Merit Recruitment in 19th and Early 20th Century European Bureaucracies

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Abstract

Methods of bureaucratic recruitment are widely viewed as having a critical effect on governance outcomes. Yet the literature – particularly the empirical literature – on bureaucratic recruitment has not substantially expanded beyond examinations of the US Federal Government and the Pendleton Act. In this paper, I examine the determinants of merit reforms more generally. I argue that governments trade off the gains from patronage – in terms of in-kind and monetary payments from seekers after posts – against the costs patronage imposes in terms of bureaucratic quality. These costs emerge because those most able to 'pay' for office under patronage may not be those best suited for administrative service. I test this claim, taking advantage of the rich temporal and cross-sectional variation in bureaucratic regimes in late 19th and early 20th century Europe. As is consistent with theoretical expectations, I find that governments are more likely to adopt merit reforms as education becomes more widespread. This is particularly true for regimes in which the politically advantaged class is small – i.e., in more autocratic regimes.

Practitioners and social scientists broadly agree that the method by which bureaucratic officials are recruited to their posts has important implications for governance outcomes

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such as corruption, regulatory quality, and economic growth. Social scientists since Weber have linked bureaucratic recruitment to governance; as he argues "assignments of services and usufructs in kind ... tends to loosen the bureaucratic mechanism"(Weber, 1978, 983). More recently, Rauch and Evans (2000) find that meritocratic recruitment is a strong determinant of bureaucratic performance and corruption in a cross-section of developing and middle income countries. Practitioners such as the World Bank have stressed the importance of bureaucratic recruitment and quality to economic development, particularly in the East Asian 'Tiger' economies (World Bank, 1993). The ICRG weights bureaucratic quality – with an emphasis on recruitment and training – in its calculations of factors that affect investor risk.[1]

In spite of the importance attributed to the meritocratic recruitment of bureaucrats, analyses of governments' decisions to adopt the merit system have predominantly focused on the United States (see, for instance Johnson and Libecap, 1994; Skowronek, 1982). Factors highly relevant to the US case – particularly the dramatic reshuffling of the federal bureaucracy following a change in the party of the president (Carpenter, 2001) – were not relevant in other countries. The unusual history of the American bureaucracy suggests that generalizing from the US experience may be problematic. Yet, comparative studies of the adoption of the merit system have been few and far between.

In this research project, I undertake a comparative study of the adoption of the merit system. I claim that patronage can be viewed as a transactional exchange in which governments offer bureaucratic posts in exchange for monetary and in-kind payments from would-be recruits. This exchange may impose costs on governments, in terms of bureaucratic performance, to the extent that those most able to pay for office are not those best suited for administrative posts.[2] To the extent that the set of candidates best-suited for office diverges from those most able to deliver monetary payments and political services to the government, patronage will be increasingly costly to maintain – and the adoption of the merit system grows more likely.

In this paper, I exploit the rich temporal and cross-sectional variation in bureaucratic recruitment in 19th and early 20th century Western Europe to test this claim. I argue that the set of would-be recruits most able to 'pay' for office will increasingly diverge from the set of potential recruits best suited for administrative tasks as levels of educational enroll-

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[1]See https://www.prsgroup.com/ICRG_Methodology.aspx.

[2]Note here that 'best suited' is from the government's perspective. Thus, a well-suited candidate may not be one that is best able to serve the public interest. In some instances, it may be the administrator best able to extract from and repress the populace at the government's behest.

ment rise. Increasing educational enrollments overwhelmingly reflect increasing levels of skills amongst the poor and middle classes, while those most likely to benefit from patronage – in this context – were predominantly members of the nobility. I further suggest that this effect is likely to be particularly pronounced in governments where levels of democratic participation are low. In such circumstances, the politically advantaged class is quite narrow, and it is unlikely that members of this class will be able to outperform the broader mass of the politically disenfranchised as education grows more widespread.

I find strong empirical support for these claims. In a sample of Western European countries with data running from roughly 1845-1944, I find that educational enrollment rates strongly predict meritocratic reforms in countries where democratic participation is low. As levels of democratic participation rise, this effect diminishes – such that no significant association exists between education rates and merit reforms in the most highly democratic countries.

1   Existing Literature

As noted above, the vast bulk of the literature on the adoption of meritocracy has focused on the US case, and particularly on the passage and extension of the Pendleton Act to cover an increasing portion of the national administration under civil service protections. A broad consensus in this literature stresses the importance of political competition, coupled with the high levels of turnover of bureaucratic officials under the spoils system, in promoting the adoption of merit recruitment and tenure protections. Levels of turnover prior to the implementation of civil service protections were truly massive – tens of thousands resigned from the postal service following transfers of power in the 1884 and 1888 elections (Carpenter, 2001). This level of turnover produced both administrative and political costs. The merit system was adopted to diminish these costs, and to constrain the ability of successor governments to employ patronage to their political benefit or to remove the sitting government's appointees (Skowronek, 1982).

Mueller (2009) attempts to place this argument in comparative perspective. He claims that parties that are faced with the potential loss of power adopt the merit system to ensure the continued provision of public goods to their supporters. In the absence of merit protections, parties adopt a spoils system, such that their appointees will devote the bulk of their effort to delivering private goods to their party and to its supporters, and will exclude those not part of the governing coalition. All partisans face the risk of being so-excluded

in the event that their party is removed from power. Rather than continue to face this risk, the incumbent government will adopt a merit system whenever the danger of loosing power becomes sufficiently high.

This argument, however, runs into substantial empirical and theoretical difficulties when placed in a comparative context. First, the empirical implications of this argument fail to hold in a number of non-US settings. For instance, in a study of patronage systems in Latin America, Geddes (1994) finds that merit recruitment is increasingly likely to be abandoned in favor of patronage as electoral competitiveness rises. In contrast to Mueller (2009), she argues that, as elections grow more competitive, the incentive for the incumbent government to rely on patronage to obtain an electoral advantage rises. Besley and Persson (2010) similarly argue that the threat of removal from office may lead governments to under-invest in developing state capacity, as their successors may use the power associated with a strong state to implement policies members of the incumbent government find detrimental. This argument suggests that, if merit recruitment leads to a more efficient state apparatus, incumbent governments would be loath to adopt merit reforms when the risk of removal is high. At least in some settings, it therefore seems that alternation in office increases concern with the short-term advantages provided by patronage, and decreases concern for the administrative benefits of moving towards meritocracy.

The second difficulty faced when generalizing from the US case is that the high levels of turnover associated with the spoils system are absent in other examples. In the early-19th century UK, for instance, appointments were allocated on the basis of patronage and party support. But, once officials were placed in their posts, subsequent decisions regarding promotion and removal were left to department heads rather than to politicians (Gash, 1953). Even in the late-17th century, when turnover in office was more common in the UK, the number of officials replaced only comprised 10 percent or so of the bureaucracy (Brewer, 1989). In other instances, merit reforms were introduced in polities that lacked competitive elections, and thus did not experience the turnover in offices associated with the spoils system. Prominent examples include 18th century Prussia, which introduced merit examinations under Frederick William I and Frederick II (Rosenberg, 1958); Napoleonic France, which required high raking officials to pass the baccalaureate (Church, 1981); Meiji era Japan (Silberman, 1993); and South Korea under the Park regime (Haggard, 1990). In Europe, the churning of bureaucratic posts that was typical in the 19th century US is only witnessed in a few instances, such as 19th century Spain (Diez, 2000), or following the ascension of fascist regimes in Germany (Caplan, 1988), Italy (Cole, 1938, 1953), and

Portugal (Opello, 1991). But even in the Spanish case, a number of positions in the elite *corps* were protected from distribution as spoils to supporters.

More explicitly comparative studies of the adoption of the merit system have generally relied heavily on theory and qualitative empirical analysis.[3] For instance, Egorov and Sonin (2004) examine agency problems in dictatorships and democracies. They contend that all governments are faced with a dilemma when selecting bureaucratic agents: Those most capable of conducting administrative tasks are also most capable of launching a coup against the government. Democracies are able to commit to a contract that renders coups incentive incompatible; autocracies are not. Therefore, they contend, democracies should be more likely to adopt a meritocratic civil service than autocracies. Geddes (1994) similarly constructs a theoretical model of merit appointments, which is then tested using qualitative evidence from Latin American countries. She contends that one can think of the choice between merit and patronage as akin to a prisoners' dilemma game played between political parties. Each party can reap electoral gains by resorting to (or promising to resort to) patronage, at the expense of all other parties. As elections grow more competitive, the rewards to defection rise, and patronage is consequently more likely to be adopted. Evans (1995) and Haggard (1990) conduct qualitative analyses of bureaucratic appointment and functions in a variety of Latin American and East Asian countries. In contrast to Egorov and Sonin (2004), these studies stress the importance of state *autonomy* from social actors – which may be enhanced under autocracy – to the adoption of merit reforms. Silberman (1993) contends that meritocratic methods of recruitment are most likely to be adopted in periods of political uncertainty, particularly when the regime seeks a means to legitimize its rule.

