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Colonial Administrators and Public Educational Investments in French West Africa

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Colonial Administrators and Public Educational Investments in French West Africa

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Abstract

This paper examines to what extent colonial district administrators mattered for colonial public investments in education in West African former colonies. To do so, I use fixed-effects methods on a newly collected district-administrator matched panel dataset, which I combine with data on colonial public investments. I find that the administrator fixed effects contribute little in explaining the variations of investments in education; the district fixed effects are in fact much more important to explain these variations. Further investigations into the heterogeneity among the administrator fixed effects suggest that it is the early colonial district administrators that matter for public educational investments. This finding thus gives support to the idea of a path dependence in the investment policy strategy across French West African districts during the colonial era.

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1 Introduction

While several developing countries have experienced relatively fast economic growth since the second half of the 20th century, most African countries remain at low levels of economic development. In fact, today, 24 of the 30 poorest countries in the world (in terms of GDP per capita) are located in Africa (Central Intelligence Agency, 2013). And yet, there exist some disparities not only across African countries but also within them (i.e., across the regions). Understanding why the African continent faces such poverty, why some of its regions grow faster than others and, in particular, to what extent history can account for its current heterogeneous but overall poor economic performance, is therefore crucial.

Since the seminal works of La Porta, López-de-Silanes, Shleifer, and Vishny (1997, 1998) and Acemoglu, Johnson, and Robinson (2001), a growing body of empirical research has investigated the role of history on long-run economic development. As noted by Nunn (2009, 2014) in his surveys on historical development, the subsequent studies that have emerged from these contributions innovate with respect to the early literature on the topic[1] for relying on the collection and analysis of new data as well as on the implementation of new empirical strategies to identify the causal impact of a historical event over time and investigate the mechanisms underlying it. However, there are still debates around the specific mechanisms that are at play.

The research question that I address in this paper is in line with the body of literature that emphasizes the role of colonial rule. More specifically, I investigate the influence of colonial administrators on educational investments made at the district level during the colonial period in French West Africa. Focusing on this region of the African continent has two main advantages. On the one hand, it enables me to control for the colonizer's identity, given that only France colonized that region of the African continent. Controlling for the colonizer's identity could be necessary, since the identity of the colonizer has been shown to be an important determinant of subsequent economic development (Grier, 1999; Bertocchi and Canova, 2002). In particular, the nature of the legal systems that colonizing countries imported to colonies could have affected the development path of former colonies (La Porta et al., 1999; Acemoglu and Johnson, 2005). On the other hand, French West Africa was remarkably homogeneous in terms of historical, anthropological, geographical and cultural features, which facilitates the comparison across its districts and makes it more reliable. Districts are the relevant level of analysis because (i) decisions regarding investments in education were made at the district level by the colonial administrators who were in charge of these districts, and (ii) colonial public investments could exhibit large variations from one district to another. Focusing on colonial administrators enables me to examine the influence of the colonial administration rather than the more general role of institutions in determining the level of public investments in human capital. Since the seminal

[1] E.g., Knack and Keefer (1995), Mauro (1995), Grier (1999), Englebert (2000a, 2000b), Bertocchi and Canova (2002), and Price (2003). See Fenske (2010) for a review of the "old" and the "new" literature on the African economic history.

work of Romer (1986) on endogenous growth theory, it is well-established that education, and more generally human capital, is an important determinant of long-term economic growth, although the question of whether the institutional cart comes after the human capital horse is still under debate (Glaeser, La Porta, López-de-Silanes, and Shleifer, 2004). For West French Africa, Huillery (2009) shows that colonial investments in education matter for current educational outcomes. By exploiting the spatial discontinuities of colonial public investments in French West Africa, she notably finds that present-day educational outcomes have been determined by the colonial investments in education, but not by those made in the health or infrastructure sectors during that period. Her results also provide evidence that the regions that received more investments in the early colonial period continued to receive more today.

The contribution of this paper to the literature is threefold. Firstly, I extend the analysis of Huillery (2009) by examining to what extent colonial administrators could have induced the variations in public investments in education across French West African districts. Specifically, I am interested in assessing the relative importance of colonial administrators in explaining these variations. The idea behind is that colonial district administrators are heterogeneous in their profiles and that they are autonomous in the policy-making decisions in their districts, especially at the beginning of the colonial era, so they could have differed in their investment decisions. Studying this question would help us better understand why some districts received more educational investments than others, and in particular, whether colonial administrators played an important role. To investigate this question – and here lies the second contribution of this study – I use fixed-effects techniques imported from the literature in labor economics (see Section 2.2 for a review). To the best of my knowledge, this paper is one of the very first attempts to bring these fixed-effects methods into the literature in economic history and development economics. My empirical strategy exploits different groups of fixed effects – namely, administrator, year and district fixed effects – and relies on the fact that colonial administrators moved from one district to another (depending on the allocation which was decided by the general governor at the head of the French West African federation) and generally stayed in a given district for a sufficient amount of time. These fixed-effects methods considerably reduce the threat of omitted-variable bias. Moreover, they enable me to identify potential heterogeneous effects among administrators. I notably test the hypothesis of a path dependence, which is that the first district administrators who arrived in the districts set the path of investments and their successors tended to follow the same investment policy strategy. The third contribution of this paper is the use of a newly collected matched district-administrator database, which enables me to assess the role of administrator with administrator fixed effects. This is possible because this dataset identifies all the administrators who were present in a given year in each district of French West Africa over the 1906-1929 period (which corresponds roughly to the first half of the colonial era).

The empirical results from these fixed-effects methods show that the administrator fixed effects explain at most 22% of the variation in the levels of educational investments, and less than

3% if district fixed effects are included in the analysis. In fact, the district fixed effects turn out to account for a much larger share of these variations. A comparison of the results from a two-way fixed-effects model and those from a three-way fixed-effects one shows that if district administrators matter, it appears to be due to a good matching between the administrators and the districts they were in charge of (presumably because of the non-perfect random allocation of administrators across districts). Also, a further investigation of heterogeneous effects among administrators suggests that it was the colonial administrators who arrived first that most influenced public educational investments. This result therefore supports the idea of a path dependence in the investment strategy.

The remainder of the paper is organized as follows. Section 2 presents literature reviews on historical development and fixed-effects methods; Section 3 describes the historical background; Section 4 presents the data; Section 5 explains the empirical framework; Section 6 reports and comments the results; Section 7 provides some robustness tests; and Section 8 concludes.

2 Related Literature

2.1 Colonial Rule and Historical Development

A large number of studies contribute to show that history matters for long-run economic development. Of our interest in this paper is the stream of the research on historical development that has documented the importance of European colonial rule. In particular, the literature has identified and examined several channels through which institutions and human capital shape contemporary economic performance (see Acemoglu, Gallego, and Robinson (2014) and Nunn (2014) for recent complete surveys). Acemoglu et al. (2001) examine the causal impact of contemporary domestic institutions on long-run economic development by instrumenting domestic institutions with a measure of European settlers' mortality rates. The authors argue that the disease environment faced by potential European settlers was a main determinant of the form of colonial rule: the Europeans settled in temperate areas where they developed institutions that promoted the protection of property rights, whereas in areas with high European mortality rates (e.g., sub-Saharan Africa), they did not settle but extracted its natural resources. However, Glaeser et al. (2004) challenge their view, which they criticize for leaving aside the role of human capital. These authors use various empirical strategies to show that human capital is the main determinant of differences in long-term economic growth. More precisely, they argue that the European colonial experience induces not only institutional variation, but also direct human-capital variation, which comes from the initial human capital of the early colonizers. Gennaioli, La Porta, López-de-Silanes, and Shleifer (2013) attempt to make a similar argument analyzing cross-regional-level data and reporting OLS results in which the variables measuring human capital and institutions are treated as exogenous while these are likely to be endogenous, thus biasing the estimates and not telling much about the causal effects on economic performance. Although

the debate is still open, the role of education on economic development is fundamental, and this view has been taken into account with the development of endogenous growth theories.

At a more micro level of analysis, a number of research papers have studied the long-run (heterogeneous) effects of colonial heritage within Africa. By taking advantage of the spatial discontinuities of colonial investment policy, Huillery (2009) investigates the impact and the persistence of early colonial public investments on current outcomes in the sectors of education, health, and infrastructures. She makes a cross-district comparison in French West Africa and finds that the regions that received more public investments in a particular public good during the colonial period not only continue to receive more investments today but they also display better current outcomes related to that public good. Specifically, more colonial educational investments lead to better educational performances today, but have no effect on contemporary outcomes related to health or infrastructures. Similarly, current health (infrastructures respectively) outcomes are determined solely by colonial investments in health (infrastructures respectively). Bolt and Bezemer (2009) also find statistical relationships between colonial investments in education and health and present-day outcomes. In line with Acemoglu et al. (2001), Huillery (2011) provides some evidence that the European settlement in West Africa had a positive effect, including in the areas involving an "extractive strategy", but the densely settled regions experienced a faster development than the poorly settled ones. In addition, several research articles deemphasize the role of colonial and current nationwide institutions in the hinterland (e.g., Davidson, 1992; Herbst, 2000; Michalopoulos and Papaioannou, 2014b), and instead focus on other features besides formal institutions, such as important historical events, cultural norms, or family ties.[2]

2.2 Fixed Effects and Leadership

Fixed effects are usually used in regressions as covariates to control for characteristics that do not vary over time; they thus help reduce the potential problem of omitted variables. However, in this paper, I am interested in fixed effects *per se*. The fixed-effects models that I use build on the literature in labor economics and have been extended to other fields, such as the finance literature in which fixed effects serve to examine the role of firm and manager heterogeneities – that is, the time-constant firm and manager characteristics – on different outcome variables, such as executive compensation (Graham, Li, and Qiu, 2012). Different methods exist for these types of fixed effects models but each has its caveats. A simple approach consists in combining the influence of firm and manager fixed effects by creating binary variables for each "spell" – i.e., each firm-manager combination. However, even though this approach reduces concerns about potential estimation bias, it prevents disentangling firm fixed effects from manager fixed effects, and it is thus impossible to measure their relative importance. This is because managers and firms share the same dimension, so one cannot identify their effects separately. Bertrand and

[2]For recent surveys, see Alesina and Giuliano (2013), Algan and Cahuc (2013), Nunn (2013), Spolaore and Wacziarg (2013), and Michalopoulos and Papaioannou (2014a).

Schoar (2003) adopt another approach which enables the separate identification of firm and manager fixed effects. They assess the influence of managers on various corporate activities by studying panel data to which they apply a three-way fixed-effects model (with firm, manager, and time fixed effects). The manager effects are disentangled from the firm effects because they restricted their sample to the managers that have changed firms at least once. Nevertheless, one potential drawback with this method is that managers that have moved could systematically differ from those who never changed firms. Moreover, because they take advantage of the fact that managers moved from one firm to another, which is not that frequently observed, their sample is relatively small. The method first developed by Abowd, Kramarz, and Margolis (1999) – thereafter called the AKM method – and later on refined by Abowd, Creecy, and Kramarz (2002) uses a longitudinal employer-employee matched dataset that can potentially alleviate the small sample size problem because it exploits instead a "connected sample", that is, a subsample composed of movers but also of the non-movers who worked in companies that had at least one mover. This may increase the sample size and thus the power of the estimates.[3] The AKM approach has notably been applied to examine political leadership. For example, Yao and Zhang (2015) use the AKM method on a unique city-leader matched dataset over the 1994-2010 period in China to study the influence of Chinese city leaders on local economic growth and how this affects their chances of promotion. Taking advantage of the fact that Chinese subnational leaders are often shuffled across cities and thus serve in more than one city, they can compare leaders over time and across the "connected cities" (and hence disentangle the leader fixed effects from the city fixed effects). And, because the city leaders face the same national institutions, they can isolate leaders' personal abilities from institutional factors. By testing the joint significance of the leader fixed effects, they find that (i) leaders matter for Chinese cities' economic growth, and (ii) age plays a key role in determining a leader's chances of promotion (i.e., there exists a negative relationship between age and chances of promotion), but (iii) personal abilities matter more in determining promotion. Graham, Li, and Qiu (2012) employ the AKM method, as well as the spell method and the three-way fixed-effects method used in Bertrand and Schoar (2003), to assess the influence of firm- and manager-specific heterogeneities in executive compensation. They find that most of the variation in executive pay is due to these time-constant firm and managerial effects.

3 Historical Background

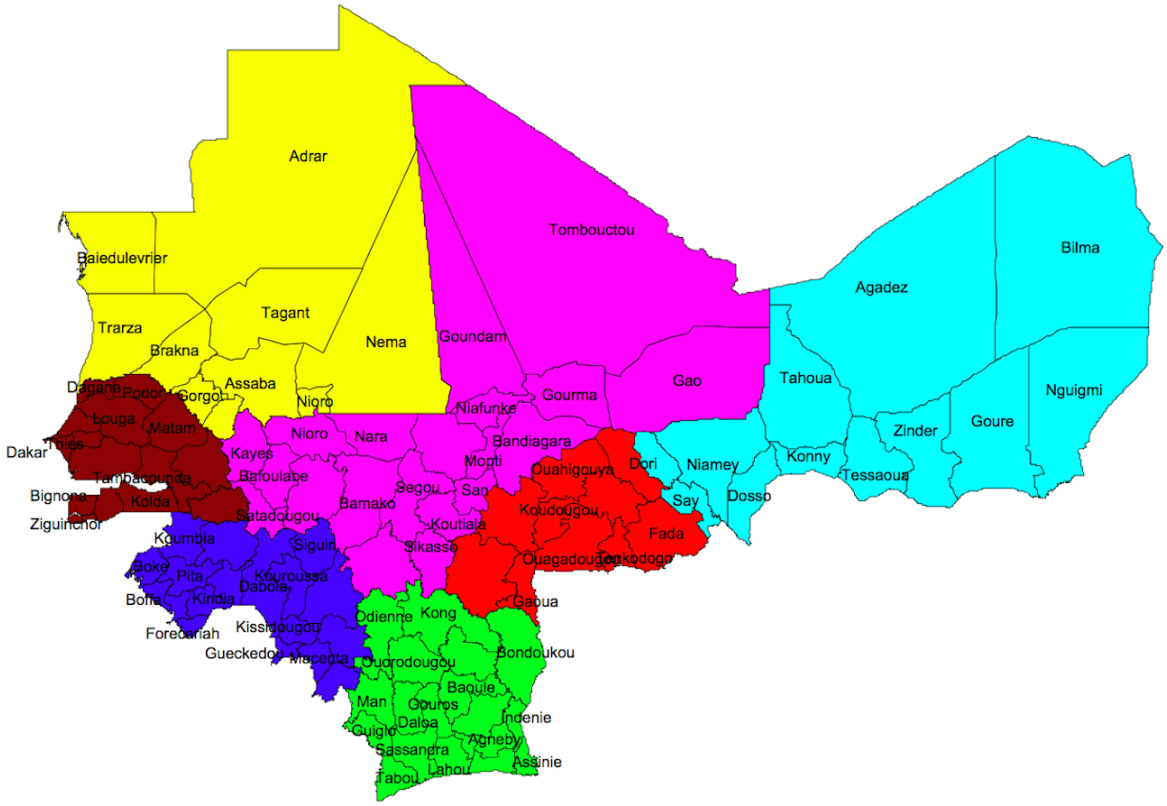
3.1 French Colonization

Geography and brief history The colonial era of French West Africa officially began in 1895 and ended in 1960 (except for French Guinea which became independent in 1958, all the other French West African colonies gained their independence in 1960). However, factually, the French

[3] It is not the case if there are no movers and/or they do not stay long enough in a given firm.

colonization started as early as 1854 from the West coast, and expanded progressively from West to East and South to North, until the official creation of the federation of colonies of West Africa in 1895 (see Huillery (2009) for details about the colonial expansion). Figure 1 is a map of French West Africa in 1925. The whole area of French West Africa was roughly eight times larger than France. The federation was divided into eight colonies – namely, Dahomey, French Guinea, Ivory Coast, Mauritania, Niger, Senegal, French Sudan, and Upper Volta[4] – which were themselves split into districts (*cercles*); there were 120 districts in 1925. Effectively, each French colony was occupied by the following years: Senegal, 1890; French Sudan, 1893; Dahomey, 1893; French Guinea, 1898; Niger, 1906; Mauritania, 1910; Ivory Coast, 1914 (Andereggen, 1994).[5] Although officially forming a federation of colonies, French West Africa was initially not administratively united. The civil administration was set up progressively from 1900 to 1920 while the federal government became effective only in 1904. Hence, the administrative occupation could be considered to have actually lasted from approximately 1910 to 1960.

