Observations		64	18	11	28	121

Notes: The baseline sample is all peers' daughters aged 15–35 in 1861 who ever married, excluding second marriages, women married to foreigners, and royals (Observations = 644). Cells report observed (O) and expected frequencies under random matching: $E = (n_i \times n_j)/N$, where n_i is the ith row's counts, n_j is the jth column's counts, and N is the table's total counts.

[a] T: Synthetic probability to marry in 1861–1863, based on marriage probability in normal times.

Table 5—The Interruption and Sorting by Title, Nonparametric Estimates

	Low-treatment (1)	High-treatment (2)	Difference (1) − (2)
Pearson's chi-squared, χ^2	24.6	3.5	21.1
	(0.000)	(0.320)	(0.000)
Likelihood ratio, $LR - \chi^2$	24.9	3.6	21.3
	(0.000)	(0.315)	(0.000)
Kendall's rank correlation, τ_b	0.20	0.11	0.09
	(0.000)	(0.191)	(0.164)
Observations	523	121	644

Notes: The baseline sample is peers' daughters aged 15–35 in 1861 who ever married, excluding second marriages, women married to foreigners, and royals (Observations = 644). Low- (high-) treatment cohorts are women with a synthetic probability to marry in 1861–1863 below (above) the eightieth percentile. τ_b ranges between -1 (negative) and $+1$ (positive assortative matching). Column 3 converts stats into Spearman correlation and uses Fisher's Z transformation (Rosenberg 2010). The distribution of τ_b is bootstrapped; p-values are in parentheses.

test requires no cells with zero count and an expected cell count of five or more in at least six cells. Both conditions are satisfied. Furthermore, the likelihood ratio test, which is accurate for small samples, confirms the results.[42]

Table 5 also presents Kendall's rank correlation coefficients (τ_b). The coefficient is based on the number of concordances (Q) and discordances (D) in paired observations. Two marriages are concordant (discordant) if the woman with higher title

[42] Formally, the likelihood ratio statistic is $\chi^2_{LR} = 2\sum_{i=1}^{r}\sum_{j=1}^{c} O_{i,j} \cdot ln\left(\frac{O_{i,j}}{E_{i,j}}\right)$.

of the two married the husband with higher (lower) title of the two. If the women's or men's titles are the same, the pair is tied. Formally,

$$\tau_b = \frac{Q - D}{\sqrt{\left(N(N-1)/2 - t_{wom}\right)\left(N(N-1)/2 - t_{men}\right)}}, \tag{6}$$

where N is the number of marriages; and $t_{wom} = \sum_i t_i(t_i - 1)/2$, where t_i is the number of ties in woman's title i (t_{men} is calculated analogously). Kendall's coefficient ranges between -1 (negative association) and $+1$ (positive association).

In the low-treatment cohort, the Kendall's rank correlation is positive and significantly different from zero: higher-titled women married men with higher titles. In other words, there was positive assortative matching. This result vanishes when the Season was interrupted: Kendall's rank correlation is halved and not significantly different from zero for the high-treatment cohort. A one-sided test rejects the null hypothesis that Kendall's rank correlation was higher for the high-treatment cohort. That said, I cannot reject the null that the high- and low-treatment have the same Kendall's rank correlation, with a p-value of 0.16.

Note that these estimates consider the baseline sample of women aged 15–35 in 1861 who ever married. This excludes unmarried individuals and men who failed to marry a peer's daughter. In online Appendix B4, I construct extended contingency tables with these populations and show that the nonparametric results are robust.

Finally, I present nonparametric estimates for the effect of the interruption on sorting by landholdings. By construction, the sample is restricted to women marrying in the landed elite—i.e., those for which Bateman (1883) lists both spouses' family landholdings. I use a Kolmogorov-Smirnov test to compare sorting by landholdings for the high- versus low-treatment cohorts. The test statistic is

$$K\text{-}S_{n,m} = \sup_y |H_n(y) - L_m(y)|, \tag{7}$$

where H and L are the cumulative distribution functions for the high- and low-treatment cohort; n and m are the sizes of each cohort; and y is the measure of sorting: the difference between spouses' percentile rank in landholdings.

Figure 8 presents the results. Panel A shows the difference between spouses' percentile ranks, in absolute value. When the Season worked smoothly, spouses were similar in terms of landholdings. For example, 50 percent of the marriages in the low-treatment cohort were between spouses ranked 20 percentiles away. In contrast, among high-treatment women, only 30 percent married husbands within 20 percentiles. Overall, the Kolmogorov-Smirnov test shows that spouses' ranks were more similar in the low- than in the high-treatment cohort. In other words, women at risk of marriage during the Season's interruption sorted less by landholdings. The evidence also suggests that after the interruption, sorting returned to its previous levels: sorting patterns are similar for low-treatment women who were "younger" and "older" than the high-treatment cohort. That is, for women who courted, respectively, after and before the interruption.

The disruption in sorting patterns is mostly driven by women marrying down. Panel B shows the difference between husband and wife in percentile ranks.