Other explicitly comparative accounts stress the importance of structural factors, rather than political institutions, in determining the form of bureaucratic recruitment. Famously, Weber (1978) contends that bureaucratic rationalization is a byproduct of economic development. Industrial capitalism, he contends, requires that the private sector can be certain of the impartial conduct of government functions. Such certainty is lacking in bureaucratic systems that do not conform to a Weberian 'ideal type' (also see, Gerth and Mills, 1970).

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[3]An exception to this general trend is (Lapuente and Nistotskaya, 2009), who argue that the choice between meritocracy and patronage is critically influenced by the discount rate of the incumbent government. Investors, they contend, are more likely to invest when a government opts for a meritocratic system of recruitment. Governments trade off this long term benefit against the short term gains offered by patronage. Those with short time horizons will, therefor, forsake the long-term benefits of meritocracy in favor of the short-term benefits from patronage. In a cross-section of Russian provinces, they find a strong correlation between the longevity of governors and the adoption of merit-based recruitment.

Others stress the importance of the threat of war in driving state development and, by extension, to the creation of a meritocratic bureaucracy. Governments faced with an external threat must develop an efficient state apparatus that places skilled officials in bureaucratic posts (Besley and Persson, 2010; Brewer, 1989; Ertman, 1997; Tilly, 1992).

This paper diverges from the methodology of the bulk of the comparative literature, in that it presents a quantitative empirical analysis of the adoption of merit reforms. To conduct this analysis, I build a cross-national database of merit reforms that codes the experience of Western European countries between 1800 and 1944.[4] This period and panel of countries exhibits substantial cross-sectional and longitudinal variation in the extent of merit reforms. Several countries adopt examination and educational requirements for officials as early as the 18th and early 19th centuries, in the case of Prussia and Sweden respectively (Herlitz, 1930; Hesslén, 1950; Rosenberg, 1958). Belgium, by contrast, does not adopt a comprehensive system of merit examinations until 1938 (Thijs and Van de Walle, 2005). Other countries exhibit substantial variation in merit reforms over time: Early Prussian meritocratic reforms are weakened in the mid- and late-19th Century (Finer, 1932; Gillis, 1971) and all but abandoned under the German Nazi regime (Caplan, 1988). France oscillates in the adoption of meritocratic reforms over the 19th century, often coinciding with changes from Empire to Republic (Church, 1981; Osborne, 1983). This rich cross-sectional and temporal variation makes this set of cases well-suited to empirical analysis.

I stress the importance of the spread of education to the adoption of merit reforms – particularly in autocratic regimes.[5] I suggest that, as the skill levels of the politically marginalized rise, the costs the patronage system imposes upon the government (in terms of foregone administrative capabilities) similarly increase. This is particularly true when the politically enfranchised class is relatively small. The spread of education in 19th and early 20th century Europe increased the skills of the lower and middle classes, precisely those most likely to be excluded from the patronage mechanism. And thus one would expect the spread of education to be associated with meritocratic reform.

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[4]Data coverage for most independent variables does not begin until 1840 or so.

[5]I am far from alone in assigning particular significance to the relationship between the bureaucracy and education. See Mueller (1984) for a discussion of this relationship in the UK and Prussia. Similarly, Reader (1966) notes the importance of the rise of the professional classes to the development of a meritocratic civil service in Britain.

2 Theoretical Expectations

Patronage is a method of recruitment in which seekers after bureaucratic office offer some form of payment – typically including an *ex ante* payment – to the incumbent government in return for posts. In many instances in medieval and early modern Europe, these payments were explicit and monetary – individuals literally purchased an office over which they exerted a property right (Ertman, 1997; Fischer and Lundgreen, 1975). Examples include tax farming, which was a common practice throughout Europe and continued in France until the Revolution; and the sale of military commissions, which was common throughout Europe and practiced in the Britain until the Crimean War (Reader, 1966). Indeed, Finer (1932, 1242) goes so far as to claim that

> In France until the Revolution, almost every office, central and local, excepting the dozen or so highest offices in the kingdom, were attainable only by private purchase, gift, or inheritance.

Payments under patronage may also be – and, in modern times, more commonly are – in-kind. In these circumstances, those seeking office typically provide political services to their leaders in exchange for posts. For instance, Lewis (2008, 69) notes that American presidents often reward former campaign workers with positions in the bureaucracy, and that often "it was the promise of such a job that perhaps motivated them to work for the campaign in the first place." Similarly, parties in the 19th century US would reward those active in the party with bureaucratic posts, and these appointees could only maintain their posts if they provided continued donations of a fraction of their salary to their party (Carpenter, 2001). In early-19th century Britain, "[politicians] regard[ed] direct and tangible political service as the only workable criterion of reward [through patronage]" (Gash, 1953, 368). These tendencies extend to authoritarian regimes. Soon after assuming power, the Nazi party reserved portions of the bureaucracy for former members of the NSDAP, SS, and SA (Caplan, 1988).

Patronage is thus a transactional system of recruitment in which posts are exchanged for goods and services. This need not be detrimental to the quality of the administration. If one's tenure and future advancement through the civil service is linked to one's abilities, and if the returns from bureaucratic service are sufficiently lucrative, those most willing to pay may also be those best able to serve. (For a model of costly signaling and career concerns, see Mattozzi and Merlo, 2008). And, in most bureaucratic systems, one's abilities are at least likely to be weakly linked to the potential for advancement and reten-

tion. Recall that, in the UK, personnel decisions following appointment were largely left in the hands of senior bureaucrats. Fischer and Lundgreen (1975, 465) note of patronage appointees, "the abler ones had the better chance to rise."

But, it should be noted that the ability to secure patronage posts is always conditional on the ability to pay. So even if circumstances arise such that patronage payments act as a signal of merit, they only signal the merit of patronage appointees relative to the pool of potential recruits able to provide the funds or services desired by the government. A subset of potential recruits will always be excluded under the patronage mechanism due to 'credit constraints' – i.e., they lack the requisite financial means or political connections to attain office. Typically (but not always), those who are most likely to be excluded are members of the lower classes. These individuals are least able to afford the financial costs of buying office, and are unlikely to be able to afford the opportunity costs of providing political services, assuming that they are even in the position to provide such services. In 19th and early 20th century Europe, members of the lower classes were particularly likely to be excluded by patronage, as common restrictions on suffrage according to birth or wealth prevented such individuals from providing the most basic of political services to the government (Przeworski, 2009).

Patronage systems in this period were therefore overwhelmingly likely to favor the aristocracy and upper classes, a fact attested to by both historians and contemporary observers. Charles Trevelyan, co-author of the Northcote-Trevelyan Report, which led to the introduction of merit examinations for the British civil service, wrote

> There can be no doubt that our high Aristocracy have been accustomed to employ the civil establishments as a means of providing for the Waifs and Strays of their families...(as cited in Mueller, 1984, 99).

Patronage provisions also heavily favored the nobility in Prussia during the 18th century (Gillis, 1971; Muncy, 1944; Rosenberg, 1958). In *ancien regime* France, higher civil service posts often explicitly required noble birth, and a ban on such requirements was one of the early byproducts of the Revolution (Church, 1981; Silberman, 1993).

To the extent that those best qualified to serve in the civil service are excluded by patronage provisions, patronage is costly to the government. All governments desire a competent administration, though the weight they place on this concern may vary. This desire may stem from concerns about retention. Governments, particularly democratic governments, need to provide sufficient public goods to members of the populace to avoid removal from power (Bueno de Mesquita et al., 2003; Przeworski, Stokes and Manin,

1999). But it may also be because a predatory government leadership desires to extract as much from the populace as possible (McGuire and Olson, 1996). It stands to reason that a competent administrative service is likely to be better able to engage in extraction from the population than a less competent bureaucracy. In either event, the exclusion of qualified recruits from bureaucratic office is likely to prove costly.