Figure 1: Map of the French West African districts in 1925



Note: Senegal is colored in brown, Mauritania in yellow, French Guinea in dark blue, French Sudan in fuschia, Ivory Coast in green, Upper Volta in red, and Niger in cyan. Source: Huillery (2006)

[4]Dahomey, French Sudan, and Upper Volta correspond respectively to today's Benin, Mali, and Burkina Faso.

[5]Upper Volta did not initially exist – it was parts of Niger, Ivory Coast, and French Sudan. It was created in 1920, dismembered in 1932, and re-created in 1947.

Administrative organization The structure of the colonial administration was pyramidal: the general governor (*gouverneur général*, later on called *haut-commissaire*) was at the head of the federation, followed by lieutenant governors (*lieutenant-gouverneurs*) at the head of the colonies, administrators (*administrateurs* or *commandants de cercles*) at the head of the districts, and African chiefs at the head of villages. The main objective of the general governor was the rational development of the federation as a whole. However, even though officially (and highly) centralized, the colonial administration was effectively decentralized. In fact, the district administrators possessed almost all of the effective power: they were considered as "the real chiefs of the French empire" (Delavignette, 1939). Before WWI, "[the colonial district administrator] is alone, he is everything, in charge of and responsible for everything [...]" (Deschamps, 1975); he is "omnipresent and omnipotent" (El Mechat, 2009). Because of physical distance (due to the size of French West Africa) and communication difficulties, the district administrators could make decisions about the policies to be implemented in their district without having to give their lieutenant governor or general governor an account of their local policies. Thus, colonial policies could have differed across the districts of a same colony.

Colonial budgets for education The budgets for investments in education came from colonies' local budgets, which were credited with local taxes raised mostly on indigenous populations. Apart from Mauritania (which benefited from federal subsidies), each colony had to bear and finance the costs of French colonization with its own resources, of which 60% were from the capitation tax. However, taxes raised at the district level were brought together at the colony level, so colonial investments made at the district level, which include investments in education, were not necessarily proportional to district taxes. Hence, a district's contributions to local budgets could be important, and yet, the investments made in that district could be low. Conversely, other districts did not contribute much, but still received large investments. On average, districts received 70% of the colonies' resources, with 15% that covered education and health expenses (staff and materials); the remaining 30% went to the government and central services (Huillery, 2009). In each district, the local budgets were voted at the end of each year. Even though local budgets did not mention any explicit investment strategy, historical documents on the French colonial administration tend to show that French district administrators had a lot of autonomy and power in policy making at the district level (Cohen, 1973). District administrators had to decide the investments in terms of public goods (namely, education, health, and infrastructures). For educational investments in particular, colonial district administrators determined how many European and African teachers, teaching assistants, and teaching materials would be needed in their district for the following year.

3.2 District Administrators

Tasks The district administrators had a lot of autonomy and were in charge of several important tasks. In terms of purely administrative and "political" tasks, they had to represent the lieutenant governor at all official events, draw up all official civil status certificates, draw the district map, and visit each village of their district at least once a year. The district administrators were also involved in the public services of their districts as they were in charge of steering French primary schools and overseeing Muslim schools, planning and overseeing the construction of transport infrastructures (bridges, roads, railways, and wells), and advising indigenous people about how to improve their agricultural methods. In addition, they also had some legal power as they could arrest and judge criminals according to the code of native status[6]. Regarding the economic and financial management of the districts, they were in charge of opening new markets, supervising tax collection, and managing the local budget (Association des anciens élèves de l'École nationale de la France d'outre-mer, 1998). In particular, the French district administrators defined the amount of taxes to collect and the number of reservists to recruit, and it was the African chiefs who were placed in charge of collecting taxes, supervising compulsory crop cultivation for the French colonizers, recruiting Africans for the French army, and rounding up forced labor (Lombard, 1967). These chiefs were deprived of their traditional civil and criminal powers and they only had the powers specifically granted to them by the French administration. In fact, the district administrators were in general hostile towards local chiefs. The authority and control of the French administrators over the local people were maintained thanks to the support of paramilitary district guards (*gardes de cercles*).

Recruiting process Prior to 1887, the district administrators' were recruited directly by the governors of the colonies. The average length of appointment in each colony was two to three years. The administrative system of each colony, including the recruitment process of district administrators, was similar to the one built by Louis Faidherbe in Senegal.[7] Shortly after being appointed as the governor of Senegal in 1854, Faidherbe noticed that even the senior civil servants (*hauts fonctionnaires*) of the colonies, which included governors and administrators, were in general unqualified. The poor quality of the body of colonial district administrators (*corps des administrateurs des colonies*) did not improve until the beginning of the 20th century. Indeed, despite large monetary incentives, it was difficult to attract qualified men to serve the colonies because life there was uncomfortable and unhealthy – e.g., almost all of the administrators suffered from frequent malaria crises, and some even contracted the sleeping disease.

[6]This code, called *code de l'indigénat* in French, treats the Africans differently from the Frenchmen (Arnett, 1937), except for the inhabitants of the four Senegalese communes of Saint-Louis, Gorée, Dakar, and Rufisque, who benefited from the status of French citizenship due to the early experiments in assimilation in that colony (Andereggen, 1994).

[7]I focus on the Senegalese colonial administration because it is well-documented and it served as a model in the other colonies. In fact, most colonies faced the same problems as Senegal regarding the administrators' recruitment.

In 1887, when all the colonial administrators were gathered in a single body, a central administration in Paris took charge of their recruitment and tried to make it more homogenous across the colonies. The French administration notably created in 1894 the Ministry of Colonies, an independent and thus more prestigious administrative entity dealing with colonial affairs, including the appointment of district administrators. Between 1887 and 1912, the central administration in Paris applied different recruitment methods, each of them contributed to improve in one way or another the recruitment of colonial administrators to serve in West Africa. Nevertheless, due to a lack of candidates also caused by the extension of the conquered lands,[8] the French colonial administration continued for several years to attract only men who were incapable of pursuing any promising career in France, that is, those with no future prospects, even people who had broken the law – e.g., criminals and former prisoners.

The body of colonial administrators was therefore very heterogeneous in its composition. Several profiles of recruits could be identified: (a) colonial officers, who were usually violent and very tough towards local people, (b) metropolitan civil servants, who in general did not choose to serve in the overseas colonial administration and were often inefficient administrators, (c) members from the administration office of the colonial canton capitals, who already had at least two years of overseas experience but were not prepared to be administrators, (d) agents,[9] who despite their lack of instruction had the advantage of already being familiar with the position of administrators thanks to their past overseas experience, and (e) pupils of the colonial school[10], who were recognized as the elite of the body and were the best administrators.

Table 1: Administrators by capacity and origins, 1887-1914.

Administrators	(a)	(b)	(c)	(d)	(e)	others
judged competent	29 (46%)	12 (30%)	17 (33%)	260 (57%)	50 (68%)	1 (10%)
judged incompetent	25 (40%)	21 (51%)	32 (63%)	113 (25%)	13 (18%)	8 (80%)
insufficient information	9 (14%)	8 (19%)	2 (4%)	85 (18%)	10 (14%)	1 (10%)

source: Cohen (1973), p. 50.

Overall, prior to World War I (WWI), although some colonial administrators were conscientious, brave, and respectful towards local people, the majority of them were violent and tyrannical. The situation progressively improved after 1914, and even more so after World War II (WWII). Nonetheless, despite the better quality of men who joined the body of colonial administrators between 1920 and 1940, the administrative system remained the same. During WWII, the colonial school continued to train a large number of administrators. However, in the 1950s, the school started to decline because of Indochina's decolonization and the creation of the *École*

[8] Between 1887 and 1913, the number of administrators was multiplied by 20.

[9] The "agents" were just under the district administrators in the hierarchy and were recruited by the governor.

[10] It was created in 1889 and named *École coloniale*, then it took the name of *École nationale de la France d'outre-mer* in 1934, and finally changed to *Institut des hautes études d'outre-mer* in 1959 just before the end of the French colonization in West Africa.

Nationale de l'Administration, a school of public administration which had a better reputation. In 1956, students of the colonial school wrote a manifesto that recommended to give internal autonomy to African countries and reduce the role of administrators to economic advisors. Thereafter, a series of decrees progressively granted them autonomy, and the body of administrators included more and more African people. After the colonies' independence, a certain number of administrators stayed in the ex-colonies to advise the new African governments.

4 Data

For my analyses, I combine the datasets from Huillery (2009, 2011, 2014) on political events, public finance, pre-colonial and geographical characteristics, with a newly collected matched district-administrator dataset. All data are at the district level, and a unit of observation is a year-district-administrator combination. Because the districts could have been modified (i.e., merged with other districts or split into several districts) between 1910 and 1928 (i.e., the period for which I have data for public investments in education), I keep track of the districts by using those of 1925 as a reference. Out of the 100 districts in 1925, 24 were formed from districts that have merged. Using the districts as of 1925 also enables us to add control variables in our regressions, since the data for these variables in Huillery (2009, 2011, 2014)'s datasets were entered using the districts as of that year.

4.1 Data for the Outcome and Control Variables

Outcome variable The dependent variable of interest is colonial public investments in education, as proxied by the yearly number of teachers per 100,000 inhabitants per district over the 1910-1928 period (when available).

Within each district, the budget devoted to educational investments for the following year was decided at the end of each year. Hence, I restrict the sample to the administrators that were present in December of each year, as they decided the budget for the following year. For example, in a given district and year t, it was the administrator present in December of year t who decided the budget for year $t+1$. However, because I want to use the panel dimension of my database, two issues arise: (i) there could have been up to three administrators in December in a given district and year (but I know neither the exact date at which the budget was voted nor the exact starting and ending dates of the administrators' stay in a given district), and (ii) some districts merged before 1925, which is problematic because for the years before the merge, there would be (at least) two administrators for a single district as of 1925. To deal with these issues, for my main analysis I select the very last administrator of December and the administrator of the district with the largest population when two districts merged.[11]

To compute the district number of teachers per 100,000 inhabitants (so that the comparison

[11] I then perform some robustness checks on other subsamples (cf. Section 7).

of public investments in education across districts is more reliable, since one could argue that the investments were greater in districts with a large population), I use data on district populations from Huillery (2009) and the *Annuaires du Gouvernement général de l'Afrique occidentale française*.[12] Data on colonial investments in education at the district level come from the annual local budgets over the 1910-1928 and are those of Huillery (2009). Restricting the database to the district-year pairs for which the data for the number of teachers is available and extrapolating the total population per district when possible, the sample contains 925 observations, of which 32% are non-extrapolated values, 53% are extrapolated ones, and the remaining 15% are missing (see Appendix D for details about the extrapolations by colony). Further restricting the sample to the administrators that were observed in at least two different districts (the "mobile sample"), there are now 516 observations, of which 38% are non-extrapolated.

Control variables The covariates that I use in my empirical analyses can be classified into the following categories: time-variant district characteristics, time-variant administrator characteristics, and time-invariant district characteristics.

The set of time-varying district and colony characteristics includes colonies' and districts' ordering as reported in the *Annuaires du Gouvernement général de l'A.O.F.*, which is used as a proxy for the importance of respectively each colony and each district within a colony relative to the others and district population. It seems important to control for these as it could well be that educational investments would be larger in more important districts. The time-variant administrator characteristics contain administrators' rank types (see Table B2 in Appendix B for details) and their past experience (as measured by the number of months they previously spent as a district administrator). Promotion to a better rank is usually based on the time spent in the colonial administration body rather than on merit.[13] It is also important to control for past experience as a district administrator, because administrators could learn and develop their abilities and skills, which could ultimately affect their decisions regarding the level of investments in education. As for the time-constant district characteristics, these mainly contain geographical and historical variables, such as districts' altitude, latitude, and longitude, a costal dummy, distance of the main city to the coast, a dummy for the presence of an important river, the average annual rainfalls per district over the 1915-1975 period, the year of colonial conquest's start, the length of local resistance, a dummy for the presence of a European trade counter (which proxies commercial development), the year of the last military intervention before final submission, and a dummy for whether the political power is centralized. Details about these control variables can be found in Appendix D. Table 2 presents some summary statistics about each of these variables in the mobile sample.

[12] For the latter, I myself collected the data (see Appendix C for details).

[13] In fact, when collecting the data at the *Archives Nationales d'Outre-Mer* in Aix-en-Provence, I found in the personal folders of a couple of colonial administrators that despite some negative comments left by the lieutenant-governors, the administrators could still manage to be promoted to the next upper rank.

Table 2: Outcome and control variables: summary statistics.

