# Online Appendix

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

Marc Goñi[1]

## Appendix A. Data appendix

In this appendix, I describe the original data sources used in this paper and present summary statistics for the main variables in the empirical analysis.

### A. 1 Peerage records

Peerage records have chronicled the family histories of the peerage of Britain and Ireland since Arthur Collins published his *Peerage* in 1710. Many follow-up genealogical studies have updated his work. Among them, three peerage records stand out: Burke's *Peerage and Baronetage*, Debrett's *The Peerage of the United Kingdom and Ireland*, and Cokayne's *Complete Peerage*. The genealogist John Burke wrote *Landed Gentry*, a similar record for knights and baronets. This last piece tends to be quite mythological, the result of centuries of word-of-mouth information. Oscar Wilde once said, "It is the best thing the English have done in fiction" (Burke et al. 2005).

For the sake of illustration, Quote A1 trascribes the entry for Charles George Lyttelton from Cokayne's *Complete Peerage*. He was born in 1842 and married when he was almost 36. He held the titles of Baron of Frankeley and Baron of Westcoote of Ballymore. Lyttelton was also Viscount Cobham and Baron Cobham, but received this honor only at age 46 on the death of a distant cousin. Note that the entry provides similar information for his wife, Mary Susan Caroline Cavendish. She was 11 years younger and the daughter of the 2nd Baron Chesham.

Hollingsworth (1964) collected this genealogical material for his study of the British peerage. He tracked all peers who died between 1603 and 1938 (primary universe) and their offspring (secondary universe). The primary universe was defined from Cokayne's *Complete Peerage*. The universe of children was found in a variety of sources: Collins' *Peerage of England*, Lodge's *Peerage of Ireland*, Douglas' *Scots Peerage*, Burke's *Extinct Peerage* and modern peerage editions by Burke and Debrett. The remaining gaps were filled from a large list of sources, among which Burke's *Landed Gentry* stands out. See Hollingsworth (1964) for details. The Cambridge Group for the History of Population and Social Structure now owns the dataset. They re-digitized the 30,000 handwritten original index sheets in 2001. In its current form, the data comprise approximately 26,000 individuals. In this paper, I consider a baseline sample of 644 women aged 15 to 35

[1]Department of Economics and FAIR, Norwegian School of Economics (NHH); Email: marc.goni@nhh.no

in 1861 who ever married.[2]

QUOTE A1: Charles Lyttelton, Cockayne's *Complete Peerage*

> CHARLES GEORGE (LYTTELTON), LORD LYTTELTON, BARON OF FRANKLEY [1794] also BARON WESTOOTE OF BALLYMORE in the peerage of Ireland [1776] also a *Baronet* [1618], s. and h., by 1st wife, b. 27 Oct. 1842; ed. at Eton and at Trin. Coll., Cambridge; M.P. for East Worcestershire, 1868-74: *suc. to the peerage*, 18 April 1876; Land Commr., 1881-89; *suc.* as VISCOUNT COBHAM AND BARON COBHAM, on the death, 26 March 1889, of his distant cousin (the Duke of Buckingham and Chandos, Viscount Cobham. & c.). under the *spec. rem.* in the creation of that dignity. 23 May 1718. He m. 19 Oct. 1878, Mary Susan Caroline, 2d da. of William George (CAVENDISH), 2d BARON CHESHAM, by Henrietta Frances. da. of the Rt. Hon. William Saunders Sebright LASCELLES. She was b. 19 March 1853.
>
> Cokayne (1893), p. 187

## A. 2 Bateman's *Great Landowners*

I collected a new dataset of family landholdings in the peerage from Bateman's *Great Landowners* (1883). The book consists of a list of all owners of 3,000 acres or more by 1876, worth £3,000 a year. Also, 1,300 owners of 2,000 acres or more are included. It is based in the Return of Owners of Land, 1873; the first complete picture of the distribution of landed property in the British Isles since William the Conqueror commissioned the *Domesday Book* in 1086. The objective of the Return was to counter the rising public clamor about the overconcentration of land ownership within the elite. Although the House of Lords stressed the importance of using reliable and independent data to refute the attacks, the Return was not without inaccuracies (Bateman 1883: preface). Bateman revised and corrected these errors. Several great landowners, however, felt disparaged. They wrote letters with outrage and demands for the immediate correction of the acres and rents assessed to them. Lord Overstone, for example, complained that "this list is so fearfully incorrect that it is impossible to correct it" (Bateman 1883: 348). Bateman's estimates, therefore, should be taken as a lower bound for the wealth of great landowners in late nineteenth-century Britain and Ireland.

In terms of coverage, Bateman's book includes 400 peers and 1,288 commoners in England and Wales (Bateman 1883, Appendix VI, p. 515). Hence, commoners represent three quarters of the great landowners listed in Bateman. That said, these 1,288 commoner great landowners own 8,497,690 acres. The 400 peers own 5,728,979 acres. In other words, commoners own 60 percent of the total acreage surveyed in Bateman.

For the sake of illustration, Quote A2 transcribes the entry for Charles George Lyttelton from Bateman (1883). He owned 6,939 acres scattered throughout his

[2] I exclude second-marriages, women married to foreigners, and members of the royal family.

estates in Worcestershire and Herefordshire. His gross annual rents from land exceeded £10,200 a year. Lyttelton attended Eton and Trinity College, Cambridge. He was a member of Brook's, a gentleman's liberal club, and sat in Parliament for Worcestershire. The letter 𝕾 denotes that he was the head of, or head of a junior branch of, a family that held land in England since the time of Henry VIII. He succeeded to the Lyttelton barony in 1876.

Quote A2: Charles Lyttelton, Bateman's *Great Landowners* (1883)

*** LYTTELTON, Lord, Hagley Hall, Stourbridge. 𝕾

| | | | | |
|---|---|---|---|---|
| *Coll.* Eton, Trin. Cam. | Worcester . . | 5,907 | . | 9,170 |
| *Club.* Brooks's. | Hereford . . | 1,032 | . | 1,093 |
| b. 1842, s. 1876, m. 1878. | | 6,939 | . | 10,263 |
| Sat for E. Worcestershire. | | | | |

Bateman (1883), p. 285

From this source, I constructed a new dataset on family landholdings in the peerage. Note that family landholdings refer to the landholdings of an individual's birth family. To construct this dataset, I followed three steps:

First, I digitized all 596 peers and peers' sons who appear both in Bateman's book and the Hollingsworth dataset. Second, I coded 353 of their wives' 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 landholdings for 227 couples in my baseline sample. Third, I searched the remaining spouses in the baseline sample, finding 97 additional couples. Overall, I match 324 out of 644 women in the baseline sample and their husbands.

## A. 3 Burke's *Heraldic dictionary*

I complement the Hollingsworth (2001) dataset with information on family seats, which are recorded in heraldic dictionaries. These dictionaries are summarized peerage records that contain additional information at the family level: religious affiliation, motto, coat of arms, and family seats. The most relevant source for my purposes is Burke's *Heraldic Dictionary* (1826). Many fathers of women in the baseline sample are recorded as owners of these family seats. Therefore, the family seats in Burke (1826) correspond, in general, to the seats where the women under analysis grew up and lived most of the year. Moreover, country seats were expensive to build and representative of long lineages. They generally remained in the hands of the same family generation after generation until the 1870s, when the aristocracy started to decline. Therefore, the seats in Burke (1826) can also be used to locate individuals born before or after this source was published. Quote A3 shows the entry in Burke (1826) for the Baron Lyttelton. The Lyttelton's family seat was Hagley Park, an eighteenth century house in Hagley, Worcestershire.

From this source, I gathered 694 seats for 498 peerage families who appear both in Burke's *Heraldic Dictionary* and in the Hollingsworth dataset. I then geo-

Quote A3: Lord Lyttelton, Burke's *Heraldic Dictionary* (1826)

> *Arms*—Quarterly: first, ar. a chev. between three escallops sa.; second, ar. a bend, cottised sa. within a bordure engr. gu. bezantée for Westcote; third, gu. a lion, rampant, within a bordure, engr. or, for Burley; fourth, France and England, quarterly, within a bordure, goboay, ar. and az. for Plantagenet [...].
> *Motto*—Ung Dieu, ung roy.
> *Seat*—Hagley Park, Worcestershire.
>
> Burke (1826), p. 115-116

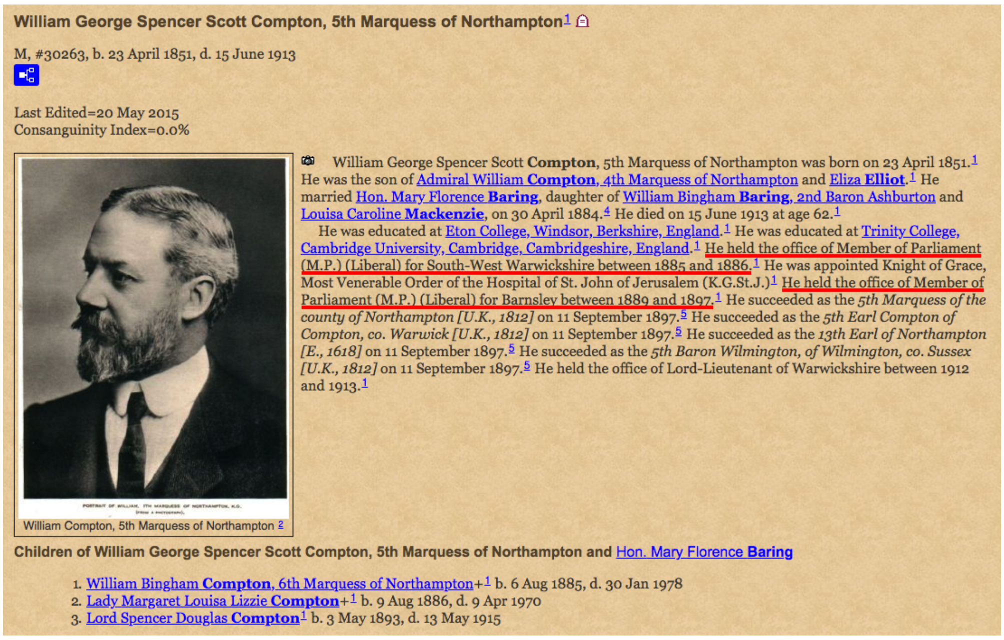
coded all seats using GeoHack. When a seat was not found, I used the coordinates from the nearest town, which sometimes was listed in Burke (1826). Note that some families owned more than one seat. For example, of the 279 women in the baseline sample with a family seat in England, two had 4 seats, 22 had 3 seats, 58 had 2 seats, and 197 had one seat.

## A. 4 Member of Parliament (MP) elections from *thepeerage.com*

I construct a new dataset on elections of Members of Parliament (MP) for the House of Commons. To do so, I use `thepeerage.com`. This website, run by Darryl Lundy, is a genealogical survey of the peerage of Britain, i.e., an online version of the peerage records described in Section A1. Specifically, the website provides biographies for all members of the peerage. These biographies state whether an individual was ever elected MP.

Since my aim is to evaluate whether a woman's marriage to a commoner affected her birth family's political power, I hand-collected 674 biographies of the fathers and the brothers of women in my baseline sample. Specifically, I consider the brothers and fathers of women aged 15 to 35 in 1861 who ever married and who have a family seat in England (N=279). As I explain in the paper, the sample is restricted to England because only there I have education provision data which I use in Section IV.B in the paper. I also collect the biographies of those who where family heads in the 1870s, when state education was introduced in England.

For the sake of illustration, Figure A4 shows the entry in `thepeerage.com` for one of the sampled brothers: William Compton, 5th Marquess of Northampton. Specifically, William was the second brother of Katrine Cecilia Compton (1845–1913), who was aged 15 at the start of the interruption, attended a fully-functioning Season afterwards, and married an Earl in 1870. Concomitantly, William's biography suggests that he had a brilliant political career. He was elected to the House of Commons at the 1885 general election as a Liberal MP for Stratford-on-Avon, a constituency formerly known as South-Warwickshire. This constituency is barely 7 miles away from William's family seat, Compton Vinyates. Hence, this election suggests that William (and his family) held some local political power, although admittedly he was MP for this constituency one year only. The biography also reports that he was elected MP for Barnsley, Yorkshire, in a by-election in 1889. He served there until 1897. Finally, between 1912 and



**William George Spencer Scott Compton, 5th Marquess of Northampton**[1]

M, #30263, b. 23 April 1851, d. 15 June 1913

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William Compton, 5th Marquess of Northampton [2]

William George Spencer Scott **Compton**, 5th Marquess of Northampton was born on 23 April 1851.[1] He was the son of Admiral William **Compton**, 4th Marquess of Northampton and Eliza **Elliot**.[1] He married Hon. Mary Florence **Baring**, daughter of William Bingham **Baring**, 2nd Baron Ashburton and Louisa Caroline **Mackenzie**, on 30 April 1884.[4] He died on 15 June 1913 at age 62.[1]

He was educated at Eton College, Windsor, Berkshire, England.[1] He was educated at Trinity College, Cambridge University, Cambridge, Cambridgeshire, England.[1] He held the office of Member of Parliament (M.P.) (Liberal) for South-West Warwickshire between 1885 and 1886.[1] He was appointed Knight of Grace, Most Venerable Order of the Hospital of St. John of Jerusalem (K.G.St.J.)[1] He held the office of Member of Parliament (M.P.) (Liberal) for Barnsley between 1889 and 1897.[1] He succeeded as the *5th Marquess of the county of Northampton [U.K., 1812]* on 11 September 1897.[5] He succeeded as the *5th Earl Compton of Compton, co. Warwick [U.K., 1812]* on 11 September 1897.[5] He succeeded as the *13th Earl of Northampton [E., 1618]* on 11 September 1897.[5] He succeeded as the *5th Baron Wilmington, of Wilmington, co. Sussex [U.K., 1812]* on 11 September 1897.[5] He held the office of Lord-Lieutenant of Warwickshire between 1912 and 1913.[1]

**Children of William George Spencer Scott Compton, 5th Marquess of Northampton and Hon. Mary Florence Baring**

1. William Bingham **Compton**, 6th Marquess of Northampton+[1] b. 6 Aug 1885, d. 30 Jan 1978
2. Lady Margaret Louisa Lizzie **Compton**+[1] b. 9 Aug 1886, d. 9 Apr 1970
3. Lord Spencer Douglas **Compton**[1] b. 3 May 1893, d. 13 May 1915

FIGURE A4: William Compton, 5th Marquess of Northampton, thepeerage.com

1913, William was Lord Lieutenant in Warwickshire—the county where his family seat is located. This reinforces the idea that him and his family had local political power around their seat. Unfortunately, thepeerage.com does not systematically provide the appointment dates for positions other than MP. Hence, I cannot use them in Section IV.A, where I test whether a family's political power was reduced *after* a woman's marriage to a commoner.

Using regular expressions, I identify whether an individual was elected MP, the year when he was elected, the constituency for which he sat, whether he was elected in the family seat's county, and how many years he served. Overall, my dataset spans 27 general elections and 97 by-elections between 1776 and 1910, contains information on 305 different MP elections, and covers 205 different constituencies.

## A. 5 Education data from Goni (2017)

To evaluate whether the Season and its implied sorting patterns affected public policies, I study the introduction of state education in England after Forster's 1870 Act. The data comes from the Reports of the Committee of Council on Education. These annual reports are "the most significant single source in existence for the study of elementary education, particularly on State interest in public education, during virtually the whole long reign of Victoria" (Stephens 1985). Importantly, the reports are suited for analysis at the regional and local level, since most of the evidence is broken down by counties and districts. Specifically, the reports detail the activities of School Boards—the local bodies in charge of raising taxes for state education.

For the sake of illustration, Figure A5 transcribes the 1877-78 report for School

Boards in Leicestershire. In all but three School Boards the main, sometimes the only, source of income were funds raised from rates (column 2). Specifically, rates were wealth taxes set by local School Boards in each Poor-law district and borough. The second source of income for School Boards were the grants from the Committee of Council on Education (column 1). These grants depended to a large extent on students' results in the reading, writing, and arithmetics national exams. The other sources of income were school fees (column 4) and books sold to students (column 5). Overall, School Boards had plenty of powers to decide how to spend all these funds: They built new state schools and paid teacher's salaries, school fees for the poorest children, and other state-school expenditures. They could also subsidize existing private schools (e.g., Voluntary schools run by the Church of England).

FIGURE A5: Report from the Committee of Council on Education, Leicestershire, 1877-78.

| School Board and County | 1. Grants from the Committee of Council on Education | | | 2a. Amount paid to Treasurer by Rating Authority | | | 2b Rate per £ on Rateable Value of District | 3. Contribution from Districts | | | 4. School Fees | | | 5. Amount received Books sold to Children | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| LEICESTER. | *£* | *s.* | *d.* | *£* | *s.* | *d.* | | *£* | *s.* | *d.* | *£* | *s.* | *d.* | *£* | *s.* | *d.* |
| *Leicester* | 2,684 | 0 | 10 | 4,500 | 0 | 0 | 3.25 | . . . | | | 2,166 | 4 | 8 | 112 | 14 | 7 |
| Anstey | 140 | 6 | 11 | 200 | 0 | 0 | 14.75 | . . . | | | 105 | 5 | 2 | . | . | . |
| Bagworth | – | – | – | – | – | – | – | – | | | – | – | – | – | – | – |
| Barrow-on-Soar | . | . | . | 20 | 0 | 0 | 0.5 | . . . | | | . | . | . | . | . | . |
| Buckminster and Sewstern, U.D. | . | . | . | 15 | 0 | 0 | 3 | . . . | | | . | . | . | . | . | . |
| Coston and Garthorpe, U.D. | 17 | 4 | 2 | 126 | 10 | 0 | 8 | . . . | | | 13 | 5 | 7 | | 5 | 11 |
| Desford | . | . | . | 121 | 1 | 0 | 5 | . . . | | | 32 | 8 | 8 | 2 | 3 | 1 |
| Dunton Basset | 64 | 6 | 0 | 63 | 0 | 0 | 5.5 | . . . | | | 38 | 14 | 0 | 1 | 9 | 4 |
| Easton Magna, U.D. | 17 | 5 | 8 | 320 | 0 | 0 | 11 | . . . | | | 23 | 5 | 0 | 1 | 13 | 5 |
| Foxton | 11 | 7 | 6 | 225 | 0 | 0 | 14.25 | . . . | | | 17 | 18 | 4 | 2 | 13 | 2 |
| Gaddesby | 32 | 15 | 0 | 65 | 0 | 0 | 5.75 | . . . | | | 22 | 5 | 4 | 1 | 19 | 9 |
| Great Dalby | 36 | 19 | 2 | 80 | 0 | 0 | 6.25 | . . . | | | 34 | 4 | 6 | 1 | 5 | 5 |
| Hinckley | 139 | 0 | 0 | 350 | 0 | 0 | 5 | . . . | | | 87 | 18 | 5 | 14 | 16 | 5 |
| Lockingtopn, U.D. | – | – | – | – | – | – | – | – | | | – | – | – | – | – | – |
| Loughborough | 168 | 4 | 8 | 500 | 0 | 0 | 3 | . . . | | | 179 | 1 | 0 | . | . | . |
| Nailstone | 34 | 12 | 0 | 80 | 0 | 0 | 3.25 | . . . | | | 33 | 10 | 1 | . | . | . |
| Oadby | 62 | 15 | 0 | 115 | 0 | 0 | 5.5 | . . . | | | 55 | 8 | 6 | . | . | . |
| Odstone and Barton-in-the Beans, U.D. | 30 | 2 | 3 | 21 | 14 | 0 | 1.5 | . . . | | | 48 | 1 | 4 | . | . | . |
| Peckleton | 23 | 10 | 2 | 55 | 0 | 0 | 3.75 | . . . | | | 12 | 8 | 4 | | 13 | 1 |
| Ratby | 100 | 13 | 6 | 217 | 10 | 0 | 6.5 | . . . | | | 48 | 18 | 0 | 5 | 0 | 5 |
| Seagrave | . | . | . | 80 | 0 | 0 | 4 | . . . | | | . | . | . | . | . | . |
| Somerby | . | . | . | 110 | 0 | 0 | 10 | . . . | | | 14 | 8 | 8 | . | . | . |
| Thornton | . | . | . | 52 | 15 | 0 | 3 | . . . | | | . | . | . | . | . | . |
| Thorpacre, U.D. | . | . | . | 14 | 0 | 9 | 0.5 | . . . | | | . | . | . | . | . | . |
| Upper and Nether Broughton, U.D. | . | . | . | 49 | 19 | 1 | 2 | . . . | | | 17 | 14 | 9 | | 14 | 6 |
| Walton-on-the-Wolds | 17 | 12 | 0 | 30 | 0 | 0 | 3 | . . . | | | 9 | 13 | 8 | . | . | . |
| Wigston Magna | 105 | 10 | 6 | 300 | 0 | 0 | 4 | . . . | | | 104 | 5 | 6 | . | . | . |

*Source:* Committee of Council on Education (1878), p. 144.

Goñi (2021) computerized the rate per £ (i.e., the tax rate) set by all 1,433 School Boards in England between 1872 and 1878 (column 2b). He also geo-referenced each School Board using GeoHack. In this paper, I evaluate education provision by 943 School Boards located in a 10-mile radius of 387 family seats in England, i.e., the family seats of women in the baseline sample. To measure local education provision, I compute the average tax rate set in a 10-mile radius of each family seat.

In addition, I hand-collected the data on total funds raised from taxes by each of these 943 School Boards (i.e., the data in column 2a in Figure A5). I use this to show that my results are similar when I measure state-education provision with tax rates and with the total funds raised from taxes (see online appendix B11).

### A. 6 Descriptive statistics

Here I present summary statistics of the main variables. First, I describe the baseline sample and provide descriptives for Section III. Second, I describe the family seats data. Third, I present the variables used to test whether the interruption of the Season affected the political power of the peerage (Section IV.A). Finally, I focus on the education-provision variables used in Section IV.B.

**Section III descriptives.** Throughout the paper, my baseline sample are all peers' daughters who ever married and who were aged 15 to 35 in 1861, when the Season's interruption began. This includes women who could potentially have married during the interruption, although with different risks, determined by their age. Specifically, Section III, uses three samples:

- *Baseline sample* (N=644). This sample includes women aged 15-35 in 1861 who ever married. It excludes second marriages, women marrying foreigners, and members of the royal family.[3]
- *Women married in the landed elite* (N=324). This sample includes women in the baseline sample for which Bateman (1883) lists both spouses' family landholdings,[4] and
- *Married and unmarried women* (N=765). To evaluate the effect of the Season on celibacy, this sample includes women in the baseline sample and women aged 15-35 in 1861 who never married. To avoid counting women who died at an early age as celibate, I exclude women dying before age 35.[5]

The sample size corresponds to what is infered from the rate of convertion of invitations to royal parties into marriages. Between 1851 and 1875, 116,030 invitations for royal parties were issued and accepted. Concomitantly, there was a total of 796 marriages in which the wife was a peer's daughter. This implies that every 150 invitations were converted into one marriage. Note that the baseline sample includes women born over 20-years. On average, one would expect 4,641 invitations per year, amounting to a total of 92,820 invitations over a 20-years period. Given that every marriage is associated to around 150 invitations, we would expect to see around 620 marriages (31 per year).

Table A1 presents summary statistics for these three samples. The *baseline sample* descriptives suggest that peers' daughters aspired to marry peers' sons, and vice versa. Only 65 percent of women married a commoner. Although this number may seem large at first sight, note that the peerage was an extremely small group. Around 1900, only one in 3,200 people in Britain (0.03% of the population)

[3]See Tables 2 (Panel A, cols. [1], [2]); 3 (Panel A, cols. [1], [3]); 4; and 5; and Figure 7
[4]See Tables 2 (Panel A, cols. [3] to [5]); 3 (Panel A, cols. [5], [7], and [9]); and Figure 8
[5]See Table 2, Panel A (col. [6]).

was an aristocrat; in Europe, the proportion was one in 100 (Beckett 1986: 35-40). Hence, a 35% rate of endogamy within this very small group is actually a large figure. Similarly, 26% of peer's daughters in this sample married the heir to a peerage. Given that in Britain only heirs inherited titles, this was an important margin of marriage quality.

TABLE A1: Summary statistics

*Baseline sample*: women aged 15-35 in 1861 who ever married

| | Baseline | | Married in landed elite | | Including unmarried | |
|---|---|---|---|---|---|---|
| | mean | sd | mean | sd | mean | sd |
| Married a commoner | 0.65 | 0.48 | 0.47 | 0.5 | . | . |
| Married an heir | 0.26 | 0.44 | 0.41 | 0.49 | . | . |
| Difference in spouses' landholdings: | | | | | | |
| in percentile rank (absolute value) | . | . | 28.90 | 21.64 | . | . |
| in percentile rank (husband − wife) | . | . | -3.32 | 35.98 | . | . |
| wife married down | . | . | 0.53 | 0.5 | . | . |
| Celibacy | . | . | . | . | 0.23 | 0.42 |
| Treatment (synthetic prob.)† | 11.1% | 7.29 | 12.0% | 7.16 | 11.3% | 7.21 |
| Married in 1861–63 | 0.14 | 0.35 | 0.15 | 0.36 | . | . |
| Duke's, marquis', earl's daughter | 0.49 | 0.50 | 0.61 | 0.49 | 0.46 | 0.50 |
| Viscount's, baron's daughter | 0.51 | 0.50 | 0.39 | 0.49 | 0.54 | 0.50 |
| Birth order (excluding heirs) | 3.69 | 2.66 | 3.79 | 2.76 | 3.87 | 2.73 |
| Peerage of England | 0.59 | 0.49 | 0.57 | 0.50 | 0.58 | 0.49 |
| Peerage of Scotland | 0.11 | 0.31 | 0.11 | 0.32 | 0.11 | 0.31 |
| Peerage of Ireland | 0.30 | 0.46 | 0.31 | 0.46 | 0.31 | 0.46 |
| Observations | 644 | | 324 | | 765 | |

† *synthetic prob. (%) to marry during Season interruption, based on marriage probs. in "normal times."*

*Notes*: The baseline sample excludes second marriages, women marrying foreigners, and members of the royal family. Unmarried women excludes those dead before age 30.

In the paper, I test whether sorting was affected by the interruption of the Season (1861–63). My treatment variable, hence, captures a woman's exposure to the interruption. Specifically, the treatment variable is the synthetic probability to marry in 1861–63, given a woman's age in 1861. This synthetic probability is based on the percentage of women marrying at each age in "normal times" (i.e., before the interruption). In the baseline sample, the average synthetic probability to marry in 1861–63 is 11.1 percent, and has a standard deviation of 7.29. Note that the synthetic probability is a good predictor of the actual percentage of women married in 1861–63: 14 percent. This suggests that social norms regarding age at marriage were not altered during the interruption. In other words, women did not defer marriage decisions until the Season resumed.

I also report summary statistics for my control variables: half of the women in the baseline sample are dukes', marquis', or earls' daughters (vs. viscounts' or barons' daughters). Most came from a family with an English peerage (60%), followed by Irish (30%) and Scottish (11%) peerages. Finally, the average daughter was the 3rd to 4th child (excluding heirs).

Next, I restrict the sample to *women married in the landed elite*, i.e. marriages for which Bateman (1883) lists both spouses' family landholdings. Note that most of the lost observations (81 percent) correspond to women marrying landless commoners. This selected sample allows me to evaluate whether the Season's interruption also affected sorting within the landed elite, that is, at the top of the distribution. Being a representative sample for the top of the distribution, some covariates are different to the ones described above. Specifically, there are more dukes', marquis', or earls' daughters than viscounts' or barons' daughters. That said, other covariates, e.g., birth order and the peerage of origin, are not significantly affected. Most importantly, my treatment variable, i.e., the synthetic probability to marry in 1861–63, does not change substantially in this selected sample. Neither does the proportion of women who married in 1861–63. Altogether, this suggests that the interruption of the Season affected women in this selected sample similarly to women in the baseline sample.

To measure sorting by landholdings, I use three variables based on the difference between spouses' *family* landholdings. Why do I use family landholdings? Under strict primogeniture, marrying a spouse from a family with few landholdings would only matter if he was the heir. However, as discussed in Section I, the aristocrats' inheritance system granted younger brothers (and sisters) a yearly allowance proportional to the size of the family estates. Hence, marrying a non-heir from a family with few landholdings implied an economic loss.

Specifically, the first measure of sorting by landholdings that I use 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. On average, there is a 28.9 percentile rank difference in my sample. Next, I look at the difference between husband's minus wife's family landholdings, in percentile ranks. On average, women married husbands 3 percentile ranks poorer.

Finally, the table also reports summary statistics for a third sample *including women who never married*. I use this sample to evaluate whether the interruption of the Season affected marital rates. On average, celibacy is around 23 percent. Note that this excludes women who married but died before age 35 (including them reduces celibacy, but only by one percentage point). Moreover, the treatment variable, i.e., the synthetic probability to marry in 1861–63, is similar to the other two samples. The remaining covariates are almost identical to the baseline sample of married women, suggesting that selection is not a major issue.

**Family seats descriptives.** I complement the data described in Table A1 with information on 694 seats for 498 peerage families from Burke (1826). Figure A6 shows the geographical location of family seats. The peerage was dispersed over the British Isles and seats were quite isolated from each other. The majority of seats were located in England, especially in the South East region.

I use this geo-referenced information in two ways. First, I use it to evaluate local education provision around peer's family seats. Light green circles in Figure A6 highlight the seats used to evaluate education provision in Section IV.B. Overall, these seats also cover most of the territory, in this case, England. The sample is restricted to England because only there I have education-provision data. More descriptives are provided below.

Second, I use the geo-referenced data on family seats to construct a covariate

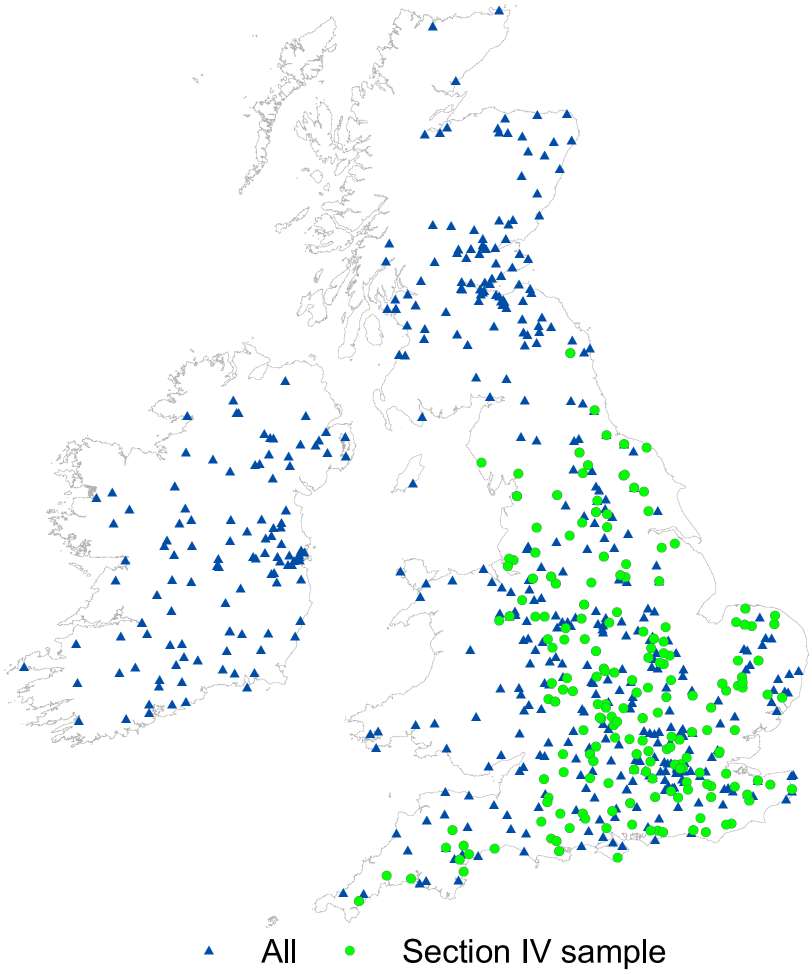



FIGURE A6: COUNTRY SEATS. The sample is 694 country seats from 498 peerage families listed in Burke's *Heraldic Dictionary*. Green circles highlight the sample used in Section 5: seats (in England) of women aged 15–35 in 1861.

measuring the distance between each seat and London. This covariate is important because attending the Season may have been more costly for women living further away. Although families usually rented a house in London for the entire Season, some stayed in their country estates and travelled there for specific events. Specifically, I measure the shortest aerial distance between a woman's family seat(s) and London, in miles. When a woman has more than one family seat, I take the minimum distance.

Unfortunately, including this covariate mechanically restricts the sample to women with family seats recorded in Burke (1826). Specifically, I have information on family seats for 484 observations in the *baseline sample* (75%), 260 observations in the *women married in the landed elite sample* (80%), and 565 observations in the *married and unmarried women sample* (74%). Note that the % success rate in matching observations is similar across samples, suggesting that this sample restriction will not entail selection issues.