Merit-based recruitment, on the other hand, makes posts available to those applicants that score most highly on some (admittedly imperfect) indicator of administrative competence. To the extent that the proxies used for merit selection are valid signals of applicants' quality, and to the extent that these systems are open to all meritorious applicants, merit-based systems are less likely to exclude qualified potential bureaucrats than are patronage systems. In adopting a merit system, the government forgoes the rents it derives from patronage in exchange for opening the recruitment process to a greater pool of qualified applicants. The role of a merit system in opening the appointment process was well-recognized in this period. To again quote Charles Trevelyan

> All our public schools and universities are seminaries of training and discipline for the service of the state ... Why, therefore, do we maintain a barrier of patronage between our public schools and universities and the public sector? (as cited in Mueller, 1984, 205).

And the effect of adopting meritocratic systems of recruitment was to open recruitment to previously unrepresented classes, particularly to members of the middle class. Gillis (1971, 29) notes that in Prussia, following the widespread adoption of merit examinations and reorganizations in the early 19th century, "the bureaucracy provided a carefully regulated opportunity for upward mobility through entrance into its ranks." Reader (1966, 95) finds that, after the Indian Civil Service adopted merit examinations in 1853, "'some 30 per cent of the candidates were sons of men who, by the standards of the day, came from the lower middle class or even further down the social scale."

Patronage systems are thus costly to the government insofar as they exclude qualified bureaucratic recruits who are unable to 'pay' for office, and grow increasingly costly as the skills of this excluded group rise. Merit systems are less likely to exclude qualified individuals from administrative office; though, in adopting the merit system, a government must forgo the rents from patronage. It therefore holds that, *ceteris paribus*, governments are more likely to adopt a merit system as the skills of the politically marginalized rise.

The breadth of educational enrollments serve as an excellent proxy for the skills of the politically marginalized at this time. As with political rights, access to education was

highly stratified by class in much of Europe throughout much of the 19th century.[6] In England, for instance, attendance at public schools and universities was common for much of the noble class, but access to education among the middle and lower classes was highly curtailed. The educational opportunities available to members of the lower classes were often confined to vocational training (Musgrave, 1968). Expansions in educational enrollment therefore overwhelmingly reflected higher levels of educational attainment among the middle and lower classes, precisely those most likely to be excluded by the patronage mechanism. As bureaucratic service required literacy and (in many instances) some degree of mathematical proficiency, higher levels of education implied that the administrative capabilities of the lower and middle classes were rising relative to those of the politically advantaged elite. It therefore follows that, as education enrollment rose, the adoption of merit reforms became more likely.

This effect is likely to be particularly pronounced in polities where levels of democratic enfranchisement (and thus participation) were relatively low. Under a relatively closed political system, wherein access to the patronage mechanism is highly restricted, patronage is likely to select the most skilled possible bureaucrats only if the distribution of skills is very highly skewed in favor of the enfranchised. Any slight reduction in this skew would all but ensure that the most skilled would-be bureaucrats belong to the marginalized classes. By contrast, under a very broad-based political system wherein most citizens had access to patronage the patronage mechanism, the distribution of skills would have to be highly skewed in favor of the marginalized (a situation that is unlikely to hold in practice) for the costs of patronage to be large. It therefore follows that the spread of education should have a greater effect in more closed political systems, in which access to the patronage mechanism is likely to be curtailed.

This result may also follow from issues of measurement. In political systems where democratic participation was highly restricted, it is most likely that patronage was confined to the aristocratic classes over much of this period. However, in more democratic systems, patronage may also have benefited members of the middle class. The expansion of education was most likely to affect the skills of the middle and lower classes. Therefore, it was more likely to affect the of the skills of the disenfranchised (relative to the enfran-

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[6]A notable exception to this generalization is the case of Prussia, where "To obtain an academic degree was such a rare occurrence that it almost implied conduct unbecoming a nobleman," (Rosenberg, 1958, 58) throughout much of the 18th century. Norms in Prussia began to change in the early 19th century, and rates of enrollment by sons of the nobility at universities increased sharply by the time this empirical analysis begins (Gillis, 1971).

chised) when suffrage was highly restricted, and is likely to be a less valid proxy of these skills as levels of democratization rise.

In what follows, I test these two claims: (1) That rising levels of education were associated with a higher probability of merit reform, and (2) That this effect was particularly pronounced in systems where democratic participation was limited. In the next section, I operationalize these concepts and describe the data sources I employ. I then describe my empirical model and present my results. My findings support the theoretical contentions advanced by this paper – merit reforms are more likely as education levels rise, and this effect is extremely pronounced in polities where democratic participation is curtailed.

3 Data Definitions

3.1 Outcome Variable

To test these predictions, I make use of a dataset that codes the adoption of merit reforms in European, particularly Western European, countries in the period from 1800-1944.[7] I code these data based on a variety of secondary sources, a list of which can be found in Appendix A. Merit reforms are coded according to a dichotomous indicator, $merit_{i,t} \in \{0, 1\}$, that takes the value of 1 in the year a merit reform is adopted and a value of 0 otherwise.[8] Reforms are considered to promote meritocratic recruitment if they (1) introduce educational or experiential requirements for bureaucratic posts, (2) create a system of merit examinations - or strengthen a system of existing examinations, or (3) require the open advertising and competitive recruitment of officials.

This coding focuses on *changes* in the degree to which a government relies on the meritocratic recruitment of officials, rather than on the *level* of meritocracy. While a focus on

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[7]The countries included in this dataset are Germany, France, the UK, Belgium, Romania, the Netherlands, Sweden, Italy, Spain, Portugal, Greece, and Denmark. I am expanding the dataset to cover Austria and Hungary. Prussia is coded as equivalent to Germany in the dataset. When Austria and Hungary are added, Austria-Hungary will be treated as equivalent to Austria, until the Compromise of 1867 allowed for separate administration the two halves of the country (Janos, 1971), at which point Hungary will be treated as a separate country.

[8]I have also experimented with a trichotomous indicator that is identical to the above save in that it takes the value -1 in the year a merit reform is repealed. In practice, there are few instances of reversals of merit reforms in the dataset, and these are confined to a small number of countries – particularly Prussia/Germany and Italy. As I only estimate models with country specific fixed-effects, these reversals contribute little to the likelihood, and the use of a trichotomous indicator necessitates the estimation of an ordered model with country-specific indicator variables, which is inconsistent in small samples. I therefore prefer the use of the dichotomous indicator.

changes rather than on levels necessarily entails some loss of information; it also improves the comparability of reforms across countries and over time. Any index that attempts to capture cross-national comparisons of the degree to which merit is considered in bureaucratic recruitment during this time period would have to rely on highly subjective judgments. For instance, the British civil service relied on a system of competitive examinations for appointment from 1870 (Chapman, 1959); the Swedish civil service combined a system of educational requirements with the open advertisement of and competition for posts (Herlitz, 1930); while the Prussians established a system of several (non-competitive) examinations and probationary periods for recruits for the higher civil service (for a detailed description, see Finer, 1932). Any coding decision as to which of these systems is the most meritocratic would be highly subjective. By contrast, little subjective judgment is required to note that the creation of the British examination system by an Order in Council in 1870 marked a meritocratic reform, nor does the claim that Nazi Germnay's passage, in 1933, of the Law for the Restoration of a Professional Civil Service – which permitted the dismissal of officials for political offenses and promoted the hiring of Nazi party members – marked a movement away from merit recruitment (Caplan, 1988).

Focusing on changes in the degree of merit recruitment, however, creates its own difficulties. Most notably, the changes in merit recruitment are unlikely to be wholly independent of the level of meritocracy. If extensive merit requirements have already been imposed, it is relatively unlikely that any further pro-merit reforms will be adopted. To help adjust for this problem, I code an indicator variable $ever\_merit$ which equals one if a country had ever implemented a merit reform in the past. Given the coverage of the reform data I use, which date back to 1800, this variable likely takes the value 1 after the earliest date in which a country ever introduce merit requirements.[9]

3.2   Explanatory Variables

The theoretical intuition developed above (which summarizes Hollyer, 2010) holds that patronage grows increasingly costly as the set of recruits selected under patronage diverges from the set of individuals qualified for posts. Governments trade off the gains to patronage – in terms of monetary and in kind payments from those seeking appointments – against the costs patronage imposes on the quality of bureaucratic officials. These costs

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[9]An exception is the case of Prussia (Germany), which first created a system of merit examinations as early 1713 (Mueller, 1984). The $ever\_merit$ indicator thus takes the value 1 for all observations of Germany in the dataset.

grow increasingly large as the skills of those able to attain bureaucratic positions under patronage decline relative to those excluded by the patronage system. To assess the validity of this theory, it is necessary to develop a measure of the skills of the disadvantaged class of potential recruits.