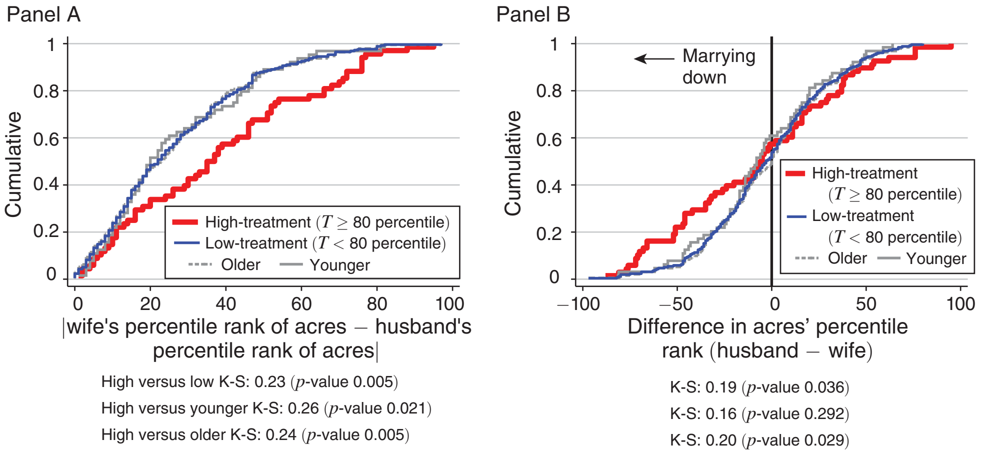

Figure 8. Sorting by Landholdings, Nonparametric Estimation

Notes: The sample are peers' daughters aged 15–35 in 1861, for which Bateman (1883) lists both spouses' family landholdings (Observations = 324). "High-treatment" cohorts are women with a synthetic probability to marry in 1861–1863, T, above the eightieth percentile (Observations = 64). "Low-treatment" comprises women with T < eightieth percentile (Observations = 256). The latter is subdivided into "younger" (Observations = 64) and "older" than the high-treatment cohort (Observations = 192).

Hence, negative values correspond to women marrying poorer husbands. While 20 percent of high-treatment women married husbands 50 percentiles poorer, only 6 percent of low-treatment women did so. The Kolmogorov-Smirnov test confirms that the distributions are different. In other words, that women with a higher risk to marry during the interruption married down more.

E. *Robustness and Extensions*

I perform several robustness checks and extensions of the analysis. This section briefly describes them; the detailed results are available in the online Appendix.

Treatment.—A woman's synthetic probability to marry during the interruption is based on (i) her age in 1861–1863 and (ii) the probability to marry at each age in a benchmark cohort who married in normal times. So far, I have used peers' daughters born in 1815–1830 as benchmark cohort. Tables B1 and B2 in the online Appendix show that my results are robust to using alternative benchmark cohorts. I report estimates of equations (2) and (3) where I define the treatment using five alternative benchmark cohorts: women born in 1810–1825, born in 1820–1835, married in 1845–1860, married in 1840–1855, and married in 1835–1850. As before, these exclude women married after 1861 and dead before age 30.

Nonparametric Estimates for Marriage Cohorts.—In Section IIID, I assigned women to the high- and low-treatment group based on their risk to marry during the interruption. That is, based on their age cohort. Online Appendix B2, instead, compares marriage cohorts: women marrying during the three-year interruption (treatment) versus women marrying three years before (control). Although marriage decisions are potentially endogenous, this exercise shows that the estimated effects are driven by women who actually married in 1861–1863. In other words, comparing marriage or age cohorts yields similar results. This further suggests that the pressure to marry young was high and that women could not endogenously select to marry during the interruption, before, or after.

Results for Men.—In online Appendix C, I estimate the effect of the Season on men. I first provide evidence that the interruption is a valid instrument for peers' sons: those who married in 1861–1863 were neither negatively selected nor the black sheep in their families.[43] That said, male aristocrats were not so pressured to marry young as women. Hence, it is less obvious how to define the treatment and sample or whether to base it entirely on ages in 1861. To overcome this, I use marriage cohorts and exploit an additional source of disruption to the Season over a longer time window: changes in the size of the marriageable cohort. Specifically, I estimate an instrumental variables model for peers and peers' sons marrying in 1851–1875. The treatment variable capturing the Season's intensity in each year is the number of attendees at royal parties. In the first stage, I instrument attendance with an indicator for the interruption and with the size of the marriageable cohort (the number of peers' daughters aged 18–24). The second stage regresses a man's marriage outcome on attendance at the Season in the year of his marriage.

The biggest threat to identification is if the size of the cohort also affected local, decentralized marriage markets, which emerged around peers' seats during the months when the Season was inactive. This scenario is unlikely. Gautier, Svarer, and Teulings (2010) and Botticini and Siow (2011) show that local, decentralized marriage markets are typically not subject to increasing returns to scale. That is, they are not affected by the size of the cohort.[44] As for the relevance assumption, the instrument accounts for substantial variation in Season's attendance: increasing the cohort by one individual increased attendance by 80 people (see online Appendix Table C2).

A higher attendance to the Season strengthened sorting among male aristocrats. It reduced peer–commoner intermarriage, although the effects are weaker than for women (online Appendix Table C2).[45] The effects are larger for sorting by landed wealth: increasing attendance at the Season by 5 percent—250 additional attendees—reduced spouses' difference in acreage by 0.6–0.7 percentile ranks (online Appendix Table C3).[46] I also show that the Season increased sorting by an index based on the first principal component of title, acreage, land rents, antiquity of the

[43] The percentage of male heirs and of higher peerage ranks married in 1861–1863 is very similar to that in the three years before and after. Also, family fixed effect estimates show that peers' sons who married in 1861–1863 did so at the same age as their brothers who married in normal times. This refutes the possibility that, within peerage families, some sons delayed marriage until the Season resumed, while others (the "black sheep") selected to marry during the interruption.