Table A2 provides summary statistics for women with at least one family seat recorded in Burke (1826). They are very similar to the baseline sample described before. For example, there was a similar proportion marrying commoners (0.62 vs. 0.65 before) and heirs (0.27 vs. 0.26 before). The difference between spouses' landholdings is similar to the one reported before, both in absolute value (29.61 vs. 28.9 before) and in terms of husband's minus wife's landholdings (-4.86 vs. -3.32 before). The celibacy rate is also as high as before (0.22 vs. 0.23 before). Importantly, the treatment variable and the proportion of women marrying in 1861–63 is the same here to the baseline sample, suggesting that the Season's interruption affected women with and without a recorded family seat similarly. The only significant difference is that dukes', marquis', and earls' daughters are more likely to have a recorded family seat.

On average, women with a recorded family seat lived 183–187 miles away from London. As illustrated in Figure A6, this suggests that the family seats of the peerage were dispersed over Britain. Importantly, the distance to London is similar across the three samples (baseline; married in the landed elite; and including unmarried). In other words, none of these sample choices seems to alter the geographical distribution of my observations.

TABLE A2: Summary statistics, women with a family seat

*Sample*: women with a recorded family seat

| | Baseline | | Married in landed elite | | Including unmarried | |
|---|---|---|---|---|---|---|
| | mean | sd | mean | sd | mean | sd |
| Married a commoner | 0.62 | 0.49 | 0.46 | 0.50 | . | . |
| Married an heir | 0.27 | 0.45 | 0.40 | 0.49 | . | . |
| Difference in spouses' landholdings: | | | | | | |
| in percentile rank (absolute value) | . | . | 29.61 | 22.21 | . | . |
| in percentile rank (husb. − wife) | . | . | -4.86 | 36.74 | . | . |
| wife married down | . | . | 0.55 | 0.5 | . | . |
| Celibacy | . | . | . | . | 0.22 | 0.41 |
| Treatment (synthetic prob.)† | 11.1% | 7.28 | 12.1% | 7.22 | 11.3% | 7.19 |
| Married in 1861–63 | 0.14 | 0.35 | 0.13 | 0.34 | . | . |
| Distance to London (in miles) | 187.8 | 139.8 | 186.3 | 138.4 | 183.6 | 140.6 |
| Duke's, marquis', earl's daughter | 0.60 | 0.49 | 0.68 | 0.47 | 0.57 | 0.50 |
| Viscount's, baron's daughter | 0.40 | 0.49 | 0.32 | 0.47 | 0.43 | 0.50 |
| Birth order (excluding heirs) | 3.74 | 2.70 | 3.76 | 2.79 | 3.98 | 2.81 |
| Peerage of England | 0.53 | 0.50 | 0.52 | 0.50 | 0.52 | 0.50 |
| Peerage of Scotland | 0.14 | 0.34 | 0.14 | 0.35 | 0.13 | 0.33 |
| Peerage of Ireland | 0.33 | 0.47 | 0.34 | 0.48 | 0.35 | 0.48 |
| Observations | 484 | | 260 | | 565 | |

† *synthetic prob. (%) to marry during Season interruption, based on marriage probs. in "normal times."*

*Notes:* All samples are restricted to women with at least one family seat recorded in (Burke 1826) The baseline sample are all peers' daughters aged 15 to 35 in 1861 who ever married. It excludes second marriages, women marrying foreigners, and members of the royal family. Married in the landed elite includes women in the baseline sample whose family landholdings and whose husband's family landholdings are listed in Bateman (1883). Including unmarried consists of women in the baseline sample and women aged 15-35 in 1861 who never married. It excludes those who died before age 30.

**Descriptives political power.** In Section IV.A in the paper, I test whether the Season's interruption and the increase in women's marriages to commoners affected the political power of the peerage. Specifically, I collect data on MP elections from thepeerage.com and look at whether the blood relatives of women marrying a commoner were less likely to be elected MP in the House of Commons. Here I present descriptive statistics for these variables.

I consider women in my baseline sample with a family seat in England. The sample is restricted to England because only there I have education-provision data for Section IV.B. My first political variable indicates, whether any brother of a sampled woman was elected MP after her marriage. Second, I look at how many years these brothers served altogether. Third, I assess whether they were elected MP in the family seat's constituency. This proxies for an individual's local political power, which could affect policies implemented locally—e.g., the introduction of state education (Stephens 1998). Finally, I also evaluate the political power of the head of a woman's birth family (henceforth, family head). Specifically, I assess whether the person who was the family head in the 1870s—when state education was introduced—was elected MP and how many years he served.

Table A3 presents the summary statistics. The average woman had three brothers. After her marriage, 40 percent of women had at least one brother elected MP, 20 percent had one brother elected MP in the family seat's county, and 18 percent had a family head who served as MP. Altogether, their brothers served 4.76 years as MP and 1.84 years as MP in the family seat's county. Similarly, after a woman's marriage, her family head served on average 1.39 years as MP. Note that the number of observations is 270 for all variables based on the MP elections of brothers. This is because 9 women had no brothers.

Women in this sample are similar to women with at least one family seat. Specifically, the proportion of women marrying a commoner, the proportion of dukes', marquis', and earls' families, and birth order are similar. Logically, the distance between family seats and London is lower than before, as now the sample excludes women with a seat in Wales, Ireland, and Scotland. That is, it excludes women living further away from London. The average synthetic probability to marry in 1861–63 is now lower. That said, the sample provides enough women with high exposure to the Season to identify its effects. For example, the percentage of women aged 19 to 22 in 1861—that is, the most exposed cohorts—is 18.78 here vs. 19.71 in the baseline sample.

My regressions also include a covariate capturing a family's previous political power. This is defined as the political variables described above, but considering only the MP elections of a woman's father before her marriage. Note that using the MP elections of her brothers before her marriage would understate a family's previous political power, e.g., for women with brothers under age 21 who were not eligible for MP elections. This covariate controls for the unlikely possibility that, as Parliament met in 1861–63, the daughters of MPs moved to London with them and attended private balls during the interruption (see Section III.A). The descriptive statistics suggest that fathers held slightly more political power (before a woman's marriage) than brothers did (after a woman's marriage). As suggested by Figure A7, though, this secular trend is due to the loss of political power after the Great Reform Act of 1832, and not closer to the period under study—i.e., the interruption of the Season and education provision in the 1870s.

The table also provides descriptive statistics for the county characteristics used

Table A3: Summary statistics for marriage and political power

| *Sample:* Women in the baseline sample with a family seat in England | mean | sd | min | max | N | source |
|---|---|---|---|---|---|---|
| Family's political power after woman's marriage: | | | | | | |
| Any brother is MP | 0.42 | 0.49 | 0 | 1 | 270 | thepeerage.com |
| All brother's MP years | 4.76 | 8.61 | 0 | 65 | 270 | thepeerage.com |
| Any brother is local MP | 0.23 | 0.42 | 0 | 1 | 270 | thepeerage.com |
| All brother's local MP years | 1.84 | 5.30 | 0 | 51 | 270 | thepeerage.com |
| Family head is MP‡ | 0.18 | 0.38 | 0 | 1 | 279 | thepeerage.com |
| Family head's MP years‡ | 1.39 | 3.90 | 0 | 22 | 279 | thepeerage.com |
| Married a commoner | 0.59 | 0.49 | 0 | 1 | 279 | Hollingsworth |
| Treatment (synthetic prob.)† | 11.38% | 7.23 | 1.57 | 22.83 | 279 | Hollingsworth |
| Controls: | | | | | | |
| Duke, marquis, earl's family | 0.58 | 0.49 | 0 | 1 | 279 | Hollingsworth |
| Viscount, baron's family | 0.42 | 0.49 | 0 | 1 | 279 | Hollingsworth |
| Number of brothers | 3.34 | 1.84 | 1 | 10 | 270 | Hollingsworth |
| Woman's birth order (excl. heirs) | 3.71 | 2.67 | 1 | 14 | 279 | Hollingsworth |
| Distance to London (miles) | 92.15 | 56.06 | 3.82 | 239.6 | 279 | Burke |
| Family's political power before woman's marriage: | | | | | | |
| Father is MP | 0.44 | 0.50 | 0 | 1 | 270 | thepeerage.com |
| Father's MP years | 5.56 | 8.63 | 0 | 50 | 270 | thepeerage.com |
| Father is local MP | 0.29 | 0.46 | 0 | 1 | 270 | thepeerage.com |
| Father's local MP years | 2.90 | 6.94 | 0 | 50 | 270 | thepeerage.com |
| Family head is MP‡ | 0.22 | 0.42 | 0 | 1 | 279 | thepeerage.com |
| Family head's MP years‡ | 2.62 | 5.74 | 0 | 37 | 279 | thepeerage.com |
| County controls: | | | | | | |
| % working in manufacturing | 54.3 | 14.0 | 33 | 86 | 39 | Hechter (1976) |
| income p.c. (in logs) | 2.7 | 0.3 | 2.24 | 3.30 | 39 | Hechter (1976) |
| % voting conservative, 1885 | 47.0 | 7.7 | 32 | 67 | 39 | Hechter (1976) |
| % non-conformists | 13.7 | 5.7 | 4 | 36 | 39 | Hechter (1976) |
| religiosity | 0.87 | 0.07 | 0.69 | 0.95 | 39 | Hechter (1976) |

† *synthetic prob. (%) to marry during Season interruption, based on marriage probs. in "normal times."*
‡ *refers to the person who was family head in the 1870s.*

*Notes:* The baseline sample are all peers' daughters aged 15 to 35 in 1861 who ever married. This table restricts the baseline sample to women with at least one recorded family seat in England (Burke 1826).

in the regression analysis. I consider the proportion of people working in manufacturing in each county, log income per capita (p.c.), the percentage voting conservative in the general elections of 1885, the percentage of non-conformists, and religiosity. These covariates are from Hechter (1976) and cover 39 counties in England. There is substantial variation in these covariates across counties. For example, Durham employed 86 percent of the workers in manufacturing in the 1870s, at a time when only 33 percent worked in manufacturing in Rutland. Kent was the richest county in per capita terms. The percentage of non-conformists, the people voting conservative, and religiosity also varied across England.

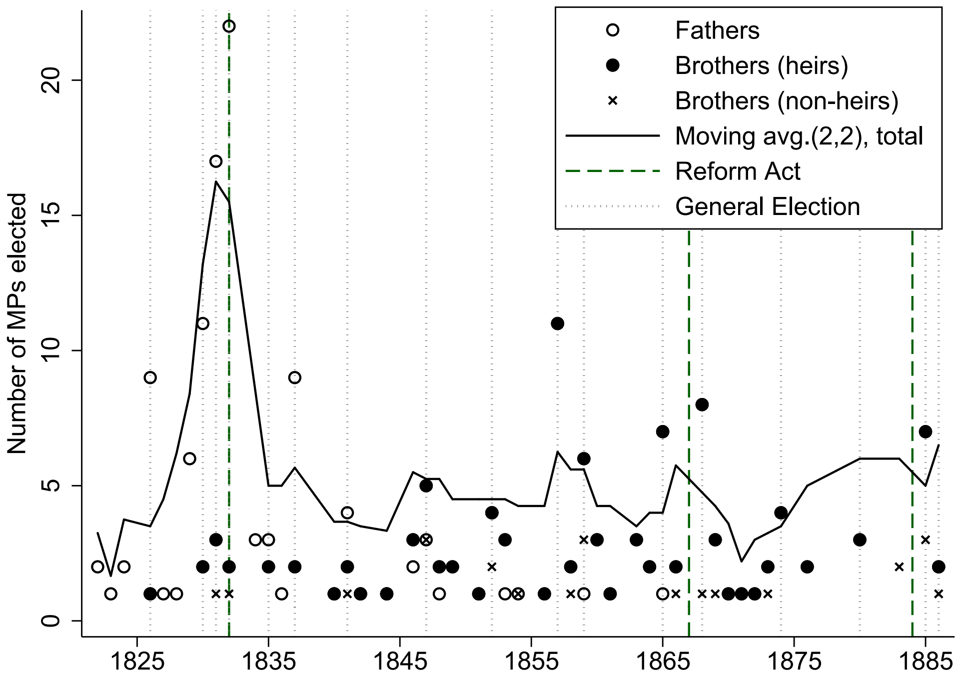



FIGURE A7: MP ELECTIONS OVERTIME. The sample are the brothers and fathers of women in my baseline sample. I exclude families with no seat in England and, for illustration, show only the 1825–1885 period. Lines date Reform Acts (1832, 1867, 1884) and general elections. MPs in between are from by-elections.

Figure A7 and Table A4 provide fine-grain descriptive statistics for the MP elections in my sample. Table A4 shows that, on average, family seats in my baseline sample were only 8.27 miles from an enfranchised constituency's centroid (std. dev. 5.14). The Table also summarizes the MP elections by type of constituency. Overall, my dataset covers 305 MP elections for 205 different constituencies. Out of these, 159 elections are for brothers and 146 for fathers of the sampled women (i.e., women in the baseline sample with a seat in England). Between 90 to 95% of the MPs were elected for county and borough constituencies. Few of the MP elections correspond to rotten boroughs. This is important, as rotten boroughs had a very small electorate, and hence, may be unrepresentative of the MP's local political power—although they certainly provided political power in the House of Commons. Note also the most of the MPs for rotten boroughs are fathers. The reason is that rotten boroughs were abolished in the 1832 Reform Act, before most of the brothers of women in my baseline sample (i.e., aged 15-35 in 1861) were born or had reached the age of majority. Specifically, only one brother in my dataset was elected for any rotten borough: John Ponsonby, 5th Earl of Bessborough. He was elected for Bletchingley and High Ferrers in 1831, before his sister's marriage in 1858. In other words, my variables capturing a family's political power *after* a woman's marriage do not incorporate MP elections in rotten boroughs.

Table A4: MP elections, by type of constituency

**Panel A.** Distance from family seat to closest enfranchised constituency:

| | mean | std. dev. | max |
|---|---|---|---|
| Distance (in miles) | 8.27 | 5.14 | 25.37 |

**Panel B.** MP elections, by type of constituency:

| | all | fathers | brothers |
|---|---|---|---|
| County constituency (%) | 47.5 | 40.0 | 54.4 |
| Borough constituency (%) | 44.3 | 46.9 | 41.9 |
| District of Burghs constituency (%) | 2.0 | 1.4 | 2.5 |
| Cinque Port constituency (%) | 0.3 | 0.7 | 0.0 |
| University constituency (%) | 0.3 | 0.7 | 0.0 |
| Rotten borough (%) | 5.6 | 10.3 | 1.3 |
| Total number of MP elections | 305 | 145 | 160 |

*Notes:* Panel A considers distance to *enfranchised* constituencies; i.e., excludes rotten boroughs. I geo-reference each constituency with: (i) its centroid when provided in wikipedia; (ii) its largest urban center. Panel B shows the number of MP elections of fathers and brothers of women in the baseline sample (i.e., aged 15-35 in 1861) with a seat in England.

Finally, Figure A8 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 in Section IV.A 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 twenty years after it, however, the number of MPs was much lower for families in which a woman married a commoner.

**Descriptives education provision.** In Section IV.B in the paper, I evaluate the impact on public policy of the increase in women's marriages to commoners and the loss of political power associated with the Season's interruption. Specifically, I look at the introduction of state education documented by the Reports of the Committee of Council on Education. Here I provide descriptive statistics on the main variables used in the analysis.

Figure A9 maps the location of all the 1,433 School Boards operating in England between 1872 and 1878. The circles are proportional to the tax rate set by each School Board. The figure suggests that School Boards spread over England. There is substantial variation in tax rates within smaller geographic units, although in general it seems that School Boards in the south levied larger taxes. For example, in Queenborough—a small town in Kent—rates were, on average, at 14 percent between 1872 and 1878. At the same time, in Forton, Lancashire, taxes were at 0.2 percent of the rateable value. Highlighted in green is the sample of 943

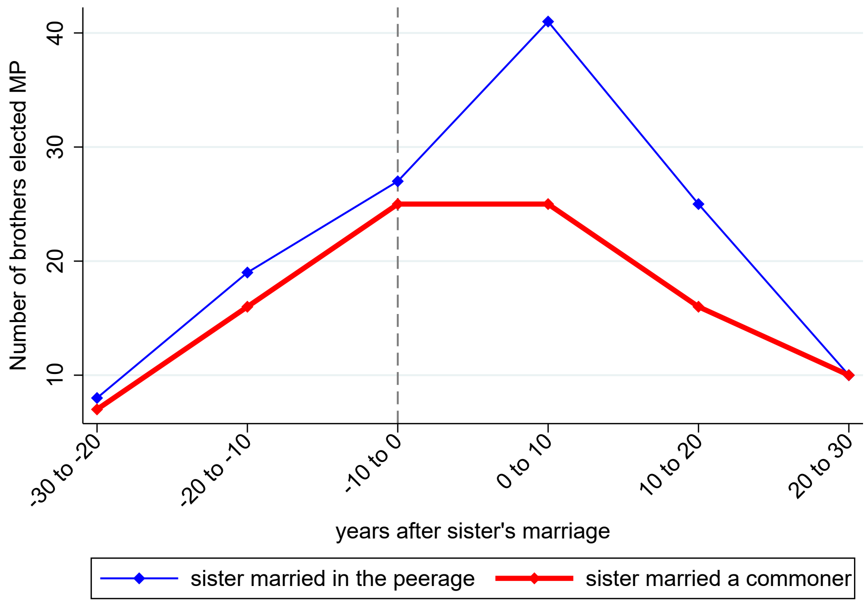



Figure A8: Marriage and political power. The sample are women in the baseline sample (i.e., aged 15-35 in 1861) and their brothers. I exclude families without a seat in England. Lines are the total number of brothers elected Members of Parliament (MP) before and after their sister's marriage. The thin blue line (thick red line) is for those whose sister married in the peerage (married a commoner).

School Boards used in Section IV.B. These are the School Boards within 10-miles of the family seats (in England) of women in my baseline sample. In terms of geographical location and tax rates, these School Boards are representative for all England.

Finally, Table A5 provides summary statistics for the variables used in Section IV.B in the paper. Since education was provided locally, the unit of analysis is a family seat (and the area around it). Specifically, I consider 387 family seats in England of women in my baseline sample—i.e., aged 15-35 in 1861 who ever married. Note that, since some families owned more than one seat, the number of observations is larger than before. Of the 279 women in the baseline sample with a family seat in England, two had 4 seats, 22 had 3 seats, 58 had 2 seats, and 197 had one seat. Hence, the total number of seats is 387.

My measure of education provision is the average tax rate set by all School Boards within a 10-miles radius of each seat. Within a 10-miles radius of the average family seat, there were 8 School Boards and tax rates were 2.3%. The standard deviation of tax rates is 1.08, suggesting that there was substantial variation across England. In online appendix B11 I consider the total funds raised from taxes (instead of the tax rate). The average School Board in my sample raised £159, which were then invested in building and running state schools, subsidizing private schools, paying the fees for the poorest children to attend school, etc.[6]

[6]This figure excludes School Board in large cities, which raised substantially larger funds

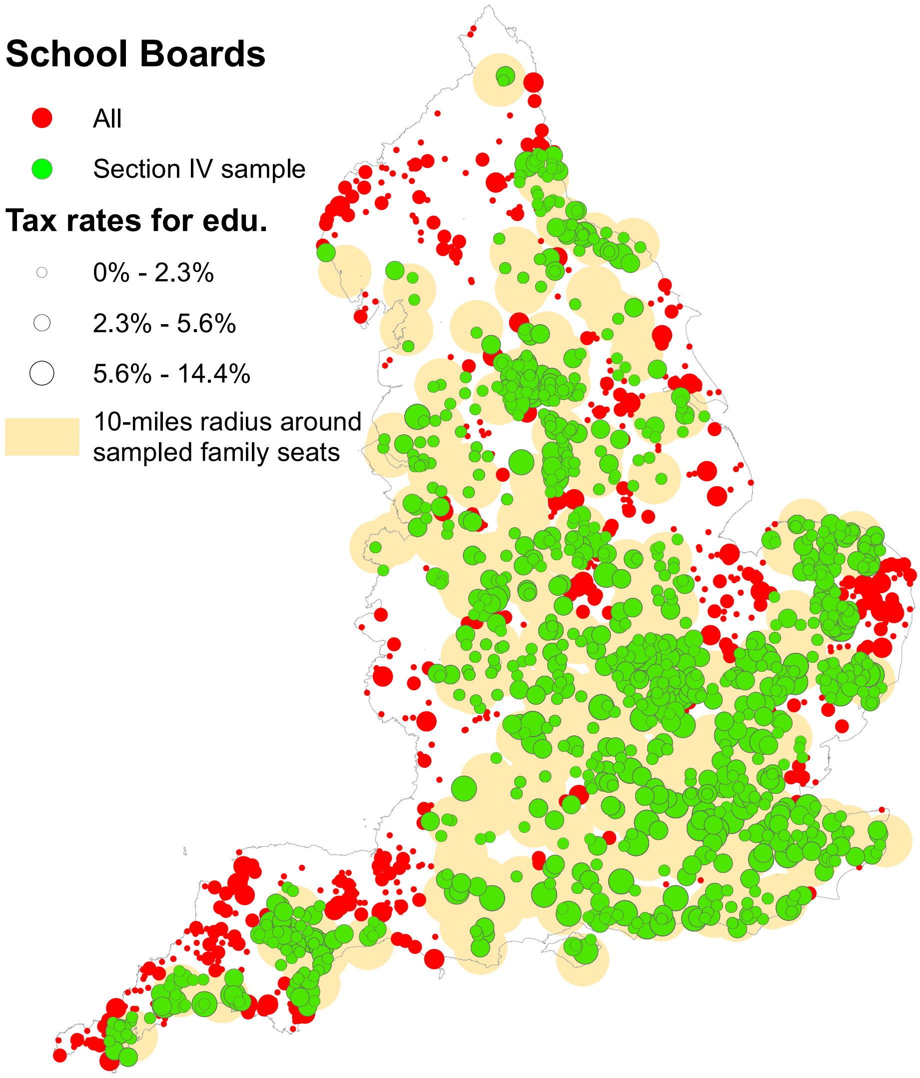



FIGURE A9: SCHOOL BOARDS. Red: 1,433 School Boards in England (1872–1878). Green: sample used in Section 5, that is, School Boards within a 10-miles radius of the family seats (in England) of women in the baseline sample.

The measures capturing the political power in each family seat have almost identical means and standard deviations as before. Forty percent of the sampled family seats had at least one brother elected MP after his sister's marriage, 17 percent had one brother elected MP in the family seat's county, and 17 percent had a family head who served as MP. In the average family seat, these brothers all together served 5 years as MP and 1.68 years as MP in the family seat's county. Similarly, in the average seat, the family head served 1.27 years as MP after a woman's marriage. Note that the number of observations is lower for all variables based on the MP elections of brothers. The reason is that in 17 seats there were only women, i.e., they had no brothers. In addition, the variables capturing the political power in each family seat *before* a woman's marriage are also similar to the ones described above.

than rural School Boards.

Table A5: Summary statistics for determinants of education provision

*Sample*: Family seats in England (and 10-miles area around it) of women in the baseline sample

| | mean | sd | min | max | N | source |
|---|---|---|---|---|---|---|
| Tax rate in 1872–78 (% average) | 2.30 | 1.08 | 0.2 | 7 | 387 | RCCE |
| Funds raised from taxes (£) | 159.0 | 137.7 | 26.4 | 1,226.4 | 387 | RCCE |
| Number of School Boards | 8.41 | 5.33 | 1 | 34 | 387 | RCCE |
| Political power in family seat after woman's marriage: | | | | | | |
| Any brother is MP | 0.43 | 0.50 | 0 | 1 | 374 | thepeerage.com |
| All brother's MP years | 5.03 | 8.92 | 0 | 65 | 374 | thepeerage.com |
| Any brother is local MP | 0.17 | 0.37 | 0 | 1 | 374 | thepeerage.com |
| All brother's local MP years | 1.68 | 5.22 | 0 | 51 | 374 | thepeerage.com |
| Family head is MP‡ | 0.17 | 0.37 | 0 | 1 | 387 | thepeerage.com |
| Family head's MP years‡ | 1.27 | 3.77 | 0 | 22 | 387 | thepeerage.com |
| Woman in seat married a commoner | 0.57 | 0.50 | 0 | 1 | 387 | Hollingsworth |
| Treatment (synthetic prob.)† | 11.51% | 7.15 | 1.57 | 22.83 | 387 | Hollingsworth |
| Controls: | | | | | | |
| Duke, marquis, earl's family | 0.61 | 0.49 | 0 | 1 | 387 | Hollingsworth |
| Viscount, baron's family | 0.39 | 0.49 | 0 | 1 | 387 | Hollingsworth |
| Number of brothers | 3.24 | 1.77 | 1 | 10 | 374 | Hollingsworth |
| Woman's birth order (excl. heirs) | 3.67 | 2.65 | 1 | 14 | 387 | Hollingsworth |
| Distance to London (miles) | 95.22 | 61.21 | 3.82 | 279.5 | 387 | Burke |
| Political power in family seat before woman's marriage: | | | | | | |
| Father is MP | 0.45 | 0.50 | 0 | 1 | 374 | thepeerage.com |
| Father's MP years | 5.94 | 8.46 | 0 | 50 | 374 | thepeerage.com |
| Father is local MP | 0.24 | 0.42 | 0 | 1 | 374 | thepeerage.com |
| Father's local MP years | 2.63 | 6.53 | 0 | 50 | 374 | thepeerage.com |
| Family head is MP‡ | 0.23 | 0.42 | 0 | 1 | 387 | thepeerage.com |
| Family head's MP years‡ | 2.80 | 5.89 | 0 | 37 | 387 | thepeerage.com |
| County controls (see Table A3) | | | | | | |

† *synthetic prob. (%) to marry during Season interruption, based on marriage probs. in "normal times."*
‡ *refers to the person who was family head in the 1870s.*

*Notes:* The sample are the family seats in England of women in the baseline sample (i.e., all peers' daughters aged 15 to 35 in 1861 who ever married). RCCE stands for Reports of the Committee of Council on Education.

As for marriage patterns, in 57 percent of the family seats one woman married a commoner. In the average seat, a woman's synthetic probability to marry in 1861–63 was 11.51%. These means are similar to the ones reported for the baseline sample, especially when restricted to women with a recorded family seat.

Regarding the control variables, the proportion of dukes', marquis', and earls' families, the number of brothers, women's birth order, and the distance to London is consistent with the statistics in Table A3.[7] Finally, my regressions also control for the set of county characteristics described in Table A3.

Altogether, the summary statistics presented here are similar to those in the baseline sample and to those in the sample of women with a seat in England. This suggests that treating a family seat as the unit of observation should not alter my main results significantly.

[7]Note that the maximum distance between a family seat and London is now larger. The reason is that now I treat each seat as a different observation. Before, when a woman had more than one family seat I took the minimum distance to London.

## Appendix B. Robustness

This appendix provides the following robustness checks:



### B. 1 Synthetic probability based on alternative benchmark cohorts.

This appendix reports estimates of equations (2) and (3) using alternative definitions of my treatment, that is, the synthetic probability to marry during the interruption of the Season (1861–63).

Specifically, a woman's synthetic probability to marry during the interruption is based on (i) her age in 1861–63 and (ii) the probability to marry at each age in a benchmark cohort who married in normal times. Here I define the treatment using six different benchmark cohorts:

*(a)* Peers' daughters born in 1815–30 (baseline definition);

*(b)* Peers' daughters born in 1810–25;

*(c)* Peers' daughters born in 1820–35;

*(d)* Peers' daughters married in 1845–60;

*(e)* Peers' daughters married in 1840–55;

*(f)* Peers' daughters married in 1835–50.

As in the main text, all benchmark cohorts exclude women married after 1861 and dead before age 30. Table B1 includes the baseline set of controls and Table B2 adds the distance between the family seat and London (hence, it restricts the sample to women with a recorded family seat).

Table B1: Synthetic probability based on alternative benchmark cohorts.

| | Married a Commoner [1] | Married an heir [2] | Spouses' landholdings (rank percentile): Difference (abs. value) [3] | Spouses' landholdings (rank percentile): Difference (husb-wife) [4] | Spouses' landholdings (rank percentile): Married down [5] | Never married [6] |
|---|---|---|---|---|---|---|
| **A**. *Baseline Treatment: synthetic probability to marry in 1861–63 based on women born in 1815-30* | | | | | | |
| Treatment | 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] |
| **B**. *Alternative Treatment: synthetic probability to marry in 1861–63 based on women born in 1810–25* | | | | | | |
| Treatment | 0.005** | -0.004* | 0.576** | -0.655*** | 0.011*** | 0.002 |
| | (0.002) | (0.002) | (0.236) | (0.214) | (0.003) | (0.002) |
| | [0.059] | [0.071] | [0.035] | [0.008] | [0.004] | [0.263] |
| **C**. *Alternative Treatment: synthetic probability to marry in 1861–63 based on women born in 1820–35* | | | | | | |
| Treatment | 0.004** | -0.003** | 0.427** | -0.460** | 0.008*** | 0.002 |
| | (0.002) | (0.001) | (0.176) | (0.178) | (0.002) | (0.002) |
| | [0.030] | [0.035] | [0.037] | [0.018] | [0.005] | [0.280] |
| **D**. *Alternative Treatment: synthetic probability to marry in 1861–63 based on women married in 1845–60* | | | | | | |
| Treatment | 0.004** | -0.003** | 0.411** | -0.416** | 0.007*** | 0.002 |
| | (0.002) | (0.001) | (0.162) | (0.166) | (0.002) | (0.001) |
| | [0.035] | [0.051] | [0.028] | [0.020] | [0.006] | [0.277] |
| **E**. *Alternative Treatment: synthetic probability to marry in 1861–63 based on women married in 1840–55* | | | | | | |
| Treatment | 0.004** | -0.003** | 0.465** | -0.460** | 0.008*** | 0.002 |
| | (0.002) | (0.002) | (0.183) | (0.182) | (0.002) | (0.002) |
| | [0.044] | [0.052] | [0.032] | [0.020] | [0.005] | [0.328] |
| **F**. *Alternative Treatment: synthetic probability to marry in 1861–63 based on women married in 1835–50* | | | | | | |
| Treatment | 0.004* | -0.003* | 0.506** | -0.502** | 0.009*** | 0.002 |
| | (0.002) | (0.002) | (0.195) | (0.189) | (0.002) | (0.002) |
| | [0.076] | [0.081] | [0.027] | [0.022] | [0.005] | [0.346] |
| Controls | YES | YES | YES | YES | YES | YES |
| Observations | 664 | 664 | 324 | 324 | 324 | 765 |
| Model | probit | probit | OLS | OLS | probit | probit |

*Notes:* This table reports estimates of equations (2) and (3). The treatment ($T$) is the *synthetic* probability to marry during the Season's interruption (1861–63), based on the probability to marry at a given age in "normal times." Formally, $T_t = p(t)+p(t+1)+p(t+3)$, where $t$, $t+1$, and $t+2$ index a woman's age in 1861, 1862, and 1863; and $p(t)$ is the probability to marry at age $t$ in a benchmark cohort who married before the interruption. Each panel uses a different benchmark cohort. All benchmark cohorts exclude women married after 1861 and dead before age 30. The samples used in the estimation are defined in Table 2. Controls are an indicators for dukes'/marquis'/earls' daughters and English titles, and birth order. Standard errors clustered by birth year in parenthesis; p-values from the bootstrap-t procedure (Cameron et al. 2008) in brackets; *** p<0.01, ** p<0.05, * p<0.1.