Here, I proxy for such skills using measures of the breadth of education in a given country-year. If those advantaged under patronage – who, during this period in Europe, largely consisted of the noble class and upper middle classes – obtained relatively high levels of education throughout the period, variation in the levels of education should be driven by its expansion to reach the middle and lower classes. As levels of educational enrollment rise, so too do the skill-levels of those disadvantaged by patronage. As a result, I predict that the costs to the government of maintaining a patronage system increase with the spread of education, and merit reforms are more likely to be adopted.

I rely on three sources for measures of educational enrollment. The first is derived from Flora (1987), and consists of the number of students enrolled at primary schools from 1830 (at the earliest) through 1975. These data are measured in varying increments and cover Austria, Belgium, Denmark, France, Germany, Ireland, Italy, the Netherlands, Norway, Sweden, Switzerland, England and Wales, and Scotland.[10] While Flora also provides data on secondary and tertiary enrollment levels, these data have narrower coverage across time and countries during the period under examination. I therefore choose to focus on primary enrollment levels.

The second set of education data are drawn from Mitchell (1975). Mitchell provides enrollment numbers in both primary and secondary education for a set of countries drawn from around the world over the period from (at earliest) 1830 to 1970.[11] I make use of the sum total of both primary and secondary enrollment in this analysis, reducing any concerns about the comparability of the classification of primary and secondary schools across countries.

The Mitchell data are more likely to be comparable across countries than the figures assembled by Flora, and offer a slightly broader coverage of states. However, the Mitchell dataset has few observations for Germany/Prussia, one of the countries for which the remaining data are most complete. Moreover, the advantage conferred by the broader coverage of the Mitchell data is weakened by my reliance on Flora's data for other controls. Thus, my preferred estimates are those that make use of the Flora data, but I report

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[10]I have added together values from England and Wales and Scotland to derive the UK figures.

[11]I draw on the data for Austria, Belgium, Denmark, France, Germany, Greece, Hungary, Italy, the Netherlands, Portugal, Romania, Spain, Sweden, and the UK in this analysis.

coefficient estimates using both measures throughout.

I additionally draw on data provided by Lindert (2004) that measure the number of primary school students per 1000 children aged 5-14 in a wide variety of countries over the 1830-1930 period. The Lindert data cover a wider range of countries than do the Flora figures. But, observations are only available in decade intervals and coverage ends 15 years before the rest of my dataset. These data are therefore less preferred than the Flora and Mitchell figures, but provide a useful robustness check on the estimates below.

To ease the merging of data that are measured at varying time intervals, I interpolate enrollment data assuming a constant (exponential) rate of growth between observations for the Flora and Mitchell data, and a linear rate of growth for the Lindert data. I do not interpolate values if there is a more than 10 year gap between observations. After merging, all data are collapsed into five year intervals. The Flora and Mitchell enrollment rates are also adjusted to reflect the size of the school aged population, using data on the size of the population aged 0-14 drawn from Rothenbacher (2002). Enrollment data are thus expressed as the fraction of those aged 0-14 enrolled in school.[12]

In addition to school enrollment rates, I include controls for the degree of participation in elections in all specifications. Many have postulated that democracies are more likely to adopt merit-based bureaucracies than autocracies (Egorov and Sonin, 2004; Mueller, 2009). Those living under democratic systems may be better able to discipline the government for poor performance, increasing the government's incentive to maintain a skilled bureaucracy. (However, for a contrasting view see Evans, 1995; Geddes, 1994; Haggard, 1990). And since democracy is likely to be correlated with other regressors of interest, it is crucial to control for a measure of democratic participation.

Moreover, democratic enfranchisement alters both the size and composition of the pool of office-seekers capable of offering political benefits to the government in exchange for posts under patronage. As suffrage is extended, the pool of job-seekers able to benefit from patronage is likely to expand and to increasingly consist of the middle and lower classes. As the set of politically advantaged seekers after bureaucratic office grows large, it is increasingly likely that some members of this group will be very highly skilled. It is therefore less likely that the government will need to resort to meritocracy to ensure a skilled bureaucracy, even as the levels of education amongst the politically disadvantaged expand. Also, when the set of politically advantaged potential recruits includes members of the middle classes, it is unlikely that improvements in education levels only affect the

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[12]I rescale the Lindert measures so that they are similarly expressed in percentage terms.

politically disenfranchised, but will increase the skill levels of a subset of potential political clients. Rising education levels are therefore likely to have a diminished effect on the government's incentive to adopt merit-reforms in highly democratic political systems. I therefore include an interaction between levels of democratic participation and education levels in all specifications.

As a measure of democratic participation, I rely on election participation rates, as a percentage of the population aged 20 and above, from Flora (1987). These data primarily reflect the extent of suffrage (Przeworski, 2009), but also capture the degree of political involvement amongst the electorate. They therefore serve as a good proxy of the theoretical concept of interest: the size of the politically connected class or winning coalition (Bueno de Mesquita et al., 2003). However values are, for obvious reasons, only available in election years. This is particularly problematic in this sample, as a gap in elections may indicate either that an elected government is serving out its term in office, or that the democratic government has been replaced by an autocracy. I therefore recode the data in the following manner: I assume that participation rates evolve in a linear fashion between elections and interpolate the data for these periods. If there is a gap between elections of 10 years or more, I compare these data with the Polity IV measure of competitiveness of executive recruitment Marshall and Jaggers (N.d.). If the competitiveness of executive recruitment is coded as a value of 1 or below – in which case "Chief executives are determined by hereditary succession, designation, or by a combination of both"[13] – I replace the value of electoral participation as a 0.[14] I replace the value of participation as missing in all years of occupation.

In subsequent robustness checks, I employ alternative measures of the degree of democratic participation. One such measure is the familiar `polity2` measure, derived from the Polity IV dataset (Marshall and Jaggers, N.d.). Another such measure is an indicator variable measuring the extent of suffrage restrictions. This indicator is coded based on the dataset employed by Przeworski (2008). The indicator takes the value 1 if all 'independent' males are granted suffrage, if there is universal male suffrage, or if either of the two previous conditions hold and the vote is also extended to some fraction of the female population.[15] The indicator takes the value 0 otherwise. To control for autocratic interludes

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[13]Polity 4 Project: Dataset Users' Manual, p. 21-22.

[14]The one exception to this rule is Austria in the 1930s. Austria held an election in 1931 prior to *anschluss*, and held no subsequent election until 1946. Here I extrapolate the participation data until 1937, and code all values after *anschluss* as missing.

[15]This corresponds to any observation with the prefix 6 or 7 in the Przeworski data.

during which no elections take place, I employ the same recoding procedure as described above for the democratic participation measure. In any period where there is a gap of more than 10 years in elections and in which the competitiveness of executive recruitment is coded as a 1 or below, this indicator takes the value of 0.

As control variables, I include measures of economic development and participation in international wars. A broad literature, dating to Weber (1978) (1922), has stressed the importance of economic development to meritocratic reforms of the bureaucracy. This may be because the capitalist system requires certainty in the performance of government functions (Gerth and Mills, 1970; Weber, 1978), because Wagner's law implies that citizens demand more and better services from the government as incomes rise, or because financially constrained governments are more desperate for the proceeds of patronage (for a similar argument, see Besley and McLaren, 1993). Regardless, economic development is a critical potential confound.

As a measure of economic development, I rely on a measure of GDP *per capita*, measured in constant 1990 Geary-Khamis dollars, drawn from Maddison (2007). These data cover a broad variety of countries from 1820-present. Values from 1820-1850 are available at decade intervals for most countries (yearly for a few), and yearly values are available subsequently. I interpolate missing values assuming a constant exponential growth rate, never interpolating if the gap in data coverage exceeds 10 years.

Finally, I include a control for participation in an interstate war, as coded by the Correlates of War project (Sarkees and Wayman, N.d.). This control is necessary as war may be associated with increased demands for public (military) goods, which may outweigh moral hazard problems a government faces when investing in bureaucratic capacities that can be abused by successors (Besley and Persson, 2010). Alternatively, states with stronger bureaucratic apparatuses may be more prone to warfare, and the demand for meritocratic reforms may stem from military concerns (Brewer, 1989). For instance, some attribute the impetus for the Northcote-Trevelyan Report to Britain's poor performance in the Crimean War (Hart, 1972).