Variables	mean	std. dev.	min.	max.
Districts' characteristics:				
Number of teachers per 100,000 inhabitants	4.6	10.5	0	157.5
Colony order	3.4	1.6	1	6
District order	0.7	0.3	0.3	1
District population	255854	439036	4444	1824954
Rainfalls (mm)	1262.4	673.6	225	3248
Altitude (feet)	895.9	615.3	0	3044
Latitude	11.2	3	4.8	16.8
Longitude	-7.3	4.7	-17	2.6
Year of the last military intervention before final submission	1900	14	1858	1930
Access to the sea (=1)	0.1	0.4	0	1
Distance of the main city to the coast (km)	497.9	351.4	0	1,300
Presence of an important river (=1)	0.7	0.4	0	1
Year of colonial conquest's start	1880	11.5	1854	1894
Local resistance length (years)	19.5	10.6	0	42
European trade counter (=1)	0	0.2	0	1
Centralized political power (=1)	0.5	0.5	0	1
Acephalous society before the colonial conquest (=1)	0.1	0.3	0	1
Administrators' characteristics:				
Administrators' experience (months)	37.6	29.2	0	157
Rank type	4.8	0.7	1	6
Number of spells	6.5	3.2	1	22
Former pupil of the colonial school (=1)	0.2	0.4	0	1
Administrators' length in the sample (months)	156.4	68.6	4	297
Observations		516		

4.2 District-Administrator Matched Data

I use a recently collected dataset that matches the districts with the administrators that governed them over time. These data come from the official journals of each colony (when available) and cover all the administrator "switches" over the 1906-1929 period (see Appendix C for further details on the data collection). I did not collect the data for the post-1930 colonial period because of a renewal of the colonial administration: from the early 1930s onwards, a majority of the administrators graduated from the colonial school (see Figure A2 in Appendix A), hence the profiles of this new cohort were much less heterogeneous and of better quality overall.

Moreover, the district administrators progressively lost their autonomy and were more and more accountable to the lieutenant governor of their colony as the infrastructures of communication improved significantly during that period (Cohen, 1973). I did not collect the data for Mauritania and Niger either because the official journals for that colony did not exist for the pre-1930 period.

Summary statistics Table 3 presents summary statistics for the district-administrator matched database, distinguishing between the initial dataset ("initial sample") and the dataset using the districts as of 1925[14] and the districts with the largest populations when districts merged before 1925 ("reference sample"). In the initial sample of 4,443 observations, there are 878 administrators and 127 districts.[15] Because not all the official journals were available, I sometimes extrapolated the dates (for 17% of the observations). Removing these extrapolations gives 749 administrators, of which 267 (35%) are reported to be temporarily in charge of a district[16] or lasted for 4 months or less in a given district (because temporary replacements were not systematically reported as such in the official journals) – thereafter, we refer to these administrators as the "temporary administrators". Using the reference sample, the dataset henceforth shrinks to 3,790 observations and contains 788 administrators with 92 districts; and, after excluding the extrapolations, the sample reduces to 683 administrators, among which 248 (36%) are temporary administrators. Panel A displays the statistics about the average stay of administrators. In both samples, an administrator lasted for 11 months on average, and for half of the administrators, the average length of stay is inferior to 11 months. Approximately 8% of the administrators have an average stay that is greater than 2 years (24 months or more) in the initial sample, compared with 8.5% in the reference sample. However, in both samples, the average stay goes up to 15 months after excluding the temporary administrators. Panel B shows the statistics about the average length of spells (that is, the unique district-administrator pairs[17]). In both samples, a spell lasts on average 12 months, and 17 months if we exclude the temporary administrators. Overall, except for the share of temporary administrators, which is significantly greater in the reference sample, these two datasets do not exhibit large differences.

Entries and attrition Given the relatively high administrators' turnover rates across districts, it is important to pay attention to entries and attrition. Figure 2 presents the share of administrators' entries and exits by year in the initial database. First, we note that the observed entries and exits are left-truncated in 1906 and right-truncated in 1929, because I assumed that

[14] I excluded the administrative capitals of Saint-Louis and Dakar from my analyses because as explained in Huillery (2009), these appeared as "direct admnistrated territories" in colonial budgets and received much more investments compared to other districts.

[15] I accounted for the fact that a few districts changed their names but kept the same borders over time (i.e., these districts did not merge or split). I also removed the 7 regions that were reported in the database, because a region (*région*) is different from a district (*cercle*), a region being composed of at least two districts.

[16] These administrators are reported as *commandants de cercle par intérim*, which happens when an administrator leaves a district and that the administrator that was appointed to succeed him has not arrived yet.

[17] If an administrator returns to a district that he had already been in charge of previously, it counts as 2 spells.

(i) the spells that ended at the beginning of the period but for which we do not know the starting date (presumably because the spells started before 1906) began in 1906, and that (ii) likewise, the spells that started at the end of the period but for which we do not know the ending date (presumably because the spells ended after 1929) finished in 1929. The share of entries over the 1906-1929 period is relatively constant: within each year, except 1906-1907, between 10% and 20% of the administrators are new in the initial sample. Similarly, the proportion of exits is relatively stable over time: except for the end of the period, within each year, between roughly 10% and 20% of the administrators exit the sample. We observe slight peaks in 1914 and 1919-1920, presumably due to World War I. The entries and exits do not seem to affect the share of administrators that graduated from the colonial school (ENFOM) given that within each year, the proportion of ENFOM alumni ranges between 10% and 15%, with the exceptions of peaks in 1915 and 1928-1929 (see Figure A1 in Appendix A).

Table 3: Average length of administrators' stays and average spells.

Panel A. Average length of stay of administrators (in months)

	Initial Sample			Reference Sample		
Length	freq.	%	mean	freq.	%	mean
[0,4]	168	22.4	2.0	157	23.0	1.9
(4,12]	264	35.2	8.4	238	34.8	8.4
(12,24]	254	33.9	16.8	230	33.7	17.0
(24,36]	39	5.2	28.5	39	5.7	28.1
(36,53]	24	3.2	50.4	19	2.8	49.5
Total	749	100	11.5	683	100	11.0
... of which temporary	267	35.6	4.9	234	34.2	4.8

Panel B. Average length of spells (in months)

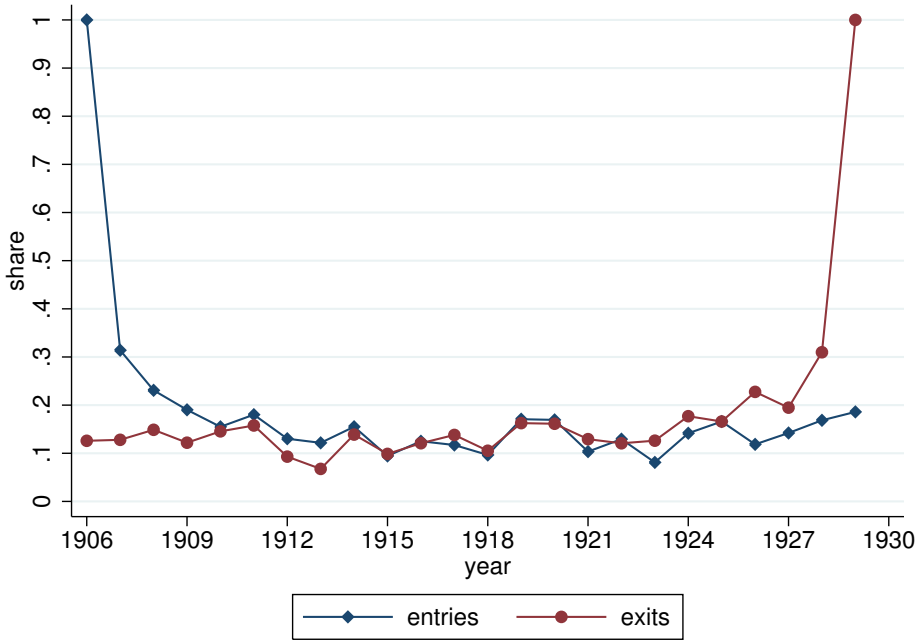
	Initial Sample			Reference Sample		
Length	freq.	%	mean	freq.	%	mean
[0,4]	544	28.5	2.0	459	27.5	2.0
(4,12]	598	31.3	8.2	522	31.2	8.3
(12,24]	528	27.7	18.0	471	28.2	18.1
(24,36]	185	9.7	28.6	166	9.9	28.6
(36,69]	54	2.8	45.5	54	3.2	45.4
Total	1,909	100	12.2	1,672	100	12.5
... of which temporary	618	32.4	2.6	523	31.3	2.6

Notes: The reference sample contains the districts as of 1925. When districts merged before 1925, I kept the district with the largest population as a reference.

Over the 1906-1929 period, an administrator remains on average 6 years in the sample (Table B4 in Appendix B). Roughly half of the administrators appear for less than 3 years while one-

quarter of them appear for more than 10 years. Administrators could quit the sample generally because of assignments to other colonies (not in French West Africa), retirement, severe injuries, or death.[18]

Figure 2: Share of administrators' entries and exits, by year.



Random assignment Whether the administrators were randomly appointed to districts matters for my identification strategy. First, looking at the distribution of switches across months enables us to see if administrators' switches are driven by leaves and the arrival of administrators in French West Africa. In the initial sample, after removing the extrapolations, each month represents 7 to 9% of the total number of switches, except for January (11.5%). Restricting the sample to the long spells (i.e., the spells that last for more than a year), the proportions do not change much (see Table B1 in Appendix B for the details of the distributions). In both cases, the share of switches that happen in January is higher compared to the other months, which might coincide with the end of administrators' leaves. In fact, it seems that a large majority of the switches happen upon the administrators' returns from their leaves or arrivals of new administrators. Out of the 2,342 spells[19], only 360 (15%) concern "immediate transfers" to another district, that is, the administrators moved to another district within 3 months after their last spell (it is therefore unlikely that they returned to mainland France in between). In addition, 317 (13.5%) of the spells correspond to administrators that went back to a district to which they

[18]Some administrators went to North Africa or Asia for example (Association des anciens élèves de l'École nationale de la France d'outre-mer, 2003) while others got severely injured, retired or passed away (these information were sometimes reported in the official journals).

[19]Removing the extrapolations does not alter the result.

had previously been appointed. This result is not so surprising since spells were relatively short.

What could however affect the arguably random allocation of administrators is the proportion of spells that corresponds to an administrator who returned to a district of which he was previously in charge[20]. Specifically, as shown in Table 4, returns are relatively more frequent in Senegal, Dahomey and Ivory Coast (around 14%-17% each) than in Haut-Senegal-Niger, French Guinea, Upper Volta (between 12% and 13% each), and even less frequent in French Sudan (roughly 8%). Out of these spells with returns, in French Guinea, Senegal and Dahomey, at least 50% correspond to spells for which a district experienced at least 5 returns. This share is lower for the other colonies (between 25% and 30%). Hence, a couple of districts in Dahomey, Senegal, and Ivory Coast appear to have been relatively more attractive to administrators, especially in the first two colonies. By contrast, districts in Upper Volta and French Sudan did not experience that many returns on average.

Table 4: Share of returns across colonies.

	Returns[a]		Many Returns[b]	
	freq.	%	freq.	%
Ivory Coast	60	14.7	18	30.0
Dahomey	54	15.7	32	59.3
French Guinea	63	12.0	31	49.2
Haut-Senegal-Niger	57	13.2	18	32.1
Upper Volta	12	12.0	3	25.0
Senegal	57	16.4	29	50.9
French Sudan	14	7.7	4	26.7

[a]The observations in a given colony with at least 1 return of an administrator to a district of which he was previously in charge.

[b]The observations in a given colony with at least 5 returns of administrators that have previously been assigned to a district of that colony.

To investigate if switches across colonies are random, I computed for each colony the share of administrators that are observed in another given colony (Table 5). It appears that switches of administrators between the most "prestigious" colonies (e.g., Senegal or French Guinea) and "less prestigious" ones (e.g., Upper Volta or Dahomey) are not frequent. For example, among all the administrators that are observed in Senegal, less than 7% were also observed in Upper Volta whereas 20% of them are also observed in French Guinea. Moreover, Dahomey seems to be relatively isolated compared to other colonies (column (2)). For instance, only 8% of

[20]In the initial sample, 57 districts experienced returns, among which 22 had considerably more returns than the others (between 5 and 9 returns). See Table B2 in Appendix B for the distribution of districts with returns by colony.

the administrators that were observed in Ivory Coast over the 1906-1929 period were also observed in Dahomey. Even though it was the general governor who decided the assignment of the administrators, in their individual administrative records (see for example Archives Nationales d'Outre-Mer (2015a-e)) they were each year asked whether they would like to remain in their current colony, and if not, to which one(s) they wish to go.[21]

Table 5: Share (%) and number of administrators' switches across colonies.

To →	Ivory Coast	Dahomey	Fr. Guinea	Upper Volta	Senegal	Fr. Sudan
From ↓	(1)	(2)	(3)	(4)	(5)	(6)
Ivory Coast	-	8.1%	12.9%	11.0%	6.2%	17.2%
	(209)	(17)	(27)	(23)	(13)	(36)
Dahomey	13.4%	-	17.3 %	8.7%	12.6%	18.1%
	(17)	(127)	(22)	(11)	(16)	(23)
French Guinea	13.2%	10.8%	-	11.8%	13.2%	19.1%
	(27)	(22)	(204)	(24)	(27)	(39)
Upper Volta	16.8%	8.0%	17.5%	-	6.6%	34.3%
	(23)	(11)	(24)	(137)	(9)	(47)
Senegal	9.7%	11.9%	20.1%	6.7%	-	23.9%
	(13)	(16)	(27)	(9)	(134)	(32)
French Sudan	13.3%	8.5%	14.4%	17.4%	11.9%	-
	(36)	(23)	(39)	(47)	(32)	(270)

Notes: Parentheses contain the corresponding number of administrators. Column 1 row 2 should be read as follows: among all the administrators that were observed in Dahomey during the 1906-1929 period, 13.4% were also observed in Ivory Coast.

Table 6: Share (%) of administrators' next move across colonies.

To →	Ivory Coast	Dahomey	Guinea	Upper Volta	Senegal	Sudan	Leave
From ↓	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Ivory Coast	0.41	0.05	0.16	0.06	0.08	0.16	0.09
Dahomey	0.08	0.43	0.14	0.09	0.09	0.09	0.08
French Guinea	0.13	0.10	0.39	0.07	0.11	0.13	0.07
Upper Volta	0.11	0.09	0.14	0.16	0.03	0.36	0.11
Senegal	0.08	0.09	0.16	0.05	0.35	0.21	0.06
French Sudan	0.11	0.04	0.13	0.18	0.12	0.33	0.08

Furthermore, when an administrator returns to French West Africa at the end of a leave, he

[21] I could not directly exploit these information in my analyses due to time and budget constraints in collecting the relevant data from the administrative records of all the administrators at ANOM (Aix-en-Provence).

usually replaces someone whose rank[22] is inferior to his. Looking at the immediate next colony destination of the administrators in a given colony (Table 6), we note that a majority of the next moves correspond to a return in the colony. For example, among the administrators observed in Ivory Coast, Dahomey and French Guinea, around 40% of their next spell turn out to be in their initial colony. Supporting the idea that cross-colony switches happen between colonies of the same "prestige," only 7% and 5% of the administrators observed respectively in French Guinea and Senegal experience their next spell in Upper Volta; and 11% of them quit the sample. Conversely, only 3% of the administrators observed Upper Volta manage to go to Senegal for their next spell.