Table B2: Synthetic probability based on alternative benchmark cohorts, controlling for distance to London.

| | Married a Commoner | Married an heir | Spouses' landholdings (rank percentile) Difference (abs. value) | Difference (husb-wife) | Married down | Never married |
|---|---|---|---|---|---|---|
| | [1] | [2] | [3] | [4] | [5] | [6] |
| **A**. *Baseline Treatment: synthetic probability to marry in 1861–63 based on women born in 1815-30* | | | | | | |
| Treatment | 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] | [0.047] | [0.282] |
| **B**. *Alternative Treatment: synthetic probability to marry in 1861–63 based on women born in 1810–25* | | | | | | |
| Treatment | 0.007** | -0.005** | 0.552** | -0.666*** | 0.011*** | 0.003 |
| | (0.003) | (0.002) | (0.252) | (0.229) | (0.004) | (0.002) |
| | [0.041] | [0.022] | [0.052] | [0.016] | [0.032] | [0.305] |
| **C**. *Alternative Treatment: synthetic probability to marry in 1861–63 based on women born in 1820–35* | | | | | | |
| Treatment | 0.005*** | -0.004** | 0.411** | -0.518** | 0.008*** | 0.002 |
| | (0.002) | (0.002) | (0.196) | (0.184) | (0.003) | (0.002) |
| | [0.016] | [0.027] | [0.064] | [0.024] | [0.029] | [0.364] |
| **D**. *Alternative Treatment: synthetic probability to marry in 1861–63 based on women married in 1845–60* | | | | | | |
| Treatment | 0.005*** | -0.004** | 0.394** | -0.457** | 0.007*** | 0.002 |
| | (0.002) | (0.001) | (0.180) | (0.171) | (0.003) | (0.002) |
| | [0.019] | [0.021] | [0.054] | [0.037] | [0.036] | [0.389] |
| **E**. *Alternative Treatment: synthetic probability to marry in 1861–63 based on women married in 1840–55* | | | | | | |
| Treatment | 0.006** | -0.004** | 0.442** | -0.484** | 0.007*** | 0.001 |
| | (0.002) | (0.002) | (0.203) | (0.187) | (0.003) | (0.002) |
| | [0.027] | [0.017] | [0.055] | [0.032] | [0.040] | [0.476] |
| **F**. *Alternative Treatment: synthetic probability to marry in 1861–63 based on women married in 1835–50* | | | | | | |
| Treatment | 0.005** | -0.004** | 0.480** | -0.497** | 0.008*** | 0.002 |
| | (0.002) | (0.002) | (0.211) | (0.199) | (0.003) | (0.002) |
| | [0.046] | [0.023] | [0.046] | [0.029] | [0.042] | [0.466] |
| Distance to London | YES | YES | YES | YES | YES | YES |
| Controls | YES | YES | YES | YES | YES | YES |
| Observations | 484 | 484 | 260 | 260 | 260 | 565 |
| Model | probit | probit | OLS | OLS | probit | probit |

*Notes:* This table reports estimates of equations (2) and (3). The treatment ($T$) is the *synthetic* probability to marry during the Season's interruption (1861–63), based on the probability to marry at a given age in "normal times." Formally, $T_t = p(t)+p(t+1)+p(t+3)$, where $t$, $t+1$, and $t+2$ index a woman's age in 1861, 1862, and 1863; and $p(t)$ is the probability to marry at age $t$ in a benchmark cohort who married before the interruption. Each panel uses a different benchmark cohort. All benchmark cohorts exclude women married after 1861 and dead before age 30. The samples used in the estimation are defined in Table 2. Controls are an indicators for dukes'/marquis'/earls' daughters and English titles, and birth order and the distance between the family seat and London (hence, the sample is restricted to women with a recorded family seat). Standard errors clustered by birth year in parenthesis; p-values from the bootstrap-t procedure (Cameron et al. 2008) in brackets; *** p<0.01, ** p<0.05, * p<0.1.

### B. 2 Non-parametric estimates for marriage cohorts.

Section III.D shows that the interruption of the Season (1861–63) reduced sorting for women at risk of marriage–that is, for women with a high *synthetic* probability to marry in 1861–63. Here, I show similar effects for women who *actually* married during the interruption. Specifically, I compare two cohorts of peers' daughters: women marrying during the three-year interruption (treatment group) vs. women marrying three years before (control group). I use non-parametric methods based on contingency tables and Kolmogorov-Smirnov distribution tests to evaluate the interruption's effect on, respectively, sorting by title and landholdings.

Admittedly, the decision to marry in 1861–63 is potentially endogenous. Yet, this specification is interesting in two respects: First, it evaluates whether the estimates in Section III.D are driven by women marrying around 1861–63 or, in contrast, by women affected by the interruption but marrying long after. More generally, if the effects in this specification (based on marriage cohorts) are similar to those in Section III.D (based on age cohorts), it would suggest that the pressure to marry young was strong. In other words, that women could not select themselves into marrying during the interruption, delay marriage plans, or anticipate them.

First, I show that the Season's interruption reduced sorting by title. Table B5 presents the contingency tables. The wife's title is arrayed across rows $i$ and the husband's title across columns $j$. Each cell reports observed frequencies ($O$) and expected frequencies under random matching ($E$).

The table suggests that when the Season ran smoothly noble women sorted in the marriage market. Consider the control group, i.e., women marrying in the three years before the interruption. Twenty-one out of 60 dukes', marquises', and earls' daughters (henceforth, DME's daughters) married peers' heirs (35 percent). If matching had been random, only 16.8 of these marriages would have taken place (28 percent). In contrast, barons' and viscount's daughters (henceforth, BV's daughters) were more likely to marry a commoner than under random matching. The patterns are clearly different for the treatment cohort, that is, the cohort married during the interruption of the Season. First, DME's and BV's daughters married commoners at similar rates. Second, the observed frequencies are similar to the expected frequencies. In other words, the sorting patterns of this cohort resemble random matching.

Table B6 compares sorting patterns across cohorts using the chi-squared tests of association described in Section III.D (see equations (5) and (6)). For women in the control group, the Pearson's chi-squared test of association ($\chi^2$) rejects the null hypothesis that marriages were randomly set. That is, women who married before the interruption of the Season sorted in terms of titles. In contrast, the null cannot be rejected for the treatment group, that is, for those marrying during the interruption of the Season. Col. [3] confirms that there is a significant difference in sorting patterns between the treatment and the control group: when the Season worked smoothly, women sorted in the marriage market; when the Season was interrupted, marriage was random with respect to title.

Compared to the results in Section III.D, the non-parametric tests are of similar

Table B3: Contingency tables, treatment based on marriage cohorts

**A.** Control group (Season): peers' daughters marrying in 1858–60

| | | | Husband's rank at age 15: Comm. | Gentry | Peer's son | Peer's heir | N |
|---|---|---|---|---|---|---|---|
| Wife: | *Baron/Viscount's daughter* | O | 36 | 3 | 2 | 4 | 45 |
| | | E | *26.5* | *3.3* | *5.2* | *9.9* | |
| | *Duke/Earl/Marquis' daughter* | O | 20 | 4 | 9 | 17 | 50 |
| | | E | *29.5* | *3.7* | *5.8* | *11.1* | |
| | N | | 56 | 7 | 11 | 21 | 95 |

**B.** Treatment group (Season): peers' daughters marrying in 1861–63

| | | | *Comm.* | *Gentry* | *Peer's son* | *Peer's heir* | N |
|---|---|---|---|---|---|---|---|
| Wife: | *Baron/Viscount's daughter* | O | 22 | 6 | 3 | 10 | 41 |
| | | E | *18.4* | *8.0* | *4.2* | *10.4* | |
| | *Duke/Earl/Marquis' daughter* | O | 17 | 11 | 6 | 12 | 46 |
| | | E | *20.6* | *9.0* | *4.8* | *11.6* | |
| | N | | 39 | 17 | 9 | 22 | 87 |

*Note*: Each cell reports observed frequencies (O) and expected frequencies under random matching (E); $E = \frac{n_i \times n_j}{N}$, where $n_i$ is the number of counts in the $i$th row, $n_j$ is the number of counts on the $j$th column, and $N$ is the total number of counts in the table.

magnitude. Especially, the tests are very similar for the treatment cohorts (i.e., women marrying in 1861–63 here, women with a high synthetic probability to marry in 1861–63 in Section III.D). In addition, note that both the sample size and the cell frequencies are smaller than those in Section III.D, Table 5. That said, the reported estimates are not a byproduct of the smaller sample size. For a 2 by 4 contingency table, the Pearson's chi-squared test requires no cells with zero count and an expected cell count of five or more in at least 6 cells. Both conditions are satisfied. Furthermore, the likelihood ratio test, which is accurate for small samples, confirms the results.[8]

To evaluate whether cohorts exhibit positive or negative assortative matching in titles, Table B6 also presents Kendall's rank correlation coefficients ($\tau_b$). Note that Kendall's coefficient ranges between -1 (negative association) and +1 (positive association). In the control group, that is, when the Season ran smoothly, 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 small and not significantly different from zero for the treatment group. Again, col. [3] shows that there is a significant difference in the Kendall's rank correlation between the treatment and the control group. Positive assortative matching disappeared when the Season was interrupted.

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

Table B4: Sorting by title, non-parametric tests based on marriage cohorts.

| | Control group (*m. in 1858–60*) [1] | Treatment group (*m. in 1861–63*) [2] | Difference [1]-[2] |
|---|---|---|---|
| Pearson's chi-squared | 17.0*** | 3.0 | 14.0* |
| | [0.001] | [0.389] | [0.058] |
| Likelihood ratio chi-squared | 18.0*** | 3.0 | 15.0** |
| | [0.000] | [0.384] | [0.046] |
| Kendall's rank correlation | 0.39*** | 0.12 | 0.28*** |
| | [0.000] | [0.262] | [0.002] |
| N | 95 | 87 | 182 |

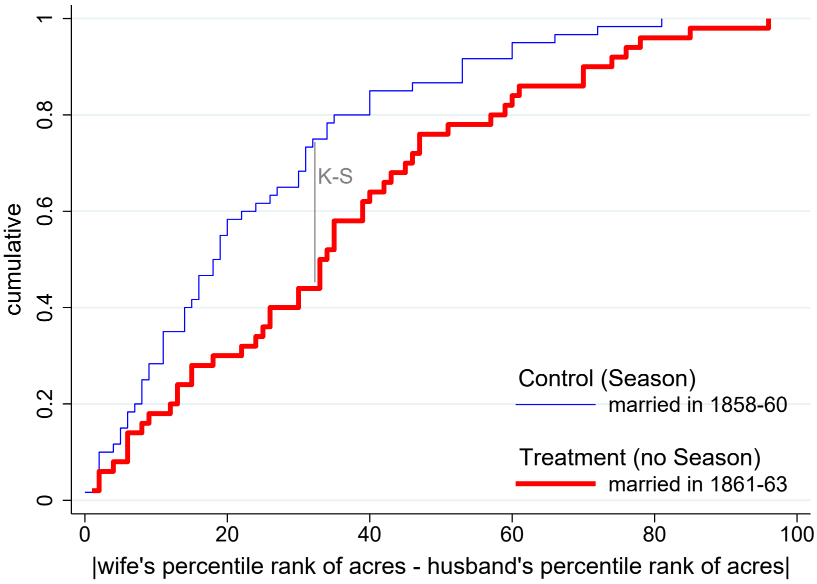
*Note*: This table presents non-parametric tests for contingency table B5. The sample is all peers' daughters marrying in 1858–63, excluding second marriages and women married above 40. Pearson's and likelihood ratio test the null that marriages were random with respect to title. Kendall's rank corr. ranges between -1 (negative assortative matching) and +1 (positive assortative matching). To compare statistics (col. [3]), I convert them into Spearman's correlations and use Fisher's Z transformation (Rosenberg 2010). To address the small sample, I report the Likelihood ratio test and bootstrap the distribution of $\tau_b$; p-values in brackets; *** p<0.01, ** p<0.05, * p<0.1

Next, I present non-parametric estimates for the effect of the interruption on sorting by landholdings. As before, the sample is restricted to women marrying in the landed elite—i.e., those for which Bateman (1883) list both spouses' family landholdings. My measure of sorting is the difference between wife's and husband's percentile rank in landholdings. Specifically, I perform a two-sample Kolmogorov-Smirnov test for the equality of distributions, where I compare this measure for women marrying in 1861–63 (treatment group) vs. women marrying three years before (control group).

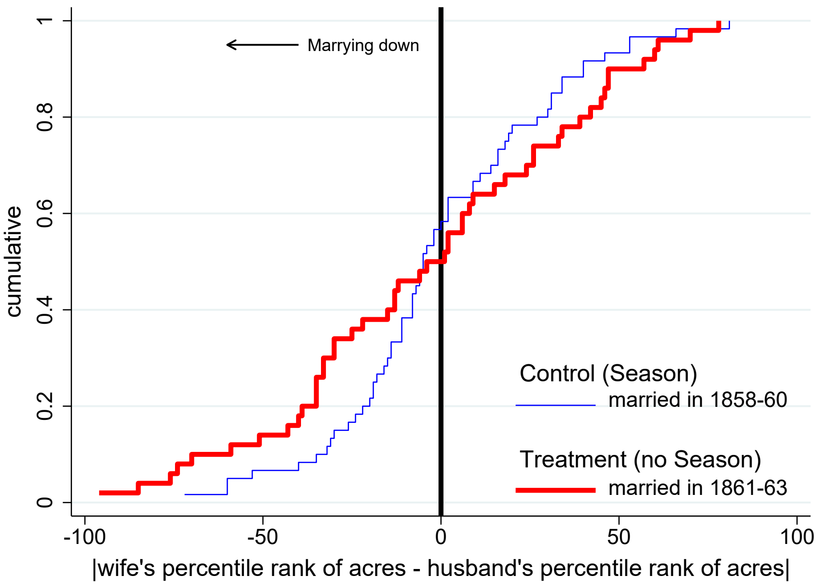
Figure B1, presents the results. The top-panel shows the difference between spouses' percentile ranks, in absolute value. When the Season worked smoothly, spouses were similar in terms of landholdings. For example, 50 percent of the marriages in the control group were between spouses ranked less than 18 percentiles away. In contrast, only 30 percent of women married during the interruption of the Season had husbands within 18 percentiles. The Kolmogorov-Smirnov test shows that the difference between spouses' rank in landholdings is significantly smaller for the control group. In other words, the interruption of the Season reduced sorting by landholdings.

As before, the disruption in sorting patterns is mostly driven by women marrying down. The bottom panel shows the difference between husband and wife in percentile ranks. Hence, negative values correspond to women marrying poorer husbands. Women marrying during the interruption were more likely to marry down than women who had married three years before. In this case, however, the Kolmogorov-Smirnov test cannot reject the null that the distributions are equal.

FIGURE B1: Sorting by landholdings, K-S tests based on marriage cohorts.




Kolmogorov-Smirnov statistic (K-S) = 0.3** (p-value = 0.011)




Kolmogorov-Smirnov statistic (K-S) = 0.2 (p-value = 0.225)

*Note*: The sample is peers' daughters marrying during the interruption of the Season in 1861–63 (N=46) and the three years before (N=57). I exclude women whose families or whose husbands' families are not in Bateman (1883), second marriages, and women married above age 40; *** p<0.01, ** p<0.05, * p<0.1

## B. 3 Contingency tables separating wives' titles.

This appendix presents non-parametric estimates for contingency tables that disaggregate wife's titles. In the main text, I followed Hollingsworth who grouped dukes', marquis', and earls' daughters in one category, and barons' and viscounts' daughters in another. Here I construct contingency tables with dukes', marquis', earls', viscounts', and barons' daughters as separate categories. To infer these title, I use a string variable in the Hollingsworth dataset (father's form of address) and biographical data from thepeerage.com. Results are robust: 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.

TABLE B5: Contingency tables separating wives' titles

**Panel A.** Low-Treatment cohorts ($T <$ 80th percentile)†

| | | | Husband's rank at age 15: | | | | |
|---|---|---|---|---|---|---|---|
| | | | *Commoner* | *Gentry* | *Peer's son* | *Peer's heir* | N |
| Wife: | *Baron's daughter* | O | 121 | 34 | 19 | 43 | 217 |
| | | E | *111.6* | *27.0* | *20.7* | *57.7* | |
| | *Viscount's daughter* | O | 40 | 1 | 1 | 6 | 48 |
| | | E | *24.7* | *6.0* | *4.6* | *12.8* | |
| | *Earl's daughter* | O | 91 | 20 | 20 | 60 | 191 |
| | | E | *98.2* | *23.7* | *18.3* | *50.8* | |
| | *Marquis' daughter* | O | 14 | 8 | 6 | 20 | 48 |
| | | E | *24.7* | *6.0* | *4.6* | *12.8* | |
| | *Duke's daughter* | O | 3 | 2 | 4 | 10 | 19 |
| | | E | *9.8* | *2.4* | *1.8* | *5.0* | |
| | N | | 269 | 65 | 50 | 139 | 523 |

**Panel B.** High-Treatment cohorts ($T \geq$ 80th percentile)†

| | | | Husband's rank at age 15: | | | | |
|---|---|---|---|---|---|---|---|
| | | | *Commoner* | *Gentry* | *Peer's son* | *Peer's heir* | N |
| Wife: | *Baron's daughter* | O | 23 | 10 | 5 | 9 | 47 |
| | | E | *24.9* | *7.0* | *4.3* | *10.9* | |
| | *Viscount's daughter* | O | 12 | 1 | 0 | 1 | 14 |
| | | E | *7.4* | *2.1* | *1.3* | *3.2* | |
| | *Earl's daughter* | O | 23 | 5 | 5 | 13 | 46 |
| | | E | *24.3* | *6.8* | *4.2* | *10.6* | |
| | *Marquis' daughter* | O | 2 | 1 | 0 | 0 | 3 |
| | | E | *1.6* | *0.4* | *0.3* | *0.7* | |
| | *Duke's daughter* | O | 4 | 1 | 1 | 5 | 11 |
| | | E | *5.8* | *1.6* | *1.0* | *2.5* | |
| | N | | 64 | 18 | 11 | 28 | 121 |

† *Treatment ($T$) is synthetic prob. to marry in 1861–63, based on marriage probs. in "normal times"*

*Note*: The baseline sample is all peers' daughters aged 15-35 in 1861 who ever married, excluding second-marriages, women married to foreigners, and members of the royal family (N=644). Each cell reports observed frequencies (O) and expected frequencies under random matching (E).

Table B6: Non-parametric tests for contingency Table B5

| | Low-Treatment [1] | High-Treatment [2] | Difference [1]-[2] |
|---|---|---|---|
| Pearson's chi-squared ($\chi^2$) | 51.58*** | 14.31 | 37.3*** |
| | [0.000] | [0.281] | [0.000] |
| Likelihood ratio chi. ($LR{-}\chi^2$) | 54.80*** | 16.08 | 38.7*** |
| | [0.000] | [0.188] | [0.000] |
| Kendall's rank correlation ($\tau_b$) | 0.18*** | 0.07 | 0.11 |
| | [0.000] | [0.366] | [0.105] |
| N | 523 | 121 | 644 |

† *Treatment ($T$) is the synthetic prob. to marry in 1861–63, based on previous cohort*

*Note*: This table presents non-parametric tests of association between spouses' titles (see contingency tables in B5). The baseline sample is all peers' daughters aged 15-35 in 1861 who ever married, excluding second-marriages, women married to foreigners, and royal family members (N=644). Low (High) treatment cohorts are women with $T <$ 80th percentile ($T \geq$ 80th p). Pearson's and likelihood ratio $\chi^2$ test the null that marriages were random with respect to title. Kendall's $\tau_b$ ranges between -1 (negative assortative matching) and +1 (positive assortative matching). To compare statistics across cohorts (col. [3]), I convert them into Spearman's correlation coeffs. and compare them using Fisher's Z transformation (Rosenberg 2010). To address the small sample, I report the $LR{-}\chi^2$ association test and bootstrap the distribution of $\tau_b$; p-values in brackets; *** p<0.01, ** p<0.05, * p<0.1

## B. 4 Extended contingency tables.

This appendix examines the robustness of the non-parametric results when (i) men and women who did not marry and (ii) men who failed to marry peers' daughters are incorporated into the analysis of sorting by title. First, I present the extended contingency tables considering these two populations (Table B7). Next, I discuss the main differences with respect to Table 4 in the main text and how these differences may affect my non-parametric estimates: chi-squared statistics and Kendall's rank correlations. Finally, I present chi-squared statistics and Kendall's rank correlations that incorporate the two previously omitted populations (Table B8).

Table B7 shows two extended contingency tables incorporating (i) men and women who did not marry and (ii) men who failed to marry peers' daughters. As before, I separate high- and low-treatment cohorts according to the wife's synthetic probability to marry during the interruption (i.e., in the top quintile vs. below it). The wife's title is arrayed across rows $i$, their husbands' titles are arrayed across columns $j$, and each cell reports the observed frequency of marriages ($O$). Differently from Table 4, here the sample are all matrimonies in the Hollingsworth dataset where the wife was aged 15–35 in 1861. Note that this includes all peer's daughters aged 15–35 in 1861 who ever married (the baseline sample, which I used in Table 4) as well as non-peer daughters aged 15–35 in 1861 who married a peer's son or a peers' heir. Unfortunately, I do not observe the four top-left cells, which correspond to marriages between commoner or gentry husbands and commoner or gentry women. The reason is that the Hollingsworth dataset only includes marriages where one spouse is related to the peerage. That said, most of these omitted marriages were not the result of the matching technology embedded in the Season, and hence, are not relevant for this study. Finally, the table also reports the number of unmarried peers' sons and peers' heirs (see last column) as well as unmarried Baron/Viscount's and Duke/Earl/Marquis' daughters (see bottom row) who were aged 15 to 35 in 1861.

TABLE B7: Alternative contingency tables

**Panel A.** Low-Treatment wife cohorts ($T <$ 80th percentile)[†]

| | | Husband's rank at age 15: *Comm-oner* | *Gentry* | *Peer's son* | *Peer's heir* | Total | Total + unmarr. |
|---|---|---|---|---|---|---|---|
| Wife: | *Commoner's daughter* | N.A. | N.A. | 213 | 241 | 454 | 454 |
| | *Gentry's dau.* | N.A. | N.A. | 42 | 53 | 95 | 95 |
| | *Baron/Viscount's dau.* | 161 | 34 | 20 | 49 | 264 | 346 |
| | *Duke/Earl/Marquis' dau.* | 108 | 31 | 30 | 90 | 259 | 318 |
| | Total | 269 | 65 | 305 | 433 | 1,072 | - |
| | Total + unmarried | 269 | 65 | 380 | 450 | - | 1,305 |

**Panel B.** High-Treatment wife cohorts ($T \geq$ 80th percentile)[†]

| | | Husband's rank at age 15: *Comm-oner* | *Gentry* | *Peer's son* | *Peer's heir* | Total | Total + unmarr. |
|---|---|---|---|---|---|---|---|
| Wife: | *Commoner's daughter* | N.A. | N.A. | 49 | 68 | 117 | 117 |
| | *Gentry's dau.* | N.A. | N.A. | 8 | 11 | 19 | 19 |
| | *Baron/Viscount's dau.* | 34 | 11 | 5 | 10 | 60 | 88 |
| | *Duke/Earl/Marquis' dau.* | 30 | 7 | 6 | 18 | 61 | 71 |
| | Total | 64 | 18 | 68 | 107 | 257 | - |
| | Total + unmarried | 64 | 18 | 93 | 110 | - | 323 |

*Note*: The sample is all matrimonies where (a) the wife was aged 15 to 35 in 1861 and (b) one spouse was a peers' offspring; Each cell reports observed frequencies (O); N.A.: not available in the Hollingsworth dataset.

Relative to Table 4 in the main text, this contingency table presents two main differences. First, it considers four additional cells for matrimonies between peer's sons and commoner's daughters, peer's sons and gentry's daughters, peer's heirs and commoner's daughters, and peer's heirs and gentry's daughters (i.e., the four top-right cells). This can affect the degree of sorting captured by the chi-squared statistics and by Kendall's rank correlation if marriage patterns differ substantially in these additional cells. For example, the chi-squared statistics in the main text suggests that wives' and husbands' titles were only associated for cohorts with a low-exposure to the interruption (see Table 5). If, in these additional cells, sorting was stronger for cohorts with a high- than with a low-exposure to the interruption, the overall chi-squared tests may look different, and the conclusion above may be reversed. At first sight, this does not seem to be the case. Focusing only on the top-right four cells across Panels A and B, we do not see substantial differences in sorting: Gentry's daughters were more likely to marry peer heirs and less likely to marry peers' sons than commoner's daughters, both in cohorts with a high- and with a low-exposure to the interruption. Nevertheless, I examine this possibility formally in Table B8.

The second main difference is that here I consider unmarried men and woman. Can this affect the corresponding chi-squared and Kendall's rank correlations? Kendall's rank correlation is based on the number of concordant, discordant, and tied marriages (see equation (6) for details). Hence, estimates are not affected whether unmarried individuals are included or not. Chi-squared tests, however, can be affected when unmarried individuals are included. The reason is that chi-squared tests compare the *observed* marriages in each cell to the *expected* marriages under a random-matching counterfactual (see equation (5)). Specifically, for each cell $i, j$, the number of expected marriages under random matching is:

$$E_{i,j} = \frac{n_i \times n_j}{N}$$

where $n_i$ is the number of counts in the $i$th row, $n_j$ is the number of counts on the $j$th column, and $N$ is the total number of counts in the table. In words, the expected number of marriages between women of title $i$ and men of title $j$ is equal to the total number of women of title $i$ ($n_i$) times the random-matching probability to encounter a men of title $j$. This random-matching probability is $\frac{n_j}{N}$, with $n_j$ being the number of men of title $j$ and $N$ interpreted as the total number of men available in the marriage market.

Whether unmarried men and women are included or not changes the number of counts in the rows, $n_i$, and columns $n_j$ (i.e., it affects the marginal distributions) as well as the total number of men available in the marriage market, $N$. Hence, even if the omission of unmarried women and men does not alter *observed* marriages, it can affect the random-matching counterfactual. Note that omitting men who failed to marry peers' daughters (i.e., men in the four additional cells described above) also affects the marginal distribution of $n_j$ and the total number of men, $n$. Hence, it can also affect the random-matching counterfactual and the chi-squared statistics.

To address the issues listed above I calculate non-parametric estimates for Table B7's extended contingency tables. That is, I include unmarried men and women and men who failed to marry peers' daughters into the analysis. Formally, the Pearson's chi-squared test of association ($\chi^2$), the Likelihood-ratio chi-squared ($\chi^2_{LR}$), and the Kendall's rank correlation ($\tau_b$) are, respectively:

$$\chi^2 = \sum_{[i,j] \in C} \frac{(O_{i,j} - E_{i,j})^2}{E_{i,j}} \ ,$$

$$\chi^2_{LR} = 2 \sum_{[i,j] \in C} O_{i,j} \cdot ln(\frac{O_{i,j}}{E_{i,j}}) \text{ , } and$$

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

where $O_{i,j}$ are the observed number of marriages in cell $\{i, j\}$. $E_{i,j}$ are the expected number of marriages under random matching in cell $\{i, j\}$. These are calculated according to the equation above. $Q$, $D$, and $t$ are the number of concordant, discordant, and tied pairs of observations in the set of cells $[i, j] \in C$ (see Section III.D for more details). Finally, $[i, j] \in C$ are the set of cells under analysis, which can include all cells in the contingency table or specific cells (e.g., cells corresponding to peers' daughters).

I consider different scenarios regarding (i) the set of cells $C$ used to construct the chi-squared tests and the Kendall's rank correlation (henceforth, sample of marriages) and (ii) who is included in the calculation of the expected frequencies, $E$ (henceforth, sample for random-matching counterfactual). Considering different samples of marriages allows me to evaluate marital sorting and the effects of the interruption of the Season separately for peer's daughters—my baseline sample—and for peers' daughters and peers' sons. In turn, I consider different samples for the random-matching counterfactual because there is not 'right' way to construct such a counterfactual. For example, the random-matching counterfactual in Table 4 is based on (randomly) re-allocating the set of married peer's daughters to the set of their husbands. That is, it assumes that the set of men and women in the marriage market is well-approximated by the observed marriages. Instead, including unmarried peer's daughters and all married and unmarried peers' sons would be equivalent to (randomly) re-allocating all these individuals into a matrimony. That is, it would assume a marriage rate of 100% and no voluntary celibacy. A priori, it is not clear which of the two reflects better a random-matching scenario with low search costs. Rather than choosing one specific random-matching counterfactual and defending its underlying assumptions, I consider different scenarios in which the random-matching counterfactual is constructed in different ways.

Table B8 presents Pearson's chi-squared test of association ($\chi^2$), the Likelihood-ratio chi-squared ($\chi^2_{LR}$), and the Kendall's rank correlation ($\tau_b$) for the high- and low-treatment contingency tables in Table B5. In Panel A, I consider my baseline scenario (corresponding to Table 4 in the main text). That is, the sample of marriages and the sample for random-matching counterfactual are peer's daughters aged 15-35 in 1861 who ever married. As explained in the main text, the two chi-squared tests and the Kendall's rank correlation are much larger for the low- than for the high-treatment cohort (see col. [3]). This suggests that sorting by title was reduced when the Season was interrupted.

Table B8: Non-parametric tests for contingency Table B7

**Panel A.**
*Sample of marriages: baseline*
*Sample for random-matching counterfactual: baseline*

| | Low-Treatment | High-Treatment | Difference |
|---|---|---|---|
| Pearson's chi-squared ($\chi^2$) | 24.6 [0.000]*** | 3.50 [0.320] | 21.1 [0.000]*** |
| Likelihood ratio chi. ($LR{-}\chi^2$) | 24.9 [0.000]*** | 3.60 [0.315] | 21.3 [0.000]*** |
| Kendall's rank correlation ($\tau_b$) | 0.20 [0.000]*** | 0.11 [0.191] | 0.09 [0.164] |

**Panel B.**
*Sample of marriages: baseline*
*Sample for random-matching counterfactual: baseline + excluded peers*

| | Low-Treatment | High-Treatment | Difference |
|---|---|---|---|
| Pearson's chi-squared ($\chi^2$) | 298.6 [0.000]*** | 76.4 [0.000]*** | 222.2 [0.000]*** |
| Likelihood ratio chi. ($LR{-}\chi^2$) | 278.9 [0.000]*** | 70.7 [0.000]*** | 208.2 [0.000]*** |
| Kendall's rank correlation ($\tau_b$) | 0.200 [0.000]*** | 0.11 [0.191] | 0.090 [0.164] |

**Panel C.**
*Sample of marriages: baseline*
*Sample for random-matching counterfactual: baseline + excluded peers + unmarried*

| | Low-Treatment | High-Treatment | Difference |
|---|---|---|---|
| Pearson's chi-squared ($\chi^2$) | 310.9 [0.000]*** | 81.8 [0.000]*** | 229.0 [0.000]*** |
| Likelihood ratio chi. ($LR{-}\chi^2$) | 83.20 [0.000]*** | 22.8 [0.000]*** | 60.40 [0.000]*** |
| Kendall's rank correlation ($\tau_b$) | 0.200 [0.000]*** | 0.11 [0.191] | 0.090 [0.164] |

**Panel D.**
*Sample of marriages: baseline + excluded peers*
*Sample for random-matching counterfactual: baseline + excluded peers*

| | Low-Treatment | High-Treatment | Difference |
|---|---|---|---|
| Pearson's chi-squared ($\chi^2$) | 385.0 [0.000]*** | 97.56 [0.000]*** | 287.5 [0.000]*** |
| Likelihood ratio chi. ($LR{-}\chi^2$) | 694.9 [0.000]*** | 175.7 [0.000]*** | 519.2 [0.000]*** |
| Kendall's rank correlation ($\tau_b$) | 0.175 [0.000]*** | 0.104 [0.208] | 0.07 [0.103] |

**Panel E.**
*Sample of marriages: baseline + excluded peers*
*Sample for random-matching counterfactual: baseline + excluded peers + unmarried*

| | Low-Treatment | High-Treatment | Difference |
|---|---|---|---|
| Pearson's chi-squared ($\chi^2$) | 374.7 [0.000]*** | 98.81 [0.000]*** | 275.9 [0.000]*** |
| Likelihood ratio chi. ($LR{-}\chi^2$) | 454.9 [0.000]*** | 115.9 [0.000]*** | 338.9 [0.000]*** |
| Kendall's rank correlation ($\tau_b$) | 0.175 [0.000]*** | 0.104 [0.208] | 0.071 [0.103] |

†Low-Treatment (High) if wife's synthetic prob. to marry in 1861–63 is $T <$ 80th percentile ($T \geq$ 80th percentile); 'Baseline': peers' daughters aged 15–35 in 1861 who ever married; 'Excluded peers': peers' sons and peers' heirs who married a non-peers' daughters aged 15–35 in 1861; 'Unmarried': unmarried peers' sons and daughters aged 15–35 in 1861; p-values in brackets; *** p<0.01, ** p<0.05, * p<0.1.

Next, Panel B presents non-parametric estimates for the baseline sample of marriages (i.e., peer's daughters aged 15-35 in 1861 who ever married). To calculate the random-matching counterfactual, it also considers peers' sons and peers' heirs who did not marry a peers' daughter. That is, it evaluates sorting along the same cells as in Table B5, but considers an extended sample for the random-matching counterfactual. Under this alternative specification, the chi-squared tests increase substantially for both the low- and the high-treatment cohorts.[9] That said, the difference between high- and low-treatment cohorts is robust. The chi-square statistic for the low-treatment cohort is four times larger than that for the high-treatment cohort. This suggests that, under this alternative calculation of the random-matching counterfactual, sorting by title decreased for cohorts exposed to the interruption of the Season. Finally, note that the Kendall's rank correlation is the same as in Panel A, as this statistic does not rely on a random-matching counterfactual.