[44] In addition, the Sargan test cannot reject the instruments' exogeneity, and I show that the bias would be small for slight violations of the exclusion restriction (see online Appendix C5).

[45] Online Appendix Table C2 also reports estimates for women. Results are consistent with those of Section IIIB, giving credibility to the econometric specification used here.

[46] The effect is stronger than for marrying commoners because of market depth. The Season, a meeting technology, was indispensable to meet the fewer great landowners' daughters.

lineage, the peerage of the title, and the ownership of woods. Finally, by centralizing the marriage decisions in London, the Season matched spouses from distant geographical origins.[47]

Increasing Returns to Scale (*IRS*).—In online Appendix C4, I use the framework described above to show that the Season displayed IRS: the output of the matching function (i.e., sorting) increases more than the proportional change in inputs (i.e., attendees). Whether the matching function displays IRS or not has important economic and demographic implications. For example, under IRS, fertility booms will echo over time: a "boom" cohort may encounter partners more easily, marry earlier on, and hence, have more children. Previous studies have estimated returns to scale in marriage markets by comparing the city and the countryside (Gautier, Svarer, and Teulings 2010; Botticini and Siow 2011). Instead, I consider a matching technology that not only pooled singles together but explicitly facilitated their courtship. In this respect, my results provide better insights for increasingly common matching technologies, e.g., dating websites.

IV. Implications: Elite Capture and Public Goods

So far, I have shown that the interruption of the Season increased women's marriages to commoners. This section examines the corresponding political-economy implications. First, I examine elections of Members of Parliament and show that a woman's marriage to a commoner reduced her blood relatives' political power in the following decades. Next, I show that families who lost political power could not effectively oppose state-education provision in the 1870s—a policy otherwise subject to capture by local landowners (Stephens 1998).

A. *Marital Sorting and Political Power*

Elsewhere it has been argued that elites pursue and maintain political power by opposing inclusive institutions,[48] by exploiting their economic power (Baland and Robinson 2008), and through personal ties—i.e., marrying strategically to prevent entry by newcomers (Marcassa, Pouyet, and Trégouët 2020). For example, Cruz, Labonne, and Querubín (2017) show that candidates for public office in the Philippines are disproportionately drawn from central families in the marriage network. Similarly, Puga and Trefler (2014) find that the wealthiest Venetian merchants used marriage alliances to monopolize the galley trade and to block political competition. The Season was no different. Attending it and marrying in the peerage was crucial for political achievement. Lady Aberdeen wrote that the Season "was a part of the very life of the people who had the largest stake in the country (…) Nobody could well come to the front without participating in it to some degree" (Ellenberger 1990, 637).

[47] Online Appendix C4 evaluates match quality, proxied by consanguinity and fertility.

[48] Sokoloff and Engerman (2000); Acemoglu (2008); Allen (2009); Galor, Moav, and Vollrath (2009).

Did the interruption of the Season and the increase in women's marriages to commoners affect the political power of peerage families? To evaluate this question, I use the synthetic probability to marry during the interruption as an instrument for a woman's marriage to a commoner. I then look at whether a woman's marriage to a commoner affected her blood relatives' probability to be elected Member of Parliament in the House of Commons. I focus on MPs because being elected is a good proxy for an individual's overall political power and influence on public policy. In normal circumstances, landed aristocrats controlled MP elections by distributing favors, rents, and jobs among the local electorate—especially before the introduction of the secret ballot in 1872 (Baland and Robinson 2008). According to Edward Stanley, "When any man attempted to estimate the probable result of a county election in England, it was ascertained by calculating the number of the great landed proprietors in the county and weighing the number of occupiers under them" (Baland and Robinson 2008, 1738).

Although women could not become MPs, the interruption of the Season and women's marriages to commoners could affect MP elections through several channels. One possibility is that a woman exposed to the interruption became poorer because she married a commoner or a husband with smaller family landholdings (and hence, with a smaller allowance). Since the husband could not provide her with the life of comfort she was accustomed to, her birth family had to step in. They mobilized considerable capital (portions, allowances, etc.) to sustain her, diverting resources away from their local electorate. Another related possibility is that a woman's marriage to a commoner reduced her birth family's social prestige and hence, its political power. Some noblewomen played the role of "power brokers," bringing "together politicians for the informal social contact which could make or break a career" (Atkins 1990, 45). Marrying a commoner limited a woman's role as power broker and her capacity to promote her kin's political career through such informal social contact. Similarly, Allen (2009) argues that many public offices were appointed through patronage, not merit. A woman's marriage to a commoner could exclude her kin from these appointments and/or limit her kin's capacity to allocate jobs (Allen 2009, 306). This, in turn, could reduce the family's grasp over the electorate.

To evaluate these issues empirically, I consider 279 women in my baseline sample (aged 15–35 in 1861) with a family seat in England. I restrict the sample to England because only there I have state-education data for Section IVB. To measure the political power of a woman's family, I check whether her brothers were elected MP and how many years they served. I also look at whether they were elected MP in the family seat's constituency. This proxies for local political power, which could affect policies implemented locally—e.g., the introduction of state education (Stephens 1998). I also evaluate the political power of the head of a woman's birth family (henceforth, family head). Specifically, I assess whether the family head in the 1870s—when state education was introduced—was elected MP and how many years he served.

