The dynamics of institutional change: Evidence from 19th century Britain*

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Abstract

How do institutional reforms work at a micro-level? I collected and digitised individual-level data on thousands of entrants to the British Civil Service from 1864-1875. I study an 1870 reform that forced exam-based hiring on a large part of, but not all, the public sector. Patronage use declined among treated organisations but remained an important hiring procedure. I consider the extent of compliance both across- and within-organisations. The degree of compliance was determined by the organisations' perception of examinations as a measure of future skill in the role. Organisations with more routine labour functions adopted examinations, those without did not. Political bargaining power appears to be pivotal in allowing non-routine organisations to evade legislation due to the presence of grey areas in the original reform.

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1 Introduction

Understanding the frictions between de jure policies, which target reform, and de facto political interests, that seek to preserve entrenched powers, remains a primary challenge for those seeking to understand the long-run evolution of institutions and state capacity. Bureaucracy is a crucial component of state capacity (Besley et al. 2022). Efforts to improve the quality of bureaucracy are multi-faceted (World Bank 2000), yet a crucial approach is to introduce merit-based selection procedures to replace patronage systems that often prioritise rent-seeking above public service delivery (Robinson and Verdier 2013).[1] Meritocratic systems are conventionally associated with higher levels of state capacity and public good provision (Rauch and Evans 2000). As such, they are seen as crucial to cementing strong bureaucratic institutions that are free from political overreach (Johnson and Libecap 1994). While the shift towards meritocratic public service is seen as central to institutional change in history, there has been limited empirical exploration of how this process occurred in practice.

In this paper, I study the first meritocratic public service reform implemented in Britain, the Order in Council of 1870. The reform replaced patronage with competitive examinations as the stock method of appointment for entry into the British civil service. It did so for the majority of, but not all, departments in the civil service.[2] The central contribution of this paper is to move beyond the general (or average) effects of institutional reform in order to consider why organisations react differently to its implementation. What motivated individual organisations in history to comply with reformist efforts, and where did they resist? Was this variation just across-organisations, or was it also within them? To study this, I collect individual-level data on the British Civil Service between 1864 and 1875. I digitised 11 years of data from the *Reports of the Civil Service Commission*. The reports provide the universe of Civil Service entrants at the clerk-level, and allow me to observe their route of entry (patronage or merit) consistently throughout the period. This individual-level data enables me to construct a unique dataset of nearly 10,000 civil service entrants. This is the first time the reports have been assembled into a single individual-level dataset.

Empirically, the chosen reform has several empirical properties that make it desirable for my chosen question. The first is the depth of data, which is gathered across the British Civil Service rather than a subset of organisations. Second, there is non-uniform reform imposed by the government upon its public sector organisations. This is rare, particularly in the modern day, as one would typically exercise such a reform unilaterally. This provides a natural control group of untreated organisations. In order to assure that the common trends assumption is met, I provide balance tests, event studies and qualitative evidence. The governing Liberal Party saw the reform as somewhat of an experiment. William Gladstone, Prime Minister at the time, stated in Parliament, the reform was implemented on 'a scale quite sufficient, even if there should be exceptions, to enable the public to test its principle on a perfect scale' (Parliament of the United Kingdom 1870b). This allows me to formally analyse heterogeneity in resistance to reform. There were two legal ways in which many reformed departments could, with some effort, continue to use patronage for each individual appointment. As such, there was a clearly defined route to resistance. Finally, and perhaps

[1] The improvement delivered by merit systems has a long intellectual heritage in the Weberian ideal of an impartial bureaucracy whose lack of vested interests leave public service as the primary concern rather than the narrow interests of a clientelist relationship (Weber 1922). Historically, it has been well noted that bureaucracies operated through patronage, and one of the crucial developments in improving state capacity has been this switch away from narrowly defined interests (Kettering 1988).

[2] Throughout the paper I refer to organisations as 'departments' given that this was the historically-used term in the British Civil Service – given the preference for organisations in the contemporary literature I use the two interchangeably.

most importantly, due to unique regulations, there is no reason to believe that patronage hires are 'missing' from the records. As such, it is plausible that these records constitute the true universe of entrants, and thus allow identification that in other settings would be clouded by sample bias.

I use a difference-in-differences strategy to estimate the effect of reform. This allows me to determine the success of policy through the comparison of reformed and unreformed departments (organisations, referred to as departments henceforth for historical accuracy). By exploiting *within-department* variation I provide an average treatment effect on the treated (ATT). This DiD approach also enables me to test potential mechanisms by exploring heterogeneous treatment effects using a triple difference specification. I initially look at the effect of reform alone. Before reform, over 99 percent of entrants to the civil service entered through a form of patronage, suggesting common trends. Exploiting within-department variation, and using both individual-level and panel-data, I find that *reformed* departments did in fact reduce patronage use relative to the control group of unreformed departments. Reformed departments were 34-40 percentage points less likely to have an patronage entrant than unreformed after 1870. Panel estimates support this, suggesting that the proportion of new hires entering through open competition increased by 33-38 percentage points. This is despite evidence of positive spillovers from the treated to the untreated group which attenuate any estimates, but do not challenge a causal interpretation. This makes clear that top down policy did have immediate positive effects on meritocratic selection in the Civil Service. Event study estimates suggest no violation of common trends due to pre-treatment trends, supporting a causal interpretation. This coefficient, however, does not indicate that selection had become entirely homogenised, as was the stated policy goal. Patronage hires continued, albeit in a reduced manner. Back of the envelope calculations indicate that they made up around 30-50% of all civil service hires annually after reform.

I then derive a simple model to explain why, and where, patronage remained a popular method of appointment after reform. The model focuses on what motivates bureaucrats to appoint through patronage versus examinations. I use this to guide my empirical analysis, which seeks to understand, both across- and within-departments, why organisations switched to open-access examination hiring as well as why certain organisations maintained their previous hiring behaviour (patronage). I point to a traditional channel, information, as crucial to this decision. Organisations across the public sector have a wide variety of tasks. Within one organisation this is also true. Where examinations offer more credible information on a candidates' potential skill in the role it is more likely that departments would shift towards their usage. On the other hand, where exams were seen as a lesser measure relative to private information, patronage may continue. To operationalise this, I consider differences in labour function, denoted as routine and non-routine tasks (Acemoglu and Autor 2011). I find that departments with more routine hires exhibit much greater adoption of examination hiring. Departments with particularly low shares of routine hiring continue with their previous rates of patronage use, suggesting that their resistance was informed by a belief that exams did not serve as an apt signal. I find that this is also true when considering differences across positions within the same department. Jobs that were routine in nature exhibit large jumps in examination use after treatment, but more complicated roles with less defined tasks do not. In other words, departments made rational decisions that maximised their own view of what they required from entrants. Such an explanation fits with the existing scholarship on the development of the British Civil Service, which stresses concerns with using examination hiring for the positions that required interpersonal skills, policy origination and confidentiality (Allen 2005; Keir 1938; Roach 1971).

I find a complementary relationship with political connections (as a proxy for de facto power) at the department level. These appear to inform the degree of resistance possible. Departments that were more connected to government had substantially higher rates of patronage relative to the treatment group, and were able to invoke politically costly loopholes ('grey areas') at a greater rate. Specifically, if departments were in the lowest quartile non-routine the degree of political connection held, using Whitehall headquarters as a proxy, led to a nearly one hundred percent increase in the relative probability a candidate entered through patronage. I find no evidence for other mechanisms.

These results provide a nuanced answer as to the dynamics of institutional reform from a micro-level in the British case. Those state organisations that comply with reform most are likely inherently motivated to do so. In the case of examination requirements, this meant that organisations who felt that examinations better matched their labour functions, both generally and for specific roles, complied. Organisations who disagreed with this premise, and felt the private information allowed through patronage was a better measure of future performance, did not comply. I suggest that the degree of bargaining power held by an organisation permitted the extent of non-compliance, given that grey areas in reform design were the main way in which an organisation could evade examination hiring. A clear policy solution does not follow cleanly, for success reforms might either specifically target this heterogeneity, or adopt the strategy of successive reform followed by the British. That this took until the 1930's to achieve full success (with the Warren Fisher Reforms) might concern those attempting to instigate policies in politically unstable environments. Generally, the results confirm that institutional reforms have nuanced effects across the public sector which require empirical research of their own.

Related Literature

My results contribute to the existing literature on institutions (Acemoglu et al. 2001; North 1991). Public sector efficiency is central to institutional theories of growth, yet there is limited empirical research into the transition to a modern, professional bureaucracy. I focus on the implementation of examination requirements, which is a common reform in history. There is existing work which considers the effects of similar reforms (e.g. Bostashvili and Ujhelyi (2019) or Moreira and Pérez (2022)), but not which studies the adoption of such a reform across the public sector. I first show that reform does have clear effects on selection through my baseline results regarding patronage use. Then I make clear that organisations implement reforms in accordance to their own functions and beliefs about what a desirable candidate is. I also show that power, and political capital, holds an important role in the ability for organisations to resist change, which concurs with the idea proposed for social classes in Acemoglu and Robinson (2006) that when de jure policy curtails previous privileges elites are still able to maintain influence through de facto means. This provides considerable clarity on the mechanisms that underlie institutional change in history.

These results also contribute to a complementary literature on the personnel economics of the state (Finan et al. 2015). This literature typically focuses on observational studies which quantify the public service effects of discretionary (patronage) versus merit appointments (Colonnelli et al. 2020; Xu 2018), or provides experimental evidence on how different adjustments can improve selection and on-the job performance (Ashraf et al. 2020; Deserranno 2019). I provide clear micro-level evidence on how reform against patronage operates. Where my paper differs from previous studies, specifically those of Moreira and

Pérez (2024) and Aneja and Xu (2024), is that I consider the public sector as a whole not specific organisations. This allows me to provide estimates on the heterogeneous effect of reform that exploit differences *across*, as well as within, organisations. My evidence suggests that conventional economic reasoning applies to the decision to comply with reform, where organisations feel that examinations provide tangible benefits they will comply and otherwise they will attempt to resist. I also concur with the findings of Moreira and Pérez (2024) in that 'grey areas' in legislative design become the target of resistance efforts. I show that this is the case across different departments, and provide a clear explanation for which departments comply and which resist, as well as the positions they seek to protect patronage privileges over.

Lastly, the paper provides a novel contribution to British economic history. There has been renewed interest in the development of the British state at the local level over the 18th and 19th centuries (Besley et al. 2025; Heldring et al. 2026). Yet, there has been limited study of the shift towards a professional bureaucracy (Allen 2005). The only existing work to quantitively evaluates meritocratic reforms in Britain is that of Xu (2018) on the Warren Fisher Reforms which focuses on senior colonial governors rather than entrants across the civil service. My results provide clear quantitative evidence that the Order in Council brought about revolutionary change in the selection procedure for entry level civil servants. This confirms earlier historical research (Chapman 2004; Roach 1971), although my results indicate that this shift was not as comprehensive, at least not immediately, as others have suggested (Keir 1938, p.425). My paper goes a step beyond existing historical research in that I provide evidence on where the reform failed to be adopted. I show that this was mainly due to departments of the civil service making the decision based on their labour needs, explaining the need for several follow-on reforms to correct non-compliant departments.[3] This helps to combine a popular explanation of why patronage was felt to be helpful, so that departments could hire those with the *right stuff*, with its (partial) persistence in the immediate period after reform (Allen 2005).

2 Historical background

2.1 The Civil Service in the 19th Century

The British public sector grew enormously from the start of the 19th century. In particular offshoots of the service like the Post Office or Customs had become increasingly large. To illustrate this, I take the number of registered civil servants in the (anonymised) British census microdata between 1851 and 1911. I present the results in Figure 1. In 1851 there were 65,520 people who registered an occupation in a category related to public service, whereas in 1911 there were 294,823. This is a nearly five-fold increase in 60 years, making clear the scale of expansion over time. Yet there is substantial heterogeneity between local public servants and national bureaucrats. The former grew enormously, as the state began to move into more traditional frontline public services such as policing and postal service (Hanlon 2024). The number of national bureaucrats, which is the group focused on here, enjoyed steady but less spectacular growth.

[3] In this way, the findings support the general theory 'of institutional change in the civil service [as] one of incremental alteration' proposed for the American experience (Johnson and Libecap 1994, p.9).

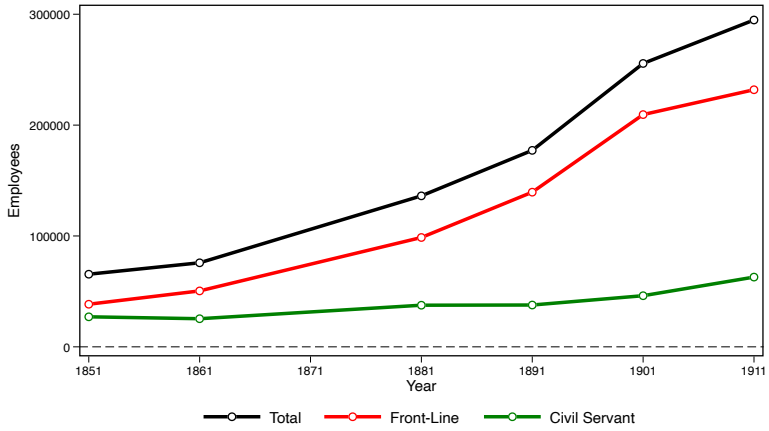


Figure 1: Growth of the public sector 1851-1911

Notes: The figure presents the total number of public sector employees in Britain between 1851-1911 in six census waves, built using anonymised census microdata from the I-CeM project (Schurer and Higgs 2023). The 1871 year is omitted owing to the loss of the census for England and Wales in this year.

2.2 Hiring in the Civil Service

Pre-1870. The focus of this paper is on hiring behaviour in the civil service for entry level positions. Historically, discretionary (patronage) appointments had been the unanimous selection procedure. Its method of appointment was entirely contingent on the exchange of social capital in return for position, junior and senior, in the pre-1855 period (Plumb and Trevelyan 1955; Chester 1981). Allen points to 'the ubiquitous and everyday feature of patronage and sale of public office among western countries from the twelfth until the nineteenth century', to which Britain was no exception (Allen 2005, p.58). This is not to argue that there had not been some significant changes over time, as 'patronage [formalised] ... into a system of written recommendations' over the 18th century (Aylmer 1980, p.94-95). This system held its advantages. In certain roles, patronage limited informational asymmetries and allowed access to better candidates (Voth and Xu 2021). There was also an inherent enforcement mechanism, where 'removal from a patronage office often resulted in social ostracism' (Allen 2005, p.61). Chapman (2004, p.11) also refers to this perception that patronage 'helped produce a body of trustworthy civil servants, animated by a certain *esprit de corps* and by useful official traditions'. This was combined with a contemporary belief that 'it was desirable to maintain class divisions in society and government' through the continued elevation of higher class entrants to public service work (Roach 1971, p.192).

There were always negative sides to patronage, but its major problem was that it could work 'only when the civil administration was small' (Allen 2005, p.63). As the service grew, there were only so many social network connections to be exploited and the reputational enforcement mechanism weakened in its consequences. This led to more incompetent civil servants who were less afraid to focus on their own enrichment, which became seen as increasingly detrimental to state performance.[4] This led to a predominantly middle class reformist movement (as with the Reform Act 1832 and Municipal Corporations

[4]Aylmer writes: 'it was - I suggest this growth in scale, including of course the escalation of the National Debt, which provided the decisive impetus towards both critical and constructive ideas for reform of the public service, and which eventually made 'persons in offices' into 'civil servants'. Like many victorious campaigns, that for administrative reform was at least half-won before it was ever fought.' (Aylmer 1980, p.108).

Act 1835) that demanded the end of discretionary appointments perceived to directly favour the aristocracy (Anderson 1965; Anderson 1967; Stansky 1973). Proponents of a merit system championed the seminal Northcote-Trevelyan report of 1854, which concluded:

"Admission into the Civil Service is indeed eagerly sought after, but it is for the unambitious, and the indolent or incapable, that it is chiefly desired... the comparative lightness of the work, and the certainty of provision in case of retirement owing to bodily incapacity, furnish strong inducements to the parents and friends of sickly youths to obtain for them employment in the service of the Government" ("The Northcote-Trevelyan Report" 1854)

Reforms instituted in 1855 brought examinations, but only in a limited form as 'heads of departments were still permitted to decide whether vacancies should be filled by simple nomination or by limited competition among nominees' (Roach 1971, p.27). Nominations, here referred to as direct appointments, required a certificate of approval from the Civil Service Commission, but it was well known that this was only withheld in extreme cases such as illiteracy or severe personal issues. Even when an examination was chosen, its participants were limited to those able to get a nomination from a given department, which perpetuated a social capital based appointment system (MacDonagh 1958; Hughes 1942).[5] This nomination system was little better because 'very few candidates, and they often of a low educational standard, were nominated for the vacancies' (Roach 1971, p.205).[6] In a 1869 parliamentary debate on the principle of open competition in the civil service, Colonel William Sykes recalled that while reviewing appointments as part of a select committee 'some scandalous abuses of patronage were brought before them... in one case it was stated that a lunatic had been appointed to a clerkship, and in another a greengrocer' (Parliament of the United Kingdom 1869). An examination infrastructure existed, where the Civil Service Commission administered a preliminary and final exam with the assistance of typically Oxford or Cambridge professors. Both oral and written questions were given in both rounds. Yet, in this era departments were able to circumvent this and avoid fair competitions. For instance, despite a nominal rule where three qualified candidates were required to be nominated per vacancy, in 1862 'only 100 of 858 such competitions... had the stated proportions' (Roach 1971, p.208).[7] After the 1868 election of the Liberal party under Gladstone, the executive ideologically shifted towards open access examination. At the same time, Chapman (2004, p.22) noted that the Civil Service Commission 'had been so skilful and successful that much of the earlier resentment, hostile motions and remarks in the House of Commons, had disappeared'. With the executive and legislature supportive of major reform, the only group left against it was the senior public servants themselves.

To provide a simplified depiction of selection in the pre-reform period. Social connection, to Government ministers, department leaders or other political party figures, was required to obtain employment.[8]

[5] The exception to this is the Indian Civil Service which introduced full open access examinations in 1855 (Cornell and Svensson 2023). There is a large historical literature on this development and its effect (Compton 1968; Moore 1964; Dewey 1973)

[6] A contemporary evaluation of this period confirms this, 'we allowed party managers and public officials to decide, during the period from 1855 to 1870, who should be examined for admission to the public service. The records show that inferior and unworthy persons generally dominated (Eaton 1880, p.233). Mr. Vincent Scully, an Irish MP at the time, concurred with such an evaluation, stating that 'it did not complete the efficiency of the service; because it expressly directed the examiners not to make any alterations with respect to the nomination of candidates' (House of Commons 1855).

[7] It was often also suspected that even in cases where there were multiple candidates 'a secret examination might be held, which might be used for the purpose of improperly rejecting candidates who would otherwise be considered qualified' (House of Commons 1855).

[8] Scully notes in the same speech mentioned before that 'he knew that it was a complete waste of time for an independent Member of Parliament to endeavour to influence patronage of a valuable nature. This class of patronage practically belonged to Government officials and heads of departments, they kept it as their pocket-money, and were not generally willing to surrender it to independent

Departments were eager to maintain this patronage system, given both the rents derived from it and a pervasive attitude that examinations did not select the *right* candidate.

1870 Order in Council. The reform dictated that:

Except as herein-after is excepted, all appointments which it may be necessary to make, after the 31st day of August next, to any of the situations included or to be included in Schedule A to this order annexed, shall be made by means of competitive examinations (Clause V, HMG 1870)

Schedule A specified that specific departments would be reformed, with the full list given in Table A5. The reform was to be applied for all clerk-level positions in reformed departments.[9] When treated departments wished to hire in these positions the law indicated that they had to do so through open-access examinations. Examinations were administered by the Civil Service Commission, using the infrastructure that had existed previously but not been utilised at the same scale. Examinations were standardised across departments, although there remained some specific variation in the subjects tested depending on the role, particularly for lower-tier positions with more technical responsibilities. New positions were circulated by the Commission, candidates paid a fee to sit the examination, and after a screening (preliminary) round the final examination was held with the best scoring candidate(s) awarded the position(s). As such, the crucial change brought about in 1870 was to *who* was able to sit the examinations. There would be several follow-up reforms introduced to tackle issues with this initial attempt, as while 'a triumph... it marked merely a stage in a long story' (Roach 1971, p.210).