All in all, it is therefore unlikely that the allocation of administrators across colonies was completely random. Nevertheless, except for a couple of districts, it seems that the allocation across the districts within a same colony was fairly random.

Sample selection Given that I select a subsample of administrators for my main analysis (namely, the "mobile sample", which contains only the administrators who were observed in at least two different districts), one may be concerned about the generalizability of the results that I obtain. Indeed, it may be that the administrators who are present in the mobile sample exhibit systematically different characteristics from those in the initial or reference samples. Table 7 presents some statistics to compare the three different samples. The mobile sample contains many less observations, it is thus not surprising that the number of spells is also greatly reduced compared to the two other samples. In the mobile sample, there are 129 administrators and 83 districts. The differences between the mobile sample and the two others in terms of the average number of spells by administrator, the average length of spells, the average number of colonies by administrator, the average number of districts by administrator, the average number of administrators by district, and the average share of movers by district are statistically different from zero at the 5% significance level. Only the average administrators' presence in the sample are not significantly different from one another across the three samples. Looking at the average length of spells by period (after excluding the extrapolations), the early period (1910-1914) exhibits relatively shorter spell length in all samples. Focusing on the initial sample, the average length of spells increased significantly from 10 months to 13 months in the following periods. This could be explained by the fact that colonization was still at its beginning so the colonial administration may have encountered some difficulties in terms of organization. We observe a similar pattern for the reference sample and the mobile sample, although in the latter the spell lengths are longer because the temporary administrators have been excluded.

[22]Colonial administrators had different ranks depending on their seniority. Table B3 in Appendix B provides a summary of the different ranks.

Table 7: Sample comparisons.

	Initial	Reference	Mobile	*p*-values		
	(1)	(2)	(3)	(1)-(2)	(1)-(3)	(2)-(3)
Nb of observations	4,443	3,789	516	-	-	-
Nb of spells	2,342	2,043	357	-	-	-
Nb of admin.	878	788	129	-	-	-
Nb of movers	436	404	129	-	-	-
Nb of districts	127	92	83	-	-	-
Nb of admin. by period:						
... 1910-1914	319	292	83	-	-	-
... 1915-1919	456	261	76	-	-	-
... 1920-1928	502	461	75	-	-	-
Avg nb of spells by admin.	5.0	4.6	3.2	0.00	0.00	0.00
	(3.64)	(3.19)	(1.33)			
Avg nb of colonies by admin.	1.4	1.3	1.5	0.14	0.29	0.05
	(0.71)	(0.65)	(0.65)			
Avg nb of districts by admin.	2.3	2.3	2.6	0.45	0.16	0.04
	(1.97)	(1.77)	(0.86)			
Avg admin.'s presence (in years)	5.9	5.6	5.8	0.46	0.91	0.75
	(6.12)	(6.00)	(3.65)			
Avg share of movers per district	0.78	0.78	1.00	1.00	0.00	0.00
	(0.19)	(0.17)	(0.00)			
Avg nb of admin. by districts	16.1	19.3	4.0	0.00	0.00	0.00
	(7.39)	(4.27)	(2.03)			
Avg length of spells (in months)	16.7	16.8	19.3	0.80	0.00	0.00
	(9.77)	(9.83)	(10.86)			
... 1910-1914 period	10.6	10.7	15.8	0.90	0.00	0.00
	(9.39)	(9.39)	(9.64)			
... 1915-1919 period	13.0	14.0	20.5	0.99	0.00	0.00
	(12.84)	(13.60)	(13.73)			
... 1920-1928 period	13.13	13.08	19.45	0.92	0.00	0.00
	(10.28)	(10.04)	(8.08)			

Notes: the last three columns display the *p*-values for the test the null hypothesis that the differences are significantly different from zero. In the first three columns, the parentheses contain standard deviations. Here, I did not exclude the extrapolations in the initial and reference samples, which explains why the numbers differ from Table 3.

5 Empirical Strategy

To investigate the influence of colonial administrators on public educational investments, I employ an empirical method that is similar to the one developed in Bertrand and Schoar (2003) and to the mover dummy variable method in Graham, Li, and Qiu (2013). I do not use the spell method, which consists of including dummies for each administrator-district pair, because it does not enable the separate identification of district fixed effects and administrator fixed effects, whereas estimating the administrator fixed effects and measuring how much of the variation in public educational investments is due to these effects is the main goal of this paper. I do not use the AKM method either because my "connected sample" contains a large proportion of stayers (around 60%), which could bias the estimates of the fixed effects that I am interested in. Furthermore, the AKM method requires a very large dataset – it is usually applied on at least hundreds of thousands of observations – while I only have hundreds of observations.

5.1 Fixed-Effects Methods

Theoretically, we could consider the following linear three-way fixed-effects model to assess the role of administrators on district-level public investments in education:

$$y_{adt} = X_{at}\beta + W_{dt}\gamma + Z_a\zeta + Q_d\rho + \theta_a + \phi_d + \tau_t + \epsilon_{adt} \tag{1}$$

The dependent variable y_{adt} is the public educational investments as proxied by the number of teachers per 100,000 inhabitants in district $d = 1, ..., D$ at time $t = 1, ..., T$ and decided by administrator $a = 1, ..., N$. On the right-hand side, X_{at} is a vector of observable administrator-level time-variant covariates, which includes the administrators' rank types and their past experience. W_{dt} represents a vector of observable district-level time-variant covariates and includes colonies' and districts' orderings (which can serve as proxies for their relative importance). The vector Z_a corresponds to a set of observable administrator-specific time-constant control variables, such as the number of years during which an administrator is observed in the sample and whether the administrator graduated from the colonial school. The vector Q_d contains district-level time-constant covariates, and it includes geographical and precolonial variables (latitude, longitude, altitude, coastal dummy, a dummy for the presence of a European trade counter, distance to the coast, the length of local resistance, and a dummy for whether the political power is centralized). Finally, θ_a, ϕ_d and τ_t are the three error components which capture the unobserved heterogeneities for administrators, districts, and time, respectively, and which can be correlated with one another but also with any of the observable covariates; and ϵ_{adt} is an erratum which is assumed to be strictly exogenous, that is, $E[\epsilon_{adt}|X_{at},\ W_{dt},\ Z_a,\ Q_d,\ \theta_a,\ \phi_d,\ \tau_t] = 0$. This strict exogeneity assumption notably implies that administrators' allocations are independent of ϵ_{adt}, which I discuss further below. Since θ_a and ϕ_d are likely to be correlated with the observables, fixed-effects methods are more appropriate than random-effects ones. Let us therefore

define $\alpha_a \equiv \theta_a + Z_a\zeta$ and $\delta_d \equiv \phi_d + Q_d\rho$ to capture both the unobservable and observable time-invariant variables that are specific respectively to the administrators and to the districts. Then equation (1) can be rewritten as follows:

$$y_{adt} = X_{at}\beta + W_{dt}\gamma + \alpha_a + \delta_d + \tau_t + \epsilon_{adt} \tag{2}$$

I use equation (2) as one of my main specifications and estimate it with least-square dummy-variable (LSDV) methods, that is, by including a set of dummy variables for each group of fixed effects, which is feasible because the dataset is not so large. This three-way fixed-effects specification has the advantage of reducing considerably the threat of omitted-variable bias and even ruling it out as long as the omitted variables are time-invariant, so the strict exogeneity assumption conditional on the observables, i.e. $E[\epsilon_{adt}|X_{at}, W_{dt}, \alpha_a, \delta_d, \tau_t] = 0$, seems satisfied. However, a drawback with this fixed-effects model is that it does not allow for the inclusion of any observable time-constant covariates (such as a dummy for whether an administrator graduated from the colonial school), since these are already captured in the fixed effects. But here it is not an issue because I am not interested in estimating the effects of specific time-invariant observable characteristics. Another potentially significant caveat of this model is that it is impossible to assess the effect of variables that have little within-group variation. In my case, with both time and district fixed effects, there may not be enough district-time variation left. This is why I use as a second main specification a two-way fixed-effects model. Specifically, I replace the district fixed effects by (observable) time-constant district-level control variables, which gives the following specification:

$$y_{adt} = X_{at}\beta + W_{dt}\gamma + Q_d\rho + \alpha_a + \tau_t + \nu_{adt} \tag{3}$$

where $\nu_{adt} \equiv \phi_d + \epsilon_{adt}$. I also estimate equation (3) with the LSDV method. Here again, I assume that the exogeneity assumption holds: $E[\nu_{adt}|X_{at}, W_{dt}, Q_d, \alpha_a, \tau_t] = 0$. Nonetheless, removing the district fixed effects comes at a price: the threat of omitted-variable bias is now greater, unless the district-level time-invariant covariates that replace them reduce this bias to zero. But, if the allocation of colonial administrators across districts is completely random, then the results from specifications (2) and (3) should not be significantly different from each other.

Even though each model has it weaknesses, we could view the three-way fixed-effects model (specification (2)) as providing a lower bound of the effects of administrators on educational investments and the two-way fixed-effects model (specification (3)) as providing an upper bound of these effects.

5.2 Identification and Test Strategies

In the two main specifications, under the exogeneity assumption, the LSDV parameter estimates β, γ, ρ and τ_t are consistent, unbiased, and efficient with homoskedastic and serially uncorrelated

random errors (Wooldridge, 2010). However, the LSDV estimators of α_a and δ_d are inconsistent, although best linear unbiased. Intuitively, adding a new cross-section of observations implies the inclusion of another fixed effect and not necessarily more information since there is an extra parameter to estimate.[23] In fact, in both specifications (2) and (3), the administrator effects α_a cannot be estimated precisely with relatively few periods per administrator, whereas in the three-way fixed-effects model (specification (2)), the district effects δ_d can be estimated precisely if districts have enough administrators who join or leave (Andrews, Schank, and Upward, 2006).

Estimating the fixed effects for the administrators that have been observed only once is impossible because the administrator effects α_a coincide with the district effects δ_d (i.e., they are perfectly collinear), so I could estimate at best only the sum of these two effects for these administrators: $\omega_{a,d} \equiv \alpha_a + \delta_d$. This is why I restricted the sample to the administrators that have been observed in at least two different districts (i.e. the "movers"). By doing so, I can separately identify the administrator fixed effects and the district fixed effects. However, as discussed in the previous section, selecting only the movers may affect the generalizability of the results obtained with this subsample. Moreover, the magnitudes of the administrator or district fixed effects can be arbitrary, and only the difference of these fixed effects can be identified. For example, we can get the difference between administrator 1 and administrator 2 by substracting $\omega_{1,d}$ from $\omega_{2,d}$, which gives: $\omega_{1,d} - \omega_{2,d} = (\alpha_1 + \delta_d) - (\alpha_2 + \delta_d) = \alpha_1 - \alpha_2$. Likewise, we get the difference between district 1 and district 2's fixed effects, $\delta_2 - \delta_1$, by substracting $\omega_{a,1}$ from $\omega_{a,2}$. Then, we can compare all the administrator fixed effects (so long as the administrators are observed in two different districts). However, the values of α_a and δ_d are not unique as we could, for example, add 10 to δ_d for all d and substract 10 from α_a for all a, and still leave $\omega_{a,d} = \alpha_a + \delta_d$ unchanged.

To examine whether administrators mattered for public investments in education, as in Bertrand and Schoar (2003), Graham et al. (2012), or Yao and Zhang (2015) among others, I first look at the share of the R-squared that is explained by the different components of the regressions. This enables us to know how much each of these components contributes to the total variation in district-level public educational investments. The R-squared of specification (2) can be computed and decomposed as follows:

$$\begin{aligned} {R^2}_{(2)} &= \frac{cov(y_{adt}, \hat{y}_{adt})}{var(y_{adt})} = \frac{cov(y_{adt}, X_{at}\hat{\beta} + W_{dt}\hat{\gamma} + \hat{\alpha}_a + \hat{\delta}_d + \hat{\tau}_t + \hat{\epsilon}_{adt})}{var(y_{adt})} \\ {R^2}_{(2)} &= \frac{cov(y_{adt}, X_{at}\hat{\beta})}{var(y_{adt})} + \frac{cov(y_{adt}, W_{dt}\hat{\gamma})}{var(y_{adt})} + \frac{cov(y_{adt}, \hat{\alpha}_a)}{var(y_{adt})} + \frac{cov(y_{adt}, \hat{\delta}_d)}{var(y_{adt})} \\ &\quad + \frac{cov(y_{adt}, \hat{\tau}_t)}{var(y_{adt})} + \frac{cov(y_{adt}, \hat{\epsilon}_{adt})}{var(y_{adt})} \end{aligned} \tag{4}$$

[23] Nevertheless, the estimates of these two groups of fixed effects are consistent if and only if the number of time periods goes to infinity (Hsiao, 2002), but this condition is not satisfied in the panel data that I use.

Likewise, the R-squared of specification (3) is calculated as follows:

$$
\begin{aligned}
R^2{}_{(3)} &= \frac{cov(y_{adt}, \hat{y}_{adt})}{var(y_{adt})} = \frac{cov(y_{adt}, X_{at}\hat{\beta} + W_{dt}\hat{\gamma} + Q_d\hat{\rho} + \hat{\alpha}_a + \hat{\tau}_t + \hat{\nu}_{adt})}{var(y_{adt})} \\
R^2{}_{(3)} &= \frac{cov(y_{adt}, X_{at}\hat{\beta})}{var(y_{adt})} + \frac{cov(y_{adt}, W_{dt}\hat{\gamma})}{var(y_{adt})} + \frac{cov(y_{adt}, \hat{\alpha}_a)}{var(y_{adt})} + \frac{cov(y_{adt}, Q_d\hat{\rho})}{var(y_{adt})} \\
&\quad + \frac{cov(y_{adt}, \hat{\tau}_t)}{var(y_{adt})} + \frac{cov(y_{adt}, \hat{\nu}_{adt})}{var(y_{adt})} \qquad (5)
\end{aligned}
$$

The third term in equation (4) and equation (5) tells us how much of the variation in educational investments is due to the administrator fixed effects.

Secondly, I perform an F test to examine the joint significance of the administrator fixed effects. More precisely, I test the null hypothesis that the administrator fixed effects α_a are jointly and significantly different from zero. A failure to reject the null hypothesis would mean that administrators do not matter for public educational investments.