Figure A8 in the online Appendix provides evidence that a woman's marriage to a commoner reduced her birth family's political power. It considers the sample of peerage families described above and reports the number of brothers elected MP before and after their sister's marriage. The thin blue line (thick red line) is for women who married in the peerage (married a commoner). Before the marriage, both groups had the same number of MPs. Ten and 20 years after it, however, the number of MPs was much lower for families in which a woman married a commoner.

My main specification is an instrumental variables model. The first stage uses the interruption of the Season to capture exogenous variation in a woman's probability to marry a commoner, M. Specifically, I model M as in Section III:

$$M_{i,j,t,s} = \beta^M T_t + \mathbf{X}'_{i,j,t,s}\delta^M + \nu^M_{i,j,t,s}, \tag{8}$$

where i indexes a woman in the baseline sample, j her birth family, t her age in 1861, and s her family seat. The treatment, T_t, is the synthetic probability to marry during the Season's interruption (see equation (3) for details). In the second stage, β^P captures the effect of marrying a commoner on family j's political power:

$$P_{i,j,t,s} = \beta^P \hat{M}_{i,j,t,s} + \mathbf{X}'_{i,j,t,s}\delta^P + \nu^P_{i,j,t,s}, \tag{9}$$

where P is one of the six measures of political power described above. To isolate the effect of marriage on political power, P is restricted to MP elections *after* i's marriage. The index s allows me to evaluate whether a member of family j was elected MP in the county where s is located. The vector $\mathbf{X}$ includes alternative predictors of political power: family j's title, the distance from seat s to London, the number of brothers,[49] and county characteristics from Hechter (1976) (percent working in manufacturing, log income p.c., percent voting conservative in the general elections of 1885, percent of nonconformists, and religiosity). I also include a covariate capturing family j's previous political power. This is defined as P but considering only the MP elections of i's father before i's marriage.[50] This covariate controls for the unlikely possibility that, as Parliament met in 1861–1863, the daughters of MPs moved to London with them and attended private balls during the interruption (see Section IIIA). The unit of observation in the regressions is a woman. Since some of the observations are sisters, I also report p-values clustered by family.

Table 6 presents IV estimates of equations (8) and (9). The first stage confirms that women with a high synthetic probability to marry during the interruption were more likely to marry a commoner. Note, however, that the sample size is smaller than in Section III and, hence, the F-statistic is low.[51] To address this, I report p-values based on Moreira's (2003) conditional likelihood ratio.

Panel A shows that, by increasing women's marriages to commoners, the interruption of the Season reduced the political power of some peerage families in the following decades. Specifically, after a woman's marriage to a commoner, her brothers were 50 percent less likely to be elected MP (column 1) and, together, they

[49] I exclude brothers who died before reaching majority (age 21) and hence, who could not be elected MP. To account for large families, I also include a quadratic term.

[50] Using the MP elections of i's brothers before i's marriage would understate a family's previous political power, e.g., for women with many brothers under age 21—the age of majority.

[51] This is because I restrict the sample to women with seats in England and include county covariates correlated with MP elections (second stage) but less relevant in the first stage.

Table 6—Women's Marriages to Commoners and Her Family's Political Power, IV Estimation

	Dependent variable: Family's political power *after* woman's marriage					
	Any brother is MP (1)	All brothers' MP years (2)	Any brother is local MP (3)	All brothers' local years (4)	Family head is MP (5)	Family head's MP years (6)
Panel A. Second stage						
Woman married a commoner	−0.54	−18.40	−0.47	−11.00	−0.49	−7.83
	[0.004]	[0.033]	[0.037]	[0.025]	[0.006]	[0.019]
	{0.025}	{0.015}	{0.102}	{0.010}	{0.007}	{0.013}
Woman married a peer/peer's son	ref.	ref.	ref.	ref.	ref.	ref.
Mean of dependent variable	0.42	4.76	0.23	1.84	0.18	1.39
	Dependent variable: Woman married a commoner					
Panel B. First stage						
Treatment (synthetic prob.)[a]	0.008	0.008	0.008	0.008	0.010	0.010
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Woman birth order	Yes	Yes	Yes	Yes	Yes	Yes
F-statistic	3.4	3.4	3.4	3.4	3.3	3.2
Observations	270	270	270	270	279	279
Baseline controls	Yes	Yes	Yes	Yes	Yes	Yes
County controls	Yes	Yes	Yes	Yes	Yes	Yes
Number of brothers	Yes	Yes	Yes	Yes	No	No
Political power before	Yes	Yes	Yes	Yes	Yes	Yes
Model	IVprobit	IV	IVprobit	IV	IVprobit	IV

Notes: The sample is 279 women in the baseline sample (i.e., aged 15 to 35 in 1861) with a family seat in England. Panel A presents estimates for the effect of a woman's marriage to a commoner on the political power of her brothers (columns 1 to 4) and of the family head in the 1870s (columns 5 and 6). Columns 1 to 4 report 270 observations because 9 women had no brothers. In odd columns, the dependent variable indicates if any brother was elected MP (column 1), elected MP in the family seat's county (column 3), or if the family head was elected MP. In even columns, the dependent variable is the corresponding number of years served as MP. Baseline controls are indicators for duke/marquess/earl's families and for English titles, and distance from family seat to London. County controls are percent working on manufacturing, income p.c., percent voting conservative in the 1885 General elections, percent nonconformists, and religiosity. When a family owns seats in different counties, I take the average. Number of brothers excludes brothers who died before age 21 and includes a quadratic term. "Political power before" is identical to the dependent variables but considers only the MP elections of fathers before a woman's marriage. Panel A reports CLR *p*-values (Moreira 2003) in square brackets and CLR *p*-values clustered by family in curly brackets; panel B reports standard errors in parentheses.