Grey areas. The wording of the reform was strict. The reform has thus been highlighted as the point where 'patronage ceased to be of importance and the modern civil service developed' (Hanham 1960, p.75). Yet, there remained clear grey areas after the reform which are not obvious on reading Clause V. Specifically, there were two legislative features that allowed departments to maintain their previous behaviour if they wished beyond simple disobedience. The first was the existence of a carve-out given to reformed departments, Clause VII, which allowed for departmental heads to bypass the Commission if a candidates possessed 'knowledge and ability... not ordinarily to be acquired in the Civil Service' (Clause VII, HMG 1870). It is important to note that this required a specific approval, 'the said chief of the department shall propose to appoint thereto a person who has acquired such qualifications in other pursuits' (Clause VII, HMG 1870) whereupon the Lords of the Treasury would consider this proposal. This made it a loophole that was dependent upon the politicians that served as Lords of the Treasury.[10] The second avenue to legally protected discretionary appointments were Acts of Parliament granted historically to certain departments that allowed them autonomy over selection procedures. To these Acts, the Order in Council was subordinate, meaning it could not overrule them. Lowe, the Chancellor, made this clear by saying 'the Treasury and all

Members' (House of Commons 1855).

[9] The dataset I use only focuses on this group of positions, so I do not observe many of the positions that were not treated by reform. I take all departments listed in schedule A as treated, and all that are not as untreated. As a robustness check, I collapse the individual departments into their parent departments to show that potential inconsistencies in listing do not drive results. It is important to note that this definition of treatment is likely fuzzy, in particular it may be that several untreated departments were in practice treated (even if not by law). This however would only downwards bias any estimates of reform effectiveness.

[10] This was noted in debates over the Order in Council by Scottish MP Roger Sinclair Aytoun, who argued that Clause VII introduced 'another species of patronage of a not less prejudicial character... namely, the appointments which might be made under the 7th section of the Order in Council that he had quoted, and which the Government might confer on their friends.' (Parliament of the United Kingdom 1870a)

other Departments to whom full powers of appointment were granted by Act of Parliament possessed the right alluded to in the question.' (Parliament of the United Kingdom 1873). Flagrant usage of such acts or invoking Clause VII too often posed a potential political problem as repeated questions and scrutiny in the House of Commons illustrate, but they provided clear potential for departments to legally adapt to reform in a manner that preserved their previous privileges.

Summary. Prior to reform, all entrants to the civil service were directly recruited by the department. They were either directly appointed, or nominated to sit examinations that were administered by the Civil Service Commission. As such, social networks were necessarily exploited to find candidates suitable for the role. Post-reform, treated departments faced a different landscape when hiring. They could offer open access, competitive, examinations administered by the Civil Service Commission where the highest performing candidate would be selected and registration was open for all. Yet, the presence of politically costly grey areas allowed departments to maintain their previous direct recruitment if they wished (albeit at a price). Understanding the mixture of patronage and merit appointments that departments settled upon is the underlying empirical question of this paper.

3 Data and Descriptive Statistics

3.1 Data

Data is sourced from the *Reports of the Civil Service Commission* for the years 1864-1875.[11] These annual reports give the name, position, department, route of entry and often grades (for examined applicants) of each successful entrant. The route of entries are open competition, limited competition and no competition. I define patronage as an entrant through limited competitions or no competition given that both were reliant on social capital. As mentioned previously, qualitative records suggest that limited competitions (examinations) were often fixed to ensure that the desired candidate won. Unfortunately there are no annual records on the entire of the serving Civil Service, such as the registers in the United States (Aneja and Xu 2024; Moreira and Pérez 2024), which mean that I observe entrants at entry rather than throughout their career. As such, the analysis is purely centred around the effects on the entry-level positions over time. A photograph of the reports is given in Figure A1.[12] One concern with the data may be that there is systematic skew in the reporting of entry routes, creating a selection bias. For example, if law is passed that seeks to promote meritocratic hiring, then reformed departments might simply stop reporting patronage, rather than stopping hiring using it. This would produced a biased estimate of reform effectiveness. A quirk of the civil service in the period, and the positions my sample is built from help restrict this concern. Any entrant to the Civil Service from 1855 had to register with, and be approved by, the Civil Service Commission in order to gain a pension after 1859 Superannuation (Pensions) Act. If a candidate was approved, as almost all

[11] Unfortunately I am unable to collect data beyond 1875 because of an alteration to the examination process that pooled all examined applicants into "high" or "low" division clerks rather than giving their allocated department. As a result it is impossible to exploit within-department variation, and so I do not use the data.

[12] This also shows how I identify those entering through discretionary appointments versus other types. Limited competition entrants are denoted in the records with a †, open competition entrants are denoted with a ‡, while direct discretionary appointments are not denoted.

were, they would be entered in the Commission's reports for that year and are thus observed in the sample.

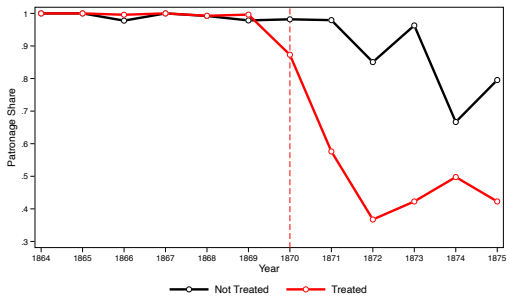
I construct a repeated cross-section of all clerk-level entrants to the service, complete with an indicator denoting their route of entry.[13] This gives me a total sample of just under 10,000 individual entrants in the period 1864-1875. I also collapse this data at the department-year level to construct an unbalanced panel data set on departmental hiring at the clerk level. Defining a department in this period is not immediately straightforward. Specific state functions were harboured within specific departments, from broad (the Treasury, or the War Office) to incredibly narrow (the Habitual Criminals Registry (Ireland) or the Chelsea Hospital). Often, departments were part of larger umbrella organisations under one parent. For example, the Chelsea Hospital was part of the broader War Office department. Yet, it was not the case that all small departments were necessarily under a parent organisation, the Ecclesiastical Commission or British Museum to give two examples. In my basic sample, I observe 104 departments that hired more than once over the period 1864-1875, and enter my preferred baseline sample as they are not 'singleton' observations (Correia 2015). For the entire sample I observe 135 departments. These departments are listed in Table A1. I consider departments based on whether they are listed as such in the original source material, to avoid introducing research error into the sample construction. However, I also attempt to use a range of historical sources, particularly *The British Imperial Calendar and Civil Service List*, to identify potential parents as well as information on functions, headquarters and ministerial oversight. Collapsing departments into their parent gives me 72 departments that I can use for a robustness check (see Table A2). Using information on government ministers in Gladstone's ministry, I create a further binary for whether a department had a minister. I construct this measure in two ways: (1) a direct requirement to equal one (i.e. the Home Office only has a minister, none of its 'children' departments do), (2) indirect departments can equal one (i.e. 'children' departments of the Home Office such as the Convict Service are counted as having a minister). I list these different groupings in Table A3. Finally, using *The British Imperial Calendar and Civil Service List* I locate the headquarter locations for departments, in particular creating a binary variable for the departments that are headquartered in Whitehall (the seat of the UK government). For the list, see Table A4.

3.2 Descriptive Trends

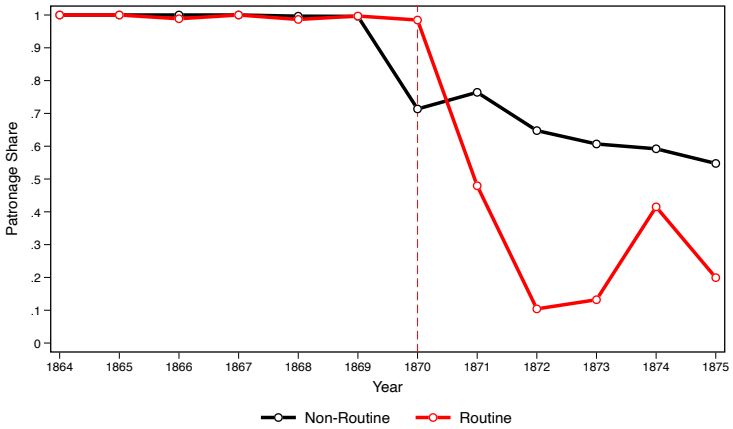
The Order in Council was introduced to alter hiring behaviour. It made the 'stock' mode of selection in reformed departments meritocratic. It was possible to continue previous appointments, although it was likely this had some associated risk. Reform occurred in one wave, 1870, and across a range of different departments (see Table A5 for a complete list of reformed departments). In Figure A2, I provide a bar chart that compares the size of the sample in each year between the control and treated. It is clear that the treated group makes up a much larger part of the civil service. This is due to the presence of Inland Revenue and the Post Office in the treated group who make up 5,365 hires, or 57.8 percent of the total sample. The ratio of hires between the two groups is however relatively static over time, the levels are simply different. This is with the exception of one spike in untreated hires in 1871 which comes from the Census Office during a census year.

[13] To briefly mention what clerk-level means, this distinction is given by the reports which only report clerk-level entrants. One can general take the term to mean high tier, in salary and barriers to entry, positions, although there is still variation in prestige within the sample.

(a) Patronage across the civil service.



(b) Patronage for treated and untreated departments.



(c) Patronage for routine and non-routine treated departments

Figure 2: Descriptive statistics, 1864–1875

Notes: The figure presents three panels describing patterns in recruitment between 1864 and 1875. Patronage appointments are defined as those made through either direct appointment or by nomination examinations. The top-left panel (a) shows the annual patronage share across the civil service. The top-right panel (b) shows the patronage share separately for treated and untreated departments. The bottom panel (c) gives the patronage share separately for treated departments that have broadly routine or non-routine labour functions splitting a continuous measure of routine tasks into above (routine) or below (non-routine) median. The reform year, 1870, is denoted by a red dashed line where applicable.

I compare trends between the treated and control departments using descriptive statistics before a more thorough analysis. In panel (a) of Figure 2, I show the share of patronage appointees over the period 1864-1875. Before treatment occurs (1870), almost every candidate to the civil service entered through patronage. This sharply changed, with an overall fall of around 50 percent to 1875. In panel (b), I split this between treated and not treated departments. There is an obvious similarity between the two before 1870, indicating common trends. Patronage makes up over 95 percent of hires in all years before 1870. After 1870 this quickly shifts, with a far more marked decrease in treated departments. Towards the end of the period (1874 and 1875), there is catch-up for the not treated group, suggesting some form of spillover which I return to later. This makes clear that the pronounced change visible in panel (a) was driven by the treated group of departments. From a common trend where (almost) all appointments were made through patronage, open access exam-based hiring became commonplace in these departments (exceeding 50 percent of total hires). Untreated departments continued to use patronage for 80 percent of their annual hires. Treated departments still used patronage for roughly 40 percent of hires after 1870, due to large differences across departments within the treated group. Panel (c) makes this clear, splitting treated departments between

routine and non-routine functions (Acemoglu and Autor 2011). This definition will be returned to later in depth, but separates departments based on their share of routine versus non-routine roles. Routine tasks are more easily assessed through examination, whereas non-routine tasks may be more accurately assessed through private information and social interactions. The data suggests that this distinction was crucial in the uptake of meritocratic appointments. Patronage falls far quicker in routine departments, while non-routine departments are only slightly less likely to use patronage than the untreated departments. This figure provides a clear message in sum: (1) departments were not giving up patronage privileges before reform, they *all* followed a common trend. (2) treated departments exhibit a clear divergence on the passage of reform. (3) this divergence in the aggregate masks that certain departments quickly shifted towards open examinations while others maintained their previous approaches. One clear explanation for this divergence is the labour functions of a department, and the perceived relevance of examinations to these functions.

I also consider differences in the type of appointment using the categories given in the source material, no competition (direct appointment), limited examination (by nomination) and open examination. These are presented in Figure A3. I consider the first two as patronage appointments, given that both require a patron to nominate a candidate. In panel (a) I consider these types of appointment over the period 1864–1875 for the entire civil service. Direct appointment was nearly 80 percent of hiring in 1870 (and between 60-80 from 1864-1870). In the years 1867-1870 it directly gained from a clear decline in limited examinations which dip from around 40 percent of hiring in 1867 to less than 20 percent in 1870. Open competition only increases in 1870 after reform, becoming the predominate method of appointment in 1872. Both forms of patronage fall after reform, but direct appointment remains the dominant form (more than 30 percent of hires) while limited examinations constitute less than 10 percent of hires after 1870. In panels (b) and (c) I split the sample into untreated and treated groups. The descriptive patterns are notably different. Direct appointments remain much more static after reform in untreated departments despite similar levels before 1870. There is a dip in panel (b) after 1871, but it remains the predominate way of hiring with over 50 percent of all hires. Open competition gains a greater foothold in treated departments, and direct appointments fall more sharply. Limited examination follows a similar trend in both, it falls towards zero. Once again, while there appears to be common trends before 1870 reform produced large differences between departments.

To formally consider differences between the treated and control groups, I look whether they exhibit similarities before treatment. Given that all departments use patronage (99% of hires pre-treatment), I focus on whether there are differences in the type of patronage, namely nominated examinations or direct appointments. I also consider whether there is a quality difference between the two, so I gather examination grades for all individuals assessed by the Civil Service Commission before 1870. I conduct a simple difference-in-means analysis to compare the treated to not treated group (a 'balance test'). This provides no evidence of imbalance between groups. Although it is clear that the treated group has far more entrants than not treated (see Figure A2), it appears that before treatment the groups exhibited similar behaviour in selection procedures, the outcome that I am interested in here. It also does not appear as if either the treated or untreated group exhibited large differences in quality, measured using examination performance for hired candidates.

Table 1: Difference-in-means

	Control mean (1)	Treated mean (2)	Difference (T-C) (3)
Route of entry:			
Limited Exam	0.298	0.297	-0.00152
	(0.0169)	(0.00743)	(0.0185)
Direct Appoint.	0.694	0.701	0.00738
	(0.0171)	(0.00745)	(0.0186)
Observations	4,508	4,508	4,508
Grades:			
Total Grade	0.735	0.727	-0.00793
	(0.00447)	(0.00366)	(0.00578)
Observations	1,159	1,159	1,159
Maths Grade	0.698	0.712	0.0141
	(0.00840)	(0.00523)	(0.00990)
Observations	1,121	1,121	1,121

Notes: The table presents a balance test between untreated and treated departments. I run a simple regression of my treatment indicator upon an outcome variable (a difference-in-means regression). For the first two outcomes, I focus on the route of entry which takes the value one if a given candidate enters the civil service through that route pre-treatment (limited examinations or direct appointment). For the final two, I look at the grades (in percent) scored by candidates, either in total or in maths.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

4 Reform and Patronage Use

4.1 Empirical Strategy

To determine how reform changed departmental hiring, I use a canonical difference-in-differences model. As such, it is possible to identify an average treatment effect on the treated (ATT) for treated departments using the following linear probability model:

$$\text{patronage}_{i,t,d} = \beta_1 \times \text{reformed}_d \times \text{post}_{i,t} + \tau_t + \theta_d + \varepsilon_i \tag{1}$$

The coefficient of interest (β_1) is the interaction term between reformed and post. This should be interpreted as the change in probability (in percentage points) that an individual i in treated department d in a given year t enters the civil service through patronage, relative to the control group post-treatment. Reformed is a dummy variable for a department being directly reformed, while post takes the value 1 if the year is 1870 or later (1870 being the year of treatment). It is possible that estimates might still be biased in a 'naive' policy analysis without fixed effects due to unobserved variation at the departmental or hiring cycle level. I therefore use departmental (θ) and year (τ) fixed effects. Intuitively, these allow the analysis to focus on *within-department* variation, an important step given that (for example) the Post Office may exhibit substantially different hiring practices to the Irish County Surveyor's Department. Equally, there may be large shocks to demand in certain years (hiring freezes, census-year surges) that lead to cross-bureaucracy behaviour adjustments. I always cluster at the department level, as this is the unit at which treatment

occurs (Abadie et al. 2022).

I also use a panel approach to complement these results. I collapse the individual data at the department-year level to create an unbalanced hiring panel. As such, the focus changes from individual level decisions to how departments at large shifted their selection procedures at an aggregated level. The panel is unbalanced because the outcome (for most departments) exists for a subset of department-years, manufacturing balance is impossible when the outcome of interest is hiring shares. For each department-year I calculate the share of entrants through patronage, which I regress upon the same treatment indicator using the equation:

$$\text{patronage share}_{d,t} = \beta_1 \times \text{reformed}_d \times \text{post}_{d,t} + \tau_t + \theta_d + \varepsilon_d \tag{2}$$

The terms of interests, and fixed effects, remain the same as before. Each regression is weighted by the number of entrants in a given department-year. The coefficient of interest (β_1) should however be interpreted as the change in share of patronage entrants in treated department d in a given year t, relative to the control group post-treatment. Standard errors are clustered at the department level once again.

In order to interpret any estimates causally in these models, it must be the case that reformed and unreformed departments would exhibit common trends without reform. There is no obvious balance issue on several pre-treatment hiring characteristics (see Table 1). Departmental fixed effects account for level differences, but it remains possible that differences in level have time-variant effects on the outcome. For instance, treated departments could include departments who were already in the process of altering their selection behaviour (and thus who selected into treatment). One way to deal with this is to exploit the high frequency nature of the annual hiring data (in an event study) to ensure that reform does drive changes, there is no anticipation and that departments were on common trends before 1870 (which Figure 2 suggests). The institutional setting lends substantial support to this common trend assumption. The reform opened a method of selection that previously did not exist (see Figure 2). There was no attempt by departments to implement open access examinations before treatment, as contemporaries noted that 'any ministry would be loath to give up such a privilege [of patronage]' (Brown 1879). Absent reform, it is almost certain that there would have been no substantive change in patronage use across all departments (regardless of their treatment status). I also use several alternative samples that remove potentially problematic departments from either the treated or untreated groups, with consistency between all specifications strongly supporting a causal interpretation.

4.2 Effect on Patronage Use

Linear Probability Results. Table 2 presents the main results on the effect of reform using Equation 1. The dependent variable takes the value one if a candidate enters through patronage. In column (1) I estimate the baseline difference-in-differences, which is my preferred specification. The estimate suggests a 34 percentage point lower probability that a given candidate entered through patronage post-reform relative to control departments. This is clearly a large economic effect, as it indicates a decline of over one-third from the original mean pre-treatment.

In order to check that these results are not driven by differences between departments that might

Table 2: Baseline DiD

	Dep. Var: Patronage (1/0)					
	(1)	(2)	(3)	(4)	(5)	(6)
Reform × Post	-0.343***	-0.349***	-0.316**	-0.454***	-0.219***	-0.270**
	(0.121)	(0.124)	(0.121)	(0.113)	(0.0670)	(0.104)
Observations	9,275	8,616	9,018	6,554	6,631	3,910
Departments	104	72	88	103	103	102
Dept FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
No Foreign Dept.		✓				
No Commissions			✓			
No Post Office				✓		
No Inland Revenue					✓	
No I.R or P.O						✓
Mean Dep. Var.	0.788	0.775	0.784	0.762	0.851	0.851

Notes: The regression model is a linear probability model, where I regress the interaction term (reformed × post-treatment) upon a binary variable for patronage use for each individual appointment from 1864-1875 in the British civil service, thus estimating whether reformed departments changed their manner of appointment post reform relative to unreformed departments. All models use department and year fixed effects. Sample respecifications in columns (2)-(6) are denoted using checkmarks, and explained in text. This leads to changes in the number of departments and observations included in each regression. The mean of the dependent variable (patronage) over the period 1864-1875 is given at the bottom of the column. Standard errors are clustered at the department.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

overstate the effectiveness of reform, I perform several sample respecifications in the other columns (columns (2)-(6)). In column (2) I drop all departments based outside of England, in column (3) I drop all commissions (such as the Epping Forest Commission). In columns (4)-(6), I deal with the issue that my sample is dominated by two treated departments, Inland Revenue and the Post Office. In column (4) I drop the Post Office, column (5) Inland Revenue and column (6) both. There is no reason to believe that these departments are not fair treatment groups, but consistency without them confirms that this is a general shift across treated organisations and not something specific to the two largest arms of the state. The effects are robust to all respecifications. Under the aforementioned assumptions, column (1) has a causal interpretation. The sample restrictions in columns (2)-(6) are motivated by different potential identification concerns regarding the types of department used for the treatment-control comparison. They yield estimates that are similar in sign, magnitude, and precision. This stability helps support such a causal interpretation.

I then estimate an event-study analogue. As mentioned before the key assumption for difference-in-differences is common trends between the treatment and control departments in the absence of reform. A partial test of this is to consider the pre-treatment effects of reform (pre-trends) which in the instance of common trends should be indistinguishable from zero. The results are presented in Figure 3. This provides clear visual evidence in support of this. There is no difference in patronage probability between the treated and control departments before 1870, an f-test cannot reject that the pre-treatment coefficients are jointly equal to zero. After reform was introduced, there are clear differences appearing in 1871 and onwards. Reformed departments hire less through patronage. This effect levels off after 1873, which concurs with the descriptives in Figure 2. It is marginally significant and slightly reduced in these two years. This comes

through an increase in meritocratic hiring in the control group, which suggests a positive spillover that I return to later. Given that this attenuates the ATT it is not a problem for a causal interpretation of reform, but indicates that the true ATE of reform is in fact larger than estimated here.