As noted above, after all observations are merged, I collapse the dataset into 5 year intervals. All variables, with the exception of the indicators for war, a wide suffrage, and meritocratic change, are taken at their means. The indicator for war is coded as a 1 if a country participated in a war during that 5 year interval. The merit indicators are similar coded as equal to 1 if a merit reform is passed in that interval. And the suffage indicator is coded as equal to 1 if, at any point in the 5 year interval, the right to vote was granted to

16

all independent or to all males. So-collapsing the data lessens any reliance on interpolated values, helps to adjust for measurement error which may miscode the precise year in which a reform took place, and reflects what is likely to be the slow adjustment of bureaucratic recruitment to changing social conditions. Summary statistics for all variables described above can be found in Table 1.

Table 1: Summary Statistics

Variable Name	Mean	Standard Dev.	Min.	Max.
Merit	0.13	0.34	0	1
Dem. Participation	34.4	29.4	0	98.2
Polity 2	-0.12	6.22	-10	10
Wide Suffrage	0.53	0.50	0	1
Flora Ed. *per capita*	42.9	8.68	19.7	59.6
Mitchell Ed. *per capita*	42.6	10.6	15.0	66.3
Lindert Ed. *per capita*	56.0	18.2	2.8	90.1
GDP *per capita*	2507	1247	641	7425
War	0.17	0.38	0	1

4 Empirical Model

To test the association between the expansion of education and merit reforms, I estimate the fixed-effects (conditional) logit regression (Chamberlain, 1980)

$$merit_{i,t} = Logit(\alpha_i + \gamma education_{i,t-1} + \delta participation_{i,t-1} + \lambda education_{i,t-1} * participation_{i,t-1} +$$
$$\mathbf{X_{i,t-1}}\beta + \mathbf{T}\zeta) \tag{1}$$

where i denotes country i, t denotes half-decade t, $\mathbf{T}$ represents a cubic-polynomial of time, $\mathbf{X}$ includes GDP *per capita*, an indicator for war, and an indicator for whether any previous merit reforms were adopted, and α_i denotes a country specific intercept term. A cubic polynomial of time is included as to control for time dependence using a very general functional form. The use of a time polynomial, rather than time dummies, helps to ensure that problems of quasi-complete separation of the data and incidental parameters are not induced (Beck, Katz and Tucker, 1998; Carter and Signorino, 2010; Wooldridge, 2002).

The use of country-specific fixed-effects in this model introduces potential concerns of estimation. Fixed-effects are necessary to adjust for potential differences in measurement

across countries. For instance, (Flora, 1987) notes that the definition of primary education varies across countries. Flora's education measures for England and Scotland are available only for public institutions; whereas, values are provided for both public and private institutions in other states. Similar issues arise in both the Lindert and Mitchell data. The estimation of country-specific fixed-effects ensures that these differences in measurement only affect regression results if they alter the change in covariate values relative to their country mean – a less restrictive assumption than simply comparing covariate values across countries.

But, estimates of the coefficient values for country-specific fixed-effects are only consistent in time. As the time period over which we observe countries grows increasingly small, our estimates of the fixed-effect coefficients are increasingly poor. Because the logit model is not additively separable in the parameters, the inconsistency of the intercept coefficients will cause the estimates of *all other* coefficient values to be inconsistent (Greene, 2002), causing what is known as a incidental parameters problem. The number of time observations per country varies from 5 to 19 in the sample, suggesting that incidental parameters might be a concern.[16]

The fixed-effects logit effectively allows the intercept parameter for each country to vary without separately estimating coefficients on country indicator variables included in the regression equation. It achieves such estimates by conditioning on the total number of merit reforms implemented in each country in the sample.[17] This model is consistent even when estimated for time-series-cross-sectional data where each panel is short (i.e., $T \leq 20$) (Chamberlain, 1980; Greene, 2002).

5   Results

Results for the estimation of equation 1 are presented in Table 2. Estimates that make use of the Flora measure of education enrollment rates are presented in the first two columns, estimates that make use of the Mitchell figures are presented in next two columns, and estimates that make use of the Lindert measure are reported in the final two columns.

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[16]Note that the fixed-effects model also implies that all countries that never implement a merit reform are dropped from the likelihood function.

[17]Formally, the fixed-effects logit estimates $\frac{e^{\sum_{t=1}^{T} \mathbf{z_{i,t}}\phi}}{\sum_{\sum_t d_{i,t}=S_i} e^{\sum_{t=1}^{T} \mathbf{z_{i,t}}\phi}}$, where the denominator represents the sum of the probabilities of witnessing each possible combination of values of $merit_{i,t}$ that yield the value of $\sum_{t=1}^{T} merit_{i,t} = S_i$ for country i.

All regressors are lagged one (five year) period to address potential concerns of reverse causality.

In all estimates employing the Flora data, education rates and democratic participation are predicted to increase the probability of merit reforms. The coefficient values on these terms are significant at the 99 and 95 percent levels, respectively. Moreover, the effect of educational enrollment on the change in the level of meritocracy is stronger in political systems with lower levels of democratic participation. Education rates are predicted to have no positive – indeed a slightly negative – impact on merit reforms if participation rates exceed 53 percent. These results are strongly consistent with theoretical expectations.

Estimates using the Mitchell and Lindert data have the same predicted signs, but are not significant at conventional levels. Coefficient values from the Mitchell regressions border on significance – educational enrollment rates are predicted to increase the probability of merit reforms ($p = 0.15$, $p = 0.16$ in the respective models), and this effect diminishes as levels of democratic participation rise.[18] Results using the Lindert data predict similar effects for the main covariates of interest; though these results are less precisely estimated ($p = 0.23$ and $p = 0.14$ on the education enrollment term in the two respective models).[19]

Some caution, however, is warranted in interpreting interaction terms in non-linear models. The functional form of the link function implies that the marginal association between the regressand any given regressor is conditional on the values of all other regressors – thus, the non-linear functional form imposes a form of interactive effect on all covarites. The interaction between covariate values imposed by the functional form may cut with or against interactive effects specified by the researcher – such that a significant coefficient on an interaction term may not be a valid indicator of the conditional effect of the covariate of interest (Ai and Norton, 2003; Berry, DeMeritt and Esarey, 2010; Greene, 2010; Nagler, 1991).

One means of analyzing interactive effect in non-linear models is through the graphical presentation of simulated predictions at covariate values of interest. I present such graphics in Figures 1. These figure presents a plot of the predicted probability of a merit reform, conditional on the level of educational enrollment (as measured by Flora). Enrollment rates are plotted on the x-axis and vary from their minimum to maximum observed levels. The graph to the left depicts predictions at the 5th percentile of the participation (i.e., a value of 0 percent of those aged 20 and above, also the lowest possible value), and the

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[18]P-values throughout are from two-tailed tests.

[19]Additional estimates replacing the fixed-effects logit model with a linear probability model return similar results.

graph to the right depicts predictions when at the 90th percentile of participation (i.e., 87 percent of those aged 20 and above.)

The complicated functional form of the fixed-effects logit regression makes the derivation of such predictions cumbersome (King, 2001). Fortunately, however, estimates from a logit model including country-dummies are quite similar to those produced by the fixed-effects logit model, suggesting that the incidental parameters problem has minimal effect in this instance.[20] I therefore simulate predicted probabilities using this simpler model, as run through CLARIFY (Tomz, Wittenberg and King, 2001).

The conditional nature of the relationship between education enrollment and merit reforms is evident in this figure. Education enrollment rates strongly predict merit reforms in relatively autocratic states. The predicted probability of a meritocratic reform increases dramatically as enrollment rates pass 40 percent, and go nearly to 1 as enrollment rates reach 50 percent. In practice, enrollment rates rarely reached these values under highly autocratic systems. However, in countries scoring below the 10th percentile in democratic participation, enrollment rates varied between 19.7 and 47.3 percent according to the Flora data. Those autocratic countries with relatively high enrollment rates were substantially more likely to implement merit reforms than those with low levels of enrollment.

By contrast, the relationship between the expansion of education and merit reforms in democracies is estimated to be slightly negative. These estimates are made with an enormous amount of error, such that the 95 percent confidence interval around the predicted probability of reform often encompasses the full unit interval. The model does an extremely poor job of forecasting reforms in highly democratic countries. This may be a result of the fact that most countries in the sample undertake merit reforms relatively early in the period, when still highly autocratic. Indeed, Belgium is the lone outlier in this regard – having adopted the merit system following the report of the Camus Commission in 1938 (Thijs and Van de Walle, 2005). Most other reforms of this late period were repeals of previous merit-based recruitment methods in autocratic (specifically fascist) regimes (Caplan, 1988; Cole, 1938, 1953; Finer, 1964; Opello, 1991). Nonetheless, it is clear that the relationship between merit reforms and educational enrollment is conditioned by regime type, as is strongly consistent with theoretical expectations.