[a]Synthetic probability (percent) to marry during interruption, based on marriage probabilities in normal times.

served 18 fewer years (column 2) than the brothers of women who married in the peerage. The loss of political power was local: the brothers of women who married a commoner were 47 percent less likely to be elected MP (column 3) and served 11 fewer years (column 4) in the county where their (birth) family seat was located.

Since women typically moved away to live with their husbands, why would their birth families' local power be affected? Even if women moved away, marrying a commoner would force her birth family to divert resources away from local electorates (see discussion above). In addition, during the Season's interruption, marriages became more local (Figure 5). In other words, women probably married "local" commoners, reducing her birth family's local prestige. Admittedly, a local marriage can help to consolidate the family's estates and, hence, to tighten the family's grasp over the electorate. That said, Mingay (1963) argues that by the late nineteenth century, local marriages were not used for estate consolidation.

Finally, I show that those who were family heads in the 1870s also lost political power. After a woman's marriage to a commoner, the head of her birth family was 49 percent less likely to be elected MP and served 8 fewer years than the family heads of women who married in the peerage.

In online Appendix B8, I show that peerage families also lost political power as a result of marriages with a family of lower estates. I estimate the IV model described above where M is based on the difference in spouses' family landholdings—which mechanically restricts the sample to those marrying in the landed elite. I find that, after a woman married down by family landholdings, her brothers were 42 percent less likely to be elected MP and served 9.8 fewer years in the county where their (birth) family seat was located (see online Appendix Table B10). Altogether, these findings illustrate a negative relationship between within-landed-elite marriages, landownership, and how contested MP elections were in late nineteenth-century England.

Admittedly, nineteenth-century Britain saw numerous political changes, including three Reform Bills extending the franchise. That said, it is unlikely that these changes can explain away my results. Figure A7 in the online Appendix plots the number of sampled individuals elected MP over time. That is, the elections of fathers and brothers of women in my baseline sample (i.e., aged 15 to 35 in 1861). In this sample, only the 1832 Reform Act significantly altered MP elections.[52] After the interruption of the Season, there is no evidence of a downward trend, even though the Reform Acts of 1867 and 1884 enfranchised the urban male working class and agricultural laborers. In other words, my sample and estimates are likely to capture the effect of the interruption of the Season and women's marriages to commoners and not broader political changes in the nineteenth century.

Finally, I explore the effect of men's marriages on political power. Here I briefly describe the results; details are in the online Appendix. First, I consider the possibility that the brothers of women exposed to the interruption were exposed to it themselves. Online Appendix B9 shows that this does not explain my estimates. I estimate the effect of a woman's marriage to a commoner on her brothers' political power, limiting the latter to those who married before the interruption. Results are robust. In fact, the birth year of women and their brothers does not overlap excessively, i.e., their exposure to the interruption is different. Next, online Appendix B10 shows that a man's marriage to a commoner also reduced his birth family's political power and that the effects are qualitatively similar for older and younger brothers. This is difficult to reconcile with the possibility that my results are driven by a secular decline in peers' political power.

[52] The 1832 reform reapportioned seats in Parliament favoring cities and industrial areas and abolished most rotten boroughs. Hence, my measures of political power *after* a woman's marriage do not incorporate elections in rotten boroughs. In fact, family seats in my sample were only 8.27 miles from an enfranchised constituency (see online Appendix Table A4 and discussion).

B. *Effects on Public Good Provision*

So far I have shown that the interruption of the Season increased women's marriages to commoners, which, in turn, reduced the political power of some peerage families. Here I evaluate the impact of these changes on public policy.

The introduction of state education in England in the 1870s provides a good testbed to evaluate this question. Education provision was decentralized and hence, subject to capture by peers with local political power (Stephens 1998). Specifically, state schools were built and run with funds from rates—wealth taxes raised by local school boards in each poor law district and borough. Since taxes were levied on land property, local elites were expected to pay for most of the new schools. However, peers galvanized into "a furry of activity to ward to the dread intrusion of a School Board" (Thompson 1963, 208). They took over school boards and brought taxes down, especially where they held local political power (Goñi 2021a). Peers with political power encouraged the election of board members favorable to their interests. The election system of board members facilitated this: First, because only those with a rent or land valued at £10 or above could elect board members. Second, because elections were based on cumulative voting, which can favor interest groups such as the landed aristocracy (Stephens 1998). Interestingly, women were eligible to sit on school boards. Yet in 1870, only seven women were elected (Hollis 1987).[53] Another related possibility is that peers with local political power could effectively lobby elected school board members to set low education taxes.

I test the hypothesis that, by affecting peers' political power, the interruption of the Season and women's marriages to commoners reduced the peerage's influence over school boards. Schematically, this can be summarized as

$$\begin{array}{c}\textit{Season}\\\textit{interruption}\end{array} \Rightarrow \begin{array}{c}\textit{Women's}\\\textit{marriages to}\\\textit{commoners}\end{array} \Rightarrow \begin{array}{c}\textit{Families'}\\\textit{political}\\\textit{power}\end{array} \Rightarrow \begin{array}{c}\textit{School}\\\textit{boards' state}\\\textit{education}\end{array}$$

My identification strategy is an instrumental variables approach that compares education provision in the vicinity of different peers' family seats. Specifically, I compare family seats in which a woman married a commoner (and, hence, the family lost political power) to family seats in which a woman married in the peerage (and, hence, the family retained political power).[54] The interruption of the Season provides exogenous variation in a woman's probability to marry a commoner and hence, in her birth family's political power. To illustrate my strategy, consider Binfield and Cassiobury, the seats of Baron Kinnaird and Earl Essex (Figure 9). These seats are only 22.5 miles apart. That is, they are subject to similar local conditions that may affect education provision. In addition, the two seats belong to families with similar status and past political influence. Only their marriage patterns

[53] Hence, my results cannot be explained by women being/not being elected to a school board.