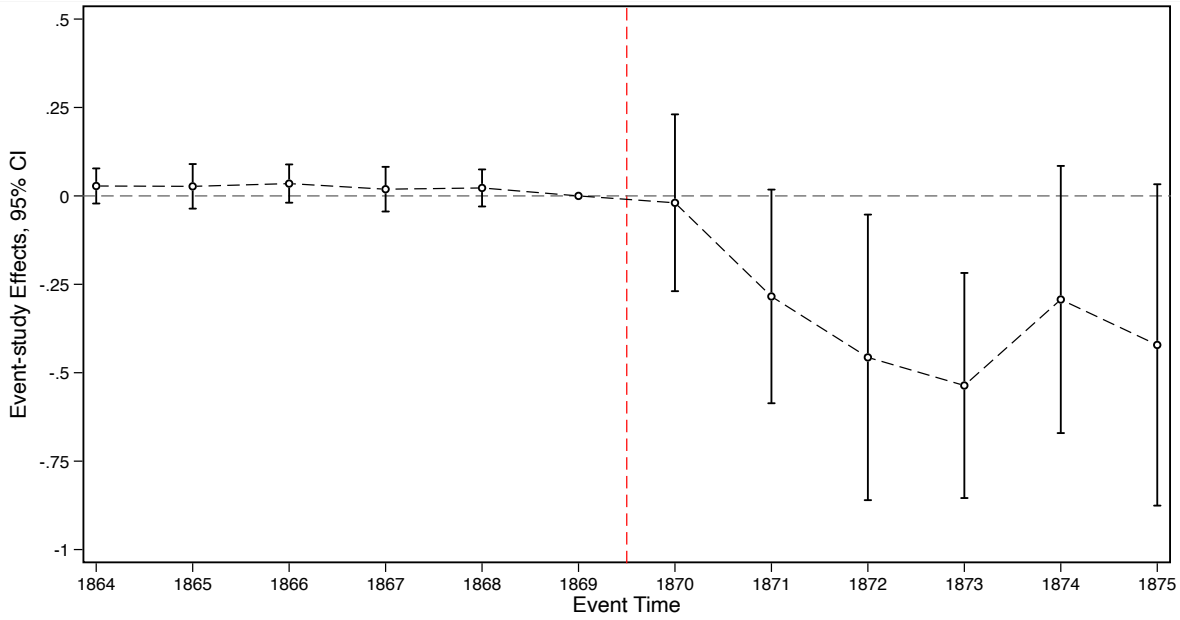


Figure 3: Patronage usage in treated departments (LPM)

Notes: The figure plots event-study effects for a leads and lags analogue of the regression model in Equation 1. The regression follows the specification used in column 1 of Table 1. All estimates are relative to event time -1 (1869). Bars indicate 95% confidence intervals calculated using clustered standard errors at the department level.

Panel Data Results. Consistency between individual-level and panel specifications would indicate that an effect of reform is present in both micro-hiring decisions and aggregated department behaviour. All regressions are weighted by number of entrants in that given year to account for large differences in intake size across department-years. The results are reported in Table 3. I adopt the same approach as in the baseline. The β_1 coefficients are remarkably similar to those presented previously. They suggest once again that reform had an immediate effect on how treated departments hired, reducing their intake through patronage substantially (between 20-44 percent decrease in patronage share). To consider the time dynamic and potential common trends violations in this model, I estimate the event-study model. The results are presented in Figure A4. Jointly, the pre-treatment coefficients are not distinguishable from zero supporting common trends. The dynamic of the effects is similar, appearing in 1871 with some convergence between the treated and control groups by 1875.

Robustness Checks. This result holds up against a variety of different robustness checks using both datasets. An advantage of the panel structure is that it allows me to use the doubly robust estimator introduced by SantAnna and Zhao (2020) to relax the assumption of unconditional parallel trends. I introduce several potential time-invariant covariates. I use pre-treatment averages for direct hiring, examinations by nomination, as well as information on whether they are a foreign department, a commission or have a direct minister. I use inverse propensity score weighting to estimate. In Table A6 the ATT is slightly reduced, but

Table 3: Panel DiD

	Dep. Var: Patronage Share (%)					
	(1)	(2)	(3)	(4)	(5)	(6)
Reform × Post	-0.332***	-0.337**	-0.297**	-0.439***	-0.203***	-0.243**
	(0.125)	(0.128)	(0.126)	(0.120)	(0.0697)	(0.101)
Observations	608	458	524	596	596	584
Departments	95	64	82	94	94	93
Dept FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
No Foreign Dept.		✓				
No Commissions			✓			
No Post Office				✓		
No Inland Revenue					✓	
No I.R or P.O						✓
Mean Dep. Var.	0.870	0.859	0.867	0.871	0.875	0.876

Notes: The regression model is a panel difference in differences, where I regress the interaction term (reformed × post-treatment) on the share of a departments entrants that enter through patronage. Each observation is weighted by the total number of hires made in that department-year. All models use department and year fixed effects. Sample respecifications in columns (2)-(6) are denoted using checkmarks, and explained in text. This leads to changes in the number of departments and observations included in each regression. Fewer departments are found in this sample due to departments that recruit multiple people in one year, so are singletons in this sample but not in the individual-level data. The mean of the dependent variable (patronage) over the period 1864-1875 is given at the bottom of the column. Standard errors are clustered at the department.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

remains economically large (21-26 percentage point fall in patronage) as well as statistically significant.[14] Another concern might be that departments seek to reform for practical reasons, namely that they have exhausted their capability to fill roles through patronage alone. In this case, it might be that departments grow, leading them to turn away from patronage. If this coincided with reform timing it might bias the results. In the panel specification, I control for (rather than weight by) the time-variant size of the hiring class in Table A7. This does not affect the results. My panel results are further robust to using nearest neighbour, conventional propensity score or coarsened exact matching – using the time-invariant covariates used in Table A6 as the matching input – with the results given in Table A8. Another concern relates to the nature of the organisational structure. There are several 'parent' departments. It could be that parent departments influence their 'children', meaning that the earlier specifications that treat them as independent are incorrect. To account for this, I can respecify the dataset so that the analysis is run at the parent level. This means that I assign treatment based on the parent, rather than the department as is previously done. This is not the correct treatment classification, and should attenuate the results by causing an improper treatment-control assignment. Yet, given the potential for influence across umbrella organisations it is reassuring to find consistency. I do this for the LPM (see Table A9) and panel (see Table A10). The results are not altered. I introduce position group fixed effects in order to exploit within-position variation in the outcome, which further refines the comparison being made.[15] The results are given in Table A11, there is no change. Another

[14] This attenuation is driven by how the estimator works, as it requires balance in period t and $t+1$, the treatment period and period after. As such, it is likely to exclude smaller departments (sample shrinks from 652 to 487), which shrinks the ATT if these departments are more likely to comply with reform.

[15] I use three position groups: clerk, assistant or other. String recognition is used to categorise them. I do not use exact position fixed effects because there is substantial variation in the titles given for roles, leading an exact title effect to miss large amounts of necessary

concern might be about three possible instances of selection into treatment. Qualitatively, there are three potential departments that perhaps acted in such a capacity, the Civil Service Commission (selects into) and the Foreign and Home Office's (opts out of), and so results should hold without the inclusion of these organisations. The results can be seen in Table A12, and are slightly larger than the baseline, likely due to an attenuation introduced by improper treatment-control assignment. Using a panel rather than LPM does not change this (see Table A13). Finally, I check that the individual-level results are consistent when using a probit or logit model (see Table A14), rather than the LPM approach in the baseline that I prefer.

Summary. Reform substantially changed hiring behaviour among treated departments, and in turn promoted meritocratic hiring in control departments after a small lag that suggests control group departments became aware of the benefits of merit hiring. As a whole, the British Civil Service underwent a radical shift towards open access examination based hiring after 1870. Empirically, it is likely that all results here substantially underestimate the actual ATE given that attenuation is introduced to the ATT by this spillover between departments.

5 Explaining compliance - mechanisms

The above section provides robust evidence on the negative effect reform had on patronage use. However, after reform treated departments, on average, still hired over 40 percent of their entrants through patronage, suggesting that there was significant non-compliance with legislation. In this section, I investigate why departments complied, and why others did not.

5.1 Model

To situate this analysis, I develop a brief model to guide the empirics. The model is structured around an individual bureaucrat in charge of a single hiring decision. They balance the decision to use patronage with the decision to use open-access examinations when hiring for one entry-level position. I use this model to try to explain variation in compliance.

Set up. There is one agent in charge of selection for a position, a managerial bureaucrat, and two perfectly substitutable hiring decisions, merit or patronage hiring, for any given entry-level position. For a given position, the candidate pool comprises of individuals who would be merit or patronage hires, $x = \{m, p\}$. The principal, the government, hope that the hiring decision is carried out to maximise productivity in the organisation. I initially assume that productivity from merit candidates is higher, leading to the term $\tau_j(\Delta\epsilon_{j,i})$ which reflects the gain in efficiency for a merit selection – the assumption being that $\Delta\epsilon_{j,i} > 0 \quad \text{for} \quad U_{j,i,merit} \quad \text{as} \quad (\epsilon_{merit} - \epsilon_{patronage}) > 0$.[16] Managerial bureaucrats are standard utility maximisers,

variation. I do utilise the exact titles later when consider job specific changes in the mechanism.

[16] And conversely this gain is reversed and subtracted from the utility function when a candidate is hired through patronage: $\Delta\epsilon_{j,i} < 0 \quad \text{for} \quad U_{j,i,patronage} \quad \text{as} \quad (\epsilon_{patronage} - \epsilon_{merit}) < 0$. I will relax this assumption later in order to explain across- and within-organisation variation in compliance as for certain functions it is possible for productivity gains to be zero or even negative for merit appointments.

with managerial bureaucrat j in change of a given selection decision i maximising by

$$U_{bureaucrat} = \tau(\Delta\epsilon) + (1 - c_\theta)\alpha(p) = y + d_\theta\alpha(p) \tag{3}$$

where $\Delta\epsilon$ becomes the constant efficiency gain (loss) from merit (patronage) y. There is also a cost function incorporated into the model ($c_\theta \in [1, 0]$), which captures the potential punishments that a given bureaucrat will face if caught using patronage. This term weights the gain ($\alpha(p)$) for that bureaucrat hiring a patronage candidate which I take to be a constant in the model.[17] The function is built from several components, which I consider later.

Pre-reform equilibrium. If we assume that prior to reform there are no enforced costs to patronage we have the utility function:

$$\lim_{c_\theta \to 0} [U_{pre} = y + d_\theta\alpha(p)] = y + \alpha(p) \tag{4}$$

As such, bureaucrats maximise utility with a corner solution with complete patronage so long as $\alpha(p) > y$. In other words, so long as the personal (narrowly-derived) gain is greater than the efficiency loss for a given position, patronage will be used, which in the pre-treatment period is (almost) always the case in my empirical setting (see Figure 1). This equilibrium leads to a pervasive usage of patronage, and the necessity to exchange a form of capital for entry to the civil service. This fits the existing qualitative literature on the reach of patronage in 19th century Britain as well as the descriptive results presented earlier in the paper (Hanham 1960; Eaton 1880).

Post-reform multiple equilibria. While previous to reform $c_\theta \simeq 0$, legislation introduces new costs through potential punishments in reformed organisations. With $c_\theta > 0$, the gains from patronage shrink accordingly as $d_\theta\alpha(p) < \alpha(p)$. The decision rule becomes whether the costs reach a critical level of c whereupon the corner solution shifts given that $U_{merit} > U_{patronage}$. As such it is possible for reform to pivot the selection equilibrium if it diminishes the gains from patronage to the extent they fall below the efficiency benefits of merit hiring.[18] The solution will always be a corner, but *which* is dependent on this critical value being exceeded. A managerial bureaucrat will choose to hire through merit when $\Delta U \leq 0$ which implies $d_\theta\alpha(p) < 2y$, or that the efficiency gains are greater than the gain from patronage. Of these terms d_θ is the only one that can vary due to reform as cost is imposed on bureaucrat j at the organisational level. This explains the baseline results in subsection 4.2, as treated (reformed) organisations should exhibit differential behaviour upon the passage of reform, conditional on the strength of the cost function leading to the inequality presented above. Specifically, they lower the share of entrants through patronage and increase merit-based hires. There is substantial heterogeneity based on the managerial bureaucrats specific utility function.

[17] This decision is mainly made for ease of use – it is possible to relax it and have a function that scales with the personal gain or attitudes. The implications of this are not particularly compelling, it simply adjusts the critical threshold at which a given manager will switch from patronage to merit.

[18] Qualitatively, this stands well with the explanation of meritocratic transitions in historical political economy, which often centre on the tipping point where patronage becomes more costly to national politicians (through several factors) than the gains they previously enjoyed (Johnson and Libecap 1994). Obviously, in this framework the decision maker is the bureaucrat, rather than the executive, but the same mechanism is likely to operate – cost increases drive change.

Labour functions. Different organisations have different labour functions in the public sector. Their mission requires employees to have specific skill sets. Previously, it was assumed that all departments exhibit a productivity gain from their merit hires. But this is a clear oversimplification, that when relaxed derives a key prediction on compliance. Perceived productivity is dependent upon the function a department carries out. Departments with routine tasks, such as registration, would perceive exams to measure future skill on the job well. Those with non-routine tasks, such as policy origination or cross-department collaboration, could alternatively perceive exams as an inferior tool compared to the usage of social networks that can limit informational asymmetries (Voth and Xu 2021). Such an argument appears qualitatively in the historical literature, as contemporaries felt that 'work, particularly in the more confidential departments, demanded qualities of loyalty and dependability which could not be measured by an examination' (Roach 1971, p.30).[19] This division is consistent with the theoretical argument raised by Allen (2005, p.76) that if 'monitoring inputs [are] relatively cheap, the crown opts for a professional bureaucracy... hired in terms of merit', given that non-routine work was far less easily measured, confidential and more reliant on trust in individuals. As such, it might be that $\Delta\epsilon_{j,i} \leq 0 \quad \text{for} \quad U_{j,i,merit}$. In this instance, variation will occur both across- and within-departments. Departments with greater shares of routine (non-routine) work will hire more (less) through examinations. Within departments, positions that are deemed more routine (non-routine) will be prioritised for examinations (patronage).

Power. Organisations also hold substantively different levels of practical, de facto, power. In an environment where there are loopholes to reform efforts, this might create heterogeneity in compliance. Consider the cost function as scaled (inversely) by organisational capacity ($c_\theta = f(z) \cdot \pi_o \quad \text{where} \quad f(z) = x \cdot \delta$). $f(z)$ is a linear punishment function that has a constant punishment x which is scaled by δ, which is a binary for the presence of legislative constraints (i.e. being reformed). This means that managerial bureaucrats consider their level of organisational power (π_o in equation) when making a hiring decision. In this setting, it is likely that power permits the capacity to exploit existing grey areas in legislation given the importance of organisational capacity to both invoke, and withstand political pushback to, patronage powers.[20] This interacts with labour functions as a necessary, rather than sufficient, condition. The desire to use examinations or patronage is determined by labour functions, but with this desire established power determines the extent of non-compliance possible.

Other mechanisms. It is possible for size to provide a complementary channel to explain compliance across departments. As noted by Johnson and Libecap (1994), patronage exploits a finite number of social connections and thus merit can be used to maintain a high quality of candidate when these connections are exploited. Departments that have high hiring burdens annually may be more likely to exhaust these connections, making the average candidate worsen. In the model this exacerbates the productivity loss through patronage ($\Delta\epsilon_{j,i}$) leading to increased usage of merit among these departments. Within-departments, variation in the expected future roles of entrants (prestige) could provide a partial explanation. Certain entrants are introduced with the capacity to reach the 'glittering prizes' within a seniority system given their

[19] Confidentiality was a large concern, in particular the necessity to know that employees were not compromised by external states, which was judged to be easier when social networks were employed in their selection rather than examinations where any candidate could in theory enter the role.

[20] To put it simply, in this setting if an organisation holds more power, it is both easier to use Clause VII (which required approval) and more likely that they hold (and are willing to incur the political cost of using) Acts of Parliament.

job (Bertrand et al. 2019). Entry-level Clerks in the Treasury in 1870 could become the leader of the Civil Service (as Edward Walter Hamilton, who entered in this year, did). Yet, those entering in more minor clerk-level positions, such as a typing clerk in the Post Office or an Assistant of Excise in Inland Revenue, could not barring an unexpected turn of events. Managerial bureaucrats knew this, and so within-department variation may also be explained by the desire to protect selection privileges over the potential future leaders of a given department.

5.2 Empirical Strategy

Throughout this section I follow a triple difference strategy. The intuition is as follows: the theoretical parameter of interest is the mechanism that explains why certain treated departments adopt reforms and why others do not. As such I include a third difference term that is interacted with the baseline difference-in-differences indicator to give the differential effect of treatment based on this term. Using individual-level data, this gives the model:

$$\text{patronage}_{i,t,d} = \beta_1 \times \text{reformed}_d \times \text{post}_{i,t} \times \text{indicator}_{d/p} + \tau_t + \theta_d + \varepsilon_i \tag{5}$$

This third term is assigned to individuals, but reflects either department d or position p level characteristics. For specifications that seek to exploit changes at the position level I impose exact position title fixed effects (ϕ_p). I make clear the precise interpretation of the β_1 for each specification in turn. Identification comes from changes within the treated group, meaning that the coefficient can be interpreted as causal if, in the absence of treatment, treated subgroups would have evolved along common trends. To test for this empirically, I use an event study approach that looks for common pre-treatment activity regarding the outcome. Given that all groups are treated, selection issues can be discounted which is the major concern with the baseline empirical strategy. In Appendix B, I collapse the data into a panel dataset to show that mechanism results are consistent when at a higher degree of aggregation. There are no qualitative differences.

5.3 Labour functions

Across-department. Departments who specialise in routine work, where entrants have clearly defined and specialised tasks, are likely to view examination as a helpful aid in selection. Meanwhile, departments who rely on intangible skills with high monitoring costs and a requirement for confidentiality might view productivity gains as either even or negative for examined candidates. To consider this empirically, I operationalise routine work across departments using a data-based classification. I take job titles and parse them for strings that have clear routine connotations.[21] I collapse these at the department level to give a share of routine entrants per department. I then use the departmental share as my third difference term as a continuous variable. Results are robust to using the entire sample, or just pre-treatment to define routine.

The results can be seen in Figure 4. The results are similar regardless of sample used. There are no pre-treatment deviations for either, before treatment there no deviation from a common trend with regard

[21] To be specific, jobs that include the strings 'assistant', 'compiler', 'index', 'audit', 'inspector', 'copyist', 'accountant', 'technical', 'registrar', 'storekeeper', 'examiner', 'steward', 'writer', 'sorter', 'transcriber', 'book keeper', 'mechanician' are classified as routine tasks. Non-routine roles are therefore typically clerk positions where responsibilities are less clearly defined in their title.

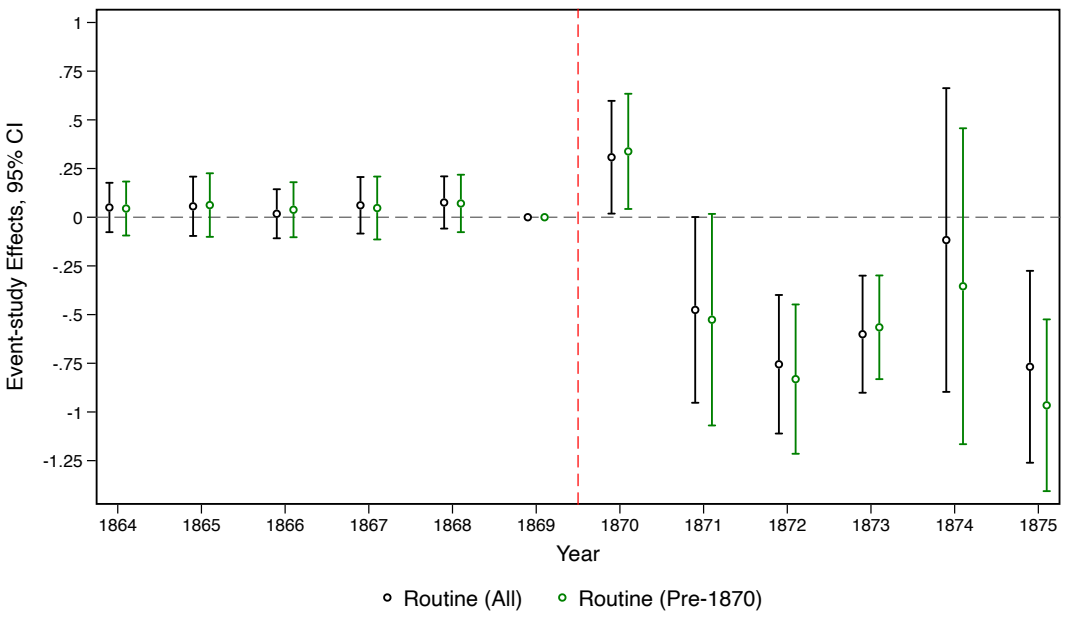


Figure 4: Patronage usage in Routine departments

Notes: The figure plots event-study effects for the following regressions. The regressions interact a continuous variable that measures the departments share of hires that are classified as routine workers, using either data from the whole period or pre-1870. All estimates are relative to event time -1 (1869). Bars indicate 95% confidence intervals calculated using clustered standard errors at the department level.

to patronage use. However, after the introduction of reform there are sharp differences. There is a large positive increase in 1870, before a clear finding appears. Routine departments report substantially lower uses of patronage. Among treated departments a 1 unit increase (zero to one hundred percent) in routine work is associated with a decline in the probability of patronage use for a given candidate between 40 and 90 percentage points depending on year. This reflects a large change from patronage to open access exams among routine departments by 1875 (as can be seen descriptively in Figure 2).