Lastly, setting the mean of α_a to zero (although I keep α_a to denote the time-demeaned fixed effects for the sake of notation simplicity), I test the null hypothesis that the normalized administrator fixed effects α_a are jointly and significantly different from zero (that is, they are different from the average effects). A failure to reject the null does not necessarily mean that administrators do not matter, because administrators in different districts may perform differently. In fact, it could rather mean that administrators are equally capable, since I estimate differences among administrators rather than absolute values of the administrator effects.

6 Empirical Results

6.1 Baseline Results

Table 8 displays the results from the three-way and the two-way fixed-effects regressions. In particular, it notably reports the p-value corresponding to the F test of the joint significance of the administrator fixed effects (under the null hypothesis that $\sum \alpha_a = 0$), the R-squared of the regressions, the contribution of the administrator fixed effects to the R-squared, and the contribution of the district fixed effects to the R-squared in the three-way fixed-effects estimations.

Unsurprisingly, the variation in the public educational investments that is explained by the model is higher in the three-way fixed-effects estimations (R-squared $\simeq 0.92$) than in the two-way fixed-effects estimations (R-squared $\simeq 0.66$). This is because the former ones control for any district-specific characteristics that are constant over time, whereas in the latter only the observable district-specific time-invariant variables for which data are available can be controlled for (column (8)). The differences in the R-squared between the two types of estimation methods may suggest that the unobservable time-constant district characteristics play an important role in explaining the variation in educational investments. In fact, in the three-way fixed-effects

estimations (columns (1)-(4)), the district fixed effects explain a very large proportion of the variance in educational investments. Even when both time-variant administrator- and district-specific characteristics are included in the specification (column (4)), the district fixed effects still explain 86% of the variance in the outcome variable. By contrast, the administrator fixed effects explain at most 2% of the variation in public investments in education, and this share shrinks to 1% once time-variant district-specific characteristics are controlled for (columns (3) and (4)). Moreover, the F tests reject the null hypothesis that the administrator effects are jointly different from zero with a large margin. Taken together, these results suggest that the administrators matter but only a little when it comes to explain the level of educational investments.

Table 8: LSDV Regression Results: 3-way FE vs. 2-way FE methods.

Estimation technique →	3-way FE method				2-way FE method			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
p-value for $\sum \alpha_a = 0$	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
R-squared	0.925	0.925	0.927	0.927	0.656	0.666	0.663	0.675
Share of R^2 due to $\hat{\alpha}_a$	0.019	0.017	0.007	0.006	0.216	0.174	0.190	0.141
Share of R^2 due to $\hat{\delta}_d$	0.901	0.899	0.868	0.866	-	-	-	-
Corr($\hat{\delta}_d$, $\hat{\alpha}_a$)	-0.19*	-0.20*	-0.21*	-0.21*	-	-	-	-
Admin. time-variant var.	No	Yes	No	Yes	No	Yes	No	Yes
District time-variant var.	No	No	Yes	Yes	No	No	Yes	Yes
District time-constant var.	No	No	No	No	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	No	No	No	No
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Admin. FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	516	516	516	516	516	516	516	516

Notes: The dependent variable is the number of teachers per 100,000 inhabitants per district. The results shown are p-values, except for the row Corr($\hat{\delta}_d$, $\hat{\alpha}_a$), which corresponds to the correlation between the estimated administrator FE and district FE; the presence of a star indicates that the correlation is statistically significant at the 1% level. Heteroskedasticity is accounted for with robust standard errors.

Turning to the two-way fixed-effects estimations (columns (5)-(8)), as noted above, the models explain less variation in the dependent variable compared to the three-way fixed-effects models, although the R-squared is still reasonably high (around 0.66). When district fixed effects are replaced by district-specific time-invariant control variables and when no time-variant controls are added, the administrator FE now explain approximately 21% of the R-squared (column (5)). Once time-variant administrator-specific characteristics are controlled for, the proportion of the

R-squared that is explained by the administrator FE reduces to 17% (column (6)) whereas when instead time-variant district-specific characteristics are controlled for, this proportion reaches 19% (column (7)). When both types of time-variant covariates are included in the regression, this share drops to 14% while the R-squared increases only by one percentage point.

Last, an F test to examine the joint significance of the normalized administrator FE[24] in the three-way fixed-effects regressions yields very high p-values for all specifications (results not shown), so it clearly fail to reject the null hypothesis that the (normalized) administrator effects are jointly different from zero. Hence, the administrators of a given district are, on average, not different from the typical administrator of that district regarding the decisions related to public investments in education. It therefore seems that administrators do not systematically take different decisions when it comes to investing in education. This could potentially suggest a path dependence, that is, the colonial administrators of a given district tend to invest the same amount in education as their predecessors.

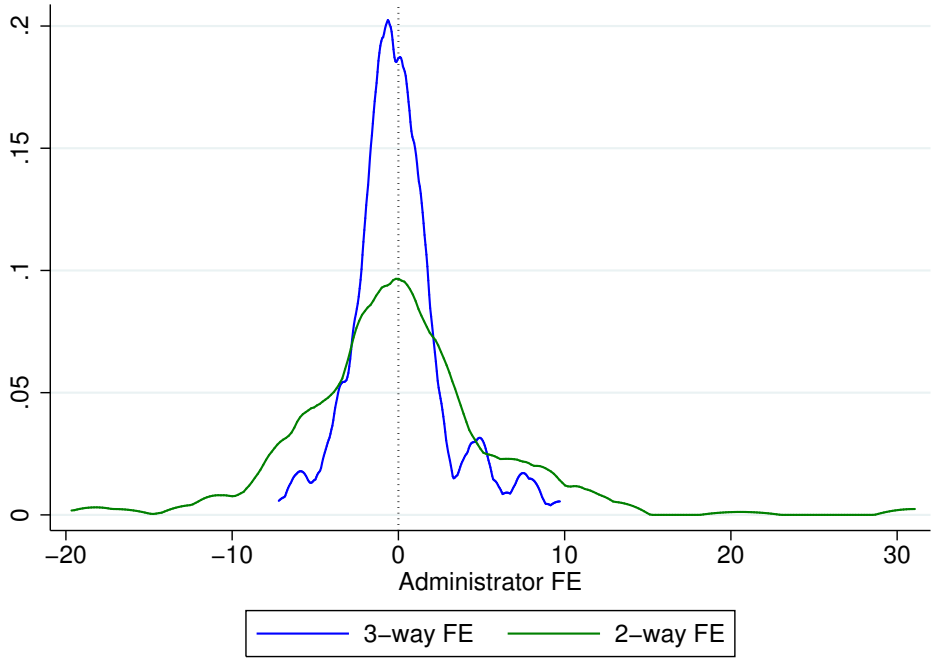
Figure 3 draws the distribution of the normalized administrator fixed effects obtained from the two types of fixed-effects estimations with the inclusion of all the time-variant control variables. The blue line represents the distribution of the demeaned administrator FE from the three-way fixed-effects model while the green line displays that of the two-way fixed-effects one. The two distributions are both bell-shaped and centred around zero, but that from the two-way fixed-effects estimation is less peaked and the values taken by the administrator FE are more dispersed, with fatter tails.

Taken together, these results suggest that the district effects are much more important than the administrator effects in explaining the variation in the level of public educational investments, as indicated by the share of the R-squared that is due to district FE in the three-way fixed-effects regressions and also by the difference in the R-squared between the two types of specifications. Overall, although district administrators matter when it comes to determine the level of investments in education, their time-invariant characteristics, such as their education level or their personality, do not seem important in explaining the variation in public educational investments. Indeed, the share of the R-squared that is explained by the administrator fixed effects ranges from 1% to 22% depending on the specification considered. Moreover, the fact that the contribution of the administrator FE to the R-squared differs significantly depending on the type of fixed effects methods used suggests that the allocation of administrators across districts is not completely random. In fact, the districts effects appear to be important, given that the estimated administrator fixed effects and district effects are strongly correlated in the three-way fixed-effects specifications (row 5 of Table 8) and that the administrator FE explain much more variation in the outcome variable once the district FE are removed (i.e., in the two-way fixed-effects specifications). These results could support the hypothesis that district-level public investments in education are path-dependent and were determined mostly by the very

[24]This is done directly with the Stata command -felsdvreg- in order to get the correct clustered standard errors for the administrator fixed effects (Cornelissen, 2008).

first district administrators of the colonial period.

Figure 3: Distribution of administrator FE: 2-way vs 3-way FE estimations.



6.2 Heterogeneous Administrator Effects

I further investigate the potential heterogeneous effects of administrators by following Bertrand and Schoar (2003) and splitting the administrator fixed effects into (collectively exhaustive and mutually exclusive) subgroups of administrator fixed effects.

Administrators' rank I first separately study these effects by creating groups of fixed effects depending on the administrators' rank type (see Table B3 in Appendix B).
Redefining $\alpha_a \equiv \alpha_{chief} + \alpha_{admin} + \alpha_{assistant} + \alpha_{military} + \alpha_{CS} + \alpha_{IA}$ and substituting it in specifications (2) and (3) respectively yields:

$$y_{adt} = X_{at}\beta + W_{dt}\gamma + \alpha_{chief} + \alpha_{admin} + \alpha_{assistant} + \alpha_{military} + \alpha_{CS} + \alpha_{IA} + \delta_d + \tau_t + \epsilon_{adt} \quad (6)$$

$$y_{adt} = X_{at}\beta + W_{dt}\gamma + \alpha_{chief} + \alpha_{admin} + \alpha_{assistant} + \alpha_{military} + \alpha_{CS} + \alpha_{IA} + Q_d\rho + \tau_t + \nu_{adt} \quad (7)$$

where α_{rank} are fixed effects for the group of administrators who are classified as *rank* in the last spell we observed them in. The rank type *rank* refers to *chief* for Chief Administrators, *admin* for Administrators, *assistant* for the Assistant Administrators, *military* for the Military Men, CS for the administrators who belong to the Personnel of Civil Services, and IA for those who belong to the Personnel of Indigenous Affairs.

Table 9: Empirical Results: Heterogeneous Administrator Effects

Estimation technique →		3-way FE method				2-way FE method			
	N	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. Administrators' rank:									
- Chief Administrator	17	0.114	0.120	0.163	0.166	0.320	0.396	0.310	0.360
- Administrator	88	0.000	0.000	0.000	0.001	0.000	0.000	0.000	0.007
- Assistant Administrator	17	0.297	0.404	0.477	0.503	0.024	0.123	0.030	0.099
- Military Men	5	0.008	0.004	0.012	0.017	0.108	0.034	0.033	0.020
B. Administrators' cohort:									
- Earliest (1906-1909)	71	0.002	0.001	0.032	0.036	0.000	0.001	0.001	0.148
- Early (1910-1914)	41	0.127	0.068	0.400	0.380	0.000	0.005	0.000	0.071
- Middle (1915-1919)	13	0.420	0.385	0.125	0.117	0.040	0.005	0.010	0.002
- Late (1920-1928)	2	0.156	0.124	0.211	0.215	0.271	0.009	0.418	0.729
C. Administrators' returns:									
- With returns	51	0.004	0.006	0.021	0.028	0.000	0.000	0.000	0.002
- No returns	76	0.000	0.004	0.019	0.022	0.002	0.194	0.063	0.335
D. ENFOM alumni:									
- ENFOM alumni	19	0.074	0.162	0.137	0.183	0.001	0.000	0.010	0.008
- Not ENFOM alumni	108	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.001
Admin. time-variant var.		No	Yes	No	Yes	No	Yes	No	Yes
District time-variant var.		No	No	Yes	Yes	No	No	Yes	Yes
District time-constant var.		No	No	No	No	Yes	Yes	Yes	Yes
District FE		Yes	Yes	Yes	Yes	No	No	No	No
Year FE		Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Administrator FE		Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations		516	516	516	516	516	516	516	516

Notes: The dependent variable is the number of teachers per 100,000 inhabitants per district. The results display only the p-value for the F test of the joint significance of a given group of administrators. Heteroskedasticity is accounted for with robust standard errors. In Panel A, there are no observations for the subgroups of administrators who were lastly observed as belonging to the Personnel of Civil Services or the Personnel of Indigenous Affairs.

Panel A of Table 9 presents the p-values for the F test of the joint significance of these subgroups of administrator fixed effects, distinguishing between the estimations with the three-way fixed-effects method (Columns (1)-(4)) and those with the two-way fixed-effects method (Columns (5)-(8)). Quite surprisingly, in both types of regressions, regardless of the covariates that are included, the test for the joint significance of the administrators who are lastly observed as being a Chief Administrator fails to reject the null hypothesis that this subgroup of fixed effects is statistically and significantly different from zero. Since there are not so many of these administrators in this analysis sample ($N = 17$), it is difficult to tell whether this result is true for all the district administrators who were promoted to the rank of Chief Administrator (in the initial sample). By contrast, the F test rejects the null at the 1% significance level for the subgroup of Administrators. The joint significance test for the subgroup of Military Men is also rejected, at the 5% level, which could be explained by the fact that these district administrators were sent to manage either military districts or districts with a higher degree of hostility, so their role may have been more decisive. The fixed effects for the subgroup of Assistant Administrators are not jointly and significantly different from zero at the 10% level (only when time-variant administrator-specific characteristics are controlled for in the two-way FE estimations), suggesting that they did not matter, presumably because they were at the beginning of their career and tended to follow the investment decisions that had previously been made.

Administrators' cohort Similarly, I now perform the decomposition of administrator fixed effects based on their cohorts. I distinguish between four cohorts: the earliest cohort refers to the administrators who were firstly observed in the 1906-1909 period (in the initial sample), the early cohort corresponds to the administrators who were firstly observed in the 1910-1914 period, the middle cohort to those who were firstly observed in the 1915-1919 period, and the late cohort to those who were firstly observed in the 1920-1928 period. Redefining $\alpha_a \equiv \alpha_{earliest} + \alpha_{early} + \alpha_{middle} + \alpha_{late}$, specifications (2) and (3) then rewrite as follows:

$$y_{adt} = X_{at}\beta + W_{dt}\gamma + \alpha_{earliest} + \alpha_{early} + \alpha_{middle} + \alpha_{late} + \delta_d + \tau_t + \epsilon_{adt} \tag{8}$$

$$y_{adt} = X_{at}\beta + W_{dt}\gamma + \alpha_{earliest} + \alpha_{early} + \alpha_{middle} + \alpha_{late} + Q_d\rho + \tau_t + \nu_{adt} \tag{9}$$

where α_{period} ($period \in \{earliest,\ early,\ middle,\ late\}$) are the fixed effects for the group of administrators who were firstly observed in a given period.