Panel C considers the baseline sample of marriages (i.e., peer's daughters aged 15-35 in 1861 who ever married) but calculates the random-matching counterfactual with an even larger sample: now, all unmarried peers' daughters and all married and unmarried peers' sons are used for the random-matching counterfactual. Relative to Panel A, the chi-squared tests increase for both the cohort exposed to the interruption and the cohort not exposed. As before, however, the Pearson's chi-squared test and the Likelihood-ratio chi-square are significantly larger for the latter.

Finally, Panels D and E consider all the cells in Table B7. That is, they evaluate marital sorting and the effects of the interruption for all women in the Hollingsworth dataset who were aged 15–35 in 1861. This includes all peer's daughters aged 15–35 in 1861 who ever married (the baseline sample) as well as non-peer daughters aged 15–35 in 1861 who married a peer's son or a peers' heir. For the random-matching counterfactual, Panel D considers the same set of observations, and Panel E also includes unmarried peers' daughters and unmarried peers' sons and heirs. Again, the main conclusions are robust to these alternative specifications: the chi-squared tests increase for both the cohort exposed to the interruption and the cohort not exposed. That said, cohorts exposed to the interruption of the Season display substantially lower statistics, suggesting that sorting by title declined. Importantly, since these panels consider a different sample of marriages, the Kendall's rank correlation could be different from that in Panel A. However, the differences are negligible. As before, in the low-treatment cohort the Kendall's rank correlation is 0.175 and significantly different from zero: higher-titled women married men with higher titles. In other words, there was

[9] By adding more peers' sons and peers' heirs, the random-matching probability to marry them increases substantially. Hence, the difference between observed and expected frequencies increases in the bottom-right cells, which in turn magnifies the degree of sorting. Note, however, that if the random-matching counterfactual could also include gentry and commoner's who failed to marry a peers' daughter, then the expected probability to marry a peers' sons and peers' heirs would decrease again. Consequently, the levels of the chi-squared coefficients would converge to those in Panel A. Unfortunately, gentry and commoners are only listed in the Hollingsworth dataset when they married a peers' daughter.

positive assortative matching. This result vanishes when the Season was interrupted: Kendall's rank correlation is only 0.1 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 0.103.

In sum, non-parametric results are qualitatively robust to the inclusion of unmarried men and women and of men who failed to marry a peers' daughter. Considering an extended contingency table and computing random-matching counterfactuals taking into account unmarried men and women increases in the chi-squared tests across cohorts. That said, Kendall's rank correlations remain unaffected and the chi-squared tests are still substantially larger for cohorts with a low-exposure to the interruption of the Season. This confirms the result that sorting by title decreased for cohorts exposed to the interruption of the Season.

### B. 5 Synthetic probability vs. synthetic hazard rate.

Defining the treatment as the synthetic probability ($T_t$) is preferable to measures based on hazard rates. The hazard rate is defined as the probability of marrying at a given age *conditional* on not having married before. In my setting, this would be the probability of marrying during the interruption (1861–63) conditional on being single.

Formally, the synthetic hazard to marry in 1861–63 would be:

$$H_t = p(t|s_{t-1} = 1) + p(t+1|s_t = 1) + p(t+2|s_{t+1} = 1) \ ,$$

where $s$ indicates singlehood; $t$, $t+1$, and $t+2$ index a woman's age in 1861, 1862, and 1863 respectively; and $p(t|s_{t-1} = 1)$ is the probability of marrying at age $t$ *conditional* on being single at age $t-1$ in normal times. That is, the synthetic hazard captures the risk of marrying in 1861–63 for those who remained single, but not for those who had married before. This is problematic, as $H_t$ tends to be high for higher ages. Hence, many older women who actually married before 1861 would be assigned a high treatment. In contrast to the synthetic probability, $T$, can be seen as the *ex ante* probability (the probability at birth or at the start of the courting process) to marry during the interruption. Hence, $T$ captures the risk of marrying in 1861–63 independently of whether a woman was single or not during the interruption. In other words, $T$ is independent of a woman's endogenous marriage decisions.

To solve the aforementioned problems with the hazard measure $H$, one could weight it with an age-specific probability to be single in normal times. That is,

$$\begin{aligned} H'_t = Prob(s_{t-1} = 1) \cdot p(t|s_{t-1} = 1) \ + \ Prob(s_t = 1) \cdot p(t+1|s_t = 1) \ + \\ + \ Prob(s_{t+1} = 1) \cdot p(t+2|s_{t+1} = 1) \ . \end{aligned}$$

This is equivalent to the synthetic probability, $T$, proposed in equation (1).

## B. 6 Synthetic probability and ages at marriage

Table B9 presents estimates of equation (3) for the baseline sample, where the dependent variable is a woman's age at marriage and the age difference between spouses. Results show that women who—based on marriage behaviour in "normal times"—were at risk of marriage in 1861–63 did not marry at an older age. Specifically, increasing a woman's synthetic probability to marry in 1861–63 by one standard deviation (7.3 pp) increases her age at marriage by only 0.1 years (c. 36 days), a difference that is not statistically different from zero. Similarly, the age difference between spouses is not significantly associated with the treatment variable. Altogether, this evidence strongly suggests that the social norms that circumscribed courting to young ages in normal years did not change during the three years in which the Season was interrupted.

Table B9: Alternative outcome variables based on age at marriage.

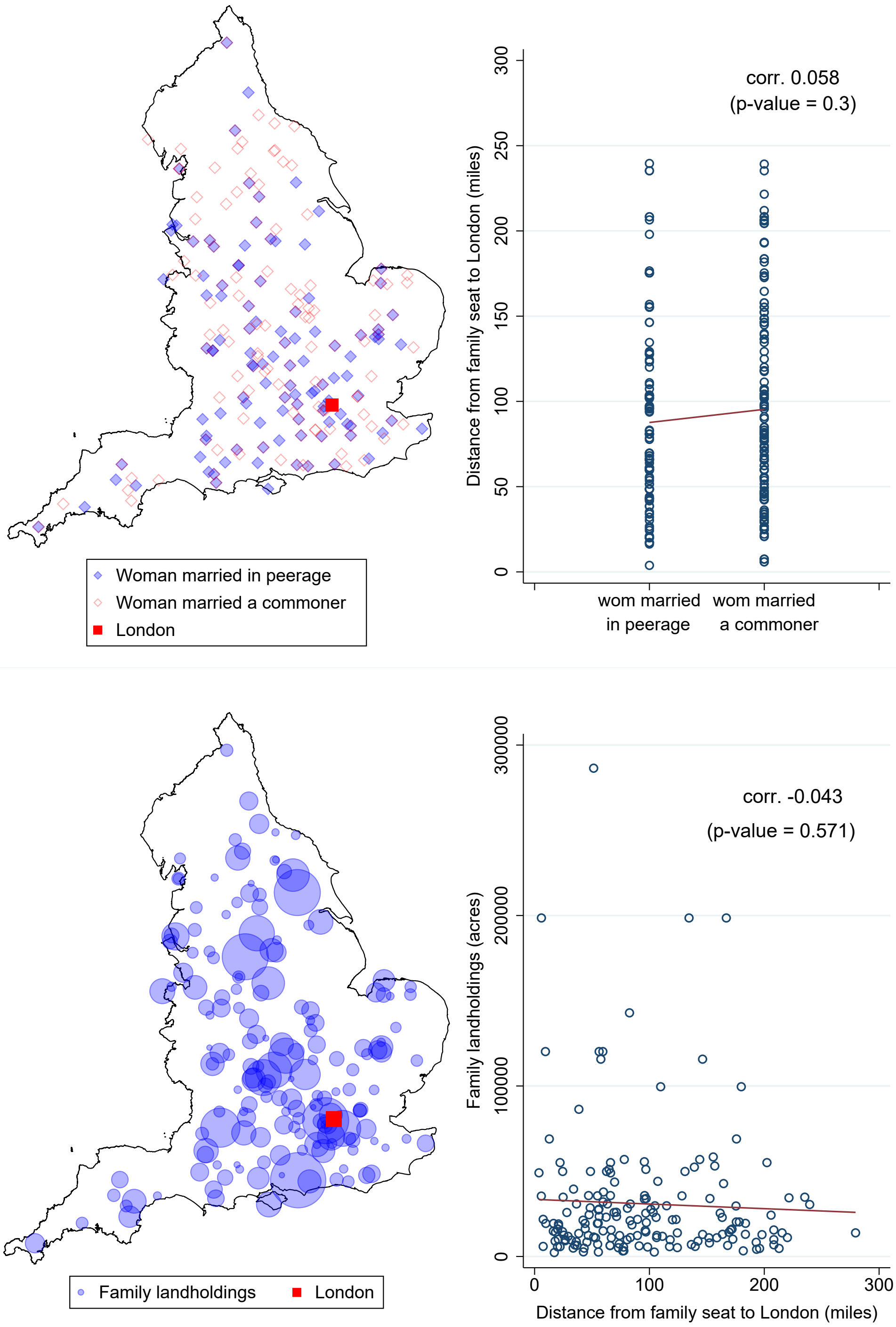
| | Age at marriage | Age at marriage | Age difference between spouses | | | |
|---|---|---|---|---|---|---|
| | | | husband − wife | | absolute value | |
| Treatment† | 0.014 | 0.016 | 0.010 | -0.016 | 0.020 | 0.002 |
| | (0.031) | (0.032) | (0.040) | (0.043) | (0.033) | (0.030) |
| Dist. to London | . | -0.005* | . | 0.002 | . | 0.001 |
| | . | (0.003) | . | (0.002) | . | (0.002) |
| Observations | 644 | 484 | 644 | 484 | 644 | 484 |
| R-squared | 0.014 | 0.022 | 0.004 | 0.006 | 0.005 | 0.006 |
| Mean DP | 25.11 | 25.15 | 6.85 | 6.88 | 7.43 | 7.42 |
| Controls | YES | YES | YES | YES | YES | YES |

†*synthetic prob. (%) to marry at interruption, based on marriage probs. in normal times.*

*Note*: This tabe uses the baseline sample (all peers' daughters aged 15-35 in 1861 who ever married, excluding second marriages, women married to foreigners, and members of the royal family); Controls are indicators for dukes'/marquis'/earls' daughters and for English titles, and birth order excluding heirs; Standard errors clustered by marriage year in parentheses; *** p<0.01, ** p<0.05, * p<0.1.

## B. 7 Correlation with distance to London.

Figure B2: Distance to London, marrying a commoner, and family landholdings.




*Note*: Baseline sample of women aged 15-35 in 1861 who ever married and whose birth-family seat was in England. *** p<0.01, ** p<0.05, * p<0.1

## B. 8 Political power and sorting by landholdings.

Table B10: Dep. variable is a 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. Effect of woman marrying a commoner (baseline):* | | | | | | |
| coef. | -0.54*** | -18.40** | -0.47** | -11.0** | -0.49*** | -7.83** |
| CLR p-val | [0.004] | [0.033] | [0.037] | [0.025] | [0.006] | [0.019] |
| | {0.025} | {0.015} | {0.102} | {0.010} | {0.007} | {0.013} |
| F-stat stg.1 | 3.4 | 3.4 | 3.4 | 3.4 | 3.3 | 3.2 |
| Obs. | 270 | 270 | 270 | 270 | 279 | 279 |
| *Panel B. Effect of difference in spouses' family landholdings (abs. value):* | | | | | | |
| coef. | -0.013*** | -0.469** | -0.012** | -0.275** | -0.012* | -0.140* |
| CLR p-val | [0.003] | [0.03] | [0.046] | [0.049] | [0.056] | [0.089] |
| | {0.033} | {0.037} | {0.068} | {0.182} | {0.141} | {0.094} |
| F-stat stg.1 | 1.6 | 1.6 | 1.7 | 1.6 | 1.4 | 1.5 |
| Obs. | 145 | 145 | 145 | 145 | 150 | 150 |
| *Panel C. Effect of difference in spouses' family landholdings (wom − husb.):* | | | | | | |
| coef. | -0.008*** | -0.433** | -0.007** | -0.245** | -0.008* | -0.123* |
| CLR p-val | [0.004] | [0.035] | [0.042] | [0.022] | [0.056] | [0.098] |
| | {0.017} | {0.035} | {0.055 } | {0.097} | {0.144} | {0.079} |
| F-stat stg.1 | 1.7 | 2 | 2 | 2.5 | 1.7 | 1.7 |
| Obs. | 145 | 145 | 145 | 145 | 150 | 150 |
| *Panel D. Effect of woman marrying down by family landholdings (indicator):* | | | | | | |
| coef. | -0.53*** | -17.12** | -0.42** | -9.83** | -0.44* | -4.95* |
| CLR p-val | [0.009] | [0.036] | [0.043] | [0.034] | [0.057] | [0.081] |
| | {0.017} | {0.033} | {0.049} | {0.109} | {0.136} | {0.073} |
| F-stat stg.1 | 1.3 | 1.5 | 1.6 | 1.8 | 1.4 | 1.5 |
| Obs. | 145 | 145 | 145 | 145 | 150 | 150 |
| Baseline co. | YES | YES | YES | YES | YES | YES |
| County co. | YES | YES | YES | YES | YES | YES |
| N. brothers | YES | YES | YES | YES | NO | NO |
| Polit. before | YES | YES | YES | YES | YES | YES |
| Model | IVprobit | IV | IVprobit | IV | IVprobit | IV |

† *Instrument: synthetic probability to marry during interruption.*

*Notes:* The sample is women in the baseline sample (aged 15-35 in 1861) with a family seat in England. Each panel presents IV estimates for the effect of a woman's marriage outcome on her birth family's political power. Sample in Panels B–D exclude women for which Bateman (1883) does not list both spouses' family landholdings. The dependent variables (family's political power) , baseline and county controls, "N. brothers," and "Polit. bef." are defined in Table 6. First-stage results not reported; CLR p-values (Moreira 2003) in square brackets and CLR p-values clustered by family in curly brackets; ***p<0.01, **p<0.05, *p<0.1

## B. 9 Political power: women and their brothers.

TABLE B11: Marriage and political power: Families where women were exposed to the interruption but brothers married before 1861.

**Panel A: Second-stage**. Dep. var: Family's political power *after* woman's marriage

| | Brother MP | | Brother local MP | | Fam. head MP 1870s | |
|---|---|---|---|---|---|---|
| | indicator | years | indicator | years | indicator | years |
| | [1] | [2] | [3] | [4] | [5] | [6] |
| Wom. married a commoner | -0.57** | -17.85*** | -0.48* | -8.55** | -0.48*** | -7.81** |
| | [0.045] | [0.002] | [0.078] | [0.02] | [0.009] | [0.04] |

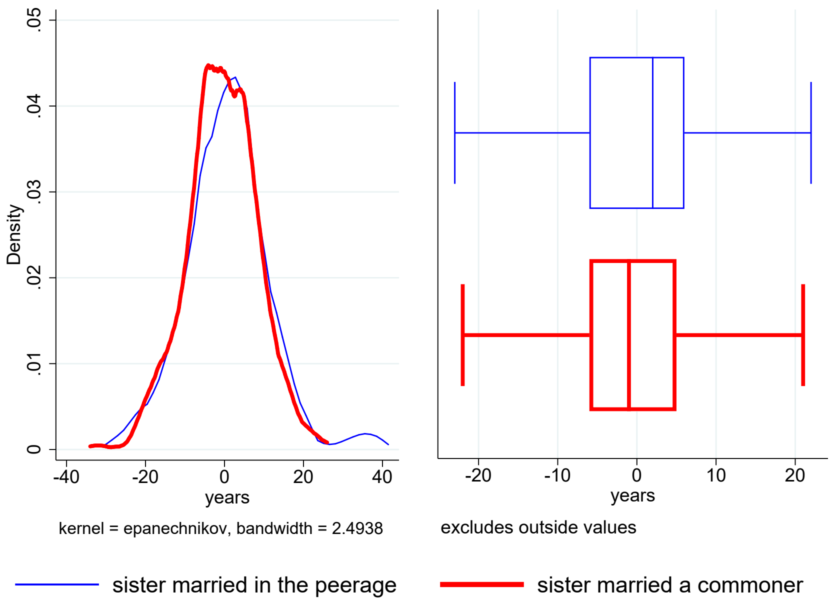
**Panel B: First-stage**. Dep. Variable: Woman married a commoner

| | [1] | [2] | [3] | [4] | [5] | [6] |
|---|---|---|---|---|---|---|
| Treatment† | 0.012** | 0.011** | 0.012** | 0.012** | 0.012** | 0.012** |
| | (0.005) | (0.005) | (0.005) | (0.005) | (0.005) | (0.005) |
| Birth order | YES | YES | YES | YES | YES | YES |
| F-statistic | 4.3 | 4.3 | 4.3 | 4.3 | 2.1 | 2.1 |
| Observations | 152 | 152 | 152 | 152 | 208 | 208 |
| Controls | YES | YES | YES | YES | YES | YES |
| N. brothers | YES | YES | YES | YES | NO | NO |
| Polit. before | YES | YES | YES | YES | YES | YES |
| Mean dep. var. in A | 0.41 | 3.47 | 0.22 | 1.4 | 0.17 | 1.31 |
| Model | IVprobit | IV | IVprobit | IV | IVprobit | IV |

† *synthetic prob. (%) to marry during interruption, based on marriage probs. in "normal times."*

*Notes:* This table presents IV estimates of equations (8) and (9). The sample is defined as in Table 6, but limited to families where the brothers (cols. [1] to [4]) and the family heads (cols. [5] and [6]) married before 1861, and hence, were not directly affected by the Season's interruption. See Table 6 for details on the covariates. Panel A reports p-values based on Moreira's (2003) conditional likelihood ratio (CLR) in brackets; Panel B reports standard errors in parenthesis; *** p<0.01, ** p<0.05, * p<0.1

FIGURE B3: Age difference between brothers and sisters.




*Note*: This figure shows the age difference between the sisters and brothers used in Table 6. Specifically, I consider women in the baseline sample with a family seat in England (N=270) and their brothers. The left Panel shows the kernel-weighted distribution of age differences, the right panel shows a box plot where adjacent lines are the lower and upper adjacent values; the box indicates the 25th and the 75th percentiles; and the central line is the median. Results suggest that birth years do not overlap excessively. In other words, the brothers of women exposed to the interruption of the Season were not necessarily affected by it themselves.

## B. 10 Political power: families where men married commoners.

In Section IV.A, I estimate an instrumental variables' model with the interruption of the Season as a source of exogenous variation in peer-commoner intermarriage. Specifically, I show that peerage families in which women married commoners lost political power in the following decades. Here I show that families in which men married a commoner also lost political power.

To do so, I consider an alternative sample: peerage families in which at least one man married during the interruption of the Season. I collect a dataset with all the biographical entries for men married in 1861–63 and their brothers (*offspring universe*, N=288). Then, I collected the same information for their fathers (*fathers universe*, N=101). Using regular expressions, I identify their political appointments in local offices, the government, religious or a judicial appointments, diplomacy or colonial posts, and positions in the royal household.

Identifying all the public appointments would involve a close examination of 389 biographies. Instead, I consider only the public posts held by the universe of fathers. In this way, I am able to see if the average offspring in the family achieved the same political power as his father. Table B12 lists all the positions considered. I classify positions into seven categories: Local offices, appointments to posts related to the government, positions related to a religious or a judicial career, appointments related to diplomacy or the colonies, and positions in the royal household. I am particularly interested in for local appointments, government offices, and religious posts. The reason is that the provision of public schooling in Britain was highly decentralized. Each School Board had plenty of powers to set tax rates on their own district. The members of School Boards, in turn, were elected by cumulative voting. This ensured some representation for local, politically powerful landowners as well as for religious authorities (Stephens 1998).

My strategy is to compare political appointments across generations. If peer-commoner intermarriage reduced a family's political power, I expect fathers to have more political appointments than their sons in families where a men married a commoner in 1861–63. Formally, this is a difference-in-differences model, where the effect of peer-commoner intermarriage on political power is captured by:

$$\Big[E(P_j^{\text{offspring}}|M_j = 0) - E(P_j^{\text{father}}|M_j = 0)\Big] - \Big[E(P_j^{\text{offspring}}|M_j = 1) - E(P_j^{\text{father}}|M_j = 1)\Big] .$$

where $P$ are public appointments; $j$ indexes families in which a man married during the interruption of the Season; and $M_j$ indicates if the family intermarried with commoners. To capture plausibly exogenous variation, $M_j = 1$ if a man of family $j$ married a commoner during the interruption, and $M_j = 0$ if a male of family $j$ married a commoner during the interruption.

Figure B4 presents the results graphically. Panel A shows that members of families in which a man married in the peerage had, on average, the same number of appointments as their fathers. The picture looks different for families in which a man had married a commoner during the interruption. In these families, offspring were appointed to one fewer public post than their fathers. The loss of political power was large for local and religious appointments. This is relevant, as the officials who could influence education provision typically held such appointments (Goñi 2021). Hence, families marrying commoners held less power and influence to distort the provision of state education.[10]

One concern is that I might be artificially assigning a higher number of appointments to individuals who held one post, but were appointed several times to it. Consider the following example: Mr. A was a Member of Parliament (MP) between 1865 and 1875. In contrast, Mr. B was elected MP three times: in 1865–1866, in 1870–72, and in 1875. With my methodology, I would consider Mr. B as having thrice the political power of Mr. A. Figure B4, Panel B addresses this by using indicators instead of the total number of appointments. Results suggest that the members of families in which a man married in the peerage had, on average, access to the same public positions as their fathers. In contrast, the members of families in which a men married out in 1861–63 had access to 20 percent fewer positions than their fathers. Again, the loss of political power was particularly important for local appointments, government and religious offices.

[10] Note that I exclude women as they were not appointed to public posts in Victorian Britain.

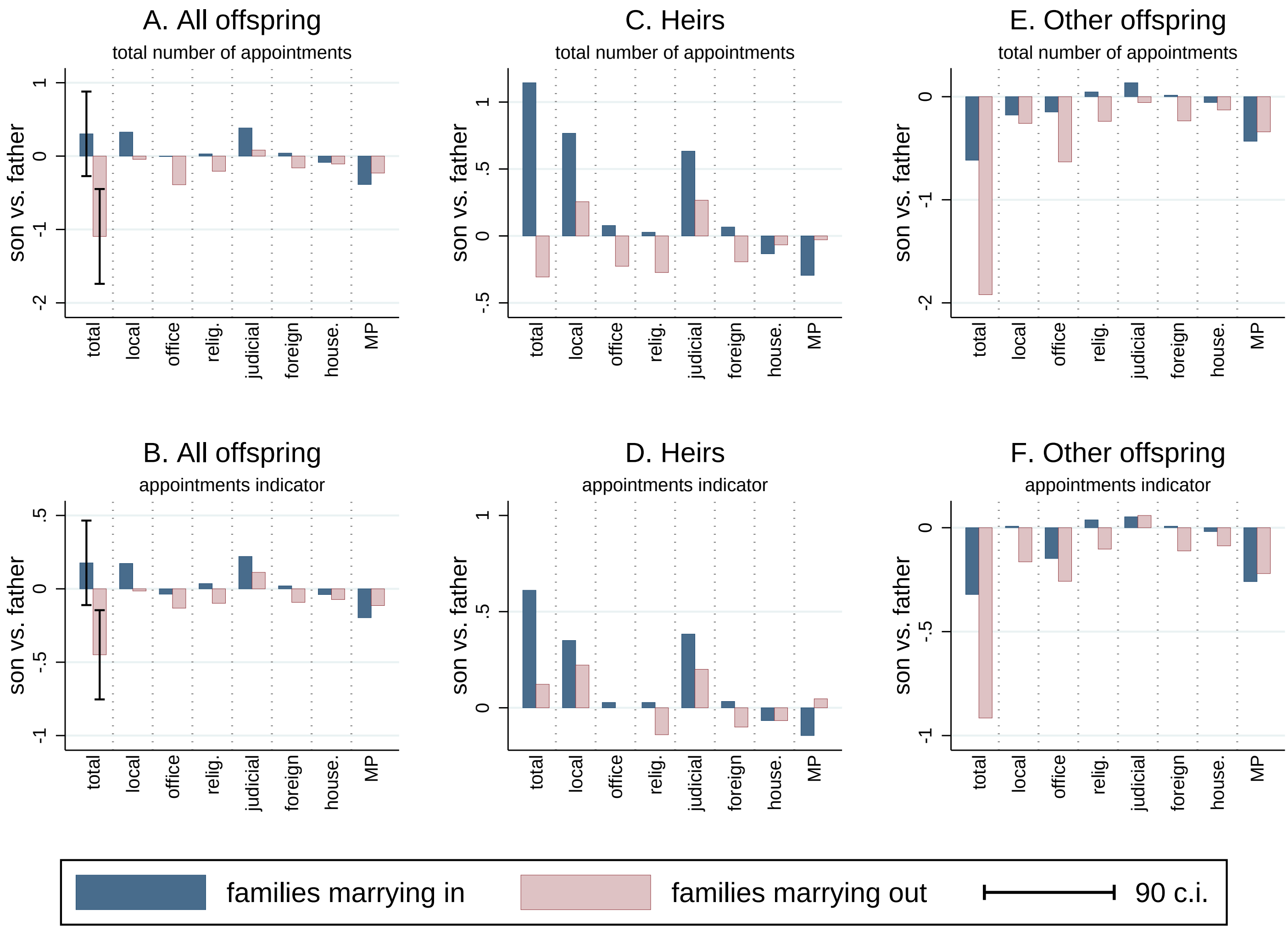



FIGURE B4: LOSS OF POLITICAL POWER, MEN. The sample is 389 men marrying during the interruption of the Season (1861–63), their brothers, and their fathers. Blue bars (red bars) are for 32 (79) families in which a man married the daughter of a peer (commoner) in 1861–63. In the top panels, bars indicate the number of public appointments of the father minus the average number of appointments of all the offspring (Panel A), of the heirs (Panel C), and of the younger sons (Panel E). The bottom panels show the corresponding difference between fathers and offspring but using position indicators rather than counts for the number of appointments to each position. All panels exclude offspring who died before 21 and consider positions held by the universe of fathers.

The remaining Panels show that the result is robust to comparing the political power of fathers to that of their heirs and to that of their younger sons. Obviously, heirs had, on average, better access to political offices than their younger brothers. However, results suggest that both heirs and younger brothers were less politically relevant than their fathers if a family member had married a commoner in 1861–63. This is so for the total number of appointments and for indicator variable equal to one if the person held a public position at least once.

The result in Panels C to F further adds to the robustness of the results. Specifically, the fact that the effects are qualitatively similar for older and younger brothers suggests that the results are not driven by a secular decline in the probability of peers having political appointments.

Table B12: Political appointments

| | |
|---|---|
| **Panel A**. Local appointments (*local*) | |
| Custos Rotulorum | *Keeper of a county's records; highest civil officer* |
| Lord Lieutenant | *Monarch's representative in a county* |
| Deputy Lieutenant (D.L.) | *Assistant to the Lord Lieutenant* |
| Chief Secretary of Ireland | *Ireland; subordinate to Lord Lieutenant* |
| High Sheriff | *Monarch's judicial representative in the county* |
| Sheriff | *Monarch's judicial representative in cities/boroughs* |
| Warden of the Stannaries | *Judicial and military functions in Cornwall* |
| Constable | *Governor of a royal castle* |
| Governor of Ireland | |
| Burgess of Glasgow | |
| **Panel B**. Office appointments (*office*) | |
| Great Offices of State | *Lord High Steward, Lord Chancellor, Lord of the Treasury, Lord President of the Council, Lord Privy Seal, Lord Great Chamberlain, Lord High Constable, Earl Marshal, First Lord of the Admiralty* |
| Cabinet position | *Prime Minister, Secretary of the Treasury, Home Department, Home Secretary, Paymaster General, President of the Board of Trade, Vice-President Board of Trade* |
| Privy Council (P.C.) | *Advisor to the Sovereign* |
| Other | *Chairman of the Customs Board, Commisioner of National Education, Deputy Chairman Customs Board* |
| **Panel C**. Religious (*relig.*) | |
| Bishop, Canon Residentiary, Chaplain, Dean, Rector, Ecclesiastical Commissioner, Lord High Commissioner of the General Assembly of the Church of Scotland | |
| **Panel D**. Judicial appointments (*judicial*) | |
| Attorney-General, Barrister, Crown Prosecutor, Justice of Peace (J.P.), Master in Chan--cery, Queen's Counsel (Q.C.) / King's Counsel (K.C.), Solicitor-General, Treasurer of the Inner Temple | |

Table B12: Political appointments (continuation)

| | |
|---|---|
| **Panel E**. Foreign appointments (*foreign*) | |
| Diplomatic | *Ambassador, Minister, Secretary of Embassy, Secretary of Legation, Envoy, Under-Secretary of Foreign Affairs* |
| Colonial | *Colonial Secretary, Under-Secretary of Colonies, Governor of Madras* |
| **Panel F**. Royal Household (*house.*) | |
| Treasurer of the Household | |
| Lord of the Bedchamber | Assisting, waiting, etc. for the King |
| Lord in Waiting | Peers who hold office in the royal household |
| Gold Stick in Waiting | Attends Monarch on ceremonial occasions |
| Groom-in-Waiting | Assisting, waiting, etc. |
| Equerry | Nominally in charge over stables |
| **Panel G**. Other | |
| Member of Parliament (*MP*), President of the Highland Agricultural Society, Chief Commissioner of Woods and Forests, First Commissioner of Woods and Forests | |

*Notes*: The positions considered are those held by the universe of fathers of men who married a commoner during the interruption of the Season (1861–63). The only exception are the Great Offices of State, which include them all. In detail, the positions Earl Marshal, Lord Great Chamberlain, Lord High Constable, and Lord President of the Council are the only ones who were not held by any individual in the universe of parents. Removing them does not alter the results.

## B. 11 Robustness for education provision.

TABLE B13: Determinants of investments in state education, robustness checks.

| (Second-stage) effect on *Av. Tax rates for education* of... | [1] | [2] | [3] | [4] | [5] | [6] | [7] |
|---|---|---|---|---|---|---|---|
| | 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 MP years |
| *Panel A. Baseline results:* | | | | | | | |
| coef. | 2.66*** | -1.69*** | -0.12** | -8.20** | -0.27*** | -1.86** | -0.62** |
| CLR p-val | (0.007) | (0.002) | (0.033) | (0.023) | (0.008) | (0.017) | (0.043) |
| F, $1^{st}$ stg | 4.33 | 10.0 | 6.16 | 10.74 | 5.85 | 1.68 | 3.16 |
| *Panel B. 50x50 mi. grid fixed effects:* | | | | | | | |
| coef. | 1.76*** | -1.13** | -0.06** | -3.00* | -0.16** | -1.58*** | -0.31** |
| CLR p-val | (0.006) | (0.016) | (0.045) | (0.054) | (0.034) | (0.007) | (0.021) |
| F, $1^{st}$ stg | 1.98 | 5.24 | 3.06 | 5.63 | 3.30 | 1.56 | 1.73 |
| *Panel C. Excluding School Boards in cities:* | | | | | | | |
| coef. | 2.30** | -1.52*** | -0.10* | -6.49* | -0.23** | -1.64** | -0.41* |
| CLR p-val | (0.018) | (0.006) | (0.064) | (0.055) | (0.019) | (0.033) | (0.096) |
| F, $1^{st}$ stg | 4.33 | 10.04 | 6.16 | 10.74 | 5.85 | 1.68 | 3.16 |
| *Panel D. Relax 10-miles area of influence:* | | | | | | | |
| coef. | 0.62** | -0.45*** | -0.03* | -1.60* | -0.06** | -0.48** | -0.12* |
| CLR p-val | (0.026) | (0.006) | (0.054) | (0.061) | (0.022) | (0.031) | (0.099) |
| F, $1^{st}$ stg | 4.33 | 10.04 | 6.16 | 10.74 | 5.85 | 1.68 | 3.16 |
| Obs. | 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 |
| N. brothers | NO | YES | YES | YES | YES | NO | NO |
| Polit. before | NO | YES | YES | YES | YES | YES | YES |
| Birth order (in 1st stage) | YES | YES | YES | YES | YES | YES | YES |

This table reports robustness checks for estimates in Table 7. Panel A reports baseline results. In Panel B, I include fixed effects for seats in the same 50-by-50 miles grid cell. That is, I estimate the effects using variation across seats which are exposed to similar local conditions. Panel C excludes School Boards in cities to show that the effects are not driven by urbanization. Finally, Panel D relaxes the assumption that the peerage exerted political influence over a 10-miles radius around their family seats. Specifically, I consider all seat-School Board dyads in England. The dependent variable is the *weighted* average tax rate, where weights decay exponentially by the distance between each School Board and the corresponding seat. Specifically, weights are $exp(-distance/14.43)$, where 14.43 is set such that School Boards 10 miles away from a seat receive a weight of 0.5; *** p<0.01, ** p<0.05, * p<0.1.

Table B14: Determinants of state education, measured with total funds raised from taxes.