The corollary to this is to consider whether jobs that are more distinctly non-routine exhibit the opposite response, if department leaders felt that exams were a worse measure than patronage they would cling on to the old appointment system relative to other treated departments. To consider this empirically, I construct a binary variable equal to 1 when a department is in the bottom quartile of routine work. I present the results of these regression in Figure 5. They show that before treatment there was a common trend, but after there is a sharp increase in relative probability that a candidate is hired through patronage in treated departments within the bottom quartile of routine work. This result is robust to using quantiles or terciles instead (see Figure A5). This indicates that non-routine departments were not adopting reform.

As a more descriptive, yet still informative, exercise I estimate the effects for each treated department individually. To do this I estimate a separate regression for each using the post-reform indicator. This gives me the individual level shift in the probability of using patronage by department. This permits a comprehensive descriptive comparison across all departments, minimising the potential for classification errors to lead to inaccurate conclusions. I drop all departments below 10 hires over the period of observation, and any which cannot calculate standard errors. I present this in Figure A6. The Admiralty, Board of Trade, Colonial Office, Education Office, Mint and Treasury specifically report zero (or close to) changes in their

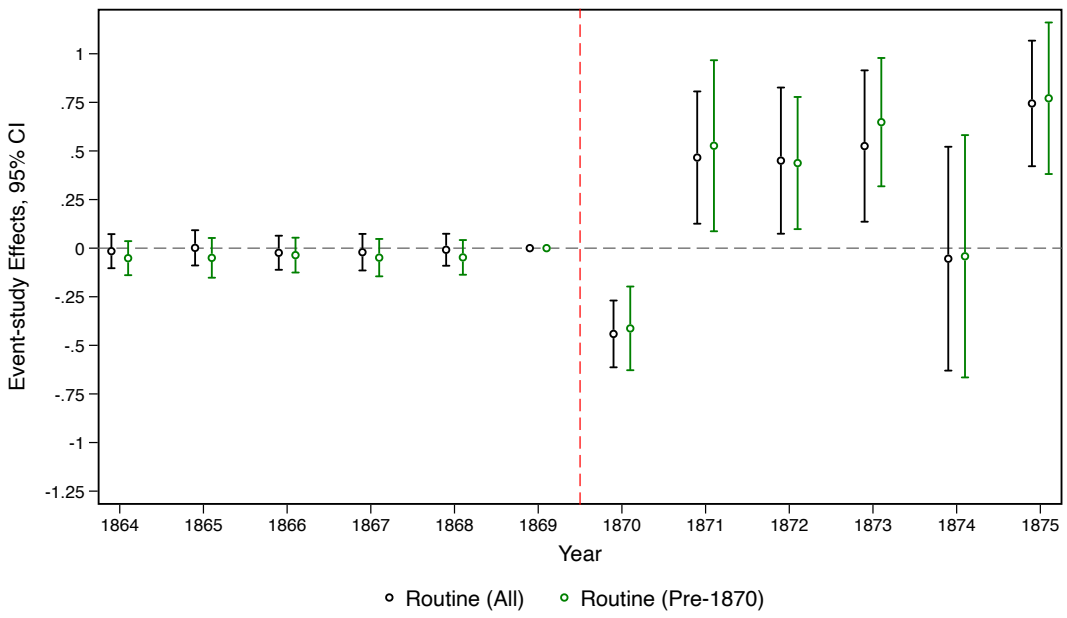


Figure 5: Patronage usage in Non-Routine departments

Notes: The figure plots event-study effects for the following regressions. The regressions interact a binary variable equal to 1 if a department is in the bottom quartile of hires classified as routine using job titles. This classification is done using either data from the whole period or pre-1870, with both results reported. All estimates are relative to event time -1 (1869). Bars indicate 95% confidence intervals calculated using clustered standard errors at the department level.

admissions procedures after reform. Qualitatively, these are non-routine departments that placed a high weight on confidentiality and trust. Routine departments such as Inland Revenue, the National Debt Office, Office of Works and Stationery Office (among others) report almost complete shifts away from patronage.

I investigate the potential spillovers to the control group. My expectation is that departments in the control group with more routine functions would copy treated departments if there were obvious productivity benefits. This might explain the lagged spillovers found in the baseline section. I estimate the change after reform for each control department individually as before, giving the results in Figure A7. For the 12 departments large enough to allow this estimation, I find further evidence in line with that of the treated group. Departments with clearly administrative functions, such as the Adjutant-General's Office or Local Government Board, shift away from patronage despite a lack of reform. Non-routine departments such as the Home Office or Science and Art Department do not. This is only suggestive, but provides a clear message.

Within-department. To consider the importance of functions within department is made challenging by the variety of job titles given in the source material. Trying to impose too much structure on these creates substantial issues. I attempt to take a parsimonious approach to the problem. To measure routine jobs, I use the routine classification employed above and also create a separate measure for 'assistant' jobs which are very frequent and clearly linked to routine practices. To measure non-routine jobs I take the title 'clerk'. The separate binaries take the value 1 when a given entrant has a job title which matches the given classification. Consistency with my across-department results would mean that exams are quickly adopted for routine jobs while patronage is still preferred for non-routine jobs when focusing on variation between positions in

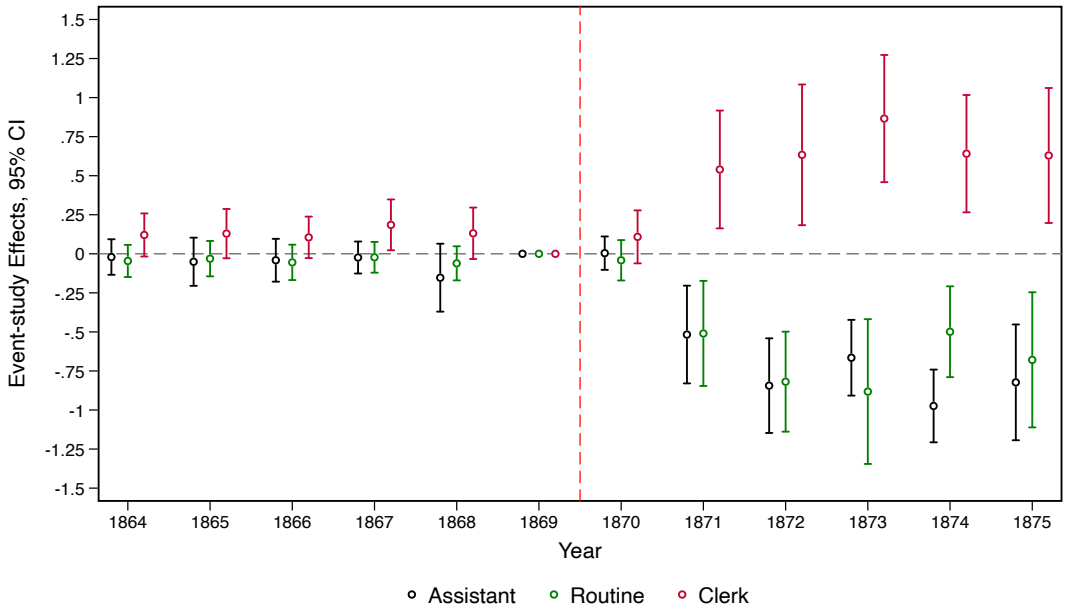


Figure 6: Patronage usage for different positions.

Notes: The figure plots event-study effects for three different triple difference regressions that exploit variation in function to explain non-compliance with reform. All regressions use department, position and year fixed effects. The regressions use separate binary variables equal to 1 when a given entrants takes a job that fits one of three categories: Assistant, Routine or Clerk. They are estimated separately, and plotted alongside one another. All estimates are relative to event time -1 (1869). Bars indicate 95% confidence intervals calculated using clustered standard errors at the department level.

the same department. I estimate the regression with department, (exact) position and year fixed effects. This identifies a coefficient from within-position changes in the probability of patronage based on whether a job is routine or non-routine, holding fixed department and common year shocks. I replicate the results using position-department fixed effects in Figure A8. This provides an even more restrictive approach that measures changes in patronage for the same position-department combination over time.

The results are presented in Figure 6. I find that routine jobs exhibit much greater declines in relative patronage probability. This is true using both the assistant or routine categories. There is no evidence that this is the result of pre-existing trends, differences rapidly appear after 1870 but are not present beforehand. By the end of the period routine jobs in treated departments are almost entirely recruited through examinations. The opposite is true for clerk positions. There is evidence of slight pre-trends, as one might expect clerks were more likely to be appointed through patronage even before reform. For the clerk position generally, there is a large increase in the relative probability that an entrant to one of these positions is hired through patronage after 1870. These results support those earlier, confirming that the across-department logic exists within them as well. They can be seen descriptively in Figure A9. Departments, generally, perceived examinations as a useful tool for certain positions. It appears that for positions that featured more complex, undefined tasks (clerks), they were markedly reduced in their adoption of reform. Departments that held a greater concentration of routine (non-routine) jobs were therefore more (less) willing to adopt reforms on average when comparing change at the departmental level.

5.4 Power

In an environment where reform can be circumvented, the degree of power held is likely to influence the capacity of an organisation to do so. In particular, power may permit the extent of patronage use for departments unconvinced that examinations provided clear benefits. Measuring this is not straightforward, as any measure captures a bundle of correlated dimensions such as historic importance, legal authority or political capital. The main proxy I use is the presence of ministers in a department. Certain departments had a direct minister (i.e. the Chancellor and the Treasury), while others had an indirect minister through their parent organisations (i.e. the Home Secretary and the Convict Service). Others had no connection. I determine ministerial presence through a binary variable if a department is listed as having a government minister in the Gladstone ministry. This should roughly proxy for a general bundle of characteristics that approximate power through political importance. Another approach is to consider location. From *The British Imperial Calendar and Civil Service List* I can identify the addresses of many departments within my sample. I can contrast departments with addresses in Whitehall with those who have offices outside the nexus of the political establishment.[22] I once again do this at both the department and parent level to illustrate consistency given likely measurement error. It should be noted that this in effect measures whether political connections lowered reform adoption.

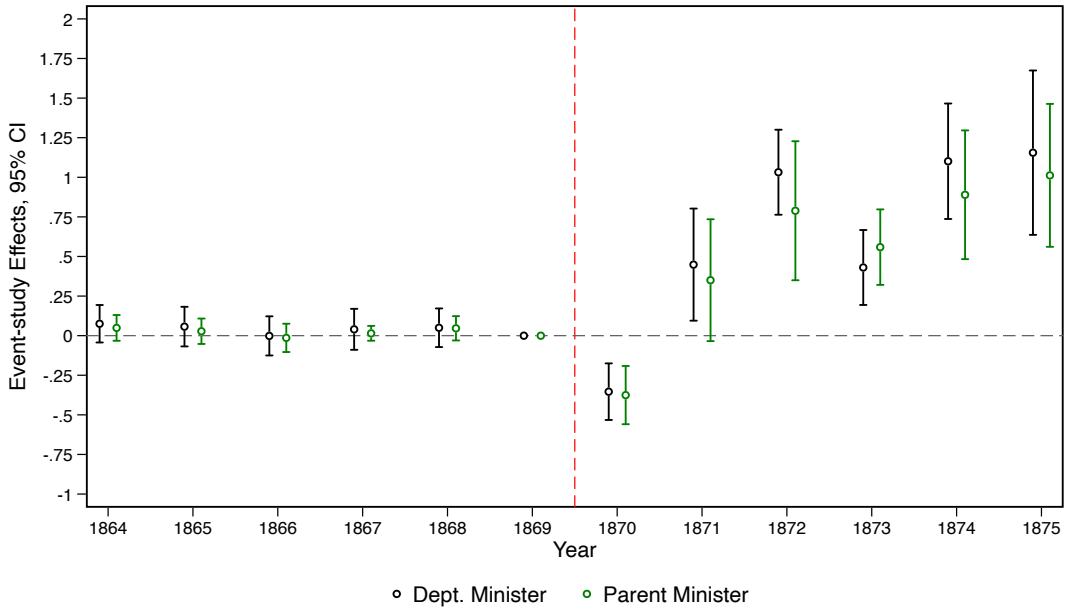


Figure 7: Patronage usage and ministerial presence

Notes: The figure plots event-study effects for two different triple difference regressions that exploit variation in function to explain non-compliance with reform. The regressions use a binary variable that equals one if a department has a minister within it as the third variable to interact with the difference-in-differences term. I define ministerial presence either at the department or parent department level, with the regression reformulated at the level chosen. All estimates are relative to event time -1 (1869). Bars indicate 95% confidence intervals calculated using clustered standard errors at the department and parent level for each specification respectively.

[22]The lists do not contain all departments in the sample. There are some which are registered as non-Whitehall departments that may in fact be Whitehall based. But the inverse is not possible, which means that the estimates will only be attenuated if the imagined mechanism, power permitting non-compliance, is correct. The departments with Whitehall bases in my sample are the Admiralty, Board of Trade, Colonial Office, Education Office, Foreign Office, Home Office, India Office, Local Government Board, Office of Woods, Paymaster-General's Office, Privy Council Office, Treasury, War Office.

I focus on two outcomes, patronage use, as before, and Clause VII usage. Patronage allows me to consider whether powerful departments were less (more) likely to use examinations (patronage). However, this does not necessarily imply that the reason for this is related to power, which is why I use Clause VII usage as an alternative outcome. Clause VII allows me to suggest that this was (at least partially) due to the increased ability to invoke loopholes that required approval from politicians, and were inherently dependent on bargaining power. I focus first on ministers. Descriptively, it is possible to plot the change in patronage use for ministerial and non-ministerial departments. This is presented in Figure A10. Despite common trends until reform there is rapid divergence where ministerial departments experience much smaller declines in patronage usage. This is true across all departments and just within treated departments. I test this empirically using a triple difference. I run the analysis defining ministers directly at the department level, but also defining it at the parent level so that 'children' departments are included. The results are presented in Figure 7. They are consistent using either definition, in the first period (1870) there is a short decline in relative patronage use before a pivot towards much greater relative use of patronage. By the end of the period the coefficients are around 100 percent greater relative probability, reflecting that non-ministerial patronage use is in effect zero amongst the treated group. To further illustrate that this is the result of bargaining power, I replace the outcome with a binary variable equal to 1 if a given candidate enters through the Clause VII loophole. This allows me to precisely measure an outcome that is (at least partially) driven by bargaining capacity amongst organisations. In Figure A12, I present the results. For the direct ministerial departments there is clear evidence of a relative increase in Clause VII usage. This difference peaks in 1873, where ministerial departments have a 43 percentage point higher likelihood that a given candidate enters through the Clause VII loophole relative to other treated, non-ministerial, departments.

I then look at Whitehall based departments using the same model. The descriptive results are given in Figure A11, suggesting common trends once again before a sharp drop that is greater for non-Whitehall departments. Interestingly the shift among non-Whitehall departments is substantially reduced on average. This leads to attenuated tripe difference estimates in Figure A13. There are some minor common trends violations, as it appears Whitehall departments used patronage at a slightly higher rate than non-Whitehall pre-treatment. After 1870 however they use it at a significantly higher level, much more so than the level of pre-trend before treatment. The Clause VII results, presented in Figure A14, show that Whitehall departments were significantly more likely to use the loophole after 1870 than non-Whitehall treated departments.

I also test for a complementary relationship between labour and power, where power permits the extent of patronage use yet functions determine whether a department uses it in the first place. Conceptually, this places power as a necessary, and functions as a sufficient condition. To do this I split the routine task variable into below and above median across departments, interacting this with the traditional difference-in-differences term as well as a final indicator variable for the presence of a minister. The quadruple interaction term ($\beta_1 \times \text{reformed}_d \times \text{post}_{i,t} \times \text{B.M.}_d \times \text{minister}_d$) gives the relative shift in patronage for treated departments below the median in routine jobs that have a minister. It therefore tests for the role of power as a complement to labour functions. The results are presented in Table 4. I find that if a department is below the median in routine work (i.e. disincentivised to use examinations), then the degree of political connection it holds significantly predicts a negative fall in relative patronage use. For Whitehall departments this is particularly evident. This illustrates the complementarity between the two mechanisms.

Table 4: Routine labour and power presence

	Dep. Var: Patronage (1/0)			
	(1)	(2)	(3)	(4)
R × P × B.M. × Minister	-0.387***			
	(0.122)			
R × P × B.M. (pre 1870) × Minister		-0.450***		
		(0.128)		
R × P × B.M. × Whitehall			-0.855***	
			(0.158)	
R × P × B.M. (pre 1870) × Whitehall				-0.886***
				(0.159)
Observations	9,275	8,777	9,275	8,777
Departments	104	92	104	92
Dept FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
Mean Dep. Var.	0.788	0.777	0.788	0.777

Notes: The regression model is a linear probability model, where I regress the interaction term (**R × P × B.M. × Minister/Whitehall**) upon a binary variable for patronage use for each individual appointment from 1864-1875 in the British civil service. B.M. stands for below median in routine jobs, which is assigned at the department level. B.M. (pre 1870) denotes that the median is calculated using the share of routine jobs before 1870, rather than the whole period. The two interactions for power are ministerial presence or being located in Whitehall. All models use department and year fixed effects. The mean of the dependent variable (patronage) over the period 1864-1875 is given at the bottom of the column. Standard errors are clustered at the department.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Unfortunately, data precludes further analysis. There is, to the best of my knowledge, no accurate record of who held superior legal authority (Acts of Parliament) over their selection process. Equally, there is no source that gives an indication of inter-organisational bargaining, or an exogenous measure of power that is uncorrelated with other dimensions. Yet, the direction of results indicates that the bundle of political access, importance and bargaining capacity permitted certain organisations to exploit grey areas in legislative design and continue their previous hiring behaviour. In particular, they appear to complement the initial mechanism, labour functions, which serves as the sufficient condition.

5.5 Alternative mechanisms

Size. Across-department variation in intakes could lead departments to shift their selection procedures. Large departments may have experienced the diminishing returns of patronage networks (which are finite), leading them to be more accepting of merit hiring given it provided access to a larger group of potential candidates for entry-level roles. This is somewhat similar to the mechanism proposed in Johnson and Libecap (1994) but in a different empirical setting. To consider this, I take the average size of annual hiring intakes pre-treatment per department, using this as a continuous triple difference term. This allows me to test whether larger departments were specifically drawn to exams due to the ability to access a wider pool of candidates. The results are presented in Table A15 (LPM) and Table A16 (panel). There is no evidence that size is an important determinant from these specifications.

Prestige. There is likely some overlap between function and prestige. It might be that clerks remain a protected class given their importance to the future of the organisation. By virtue of their capacity to occupy the 'glittering prizes' a certain class of positions might end up being focused upon. To test this, I employ a triple difference where I compare whether there was a specific change for highest tier positions within this group relative to other positions.[23] The results are presented in Figure A15. There is no relative change for this group after reform. This suggests that within-department it was the capacity for exams to test for future performance in the role that determined compliance, not a desire to protect certain positions based on their future importance to the organisation.

6 Conclusion

In this paper, I focus on how public sector reforms affect the *entire* cross-section of the public sector in history. Reform is critical to the long-run promotion of good governance, stable institutions and economic development. The reform I focus on, the removal of patronage, is commonly thought to limit political influence in the public sector, producing non-partisan, career-long, bureaucrats that prioritise state functions (Weber 1922). Yet, while reforms such as this are commonly promoted we know little about their implementation, in history or the present. In particular, there remains an open question as to how different organisations adapt to reform, an important question in understanding the long-run evolution of institutions as well providing policy-relevant research. Here, I try to provide clear empirical evidence on the dynamics of reform by studying a pivotal public sector reform in British history. Rare data at the individual-level allows me to compile data on the universe of entrants to high-tier positions and explore this heterogeneity without sample constraints. I initially estimate clear policy success, finding that reformed departments used patronage far less after reform. This result is robust to several respecifications. Exploiting heterogeneity across- and within-departments I investigate continued non-compliance with reforms. Building on a brief theoretical model to guide my analysis, I suggest that labour functions served as a crucial reason for compliance. Departments that performed tasks that were routine or technical in nature shifted towards examinations given their measure of future skill, whereas those departments who prioritised skills for non-routine, potentially confidential and difficult to monitor, work were far more non-compliant. I also find complementary evidence that power, measured by political capital, amongst non-routine departments may help determine the degree of non-compliance possible for a department. Institutions changed, but they did so in response to their own interests. Inequality in the distribution of power amongst the public sector further exacerbated this speed of change.