[54] Online Appendix B7 shows that the distance from a woman's birth-family seat to London is not significantly associated to her probability to marry a commoner (correlation 0.058, p-value 0.335) or to the size of the family landholdings (correlation −0.39, p-value 0.616).

differ. Olivia Kinnaird grew up in Binfield. She was 22 when the Season was interrupted and married a commoner. In the following decades, none of her brothers were elected MP and her father, who was the family head in 1870, did not hold any public position in England. In contrast, Adela Capel, from Cassiobury, was 33 in 1861, attended the Season before its interruption, and married an earl. Her father, who was the family head in 1870, had been a famous supporter of Robert Peel and was offered the position of Lord Lieutenant of Ireland. In sum, these different marriages affected the political power of the respective families and hence, their capacity to undermine state education in the 1870s. On average, school boards in a 10-mile radius of Binfield, the seat of Olivia Kinnaird, taxed wealth at 4.2 percent. By contrast, near Cassiobury, the average tax rate was only 1.5 percent.

Formally, I estimate the relation between women's marriages to commoners, their birth families' political power, and education provision in two steps. First, I document a reduced-form effect of women's marriages to commoners on education provision. To do so, I estimate an IV model where the first stage takes the form of equation (8); i.e., I use a woman's synthetic probability to marry in 1861–1863, T, as an instrument for her probability to marry a commoner M. The second stage is

$$E_{i,j,t,s} = \beta^e \hat{M}_{i,j,t,s} + \mathbf{X}'_{i,j,t,s}\delta^e + \nu^e_{i,j,t,s}, \tag{10}$$

where i indexes a woman in the baseline sample, j her birth family, t her age in 1861, and s her birth family seat. E captures the provision of state education. Specifically, E is the average tax rate set by school boards in a ten-mile radius of seat s between 1872 and 1878. The coefficient β^e captures the reduced-form effect of i's marriage to a commoner on education provision around her family seat s.

Second, I show that this reduced-form effect is explained by the loss of political power associated with marrying a commoner. Again, the interruption of the Season is the source of exogenous variation: in the first stage, I directly use a woman's synthetic probability, T, as an instrument for her birth family's political power, P:

$$P_{i,j,t,s} = \eta T_t + \mathbf{X}'_{i,j,t,s}\theta + \xi_{i,j,t,s}. \tag{11}$$

In the second stage, β^E captures the effect of peerage families' political power on education provision:

$$E_{i,j,t,s} = \beta^E \hat{P}_{i,j,t,s} + \mathbf{X}'_{i,j,t,s}\delta^E + \nu^E_{i,j,t,s}. \tag{12}$$

All estimates are based on my baseline sample: women aged 15–35 in 1861 who ever married. As before, I measure the political power of a woman's birth family with the MP elections of her brothers. For education provision, I use the reports of the Committee of Council on Education, digitized by Goñi (2021b). I look at wealth taxes from 943 school boards located within 10 miles of the family seats in my sample. Since education was provided locally, the unit of observation in my regressions is a family seat (and the area around it). That is, I consider the 387 family seats of women in the baseline sample. Since some families owned more than one seat, the

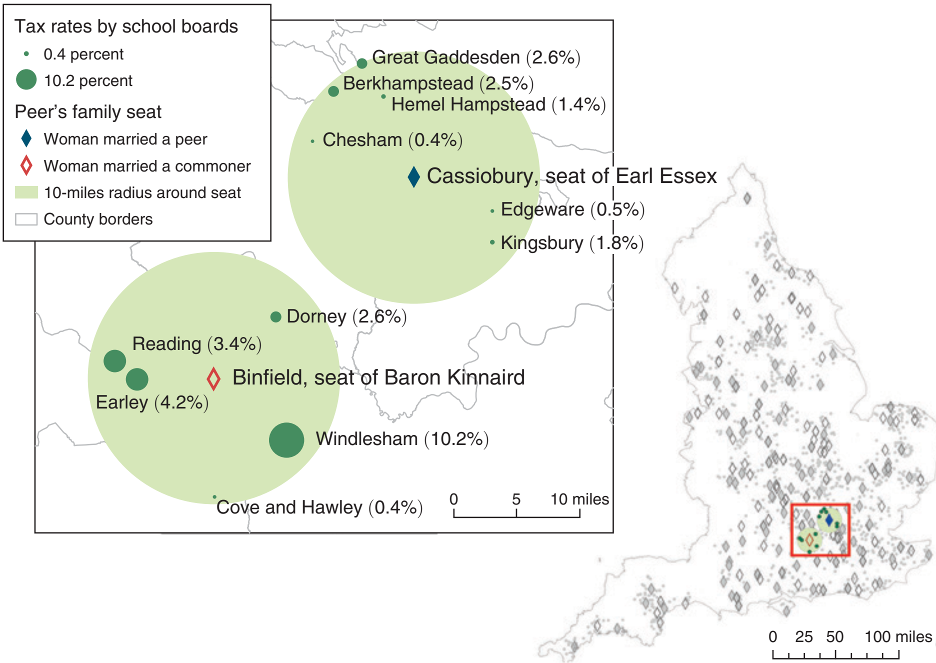

Figure 9. Investments in State Education around Peers' Family Seats, Example

Notes: This map shows the average tax rate set by school boards near Cassiobury and Binfield in 1872–1878. It also shows all sampled seats in England and the school boards in a ten-mile radius. Shapefiles for England provided by the Historic Counties Trust, Historic County Borders Project.

number of observations is larger than in the previous section.[55] To address this, I report p-values clustered by family. The sample is restricted to England because only there I have education provision data.