Panel B of Table 9 reports the results from equations (8) and (9). Interestingly, the joint significance test for the group of administrators who were firstly observed in the 1910-1914 period rejects the null that the fixed effects for this group are significantly different from zero at the 5% significance level. This result is valid even for the three-way fixed-effects one with the time-variant covariates (column (4)), but not for the two-way fixed-effects estimation with these covariates (column (8)). This could mean that there are unobservable time-constant district-specific characteristics that I could not control for and that are yet important to explain the

influence of the earliest administrators on decisions regarding investments in education. Adopting a more conservative approach, I now focus on the three-way fixed-effects estimation results. In contrast to the results for the earliest cohort, the fixed effects for each of the other cohorts are jointly and significantly not different from zero. These results are in line with the hypothesis of path dependence, that is, the fact that the very first colonial administrators' investments were decisive in setting the path of investments for the administrators that arrived afterwards.

Administrators' returns I also decompose the administrator fixed effects into those who returned at least once to a district they had been allocated to and those who did not. Formally, I redefine $\alpha_a \equiv \alpha_{returned} + \alpha_{noreturn}$, and substitute it in equations (2) and (3).

$$y_{adt} = X_{at}\beta + W_{dt}\gamma + \alpha_{returned} + \alpha_{noreturn} + \delta_d + \tau_t + \epsilon_{adt} \tag{10}$$

$$y_{adt} = X_{at}\beta + W_{dt}\gamma + \alpha_{returned} + \alpha_{noreturn} + Q_d\rho + \tau_t + \nu_{adt} \tag{11}$$

Panel C of Table 9 presents the p-values of the F test for the joint significance of the fixed effects of these subgroups. The joint significance of each of these subgroups passes the F test at the 5% level. Hence, the heterogeneous effects of the administrator fixed effects cannot be disentangled by distinguishing between those who returned to their districts at least once and those who did not.

Colonial school's alumni I apply the same procedure by distinguishing the administrators who graduated from the colonial school (α_{ENFOM}) and those who did not ($\alpha_{notENFOM}$). Here again, substituting $\alpha_a \equiv \alpha_{ENFOM} + \alpha_{notENFOM}$ into equations (2) and (3) gives:

$$y_{adt} = X_{at}\beta + W_{dt}\gamma + \alpha_{ENFOM} + \alpha_{notENFOM} + \delta_d + \tau_t + \epsilon_{adt} \tag{12}$$

$$y_{adt} = X_{at}\beta + W_{dt}\gamma + \alpha_{ENFOM} + \alpha_{notENFOM} + Q_d\rho + \tau_t + \nu_{adt} \tag{13}$$

Panel D of Table 9 reports the p-values for the joint significance of each of these two subgroups of administrator FE. Regardless of the specifications considered, the fixed effects for the group of administrators that did not attend the colonial school are jointly and significantly different from zero. This result is not so surprising if we consider the fact that the colonial school did not send many administrators to West Africa prior to 1930 (see Figure A2 in Appendix A) because it was not very well-developed. In fact, it is only from 1926 onwards (with the arrival of a new school director) that the colonial school was deeply restructured, with a more selective entrance examination, the recruitment of new professors, and a greater focus on the reality of the colonial administration. The alumni of the colonial school were therefore better trained to become colonial district administrators (Cohen, 1973). Hence, prior to the 1930s, which is the period we consider here, there were more heterogeneities among district administrators, and these

may explain the rejection of the null hypothesis for the joint significance test for that subgroup.[25] The results for the F test on the subgroup of the colonial school's alumni are more contrasted: the null hypothesis is rejected at the 5% significance level in the two-way FE estimations, but not in the three-way FE. Here again, this suggests that district effects play an important role in explaining public educational investments. It may be that the colonial administrators who graduated from the colonial school were assigned to districts in which they could express their qualities and skills. This could explain why once the region fixed effects are removed, the fixed effects of ENFOM alumni become jointly and significantly different from zero.

7 Robustness Checks

To test the randomness of attrition in the mobile sample that I use for my main analysis, I follow Yao and Zhang (2011) and create a dummy variable indicating whether administrator a leaves the sample in year t. I then check the correlation between that dummy and the residuals obtained from the estimations of equations (2) and (3). Regardless of the type of estimation used and the covariates that are included in the regression, the correlation is not statistically significant at any conventional significance level (results not displayed), which suggests that attrition is orthogonal to the error term.

To examine the robustness of the main results I obtained, I normalize the number of teachers per district by the population of the corresponding district as of 1925. Ideally, I would have normalized by the population of a year prior to the start of the French colonization, because population dynamics could have been driven by the colonial policy, which differed not only across colonies but also across districts within the same colony. Unfortunately, I cannot do so due to data limitations. As a matter of fact, 1925 is the only year for which the data for the district population exists for all districts. Nonetheless, it could still be interesting to see whether the results differ since almost two-thirds of the values for the outcome variable in my main analysis correspond to extrapolations. The associated mobile sample now contains 582 observations, 144 administrators, and 83 districts. The empirical results from the three-way and the two-way FE estimations on this sample exhibit similar patterns as those from the main analysis (see Table B5 in Appendix B).

As another robustness check, I remove the districts that experienced a lot of returns of former administrators. These frequent returns could indicate that those districts were for some reasons more attractive (perhaps because they were more prosperous and/or more easily manageable), so they may drive some of the results obtained in the main analysis. Table 10 reports the results when excluding the districts with many returns (i.e., 5 or more). We first note that the joint significance test on the administrator FE is again passed, regardless of the specification considered. However, in the three-way FE estimations, even though the R-squared has decreased

[25] Rejection of the null hypothesis in an F test tells us that the effects are jointly different from zero, but it does not exclude the possibility that a subset of these effects are jointly not different from zero.

by approximately 5 percentage points compared to the main results, interestingly, the share explained by the administrator FE is now much greater and ranges from 8% to 11.5% depending on the specification considered, while it amounted to less than 2% in the main results. By contrast, now the share of the R-squared due to district fixed effects is much lower, albeit still high. It reduces to approximately 58% once the time-variant characteristics of the administrators and districts have been controlled for. In the two-way FE estimations, the R-squared is also greater than that in the main results (0.80 vs 0.66), thus reducing the gap in the model fit between the two types of estimation methods. Taken together, these results suggest that the districts with many returns partly drove the main results. There seems to be some (self-)selection of administrators into these districts. Nevertheless, excluding these districts does not seem to fully account for the non-random allocation of district administrators since the contribution of the administrator FE to the R-squared is still considerably higher (roughly as twice as high) compared to the three-way FE estimations. This is confirmed by the significance at the 1% level of the correlation between the estimated coefficients of the administrator FE and the district FE in the three-way FE regressions.

A last robustness test consists in running the analysis on other subsamples. The mobile sample that I have used in my analyses so far contains the very last administrators who were present in a given district each year, since local budgets were decided in December. However, switches could sometimes happen in December, which means that there may have been two administrators in a given district-year observation. I therefore create other samples of analysis in which I select the other administrator who was present in December in case only one switch happened in December, and the first administrator of December when three administrators were observed for a given district-year combination. Let us name these samples S1, S2, S3, respectively. In addition, I create another sample, S4, which contains the observations for which there is only one administrator observed in December (i.e., in this sample, I dropped all those for which there are at least two administrators observed for a given district-year combination). I add the suffix "pop" to refer to the subsamples when selecting the districts with the largest population in case two districts merged before 1925, the year of reference that I use to keep track of the districts, while the suffix "dis" will refer to the subsamples when selecting the district whose name remained after merging with another district by 1925. We thus have 7 different samples for the robustness checks, namely S2pop, S3pop, S4pop, S1dis, S2dis, S3dis, and S4dis. These samples will be compared to S1pop, the mobile sample that I have so far used for my empirical analyses.

Table B6 in Appendix B provides a simple comparison of these subsamples in terms of the number of observations, the number of administrators, the number of districts they each contain, and the share of observations for which the administrators of the sample is also the one who stayed the longest in a given year in a district (since several switches could happen within a year in a given district). There is not much difference with respect to S1pop, except for S4pop and S4dis, which contain less observations because I removed the observations when there were more than

one administrator observed in December in a district in a given year. Also, in the samples S2pop and S2dis, the share of the administrators that correspond to those who stayed in a given district of a given year is higher by 5 percentage points than in the other subsamples. Out of the 128 administrators present in S1pop, 116 are also present in all of the other samples (namely, S2pop, S3pop, S4pop, S1dis, S2dis, S3dis, and S4dis), and there are no administrators that are observed only in one sample.

Table 10: Robustness Checks Results: Without The Districts With Many Returns

Estimation technique →	3-way FE method				2-way FE method			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
p-value for $\sum \alpha_a = 0$	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
R-squared	0.862	0.863	0.870	0.871	0.796	0.798	0.802	0.803
Share of R^2 due to $\hat{\alpha}_a$	0.115	0.113	0.080	0.094	0.233	0.229	0.170	0.167
Share of R^2 due to $\hat{\delta}_d$	0.682	0.678	0.580	0.578	-	-	-	-
Corr($\hat{\delta}_d$, $\hat{\alpha}_a$)	-0.41*	-0.42*	-0.31*	-0.36*	-	-	-	-
Admin. time-variant var.	No	Yes	No	Yes	No	Yes	No	Yes
District time-variant var.	No	No	Yes	Yes	No	No	Yes	Yes
District time-constant var.	No	No	No	No	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	No	No	No	No
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Admin. FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	423	423	423	423	423	423	423	423

Notes: Heteroskedasticity is accounted with robust standard errors. The dependent variable is the number of teachers per 100,000 inhabitants per district. The results shown are p-values, except for the row Corr($\hat{\delta}_d$, $\hat{\alpha}_a$), which corresponds to the correlation between the estimated administrator FE and district FE; the presence of a star indicates that the correlation is statistically significant at the 1% level.

As reported in Table B7, running the analysis on the other samples does not significantly alter the baseline results. The only noticeable change is for the two-way fixed-effects estimations of columns (2) and (6), in which the share of the R-squared explained by the administrator fixed effects is higher than the one of S1pop (0.216 vs 0.143). The corresponding distributions of the administrator fixed effects are shown in Figure A3 of Appendix A. Here as well, the patterns are similar overall to those of the main specifications. Regarding the heterogeneous effects, almost none of the results are affected in terms of significance when I perform the analysis on the other subsamples (results not shown). Only with S2pop, S2dis, S4pop, and S4dis, the p-value for the Assistant Administrators' fixed effects falls from 0.099 (with S1pop) to nearly zero in the two-

way fixed-effects estimation with all the time-variant covariates; but this does not substantially affect the interpretation of the results.

8 Concluding Remarks

In this paper, I apply fixed-effects techniques to investigate the role of colonial district administrators on public investments in education in French West Africa. The empirical framework that I use enables me to analyze the importance of administrator effects in the observed variation in educational investments at the district level. More precisely, I measure how much of this variation is due to both observed and unobserved administrator-specific time-invariant characteristics, and I examine whether the estimated administrator effects are jointly significant.

I find that the administrator fixed effects have little explanatory power for the variance in public educational investments. At best, these administrator effects contribute up to 22% in this variation depending on the specification and method considered. However, once district fixed effects are included in the estimation, these district effects capture almost all the variation in the outcome variable, reducing the administrator effects to only 1% to 2% of the variation. This suggests it is the matches between administrators and districts that matter more, rather than the intrinsic administrators' time-constant characteristics, such as their education level. The results further reveal that these administrator effects are in fact heterogeneous: they suggest that the administrators who arrived first in the districts are relatively more important than those who came later. These findings thus support the idea of a path dependence in the policy strategy in investments in human capital in French West Africa during the colonial era. Hence, the path of the level of educational investments, which explains that the regions with more public investments during the early colonial period continued to receive more of them today (as found in Huillery (2009)), seems to depend mainly on time-constant district characteristics and on the very first district administrators who arrived in the districts.

Appendices

Appendix A. Figures

Figure A1. Proportion of ENFOM alumni, by year (1906-1929)