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[20]Estimates from the logit model with country dummies are available from the author on request.

Figure 1: Change in the Predicted Probability of Reform vs. Change in Enrollment Rates
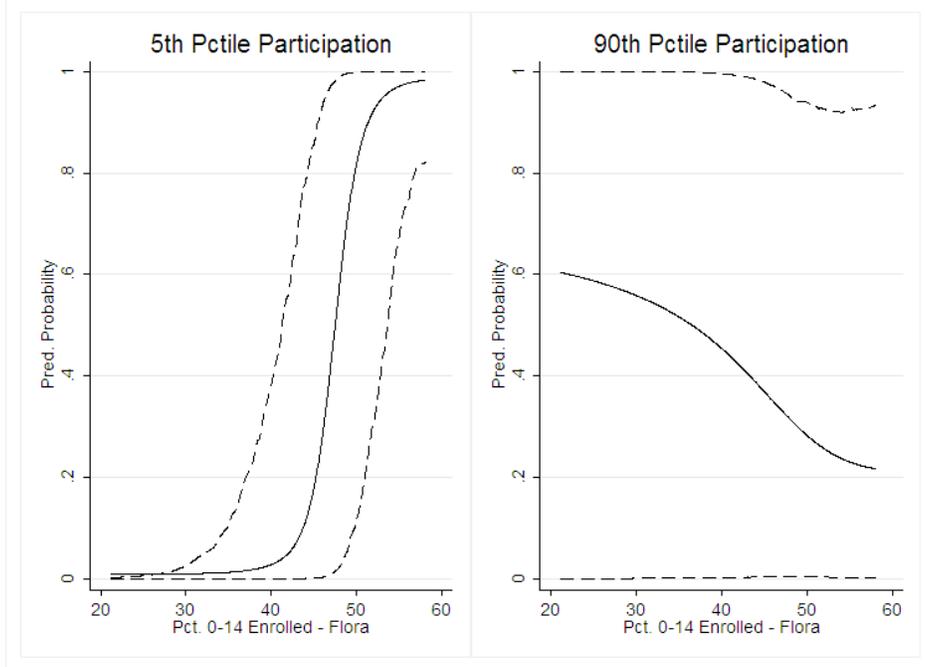
A plot of the predicted probability that a (pro) merit reform is undertaken by country i in time t against levels of educational enrollment, as measured by Flora (1987). 95 percent confidence intervals are marked by dashed lines. Levels of educational enrollment are reported on the x-axis, the predicted probability of reform is plotted on the y-axis. The graph to the left depicts the predicted probability of reform when levels of democratic participation are at their 5th percentile (i.e., 0 percent of the population over age 20 participated in elections). The graph to the right depicts predicted probability of reform when levels of democratic participation are at their 90th percentile(i.e., 87 percent of those aged 20 and above participated in elections). Country dummies are set at values such that the UK dummy is equal to 1 and all other equal to 0.

6 Robustness

6.1 Alternative Measures of Political Exclusion

It is possible that the results reported above stem from the definition of democratic participation, which here is a measure of the percentage of the population aged 20 and above that participated in elections. While this measure is advantageous as it is a continuous

(over the 0-100 interval) measure that does not rely on subjective judgments of the degree of democracy; it may be objected voter participation does not adequately reflect the extent to which the pool of subjects eligible for patronage is restricted. To address this concern, I run identical specifications to those discussed above employing alternative measures of democracy. One such measure is the frequently used `polity2` variable, an index that runs from -10 to 10 and that reflects features such as the competitiveness of executive recruitment, institutional constraints on the executive, and freedom of electoral participation. The other measure is an indicator for suffrage restrictions that is coded based on Przeworski's 2008 data on suffrage extensions. This indicator is equal to 1 if all independent males or all males are permitted to vote, and is coded as zero if there are property, income, or literacy-based restrictions on suffrage.

Table 3 reports coefficient estimates when the `polity2` measure is used as an indicator of democracy. Estimates from this model are very similar to those reported in Table 2 above. Education is predicted to increase the probability of merit reforms in all models, and these results are significant at the 95 percent level when the Flora education data are employed. All models also indicate that the association between educational enrollment rates and merit reforms declines in more democratic polities – a result that is significant at the 90 percent level when both the Flora and Mitchell data are employed. The results using the Mitchell and Lindert data are somewhat less precisely estimated than when the Flora data are used, but the results with regards to education border on significance in most models (p-values of 0.15 and 0.13 with the Mitchell data, of 0.17 and 0.30 with the Lindert data).

Figure 2 presents a graphical depiction of the change in the predicted probability of a merit reform as levels of educational enrollments (as measured by Flora) change. The graph to the left depicts predications when the polity score is at its 5th percentile (i.e., -9); while the graph to the right depicts predictions when polity is at its 90th percentile (i.e., 10, also the highest possible value).

The predictions plotted in this figure are quite similar to those in Figure 1, which are based on the model estimated with the democratic participation in place of the `polity2` variable. The conditional nature of the relationship between educational enrollment rates and merit reforms is quite starkly apparent. The probability of reform rises sharply as enrollment rates increase in highly autocratic polities. However, this relationship is nearly flat, and very imprecisely predicted, in democratic polities.

Finally, I re-estimate the model using an indicator variable – `wide suffrage` – in place

Figure 2: Change in the Predicted Probability of Reform vs. Change in Enrollment Rates
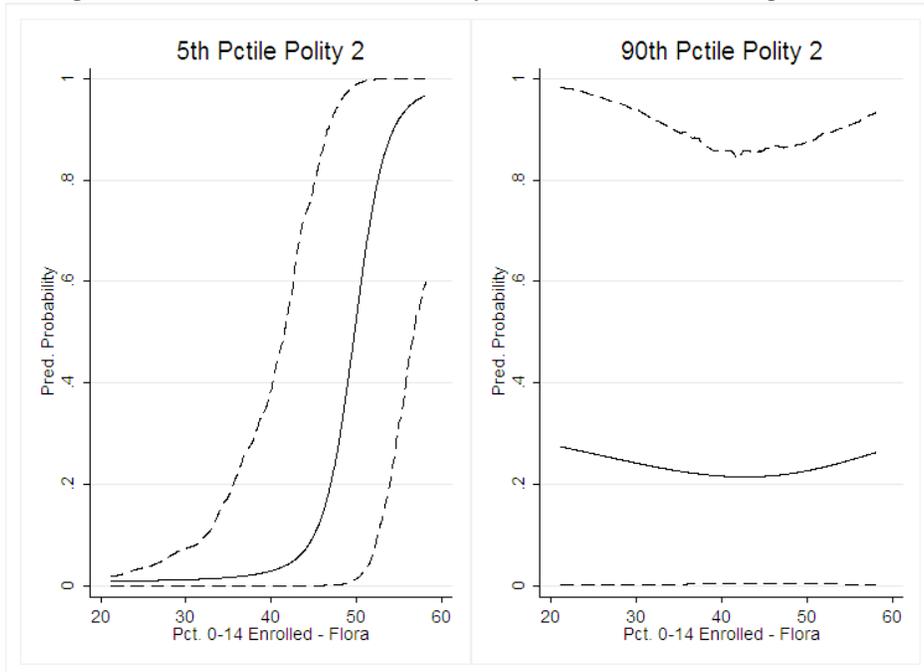
A plot of the predicted probability that a (pro) merit reform is undertaken by country i in time t against levels of educational enrollment, as measured by Flora (1987). 95 percent confidence intervals are marked by dashed lines. Levels of educational enrollment are reported on the x-axis, the predicted probability of reform is plotted on the y-axis. The graph to the left depicts the predicted probability of reform when levels of `polity2` are at their 5th percentile (i.e., -9). The graph to the right depicts predicted probability of reform when levels of democratic `polity2` are at their 90th percentile(i.e., 10). Country dummies are set at values such that the UK dummy is equal to 1 and all other equal to 0.

of both the `polity2` index and democratic participation rates. As noted above, this indicator takes the value of 1 when there is broad male suffrage, and a value of 0 otherwise. Results from this specification are reported in Table 4.

As in previous estimates, the coefficients on all measures of educational enrollment are positive and the coefficients on the interaction between enrollment rates and suffrage are negative. As with previous models, this relationship is most precisely estimated when the Flora figures are used, though coefficients on the interaction term are also significant when the Mitchell measures of educational enrollment are employed. In this instance, the

coefficients on enrollment rates are more precisely estimated when using the Lindert than with using the Mitchell data (p-values of 0.16 as opposed to 0.22).