Table 7 reports the results. First-stage estimates in panel B confirm my previous findings: increasing a woman's synthetic probability to marry during the interruption increased her probability to marry a commoner (column 1). It also decreased her brothers' probability to be elected MP (column 2), to be elected MP in the family seat's county (column 4), and how many years her brothers served as MP (columns 3 and 5) after her marriage. The family heads in the 1870s were similarly affected (columns 6 and 7). The magnitudes are large. For example, every percentage point increase in a woman's risk to marry in 1861–1863 reduced by 1 pp her family head's probability to be elected MP. That said, the F-statistics are low. To address this, panel A reports CLR p-values adjusted for weak instruments.[56]

Panel A reports second-stage estimates. Column 1 documents a reduced-form effect of peer–commoner intermarriage on education provision. Wealth taxes for education were 2.66 percentage points higher near the seats of families in which a

[55] Of the 279 women in the baseline sample with a family seat in England, 2 had 4 seats, 22 had 3 seats, 58 had 2 seats, and 197 had 1 seat. Hence, the total number of seats is 387.

[56] The Lagrange multiplier K and J overidentification tests are consistent with CLR p-values.

Table 7—Determinants of Investments in State Education, IV Estimation

	Average tax rate for education within ten miles of family seat (percent)						
Dependent variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A. Second stage							
Woman married a commoner	2.66	—	—	—	—	—	—
	(0.007)						
	[0.035]						
Any brother is MP	—	−1.69	—	—	—	—	—
		(0.002)					
		[0.015]					
All brothers' MP years	—	—	−0.12	—	—	—	—
			(0.033)				
			[0.063]				
Any brother is local MP	—	—	—	−8.20	—	—	—
				(0.023)			
				[0.049]			
All brothers' local MP years	—	—	—	—	−0.27	—	—
					(0.008)		
					[0.033]		
Family head is MP	—	—	—	—	—	−1.86	—
						(0.017)	
						[0.055]	
Family head years MP	—	—	—	—	—	—	−0.62
							(0.043)
							[0.12]
		Political power in family seat *after* woman's marriage					
	Woman married a commoner	Any brother is MP	All brothers' MP years	Any brother is local MP	All brothers' local years	Family head is MP	Family head's MP years
Panel B. First stage							
Treatment[a]	0.008	−0.008	−0.170	−0.004	−0.085	−0.009	−0.053
	(0.027)	(0.001)	(0.006)	(0.086)	(0.018)	(0.001)	(0.046)
	[0.056]	[0.003]	[0.008]	[0.053]	[0.008]	[0.005]	[0.050]
Birth order	Yes	Yes	Yes	Yes	Yes	Yes	Yes
F-stat	4.33	10.0	6.16	10.74	5.85	1.68	3.16
Observations	387	374	374	374	374	387	387
Baseline co.	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County co.	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of brothers	No	Yes	Yes	Yes	Yes	No	No
Pol. before	No	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table reports estimates of equations (8) and (10) (column 1) and equations (11) and (12) (columns 2–7). The sample is women in the baseline sample with a family seat in England. The unit of observation is a family seat (and the ten miles around it). Some women had several seats, hence, Observations = 387 is larger than before. In panel A, variables indicate if a woman i living in seat s married a commoner (column 1), if any of her brothers was elected MP (column 2), elected MP in the family seat's county (column 4), or if the family head in the 1870s was elected MP (column 6). Columns 3, 5, and 7 consider the corresponding number of years as MP. Columns 1 to 4 report fewer observations because nine women had no brothers. Baseline and county controls, "Number of brothers," and "Pol. before" are defined in Table 6. Parentheses report p-values (first stage) and CLR p-values adjusted for weak IV (second stage). Brackets report CLR p-values adjusted for family clusters.

[a]Synthetic probability (percent) to marry during interruption, based on marriage probabilitie in normal times.

woman (exogenously) married a commoner than near seats of families in which a woman married in the peerage. The effect is economically meaningful. Given that the average tax in the sampled school boards is only 2.3 percent, the estimated effect amounts to doubling tax rates.

Columns 2 to 7 show that this reduced-form effect is the result of a loss of political power associated with women's marriages to commoners. Wealth taxes for education were lower near the seats where peers had retained political power than near the seats where they had lost it after a woman's marriage to a commoner. Specifically, taxes were 1.69 pp lower near seats where any brother was elected MP (column 2). Every additional year served as MP reduced taxes by 0.12 pp (column 3). These effects are larger when I narrow the focus on local political power (columns 4 and 5): if any brother was elected MP in the family seat's county, taxes for education were 8.2 pp lower. Every additional year as local MP reduced taxes by 0.27 pp, twice the effect in column 3. Finally, families who lost political power in the 1870s—when the policy was implemented—were also less likely to capture school boards and push taxes down: near the seats in which the 1870s' family head was elected MP, education taxes were 1.9 pp lower (columns 6 and 7).