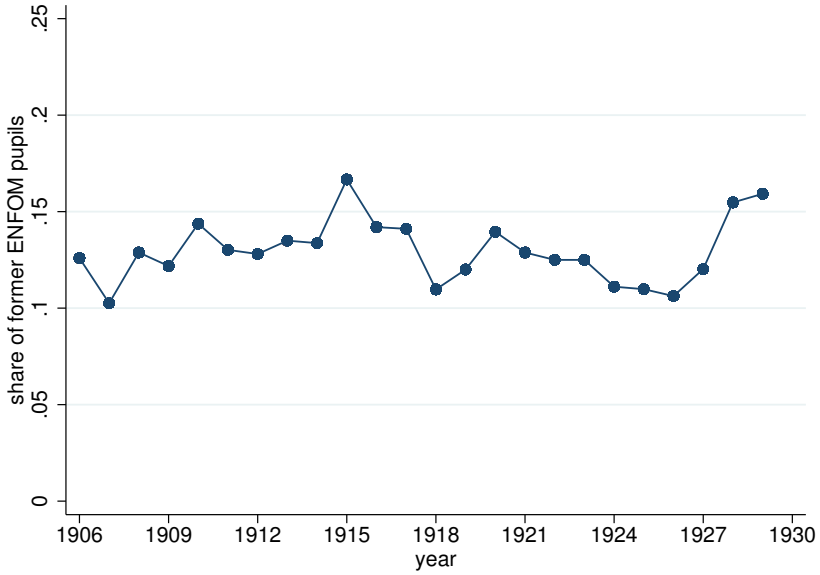
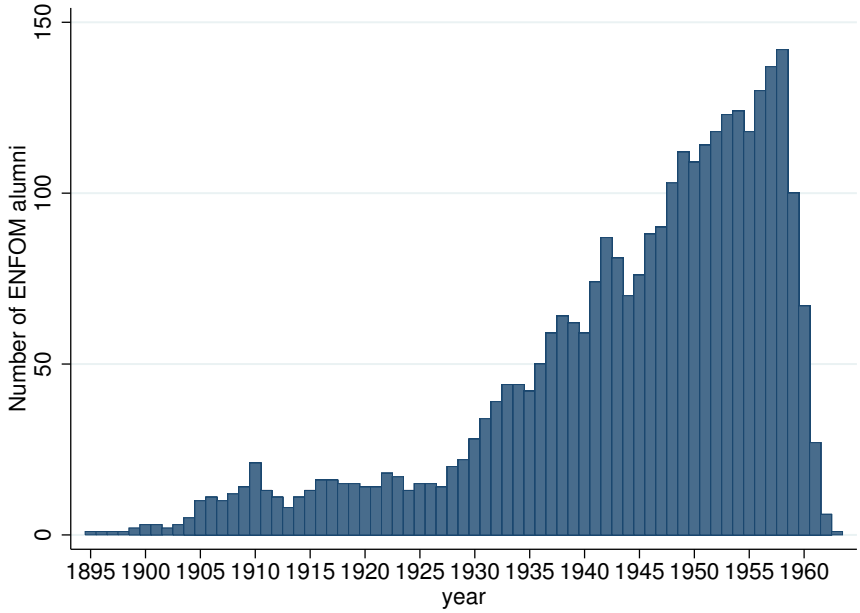
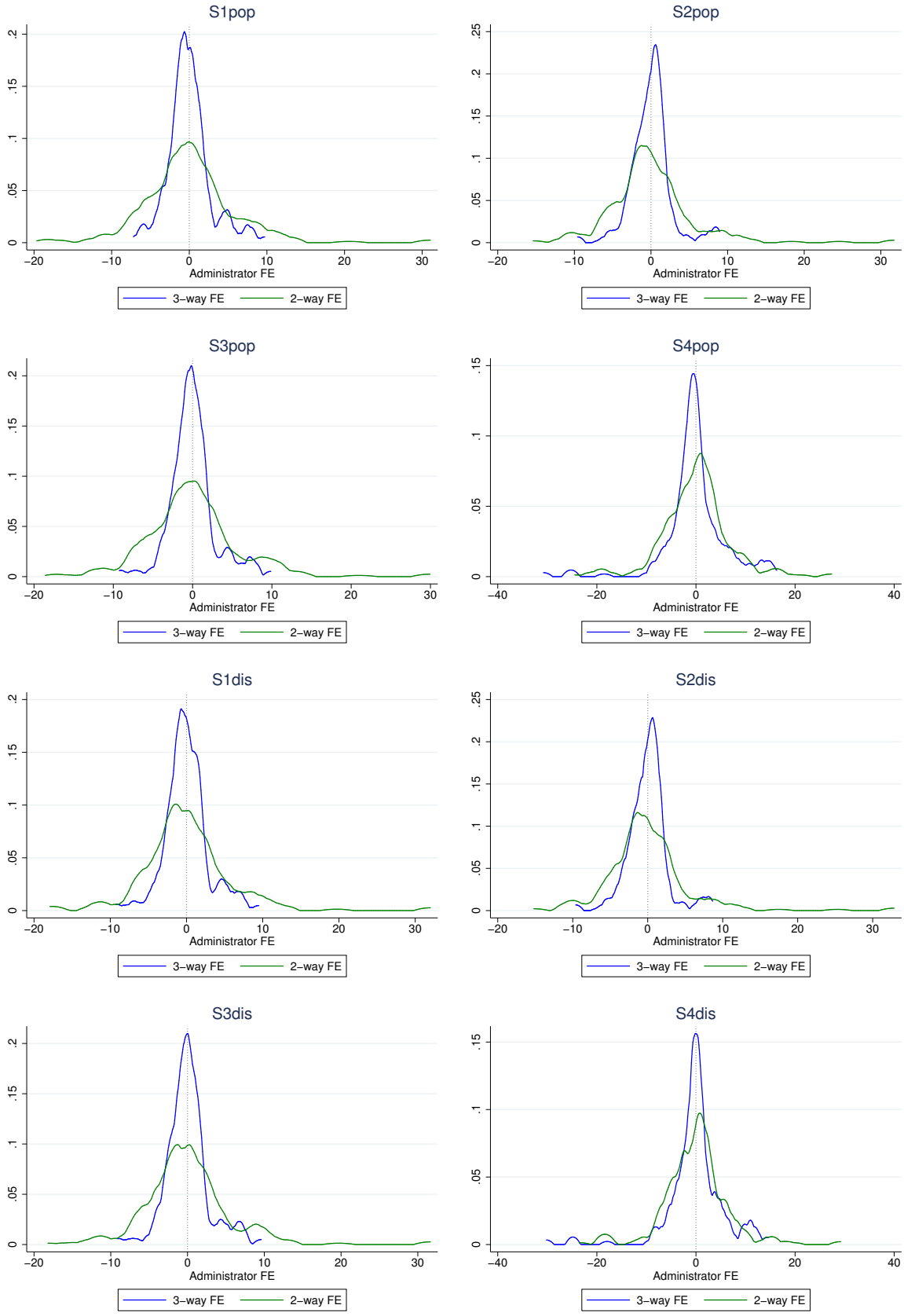


Figure A2. Number of ENFOM alumni,[a] by year (1895-1963)



[a]This chart shows the annual number of the alumni of the colonial school ENFOM who were reported to be district administrators in French West Africa (see Appendix C).

Figure A3. Distribution of administrator FE, by subsample.



Appendix B. Tables

Table B1. Distribution of switches, by month

	Long Spells[a]		All Spells	
	freq.	%	freq.	%.
January	96	11.9	225	11.5
February	74	9.2	147	7.5
March	81	10.0	170	8.7
April	65	8.0	178	9.1
May	61	7.5	190	9.7
June	71	8.8	189	9.7
July	67	8.3	169	8.7
August	57	7.1	151	7.7
September	54	6.7	128	6.6
October	59	7.3	117	6.0
November	58	7.2	137	7.0
December	58	7.2	142	7.3
Total	801	100	1,943	100

[a]Sample with long spells only, i.e. those that last for at least one year. Note also that both samples do not include extrapolations.

Table B2. Number of administrators' returns to districts, by colony.

Number of returns →	0	1	2	3	4	5	6	7	8
Ivory Coast	3	2	6	4	4	2	0	0	1
Dahomey	0	4	1	4	1	1	3	1	1
French Guinea	7	6	5	4	1	2	2	0	0
Haut-Senegal-Niger	6	6	5	8	1	0	1	0	1
Upper Volta	3	2	0	1	0	0	0	0	0
Senegal	3	2	5	4	1	3	0	2	0
French Sudan	2	1	2	0	1	1	0	0	0

Note: column 8 row 1 should read "in Ivory Coast, only 1 district counts 8 administrators' returns.

Table B3. Ranks of administrators.

Variable coded	Rank type	Rank	Variable coded
		1st class	18
6	Chief Administrator	2nd class	17
	Administrateur en Chef des Colonies	3rd class	16
		1st class	15
5	Administrator	2nd class	14
	Administrateur des Colonies	3rd class	13
		1st class	12
4	Assistant Administrator	2nd class	11
	Administrateur-Adjoint des Colonies	3rd class	10
		Lieutenant-Colonel	9
		Battalion Chief	8
3	Military	Captain	7
	Militaire	Lieutenant	6
		Sublieutenant	5
2	Civil Services		4
	Personnel des Services Civils		
		1st class	3
1	Indigenous Affairs	2nd class	2
	Personnel des Affaires Indigènes	3rd class	1

Note: the top corresponds to the highest rank while the bottom is the lowest rank.

Table B4. Length of administrators' presence in the initial sample.

Length	%	cum.
Less than 1 year	27.3	27.3
1-2 years	13.1	40.4
2-3 years	8.3	48.7
3-5 years	9.6	58.3
5-10 years	18.4	76.7
10-15 years	11.2	87.9
15-20 years	9.6	97.5
20-25 years	2.5	100.0
Average (in years)	5.9	

Table B5. Robustness Checks Results: Normalized Outcome Variable.

Estimation technique →	3-way FE method				2-way FE method			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
p-value for $\sum \alpha_a = 0$	0.000	0.000	-	-	0.000	0.000	-	-
R-squared	0.941	0.945	-	-	0.756	0.774	-	-
Share of R^2 due to $\hat{\alpha}_a$	0.018	0.018	-	-	0.180	0.170	-	-
Share of R^2 due to $\hat{\delta}_d$	0.914	0.927	-	-	-	-	-	-
Corr($\hat{\delta}_d$, $\hat{\alpha}_a$)	-0.16*	-0.17*	-	-	-	-	-	-
Admin. time-variant var.	No	Yes	No	Yes	No	Yes	No	Yes
District time-variant var.	No	No	Yes	Yes	No	No	Yes	Yes
District time-constant var.	No	No	No	No	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	No	No	No	No
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Admin. FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	582	582	-	-	582	582	-	-

Notes: The dependent variable is the number of teachers per district normalized by the population as in 1925. The results shown are p-values, except for the row Corr($\hat{\delta}_d$, $\hat{\alpha}_a$), which corresponds to the correlation between the estimated administrator FE and district FE; the presence of a star indicates that the correlation is statistically significant at the 1% level. Heteroskedasticity is accounted with robust standard errors. Columns (3)-(4) and (7)-(8) do not display any results because there are too many parameters to estimate and when performing the joint significance test on administrator FE, Stata automatically dropped several constraints.

Table B6. Simple Comparison Across Samples

Sample	Nb of obs.	Nb of admin.	Nb. of districts	% "long admin."
S1pop	516	128	83	63.6
S2pop	514	132	84	68.9
S3pop	511	128	83	64.2
S4pop	457	119	80	63.7
S1dis	516	129	83	63.6
S2dis	513	132	84	69.0
S3dis	513	129	83	64.1
S4dis	461	119	80	63.8

Table B7. Robustness Checks Results: Different Subsamples.

Subsample →	S1pop	S2pop	S3pop	S4pop	S1dis	S2dis	S3dis	S4dis
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. 3-way FE methods								
p-value for $\sum \alpha_a = 0$	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
R-squared	0.927	0.930	0.927	0.969	0.926	0.931	0.927	0.968
Share of R^2 due to $\hat{\alpha}_a$	0.006	0.022	0.006	0.002	0.000	0.021	0.004	0.001
Share of R^2 due to $\hat{\delta}_d$	0.866	0.868	0.865	0.898	0.870	0.871	0.867	0.898
Corr($\hat{\delta}_d$, $\hat{\alpha}_a$)	-0.21*	-0.12*	-0.23*	-0.38*	-0.25*	-0.12*	-0.22*	-0.36*
B. 2-way FE methods								
p-value for $\sum \alpha_a = 0$	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
R-squared	0.674	0.708	0.690	0.673	0.674	0.708	0.690	0.673
Share of R^2 due to $\hat{\alpha}_a$	0.143	0.216	0.171	0.122	0.144	0.216	0.171	0.122
Number of observations	516	514	511	457	516	513	513	461

Notes: The dependent variable is the number of teachers per 100,000 inhabitants per district. The administrator and district time-variant control variables are included in all specifications. The two-way FE estimations include the district time-constant covariates as well. The row Corr($\hat{\delta}_d$, $\hat{\alpha}_a$) shows the correlation between the estimated administrator FE and district FE; the presence of a star indicates that the correlation is statistically significant at the 1% level. Heteroskedasticity is accounted for with robust standard errors.

Appendix C. Data Collection

District-administrator matched dataset

Data for the district-administrator matched database mainly comes from the colonies' official journals (*Journaux Officiels des Colonies*). Alexandre Aubourg and I went through them at the *Bibliothèque François-Mitterrand* (Paris), *Bibliothèque Cujas* (Paris), and *Bibliothèque de Documentation Internationale Contemporaine* (Nanterre), either in the microfilm format or in the hard-copy one. These journals are bi-monthly publications (except for the official journals of Senegal, which were weekly publications) and contain information about almost all of the switches that happened from 1906 to 1929 for each colony, except that of Niger and Mauritania. More precisely, for each year and each colony, these journals report the switches by providing the approximate arrival date of the new colonial district administrator, his last name (sometimes when there were homonyms, they also report the first and middle names), the name of the district to which he is assigned, and the last name of the colonial administrator that he replaces. Some administrators were reported as temporary (*par interim*) but they lasted for more than one year, which is unlikely, i.e. the replacement turned out to be permanent rather than temporary. Conversely, some were not reported as "par intérim" but were there as such. To solve this issue, given that the average length of stay of the temporary administrator is 4 months, for the analyses in this paper, I assume that administrators were temporary whenever either they were reported as "par intérim" and last for less than a year, or their spell lasted for 4 months or less (regardless

of whether they were reported as "par intérim").

Because of the homonyms and the fact that not all the journals were available, we had to look for the information in other sources and perform extrapolations when the information was still missing. To help us identify the administrators when there were homonyms, we collected information from:

- The *Annuaires du Gouvernement général de l'Afrique Occidentale française* at the *Académie des Sciences d'Outre-Mer* (Paris): these annual reports listed the full names of all the administrators present in a given year, with their birthdates, the colonies they were in during that year, their starting date in the federation, and the dates of their promotion to a higher rank. These annual reports were available only for the following years: 1900, 1906, 1909, 1910, 1911, 1912, 1913-14, 1915-16, 1917-21, and 1922.

- The *Dictionnaire biographique des Anciens Élèves de l'ENFOM* at the *Académie des Sciences d'Outre-Mer* (Paris): this is a glossary gathering several personal information on all of the alumni of the colonial school (graduation date, birthdate, place of birth, degrees, etc.). It also reported their career in the colonial administration.

- The *dossiers de personnel colonial* at the *Archives Nationales d'Outre-Mer* (Aix-en-Provence): the archives there contain the individual records of the former colonial administrators. Some restrictions apply though regarding which records can be viewed.

- The online database *Léonore*: this is a website containing information about all the individuals who received the Legion of Honor. Pictures of the professional records of these individuals could sometimes be found.

Data on district population

The data for the variable on district populations comes from Huillery (2009). I completed the values for this variable when it was missing by collecting them in the *Annuaires du Gouvernement général de l'Afrique Occidentale française*, which can be found at the *Académie des Sciences d'Outre-Mer* (Paris). However, I noticed that for some years the populations reported was exactly the same, which presumably means that these annual reports did not necessarily update the information on district populations for each new publication.