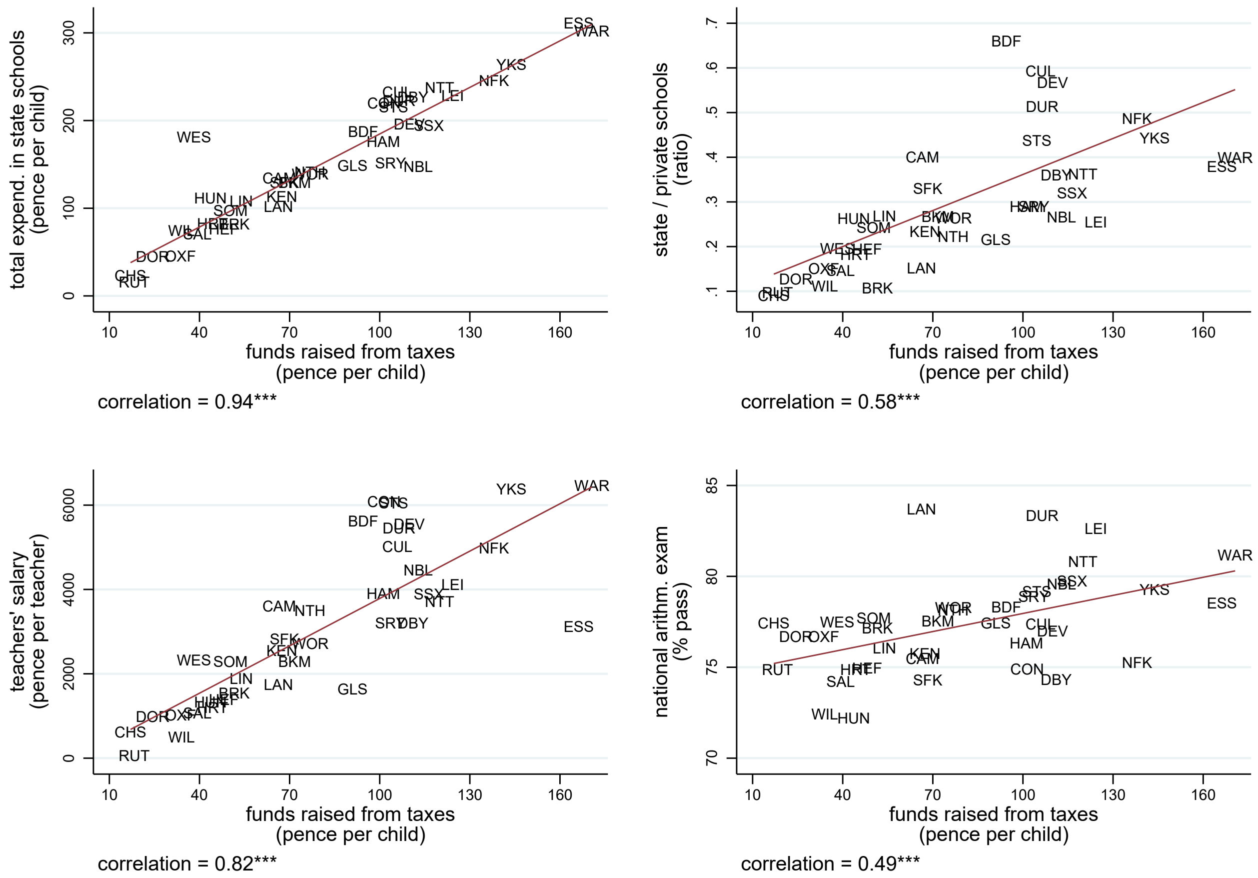
| **Panel A: Second stage** | Total education funds raised from taxes by av. School Board within 10 miles of family seat | | | | | | |
|---|---|---|---|---|---|---|---|
| | log(funds) [1] | log(funds) [2] | log(funds) [3] | log(funds) [4] | log(funds) [5] | log(funds) [6] | log(funds) [7] |
| Wom. marr. a commoner | 1.75*** (0.008) [0.066] | . | . | . | . | . | . |
| Any brother is MP | . | -1.18*** (0.001) [0.018] | . | . | . | . | . |
| All brothers' MP years | . | . | -0.11** (0.028) [0.081] | . | . | . | . |
| Any brother is local MP | . | . | . | -15.02** (0.019) [0.106] | . | . | . |
| All brothers' local MP year | . | . | . | . | -0.20*** (0.010) [0.072] | . | . |
| Family head is MP | . | . | . | . | . | -1.33** (0.015) [0.053] | . |
| Family head years MP | . | . | . | . | . | . | -1.288** (0.028) [0.128] |

| **Panel B: First stage** | | Political power in family seat *after* woman's marriage | | | | | |
|---|---|---|---|---|---|---|---|
| | Woman marr. a common | Any brother MP | All brothers' MP year | Any brother local MP | All brothers' local year | Family head is MP | Family head's MP year |
| Treatment† | 0.008** (0.027) [0.056] | -0.008*** (0.01) [0.03] | -0.170*** (0.006) [0.008] | -0.004* (0.086) [0.053] | -0.085** (0.018) [0.008] | -0.009*** (0.001) [0.005] | -0.054** (0.046) [0.05] |
| 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 |
| Obs. | 387 | 374 | 374 | 374 | 374 | 387 | 387 |
| Baseline co. | YES | YES | YES | YES | YES | YES | YES |
| N. brothers | NO | YES | YES | YES | YES | NO | NO |
| Pol. before | NO | YES | years | YES | years | YES | years |

†*synthetic prob. (%) to marry during Season interruption.*

*Notes:* This table reports IV estimates of eq. (8) and (10) (col. [1]) and of eq. (11) and (12) (cols. [2] to [7]). The Dep. Var. in Panel A is the total education funds raised from taxes (instead of the tax rate). Funds are in logs and exclude School Boards in cities. As before, I use women in the baseline sample with a family seat in England. The unit of observation is a family seat (and the area around it). Baseline controls, number of brothers, and "Pol. before" (the family's political power before a woman's marriage) are identical to Table 7. Parenthesis report p-values (first stage) and CLR p-values adjusted for weak IV (second stage). Brackets report CLR p-values adjusted for family clusters; ***p<0.01, **p<0.05, *p<0.1.

Figure B5: Funds raised from taxes and other measures of state-education.




*Note*: The sample is 38 historic counties in England (excluding the London area). Data is available for different periods: funds from rates (1871–94), expenditures in state schools (1879-94, except 1880), the ratio of state to private schools (1879-98), expenditure on teacher's salaries (1878), and exam results (1879-90). The data is from Goñi (2021).

## Appendix C. Instrumental variables estimation for men

This appendix estimates the effect of the Season on men. In the first part of the appendix, I provide evidence that the interruption of the Season is a valid instrument for peers' sons—despite the fact that the social pressure to marry young was less acute than that for women. Specifically, I show that peers' sons who married during the interruption where neither negatively selected nor the black sheep in the family. That said, male aristocrats were not so pressured to marry young as women, and hence, it is less obvious how to define the treatment variable or whether to base it on their age in 1861. In the second part of this appendix, I present an instrumental variables' strategy to overcome these issues.

### C. 1 The interruption of the Season and men

Admittedly, men were not so pressured to marry young as women. One possibility is that in some peerage families a son delayed marriage until the Season resumed, while another (the "black sheep") selected to marry during the interruption (1861–63). This appendix discusses anecdotal and empirical evidence which strongly suggests that this was not the case.

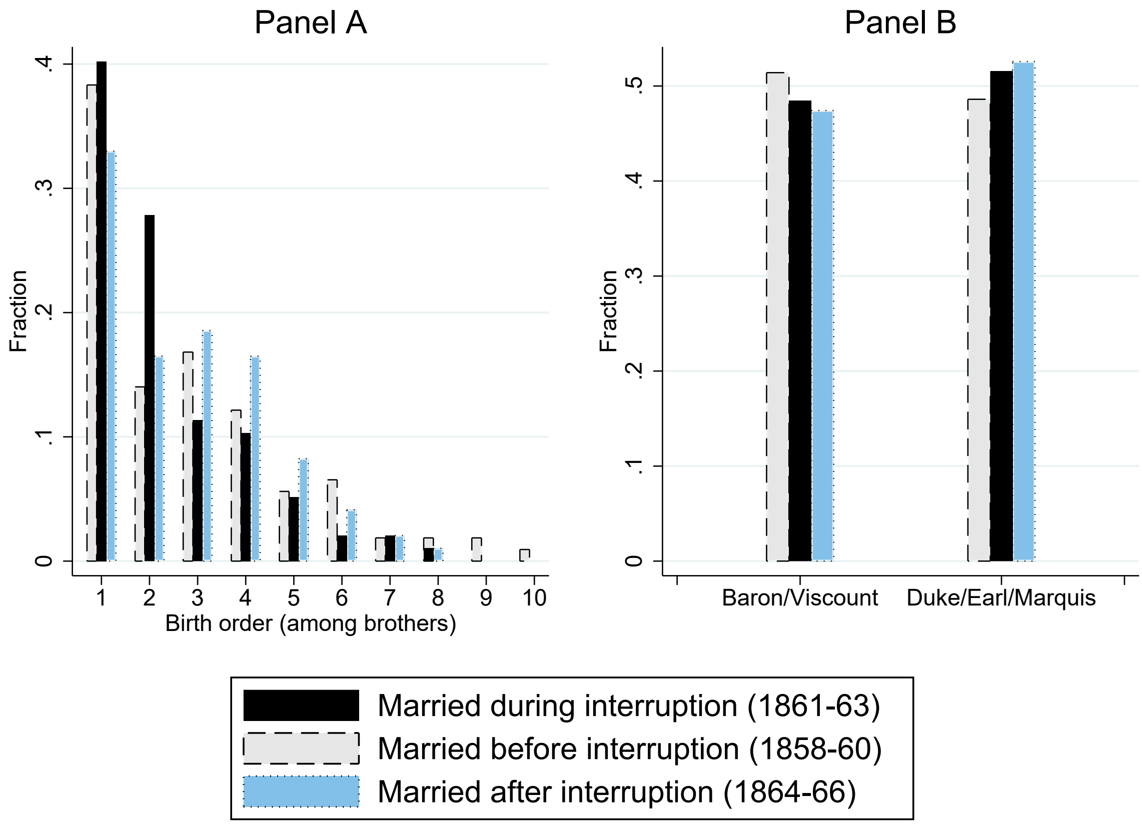
First, as discussed in Section III.A, it is unlikely that men (and women) anticipated marriages or waited for the Season to resume because the timing and duration of the interruption were unpredictable. Nobody expected Prince Albert to die in 1861 or royal balls in the Season to resume in 1864.

Second, male aristocrats who chose to marry during the three-year interruption of the Season are not different in terms of observable characteristics to those who married three years before and three years after. One revealing observable is the birth order. The first-born son inherited the family's title and landholdings. Although his brothers were entitled to an allowance raised from the family estate, there was an obvious advantage in being the first-born. Hence, if those who married during the interruption were the family's "black sheep," we would expect fewer first-born peers' sons to marry during the interruption. Figure C1, Panel A shows that this was not the case. Forty percent of peers' sons marrying during the interruption were first-born sons a similar percentage than three years before the interruption and larger than three years after. The latter strongly suggests that first-born sons did not defer marriage decision until the Season resumed in 1864. In addition, Figure C1, Panel B shows that the share of dukes', marquis', and earls' sons marrying during the interruption is identical to that in the three years before or the three years after. In other words, men coming from the lower ranks of the peerage did not select to marry during the interruption.

Third, I compare ages at marriage of all peers' sons married during the interruption and their brothers. If, within a peerage family, some sons delayed marriage until the Season resumed, while others (the "black sheep") selected to marry during the interruption, we would expect large differences in marriage ages between brothers. To explore this possibility, I estimate a (family) fixed-effects model:

$$Ageatmarriage_{i,j} = \beta \ \hat{Interruption}_{i,j} + \mathbf{mu}_j + BirthOrder_{i,j} + \epsilon_{i,j},$$

FIGURE C1: No selection for peers' sons.




*Note*: The sample are all peers' sons who married during the three-year interruption of the Season (1861–63), the three years before (1858–60), or the three years after (1864–66).

where $i$ indicates individuals (peers' sons); $j$ indicates families; $Interruption$ is equal to one if $i$ married in 1861–63 and zero otherwise; and $\mu$ are family fixed effects. By including family fixed effects, I effectively capture variation in age at marriage between brothers who married during the interruption and brothers who did so in normal times. Since birth order, especially being a first-born, may be an important determinant of marriage choices, I also include a set of birth-order indicators (among brothers). I estimate this model for all peers' sons who married during the interruption and their brothers.

Table C1 reports the results. I find that men who married during the interruption did so at a very similar age than their brothers who married in normal times. Specifically, my estimate for the coefficient $\beta$ is very small (it corresponds to half a year) and not significantly different from zero.

Table C1: Dependant Variable: Age at mariage.

| | [1] | [2] |
|---|---|---|
| Married during interruption (1861–63) | -0.545 | -0.545 |
| | (1.035) | (1.050) |
| Observations | 258 | 258 |
| % correct | 0.49 | 0.49 |
| Birth order indicators | YES | YES |
| Family fized effects | YES | YES |
| Cluster | no | by family |

*Note*: Sample are all peers' sons who married in 1861 and their brothers who married in normal times. Constants are not reported. Standard errors clustered by family are in parentheses; *** p<0.01, ** p<0.05, * p<0.1.

Altogether, this evidence suggests that male aristocrats did not negatively select to marry during the interruption of the Season. Because its timing and duration were unpredictable, men could not delay marriage until the Season resumed despite the lower social pressure to marry young as compared to women.

## C. 2 Instrumental variables' specification

Here I present an instrumental variables' model to estimate the effects of the Season for men. In the previous section, I showed evidence suggesting that the interruption of the Season is a valid instrument for peers' sons. That said, male aristocrats were not so pressured to marry young as women, and hence, it is less obvious how to define the treatment variable or whether to base it on their age in 1861 as in the main text. To overcome this, I use a different empirical strategy. In detail, I exploit an additional, more subtle source of disruption to the Season over a longer time window: changes in the size of the marriageable cohort. The idea is that smaller cohorts potentially attracted less people to attend the Season, disrupting its well-functioning. At the same time, it is unlikely that male aristocrats delayed or anticipated their marriage decisions as a result of these subtle changes. Under these premises, I estimate an instrumental variables model with the interruption of the Season and changes in the size of the marriageable cohort as sources of exogenous variation in attendance at the Season.

I begin by defining the sample, presenting the econometric specification, and providing evidence to support the identifying assumptions.

My baseline sample are all peers and peers' offspring marrying in 1851–75. Relative to Section III, this sample is different in two respects: First, it covers a longer time window. This is necessary to capture sufficient variation in cohort sizes and its (more subtle) effects on the Season.[11] Second, the sample here is

[11]In some specifications, I estimate an IV model with the Season's interruption as the only instrument (i.e., I do not include the cohort size instrument). There, I the longer time window is not needed, and hence, I restrict the sample to peers and peers' offspring marrying in 1858–66, i.e., those marrying during the interruption, three years before, and three years after. Results

based on marriage cohorts instead of age cohorts. Again, the reason is that men were not as pressured to marry young as women, and hence, it is less obvious which age cohorts are in the marriage market at different points in time.

The treatment variable capturing the Season's "intensity" in each year is the number of attendees at royal parties, $A_t$. I treat it as an endogenous variable:[12]

$$A_t = \mathbf{Z}_t'\rho + \mathbf{V}_t'\eta + d1851_t + \nu_t \ , \tag{1}$$

where $\mathbf{Z}_t$ is the vector of instruments. It includes an indicator for the interruption of the Season (1861–63) and the size of the marriageable cohort—i.e., the number of peers' daughters aged 18–24 at year $t$.[13] $\mathbf{V}_t$ includes alternative predictors: the sex ratio, the proportion of similar potential partners in the market, and the length of the railway network. The latter is the number of miles of railway in Britain and Ireland each year and is based on Mitchell (1988), Ch.10, Table 5. Since industrialists' riches were partly associated with the railway sector (see Rubinstein 1977), the length of the railway proxies for the desirability of industrialists in the marriage market. In addition, it may capture the effect of commuting costs associated to attending the Season's key events. To address the latter more precisely, I show that results are robust to controlling for the distance from the family seat to London. To account for the time effects described in Figure 3, $\mathbf{V}_t$ also includes a time trend, decade fixed effects, and an indicator for the Great Exhibition in 1851. Table C13 in this appendix provides detailed descriptions on all covariates.

The effect of the Season on marital sorting is captured by coefficient $\beta$ in

$$Pr\left(y_{i,t}|\hat{A}_t, \mathbf{V}_t, \mathbf{X}_{i,t}\right) = \Phi\left(\beta\ \hat{A}_t + \mathbf{V}_t'\lambda + \mathbf{X}_{i,t}'\delta\right) \ , \tag{2}$$

where $y_{i,t}$ is a discrete outcome for individual $i$ marrying in year $t$ (e.g., married a commoner) and $\phi$ is the CDF of the standard normal distribution. For continuous outcomes, I estimate

$$Y_{i,t} = B\ \hat{A}_t + \mathbf{V}_t'\Lambda + \mathbf{X}_{i,t}'\Delta + \epsilon_{i,t} \ , \tag{3}$$

where the dependent variable is the difference between husband's and wife's continuous characteristic $Y$ (e.g., difference in acres). In both models, $\mathbf{X}_{i,t}$ is a vector of individual controls, including title, age at marriage, or birth order.

Next, I discuss the identifying assumptions; i.e., that the instruments are relevant and that the exclusion restriction is satisfied. In Section III.A and Appendix C1, I show that the interruption of the Season satisfies these assumptions for both men and women. The size of the cohort is also a relevant instrument:

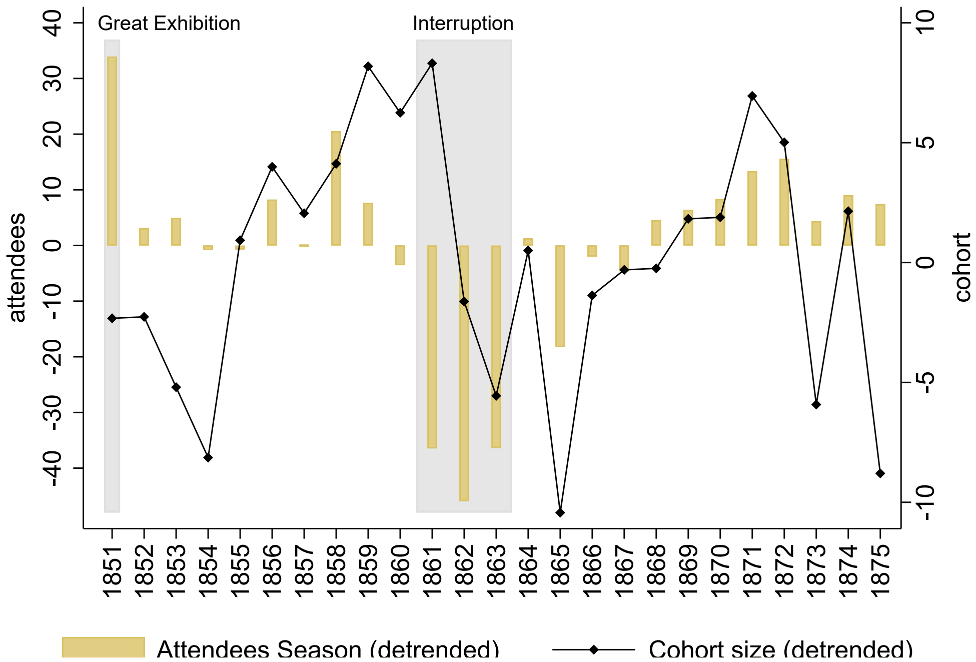
are robust to using the full sample in these specifications too (available upon request).

[12]One possibility is that when intermarriage increased, more parties were organized to restore sorting. The relation between attendance to the Season and sorting could also be driven by economic factors; e.g., a drop in land value would impoverish the nobility, reduce attendance, and increase marriages to wealthy commoners to alleviate debts.

[13]I choose the 18-24 age range because (a) eighteen was a common date at which women were presented at court—i.e., announced to the marriage market (Davidoff 1973); and (b) after age 24, the probability to marry decreased sharply (see, e.g., Figure 4).

First-stage estimates suggest that it is positively and strongly correlated with attendance at the Season. In other words, larger cohorts attracted more people to the London Season. Figure C2 explores further this correlation. Specifically, the Figure displays de-trended values for both the cohort size instrument and attendance at the Season between 1851 and 1875. Two patterns emerge: First, there is substantial variation in the size of the cohort from year to year. Overall, the instrument ranges between cohorts of 237 to 286, with a standard deviation of 15 individuals (6 percent of the mean).[14] Second, the figure suggests that when a large cohort reached marriageable age, attendance to the Season increased. For example, note that whenever the size of the cohort was above the trend, so was attendance to the Season (with the exception of the interruption in 1861–63). Finally, note that the variation in cohort size is magnified by the Season's increasing returns to scale (see results below). Since the matching technology was more efficient under a larger number of participants, small changes in the size of the cohort could lead to large changes in attendance and large effects on sorting patterns. Altogether, this suggests that the instrument likely satisfies the relevance condition.

FIGURE C2: Cohort size instrument and Season attendance.




*Note*: Bars show attendees at royal parties (left axis). The connected line (right axis) shows each year's cohort size—i.e., peers' daughters aged 18–24. Both variables are de-trended

The size of the marriageable cohort is plausibly exogenous, as no one plans how many children to have based on projections of marriage market conditions 20 years in the future. That said, the exclusion restriction would be violated if

[14] While the average marriageable cohort in 1851–75 was of 262 women, the five largest cohorts range between 278 to 286. In other words, they are 6 to 10 percent larger.

changes in the size of the cohort also affected local, decentralized marriage markets, which emerged around peers' family seats during the months when the Season was inactive. This scenario is unlikely for four reasons: First, Gautier et al. (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 changes in the size of the cohort. Second, it is unlikely that the instrument affects local marriage markets outside the London Season because the size of the cohort does not vary much locally. Only when aggregated nationwide is the variation meaningful. In other words, marriage behavior would not be affected unless the marriage market was centralized. Third, as I exploit two instruments, I use the Sargan test. Across specifications, I cannot reject that the cohort-size instrument is exogenous. Fourth, appendix C5 shows that the bias of the IV estimates would be small even if the cohort size instrument had some correlation with unobservables that affect marriages.

Finally, changes in the size of the cohort may affect marriage outcomes in local markets if they distort sex ratios or if they affect the composition of a cohort, e.g., in terms of titles or family landholdings. To address this, I include a broad set of covariates capturing sex ratios and the composition of a cohort. For example, I include the ratio of heirs to peers' daughters, the proportion of similar partners in the marriage market in terms of social class, land class (both in terms of acreage and land rents), and geographical origin.[15] Formally, the inclusion of these covariates guarantees that the conditional independence assumption holds, i.e., that, conditional on the regression covariates, the size of the cohort has no direct effect on marriage outcomes other than by affecting attendance to the Season.

### C. 3 Results

Table C2, Panel B presents first-stage estimates. Columns [2] and [5] confirm that the interruption of the Season accounts for much of the variation in attendance in 1858–66. In columns [3] and [6], I add the second instrument and I use the full sample (1851–75). Estimates show that the size of the marriageable cohort is positively correlated with attendance at the Season: One additional marriageable woman attracted around 70 people to royal parties. Across specifications, the F-test is large enough to eliminate concerns about weak instruments.

Panel A presents second-stage estimates for the effect of the Season on the rate of intermarriage with commoners. First, I consider the sample of peers' daughters as a validation exercise. In short, I want to evaluate whether the IV specification used here delivers similar results to my benchmark estimates in Section III.B for a comparable sample of women. Using exogenous variation from the interruption of the Season alone (col. [2]), I find that increasing the number of attendees by five percent—250 additional attendees—would decrease the probability of the average peer's daughter marrying a commoner by ca. one percentage point.[16] The results are very similar when I include exogenous variation in attendance from changes

[15]See Table C13 for details on how each variable is constructed.

[16]In cols. [2] and [3] I use a narrow time window (1858–66) to show that the effects are driven by the interruption of the Season and not by events happening towards the end of the sample.

in cohort size (col. [3]) and I use the full sample (1851–75). Overall, these results are consistent with those of Section III.B, lending credibility to the IV approach used here to estimate the effects of the Season on men.

Cols. [4] to [6] show that the Season also affected male aristocrats, although the effects are weaker than those for women. IV estimates suggest that increasing attendance by five percent—250 additional attendees—decreases the probability of marrying a commoner by 0.5–1 percentage points. However, in the full specification the estimated coefficient is not significantly different from zero (col. [6]).

The coefficients of the control variables suggest that higher-titled individuals were less likely to marry commoners and that peers' daughters were pressured to marry young: growing a year older is associated with an increase in the probability of marrying a commoner by 2%. The railway network is positively associated with peer-commoner intermarriage. One possible explanation is that as the railway expanded and the riches of industrialists grew, their daughters became more attractive despite their lack of landholdings. Furthermore, I find a weak association between the ratio of heirs to peers' daughters and the probability of marrying a commoner for women. This suggests that primogeniture generated an imbalance against women. Since only first-born males inherited the title, women competed for a limited number of heirs, while heirs had an ampler choice of peers' daughters. Overall, the IV model correctly predicts the probability of marrying a commoner in 70-75% of cases. The IV and probit marginal effects are very similar, suggesting that the endogeneity bias might be small. In addition, the Sargan test implies that I cannot reject the exogeneity of the instruments.

Table C3 estimates the effect of the Season on sorting by landed wealth. I restrict the sample to male great landowners; i.e., peers and peers' sons in Bateman (1883). First, I evaluate the probability to marry a bride from a family in the same wealth class, according to Bateman's categories.[17] The IV estimates in Panel A show a strong, positive effect of the Season on this sorting measure: 250 additional attendees increase the probability to marry in the same class by 1 to 2 percentage points (cols. [2] and [3]). These effects are not the result of an arbitrary definition of classes. In cols. [4] to [6], I consider a continuous sorting measure: the difference between spouses' percentile rank of acres, in absolute value. A value of zero indicates that both spouses are in the same percentile, larger values indicate less sorting. Estimates suggest that 250 additional attendees at the Season would reduce this measure of mismatch by 0.6 to 0.7 percentiles. Overall, the IV estimates are similar when using exogenous variation from both instruments or from the interruption of the Season alone. Panel B repeats the exercise using land rents as a measure of landed wealth. The dependent variable is the probability to marry a bride in the same or in a contiguous decile of the land-rents distribution (cols. [1] to [3]) and the difference between spouses' percentile rank of land rents, in absolute value (cols. [4] to [6]). The effects are similar to those in Panel A: 250 additional attendees increase the probability that spouses earn similar land rents by 2.25 percentage points, and decrease mismatch by 0.4 to 0.7 percentiles.

[17] Classes are: $>$ 100,000 acres, 50,000 to 100,000 acres, 20,000 to 50,000 acres, 10,000 to 20,000 acres, 6,000 to 10,000 acres, and 2,000 to 6,000 acres.

Table C2: Attendance to the Season and peer-commoner intermarriage, IV estimation.

| | [1] | [2] | [3] | [4] | [5] | [6] |
|---|---|---|---|---|---|---|
| Panel A: Second stage | Dep. Variable: Married a commoner | | | | | |
| | Women | | | Men | | |
| | probit | IVprobit | IVprobit | probit | IVprobit | IVprobit |
| Attendees Season (100's) | -0.003*** | -0.003*** | -0.003*** | -0.002* | -0.004** | -0.002 |
| | (0.001) | (0.001) | (0.001) | (0.001) | (0.002) | (0.002) |
| Observations | 775 | 279 | 775 | 979 | 361 | 979 |
| % correct | 70 | 71 | 69 | 75 | 76 | 75 |
| Sargan test | . | . | 1.7 | . | . | 2.46 |
| | . | . | (p=0.43) | . | . | (p=0.3) |
| Individual co. | YES | YES | YES | YES | YES | YES |
| Cohort co. | YES | NO | YES | YES | NO | YES |
| Decade FE | YES | YES | YES | YES | YES | YES |
| Trend | YES | YES | YES | YES | YES | YES |
| Sample years | 1851–75 | 1858–66 | 1851–75 | 1851–75 | 1858–66 | 1851–75 |
| Panel B: First stage | Dep. Variable: Attendees at the Season (100's) | | | | | |
| Interruption (1861–63) | – | -48.9*** | -32.0*** | – | -49.5*** | -32.4*** |
| | – | (10.5) | (7.9) | – | (10.4) | (7.6) |
| Marriage cohort size | – | | 0.7*** | – | | 0.7*** |
| | – | | (0.2) | – | | (0.2) |
| Sample years | – | 1858–66 | 1851–75 | – | 1858–66 | 1851–75 |
| F-test | – | 21 | 69 | – | 23 | 75 |
| Cohort controls | – | NO | YES | – | NO | YES |
| Decade FE and trend | – | YES | YES | – | YES | YES |
| Indicator for Great Exhibition (1851) | | YES | YES | – | YES | YES |

*Note*: The sample for Panel A is all peers and peers' offspring first-marrying in the sample years. "Attendees Season (100's)" is the number of attendees at royal parties in the Season, in hundreds. Individual controls are indicators for title at age 15 (i.e., Commoner, Barons'/Viscounts' offspring, Dukes'/Marquis'/Earls' offspring, Barons'/Viscounts' heir, and Dukes'/Marquis'/Earls' heir), age at marriage, and an indicator for English peerages. For women, I also include birth order excluding heirs. Cohort controls are railway length, sex ratios, and the proportion of similar partners in the market: number of peers' offspring aged ± 2 in the same social class. For women, I also includes the ratio of heirs to peers' daughters. See appendix Table C13 for detailed descriptions. Constants are not reported. Standard errors clustered by year are in parentheses; *** p<0.01, ** p<0.05, * p<0.1.

Note that the effect on sorting by landed wealth is stronger than on peer-commoner intermarriage. This is due to differences in market depth: there were more peers' daughters than daughters of great landowners. The Season, a matching technology that facilitated encounters, was indispensable to meet the latter.[18]

Admittedly, titles and land were not the only components of socio-economic status. Spouses may have sorted by the antiquity of the lineage, the peerage's prestige, the possession of hunting woods, etc. To estimate the effects of the Season on sorting by socio-economic status broadly defined, I construct a pecking order; i.e., I order all individuals according to a general socio-economic index. The index is the first principal component from six variables: rank in the peerage (baron/viscount's son; earl/duke/marquis' son; baron/viscount's heir; earl/duke/marquis' heir), peerage of the title (England, Ireland, or Scotland), acreage, land rents, ownership of woods, and the antiquity of the lineage (whether the family held land since the time of Henry VIII). For wives, I also include a variable indicating if they were heirs or co-heirs. Since women did not inherit peerage titles, this variable mostly refers to other assets (e.g., personal property) inherited by women of commoner origin. My measure of sorting is the difference between spouses' ranking, in absolute value. A value of zero indicates that the nth ranked man is married to the nth ranked woman, larger values indicate less sorting by socio-economic status. IV estimates show that the Season reduced spouses' differences in socio-economic status (Panel C). In the full specification, increasing attendance at the Season by 600–1,000 people would allow male aristocrats to marry a wife eight positions closer in the pecking order (col. [6]). Given that, on average, spouses are separated by ca. 80 positions, moving eight positions closer corresponds to a ten-percent increase in sorting by socio-economic status.

Panel D shows that the Season matched spouses from distant geographical origins. For every 250 additional attendees, the probability of marrying a wife from within 100 miles decreases by 1-2 percentage points, and the distance between spouses' seats increases by around 4 miles. This is consistent with the fact that, by centralizing the marriage decisions in London, the Season allowed singles from all over the country to meet, to court, and, eventually, to marry.

Finally, Table C4 reports estimates controlling for the distance from an individual's family seat to London. Specifically, I estimate the IV model in equations (1)–(2) and (1)–(3) using the full sample of individuals marrying in 1851–75 and the two instruments: the interruption of the Season and changes in cohort size. Compared to before, the sample is smaller because it is restricted to families with seats recorded in Burke (1826).

As stressed in Section III.B, this covariate is potentially important because attending the Season may have been more costly for those living further away. However, the estimates suggest that peer-commoner intermarriage, sorting by acres, and sorting by socio-economic status is not significantly associated to the distance from London. Only the probability of marrying in the same land rents class (col. [5]) displays a significant association with the distance to London.