Differences in tax rates across school boards could be explained by local socioeconomic conditions. To address this, all regressions include county-level covariates that are potentially correlated with education provision: income per capita; religiosity; and the percentage of employment in manufacturing, of nonconformists, and of conservative vote in the 1885 general election. I also control for the distance between family seats and London—which may be correlated with attendance to the Season. Online Appendix B11 performs three additional robustness checks: First, I include fixed effects for seats in the same 50-by-50 miles grid cell. That is, I estimate the effects using variation within close-by geographic areas. Second, I exclude school boards in cities to show that the effects are not driven by urbanization. Third, I relax the assumption that peerage families influenced school boards in a ten-miles radius around their seats. To do so, I pair every seat with each of the 1,433 school boards in England. I then define the dependent variable as the *weighted* average tax rate, where weights decay exponentially by the distance between school boards and seats.

So far, I have argued that, after a woman's marriage to an aristocrat, her birth family retained political power and hence, could effectively capture local school boards and undermine state education. Next, I consider two alternative mechanisms. One possibility is that, after a woman's marriage to an aristocrat, the budget constraint of her birth family was relaxed, allowing them to set lower taxes without affecting the funds raised for education or the quality of state schools. Online Appendix B11 shows that this was not the case. I find very similar results when I use the total education funds raised from taxes instead of the tax rate: near the family seats in which a woman married an aristocrat, the average school board not only set lower tax rates but also raised 1.7 percent fewer funds for education than near seats of families in which a woman married a commoner. Education funds were also lower where peers had retained political power than where they had lost it after a woman's marriage to a commoner (online Appendix Table B14).[57]

Another possibility is that families in which a woman married an industrialist—the main supporters of state education—became more sympathetic to the husband's plight

[57] Additional evidence from Goñi (2021a) suggests that funds raised from taxes are correlated with county-level measures of state-education quality (see Figure B5 in the online Appendix).

and changed their political preferences. This could affect institutional outcomes requiring a broad coalition in Parliament.[58] Empirically, this hypothesis is hard to evaluate, as I do not observe whether the husband was an industrialist or his political preferences. Alternatively, I can assess the political preferences of husbands in the peerage. In online Appendix C4, I show that the Season's interruption did not alter sorting by political preferences: peers in liberal (conservative) clubs continued to marry the daughters of liberal (conservative) club members. In other words, it is unlikely that the interruption altered preferences for redistribution, education provision, etc., at least among those marrying in the peerage.[59]

Altogether, the evidence suggests that the interruption of the Season (exogenously) increased marriages between peers' daughters and commoners. As a consequence, peerage families lost political power, limiting their ability to take over local school boards and undermine the introduction of state education in the 1870s.

V. Conclusion

In nineteenth-century Britain, from Easter to August each year, the children of the nobility engaged in a whirlwind of social events—the Season. From presentations at court to royal parties, the objective was to pull together the right sort of suitors and to aid in their courtship. This paper shows that by reducing search costs and segregating "undesirable" suitors, this institution crucially contributed to marital sorting. To establish causality, I focus on three years during which the Season was interrupted by the deaths of Queen Victoria's mother and husband. Women who were at risk of marriage when the Season was interrupted were 30 percent less likely to marry peers' heirs and 40 percent more likely to marry commoners. Within the landed elite, sorting by landholdings decreased by 30 percent, and women married husbands 44 percentile ranks poorer. These findings reconcile the empirical evidence with the theoretical search literature[60] by showing that a matching technology with low search costs and market segmentation can generate sorting.

The interruption of the Season halted social interactions within the peerage temporarily. By affecting marriage decisions, however, it had long-lasting consequences. Using data on elections of Members of Parliament, I show that a woman's marriage to a commoner halved her blood relatives' probability to be elected MP, especially for local constituencies near the family seat. This, in turn, affected the introduction of state education by local school boards in the 1870s. Education investments were twice larger near the domains of families in which a woman married a commoner (and hence, the family lost political power) than near the domains of families in which a woman married a peer (and hence, the

[58] Similarly, Jha (2015) argues that, in the 1640s, common financial interests facilitated a broad coalition favoring Parliament supremacy across otherwise different groups.

[59] In addition, results are not driven by the late-1870s fall in land values (see Section I). Land values fell due to a nationwide fall in grain prices. My identification strategy exploits local variation in education provision within England and hence, is not affected by nationwide trends.

[60] Burdett and Coles (1997); Eeckhout (1999); Bloch and Ryder (2000); Shimer and Smith (2000); Adachi (2003); Atakan (2006).

family retained political power). These findings have important implications: they show empirically that, in late nineteenth-century England, marital sorting played an important role for elite consolidation (Puga and Trefler 2014; Marcassa, Pouyet, and Trégouët 2020), distorted public goods' provision and the emergence of inclusive institutions.[61]

The marriage market embedded in the London Season echoes with some increasingly popular matching technologies. These typically lower search costs but can also restrict the choice sets for partners through search filters and customized recommendations.[62] Whether such matching technologies will lead to greater sorting is an intriguing question for future research.

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[61] Sokoloff and Engerman (2000); Acemoglu (2008); Allen (2009); Galor, Moav, and Vollrath (2009).

[62] For example, dating websites like "Ivy Date" or "nChooseTwo" (Harvard University, MIT, and Boston University) aim at matching top university students; Gray and Farrar, an exclusive dating service in Britain, only admits those who can afford a (cheapest) fee of £15,000.

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