The results provide unique evidence on how institutional change *actually* occurs in history at the micro-level. Economic history is marked by attempts, and failures, to change institutions. In Britain, it would take until after the Second World War for the public sector to be purged of patronage. My evidence shows that departments typically acted in their own self-interest, rather than because they were compelled to. Attitudes surrounding the importance of patronage in installing candidates with the right stuff for roles that prioritised interpersonal communication, often confidential and varied work did not change. Queen Victoria, while in correspondence with Gladstone, made this clear by writing 'a young man might

[23] The positions are higher tier clerk roles, specifically Clerk, Chief Clerk, Senior Clerk, First Class Clerk, Senior First-Class Clerk, and Junior First-Class Clerk.

be very ineligible, and still after having been proclaimed to the world as first in ability' (Queen Victoria 2009, p.10-11). As such, reforms were able to reach mainly the positions where it was deemed that a man without systematic social connections was acceptable. This clearly constituted the majority of entry level positions, but it would take more reform, and further breakdown in social order, before such conventions were broken and examination-based hiring became universal in the British civil service.

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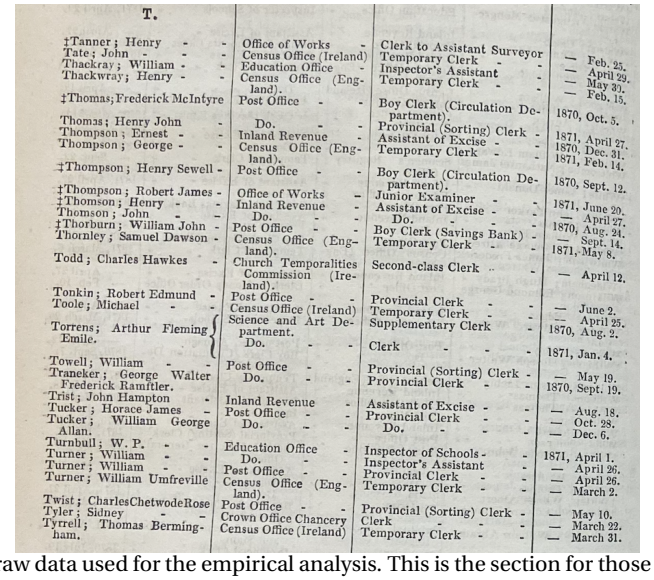
A Additional Figures and Tables

Figures

Figure A1: Example of raw data



T.

‡Tanner; Henry - -	Office of Works -	Clerk to Assistant Surveyor -	— Feb. 25.
Tate; John - - -	Census Office (Ireland)	Temporary Clerk - -	— April 29.
Thackray; William - -	Education Office -	Inspector's Assistant - -	— May 31.
Thackwray; Henry - -	Census Office (England).	Temporary Clerk - -	— Feb. 15.
‡Thomas; Frederick McIntyre	Post Office - -	Boy Clerk (Circulation Department).	1870, Oct. 5.
Thomas; Henry John -	Do. - -	Provincial (Sorting) Clerk -	1871, April 27.
Thompson; Ernest - -	Inland Revenue -	Assistant of Excise - -	1870, Dec. 31.
Thompson; George - -	Census Office (England).	Temporary Clerk - -	1871, Feb. 14.
‡Thompson; Henry Sewell -	Post Office - -	Boy Clerk (Circulation Department).	1870, Sept. 12.
‡Thompson; Robert James -	Office of Works -	Junior Examiner - -	1871, June 20.
‡Thomson; Henry - -	Inland Revenue -	Assistant of Excise - -	— April 27.
Thomson; John - -	Do. - -	Do. - - -	1870, Aug. 24.
‡Thorburn; William John -	Post Office - -	Boy Clerk (Savings Bank) -	— Sept. 14.
Thornley; Samuel Dawson -	Census Office (England).	Temporary Clerk - -	1871, May 8.
Todd; Charles Hawkes -	Church Temporalities Commission (Ireland).	Second-class Clerk .. -	— April 12.
Tonkin; Robert Edmund -	Post Office - -	Provincial Clerk - -	— June 2.
Toole; Michael - -	Census Office (Ireland)	Temporary Clerk - -	— April 25.
Torrens; Arthur Fleming Emile.	Science and Art Department.	Supplementary Clerk -	1870, Aug. 2.
	Do. - -	Clerk - - -	1871, Jan. 4.
Towell; William - -	Post Office - -	Provincial (Sorting) Clerk -	— May 19.
Traneker; George Walter Frederick Ramftler.	Do. - -	Provincial Clerk - -	1870, Sept. 19.
Trist; John Hampton -	Inland Revenue -	Assistant of Excise - -	— Aug. 18.
Tucker; Horace James -	Post Office - -	Provincial Clerk - -	— Oct. 28.
Tucker; William George Allan.	Do. - -	Do. - - -	— Dec. 6.
Turnbull; W. P. - -	Education Office -	Inspector of Schools - -	1871, April 1.
Turner; William - -	Do. - -	Inspector's Assistant - -	— April 26.
Turner; William - -	Post Office - -	Provincial Clerk - -	— April 26.
Turner; William Umfreville	Census Office (England).	Temporary Clerk - -	— March 2.
Twist; CharlesChetwodeRose	Post Office - -	Provincial (Sorting) Clerk -	— May 10.
Tyler; Sidney - -	Crown Office Chancery	Clerk - - -	— March 22.
Tyrrell; Thomas Bermingham.	Census Office (Ireland)	Temporary Clerk - -	— March 31.

Notes: The figure presents the raw data used for the empirical analysis. This is the section for those with surnames beginning with 'T' from the *Reports of the Civil Service Commission* in the year 1870-1871. Each entrants full name, department, role and date of examination are given. Limited competition entrants are denoted in the records with a †, open competition entrants are denoted with a ‡, while direct discretionary appointments have no denotion.

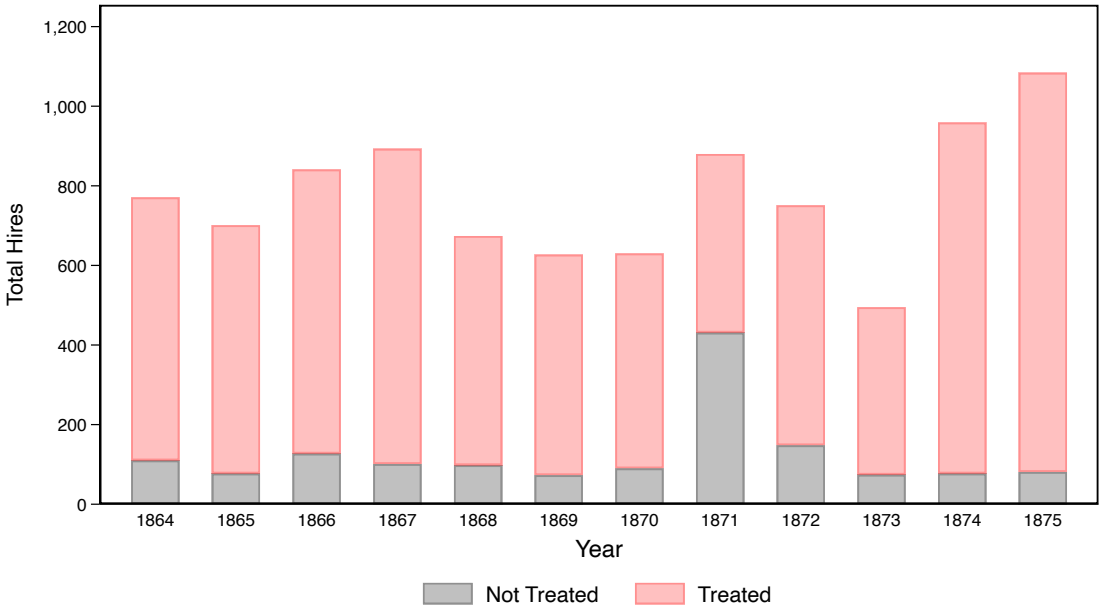
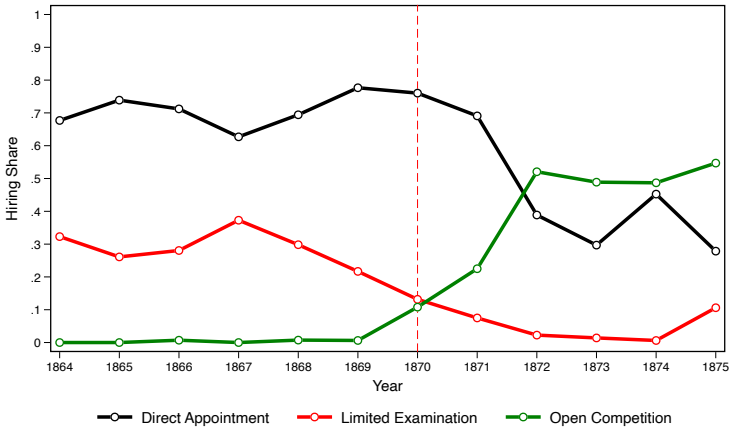
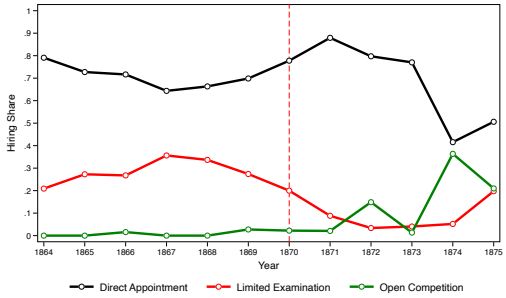


Figure A2: Annual hires split between treated and not treated departments.

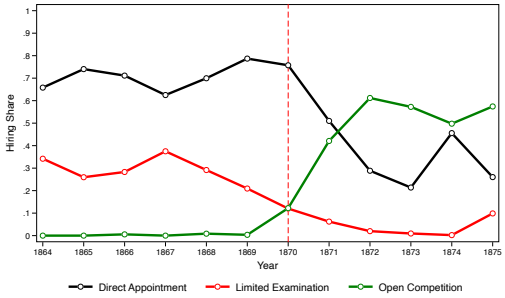
Notes: The figure presents the annual hires from 1864-1875 across the British Civil Service. It splits between treated and untreated departments in order to show the share in both groups.



(a) Direct, Limited and Open Appointments across civil service.



(b) Direct, Limited and Open Appointments in untreated.



(c) Direct, Limited and Open Appointments in treated.

Figure A3: Further Descriptive statistics, 1864–1875

Notes: The figure presents three panels describing patterns in recruitment between 1864 and 1875. Three types of appointment are included: direct, limited and open. The top panel (a) shows the annual hiring share for each type across the civil service. The bottom-left panel (b) shows the hiring shares for untreated departments. The bottom-right panel (c) shows the hiring shares for treated department. The reform year, 1870, is denoted by a red dashed line where applicable.

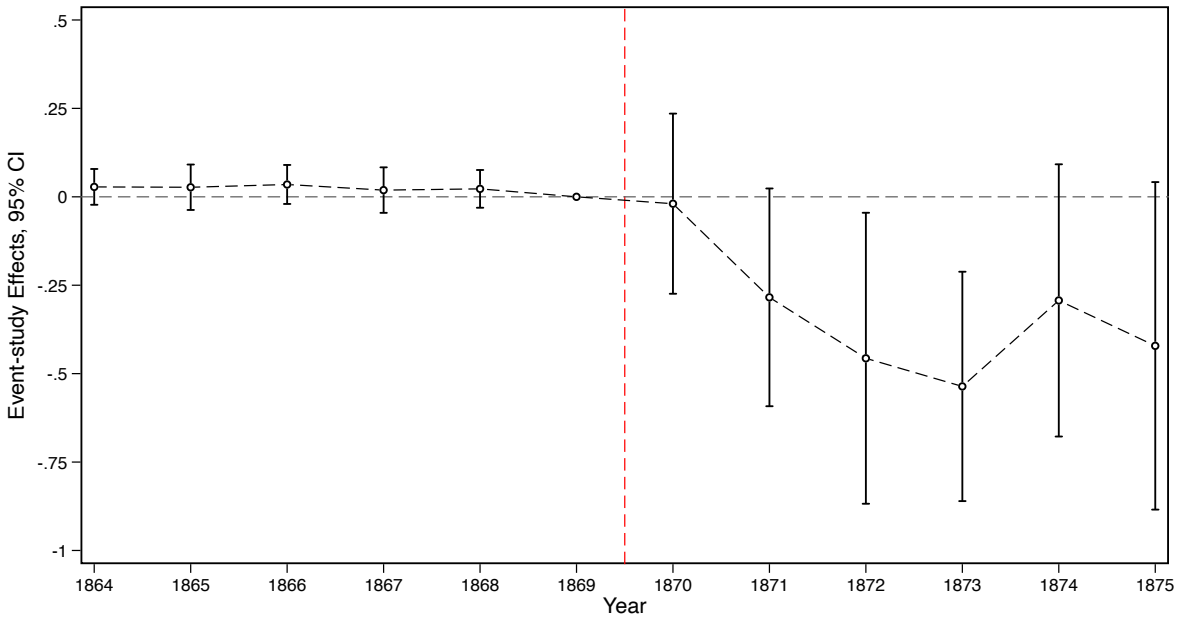


Figure A4: Patronage usage in treated departments (Panel w/ Analytic Weights)

Notes: The figure plots event-study effects for a leads and lags analogue of the regression model in Equation 2, shown in **??**. The regression follows the specification used in column 3 of Table 3. Observations are weighted using analytic weights (`aweights` in Stata) to reflect the total size of the intake for a given department-year. All estimates are relative to event time -1 (1869). Bars indicate 95% confidence intervals calculated using clustered standard errors at the department level.

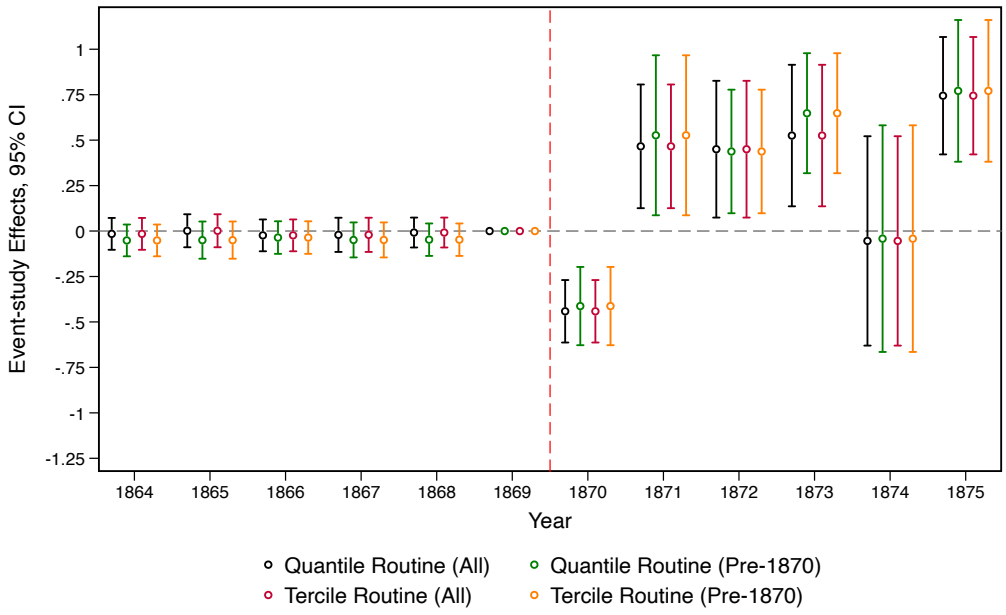


Figure A5: Patronage use in Non-Routine Departments (Robustness)

Notes: The figure plots event-study effects for four different triple difference regressions that exploit variation in department labour functions to explain non-compliance with reform. They interact a binary variable equal to one if a department is in the bottom quantile or tercile of hires classified as routine using job titles. This classification is done using either data from the whole period of pre-1870, with both results reported. All estimates are relative to event time -1 (1869). Bars indicate 95% confidence intervals calculated using clustered standard errors at the department level.

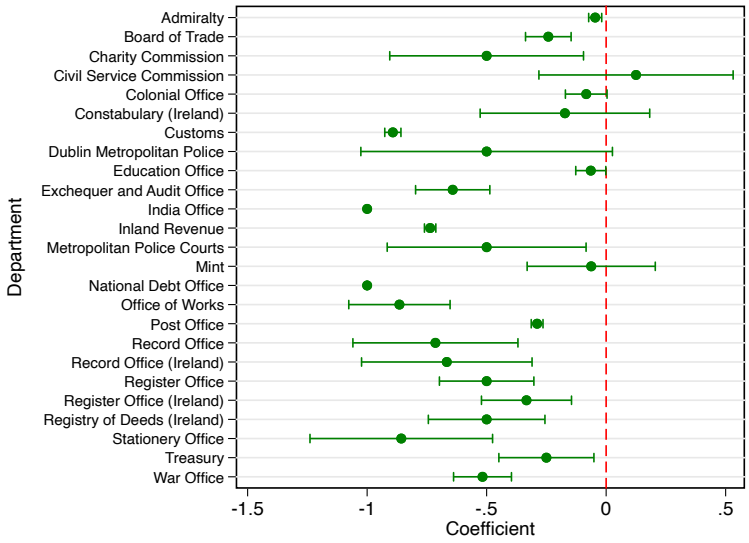


Figure A6: Change in patronage use by department

Notes: The figure plots the coefficients from a regression model that for each individual treated department regresses the post term on a binary for patronage. This gives the individual change in probability of using patronage for a given position before and after treatment (reform) for each department. The dotted red line gives a zero coefficient, or no difference. All departments that hire more than 10 people over the period 1864-1875 are included. Bars indicate 95% confidence intervals calculated using robust standard errors.

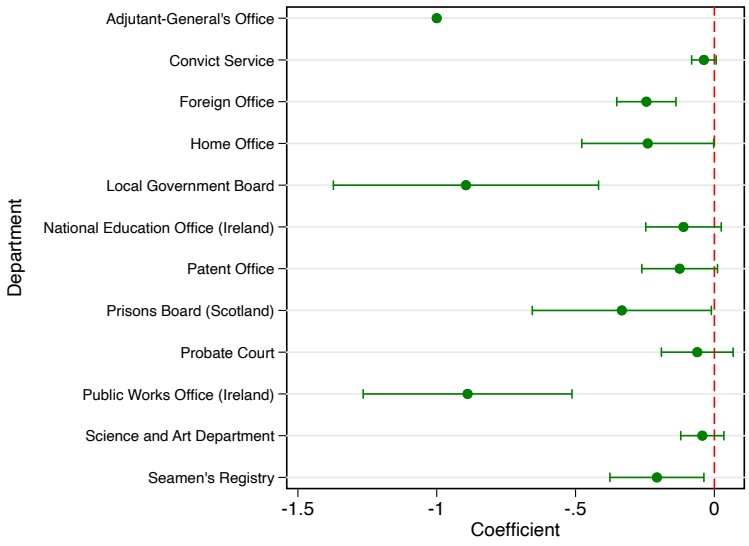


Figure A7: Change in patronage use among control group departments

Notes: The figure plots the coefficients from a regression model that for each individual treated department regresses the post term on a binary for patronage. This gives the individual change in probability of using patronage for a given position before and after treatment (reform) for each control group department. The dotted red line gives a zero coefficient, or no difference. All departments that hire more than 10 people over the period 1864-1875 are included. Bars indicate 95% confidence intervals calculated using robust standard errors.