In summary, educational enrollment rates are positively associated with merit reforms in autocracies and less strongly associated with reforms in democracies across a wide variety of specifications. These results hold when using several different measures of enrollment rates and a variety of different measures of democracy. These results are strongly in keeping with theoretical expectations, which hold that the decision of whether to adopt patronage or merit recruitment must be conditional on the distribution of skills amongst potential recruits. When the politically disenfranchised are highly skilled, the opportunity costs the government faces from adopting patronage, in terms of foregone bureaucratic capacity, are large. Consequently, merit reforms are most likely to be adopted when the disenfranchises are relatively skilled.

6.2   Endogeneity

Concern may reasonably be expressed, however, that the causal mechanism underlying the relationship between educational enrollment rates and the adoption of merit reform is not as described above. It is possible, for instance, that government's expanded educational systems with the aim of providing more skilled bureaucrats. As Weber (1976, 328) claims with respect to France, "Other times had seen the growth of a state bureaucracy that triggered the expansion of education to fill the available posts." Enrollment rates and merit reforms may be jointly determined by a common omitted – and likely unmeasurable – factor: the government's demand for increased state capacity. Alternatively, it is possible that an increased focus on merit qualification in the appointment of bureaucrats led to an increased demand for education. As a result, enrollment rates expand as the demand for skilled employees increased.

The structure of the empirical model above limits the adverse effects of these potential sources of endogeneity. The lag structure employed limits the threat of reverse causation – changes in recruitment policies were unlikely to directly cause changes in enrollment rates during the *prior* five year period. Moreover, it is not obvious why these alternative causal mechanisms would have a stronger influence in relatively autocratic and a lesser influence in democratic states.

However, these processes may operate with sufficiently large lags that endogeneity may still be a concern. The first such concern is that governments expanded the education system in order to obtain a more skilled bureaucracy. Thus, both merit recruitment and

educational requirements are driven by a common factor – the government's desire for increased state capacity. There are reasons to be dubious of such a claim. During this period, education was provided by a variety of private sector sources – particularly by religious organizations (Musgrave, 1968; Nuhoglu Soysal and Strang, 1989). Indeed, conflicts between these organizations and the government over provision of education were common. It is not evident whether, even if the desire existed, the state possessed the capacity to expand and reform the educational system for the purpose of increasing bureaucratic capacity. Indeed, in an analysis of 17 European states during this period, Nuhoglu Soysal and Strang (1989) find a negative association between compulsory education laws and school enrollment. This negative association is significant at the 95 percent level when their sample is confined to states that did not have a national church.

Were the state to deliberately expand education with the aim of increasing bureaucratic quality, it seems likely that it would do so through the passage of compulsory education laws. Such laws would ensure a broad pool of qualified potential recruits would be available for bureaucratic service. A prerequisite for such a strategy to be effective is that compulsory education laws substantially affect enrollment rates. Were these laws to have no – or only a small – effect, it seems improbable that the state could deliberately increase enrollment to satisfy its demands for skilled bureaucrats.

I examine the effects of compulsory education laws on enrollment rates through the use of a difference-in-differences analysis. I regress the change in *per capita* primary school enrollment on indicators for the change in compulsory education laws. All data in these analyses are from Flora (1987). My regressors include an indicator for the adoption or expansion of compulsory education requirements, an indicator for only those compulsory requirements that affect primary-aged children, and a measure of the change in the number of years of compulsory education. I additionally control for changes in GDP *per capita*, on the grounds that changes in economic conditions may affect both the demand for education and the demand for skilled workers (and thus, the presence or absence of compulsory education requirements). Results from these regressions are reported in Table 5.

As can be seen from these results, changes in compulsory education requirements are consistently associated with increases in enrollment rates – significantly so when the change in the number of years required is used as a regressor. However, these changes are rather small. The adoption of compulsory education requirements are associated with an increase of roughly 1.4 percentage points in the number of school aged children enrolled in primary school. Each year of additional education requirements is associated

25

with an increase of 0.4 percentage points in enrollment. This amounts to approximately 0.05 standard deviations in Flora's coding of the educational enrollment variable.

I also add controls for compulsory education requirements to the specifications used to assess the relationship between merit recruitment and educational enrollments above. Table 6 reports the coefficients from a fixed-effect logit regression of the adoption of merit standards against compulsory education requirements and the same set of controls as used above. Here compulsory education requirements are coded in levels rather than changes – in alternate specifications I control for an indicator of whether or not a compulsory education requirement was in place and a measure of the number of years of compulsory education required. Importantly, these regressions adjust for government attempts to increase enrollment rates – the coefficient on the enrollment variable reflects the association between merit adoptions and education after partialing out the role of deliberate government attempts to expand education by imposing mandated minimum years of schooling.

The results from this regression are very similar to those reported in Table 2 above. Educational enrollment rates are strongly associated with the adoption of merit reforms, even after controlling for compulsory education requirements. This association remains strongest in highly autocratic polities, and is decreasing in levels of democratic participation. And these results remain highly significant. It therefore seems that the association between enrollment rates and education levels is not simply a spurious association brought on by the government's desire to improve state capacity. Rather, these results are consistent with my theoretical contention that the government must weigh the costs of excluding highly-skilled members of the politically disenfranchised class when operating a patronage system of appointments.

Another alternative causal mechanism holds that increases in the demand for skilled officials under the merit system led to an expansion of school enrollments, as demand for educational qualifications increased. It is impossible to dismiss this concern entirely – changes in the demand for skilled bureaucrats must have had some effect on the demand for education, at the margin. However, it is improbable that this relationship drives the strong correlations we witness in the data.

First, as noted above, the lag structure of the empirical model employed cuts against this mechanism. Nor is it entirely clear why the demand for education would be more heavily effected by the adoption of a merit system in a relatively autocratic polity than in a relatively democratic one.

More importantly, demand-side factors likely played a smaller role in the expansion of

education during this period than did supply-side considerations. And demand for bureaucratic posts constituted a small portion of the demand for skilled employees. Demand for education was high – at least amongst the middle and upper classes – throughout the nineteenth century. As Musgrave (1968, 15) notes, "The rest of the middle class also wanted an education. Many could not afford to send their sons away to a public school..." The supply of schools was limited in part due to a heavy reliance on either religious institutions or tuitions for funding – which, given the positive externalities to schooling and the presence of credit constraints ensured that supply would be limited below optimal levels. For instance, at the turn of the nineteenth century, primary education in Prussia was primarily left to the church (Paulsen, 1912). In the UK, enrollment rates expanded rapidly after the 1870 Education Act granted government subsidies to local school boards that supported both state schools and privately organized (largely religious) 'voluntary' schools (Musgrave, 1968).

Demand for education was no doubt also dramatically increasing over the nineteenth and early twentieth centuries. This rise was largely driven by an increase in the size of the professional classes: including the medical, legal, pharmaceutical, and clerical professions. The middle of the nineteenth century saw the establishment of professional bodies – such as the Bar and Medical Associations – and, for the first time, the creation of minimal requirements for entry into these professions (Reader, 1966). These requirements often included the passage of written examination and minimal educational standards. While rising standards for employment in the civil service likely contributed to this increasing demand; the relatively small size of the civil service relative to the broader professional class suggests that its effect on enrollment rates was likely to be small. For instance, Silberman (1993, 343-344) puts total employment in 'public offices' in the UK in 1829 at 22,367.

Thus, while it is possible that increasing demand for skilled bureaucrats had some effect on educational enrollment rates; this effect is unlikely to be driving the results documented above. Variation in enrollment was primarily driven by factors exogenous to the demand for skilled bureaucrats. And it is unlikely that those making decision of whether or not to enroll their children in primary education were forecasting the methods the government would use to recruit bureaucrats several years in the future. Rather, it seems more plausible that the direction of causality runs in the manner asserted by the theory advanced in this paper: that expanding educational enrollment rates promoted the adoption of the merit system.

7 Conclusion

In this paper, I examine merit recruitment from a comparative perspective. I offer a general argument of when governments will find maintaining a patronage system costly relative to adopting a system of merit recruitment, and consequently when a merit reforms are likely to be adopted. Patronage is costly insofar as potentially skilled citizens may be excluded from bureaucratic office by virtue of their inability to provide payment to the government in exchange for posts. As this portion of citizenry grows increasingly skilled, the government's opportunity cost – in terms of forgone administrative capability – rises, making the adoption of a merit system more likely. In keeping with this theory, I find that 19th and early 20th century Western European governments were more likely to adopt the merit system as educational enrollment expanded, and that this association is particularly pronounced in instances when the patronage mechanism was likely to be particularly exclusionary. These findings are strongly consistent with the theory advanced.