[18] This is line with the fact that some people are willing to disregard education to marry within caste, but do not do so in equilibrium if the market is sufficiently deep (Banerjee et al. 2013)

Table C3: Attendance to the Season and other sorting patterns, IV estimation for men.

| | [1] probit | [2] IVprobit | [3] IVprobit | [4] OLS | [5] IV | [6] IV |
|---|---|---|---|---|---|---|
| Panel A. Sorting by landholdings (acres) | | | | | | |
| | Wife in same land class | | | Mismatch | | |
| Attendees Season (100's) | 0.005*** | 0.003** | 0.008*** | -0.205 | -0.268** | -0.224** |
| | (0.002) | (0.001) | (0.002) | (0.122) | (0.112) | (0.112) |
| Observations | 257 | 101 | 257 | 171 | 63 | 171 |
| % correct | 84 | 83 | 84 | . | . | . |
| F-stat first stage | - | 23 | 66 | - | 19 | 72 |
| Sargan test, p-value | . | . | 0.2 | . | . | 0.5 |
| Panel B. Sorting by landholdings (land rents) | | | | | | |
| | Wife in same land class | | | Mismatch | | |
| Attendees Season (100's) | 0.008*** | 0.009*** | 0.009*** | -0.209** | -0.278*** | -0.167* |
| | (0.001) | (0.002) | (0.002) | (0.101) | (0.107) | (0.086) |
| Observations | 257 | 101 | 257 | 171 | 63 | 171 |
| % correct | 77 | 74 | 77 | . | . | . |
| F-stat first stage | - | 23 | 64 | - | 19 | 75 |
| Sargan test, p-value | . | . | 0.9 | . | . | 0.5 |
| Panel C. Sorting by socio-economic status, constructed using PCA | | | | | | |
| | | | | Mismatch | | |
| Attendees Season (100's) | - | - | - | -0.659** | -1.396* | -0.745** |
| | - | - | - | (0.316) | (0.744) | (0.343) |
| Observations | - | - | - | 170 | 62 | 170 |
| F-stat first stage | - | - | - | - | 19 | 76 |
| Sargan test, p-value | | | | . | . | 0.7 |
| Panel D. Geographic endogamy | | | | | | |
| | Wife < 100 miles | | | Distance in miles | | |
| Attendees Season (100's) | -0.007*** | -0.004** | -0.007* | 1.17* | 1.51*** | 1.80** |
| | (0.003) | (0.002) | (0.004) | (0.62) | (0.29) | (0.74) |
| Observations | 167 | 63 | 167 | 167 | 63 | 167 |
| % correctly predicted | 68 | 63 | 68 | . | . | . |
| F-stat first stage | - | 28 | 55 | - | 28 | 55 |
| Sargan test, p-value | . | . | 0.2 | . | . | 0.4 |
| Individual controls | YES | YES | YES | YES | YES | YES |
| Cohort controls | YES | NO | YES | YES | NO | YES |
| Decade FE and trend | YES | YES | YES | YES | YES | YES |
| Interruption IV | - | YES | YES | - | YES | YES |
| Cohort size IV | - | NO | YES | - | NO | YES |
| Sample years | 1851-75 | 1858-66 | 1851-75 | 1851-75 | 1858-66 | 1851-75 |

*Note*: The sample is married peers and peers' sons listed in (Bateman 1883) (Panels A to C) or whose family seat and wife's family seat is in Burke (1826) (Panel D). "Wife in same land class" is based on 6 classes in Panel A (>100,000 acres; 50,000 to 100,000; 20,000 to 50,000; 10,000 to 20,000; 6,000 to 10,000; and 2,000 to 6,000, Bateman 1883: 495) and on the probability of marrying in the same decile or a contiguous decile of the land rent's distribution in Panel B. "Mismatch" is the absolute value of the difference in spouses': percentile rank of acres and land rents (Panels A and B), and socio-economic status rank (Panel C). This excludes men marrying wives whose family is not in Bateman (1883). In Panel C, dep. vars. are based on the minimum aerial distance between spouses' seats. Individual controls are age at marriage and an indicator for English titles in all Panels, acres (Panel A), land rents (Panel B), all variables in the PCA (Panel C), and title at age 15 (Panel D). Cohort controls are railway length, sex ratios, and the proportion of similar partners in the market: women aged 18–24 from each man's acres class (Panel A), land rents' class (Panels B and C), and women aged ± 2 whose family seat is in the same first-level NUTS in England and Wales, electoral region in Scotland, or province in Ireland (Panel D). See appendix Table C13 for details. s.e. clustered by year; *** p<0.01, ** p<0.05, * p<0.1

Table C4: Attendance to the Season and sorting patterns, IV estimation controlling for distance to London.

| | Peer-commoner inter. | | Sorting by acres | | Sorting by land rents | | Sorting by SES (pca) |
|---|---|---|---|---|---|---|---|
| | Married a common. [1] | Married a common. [2] | Wife in same land class [3] | Mismatch [4] | Wife in same land class [5] | Mismatch [6] | Mismatch [7] |
| Attendees Season (100's) | -0.005*** | -0.000 | 0.011*** | -0.427* | 0.014*** | -0.390*** | -1.051* |
| | (0.002) | (0.001) | (0.003) | (0.240) | (0.004) | (0.135) | (0.586) |
| Distance to London | 0.000 | 0.000 | 0.000 | 0.001 | 0.001** | -0.022 | -0.015 |
| | (0.000) | (0.000) | (0.000) | (0.027) | (0.000) | (0.015) | (0.039) |
| Observations | 584 | 649 | 173 | 118 | 173 | 118 | 117 |
| F-stat, first stage | 66 | 76 | 71 | 70 | 69 | 74 | 76 |
| Instruments: | | | | | | | |
| Interruption | YES | YES | YES | YES | YES | YES | YES |
| Cohort size | YES | YES | YES | YES | YES | YES | YES |
| Individual controls | YES | YES | YES | YES | YES | YES | YES |
| Cohort controls | YES | YES | YES | YES | YES | YES | YES |
| Decade FE and trend | YES | YES | YES | YES | YES | YES | YES |
| Sample | women | men | men | men | men | men | men |
| Sample years | 1851-75 | 1851-75 | 1851-75 | 1851-75 | 1851-75 | 1851-75 | 1851-75 |
| Model | IV probit | IV probit | IV probit | IV | IV probit | IV | IV |

*Note*: This table presents IV estimates of equation systems (1)–(2) and (1)–(3). The sample is all peers and peers' offspring first-marrying in 1851–75. Here I control for the distance between an individual's family seat and London. Hence, the sample is restricted to individuals with a recorded family seat. Col. [1] considers women, the rest consider men. In cols. [3] to [7], the sample is restricted to individuals listed in Bateman's *Great Landowners* (1883). "Attendees Season (100's)" is the number of attendees at royal parties in the Season, in hundreds. "Wife in same land class" is based on 6 classes in col. [3] (>100,000 acres; 50,000 to 100,000; 20,000 to 50,000; 10,000 to 20,000; 6,000 to 10,000; and 2,000 to 6,000, Bateman 1883: 495) and on the probability of marrying in the same decile or a contiguous decile of the land rent's distribution in col. [4]. "Mismatch" is the absolute value of the difference in spouses': percentile rank of acres, land rents, and socio-economic status rank. This excludes men marrying wives whose family is not in Bateman (1883). The corresponding individual and cohort controls are listed in Tables C2 and C3. Standard errors clustered by marriage year in parentheses; *** p<0.01, ** p<0.05, * p<0.1.

In addition, the main estimates for the effect of the Season on sorting are robust. After controlling for the distance to London, I find that increasing the number of attendees at the Season reduces peer-commoner intermarriage for peers' daughters (col. [1]). As before, attendance to the Season is not so strongly associated to peer-commoner intermarriage for men (col. [2]). In contrast, largely attended Seasons increased sorting by acres (cols. [3] and [4]), by land rents (cols. [5] and [6]), and by a socio-economic index computed using p.c.a (col. [7]).

### C. 4 Extensions

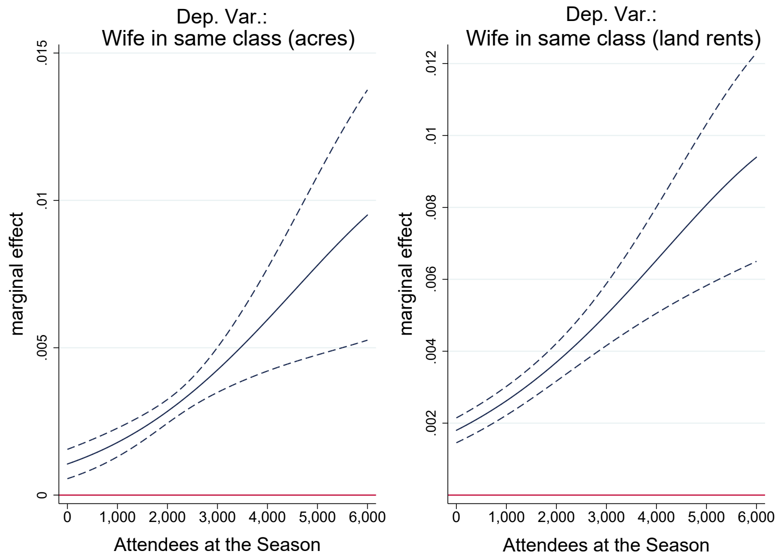
Here I extend the analysis of the IV model described above. First, I show that the matching technology embedded in the Season displays increasing returns to scale. I then stratify my dataset by observables in order to identify the segments of the peerage for which the effects of the Season are more pronounced. Next, I look at fertility rates of matched couples and their degree of consanguinity to examine whether the Season had any effects on match quality. Finally, I examine the role of homophilic preferences as an important determinant of marital sorting.

**Increasing returns to scale.** Here, I evaluate whether the matching technology embedded in the Season displays increasing returns to scale. Consider a meeting function $M(p)$, where $p$ are the number of participants and $M$ gives the number of encounters. $M(p)$ displays increasing returns to scale if the number of encounters grows more than proportionally to the number of participants. This prediction is hard to test empirically, as we typically observe matched couples and not the number of encounters. To overcome this, I exploit a well-established implication of search models: a larger number of encounters is associated with an increase in positive assortative matching (Burdett and Coles 1997). In other words, if positive assortative matching grows more than proportionally to the number of participants, this suggests that the number of encounters has increased, and hence, that the matching technology displays increasing returns to scale. In my setting, I evaluate whether the effects of the Season on marital sorting are stronger for largely attended Seasons.

Specifically, I use the IV probit model in equation 2 to estimate nonlinear marginal effects of attendance to the Season. Figure C3 presents the results for the probability of marrying in the same class in terms of acres and land rents. When the Season is largely attended, bringing in additional guests has a larger marginal effect. In other words, the Season was subject to increasing returns: as more people attended, singles met at a higher speed, and sorting strengthened.

This result is important in two respects. On the one hand, it validates the identifying assumptions. By affecting attendance, the size of the marriageable cohort could distort or reinforce the efficiency of the Season. In contrast, the local, decentralized markets where people courted when the Season was inactive do not typically display increasing returns to scale, and hence, were not affected by the size of the cohort. On the other hand, whether the matching technology displays decreasing, constant, or increasing returns to scale has important implications. Will a shortage of singles decrease marriage rates? Will sorting grow at a slower

Figure C3: Increasing returns to scale, Probit estimation.




*Note*: This figure plots the marginal effect of 100 additional Season attendees using the probit IV model in 1 and 2. Marginal effects are evaluated at different values of attendance (x-axis) and at the means of all other covariates. Dashed lines are 90% confidence intervals.

or faster rate than the number of users of online matching sites? Gautier et al. (2010) and Botticini and Siow (2011) estimate returns to scale in marriage markets by comparing the city and the countryside. In contrast, I consider a matching technology that not only pooled singles together, but explicitly facilitated their introduction and courtship. In this respect, my results provide better insights for matching technologies such as online sites and speed-dating events.

In addition, this result suggests a potential explanation to why fertility booms tend to echo over time. If the marriage market displays increasing returns to scale, a "boom" cohort may encounter partners more easily, and hence, marry at higher rates, earlier on, etc. As a consequence, the fertility of this large cohort may be boosted. The importance of this channel for fertility booms, however, is a question for future research.

**Sample stratification.** Next, I compare the effects of the Season across different segments of the peerage. Because my sample is not large enough to perform stratifications at a fine-grain level, I subdivide the sample into only two subgroups of approximately equal size. First, I subdivide the sample of great landowners married in 1851–75 (N=257) into heirs (N=126) vs. non-heirs (N=131) and compare estimates of equation systems (1) and (2); (1) and (3) for each subgroup. I then perform the same exercise dividing the sample into landowners with an acres above (N=130) vs. below the median (N=127), with land rents above (N=128) vs. below the median (N=129), and with my PCA socio-economic status index above (N=129) vs. below the median (N=128).

Table C5 presents the results. I find stronger and more tightly identified effects for individuals of higher socio-economic position. When the Season was

## Table C5: Sample stratification

| | [1] | [2] | [3] | [4] | [5] | [6] | [7] | [8] | [9] |
|---|---|---|---|---|---|---|---|---|---|
| | | | | Acres | | Land rents | | SES, using PCA | |
| | Baseline | Heir | No-heir | Above median | Below median | Above median | Below median | Above median | Below median |
| Dep. Variable: | | | | | | | | | |
| Wife in acres class | 0.008*** | 0.018*** | 0.003 | 0.010*** | 0.005 | 0.011*** | 0.002 | 0.010*** | 0.008*** |
| | (0.002) | (0.003) | (0.002) | (0.004) | (0.003) | (0.004) | (0.004) | (0.003) | (0.003) |
| | 257 | 126 | 131 | 130 | 127 | 128 | 129 | 129 | 128 |
| Wife in rents class | 0.009*** | 0.009** | 0.006** | 0.010* | 0.008** | 0.012** | 0.006* | 0.008 | 0.010*** |
| | (0.002) | (0.004) | (0.002) | (0.005) | (0.003) | (0.005) | (0.004) | (0.005) | (0.003) |
| | 257 | 126 | 131 | 130 | 127 | 128 | 129 | 129 | 128 |
| Mismatch in acres | -0.22** | -0.44 | -0.16 | -0.51** | 0.24 | -0.72*** | 0.54** | -0.33 | -0.34 |
| | (0.11) | (0.33) | (0.23) | (0.20) | (0.24) | (0.22) | (0.25) | (0.33) | (0.26) |
| | 171 | 96 | 75 | 94 | 77 | 100 | 71 | 86 | 85 |
| Mismatch in rents | -0.17* | -0.19 | 0.03 | -0.21 | -0.11 | -0.27 | 0.05 | -0.47*** | 0.02 |
| | (0.09) | (0.16) | (0.23) | (0.28) | (0.21) | (0.20) | (0.17) | (0.16) | (0.16) |
| | 171 | 96 | 75 | 94 | 77 | 100 | 71 | 86 | 85 |
| Mismatch in SES | -0.63* | -0.02 | -1.08*** | -0.62 | -0.52 | -1.03*** | -0.56 | -0.49 | -0.62* |
| | (0.33) | (0.64) | (0.41) | (0.60) | (0.41) | (0.38) | (0.58) | (1.15) | (0.36) |
| | 170 | 95 | 75 | 94 | 76 | 100 | 70 | 86 | 84 |
| Distance btw seats | 1.8** | -0.5 | 3.5*** | 2.0** | 8.2*** | 1.2 | 7.7*** | 1.3* | 13.2*** |
| | (0.7) | (1.0) | (1.3) | (0.9) | (2.3) | (0.8) | (1.6) | (0.7) | (5.0) |
| | 167 | 78 | 89 | 144 | 23 | 145 | 22 | 141 | 26 |

*Note*: This table reports IV marginal effects of the number of attendees at royal parties (in 100s of attendees) on the corresponding marriage outcome in the rows. Each column divides the sample by heir status, acreage, land rents, and the SES index constructed using PCA. Regression samples and covariates are described in Tables C2 and C3. First-stage results are reported in Table C2, Panel B. Standard errors clustered by year are in parentheses; *** p<0.01, ** p<0.05, * p<0.1

(exogenously) well-attended, sorting by landholdings (rows [1] and [3]), land rents (rows [2] and [4]), and socio-economic ranking (row [5]) in general increased more for peers' heirs and for landowners in possession of larger estates.[19]

In contrast, the effect of the Season on geographic endogamy, i.e., the distance between spouses' family seats, comes from lower-status individuals. Non-heirs, lesser landowners, and individuals with low socio-economic index married spouses from farther away when the Season ran smoothly. This suggests that heirs' younger brothers were not reduced to staying at their country seats. They also participated in the Season, courted, and married women from all over Britain and Ireland.

**Match quality.** Does the matching technology affect the quality of the match? Next, I evaluate the effects of the London Season on two measures that proxy for match quality: the degree of consanguinity between spouses and fertility.

Consanguinity is a good proxy for match quality. Although the genetic risks associated with consanguinity are debated (Bittles 2012), there is an old stigma on cousins marrying. For example, Charles Darwin, who was married to his first cousin Emma Wedgwood, was deeply concerned with the health of his ten children. In Goñi (2015), I show that the London Season prevented consanguinity. When the Season was interrupted after the death of Prince Albert and Queen Victoria's mother, cousin marriage flourished. In particular, as marriage decisions shifted to the local marriage markets, the children of the nobility faced a reduced pool of proper singles. In these circumstances, many considered marrying within their extended family to secure a noble match. The percentage of people marrying their second cousin increased by a factor of five, from 0.5 percent the years before and after the interruption (i.e., 1859–60 and 1864–67), to almost three percent in 1861–63. These matches were of poorer quality: their children were more likely to die before reaching marriage age; they had fewer children; and they were 50 percent more likely to be childless.

Alternatively, one could evaluate the quality of the match by looking at how many children couples produced. Among noblemen, producing enough children to ensure succession is paramount. Not all couples were equally successful. Take, for example, the marriage of the Duke of Northumberland to Edith Campbell: they had 13 children, ten of them after an heir was born. Other couples may not have been so eager to visit each other's chambers.[20]

Table C6 reports the coefficients from a regression of fertility on attendance at the Season. In cols. [1] and [2], the dependent variable is the number of children born to each couple. Results are modest and not significant when I instrument attendance at the Season with its interruption in 1861–63 and with the size of the cohort. In cols. [3] and [4], I consider the number of births per year of fertile-marriage lifetime (i.e., between ages 15 and 40). Results are small but significant:

[19] There are only two exceptions: The effect on sorting by SES index is strong for non-heirs, and the probability of marrying in one's land-rent class is stronger for those with a small SES.

[20] Marital happiness is difficult to measure, especially since divorce was not common at the time. Although the Divorce and Matrimonial Causes Act of 1857 allowed divorce on the grounds of adultery, between 1851–75 only 31 peers and peers' offspring actually divorced. The number of children born to a couple, instead, can serve as a proxy for match quality.

100 additional guests in the Season increased by 0.002 the number of births per year, which is equivalent to 0.1 standard deviations. In cols. 5 and 6, I look at a measure that takes into account the biological maximum fertility of women: births relative to the births that a Hutterite women would have produced. Hutterites are a traditionalist Christian church fellowship that rejects birth control, and, thus, can be taken as a proxy for unconstrained fertility (Clark 2007; Voigtlander and Voth 2013). OLS and IV results suggest that when the Season ran smoothly, matched couples were closer to the Hutterites—that is, to the biological maximum fertility: 250 additional guests at royal parties increased the number of children born, relative to Hutterites, by one percentage point.

Table C6: The Season and the quality of the match (1851–75)

| | [1] | [2] | [3] | [4] | [5] | [6] |
|---|---|---|---|---|---|---|
| | Dep. Variable: Number of births | | | | | |
| | total | | per year | | relative to Hutterites | |
| | OLS | IV | OLS | IV | OLS | IV |
| Attendees Season | 0.025** | 0.020 | 0.002*** | 0.002*** | 0.004** | 0.004** |
| | (0.011) | (0.014) | (0.001) | (0.001) | (0.002) | (0.002) |
| Observations | 466 | 479 | 466 | 479 | 462 | 474 |
| R-squared | 0.098 | | 0.087 | | 0.068 | |
| Sargan test | . | 1.30 | . | 1.10 | . | 0.18 |
| | . | p=0.5 | . | p=0.6 | . | p=0.9 |
| Individual controls | YES | YES | YES | YES | YES | YES |
| Marriage market co. | YES | YES | YES | YES | YES | YES |
| Decade FE | YES | YES | YES | YES | YES | YES |
| Year trend | YES | YES | YES | YES | YES | YES |
| Interruption IV | - | YES | - | YES | - | YES |
| Cohort size IV | - | YES | - | YES | - | YES |
| F-stat from first stage | - | 20.9 | - | 20.9 | - | 20.9 |

*Note*: The sample is all peers and peers' daughters first marrying in 1851–75. I exclude women marrying over age 30 and women having one or no children. The former might have been hard-pressed to have children before age 35, when fertility sharply declines, biasing the estimates upwards. The latter may have been infertile or had difficulties produceing an heir, biasing the estimates downwards. Columns [1] and [2] report marginal effects of 100 additional attendees at the Season on the total number of births. In Columns [3] and [4], the dependent variable is the number of births per year, considering the number of years a woman was effectively married between ages 15–40. Columns [5] and [6] consider the number of births relative to the births that an average Hutterite woman would have had in her place. Hutterites marital fertility rates are 0.55 for ages 20–24, 0.502 for 25–29, 0.447 for 30–34, 0.406 for 35–39, and 0.222 for 40–44 (Clark 2007). I exclude women for whom the Hutterite counterfactual does not produce more than one child. Individual controls include indicators of social position at age 15, age at marriage, birth order (excluding heirs), and an indicator for English titles. Marriage market controls include sex ratio and railway length. See Table C13 in this appendix for detailed descriptions. Columns [2], [4], and [6] use the first stage reported in Table C2, Panel B. Standard errors clustered by year are in parentheses; *** p<0.01, ** p<0.05, * p<0.1

Altogether, this suggests that the matching technology embedded in the Season had positive effects on match quality: it reduced the degree of consanguinity between spouses, and matched couples presented slightly better fertility records. The estimated effects, however, are much smaller than the effects on sorting by social position or riches. Marriages in the Season were, after all, not so much a matter of love as an important social and economic arrangement.

**Homophilic preferences.** Homophily—a preference for others who are like ourselves—may lead to assortative matching in the marriage market. In this subsection, I show that, in contrast to sorting by socio-economic status, sorting with respect to political ideology is explained by homophilic preferences and is not affected by search costs or marriage market segmentation.

Table C7 cross-tabulates the spouses' political ideology. To do so, I use the fact that most peers belonged to political clubs: Brook's, Reform, and Devonshire were liberal clubs, and Carlton, Jr. Carlton, Conservative, and St. Stephen's were Tory clubs (Bateman 1883: 497). Specifically, I proxy a husband's and a wife's ideology by the allegiance of the club they or their families belonged to.

In most cases, husband and wife shared the same political views. For example, 39 percent of liberal husbands married wives from liberal families, while under a random assignment, only 29 percent of them would have married a wife with the same ideology. Aggregate statistics confirm these patterns, although they also suggest that sorting in this dimension was not as strong as sorting by social position.

Figure C4 plots the probability of marrying a like-minded partner over time. Political endogamy seems to be independent of the number of attendees at royal parties, and it was not affected by the interruption of the Season. Therefore, I conclude that in contrast to sorting by socio-economic status, sorting by political ideology was driven mainly by homophilic preferences, independent of search costs and marriage market segmentation.

Table C7: Marriage and political preferences (1817–75)

| | | Wife's father or brother | | |
|---|---|---|---|---|
| Husband: | | *Liberal club* | *Tory club* | N |
| *Liberal club* | observed | 39.5 | 60.5 | 43 |
| | expected | 29.3 | 70.7 | |
| | difference | 10.2* | -10.2* | |
| *Tory club* | observed | 24.7 | 75.3 | 97 |
| | expected | 29.3 | 70.7 | |
| | difference | -4.6* | 4.6* | |
| N | | 41 | 99 | 140 |
| | | *Pearson chi-squared (1)*: | 3.15* | (0.08) |
| | | *Gamma test*: | 0.33 | (0.17) |

*Note*: The sample comprises all 142 peers and peers' sons who (1) first married in 1817–75; (2) are listed in (Bateman 1883); (3) belonged to a political club; and (4) married a wife who had a relative in a political club. I include individuals marrying from 1817 onwards to increase the sample size. Brook's, Reform, and Devonshire are liberal Clubs; Carlton, Junior Carlton, Conservative, or St. Stephen's are Tories (Bateman 1883: 497); *** p<0.01, ** p<0.05, * p<0.1

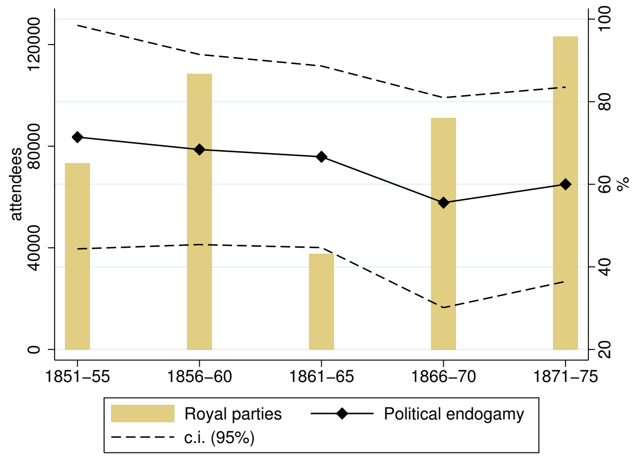



FIGURE C4: POLITICAL ENDOGAMY OVER TIME. Shaded bars show attendees at royal parties over five-year intervals (left axis). For the connected line, the sample is all 92 peers who (1) first married in 1851–75; (2) were listed by Bateman (1883); (3) belonged to a political club; and (4) married a wife with a relative in a political club. Diamonds display the percentage marrying a wife with a relative in a club with the same ideology (right axis).

## C. 5 Robustness

In this section, I examine the robustness of the results for men. I begin by estimating the reduced-form effect of the Season's interruption. Then, I use alternative measures of the London Season. Next, I explore the validity of the cohort size instrument. To do so, I first assess the bias of the estimates in case the cohort size instrument is "plausibly" exogenous—i.e., it has some correlation with unobservables that are influencing marriage outcomes. Second, I gauge the potential effect of unobserved variables using an insight from Altonji et al. (2005). Finally, I show that results are robust to a "classic" IV specification where all covariates (i.e., individual-level and time-varying covariates) are included in the first stage.

**Reduced-form effects of the interruption.** Here I provide reduced-form estimates of the interruption of the Season on marriage outcomes for men. Formally, i estimate

$$Pr\left(y_{i,t}|Interr\hat{u}ption_t, \mathbf{V}_t, \mathbf{X}_{i,t}\right) = \Phi\left(\beta\ Interr\hat{u}ption_t + \mathbf{V}_t'\lambda + \mathbf{X}_{i,t}'\delta\right)\ ,$$

where $y_{i,t}$ is a discrete outcome for individual $i$ marrying in year $t$ (e.g., married a commoner); $\mathbf{V}_t$ includes alternative predictors: sex ratios, the proportion of similar potential partners in the market, and the length of the railway network; and $\mathbf{X}_{i,t}$ is a vector of individual controls, including title, age at marriage, and birth order. For continuous outcomes (e.g., missmatch in acres) I estimate the analogous OLS regression.

Table C8 reports the results. As in the baseline estimates, I find that the interruption of the Season increases peer-commoner intermarriage for peers' daughters (col. [1]) and is also associated to peer-commoner intermarriage for men (col.

[2]). In contrast, when the Season functioned smoothly, men sorted more by acres (cols. [3] and [4]), by land rents (cols. [5] and [6]), and by a socio-economic index computed using p.c.a (col. [7]).

**Alternative measures of the Season.** Table C9 examines the robustness of my results to using alternative measures of attendance at the London Season. Column [1] reports the baseline estimates using attendees at all royal parties as the independent variable. Alternatively, column [3] uses the number of attendees at balls and concerts, the quintessence of the Season. The reported marginal effects are slightly stronger, except for those capturing the effect on geographic sorting (i.e., the probability that the wife is in the same region and the distance between spouses' family seats).

A potential weakness of my analysis is that noblemen who were hard-pressed to marry into well-positioned families could have been more eager to attend the Season. If this happened more when the size of the marriageable cohort was larger, the estimates from the equation systems (1) and (2); (1) and (3) would be biased. To account for this possibility, columns [2] and [4] use invitations issued to royal parties instead of the actual number of attendees. Marginal effects and standard errors are robust to this alternative measure of the Season.

**Plausibly exogenous instrument.** A key assumption of my identification strategy is that my instruments affect marriage outcomes only through the Season. In Section III.A in the paper, I argue that the interruption of the Season in 1861–63 satisfies the exclusion restriction. Here, I evaluate whether this is also the case for variation in the size of the cohort.

The Sargan tests in Tables C2 and C3 cannot reject the instrument's exogeneity. Note that the test assumes that at least one instrument is valid. Since the Season's interruption after the deaths of Victoria's mother and husband is arguably an exogenous shock, the Sargan test is informative for the cohort size instrument.

In addition, my results are not very sensitive to a hypothetical correlation between the size of the cohort and unobservables affecting marriage outcomes. To show this, I rewrite equations (1) to (3) as a two-stage least-square system:

$$A_t = \rho \; Cohort\ size_t + P\ (1861\text{–}63)_t + \mathbf{V}'_t\eta + \mathbf{X}'_{i,t}\delta + \nu_t$$

$$y_{i,t} = \beta\ \hat{A}_t + \mathbf{V}'_t\lambda + \mathbf{X}'_{i,t}\delta + \gamma\ Cohort\ size_t + \epsilon_{i,t},$$

where $\gamma$ is the direct effect of the size of the cohort on marriage outcomes—i.e., the effect that does not go through attendance at royal parties, captured by coefficient $\rho$. In this simple case, $\beta(\gamma) = \beta(\gamma = 0) + \dfrac{\gamma}{\rho}$, where $\dfrac{\gamma}{\rho}$ is the bias from violating the exclusion restriction. Table C10 reports the effects of the Season for different values of $\gamma$.

TABLE C8: Reduced-form effects of the interruption of the Season

| | Peer-commoner inter. | | Sorting by acres | | Sorting by land rents | | SES (pca) | Geographic endogamy | |
|---|---|---|---|---|---|---|---|---|---|
| | Married a common. [1] | Married a common. [2] | Wife same land class [3] | Mismatch [4] | Wife same land class [5] | Mismatch [6] | Mismatch [7] | Wife < 100 mi. [8] | Distance in miles [9] |
| **A.** Sample years: 1858–66 | | | | | | | | | |
| Interruption | 0.164** | 0.204** | -0.154*** | 13.018** | -0.461*** | 13.373** | 67.598** | 0.219** | -74.972*** |
| | (0.075) | (0.087) | (0.052) | (5.219) | (0.103) | (4.187) | (26.484) | (0.110) | (20.525) |
| Obs. | 279 | 356 | 101 | 63 | 101 | 63 | 62 | 60 | 62 |
| **B.** Sample years: 1851–75 | | | | | | | | | |
| Interruption | 0.179*** | 0.171* | -0.071 | 13.152** | -0.228** | 2.066 | 53.074*** | -0.091 | -58.128* |
| | (0.064) | (0.089) | (0.074) | (4.999) | (0.111) | (5.417) | (17.121) | (0.123) | (31.954) |
| Obs. | 775 | 979 | 257 | 171 | 257 | 171 | 170 | 167 | 167 |
| Individual co. | YES | YES | YES | YES | YES | YES | YES | YES | YES |
| Decade FE | YES | YES | YES | YES | YES | YES | YES | YES | YES |
| Trend | YES | YES | YES | YES | YES | YES | YES | YES | YES |
| Model | probit | probit | probit | OLS | probit | OLS | OLS | probit | OLS |

*Note*: Panel B also includes cohort controls. Standard errors clustered by marriage year in parentheses; *** p<0.01, ** p<0.05, * p<0.1.

Table C9: Alternative measures of attendance to the Season

| | [1] | [2] | [3] | [4] |
|---|---|---|---|---|
| | Independent variable capturing the effect of the Season: | | | |
| Dependent variable: | Attendees at royal parties (baseline) | Invited at royal parties | Attendees at balls and concerts | Invited at balls and concerts |
| Husband is a commoner | -0.003*** | -0.003*** | -0.005*** | -0.005*** |
| | (0.001) | (0.001) | (0.001) | (0.001) |
| Wife in same land class, acres | 0.008*** | 0.007*** | 0.008*** | 0.007*** |
| | (0.002) | (0.002) | (0.002) | (0.002) |
| Wife in same land class, rents | 0.009*** | 0.008*** | 0.009*** | 0.008*** |
| | (0.002) | (0.002) | (0.002) | (0.002) |
| Mismatch in acres | -0.224** | -0.21* | -0.266** | -0.245** |
| | (0.112) | (0.107) | (0.116) | (0.11) |
| Mismatch in land rents | -0.167* | -0.158* | -0.204** | -0.191** |
| | (0.086) | (0.082) | (0.084) | (0.078) |
| Mismatch in SES, using PCA | -0.740** | -0.685** | -0.897* | -0.827* |
| | (0.343) | (0.327) | (0.472) | (0.445) |
| Wife in same region | -0.007* | -0.007* | -0.005 | -0.005 |
| | (0.004) | (0.004) | (0.006) | (0.006) |
| Distance btw. spouses' seats | 1.787** | 1.668** | 1.407* | 1.266 |
| | (0.732) | (0.708) | (0.851) | (0.792) |

*Note*: This table reports IV marginal effects of the number of guests at the Season (in 100s of guests) on the corresponding marriage outcome in the rows. Each column defines guest in a different manner. Regression samples and covariates are described in Tables C2 and C3. First-stage results are reported in Table C2, Panel B. Standard errors clustered by year are in parentheses; *** p<0.01, ** p<0.05, * p<0.1

Across specifications, the estimated coefficients and the standard errors do not vary much when $\gamma < 0.1 \cdot \beta$ and $\gamma < 0.5 \cdot \beta$. The bias is meaningful only under a large violation of the exclusion restriction—i.e., when the direct effect of the cohort is almost the same as the effect of the Season. Although these results do not allow me to make inferences about my estimates, they suggest that for plausible small violations of the exclusion restriction, the cohort size instrument would still be valid.