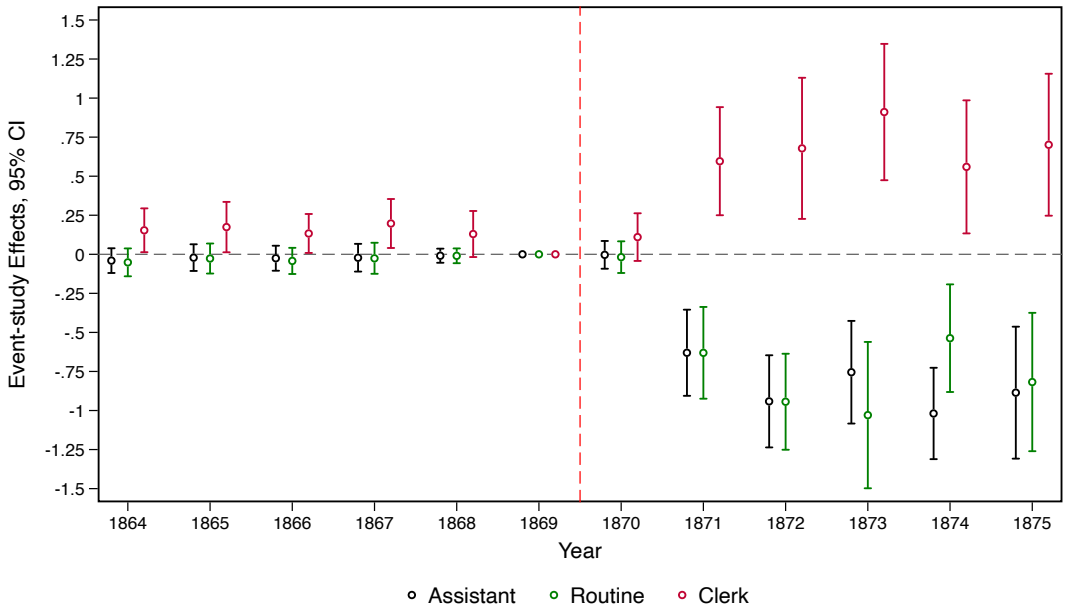
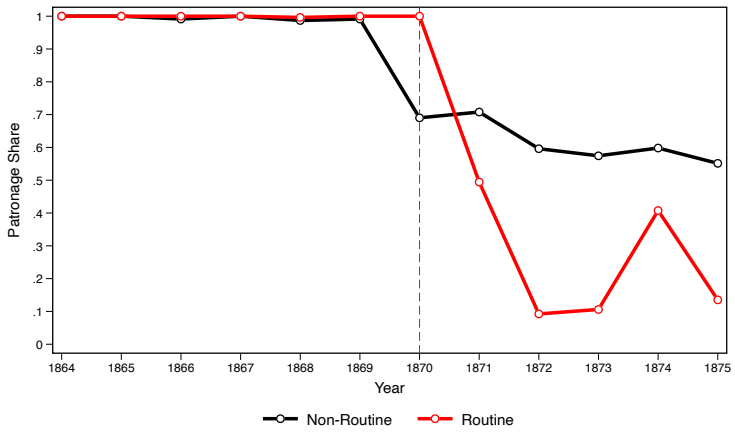
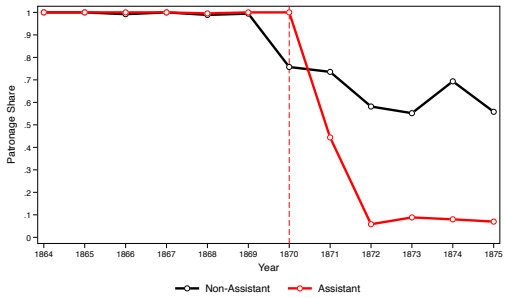


Figure A8: Patronage usage for different positions using position-department fixed effects.

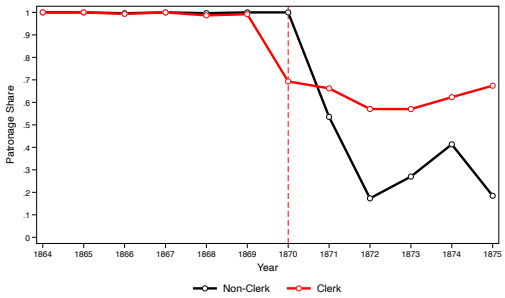
Notes: The figure plots event-study effects for three different triple difference regressions that exploit variation in function to explain non-compliance with reform. All regressions use position-department and year fixed effects. The regressions use separate binary variables equal to 1 when a given entrants takes a job that fits one of three categories: Assistant, Routine or Clerk. They are estimated separately, and plotted alongside one another. All estimates are relative to event time -1 (1869). Bars indicate 95% confidence intervals calculated using clustered standard errors at the department level.



(a) Routine jobs and patronage use.



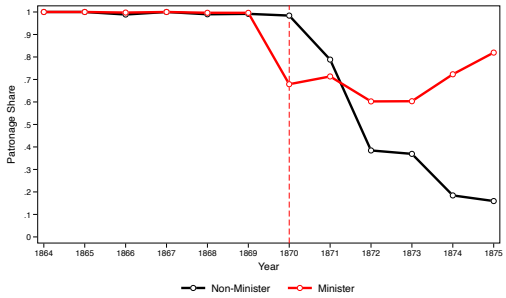
(b) Assistant jobs and patronage use.



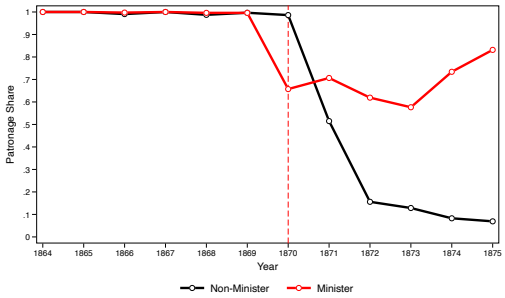
(c) Clerk jobs and patronage use.

Figure A9: Routine, Assistant and Clerk jobs and patronage use for treated departments, 1864–1875

Notes: The figure presents three panels describing patterns in recruitment among treated departments between 1864 and 1875. The sample is split along three dimensions in each panel: routine, assistant and clerk. The top panel (a) shows the annual patronage share for routine jobs versus the rest. The bottom-left panel (b) shows the annual patronage share for assistant jobs versus the rest. The bottom-right panel (c) shows the annual patronage share for clerk jobs versus the rest. The reform year, 1870, is denoted by a red dashed line where applicable.



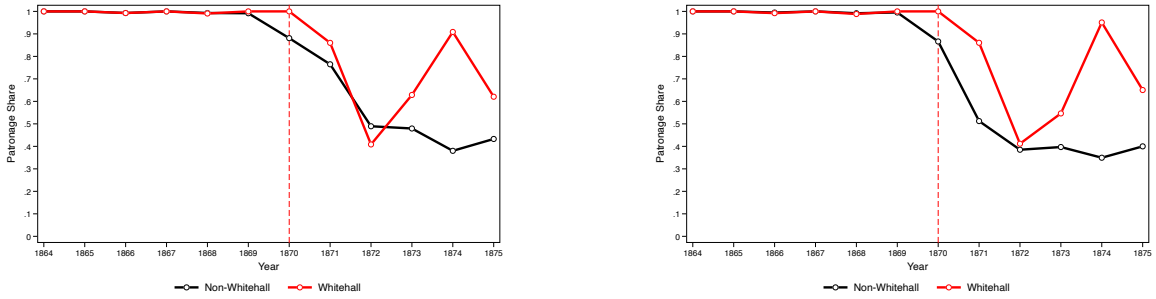
(a) Minister vs. Non-Minister All Depts.



(b) Minister vs. Non-Minister Treated Depts.

Figure A10: Descriptive comparison of ministerial versus non-ministerial departments 1864–1875

Notes: The figure presents two panels describing patterns in recruitment between 1864 and 1875. The outcome (y-axis) is the share hired through patronage. The left panel (a) shows the patronage shares for all departments, splitting them by whether they have a direct minister. The right panel (b) shows the patronage shares for treated departments alone, also splitting them by whether they have a direct minister. The reform year, 1870, is denoted by a red dashed line where applicable.



(a) Whitehall vs. Non-Whitehall All Depts.

(b) Whitehall vs. Non-Whitehall Treated Depts.

Figure A11: Descriptive comparison of Whitehall versus non-Whitehall departments 1864–1875

Notes: The figure presents two panels describing patterns in recruitment between 1864 and 1875. The outcome (y-axis) is the share hired through patronage. The left panel (a) shows the patronage shares for all departments, splitting them by whether they have a Whitehall base. The right panel (b) shows the patronage shares for treated departments alone, also splitting them by whether they have a Whitehall base. The reform year, 1870, is denoted by a red dashed line where applicable.

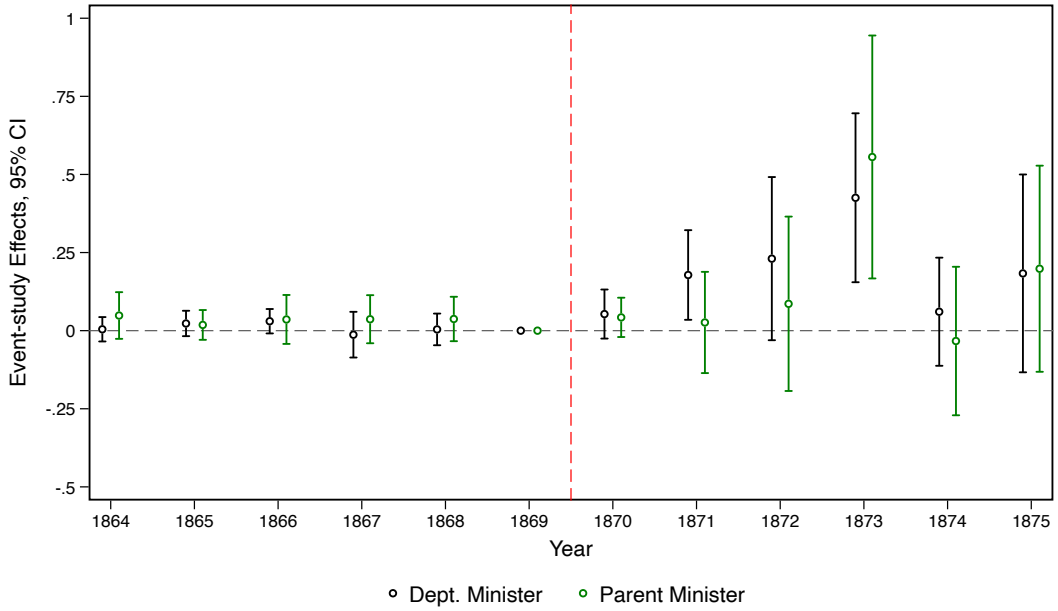


Figure A12: Ministerial presence and Clause VII usage

Notes: The figure plots event-study effects for two different triple difference regressions that exploit status in function to explain non-compliance with reform. The outcome in all regressions is the usage of Clause VII, a loophole. The regressions use a binary variable that equals one if a department has a minister within it as the third variable to interact with the difference-in-differences term. I define ministerial presence either at the department or parent department level, with the regression reformulated at the level chosen. All estimates are relative to event time -1 (1869). Bars indicate 95% confidence intervals calculated using clustered standard errors at the department and parent level for each specification respectively.

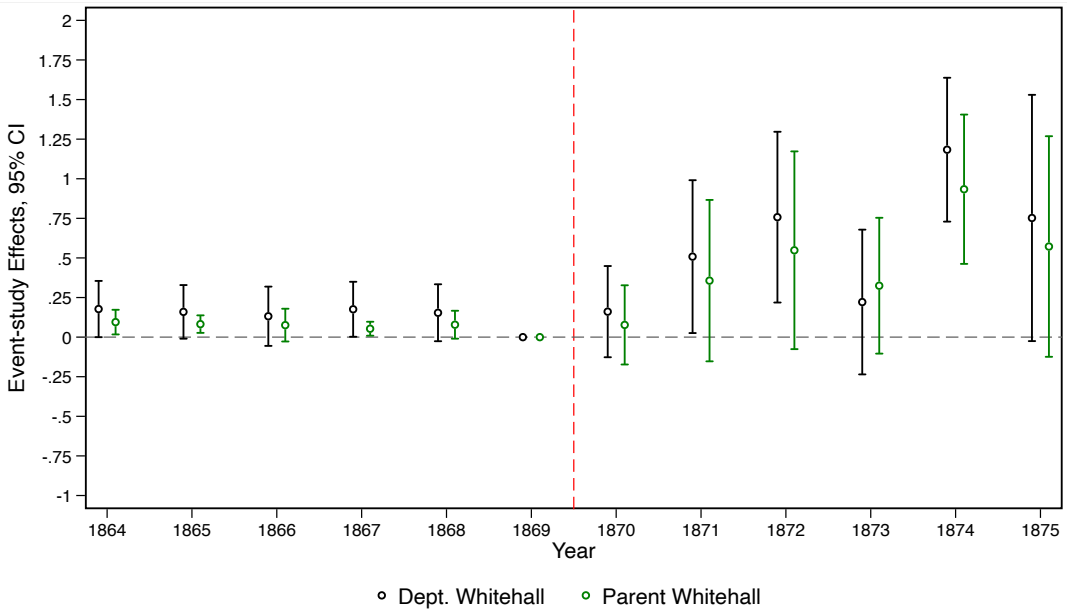


Figure A13: Whitehall presence and Patronage usage

Notes: The figure plots event-study effects for four different triple difference regressions that exploit variation in function to explain non-compliance with reform. The regressions use a binary variable that equals one if a department is based in Whitehall as the third variable to interact with the difference-in-differences term. I define Whitehall presence either at the department or parent department level, with the regression reformulated at the level chosen. All estimates are relative to event time -1 (1869). Bars indicate 95% confidence intervals calculated using clustered standard errors at the department and parent level for each specification respectively.

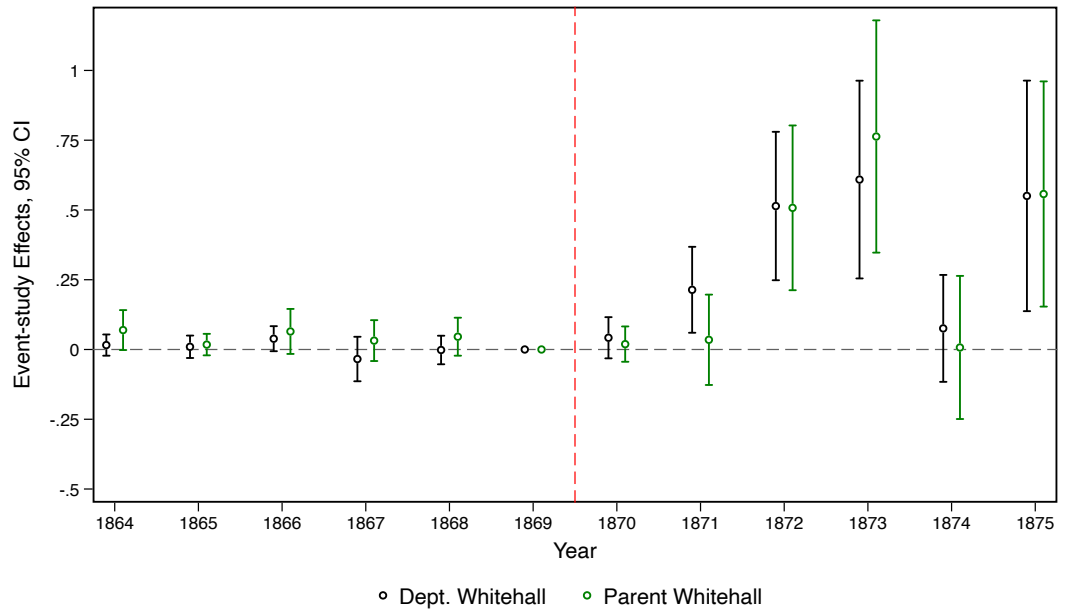


Figure A14: Whitehall presence and Clause VII usage

Notes: The figure plots event-study effects two different triple difference regressions that exploit variation in status to explain non-compliance with reform. The outcome in all regressions is the usage of Clause VII, a loophole. The regressions use a binary variable that equals one if a department is based in Whitehall as the third variable to interact with the difference-in-differences term. I define Whitehall presence either at the department or parent department level, with the regression reformulated at the level chosen. All estimates are relative to event time -1 (1869). Bars indicate 95% confidence intervals calculated using clustered standard errors at the department and parent level for each specification respectively.

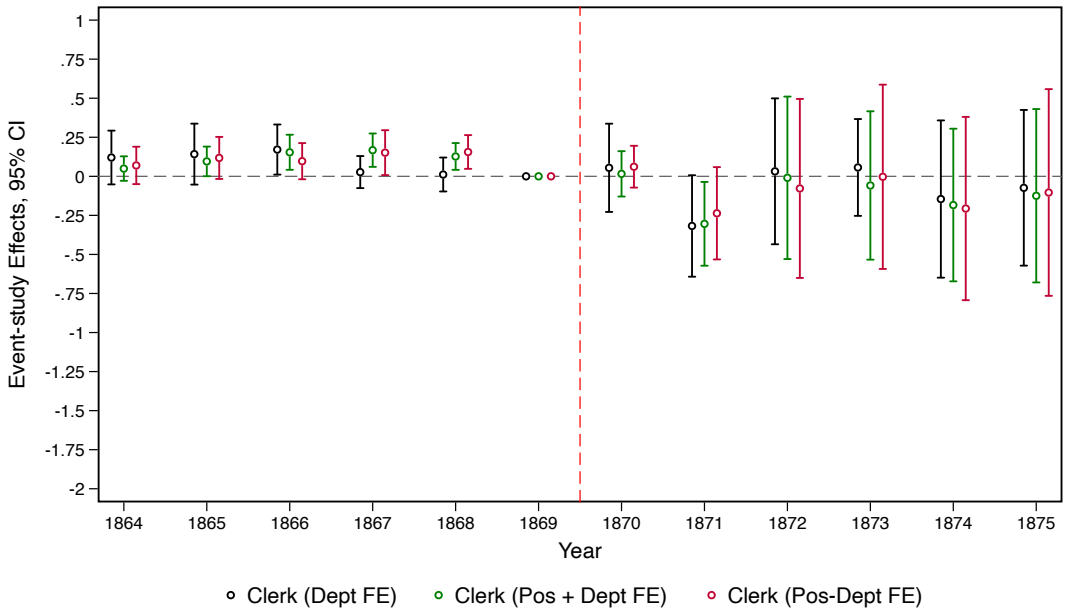


Figure A15: Higher tier clerks and Patronage usage

Notes: The figure plots event-study effects three different triple difference regressions that exploit variation in status to explain non-compliance reform. The outcome in all regressions is the usage of patronage. The regressions use a binary variable that equals one if a given entrant holds a higher tier clerk position where they have a more realistic chance of reaching the highest offices within that department. All estimates are relative to event time -1 (1869). Bars indicate 95% confidence intervals calculated using clustered standard errors at the department level.

Tables

Table A1: Departments in the analysis

Department	Department	Department
Adjutant-General's Office	Admiralty	Admiralty Court (England)
Admiralty Court (Ireland)	Bankruptcy Court (England)	Board of Supervision (Scotland)
Board of Trade	British Museum	Broadmoor Criminal Lunatic Asylum
Cape of Good Hope Observatory	Census Office (England)	Census Office (Ireland)
Census Office (Scotland)	Ceylon Public Works Department	Chancery Court (Ireland)
Chancery Pay Office	Charity Commission	Chelsea Hospital
Chief Secretary's Office	Chief Secretary's Office (Ireland)	Church Temporalities Commission (Ireland)
Civil Service Commission	Clerkenwell	Coal Mines
Colonial Convict Service	Colonial Office	Commander-in-Chief's Office
Constabulary (Ireland)	Convict Service	Copyhold, Inclosure & Tithe Commission
Council of Military Education	County Surveyorship	Court of Bankruptcy (Ireland)
Court of Common Pleas	Court of Session (Scotland)	Crown Office (Scotland)
Crown Office Chancery	Customs	Diplomatic Service
Dublin Metropolitan Police	Duchy of Lancaster	Dundrum Central Criminal Lunatic Asylum
Ecclesiastical Commission	Edinburgh Observatory	Education Board (Scotland)
Education Office	Emigration Office	Endowed Schools Commission
Endowed Schools Commission (Scotland)	Epping Forest Commission	Examiner of Criminal Law Accounts
Exchequer (Ireland)	Exchequer and Audit Office	Exchequer of Pleas
Factory Inspectors' Department	Fisheries Board (Scotland)	Fisheries Office (Ireland)
Foreign Office	Four Courts (Ireland)	Friendly Societies Commission
Habitual Criminals Registry (Ireland)	Home Office	House of Commons
India Audit Office	India Office	Inland Revenue
Inspector of Reformatories	Inspector-General Prisons (Ireland)	Joint Stock Companies' Registry
Judgements Registration Office (Ireland)	Land Revenue Record Office	Landed Estates Court (Ireland)
Loan Fund Board (Ireland)	Local Government Board	London Gazette
London University	Lunacy Board (Scotland)	Lunacy Commission
Lunatic Asylums (Ireland)	Mercantile Marine Office	Metropolitan Police
Metropolitan Police (Commissioner's Office)	Metropolitan Police Courts	Military Offices (Ireland)
Mint	National Debt Office	National Education Office (Ireland)
Nautical Almanac Office	Office of Comptroller	Office of Direct of Chancery (Scotland)
Office of Examiners	Office of Woods	Office of Works
Parliament Office	Patent Office	Paymaster-General's Office
Poor Law Commission	Poor Law Commission (Ireland)	Post Office
Prisons Board (Scotland)	Privy Council Office	Probate Court
Probate Court (Ireland)	Public Works Loan Office	Public Works Office (Ireland)
Quartermaster-General's Office	Quartermaster-General's Office (Ireland)	Queen's Bench
Queen's University (Ireland)	Q.L.T.R.O (Scotland)	Railway Commission
Record Office	Record Office (Ireland)	Register Office
Register Office (Ireland)	Register Office (Scotland)	Register of Judgements
Registry of Deeds (Ireland)	Registry of Designs	Registry of Judgements (Ireland)
Registry of Petty Sessions Clerks (Ireland)	Royal Hibernian Military School	Royal Irish Academy
Royal Observatory	Royal Parks	Science and Art Department
Seamen's Registry	State Paper Office (Dublin)	Stationery Office
Temporary Commissions	Treasury	Universities' Commission
Unseaworthy Ships Commission	Valuation Office (Ireland)	War Office

Notes: The table above gives all listed departments given as hiring an entrant in the original source material. The source material is cleaned (and standardised) for spelling mistakes, name changes and slight differences over time. Q.L.T.R.O (Scotland) stands for Queen's and Lord Treasurer's Rememberancer's Office (Scotland).