The theory advanced in this paper is formulated in a general manner – and should be expected to pertain to bureaucratic systems outside of the European context. Patronage is always an exchange-based relationship in which payments and favors are offered for posts, and should thus always be expected to exclude some portion of the population – typically the politically and economically disadvantaged portion – from competing for office. To the extent that those so-excluded are likely to be skilled administrators, patronage will impose a cost on the government in terms of the productivity of the bureaucratic apparatus. The findings of this paper therefore have implications well beyond the European case. Of course, this theory's predictive power remains to be established in settings outside of Western Europe during the 1845-1944 period, and the operationalization of the theoretically relevant measures (for example, the composition and skills of those marginalized under patronage) may vary across cases. I leave such further empirical analyses to future work.

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A   Data Appendix

Table 2: Fixed-Effect Logit Results

	Flora Ed. Measures	Mitchell Ed. Measures		Lindert Ed. Measures	
Education *per capita*	0.916***	0.198	0.198	0.122	0.135
	[0.249,1.584]	[-0.076,0.472]	[-0.070,0.465]	[-0.077,0.321]	[-0.042,0.311]
Dem. Participation	0.717**	0.218	0.223	0.038	0.009
	[0.096,1.338]	[-0.052,0.487]	[-0.050,0.495]	[-0.162,0.238]	[-0.172,0.190]
Education *per capita*	-0.015**	-0.005*	-0.005*	-0.001	-0.001
× Dem. Participation	[-0.028,-0.002]	[-0.010,0.001]	[-0.010,0.001]	[-0.004,0.002]	[-0.003,0.002]
GDP *per capita*	-0.002	-0.001		0.001	
	[-0.005,0.001]	[-0.003,0.001]		[-0.002,0.004]	
War	-0.821	-0.732		-0.870	
	[-3.009,1.368]	[-2.740,1.275]		[-2.486,0.745]	
Ever Merit	-1.635	-0.726	-1.280	-0.479	-0.465
	[-5.378,2.107]	[-4.753,3.301]	[-5.096,2.536]	[-3.018,2.060]	[-2.673,1.742]
Cubic Time Polynomial	✓	✓	✓	✓	✓
# of Countries	6	6	6	6	6
# of Obs	65	64	64	80	82

Results of a fixed-effects (conditional) logit regression of $merit_{i,t}$ on covariates. 95 percent confidence intervals are presented in brackets. * denotes significance at the 90 percent level, ** denotes significance at the 95 percent level, and *** denotes significance at the 99 percent level.

Table 3: Fixed-Effect Logit Results: Polity 2 Measure

	Flora Ed. Measures		Mitchell Ed. Measures		Lindert Ed. Measures	
Education *per capita*	0.575**	0.496**	0.201	0.185	0.091	0.046
	[0.080,1.071]	[0.101,0.890]	[-0.066,0.467]	[-0.056,0.427]	[-0.039,0.220]	[-0.042,0.134]
Polity 2	2.625*	2.152*	1.355	1.255	0.203	0.150
	[-0.326,5.577]	[-0.104,4.408]	[-0.317,3.028]	[-0.267,2.777]	[-0.302,0.707]	[-0.252,0.551]
Education *per capita*	-0.058*	-0.047*	-0.028*	-0.026*	-0.002	-0.002
× Polity 2	[-0.121,0.005]	[-0.094,0.000]	[-0.061,0.005]	[-0.055,0.004]	[-0.010,0.006]	[-0.008,0.005]
GDP *per capita*	0.001		0.000		-0.000	
	[-0.002,0.005]		[-0.003,0.003]		[-0.003,0.002]	
War	-0.399		-0.750		-0.169	
	[-2.454,1.657]		[-2.818,1.318]		[-1.446,1.109]	
Ever Merit	-3.236	-2.434	-1.352	-1.458	-0.835	-0.745
	[-10.544,4.072]	[-8.665,3.798]	[-5.956,3.253]	[-5.759,2.843]	[-3.118,1.448]	[-2.712,1.221]
Cubic Time Polynomial	✓	✓	✓	✓	✓	✓
# of Countries	6	6	6	6	9	10
# of Obs	67	67	69	69	115	125

Results of a fixed-effects (conditional) logit regression of $merit_{i,t}$ on covariates. 95 percent confidence intervals are presented in brackets. * denotes significance at the 90 percent level, ** denotes significance at the 95 percent level, and *** denotes significance at the 99 percent level.

Table 4: Fixed-Effect Logit Results: Suffrage Restrictions

	Flora Ed. Measures		Mitchell Ed. Measures		Lindert Ed. Measures	
Education *per capita*	0.327**	0.345**	0.181	0.184	0.100	0.086
	[0.019,0.635]	[0.030,0.661]	[-0.112,0.474]	[-0.111,0.479]	[-0.040,0.240]	[-0.035,0.206]
Wide Suffrage	3.878	4.853	4.330	4.150	0.491	2.127
	[-7.936,15.693]	[-6.310,16.015]	[-6.614,15.275]	[-6.601,14.901]	[-6.100,7.082]	[-3.972,8.227]
Education *per capita*	-0.146	-0.175	-0.137	-0.138	-0.038	-0.066
× Wide Suffrage	[-0.427,0.135]	[-0.443,0.094]	[-0.398,0.124]	[-0.396,0.120]	[-0.156,0.081]	[-0.177,0.045]
GDP *per capita*	-0.001		-0.001		-0.001	
	[-0.003,0.002]		[-0.003,0.002]		[-0.003,0.001]	
War	-0.225		-0.672		-0.075	
	[-2.215,1.766]		[-2.740,1.397]		[-1.396,1.246]	
Ever Merit	-0.801	-1.202	-0.900	-1.158	-1.001	-1.371
	[-4.647,3.045]	[-4.857,2.454]	[-4.595,2.796]	[-4.839,2.523]	[-3.337,1.334]	[-3.178,0.436]
Cubic Time Polynomial	✓	✓	✓	✓	✓	✓
# of Countries	6	6	6	6	9	10
# of Obs	67	67	70	70	116	130

Results of a fixed-effects (conditional) logit regression of $merit_{i,t}$ on covariates. 95 percent confidence intervals are presented in brackets. * denotes significance at the 90 percent level, ** denotes significance at the 95 percent level, and *** denotes significance at the 99 percent level.

38

Table 5: Difference-in-Differences Regression of *per capita* Enrollment Rates on Compulsory Education Requirements

	Expansion of Comp. Ed.	Primary Ed. Comp.	Years Compulsory Ed.
Δ compulsory	1.349	1.925	0.401**
	[-0.441,3.140]	[-0.710,4.561]	[0.006,0.796]
Δ GDP *per capita*	-0.002*	-0.001	-0.001*
	[-0.003,0.000]	[-0.003,0.000]	[-0.003,0.000]
Constant	1.298***	1.303***	1.276***
	[0.600,1.995]	[0.605,2.001]	[0.589,1.962]
# of Obs.	114	114	114

Results of an OLS difference-in-differences specification regressing changes in enrollment rates (as measured by Flora (1987)) on changes in compulsory education laws. 95 percent confidence intervals are presented in brackets. * denotes significance at the 90 percent level, ** denotes significance at the 95 percent level, and *** denotes significance at the 99 percent level.

Table 6: Fixed-Effects Logit Results Controlling for Compulsory Education

	Compulsory Ed.	Years Compulsory Ed.
Flora Ed. *per capita*	0.936***	0.910***
	[0.282,1.590]	[0.255,1.566]
Dem. Participation	0.588*	0.669**
	[-0.025,1.201]	[0.017,1.321]
Flora Ed. *per capita*	-0.013**	-0.014**
$\times$ Dem. Participation	[-0.025,-0.000]	[-0.027,-0.001]
GDP *per capita*	-0.002	-0.002
	[-0.005,0.001]	[-0.005,0.001]
War	-0.581	-0.754
	[-2.956,1.795]	[-3.002,1.494]
Ever Merit	-1.648	-1.291
	[-5.649,2.352]	[-5.403,2.821]
Compulsory	-3.497	-0.166
	[-9.922,2.927]	[-0.968,0.637]
Cubic Time Polynomial	✓	✓
# of Countries	6	6
# of Obs	65	65

Results of a fixed-effects (conditional) logit regression of $merit_{i,t}$ on covariates. 95 percent confidence intervals are presented in brackets. * denotes significance at the 90 percent level, ** denotes significance at the 95 percent level, and *** denotes significance at the 99 percent level.

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