**Assessing selection on unobservables.** Here, I evaluate the size of the endogeneity bias in the absence of instrumentation. Note that the IV and raw marginal effects reported in Tables C2 and C3 are quite similar, suggesting that the bias is, in fact, small.

The endogeneity bias will be large if there is substantial unobserved heterogeneity. To assess the potential effect of unobserved variables, I use the insight from Altonji et al. (2005) that selection on observables can be used to gauge the potential bias from unobservables. The strategy involves examining how much the coefficient of interest changes as control variables are added and then inferring how strong the effect of unobservables has to be to explain away the estimated effect. Formally, consider two individual regressions of the form $y_{i,t} = \beta\ A_t + \mathbf{V}_t'\lambda + \mathbf{X}_{i,t}'\delta + \epsilon_{i,t}$. In one regression, $\mathbf{V}_t$ and $\mathbf{X}_{i,t}$ include only a subset of control variables. Call the coefficient of interest in this "restricted" regression $\beta^R$. In the other regression,

Table C10: IV estimates for plausibly exogenous cohort size instrument

| | Baseline [1] | $\gamma = .1 \cdot \beta$ [2] | $\gamma = .25 \cdot \beta$ [3] | $\gamma = .5 \cdot \beta$ [4] | $\gamma = .75 \cdot \beta$ [5] |
|---|---|---|---|---|---|
| $\hat{\beta}(\gamma)$ for Husband is a commoner | -0.010*** | -0.012*** | -0.014** | -0.018** | -0.022** |
| | (0.004) | (0.004) | (0.005) | (0.007) | (0.01) |
| $\hat{\beta}(\gamma)$ for Wife in same acres class | 0.034*** | 0.038*** | 0.046*** | 0.057*** | 0.069*** |
| | (0.01) | (0.011) | (0.014) | (0.018) | (0.023) |
| $\hat{\beta}(\gamma)$ for Wife in same rents class | 0.031*** | 0.036*** | 0.042*** | 0.054*** | 0.065*** |
| | (0.006) | (0.007) | (0.009) | (0.013) | (0.017) |
| $\hat{\beta}(\gamma)$ for Mismatch in acres | -0.224** | -0.251** | -0.291* | -0.359* | -0.43* |
| | (0.112) | (0.127) | (0.150) | (0.190) | (0.230) |
| $\hat{\beta}(\gamma)$ for Mismatch in rents | -0.167* | -0.187* | -0.217* | -0.267* | -0.317* |
| | (0.086) | (0.097) | (0.115) | (0.144) | (0.174) |
| $\hat{\beta}(\gamma)$ for Mismatch in SES | -0.740** | -0.829** | -0.962** | -1.185** | -1.407** |
| | (0.343) | (0.390) | (0.463) | (0.6) | (0.712) |
| $\hat{\beta}(\gamma)$ for Wife in same region | -0.027* | -0.030* | -0.036* | -0.045* | -0.054* |
| | (0.015) | (0.017) | (0.020) | (0.025) | (0.030) |
| $\hat{\beta}(\gamma)$ for Distance between seats | 1.787** | 2.028** | 2.390** | 2.994** | 3.597** |
| | (0.732) | (0.840) | (1.009) | (1.304) | (1.606) |

*Note*: This table reports IV point estimates $\hat{\beta}(\gamma)$ for the effects of the number of attendees at royal parties (in 100s) on the corresponding marriage outcome in the rows. Each column assumes different values for $\gamma$, the direct effect of the cohort size instrument on marriage outcomes—i.e., $Y_{i,t} = \beta \hat{A}_t + \mathbf{X}'_{i,t}\lambda + \mathbf{V}'_t\delta + \gamma\ Cohort_t + \epsilon_{i,t}$ in the second stage described in Section C of the paper. Regression samples and covariates are described in Tables C2 and C3. First-stage results are reported in Table C2, Panel B. Robust standard errors are in parentheses; *** p<0.01, ** p<0.05, * p<0.1

covariates include the "full set" of controls. The corresponding coefficient is $\beta^F$. The ratio $\beta^F/(\beta^R - \beta^F)$ reflects how large the selection on unobservables needs to be (relative to observables) for results to become insignificant.

Table C11 suggests that the endogeneity bias arising from unobserved heterogeneity is small. Of the 24 ratios reported,[21] none is less than one. The ratios range from 1.2 to 68.5, with a mean ratio of 5.8. For example, when the restricted set of controls includes only time effects, the effect of unobservables would have to be ca. two times larger than the effect of the covariates to explain away the impact of the Season on the probability of peers' daughters marrying commoners.

**Classic IV specification.** In Section III, I use a triangular IV model in which the treatment and the instruments vary at the year level, whereas marriage outcomes are measured at the individual level. Specifically, I exclude the individual-level covariates from the first stage and estimate the recursive equation systems (1) and (2); and (1) and (3) using the STATA user-written command cmp (Roodman 2015). As opposed to ivprobit, the command cmp is suitable for triangular IV models.

Here, I estimate a "classic" IV model including all covariates in the first stage and show that results are robust. Formally, the number of attendees at royal

[21] Ratios for the distance between spouses' seats are not reported because Table C3 already makes clear that the endogeneity bias is strong in this dimension.

parties in a given year, $A_t$, is treated as an endogenous variable and modeled as

$$A_t = \mathbf{Z}_t'\rho + \mathbf{V}_t'\eta + \mathbf{X}_{i,t} + \nu_t \ , \tag{4}$$

where $\mathbf{Z}_t$ is the vector of instruments. It includes an indicator for the interruption of the Season (1861–63) and the size of the marriageable cohort—measured as the number of peers' daughters between ages 18 and 24 at year $t$, and an indicator for 1851, the year of the Great Exhibition. The vector $\mathbf{V}_t$ includes alternative predictors such as the sex ratio or the length of the railway network—a proxy for commuting costs. The vector also includes a time trend, and decade fixed effects, which, together with the an indicator for 1851 should account for the time effects described in Figure 3.

The effect of the Season on sorting along discrete characteristics (e.g., social class) is captured by coefficient $\beta$ in

$$Pr\left(y_{i,t}|\hat{A}_t, \mathbf{V}_t, \mathbf{X}_{i,t}\right) = \Phi\left(\beta\ \hat{A}_t + \mathbf{V}_t'\lambda + \mathbf{X}_{i,t}'\delta\right)\ , \tag{5}$$

where $y_{i,t}$ is an outcome for individual $i$ marrying in year $t$ (e.g., married a commoner) and $\phi$ is the CDF of the standard normal distribution. For continuous characteristics, I estimate

$$Y_{i,t} = B\ \hat{A}_t + \mathbf{V}_t'\Lambda + \mathbf{X}_{i,t}'\Delta + \epsilon_{i,t}\ , \tag{6}$$

where the dependant variable is the difference between husband's and wife's continuous characteristic $Y$ (e.g., difference in acres). In both models, $\mathbf{X}_{i,t}$ is a vector of individual controls, including social status, age at marriage, or birth order.

Table C12 presents the results of this IV model including all covariates in the first stage. Panel A presents the effect of the Season on the rate of intermarriage with commoners. As before, columns [1] to [3] only consider women. The results are consistent with those of Sections C in the appendix and III.B in the main text: the Season was a key determinant of sorting by social position among women. For example, using exogenous variation from the interruption of the Season alone (col. [2]), I find that increasing the number of atendees by five percent—250 additional attendees—decreases the probability of the average peer's daughter marrying a commoner by ca. one percentage point. Cols. [4] to [6] show that the Season is negatively associated with the rate of peer-commoner intermarriage for peers and peers' sons, although the IV effects are smaller than those for women and not significantly different from zero (col. [6]).

Panels B and C confirm that the effects of the Season on sorting by landed wealth are largely robust to this alternative specification. In the full specification, every 250 additional attendees would increase the chances of men marrying within the same wealth class by 2 (acreage) and 2.25 (land rents) percentage points. In cols. [4] to [6], I consider a continuous measure of sorting: the absolute value of the difference between spouses' acreage in percentiles. Every 250 additional attendees at royal parties would decrease this measure of mismatch by 0.6 (acres) and 0.4 (land rents).

Table C11: Using selection from observables to assess the selection on unobservables

| | | [1] | [2] | [3] | [4] | [5] | [6] |
|---|---|---|---|---|---|---|---|
| | | Husband is a commoner | Wife in same class | | Mismatch | | SES ranking using PCA |
| Controls in restricted set | Controls in full set | | acres | rents | acres | rents | |
| None | All | 1.2 | 1.8 | 1.5 | 1.2 | 1.2 | 2.5 |
| Time effects | All | 1.9 | 2.5 | 3.9 | 2.0 | 1.5 | 4.1 |
| Time effects + cohort controls | All | 8.0 | 5.8 | 68.5 | 6.1 | 3.4 | 3.8 |
| Time effects + class controls | All | 2.2 | 2.5 | 5.3 | 1.9 | 1.5 | 3.8 |

*Note*: Each cell reports ratios based on the coefficients for $Y_{i,t} = \beta\ A_t + \mathbf{X}'_{i,t}\lambda + \mathbf{V}'_t\delta + \epsilon_{i,t}$ from two individual-level linear regressions. $Y_{i,t}$ is the marriage outcome in the columns. $A_t$ is the number of attendees at royal parties (in 100s of attendees). In one regression, the covariates $\mathbf{X}_{i,t}$ and $\mathbf{V}_{i,t}$ include the "restricted set" of control variables, described in each row. Call the coefficient of interest in this "restricted" regression $\beta^R$. In the other regression, covariates include the full set of controls. Call the coefficient of interest in this full regressions $\beta^F$. The reported ratio is the absolute value of $\beta^F/(\beta^R - \beta^F)$ (Altonji et al. 2005). Time effects are a linear trend and decade fixed effects. Cohort controls are sex ratio and the proportion of similar partners in the market. Class controls include indicators of social position at age 15 in col. [1], acreage and land rents percentile in cols. [2] to [5], and all the socio-economic variables used in the PCA in col. [6] plus the proportion of similar partners in the market. Regression samples and the full set of controls are described in Tables C2 and C3.

Table C12: Robustness using standard IV

| | [1] | [2] | [3] | [4] | [5] | [6] |
|---|---|---|---|---|---|---|
| Panel A: | Spouse is a commoner | | | | | |
| | Women | | | Men | | |
| | probit | IVprobit | IVprobit | probit | IVprobit | IVprobit |
| Attendees Season (100's) | -0.003*** | -0.003*** | -0.003*** | -0.002* | -0.004** | -0.002 |
| | (0.001) | (0.001) | (0.001) | (0.001) | (0.002) | (0.002) |
| Observations | 775 | 279 | 775 | 979 | 356 | 979 |
| F-stat from first stage | . | 21 | 67 | . | 22 | 78 |
| % correctly predicted | 70 | 71 | 70 | 75 | 76 | 75 |
| Sargan test (p-val) | . | . | p=0.22 | . | . | p=0.28 |
| Panel B: | Wife in same land class (acres) | | | Mismatch (acres) | | |
| | probit | IVprobit | IVprobit | OLS | IV | IV |
| Attendees Season (100's) | 0.005*** | 0.003** | 0.008*** | -0.205 | -0.278*** | -0.227** |
| | (0.002) | (0.001) | (0.002) | (0.122) | (0.106) | (0.108) |
| Observations | 257 | 101 | 257 | 171 | 63 | 171 |
| F-stat from first stage | . | 19 | 70 | . | 18 | 81 |
| % correctly predicted | 84 | 83 | 84 | . | . | . |
| Sargan test (p-val) | . | . | p=0.33 | . | . | p=0.46 |
| Panel C: | Wife in same land class (land rents) | | | Mismatch (land rents) | | |
| | probit | IVprobit | IVprobit | OLS | IV | IV |
| Attendees Season (100's) | 0.008*** | 0.010*** | 0.009*** | -0.209** | -0.284*** | -0.167** |
| | (0.001) | (0.003) | (0.002) | (0.101) | (0.100) | (0.082) |
| Observations | 257 | 101 | 257 | 171 | 63 | 171 |
| F-stat from first stage | . | 19 | 68 | . | 18 | 82 |
| % correctly predicted | 77 | 80 | 77 | . | . | . |
| Sargan test (p-val) | . | . | p=0.96 | . | . | p=0.59 |
| Panel D: | | | | Mismatch (pca) | | |
| | | | | OLS | IV | IV |
| Attendees Season (100's) | | | | -0.66** | -1.48** | -0.75** |
| | | | | (0.32) | (0.71) | (0.33) |
| Observations | | | | 170 | 62 | 170 |
| R-squared | | | | | 0.5 | 0.5 |
| F-stat from first stage | | | | . | 15 | 87 |
| Sargan test (p-val) | | | | . | . | p=0.38 |
| Panel E: | Wife < 100 miles | | | Distance in miles | | |
| | probit | IVprobit | IVprobit | OLS | IV | IV |
| Attendees Season (100's) | -0.007*** | -0.004** | -0.007* | 1.169* | 1.514*** | 1.788** |
| | (0.003) | (0.002) | (0.004) | (0.622) | (0.275) | (0.719) |
| Observations | 167 | 60 | 167 | 167 | 62 | 167 |
| F-stat from first stage | . | 36 | 63 | . | 36 | 63 |
| % correctly predicted | 68 | 68 | 68 | | | |
| Sargan test (p-val) | . | . | p=0.12 | . | . | p=0.51 |
| Individual controls | YES | YES | YES | YES | YES | YES |
| Marriage market controls | YES | NO | YES | YES | NO | YES |
| Decade FE and trend | YES | YES | YES | YES | YES | YES |
| Sample years | 1851-75 | 1858-66 | 1851-75 | 1851-75 | 1858-66 | 1851-75 |

*Note*: Samples and covariates are defined in Tables C2 and C3. Standard errors are in parentheses; *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

In Panel D, I consider the pecking order defined in Section C. Specifically, my measure of sorting is the absolute value of the difference between the ranking of a male aristocrat and the ranking of his wife—i.e., when the measure is 0, it indicates that the *ith* ranked man is married to the *ith* ranked woman. When the measure takes larger values, it indicates less sorting. Whether I use exogenous variation in attendance due to the interruption of the Season, or the full set of instruments including changes in cohort size, I find that attendance at the Season increases sorting. Increasing attendance by 600 people would allow male aristocrats to marry a wife four to nine positions closer in the pecking order. Given that the mean difference between spouses' socio-economic rank is ca. 80 positions, this corresponds to a five to ten percent increase in sorting. These effects are very similar to those estimated in Section C using a triangular IV model.

Finally, Panel E shows that the results on geographic sorting are similar for this alternative specification. As before, for every 250 additional attendees, the probability of marrying a wife from within 100 miles decreases by 1-1.75 percentage points, and the distance between spouses' seats increases by around 4 miles.

## C. 6 Variable definitions

Table C13: Definition of main regression variables

| | |
|---|---|
| **Dependent variables** | |
| *Spouse is a commoner* | Spouse was neither a peer nor the son of a peer. Note that in Britain members of the gentry were considered commoners. |
| *Wife in same class (acres)* | Wife's family is in the same land class (acres). Classes are: >100,000 acres; 50,000 to 100,000; 20,000 to 50,000; 10,000 to 20,000; 6,000 to 10,000; and 2,000 to 6,000 (Bateman (1883): 495). |
| *Wife in same class (rents)* | Wife's family is in the same decile or a contiguous decile of the distribution of land rents. |
| *Mismatch (acres)* | Absolute value of the difference in spouses' acreage percentile. |
| *Mismatch (land rents)* | Absolute value of the difference in spouses' land rents percentile. |
| *Mismatch (PCA)* | Absolute value of the difference in SES ranking based on principal component analysis. |
| *Wife < 100 miles* | Indicator for spouses whose family seats are located within 100 miles. |
| *Distance btw. seats* | Aerial distance in miles between spouses' family seats. |
| **Independent variables and instruments** | |
| *Attendees (100s)* | Numbers attending royal parties during the London Season in a given year (in hundreds). |
| *Aged 22 in 1861* | Whether a woman was 22 on March 31, 1861—when the interruption of the Season started |
| *Interruption (1861–63)* | Indicator for years 1861–63, when the Season was disrupted by Queen Victoria's mourning. |
| *Marriage cohort size* | Number of peers' daughters aged 18–24 in a given year. |

Table C13: Definition of main regression variables (continuation)

| **Covariates** | |
|---|---|
| *Proportion of similar partners in the market (social status)* | Percentage of people of the opposite sex aged $\pm$ 2 years in the same social class as individual $i$. Social classes are (i) duke, marquis, or earl; (ii) baron or viscount; (iii) baronet; (iv) knight; and (v) commoner. |
| *Proportion of similar partners in the market (acres)* | Percentage of women aged 18–24 in the same class (acres) as landowner $i$. Classes are: >100,000 acres; 50,000 to 100,000; 20,000 to 50,000; 10,000 to 20,000; 6,000 to 10,000; and 2,000 to 6,000 (Bateman 1883: 495). |
| *Proportion of similar partners in the market (land rents)* | Percentage of women aged 18–24 in the same decile or a contiguous decile of the distribution of land rents as landowner $i$. |
| *Relative size of local marriage market* | Percentage of women aged $\pm$ 2 years whose family seat is in the same NUTS1 (England & Wales), electoral region (Scotland), or province (Ireland) as landowner $i$'s seat. |
| *Sex ratio* | Number of peers and peers' sons aged 19–25 to peers' daughters aged 18–24. When women are underreported, I assume that the number of women was 0.95 times the number of men born in the same year. |
| *Ratio of heirs to peers' daughters* | Number of peers' heirs aged 19–25 to peers' daughters aged 18–24. When women are underreported, I assume that the number of women was 0.95 times the number of men born in the same year. |
| *Railway length* | Miles of railway in Britain and Ireland in a given year (Mitchell 1988, Ch.10, Table 5). |
| *Social position* | Indicators for each of the following social positions: Commoner, Barons'/Viscounts' offspring, Dukes'/Marquis'/Earls' offspring, Barons'/Viscounts' heir (at age 15), and Dukes'/Marquis'/Earls' heir (at age 15). |
| *Birth order (exc. heirs)* | Birth order among all siblings excluding heirs. |
| *Acreage (percentile)* | Percentile in the distribution of acres from Bateman (1883) (i.e., percentile among all peers and peers' offspring owning more than 2,000 acres, worth £2,000 a year by 1876). |
| *Land rents (percentile)* | Percentile in the distribution of land rents from Bateman (1883) (i.e., percentile among all peers and peers' offspring owning more than 2,000 acres, worth £2,000 a year by 1876). |
| *Ownership of woods* | Whether the family was in possession of woods. |
| *Land since Henry VIII* | Indicates that the family is listed in Shirley's "Noble and Gentle Men of England" as holding land in England since the time of Henry VII. |
| *Age at marriage* | Age at marriage, based on birth and marriage dates. |
| *English title* | Father held a title in the peerage of England (as opposed to a Scottish or Irish peerage). |
| *Indicator for 1851* | Indicator for 1851, year of the Great Exhibition in London. |

## Appendix D. Additional descriptive evidence

This appendix discusses the importance of land as a source of wealth after the Industrial Revolution and compares the peerage and the top one percent in the United States today.

### D. 1 Landownership after the Industrial Revolution

Despite the advent of the Industrial Revolution, the lion's share of wealth was in the hands of the peerage (Cannadine 1990), who could be seen as the top one percent—or rather, the top 0.03 percent—of late-nineteenth century Britain. Here I present additional evidence suggesting that wealth was extremely concentrated in the hands of few landowners by the 1870s. Specifically, Figure D1 plots the percentage of people leaving £1,000,000 or more, by source of wealth. The data are from Rubinstein (1977). In the first half of the nineteenth century, 95 percent of all millionaires were landowners. The percentage remained around 80 percent until the 1870s. At the turn of the century, land was still the principal source of wealth. Although the share of millionaires who earned their fortunes in the manufacturing, food, drink, or tobacco industries grew over time, it was not until the twentieth century that this source of wealth became more important than landownership. The percentage of people earning their fortunes in commerce follows a similar pattern. Finally, professionals and public administrators always represented the smallest share of millionaires.

Several anecdotes illustrate this pattern. In the 1890s, the London estates of the Duke of Westminster were worth £14 million, at a time when the typical manufacturer left around £100,000 and the richest businessman left no more than £6 million. The catch-up process took a long time: businessmen began to leave estates exceeding that of Westminster only in the 1920s (Rubinstein 1977: 104).

This evidence is consistent with previous work analyzing the evolution of inequality in Britain in the long run. Long and Ferrie (2013) show low rates of occupational mobility in the nineteenth century. Clark and Cummins (2015) follow a different approach. Tracking wealth at death of people with rare surnames, they conclude that the top of the wealth distribution in England has remained largely unchanged since 1800. According to this view, the Industrial Revolution did not seem to accelerate social mobility.

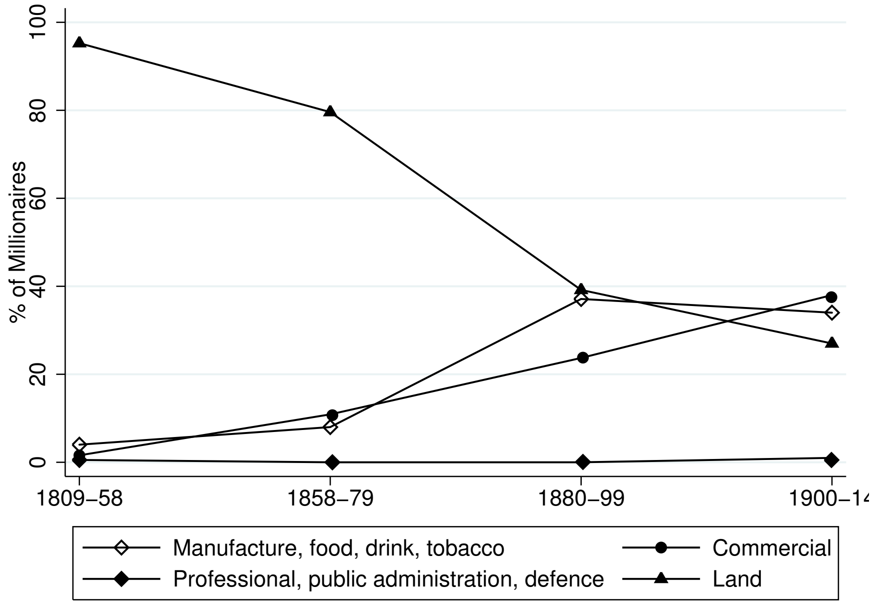



Figure D1: Top wealth-holders by source of wealth. The sample is all persons leaving £1,000,000 or more and landowners leaving estates worth over £33,000 per year (equivalent to £1,000,000 in 30 years), based upon Bateman (1883). Individuals are assigned to the occupation in which they made their fortune, based on biographical evidence. *Source*: Rubinstein (1977)

## D. 2 The peerage vs. the top 1 percent

Admittedly, this paper looks at a very specific population living in a very specific historical setting. Can the study of the British peerage in the nineteenth century tell us something about modern-day elites? Table D1 compares the nineteenth-century British peerage to the people at the top 1 percent of the income distribution in the United States today. Both elites are immensely rich. The average annual income of an individual in the top 1 percent is $1,300 thousand (Wolff 2013). The average British peer listed in (Bateman 1883) as a great landowner earned £23,400 a year. To calculate the equivalent in current US dollars, I first multiply this number by the percentage increase in the retail price index from 1870 to 2008. Then, I use a 1.5 pound-to-dollar exchange rate. This gives me an annual income of about $2,500 thousand. Peers' incomes were almost double those of the top 1 percent.

In terms of marriage outcomes, both elites are remarkably similar. According to Bakija et al. (2012), 12.5 percent of members of the top 1 percent never marry. The corresponding percentage for peers and peers' sons is 15.2. Interestingly, marital sorting is also similar across groups. Among the top 1 percent, 24 percent marry spouses in top-notch jobs: executives, managers, supervisors, financial professionals, lawyers, business operations specialists, scientists, entrepreneurs, and skilled salespeople (excluding real estate). This percentage is almost the same as the proportion of peers and peers' sons marrying a peer daughter between 1851 and 1875, 25.8 percent.

Finally, the top 1 percent and the peerage show similar support for liberal policies (by the standards of their times). Combined Gallup polls between 2009 and 2011 indicate that 20 percent of the very wealth identify themselves as liberals (Saad 2011). Similarly, 20.7 percent of peers listed in Bateman (1883) as great landowners were members of liberal clubs such as Brook's, Reform, and Devonshire (Bateman 1883: 497). The support for conservative policies, however, was clearer among peers; 55.2 percent of them belonged to Tory clubs (Carlton, Junior Carlton, Conservative, or St. Stephen's), while only 39 percent of the top 1 percent call themselves conservatives.

Table D1: The British peerage and the top 1 percent in the United States today

| US top 1 percent | | British peerage | |
|---|---|---|---|
| Not married (%) | 12.5 | Not married (%) | 15.2 |
| Spouse in top-notch job (%) | 24 | Spouse is peer daughter (%) | 25.8 |
| Liberal (%) | 20 | Member of a liberal club (%) | 21.0 |
| Conservative (%) | 39 | Member of a tory club (%) | 56.1 |
| Average annual income ($) | 1,300k | Rents from land (1870 £) | 23.4k |
| | | (2008 $) | 2,500k |

*Notes for the top 1 percent*: The sample for rows 1 and 2 is all households with incomes above $295,000 (excluding capital gains), as reported by the Statistics of Income division of the Internal Revenue Center in 2005 (Bakija et al. 2012: Table 4). Row 2 restricts the sample to married individuals. Rows 3 and 4 consider households surveyed in the Gallup polls in 2009–11 with incomes of at least $516,633, which corresponds to the top 1 percent in 2010 based on data from the Tax Policy Center. Row 4 uses households in the top 1 percent from the 2010 Survey of Consumer Finances (Wolff 2013)

*Notes for the peerage*: The sample for row 1 is peers and peers' sons born in 1820–45 living more than 35 years (i.e., marriageable in 1851–75). Row 2 considers peers and peers' sons first marrying in 1851–75. Rows 3 to 5 consider peers and peers' sons first marrying in 1851–75 and listed in Bateman (1883) as great landowners. See the text for details.

## Appendix E. Conceptual framework appendix

This section develops a basic conceptual framework for understanding the determinants of marital sorting. I construct a two-sided search model that builds on Burdett and Coles (1997) and incorporate endogenous market segmentation. Using this model, I derive testable implications for the effect of a reduction in search costs and an increase in market segmentation on the strength of marital assortative matching.

### E. 1 Baseline two-sided search model

Consider a market populated with a continuum of ex-ante heterogeneous men and women who wish to form long-term partnerships. Agents are characterized by their socio-economic status: $x$ for men and $y$ for women. Let $x$ and $y$ be distributed according to $F(x)$ and $G(y)$ over $[0, 1]$. The corresponding density functions are $f(x)$ and $g(y)$. All agents agree on how to rank one another. When a type $x$ man matches with a type $y$ woman, the former receives utility $y$ and the latter receives utility $x$,

$$
\begin{aligned}
u_x(y) = y \quad & \forall x \in [0, 1] \\
u_y(x) = x \quad & \forall y \in [0, 1].
\end{aligned}
$$

Therefore, I follow Collin and McNamara (1990), Smith (1995), Bloch and Ryder (2000), Burdett and Coles (1997), and Eeckhout (1999) and assume utility to be nontransferable.

Time is discrete and patience is determined by a discount factor $\beta > 0$. All men and women start their lives as singles, a state that yields no payoff. Because of search costs, it takes time for agents to meet. The rate at which contacts are made is determined by a matching function. Given the measures of men ($\lambda^m$) and women ($\lambda^w$), the number of encounters is given by $\alpha M(\lambda^m, \lambda^w)$, where $\alpha$ is the efficiency of the matching function and $M$ is increasing in both its arguments. I define $\mu_w(\lambda^m, \lambda^w, \alpha) = \dfrac{\alpha M(\lambda^m, \lambda^w)}{\lambda^w}$ as the encounter rate for single women (analogous for single men).

When two singles meet, they decide whether to propose or not. A match is formed when both propose to each other. These agents then leave the pool of singles but are automatically replaced by two clones. This guarantees that the distributions $G$ and $F$ are time invariant.[22]

I now define optimal behavior for any woman $y$. Although being single is undesirable, it does not necessarily mean that a woman will match with the first person she meets. It might be wise to wait until a proper proposal comes. Thus, $V(y)$, the value of being unmatched for women $y$, depends on the probability of

[22] In the context of the Season, this assumption is justified by the fact that when the daughter of a noblemen gets married, her younger sister replaces her by coming out in the Season.

eventually encountering "acceptable" agents. Formally,

$$(1-\beta)V(y) = \beta\,\mu_w(\lambda^m, \lambda^w, \alpha)\,\Omega(y)\int_0^1 max\left\langle \frac{x}{1-\beta} - V(y), 0\right\rangle dF(x|y)\ , \quad \text{(E.7)}$$

where $\Omega$ stands for the proportion of males who propose to her; $F(x|y)$ is the distribution of their socio-economic status; and $\frac{x}{1-\beta}$ is the value function for a woman of type $y$ married to a man of type $x$.

A woman $y$ will agree to marry a man $x$ if and only if $\frac{x}{1-\beta} \geq V(y)$; that is, she uses a reservation strategy. Note that reservation strategies will be nondecreasing in type. Consider two women with types $y_h$ and $y_l$, where $y_h > y_l$. Men accepting woman $y_l$ will also be willing to marry woman $y_h$. Therefore, she cannot fare worse in the marriage market than any lower ranked women—i.e., $V(y_h) \geq V(y_l)$. (Burdett and Coles 1997) used this argument to find a unique equilibrium, which they called the Class Partition Equilibrium:

**Proposition 1** *(Class Partition Equilibrium.) The marriage equilibrium consists of a sequence of strictly decreasing reservation strategies* $\{a^n\}_{n=0}^{N^w}$ *for women and* $\{b^n\}_{n=0}^{N^m}$ *for men, where* $a^0 = b^0 = 1$ *and for all* $n \geq 1$, $a^n$ *and* $b^n$ *satisfy*

$$a^n = \frac{\beta}{1-\beta}\mu_w(\lambda^m, \lambda^w, \alpha)\int_{a^n}^{a^{n-1}} [x - a^n] f(x) dx \quad \text{(E.8)}$$

*and*

$$b^n = \frac{\beta}{1-\beta}\mu_m(\lambda^m, \lambda^w, \alpha)\int_{b^n}^{b^{n-1}} [x - b^n] g(x) dx\ . \quad \text{(E.9)}$$

PROOF: This proof follows Burdett and Coles (1997) and goes by induction. For the basis step, consider the problem faced by the most desirable woman ($y = 1$). All men will propose to her, so $\Omega(1) = 1$ and $F(x|1) = F(x)\ \forall x$. Hence, her optimal reservation match is $r(1) = \frac{\beta}{1-\beta}\mu_w(\lambda^m, \lambda^m, \alpha)\int_{r(1)}^{1}[x - r(1)]f(x)dx$. The reservation strategy for the most attractive man, $\rho(1)$, is derived analogously. Note that $r(1) < 1$, $\rho(1) < 1$, $r(1) = a^1$, and $\rho(1) = b^1$ as defined in Proposition 1.

Note also that if the most desirable woman ($y = 1$) or man ($x = 1$) is willing to accept an individual, then that individual shares the same reservation strategy as the most desirable of her sex. Consider a man of type $x \in [r(1), 1]$. Since the most desirable woman is willing to marry him, all women will be willing to marry him, and, hence, $\Omega(1) = 1$ and $G(y|x) = G(y)\ \forall y$. This implies that $\rho(x) = \rho(1)$. The same is true for women of type $y \in [\rho(1), 1]$. Redefine $a^1 \equiv r(1)$ and $b^1 \equiv \rho(1)$. It follows clearly that men with $x \in [a^1, 1]$ and women with $y \in [b^1, 1]$ form an endogamic marriage class (class 1), in that agents in this class only marry members of this same class and reject all others.