Table A2: Parent Departments in the analysis

Department	Department	Department
Admiralty	Admiralty Court (Ireland)	Board of Supervision (Scotland)
Board of Trade	British Museum	Census Office (England)
Census Office (Ireland)	Census Office (Scotland)	Ceylon Public Works Department
Chancery Court (Ireland)	Charity Commission	Chief Secretary's Office
Chief Secretary's Office (Ireland)	Church Temporalities Commission (Ireland)	Civil Service Commission
Clerkenwell	Colonial Office	Constabulary (Ireland)
County Surveyorship	Court of Bankruptcy (Ireland)	Court of Session (Scotland)
Crown Office (Scotland)	Customs	Dublin Metropolitan Police
Duchy of Lancaster	Ecclesiastical Commission	Education Board (Scotland)
Education Office	Endowed Schools Commission (Scotland)	Epping Forest Commission
Exchequer (Ireland)	Exchequer and Audit Office	Fisheries Board (Scotland)
Fisheries Office (Ireland)	Foreign Office	Four Courts (Ireland)
Friendly Societies Commission	Habitual Criminals Registry (Ireland)	Home Office
House of Commons	India Office	Inland Revenue
Inspector-General Prisons (Ireland)	Judgements Registration Office (Ireland)	Landed Estates Court (Ireland)
Loan Fund Board (Ireland)	Local Government Board	London University
Lunacy Board (Scotland)	Lunacy Commission	Lunatic Asylums (Ireland)
Metropolitan Police	Military Offices (Ireland)	National Debt Office
National Education Office (Ireland)	Office of Direct of Chancery (Scotland)	Office of Examiners
Office of Woods	Office of Works	Parliament Office
Patent Office	Paymaster-General's Office	Poor Law Commission (Ireland)
Post Office	Prisons Board (Scotland)	Privy Council Office
Probate Court (Ireland)	Public Works Loan Office	Public Works Office (Ireland)
Quartermaster-General's Office (Ireland)	Queen's University (Ireland)	Q.L.T.R.O (Scotland)
Railway Commission	Record Office	Record Office (Ireland)
Register Office (Ireland)	Register Office (Scotland)	Registry of Deeds (Ireland)
Registry of Judgements (Ireland)	Registry of Petty Sessions Clerks (Ireland)	Royal Irish Academy
Royal Mint	State Paper Office (Dublin)	Stationery Office
Supreme Court of Judicature	Temporary Commissions	Treasury
Unseaworthy Ships Commission	Valuation Office (Ireland)	War Office

Notes: The table above gives all listed parent departments given as hiring an entrant in the original source material. The source material is cleaned (and standardised) for spelling mistakes, name changes and slight differences over time. Q.L.T.R.O (Scotland) stands for Queen's and Lord Treasurer's Rememberancer's Office (Scotland).

Table A3: Ministerial Departments in the analysis

Department	Department	Department
Adjutant-General's Office	**Admiralty**	Admiralty Court (England)
Board of Trade	Broadmoor Criminal Lunatic Asylum	Cape of Good Hope Observatory
Chelsea Hospital	**Chief Secretary's Office (Ireland)**	Coal Mines
Colonial Convict Service	**Colonial Office**	Commander-in-Chief's Office
Convict Service	Copyhold, Inclosure & Tithe Commission	Council of Military Education
Diplomatic Service	**Duchy of Lancaster**	Dundrum Central Criminal Lunatic Asylum
Edinburgh Observatory	Emigration Office	Factory Inspectors' Department
Foreign Office	**Home Office**	India Audit Office
India Office	Inspector of Reformatories	Joint Stock Companies' Registry
Local Government Board	Mercantile Marine Office	Nautical Almanac Office
Office of Works	**Paymaster-General's Office**	Poor Law Commission
Post Office	**Privy Council Office**	Quartermaster-General's Office
Register Office	Registry of Designs	Royal Hibernian Military School
Royal Observatory	Royal parks	Seamen's Registry
Treasury	**War Office**	

Notes: The table above gives the departments in the civil service that have a minister in the Gladstone Ministry. Direct ministers are given by the bold font, reflecting the most obvious connection to the ministry. The non-bold departments are those connected through their parent department (i.e. the Convict Service is connected to the Home Secretary given its parent is the Home Office).

Table A4: Whitehall Departments in the analysis

Department	Department	Department
Adjutant-General's Office	**Admiralty**	**Admiralty Court (England)**
Board of Trade	Broadmoor Criminal Lunatic Asylum	Cape of Good Hope Observatory
Chelsea Hospital	Coal Mines	Colonial Convict Service
Colonial Office	Commander-in-Chief's Office	Convict Service
Copyhold, Inclosure & Tithe Commission	Council of Military Education	Diplomatic Service
Dundrum Central Criminal Lunatic Asylum	Edinburgh Observatory	**Education Office**
Emigration Office	Factory Inspectors' Department	**Foreign Office**
Home Office	India Audit Office	**India Office**
Inspector of Reformatories	Joint Stock Companies' Registry	**Local Government Board**
Mercantile Marine Office	Nautical Almanac Office	**Office of Woods**
Paymaster-General's Office	Poor Law Commission	**Privy Council Office**
Quartermaster-General's Office	Register Office	Registry of Designs
Royal Hibernian Military School	Royal Observatory	Science and Art Department
Seamen's Registry	**Treasury**	Universities' Commission
War Office		

Notes: The table above gives the departments in the civil service that have a Whitehall headquarters found in *The British Imperial Calendar and Civil Service List.* Direct ministers are given by the bold font. The non-bold departments are those connected through their parent department (i.e. the Convict Service is connected to Whitehall given its parent is the Home Office).

Table A5: Treated Departments in the analysis

Department	Department	Department
Admiralty	Board of Trade	Charity Commission
Chief Secretary's Office (Ireland)	Civil Service Commission	Colonial Office
Constabulary (Ireland)	Customs	Dublin Metropolitan Police
Education Office	Emigration Office	Exchequer and Audit Office
India Audit Office	India Office	Inland Revenue
Inspector of Reformatories	Joint Stock Companies' Registry	London Gazette
London University	Lunacy Commission	Metropolitan Police Courts
Mint	National Debt Office	Office of Woods
Office of Works	Paymaster-General's Office	Poor Law Commission
Post Office	Privy Council Office	Q.L.T.R.O (Scotland)
Record Office	Record Office (Ireland)	Register Office
Register Office (Ireland)	Register of Judgements	Registry of Deeds (Ireland)
Registry of Designs	Registry of Petty Sessions Clerks (Ireland)	Science and Art Department
Seamen's Registry	Stationery Office	Treasury
War Office		

Notes: The table above gives the departments listed as treated in Schedule A of the 1870 Order in Council. These were the departments expressively ordered to implement open access examination as the stock method of appointment for entry level positions. Q.L.T.R.O (Scotland) stands for Queen's and Lord Treasurer's Rememberancer's Office (Scotland).

Table A6: Conditional DiD Estimates

	Dep. Var: Patronage Share (%)				
	(1)	(2)	(3)	(4)	(5)
Pre-Treatment Average	0.00775	0.00779	0.00772	0.00768	0.00768
	(0.00756)	(0.00756)	(0.00842)	(0.00755)	(0.00842)
ATT	-0.262***	-0.233***	-0.264***	-0.211***	-0.214**
	(0.0682)	(0.0789)	(0.0683)	(0.0771)	(0.0837)
Observations	487	487	487	487	487
Dept FE	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓
Pre-treatment Hiring	✓	✓	✓	✓	✓
Foreign Dept.		✓			✓
Commission			✓		✓
Minister				✓	✓
Stable IPW	✓	✓	✓	✓	✓
Mean Dep. Var.	0.884	0.884	0.884	0.884	0.884

Notes: The above tables shows the results of the DiD estimator provided by Callaway and SantAnna 2021. This allows one to condition on several variables, which are given by the checkmarks in the table. Results are reported for the average treatment effect on the treated and the pre-treatment average.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table A7: Panel DiD controlling for intake size

	Dep. Var: Patronage Share (%)					
	(1)	(2)	(3)	(4)	(5)	(6)
Reform × Post	-0.375***	-0.368***	-0.341***	-0.383***	-0.360***	-0.368***
	(0.0588)	(0.0670)	(0.0641)	(0.0605)	(0.0585)	(0.0604)
Observations	608	460	524	596	596	584
Departments	95	65	82	94	94	93
Dept FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
No Foreign Dept.		✓				
No Commissions			✓			
No Post Office				✓		
No Inland Revenue					✓	
No I.R or P.O						✓
Mean Dep. Var.	0.870	0.859	0.867	0.871	0.875	0.876

Notes: The regression model is a panel difference in differences, where I regress the interaction term (reformed × post-treatment) on the share of a departments entrants that enter through patronage. I control for the time-variant size of hiring class in each department-year to avoid department growth contaminating my estimates. All models use department and year fixed effects. Sample respecifications in columns (2)-(6) are denoted using checkmarks, and explained in text. This leads to changes in the number of departments and observations included in each regression. Fewer departments are found in this sample due to departments that recruit multiple people in one year, so are singletons in this sample but not in the individual-level data. The mean of the dependent variable (patronage) over the period 1864-1875 is given at the bottom of the column. Standard errors are clustered at the department.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table A8: ATT's using matching

	Dep. Var: Patronage Share (%)				
Matching Technique:	NN (2)	NN (3)	NN (4)	PS	CEM
	(1)	(2)	(3)	(4)	(5)
Reformed × Post	-0.432***	-0.441***	-0.425***	-0.441***	-0.330***
	(0.0640)	(0.0565)	(0.0557)	(0.0404)	(0.0719)
Observations	599	599	599	599	479
Mean Dep. Var.	0.872	0.872	0.872	0.872	0.853

Notes: The above table gives a series of robustness checks done using different matching techniques. The technique used is given by the title at the top of each column. The final column uses CEM matching to weight a simple regression (without fixed effects) of the treatment. All matching is done using the following time invariant covariates: pre-treatment hiring shares for each department (direct and by nomination examinations), whether the department is foreign, is a commission or has a minister.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table A9: LPM DiD w/ Parents

	Dep. Var: Patronage (1/0)					
	(1)	(2)	(3)	(4)	(5)	(6)
Reform × Post	-0.300**	-0.300**	-0.265**	-0.400***	-0.182**	-0.216*
	(0.124)	(0.128)	(0.128)	(0.124)	(0.0789)	(0.111)
Observations	9,288	8,629	9,028	6,567	6,644	3,923
Parent Depts.	72	40	62	71	71	70
Parent FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
No Foreign Dept.		✓				
No Commissions			✓			
No Post Office				✓		
No Inland Revenue					✓	
No I.R or P.O						✓
Mean Dep. Var.	0.788	0.775	0.784	0.762	0.850	0.851

Notes: The regression model is a linear probability model, where I regress the interaction term (reformed × post-treatment) upon a binary variable for patronage use for each individual appointment from 1864-1875 in the British civil service, thus estimating whether reformed departments changed their manner of appointment post reform relative to unreformed departments. The specification is the same as that used in Table 2 but using the parent departments rather than the individual departments to account for potential influence over umbrella departments. All models use parent and year fixed effects. Sample respecifications in columns (2)-(6) are denoted using checkmarks, and explained in text. This leads to changes in the number of departments and observations included in each regression. The mean of the dependent variable (patronage) over the period 1864-1875 is given at the bottom of the column. Standard errors are clustered at the parent department.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table A10: Panel DiD w/ Parents

	Dep. Var: Patronage Share (%)					
	(1)	(2)	(3)	(4)	(5)	(6)
Reform × Post	-0.323***	-0.290***	-0.291***	-0.333***	-0.307***	-0.317***
	(0.0684)	(0.0843)	(0.0762)	(0.0705)	(0.0687)	(0.0710)
Observations	465	317	403	453	453	441
Parent Depts.	68	38	60	67	67	66
Dept FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
No Foreign Dept.		✓				
No Commissions			✓			
No Post Office				✓		
No Inland Revenue					✓	
No I.R or P.O						✓
Mean Dep. Var.	0.852	0.828	0.848	0.853	0.859	0.859

Notes: The regression model is a panel difference in differences, where I regress the interaction term (reformed × post-treatment) on the share of a departments entrants that enter through patronage. The specification is the same as that used in Table 3 but using the parent departments rather than the individual departments to account for potential influence over umbrella departments. All models use parent and year fixed effects. Sample respecifications in columns (2)-(6) are denoted using checkmarks, and explained in text. This leads to changes in the number of parent and observations included in each regression. Fewer parents are found in this sample due to departments that recruit multiple people in one year, so are singletons in this sample but not in the individual-level data. The mean of the dependent variable (patronage) over the period 1864-1875 is given at the bottom of the column. Standard errors are clustered at the parent.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table A11: Baseline DiD w/ Position Group Fixed Effects

	Dep. Var: Patronage (1/0)		
	(1)	(2)	(3)
Reform × Post	-0.343***	-0.344***	-0.296***
	(0.121)	(0.122)	(0.0855)
Observations	9,275	9,275	9,275
Departments	104	104	104
Dept FE	✓	✓	✓
Year FE	✓	✓	✓
Position Group FE		✓	
Position Group × Year FE			✓
Mean Dep. Var.	0.788	0.788	0.788

Notes: The regression model is a linear probability model, where I regress the interaction term (reformed × post-treatment) upon a binary variable for patronage use for each individual appointment from 1864-1875 in the British civil service, thus estimating whether reformed departments changed their manner of appointment post reform relative to unreformed departments. All models use department and year fixed effects. Respecifications in columns (2)-(3) add different forms of position fixed effect to the baseline specification given in column (1). I use position group fixed effects given the enormous number of specific positions. The groups are 'Clerk', 'Assistant' and 'Other'. The mean of the dependent variable (patronage) over the period 1864-1875 is given at the bottom of the column. Standard errors are clustered at the department.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table A12: LPM DiD w/o Home Office, Foreign Office & Civil Service Commission

	Dep. Var: Patronage (1/0)					
	(1)	(2)	(3)	(4)	(5)	(6)
Reform × Post	-0.364***	-0.375***	-0.337***	-0.476***	-0.246***	-0.303***
	(0.120)	(0.123)	(0.121)	(0.111)	(0.0685)	(0.103)
Observations	9,090	8,431	8,833	6,369	6,446	3,725
Departments	101	69	85	100	100	99
Parent FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
No Foreign Dept.		✓				
No Commissions			✓			
No Post Office				✓		
No Inland Revenue					✓	
No I.R or P.O						✓
Mean Dep. Var.	0.788	0.775	0.784	0.761	0.852	0.854

Notes: The regression model is a linear probability model, where I regress the interaction term (reformed × post-treatment) upon a binary variable for patronage use for each individual appointment from 1864-1875 in the British civil service, thus estimating whether reformed departments changed their manner of appointment post reform relative to unreformed departments. The specification is the same as that used in Table 2 but in all regressions three departments are dropped to avoid potential selection issues w.r.t. treatment. They are the Home Office, Foreign Office and Civil Service Commission. All models use department and year fixed effects. Sample respecifications in columns (2)-(6) are denoted using checkmarks, and explained in text. This leads to changes in the number of departments and observations included in each regression. The mean of the dependent variable (patronage) over the period 1864-1875 is given at the bottom of the column. Standard errors are clustered at the department.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table A13: Panel DiD w/o Home Office, Foreign Office & Civil Service Commission

	Dep. Var: Patronage Share (%)					
	(1)	(2)	(3)	(4)	(5)	(6)
Reform × Post	-0.395***	-0.397***	-0.363***	-0.405***	-0.381***	-0.391***
	(0.0603)	(0.0684)	(0.0667)	(0.0621)	(0.0604)	(0.0624)
Observations	578	430	494	566	566	554
Departments	92	62	79	91	91	90
Dept FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
No Foreign Dept.		✓				
No Commissions			✓			
No Post Office				✓		
No Inland Revenue					✓	
No I.R or P.O						✓
Mean Dep. Var.	0.878	0.868	0.876	0.878	0.883	0.884

Notes: The regression model is a panel difference in differences, where I regress the interaction term (reformed × post-treatment) on the share of a departments entrants that enter through patronage. The specification is the same as that used in Table 3 but in all regressions three departments are dropped to avoid potential selection issues w.r.t. treatment. They are the Home Office, Foreign Office and Civil Service Commission. All models use department and year fixed effects. Sample respecifications in columns (2)-(6) are denoted using checkmarks, and explained in text. This leads to changes in the number of departments and observations included in each regression. Fewer departments are found in this sample due to departments that recruit multiple people in one year, so are singletons in this sample but not in the individual-level data. The mean of the dependent variable (patronage) over the period 1864-1875 is given at the bottom of the column. Standard errors are clustered at the department.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table A14: Probit and Logit models

	Dep. Var: Patronage (1/0)			
	(1)	(2)	(3)	(4)
Reformed × Post	-1.747***	-3.537***	-1.746***	-3.535***
	(0.520)	(1.220)	(0.206)	(0.583)
Observations	9,306	9,306	9,307	9,307
Probit	✓			
Logit		✓		
Probit (Jackknife)			✓	
Logit (Jackknife)				✓
Mean Dep. Var.	0.788	0.788	0.788	0.788

Notes: The above table gives a series of robustness checks done using probit and logit specifications instead of the baseline LPM model. These techniques use individual data, as in Table 2, but without any fixed effects due to the nature of probit/logit models. The type of model is given in the footnotes of the table – the only change in the latter two columns is the introduction of jackknifed standard errors to further illustrate robustness (denoted JK in column title).
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table A15: LPM triple difference using pre-treatment size of hiring

	Dep. Var: Patronage (1/0)					
	(1)	(2)	(3)	(4)	(5)	(6)
Reform $\times$ **Post**	-0.236	-0.244	-0.178	-0.149	-0.308*	-0.244**
	(0.177)	(0.190)	(0.186)	(0.147)	(0.157)	(0.109)
R $\times$ **P** $\times$ **Pre-Hires**	-0.00369	-0.00212	-0.00755	-0.00576	-0.00223	-0.00404
	(0.00805)	(0.00898)	(0.00867)	(0.00878)	(0.00782)	(0.00940)
Observations	9,275	8,616	9,018	6,554	6,631	3,910
Departments	104	72	88	103	103	102
Dept FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
No Foreign Dept.		✓				
No Commissions			✓			
No Post Office				✓		
No Inland Revenue					✓	
No I.R or P.O						✓
Mean Dep. Var.	0.788	0.775	0.784	0.762	0.851	0.851

Notes: The regression model is a linear probability model, where I use a triple difference to consider whether the effect of my difference-in-difference term (reformed $\times$ post-treatment) varies based on the hiring size of a given department. To do this I take the average hiring intake per year from 1864-69, with departments that do not make a single hire give a zero value. The term **Reform** $\times$ **Post** gives the effect at a zero value, while **R** $\times$ **P** $\times$ **Pre-Hires** gives the effect of a one unit increase (one person) in average hiring pre-treatment. All models use department and year fixed effects. Sample respecifications in columns (2)-(6) are denoted using checkmarks, and explained in text. This leads to changes in the number of departments and observations included in each regression. The mean of the dependent variable (patronage) over the period 1864-1875 is given at the bottom of the column. Standard errors are clustered at the department.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table A16: Panel triple difference using pre-treatment size of hiring

	Dep. Var: Patronage Share (%)					
	(1)	(2)	(3)	(4)	(5)	(6)
Reform $\times$ **Post**	-0.347***	-0.336***	-0.290***	-0.340***	-0.357***	-0.369***
	(0.0700)	(0.0820)	(0.0766)	(0.0700)	(0.0691)	(0.0724)
R $\times$ **P** $\times$ **Pre-Hires**	-0.00600	-0.00490	-0.0112	-0.00694	-0.00475	-0.00315
	(0.00835)	(0.00877)	(0.00921)	(0.00836)	(0.00826)	(0.00953)
Observations	608	460	524	596	596	584
Departments	95	65	82	94	94	93
Dept FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
No Foreign Dept.		✓				
No Commissions			✓			
No Post Office				✓		
No Inland Revenue					✓	
No I.R or P.O						✓
Mean Dep. Var.	0.870	0.859	0.867	0.871	0.875	0.876

Notes: The regression model is a panel difference in differences, where I use a triple difference to consider whether the effect of my difference-in-difference term (reformed $\times$ post-treatment) varies based on the hiring size of a given department. To do this I take the average hiring intake per year from 1864-69, with departments that do not make a single hire give a zero value. The term **Reform** $\times$ **Post** gives the effect at a zero value, while **R** $\times$ **P** $\times$ **Pre-Hires** gives the effect of a one unit increase (one person) in average hiring pre-treatment. All models use department and year fixed effects. Sample respecifications in columns (2)-(6) are denoted using checkmarks, and explained in text. This leads to changes in the number of departments and observations included in each regression. Fewer departments are found in this sample due to departments that recruit multiple people in one year, so are singletons in this sample but not in the individual-level data. The mean of the dependent variable (patronage) over the period 1864-1875 is given at the bottom of the column. Standard errors are clustered at the department.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

B Mechanism results using department panel

The baseline results use an individual-level model, with panel results used to shown consistency and assuage concerns about a form of disaggregation bias. In this section I replicate the mechanism results using a departmental panel. For the results that exploit position-department variation, I construct a panel at this level. I focus on the three main mechanisms: across- and within-department labour functions, as well as power across departments.