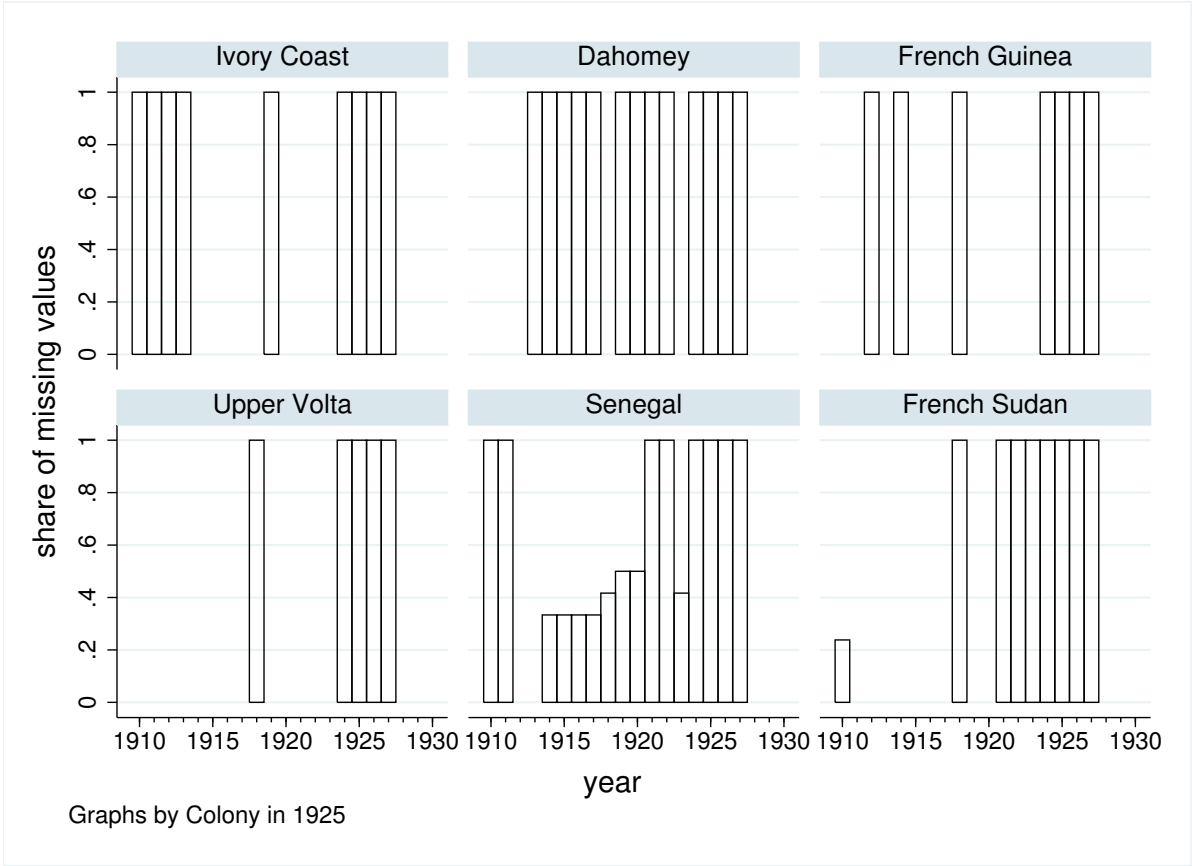
In the initial sample, I kept the values from Huillery (2009) whenever they were available. I replaced the missing value for the district population variable by the values reported in the annual reports of the general government of French West Africa. Then I performed mainly linear extrapolations to fill in the missing values for that variable in my analyses. When performing linear extrapolations for this variable, I sometimes discarded the values from these annual reports, and I did not extrapolated for the year 1928 because the last year for which the data was available is 1922, so I adopted a more conservative approach by deciding not to extrapolate in that case, as detailed in Appendix D. In the mobile sample, 63% of the observations correspond to extrapolations.

Appendix D. Details on Variables

Number of teachers per 100,000 inhabitants per district

This variable serves as a proxy for the districts' public investments in education. However, the data for public educational investments were not available for all the years over the 1910-1928 period. Specifically, for the number of teachers per district, 796 of the 1,742 observations (i.e., roughly half of the sample) correspond to missing values. Except Senegal and French Sudan, when data are missing for a given year in a colony, they are missing for all the districts of that colony (see Figure D1). To gain efficiency in the estimates that I obtain, I remove for each colony the years in which all the observations of the number of teachers per district are missing. I am left with 968 observations, of which 4.4% correspond to missing values.

Figure D1. Share of missing observations over the 1910-1928 period, by colony.



Note: These graphs are drawn using the districts as of 1925 and by keeping the districts with the largest populations when districts merged before 1925.

To compute the number of teachers per 100,000 inhabitants per district, I also need the district population. The data for this variable comes from Huillery (2009) and completed by the information found in the *Annuaires du Gouvernement général de l'A.O.F.* for the years 1910, 1911, 1912, 1914, 1916, 1921, and 1922. However, the data are not available for all the years,

and the data from the *Annuaires* were sometimes the same over a short period of time for the districts of a few colonies (cf. below for more detail). I therefore extrapolated a certain number of years for some districts: in most cases, I arbitrarily performed a linear extrapolation for the missing values between the two nearest available years, except if the district changed its borders in-between (i.e., merged or split). For instance, if in a given district the population was missing in 1915 but not in 1914 and 1916, I assumed that the value in 1915 was the average of the values 1914 and 1916. I below detail how the extrapolations are made for the districts of each colony (using the districts as of 1925 and the largest populations when districts merged):

- Ivory Coast (non-missing values for the number of teachers: 19 districts, 1914-1928):
  - Linear extrapolations from 1914 to 1922: Assinié, Baoulé, Bondoukou, Indénié, and Séguéla.
  - Linear extrapolations from 1914 to 1921: Guiglo.
  - Linear extrapolations from 1916 to 1922: Odienné.
  - Linear extrapolations from 1914 to 1916, and from 1916 to 1922: Agnéby, Gouros, Kong, Lagunes, Lahou, Man, N'Zi-Comoé, Sassandra, Tabou, and Tagouanas.
  - Extend the linear extrapolations to 1915: Odienné.
  - Extend the linear extrapolations to 1923: Agnéby, Daloa, Guiglo (using 1921-1922), and Assinié, Baoulé, Bondoukou, Gouros, Indénié, Kong, Lagunes, Lahou, Man, N'Zi-Comoé, Odienné, Sassandra, Séguéla, Tabou, Tagouanas (using 1922).
  - No extrapolations for Bassam 1915-1928 and Daloa 1915-1921.
  - No extrapolations for 1928 for all districts.
- Dahomey (non-missing values for the number of teachers: 11 districts, 1910-1928):
  - Linear extrapolations from 1910 to 1912: Abomey, Borgou, Djougou, Mono, Moyen-Niger, Porto-Novo, and Savalou.
  - Linear extrapolations from 1910 to 1923: Allada.
  - Extrapolations for 1911-1912 using 1910: Ouidah.
  - Extrapolations for 1910-1911 using 1912: Cotonou.
  - No extrapolations for 1918, 1923, and 1928 for all districts except Allada.
  - No extrapolations and Atacora.
- French Guinea (non-missing values for the number of teachers: 18 districts, 1910-1928):
  - Linear extrapolations from 1911 to 1916: Beyla, Boffa, Boké, Conakry, Dabola, Forécariah, Kankan, Kindia, Kissidougou, Koumbia, Kouroussa, Labé, Macenta, Mamou, and Siguiri.

- Linear extrapolations from 1916 to 1922: Beyla, Dabola, Forécariah, Kankan, Kindia, Kissidougou, Koumbia, Kouroussa, Labé, Mamou, and Siguiri.
- Linear extrapolations from 1916 to 1921: Boffa, Boké, and Conakry.
- Linear extrapolations from 1911 to 1922: Guéckédou, Macenta, and N'Zérékoré.
- Linear extrapolations from 1911 to 1921: Pita.
- Linear extrapolations for 1923 using 1921-1922: Beyla, Boffa, Boké, Conakry, Dabola, Forécariah, Kankan, Kindia, Kissidougou, Koumbia, Kouroussa, Labé, Mamou, Pita, and Siguiri.
- Linear extrapolations for 1910-1915 using the trend of 1916-1921: Conakry.
- Extrapolation for 1923 using 1922: Guéckédou, Macenta, and N'Zérékoré.
- No extrapolation for 1928 for all districts.

- Upper Volta (non-missing values for the number of teachers: 11 districts, 1910-1928):
  - Linear extrapolations from 1910 to 1912: Kaya, Koudougou, Ouagadougou, Ouahigouya, and Tenkodogo.
  - Linear extrapolations from 1911 to 1914: Dédougou.
  - Linear extrapolations from 1912 to 1914: Bobo-Dioulasso, Dori, Fada, Kaya, Koudougou, Ouagadougou, Ouahigouya, Say, and Tenkodogo.
  - Linear extrapolations from 1912 to 1916: Gaoua.
  - Linear extrapolations from 1914 to 1921: Bobo-Dioulasso, Dédougou, Dori, Fada, Kaya, Koudougou, Ouagadougou, Say, and Tenkodogo.
  - Linear extrapolations from 1914 to 1922: Ouahigouya.
  - Linear extrapolations from 1916-1921: Gaoua.
  - Linear extrapolations from 1921 to 1923: Bobo-Dioulasso, Dédougou, Dori, Fada, Gaoua, and Say.
  - No extrapolation for 1928 for all districts.

- Senegal (non-missing values for the number of teachers: 12 districts, 1910-1928):
  - Linear extrapolations from 1912 to 1914: Bakel, Baol, Dagana, Haute-Gambie, Louga, Matam, Podor, Sine-Saloum, Tambacounda, Thiès, and Tivaouane.
  - Linear extrapolations from 1914 to 1916: Bakel, Baol, Dagana, Haute-Gambie, Matam, Podor, Sine-Saloum, and Tambacounda.
  - Linear extrapolations from 1912 to 1916: Casamance.
  - Linear extrapolations for 1917 using the previous trend: Baol.

  - Linear extrapolations for 1917-1918 using the previous trend: Sine-Saloum, and Tambacounda.
  - Linear extrapolations for 1917-1920, using the previous trend: Bakel, Dagana, Haute-Gambie, Matam, and Podor.
  - Extrapolations for 1920, using the past trend: Tambacounda.
  - Linear extrapolations for 1923, using the past trend: Bakel, Haute-Gambie, Matam, Podor, and Sine-Saloum.
  - No extrapolations for 1923: Casamance, Dagana, and Tambacounda.
  - No extrapolations for 1928 for all districts.

- French Sudan (non-missing values for the number of teachers: 21 districts, 1910-1928):
  - Linear extrapolations from 1910 to 1912: Mopti.
  - Linear extrapolations from 1910 to 1914: Goundam, and Gourma.
  - Linear extrapolations from 1911 to 1914: San.
  - Linear extrapolations from 1912 to 1916: Bandiagara.
  - Linear extrapolations from 1912 to 1914: Bafoulabé, Bamako, Bougouni, Gao, Kayes, Kita, Koutiala, Macina, Mopti, Nara, Niafunké, Nioro, Satadougou, Ségou, and Sikasso.
  - Linear extrapolations for 1915-1920, using the past trend: Bafoulabé, Bamako, Bougouni, Gao, Goundam, Gourma, Kayes, Kita, Koutiala, Macina, Mopti, Nara, Niafunké, Nioro, San, Satadougou, Ségou, and Sikasso.
  - Linear extrapolations for 1917-1920, using the past trend: Bandiagara.
  - Extrapolations for 1911 and 1913, using 1912: Tombouctou and Nema.
  - No extrapolations for 1914-1923: Tombouctou and Nema.
  - No extrapolations for 1928 for all districts.

After extrapolating the values, I dropped all the observations for which the value for the number of teachers is missing. This procedure yields 925 observations, of which 53% have been extrapolated, 32% of values for the outcome variable that have not been extrapolated and are not missing. Looking at the distribution within each colony:

Table D1. Extrapolations for the outcome variable, by colony.

	Non-extrapolated	Extrapolated	Missing	Total
Ivory Coast	52	107	29	188
	(27.8%)	(56.9%)	(15.4%)	(100%)
Dahomey	19	12	34	65
	(29.2%)	(18.5%)	(52.3%)	(100%)
French Guinea	71	124	18	213
	(33.3%)	(58.2%)	(8.5%)	(100%)
Upper Volta	59	78	11	148
	(39.9%)	(52.7%)	(7.4%)	(100%)
Senegal	28	50	15	93
	(30.1%)	(53.8%)	(16.1%)	(100%)
All colonies	299	488	138	925
	(32.3%)	(52.8%)	(14.9%)	(100%)

Time-varying administrator characteristics

→ Administrators' rank: I created a index variable that ranges from 1 to 6 and is increasing in the rank of the administrator. Administrators' ranks can be classified as follows (from the lowest to the highest): Personnel des Affaires indigènes des Colonies, Personnel des Services civils des Colonies, Militaire, Administrateur-Adjoint des Colonies, Administrateur des Colonies, and Administrateur en Chef des Colonies. Because the rank was sometimes non-reported, I extrapolated the rank for 21 out of 795 observations (that is, 2.6% of the observations) by replacing the missing values by the average rank by district in the reference sample.

→ Administrators' past experience as a district administrator: this variable is computed based on the past number of months an administrator was in charge of a district at a given point in time. For example, if (i) an administrator is observed the first time as being in charge of a district in Senegal for 6 months in 1910, (ii) the second time he is observed to be the district administrator of a district in Ivory Coast for 3 months in 1915, and (iii) the third time, he is in charge of another district in Ivory Coast for 2 months in 1916, then he past experience would be 9 months in 1915, but 11 months in 1916. Out of the 795 observations of the initial sample, 12 values (1.5%) of administrators' experience have been extrapolated.

Time-variant district characteristics

→ Colonies' ordering: in the *Annuaires du Gouvernement général de l'A.O.F.*, colonies appeared in different orders depending on the years. This may be used as a proxy for the importance of colonies relative to one another. It is not clear what criteria were chosen to classify the colonies though.

→ Districts' ordering: similarly, for each colony, the information about the districts appeared in

a particular order, which presumably reflects the importance of a district compared to the others, so I use this ranking to construct a variable that classifies districts within a colony into quartiles because the number of districts differs from one colony to another but also because it could differ from one year to another within a same colony. However, the ordering was missing for some districts, I therefore had to extrapolate as follows: (i) if the ordering is the same before and after the year of the missing value, then the missing value takes this ordering, (ii) if the ordering before the year of the missing value differs from the ordering after, then in the main analysis, I assign the ordering of the previous year, and as a robustness check, I perform the analysis using the ordering of the following year. Overall, I extrapolated 207 out of 795 observations (26%) for this variable in the reference sample.
→ District population: as described previously, this data is from Huillery (2009) and the *Annuaires du Gouvernement général de l'A.O.F.*. It reports the number of inhabitants per district in a given year.

Time-invariant district characteristics
→ Geographical variables: altitude, latitude, longitude, annual rainfalls, access to the sea dummy, presence of an important river dummy, and distance to the coast.
→ Pre-colonial variables: centralized political power dummy and European trade counter dummy.
→ Conquest variables: year of colonial conquest's start and length of the local resistance to colonial conquest.
The data for all these three types of variables comes from Huillery (2009); details can be found there.

Time-constant administrators' characteristics
→ Administrators' total number of years in the colonial administration: this variable is constructed based on the first and last time the administrator is observed in the sample; it proxies the total number of years a given administrator has been in the colonial administration.
→ Administrators' graduation from the colonial school: it corresponds to a dummy variable that takes value 1 if the administrator was a former pupil of ENFOM, the colonial school located in the south of mainland France. The information for this variable comes from the *Dictionnaires bibliographiques* (Association des anciens élèves de l'Ecole coloniale de la France d'outre-mer, 2003).

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