Now, assume that for $n-1$, men with $x \in [a^{n-1}, a^{n-2}]$ and women with $y \in [b^{n-1}, b^{n-2}]$ form an endogamic marriage class (class $n-1$), in that agents in this class only marry members of this same class, reject individuals of lower type, and are rejected by those in class $n-2$.

For the inductive step, consider the most desirable women not in class $n-1$, $y' + \epsilon = b^{n-1}$ for an arbitrarily small $\epsilon > 0$. By the inductive assumption, she is rejected by class $n-1$ men. However, for all the men with $x < a^{n-1}$, she is the best available suitor. Thus, they all will propose to her. That is, $\Omega(y') = F(a^{n-1})$. The density function of these men under class $n-1$ is given by $\frac{f(x)}{F(a^{n-1})}$ for $x \leq a^{n-1}$. Substituting this into equation (E.7) yields:

$$r(y') = \frac{\beta}{1-\beta} \frac{\alpha M(\lambda^m, \lambda^w)}{\lambda^w} \int_{r(y')}^{a^{n-1}} (x - r(y')) f(x) dx \ .$$

Similarly, for men $x' + \epsilon = a^{n-1}$, $\Omega(x') = G(b^{n-1})$ and $\frac{g(y)}{G(b^{n-1})}$ for $y \leq b^{n-1}$. Thus,

$$\rho(x') = \frac{\beta}{1-\beta} \frac{\alpha M(\lambda^m, \lambda^w)}{\lambda^m} \int_{\rho(x')}^{b^{n-1}} (y - \rho(x')) g(y) dy \ .$$

Again, redefine $r(y') \equiv a^n$ ($\rho(x') \equiv b^n$), which denotes the lowest type man (woman) acceptable to the most desired women (man) not in class $n-1$. Since $r(\cdot)$ ($\rho(\cdot)$) is nondecreasing, all women (men) not in class $n-1$ will propose to a man (woman) with $x \geq a^n$ ($y \geq b^n$). Men satisfying $x \in [a^n, a^{n-1}]$ and women with $y \in [b^n, b^{n-1}]$ form marriage class $n$: they only accept each other, reject those of lower type, and are rejected by those in class $n-1$. *Q.E.D.*

Under this simple preference specification in which one's type affects her payoff only through whom she can match with, positive assortative matching arises naturally.[23] The highest ranked men and women form endogamic marriage classes, while individuals in the lower tail of the socio-economic distribution, although preferring to marry top partners, are "forced" together. Sorting will therefore be stronger in equilibria with a larger number of smaller classes.[24] I use this intuition to define the degree of sorting in a marriage equilibrium.

**Definition 1** *(Sorting) A marriage equilibrium $\{a^n\}_{n=0}^{N^w}$, $\{b^n\}_{n=0}^{N^m}$ displays a larger degree of sorting than an equilibrium $\{\hat{a}^n\}_{n=0}^{\hat{N}^w}$, $\{\hat{b}^n\}_{n=0}^{\hat{N}^m}$ if $a^n \geq \hat{a}^n$ and $b^n \geq \hat{b}^n$ for all $n$, holding with inequality for some $n$, and $N^i \geq \hat{N}^i$ for $i = m, w$.*

[23] More generally, a class partition equilibrium arises when utility is non-transferable and preferences are multiplicatively separable—i.e., $u_i(x, y) = f_1(x) \cdot f_2(y)$ for $i = x, y$ (Eeckhout 1999).

[24] To illustrate this, consider two extreme cases. If there is only one marriage class, all agents marry the first person they meet; the characteristics of your spouse are completely independent of your own. That is, there is no sorting at all. Instead, consider an equilibrium in which people only marry those who look exactly like themselves. In this case, there are an infinite number of "singleton" marriage classes, leading to perfect positive assortative matching.

## E. 2 Implications of a reduction in search costs

Because of search frictions, it takes time for agents to meet. Here I consider two ways in which the matching technology can increase the rate at which contacts are made, and, thus, reduce search costs: a more efficient matching technology and an increase in market depth. Formally, I assume that the encounter rate $\mu_i(\lambda^m, \lambda^w, \alpha) = \dfrac{\alpha M(\lambda^m, \lambda^w)}{\lambda^w}$ for $i = \{w, m\}$ is increasing in $\alpha$ and in $\lambda^i$. Thus, I depart from the standard assumption in the literature of constant returns to scale.[25] [26]

How is marital sorting affected by such reduction in search costs? The main trade-off that agents face in this model is between marrying sooner to enjoy marriage flow utility and waiting to get a proper match. As search costs are reduced, the value of waiting increases; singles reject more offers and end up forming a larger number of smaller classes in equilibrium. In other words, sorting increases. Figure E1 gives an example of how the class equilibrium changes as the matching technology becomes more efficient and as participation rates increase. In this example, populations are symmetric, $\lambda^m = \lambda^w = \lambda$, and types are uniformly distributed on $[0, 1]$, $F(x) = G(x) = x$. The matching function is $M(\lambda) = \lambda^2$, so the encounter probability is subject to increasing returns to scale. Equilibrium classes are, thus, $a^0 = 1$ , $a^n = a^{n-1} - \frac{\sqrt{1-\beta}}{\beta\alpha\lambda}\left(\sqrt{1-\beta+2\beta\alpha\lambda a^{n-1}} - \sqrt{1-\beta}\right)$. Assuming that $\lambda = 1$ and the discount factor $\beta$ is 0.8, an increase in the efficiency of the matching technology from $\alpha = 0.5$ to $\alpha = 1$ increases the number and decreases the size of the equilibrium marriage classes (Panel A). Similarly, an increase in the mass of participants from $\lambda = 1$ to $\lambda = 1.5$ also leads to more sorting in the marriage market (Panel B).

Proposition 2 generalizes this result:

**Proposition 2** *As the matching technology becomes more efficient (larger $\alpha$) and as the measure of men and women increases (larger $\lambda^m$, $\lambda^w$), the degree of sorting in equilibrium increases.*

Proof: This proof follows (Bloch and Ryder 2000). According to Proposition 1, class bounds are such that

$$a^n - \frac{\beta}{1-\beta}\frac{\alpha M(\lambda^m, \lambda^w)}{\lambda^w}\int_{a^n}^{a^{n-1}}(x - a^n)f(x)dx = 0 \ .$$

[25] Other models departing from the constant returns to scale assumption include Mortensen (1988), Chiappori and Weiss (2006), Anderberg (2002), and Gautier et al. (2010). In my context, this choice is justified by the fact that noble families from all over the country moved to London to get their offspring married, which hints at the existence of some sort of increasing returns to scale in the matching technology embedded in the Season.

[26] The clone replacement assumption—i.e., the fact that matched agents are automatically replaced by two clones in the pool of singles—is crucial in order to avoid multiple equilibria once I introduce increasing returns to scale.

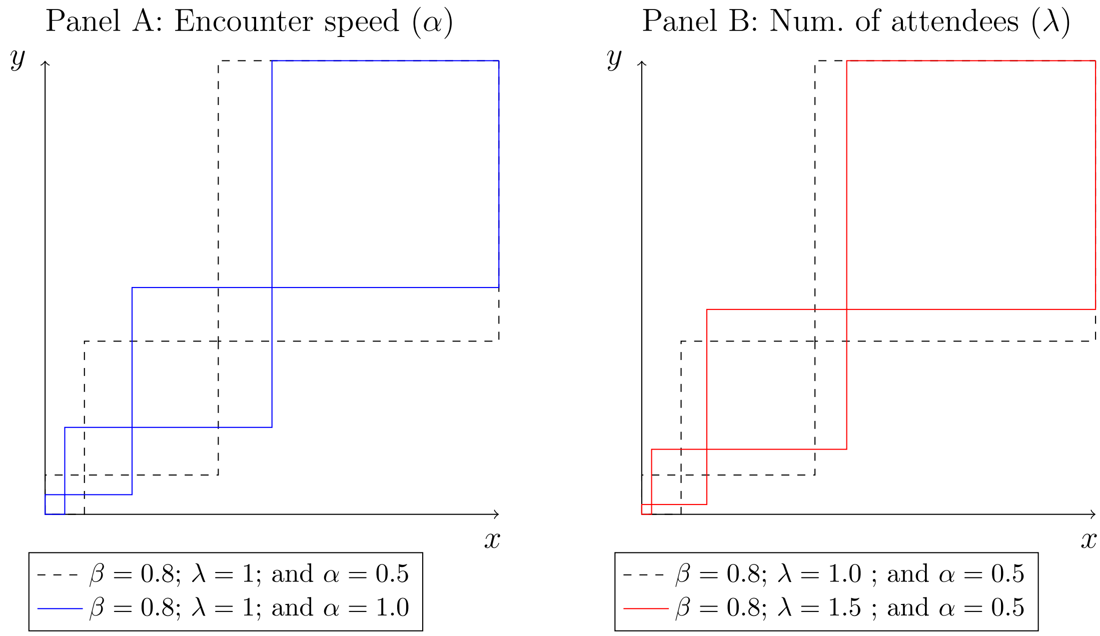



FIGURE E1: COMPARATIVE STATICS ON SEARCH COSTS

This figure displays the equilibrium defined in Proposition 1 with symmetric populations, $\lambda^m = \lambda^w = \lambda$, and types uniformly distributed on $[0, 1]$, $F(x) = G(x) = x$. The matching function is $M(\lambda) = \lambda^2$, so the encounter probability is subject to increasing returns to scale.

Using the implicit function theorem, the Leibniz integral rule, and some rearrangement, I find that

$$\frac{\partial a^n}{\partial \alpha} = \frac{\frac{\beta}{1-\beta}\frac{M(\lambda^m,\lambda^w)}{\lambda^w}\int_{a^n}^{a^{n-1}}(x-a^n)f(x)dx}{1+\frac{\beta}{1-\beta}\frac{\alpha M(\lambda^m,\lambda^w)}{\lambda^w}[F(a^{n-1})-F(a^n)]} \geq 0\ .$$

Similarly, if the matching technology is subject to increasing returns to scale—i.e., $\frac{\partial M(\lambda^m,\lambda^w)/\lambda^w}{\partial \lambda^w} > 0$ then

$$\frac{\partial a^n}{\partial \lambda^w} = \frac{\frac{\beta}{1-\beta}\alpha\frac{\partial M(\lambda^m,\lambda^w)/\lambda^w}{\partial \lambda^w}\int_{a^n}^{a^{n-1}}(x-a^n)f(x)dx}{1+\frac{\beta}{1-\beta}\frac{\alpha M(\lambda^m,\lambda^w)}{\lambda^w}[F(a^{n-1})-F(a^n)]} \geq 0\ .$$

The proof now goes by induction. For the basis step ($n = 1$), note that $\frac{\partial a^1}{\partial \alpha} > 0$ and $\frac{\partial a^1}{\partial \lambda^w} > 0$. Assume that for $n-1$, $\frac{\partial a^{n-1}}{\partial \alpha} \geq 0$ and $\frac{\partial a^{n-1}}{\partial \lambda^w} \geq 0$. For the inductive step note that

$$\frac{da^n}{d\alpha} = \frac{\partial a^n}{\partial \alpha} + \frac{\partial a^n}{\partial a^{n-1}}\frac{\partial a^{n-1}}{\partial \alpha}$$

and

$$\frac{da^n}{d\lambda^w} = \frac{\partial a^n}{\partial \lambda^w} + \frac{\partial a^n}{\partial a^{n-1}} \frac{\partial a^{n-1}}{\partial \lambda^w} \ .$$

By the inductive hypothesis, $\frac{\partial a^{n-1}}{\partial \alpha} \geq 0$ and $\frac{\partial a^{n-1}}{\partial \lambda^w} \geq 0$. Also, using the implicit function theorem, Leibniz integral rule, and some rearrangement, it can be shown that

$$\frac{\partial a^n}{\partial a^{n-1}} = \frac{\frac{\beta}{1-\beta} \frac{\alpha M(\lambda^m, \lambda^w)}{\lambda^w} (a^{n-1} - a^n) f(a^{n-1})}{1 + \frac{\beta}{1-\beta} \frac{\alpha M(\lambda^m, \lambda^w)}{\lambda^w} [F(a^{n-1}) - F(a^n)]} \geq 0 \ .$$

Therefore, $\frac{da^n}{d\alpha} \geq 0$ and $\frac{da^n}{d\lambda^w} \geq 0$. A similar argument shows that $\frac{db^n}{d\alpha} \geq 0$ and $\frac{db^n}{d\lambda^m} \geq 0$ for all $n = 1, ..., N^m$. *Q.E.D.*

Furthermore, if the increase in the encounter rate is large enough, the equilibrium might reach perfect assortative matching—i.e., the n$^{\text{th}}$ ranked woman marries the n$^{\text{th}}$ ranked man.

**Proposition 3** *(Adachi 2003) As search costs become negligible, the set of equilibria converges to the set of stable matches derived under the deferred acceptance algorithm (Gale and Shapley 1962), with perfect assortative matching.*

Proof: This proof follows Bloch and Ryder (2000). For ease of exposition, assume men and women are symmetric—i.e., $\lambda \equiv \lambda^m = \lambda^w$, and $F(x) = G(y)\ \forall x = y \in [0,1]$. I start by defining the set of stable matches under the deferred acceptance algorithm (Gale and Shapley 1962).

**Definition 2** *A matching is a one-to-one measure-preserving mapping from the set of men to the set of women. A matching is optimal if it maximizes total utility. A matching $\sigma$ is unstable if there exists a blocking couple (x,y) in which both x and y are individually better off together than with the agent to which they are matched under $\sigma$—i.e., $y > \sigma(x)$ and $x > \sigma^{-1}(y)$. The Gale-Shapley deferred acceptance algorithm yields a stable and optimal matching $\nu$.*

**Lemma 1** *Under the assumption than men and women are symmetric, the Gale-Shapley deferred acceptance algorithm yields a unique stable and optimal matching $\nu$ such that $\nu(x) = x$.*

To proof lemma 1, note that under symmetric populations and since one's type does not affects her payoff, any measure-preserving mapping is optimal. Formally, $\mathcal{U}_\nu = \int_0^1 x f(x) dx = \mathcal{U}_\sigma = \int_0^1 \sigma(x) f(x) dx$ for any measure-preserving matching $\sigma$, where $\mathcal{U}$ is the total utility.

Consider any measure-preserving matching $\sigma : [0,1] \to [0,1]$ such that $\sigma(x) \neq \nu(x)$. To show that such mapping $\sigma$ is not stable, I partition the set of men into three disjoint sets: those who are better or under $\sigma$, those who are assigned to the same women under $\sigma$ and $\nu$, and those that prefer their $\nu$ assignment.

$$X = \{x \in [0,1] : \sigma(x) > \nu(x)\}$$

$$Y = \{x \in [0,1] : \sigma(x) = \nu(x)\}$$

$$Z = \{x \in [0,1] : \sigma(x) < \nu(x)\}$$

Since $\sigma$ and $\nu$ are measure preserving and $\sigma(x) \neq \nu(x)$, $X$ and $Z$ have a positive measure. Now note that $\sigma^{-1}(x_0) = \sigma^{-1}(\nu(x_0)) = x_1$ can be interpreted as a mapping assigning to any man $x_0$ the man $x_1$ whom, under $\sigma$, is matched to $x_0$'s partner under $\nu$.

Clearly, $\sigma^{-1}(Y) = Y$, since these are the men whose assigned women do no change under $\sigma$ and $\nu$. Hence, $\sigma^{-1}(X \cup Z) = X \cup Z$. I now show that $\sigma^{-1}(X) \neq X$. Suppose $x_1 = \sigma^{-1}(x_0) \in X \ \forall x_0 \in X$. Then $\sigma(x_1) = \nu(x_0) > \nu(x_1)$. Since $\nu(x) = x \ \forall x$, $x_o > x_1$. Hence, $\sigma^{-1}$ would map $X$ into a proper subset of $X$. Therefore, for $\sigma^{-1}$ to be measure preserving, there must be a full measure $x \in Z : \sigma^{-1}(x) \in X$. But if $\sigma^{-1}(x) \in X$, then $x > \sigma^{-1}(x)$ so that woman $\nu(x) = x$ prefers $x$ to her match according to $\sigma$. Further, since $x \in Z$, $\sigma(x) < \nu(x)$ so man $x$ prefers woman $\nu(x) = x$ to his current match $\sigma(x)$. This couple $(x, x)$ is indeed a blocking couple, implying that $\sigma \neq \nu$ is unstable.

Finally, to show that $\nu(x) = x$ is stable, consider any blocking couple $(x, y) : y \neq x$. If $y > x$, then the women prefers $\nu^{-1}(y) = y$ to x. If $y < x$, it is the man who prefers $\nu(x) = x$ to $y$. This implies that the set of blocking couples for $\nu(x) = x$ is empty. This concludes the proof of lemma 1.

Once equipped with Lemma 1, it is straightforward to show that as search costs disappear, the marriage equilibrium converges to $\nu(x) = x$. According to Proposition 2, as $\alpha$ increases, marriage classes in equilibrium become smaller. Formally,

$$a^n = \frac{\beta}{1-\beta} \frac{\alpha M(\lambda)}{\lambda} \int_{a^n}^{a^{n-1}} (x - a^n) f(x) dx$$

is such that $\dfrac{\partial a^n}{\partial \alpha} \geq 0$. Similarly, using the implicit function theorem, the Leibniz integral rule, and some rearrangement,

$$\frac{\partial a^n}{\partial \beta} = \frac{\frac{\beta}{(1-\beta)^2} \frac{\alpha M(\lambda)}{\lambda} \int_{a^n}^{a^{n-1}} (x - a^n) f(x) dx}{1 + \frac{\beta}{1-\beta} \frac{\alpha M(\lambda)}{\lambda} [F(a^{n-1} - F(a^n)]} \geq 0 \ .$$

Now I show that $\dfrac{da^n}{d\beta} \geq 0$ by induction. Clearly, for $a^1$, $\dfrac{\partial a^1}{\partial \beta} > 0$. For any $n > 2$, $\dfrac{da^n}{d\beta} = \dfrac{\partial a^n}{\partial \beta} + \dfrac{\partial a^n}{\partial a^{n-1}} \dfrac{\partial a^{n-1}}{\partial \beta} \geq 0$ since $\dfrac{\partial a^n}{\partial \beta} \geq 0$, $\dfrac{\partial a^n}{\partial a^{n-1}} \geq 0$ as shown in the proof of Proposition 2, and $\dfrac{\partial a^{n-1}}{\partial \beta} \geq 0$ by the inductive hypothesis.

As search costs disappear, that is, as the matching efficiency $\alpha$ and the discount factor $\beta$ increase, the class bounds $a^n$ collapse to two sequences $\{x\}_{x\in[0,1]}$. The highest-type men and women $x = 1$ consequently adopt a threshold strategy such that they only match with agents of type $x = 1$. The highest ranked men and women not in class 1 again adopt a threshold strategy such that they only match with the highest ranked agents not in class 1. Iteration of this argument gives rise to $\nu(x) = x$, the unique stable and optimal matching derived by the Gale-Shapley deferred acceptance algorithm (Lemma A1). *Q.E.D.*

Two testable implications follow from propositions 2 and 3. When the London Season was well-attended, the children of the nobility should marry others who are closer in the "pecking order". Instead, when the Season was disrupted after the deaths of Prince Albert and Queen Victoria's mother, I expect to observe much less sorting in terms of social status and landed wealth.

### E. 3 Implications of market segmentation

In this section, I introduce endogenous segregation in the model and evaluate its effects on marital sorting. Although segregation in the marriage market is common, this feature is not usually incorporated into two-sided models of marriage search. Bloch and Ryder (2000) and Jacquet and Tan (2007) stand as notable exceptions.

Henceforth, for ease of exposition, I assume that the male and female populations are symmetric—i.e., that $\lambda^m = \lambda^w = 1$ and $F(x) = G(x)\ \forall x \in [0, 1]$. I introduce a market maker to the economy who proposes excluding the least desirable suitors from the marriage market by charging a participation fee $p$. Each agent can then decide whether to go to the exclusive marketplace and avoid meeting these suitors at a cost $p$ or to remain in the unrestricted marriage market. I call an equilibrium in which the least desirable suitors are excluded a segregation equilibrium.

**Definition 3** *A segregation equilibrium is a measurable subset* $(z, 1]$ *such that for all* $x \in (z, 1]$, $\tilde{V}(x) - p \geq V(x)$, *where* $\tilde{V}$ *and* $V$ *are the corresponding values of searching in the exclusive and the unrestricted marriage markets, respectively.*

Since the matching technology has increasing returns to scale, this model is subject to multiple equilibria. I show that a segregation equilibrium exists by constructing one. I first define the marriage equilibria in the unrestricted and exclusive markets under segregation. After that, I calculate the equilibrium fee $p^*$. Finally, I show that under segregation no agent has an individual incentive to switch from the exclusive to the unrestricted market, or vice versa.

Provided that the segregation equilibrium exists, the unrestricted marriage market is characterized by a mass $F(z)$ of individuals distributed according to $\dfrac{f(x)}{F(z)}$. The equilibrium takes the form of a class partition $\{a^n\}_{n=0}^{N}$ in which the cluster's bounds $a^n$ are defined according to Proposition 1. Similarly, the exclusive marriage market would be populated with $1 - F(z)$ individuals distributed over $\frac{f(x)}{1-F(z)}$. The equilibrium will also take the form of a class partition $\{\tilde{a}\}_{n=0}^{\tilde{N}}$.

The participation fee $p$ has to be such that agents of type $z$ do not want to switch to the exclusive marriage market. Note that a type $z$ agent would be the most desirable individual in the unrestricted market. Thus, her value of search there would correspond to the value of search in the top class $[a^1, z]$

$$V(z) = \frac{\beta}{1-\beta} \frac{\alpha M(F(z))}{F(z)} \int_{V(z)}^{z} (x - V(z)) \frac{f(x)}{F(z)} dx \ .$$

In contrast, in the exclusive marriage market, $z$ would be on the lowest class $[z, \tilde{a}^{\tilde{N}}]$, with a value of search of

$$\tilde{V}(z) = \frac{\beta}{1-\beta} \frac{\alpha M(1-F(z))}{1-F(z)} \int_{z}^{\tilde{a}^{\tilde{N}}} (x - z) \frac{f(x)}{1-F(z)} dx \ .$$

Therefore, for the segregation equilibrium to exist, the participation fee has to be such that $p^* = \tilde{V}(z) - V(z)$.

Now I show that with this $p^*$ and under the belief that types above $z$ participate in the exclusive marriage market, all agents of type $x < z$ have an individual incentive to remain in the unrestricted market. First, consider all agents in $[a^1, z)$. Following the intuition in Proposition 1, they will behave in the same way as $z$ in the unrestricted marriage market, since there they are desired by the highest-type of the opposite sex. So, the value of searching for a mate in the unrestricted market is such that $V(x) = V(z) = a^1$ for all $x \in [a^1, z)$. Alternatively, if agents in $[a^1, z)$ switched to the exclusive marriage market, they would at most be included in the last marriage class, as agent $z$. It could even be the case that $\tilde{a}^{\tilde{N}} > x$ for some $x \in [a^1, z)$, which means that nobody in the exclusive marriage market would marry them. In such a case, she would only marry agents of type $x < z$ who also had switched markets and therefore have a value of search $\tilde{V}(x) \leq \tilde{V}(z)$. Altogether, this implies that for all $x \in [a^1, z)$, $V(x) \geq \tilde{V}(x) - p^*$, and, thus, they prefer the unrestricted market.

This result is not so clear for men and women in the second class of the unrestricted market—i.e., $x \in [a^2, a^1)$. If, for example, the exclusive marriage market is such that $\tilde{a}^{\tilde{N}} < z$, it might be that some of these individuals of type $x \in [a^2, a^1)$ are $x > \tilde{a}^{\tilde{N}}$. In that case, they would be accepted by the lowest class within the exclusive marriage market, implying $V(x) < V(z) = \tilde{V}(z) - p^* = \tilde{V}(x) - p^*$. Therefore, in order to have a segregation equilibrium, it must be that $z = \tilde{a}^{\tilde{N}}$. If this assumption holds, then $\tilde{V}(x) < \tilde{V}(z)$, implying that $V(x) > \tilde{V}(x) - p*$ for all $x < a^1$. In other words, individuals of type $x < a^1$ also prefer to remain in the unrestricted market.

Finally, I show that no type with $x > z$ has an incentive to switch markets. Consider first the individuals of type $x \in [z, \tilde{a}^{\tilde{N}-1})$, that is, in the lowest marriage class of the exclusive market. For them, $\tilde{V}(x) = \tilde{a}^{\tilde{N}-1} = \tilde{V}(z)$. If they instead switch to the unrestricted marriage market, they will be the most attractive types there, in the top class. Thus, $V(x) = V(z)$. It then follows that $\tilde{V}(x) - p = V(x)$.

Since the equilibrium cluster's bounds $\tilde{a}^n$ are nondecreasing in $x$, for all $x > \tilde{a}^{\tilde{N}-1}$, the value of searching in the exclusive market is such that $\tilde{V}(x) > \tilde{a}^{\tilde{N}-1} = \tilde{V}(z)$. Then, $\tilde{V}(x) - p > \tilde{V}(z) - p = V(z) = V(x)$; that is, all types with $x > z$ prefer to pay the fee $p^*$ and attend the exclusive market. This concludes the construction of the segregation equilibrium. *Q.E.D*

To produce clear-cut comparative statics, I need to impose more structure on the matching technology. To see why, consider a technology where the fraction of the population that is matched increases too fast with respect to the measure of agents. In such a case, segregation will have two effects: first, it will reduce the number of participants and consequently the speed of encounters between remaining singles. Second, segregation will restrict the choice set and soften the congestion externality imposed by agents who meet but will never match. Since I am interested in understanding the second effect, I impose a limit on the degree of increasing returns to scale:

$$2\alpha \frac{M(\lambda)}{\lambda} \geq \alpha M_\lambda(\lambda) > \alpha \frac{M(\lambda)}{\lambda} \,. \tag{E.10}$$

Therefore, I assume that the matching technology is less than quadratic: the number of matches increases by a factor less than 4 when the number of participants in the market doubles (Jacquet and Tan 2007).

How would the marriage equilibrium in the exclusive marriage market be affected by an increase in segregation? Under equation E.10, segregation affects sorting through restricting the choice set to more similar individuals, but also by reducing the congestion externality. As agents do no longer meet others with whom they would never match, the rate at which singles meet proper types increases. This will increase the value of waiting, will lead to more rejections in the marriage market, and, finally, to an increase in sorting.

Figure E2 gives an example of how the class equilibrium changes as lower-ranked individuals are excluded from the market. The model is calibrated for the case of symmetric populations ($\lambda^m = \lambda^w$) and uniform distributions on $[0, 1]$, $F(x) = G(x) = x$. The matching function is $M(\lambda) = \lambda^{1.1}$, so the encounter probability is subject to increasing returns to scale but matched agents do not increase too fast with respect to the measure of agents—i.e., $2\alpha\frac{M(\lambda)}{\lambda} \geq \alpha M'(\lambda) > \alpha\frac{M(\lambda)}{\lambda}$. Equilibrium classes in the exclusive marriage market are, thus, $\tilde{a}^0 = 1$, $\tilde{a}^n = \tilde{a}^{n-1} - \frac{(1-z)^2\sqrt{1-\beta}}{\beta\alpha M(1-z)}\left(\sqrt{1-\beta + 2\frac{\beta\alpha M(1-z)}{(1-z)^2}\tilde{a}^{n-1}} - \sqrt{1-\beta}\right)$. Assuming that $\lambda = 1$ and the discount rate $\beta$ is 0.8, an increase in the segmentation from $\alpha = 0$ to $\alpha = 0.24$ leads to more sorting: on the one hand, the choice set is restricted to more similar individuals. On the other hand, the congestion externality, the time agents spent meeting types $x < z$ with whom they will never marry, is reduced. This leads to an increase in class bounds, and, therefore, to an increase in sorting in the exclusive marriage market.

Proposition 4 generalizes this result:

**Proposition 4** *As segregation increases (larger z), the degree of sorting in equilibrium increases.*

Proof: From Proposition 1, it is clear that marriage classes in the exclusive market are defined such that:

$$\tilde{a}^n - \frac{\beta}{1-\beta}\alpha\frac{M(1-F(z))}{[1-F(z)]^2}\int_{\tilde{a}^n}^{\tilde{a}^{n-1}}(x-\tilde{a}^n)f(x)dx = 0 \ .$$

Using the implicit function theorem, Leibniz integral rule, and some rearrangement, I find that

$$\frac{\partial \tilde{a}^n}{\partial z} = \frac{f(z)\frac{\beta}{1-\beta}\frac{1}{[1-F(z)]^2}\left[\frac{2\alpha M(1-F(z))}{1-F(z)} - \alpha M_\lambda(1-F(z))\right]\int_{\tilde{a}^n}^{\tilde{a}^{n-1}}(x-\tilde{a}^n)f(x)dx}{1+\alpha\frac{\beta}{1-\beta}\frac{M(1-F(z))}{[1-F(z)]^2}\int_{\tilde{a}^n}^{\tilde{a}^{n-1}}(x-\tilde{a}^n)f(x)dx} \ .$$

Since, by assumption $\frac{2\alpha M(1-F(z))}{1-F(z)} \geq \alpha M_\lambda(1-F(z))$, it follows that $\frac{\partial \tilde{a}^n}{\partial z} \geq 0$.

The proof now goes by induction. For the basis step $(n = 1)$, note that $\frac{\partial \tilde{a}^1}{\partial z} \geq 0$. Assume that for $n-1$, $\frac{\partial \tilde{a}^{n-1}}{\partial z} \geq 0$. For the inductive step note that

$$\frac{d\tilde{a}^n}{dz} = \frac{\partial \tilde{a}^n}{\partial z} + \frac{\partial \tilde{a}^n}{\partial \tilde{a}^{n-1}}\frac{\partial \tilde{a}^{n-1}}{\partial z} \ .$$

By the inductive hypothesis, $\frac{\partial \tilde{a}^{n-1}}{\partial z}$. Also, as shown in the proof of Proposition 2,

$$\frac{\partial \tilde{a}^n}{\partial \tilde{a}^{n-1}} = \frac{\frac{\beta}{1-\beta}\frac{\alpha M(1-F(z))}{1-F(z)}(\tilde{a}^{n-1}-\tilde{a}^n)f(\tilde{a}^{n-1})}{1+\frac{\beta}{1-\beta}\frac{\alpha M(1-F(z))}{1-F(z)}[F(\tilde{a}^{n-1})-F(\tilde{a}^n)]} \geq 0 \ .$$

Therefore, $\frac{d\tilde{a}^n}{dz} \geq 0$. *Q.E.D.*

In the empirical analysis, I test Proposition 4 by looking at how marriage behavior responded to the interruption of the Season after the death of Prince Albert and Queen Victoria's mother. Because royal parties were canceled, poor and insignificant suitors were not fully screened out, that is, marriage market segmentation was reduced.

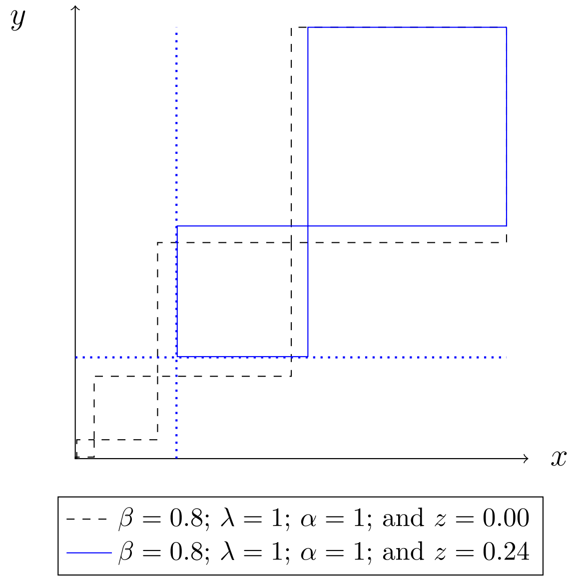



FIGURE E2: COMPARATIVE STATICS ON MARKET SEGMENTATION

This figure displays the equilibrium in the exclusive marriage market when a matchmaker can induce segregation by imposing a participation fee $p$. In this example, populations are symmetric, $\lambda^m = \lambda^w = \lambda$, and types are uniformly distributed on $[0, 1]$, $F(x) = G(x) = x$. The matching function is $M(\lambda) = \lambda^{1.1}$, so the encounter probability is subject to increasing returns to scale but matched agents do not increase too fast with respect to the measure of agents—i.e., $2\alpha\frac{M(\lambda)}{\lambda} \geq \alpha M'(\lambda) > \alpha\frac{M(\lambda)}{\lambda}$.

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