Across-department labour functions. I re-estimate the main results from this section of the mechanism. The joint f-statistic always fails to reject the null hypothesis that pre-treatment coefficients are equal to zero, and there is no evidence across specifications of any individual pre-trends. Effects are qualitatively similar to those in the main text of the paper.

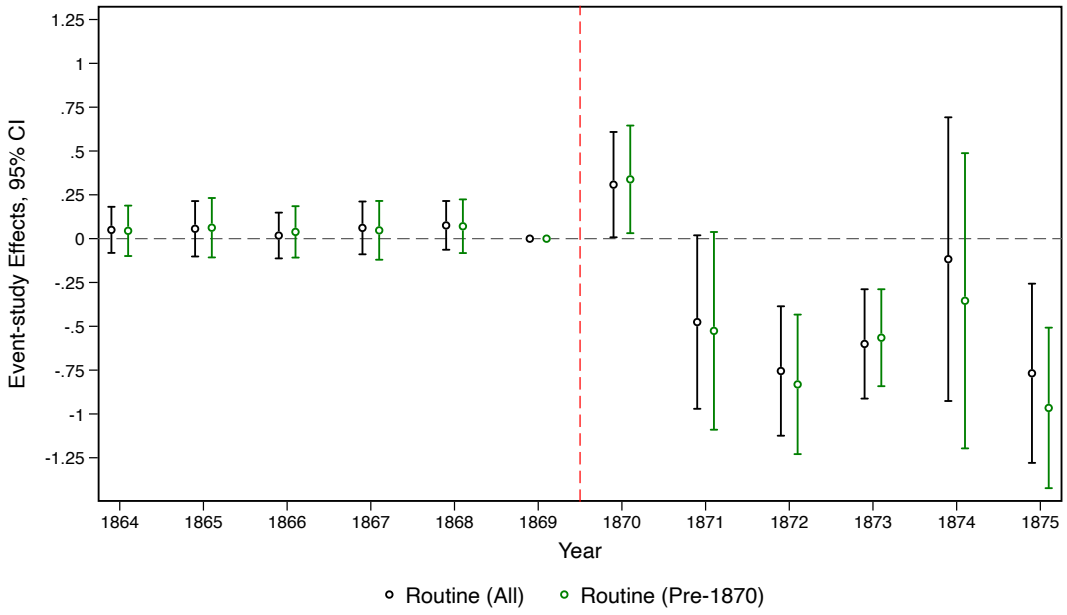


Figure A16: Routine presence and Patronage usage (Dept)

Notes: The figure plots event-study effects for the following regressions using a panel data set, so that the outcome is the share hired through patronage. All regressions are weighted by the number of entrants to a given department in that year. The regressions interact a continuous variable that measures the departments share of hires that are classified as routine workers, using either data from the whole period or pre-1870. All estimates are relative to event time -1 (1869). Bars indicate 95% confidence intervals calculated using clustered standard errors at the department level.

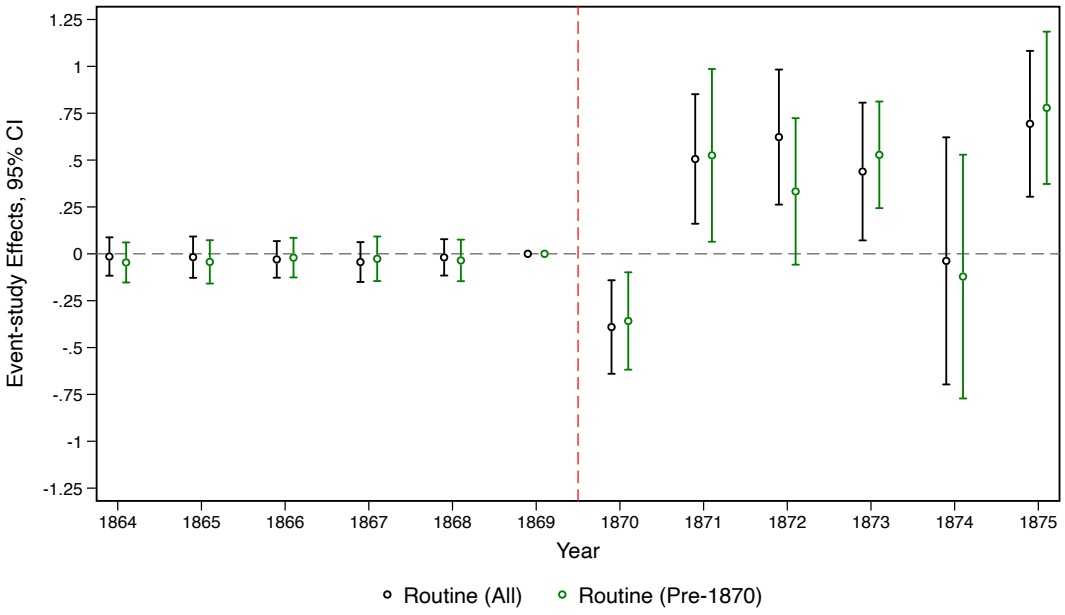


Figure A17: Non-Routine presence and Patronage usage (Dept)

Notes: The figure plots event-study effects for the following regressions using a panel data set, so that the outcome is the share hired through patronage. All regressions are weighted by the number of entrants to a given department in that year. The regressions interact a binary variable equal to 1 if a department is in the bottom quartile of hires classified as routine using job titles. This classification is done using either data from the whole period or pre-1870, with both results reported. All estimates are relative to event time -1 (1869). Bars indicate 95% confidence intervals calculated using clustered standard errors at the department level.

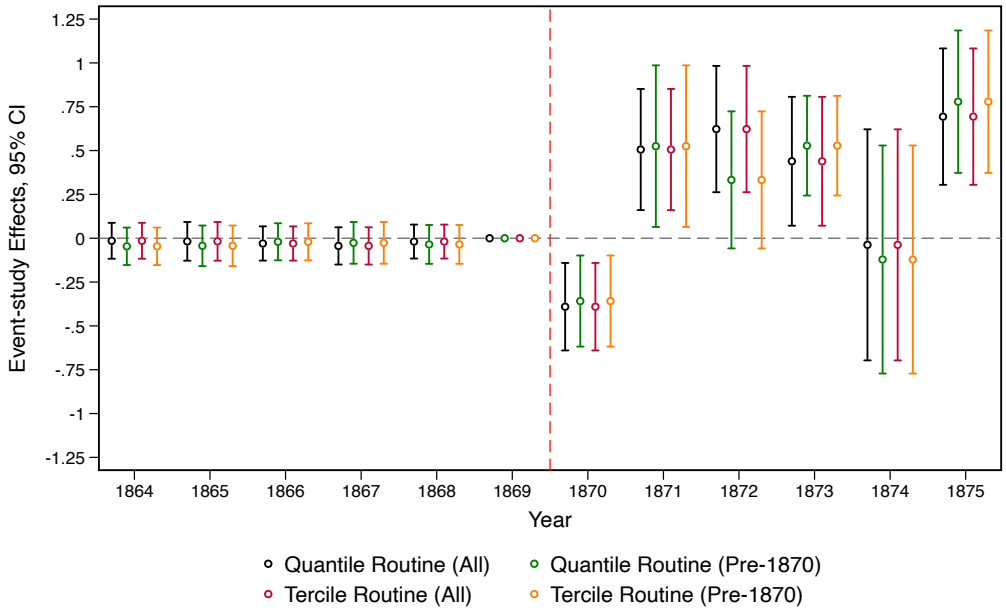


Figure A18: Non-Routine presence and Patronage usage (Dept Robustness)

Notes: The figure plots event-study effects for four different triple difference regressions that exploit variation in department labour functions to explain non-compliance with reform. They use a panel data set, so that the outcome is the share hired through patronage. All regressions are weighted by the number of entrants to a given department in that year. They interact a binary variable equal to one if a department is in the bottom quantile or tercile of hires classified as routine using job titles. This classification is done using either data from the whole period of pre-1870, with both results reported. All estimates are relative to event time -1 (1869). Bars indicate 95% confidence intervals calculated using clustered standard errors at the department level.

Within-department labour functions. I re-estimate the main results from this section of the mechanism, using a position-department panel ($N = 984$).[24] As with the individual-level results I find no evidence of pre-trends for routine and assistant positions, and slight pre-trends for clerk level positions. I confirm the same trend as previously, routine positions experience a large relative reduction in patronage usage while non-routine positions experience a large relative increase, among treated departments. However, one concern is that these units are not representative, some departments use different titles for the same job. To address this I group the titles into three categories, clerk, assistant or other. This gives a low (clerk), middle (other) and high (assistant) grouping of routine work per department. I then estimate the triple difference regressions for the clerk and assistant groups. These give the same trends as in the position-department panel, suggesting that differences in titles does not influence the results.

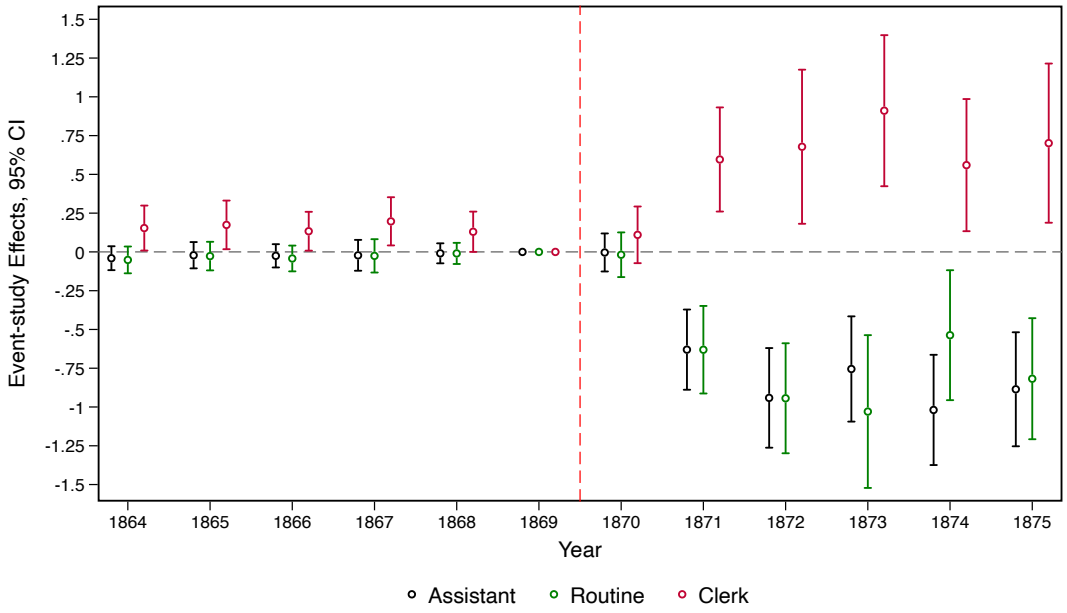


Figure A19: Patronage usage for different positions (Dept).

Notes: The figure plots event-study effects for three different triple difference regressions that exploit variation in function to explain non-compliance with reform. All regressions use position-department and year fixed effects. They use a panel data set, so that the outcome is the share hired through patronage. All regressions are weighted by the number of entrants to a given department in that year. The regressions use separate binary variables equal to 1 when a given entrants takes a job that fits one of three categories: Assistant, Routine or Clerk. They are estimated separately, and plotted alongside one another. All estimates are relative to event time -1 (1869). Bars indicate 95% confidence intervals calculated using clustered standard errors at the department level.

[24]There are 237 position-department units that exhibit variation over time. 277 are dropped because they appear only in one time period. The department analysis has 95 units that exhibit variation over time, with 40 dropped because they appear only in one time period.

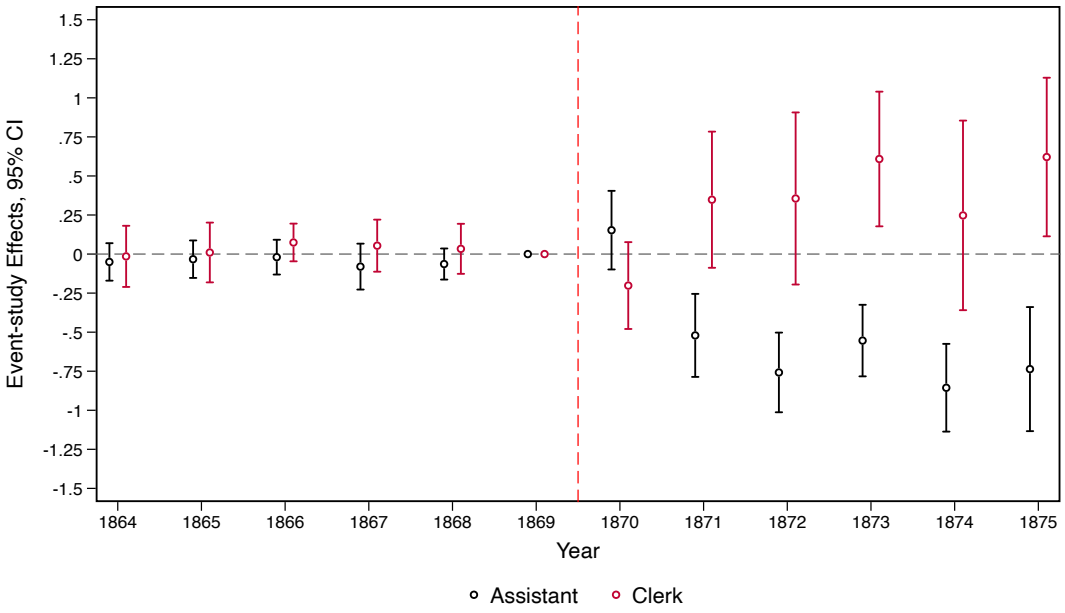


Figure A20: Patronage usage for different positions (Dept Position Group).

Notes: The figure plots event-study effects for two different triple difference regressions that exploit variation in function to explain non-compliance with reform. The dataset is collapsed into three categories (clerk, assistant, other) for each department-year. All regressions use position group-department and year fixed effects. They use a panel data set, so that the outcome is the share hired through patronage. All regressions are weighted by the number of entrants to a given department in that year. The regressions use separate binary variables equal to 1 when a given entrants takes a job that fits one of three categories: Assistant, Routine or Clerk. They are estimated separately, and plotted alongside one another. All estimates are relative to event time -1 (1869). Bars indicate 95% confidence intervals calculated using clustered standard errors at the department level.

Power. I re-estimate the main results from this section of the mechanism using a department panel. I look at Minister and Whitehall binary variables, first at the department level and second at the parent department level to ensure consistency. There are no major differences between these results and those at the individual level reported in text.

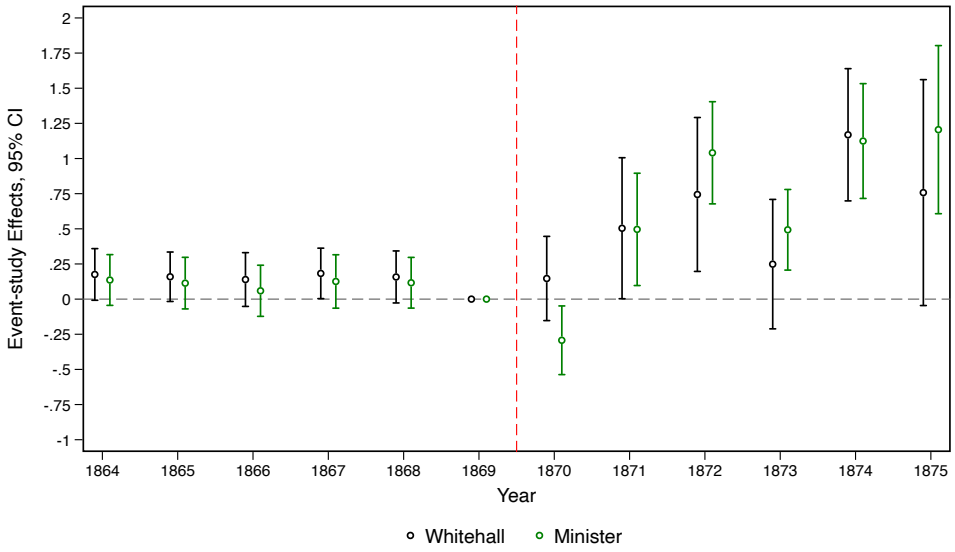


Figure A21: Power and Patronage usage (Dept)

Notes: The figure plots event-study effects for two different triple difference regressions that exploit variation in department power to explain non-compliance with reform. They use a panel data set, so that the outcome is the share hired through patronage. All regressions are weighted by the number of entrants to a given department in that year. They interact a binary variable equal to one if a department has a government minister or is located in Whitehall. All estimates are relative to event time -1 (1869). Bars indicate 95% confidence intervals calculated using clustered standard errors at the department level.

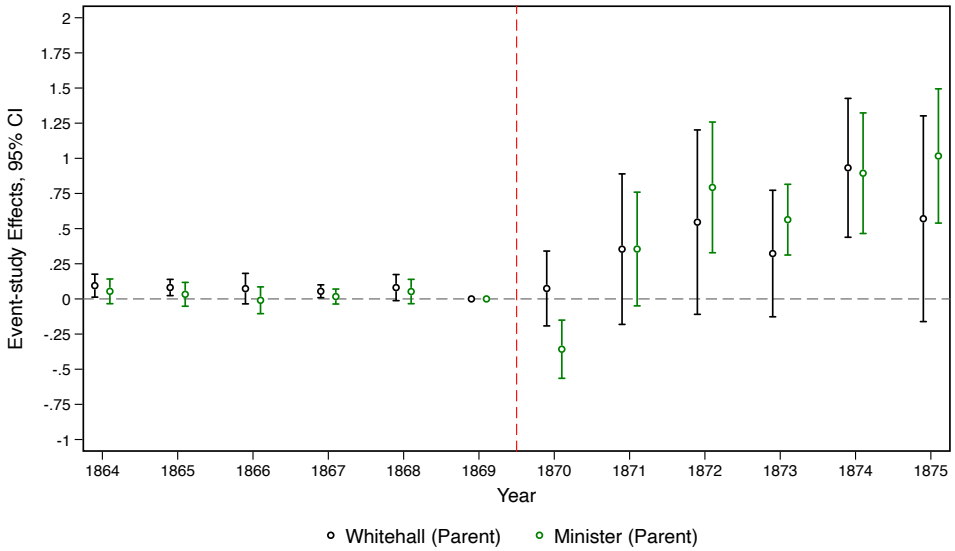


Figure A22: Power and Patronage usage (Parent Dept)

Notes: The figure plots event-study effects for two different triple difference regressions that exploit variation in (parent) department power to explain non-compliance with reform. To ensure that the results are not driven by misassignment of treatment across children departments, everything is aggregated to the parent level. They use a panel data set, so that the outcome is the share hired through patronage. All regressions are weighted by the number of entrants to a given (parent) department in that year. They interact a binary variable equal to one if a department has a government minister or is located in Whitehall. All estimates are relative to event time -1 (1869). Bars indicate 95% confidence intervals calculated using clustered standard errors at the department level.