“Enrichissez-vous!”: Education, Suffrage, and Local Development in July Monarchy France

Etienne Compérat*

Sciences Po

Abstract

Education and democracy are often considered as drivers of economic growth. However, causal evidence on either channel remains limited, and evidence on the interaction between the two is scarcer still. This paper attempts to study their joint role in 19th-century France using two population-threshold reforms enacted under the July Monarchy: the Municipal Law of 1831, which expanded local voting rights through a commune-size-based suffrage schedule, and the Guizot Law of 1833, which mandated a boys' primary school in municipalities above 500 inhabitants. Using a newly assembled arrondissement-level dataset of France from 1830 to 1865, I implement two complementary empirical designs. A static OLS specification examines the relationship between pre-reform education, baseline municipal suffrage exposure, and industrial wages in the 1840s, before post-Guizot schooling expansion could fully affect labor-market outcomes. A dynamic IV specification then instruments changes in male primary schooling with arrondissement exposure to the Guizot population threshold and evaluates wage growth into the 1860s. In this pre-national-accounts setting, average industrial wages serve as an indicator of local labor productivity and living standards, and are used here as a proxy for local economic development. The evidence shows higher pre-reform male primary education is positively associated with higher industrial wages in the 1840s. By contrast, I find no robust evidence of an independent wage effect of municipal suffrage exposure, no evidence of complementarity between education and political participation, and no statistically significant medium-run wage effect of policy-induced schooling expansion in the IV specification. These results are consistent with long adjustment lags for schooling returns and with the limited economic powers of municipalities under the centralized institutions of the July Monarchy. Finally, I also show that a central identification challenge is that commune-size distributions are historically structured and spatially correlated with pre-existing regional differences, which jointly shape exposure to both laws. The paper therefore contributes to the literature on the causes of growth in two ways: by combining evidence on education and political participation in a unified historical setting, and by highlighting the identification challenges that arise when institutional and demographic legacies are spatially correlated.

*I am sincerely grateful to my thesis supervisors Roberto Galbiati and Emeric Henry for their guidance, support, and insightful feedback throughout this project. I also want to thank Kevin O'Rourke for serving on my jury and for his thoughtful evaluation of my work. Finally, a special thanks to my brother and roommate Nicolas, for patiently enduring countless conversations about 19th-century French economic development.

1 Introduction

"Establish your government, strengthen your institutions, enlighten yourselves, enrich yourselves, improve the moral and material condition of our France."

- François Guizot, 1843 ;
French minister of foreign affairs (1840–1848), previously minister of Public Instruction (1832–1837)

Few questions in economics have received as much attention as the origins of growth. Among the leading explanations, two main drivers are often emphasized: institutions and human capital. An 1843 quote from François Guizot, then French minister of foreign affairs, captures how these factors may have been understood within the mid-nineteenth-century French state. In his address to the Chamber of Deputies, the leading figure of the July Monarchy framed institutional consolidation and individual "enlightenment" as the foundations for social order and material progress, exhorting his compatriots to enrich themselves by these means (an injunction that became famous). This rhetoric strongly echoed the governing ideology of the regime at a time when the country was undergoing profound structural change, and in particular the reform agenda of the early 1830s. By 1840, the constitutional monarchy had expanded suffrage rights to millions of citizens and laid the foundations of a national mass-education system, thereby broadening political participation and fostering the rise of a more literate middle class.

The ways democratic institutions and education shape economic development, and whether they reinforce each other, has been a longstanding debate in the growth literature. In institutionalist tradition, democracy is viewed as a driver of development, since it is associated with stronger property rights, political accountability, and greater investment in public goods (Acemoglu et al., 2019; Gerring et al., 2005). A key implication is that political inclusion could shape economic outcomes, rather than merely emerging as a byproduct of rising income or education. However, in human-capital tradition, education is seen not only as a direct source of productivity growth (Glaeser et al., 2004) but also as a force shaping political behavior and the emergence or stability of democratic institutions (Glaeser et al., 2007). These perspectives imply potentially reinforcing mechanisms: democracy may influence growth partly through public service provision, especially education (Baum & Lake, 2003), while education may in turn affect democratic participation and institutional stability. Nevertheless, empirical evidence remains limited on how these channels jointly operate, and especially on whether they interact in historical settings.

In this paper, I leverage two reforms enacted under the July Monarchy whose implementation depended on commune population thresholds to study these two channels jointly in mid-nineteenth-century France. The first is the Law on Municipal Organization of 1831, which broadened political participation by expanding voting rights in municipal council elections to roughly 2.7 million male taxpayers (Degrave et al., 2024). Importantly, the law determined the number of eligible voters as a function of commune population, implying higher suffrage shares in smaller communes and generating systematic variation in municipal political participation across space. The second is the Guizot Law of 1833, which laid the foundations of mass primary schooling by requiring every municipality with more than 500 inhabitants to open and fund a boys' primary school within six years -a reform that nearly doubled the number of schools within a decade (Blanc & Kubo, 2026). Together, these threshold-based reforms created geographically structured variation in exposure to democratization (through municipal suffrage) and to schooling expansion, which I aggregate to the arrondissement level to examine local economic outcomes. This setting raises the following question: How did the expansions of primary schooling and democratic participation in 1830s France shape local economic development?

To answer this question, I assemble an arrondissement-level dataset covering nearly all of metropolitan France between 1830 and 1865. I reconstruct exposure to both reforms from commune population distributions and the legal rules they triggered, and then aggregate these measures to the arrondissement level. Economic outcomes are drawn from the industrial surveys of 1839–1847 and 1860–1865, which provide information on average wages for male industrial workers and allow local labor-market performance to be compared across space and over time.

Average industrial wages serve as the primary outcome throughout. In pre-national-accounts economies, wages are the principal available indicator of labor productivity and living standards (Allen, 2001); in competitive labor markets they reflect the marginal productivity of labor, so that arrondissements with higher wages tended to be those with more productive, capital-intensive industrial activity, a pattern consistent with evidence from these same surveys showing that steam-adopting industries paid significantly higher wages than their labor-intensive counterparts (Ridolfi, Salvo & Weisdorf, 2023). This measure has real limitations: it misses agricultural activity, which remained dominant in many regions, and covers only the male industrial workforce. These are discussed in the Data section.

I use two complementary empirical designs that identify different relationships. First, I estimate a static cross-sectional specification relating pre-reform schooling and baseline political participation to industrial wages in the 1840s, before the effects of post-Guizot schooling expansion could fully materialize in the labor market. Second, I estimate a dynamic specification for wage growth between the 1840s and the 1860s, instrumenting changes in male pupils per 10,000 inhabitants with arrondissement exposure to the Guizot population threshold. In both designs, political participation is measured through the share of eligible municipal voters within the arrondissement, while a broad set of demographic, industrial, institutional, and economic controls is included to account for cross-arrondissement heterogeneity. Together, these two designs make it possible to compare the cross-sectional relationship between schooling, democratic participation, and wages with the medium-run effect of policy-induced schooling expansion.

The results suggest that higher levels of male primary education prior to the Guizot Law are positively associated with higher industrial wages in the 1840s. By contrast, I find no robust evidence of an independent or interactive effect of political participation on wages, and the dynamic IV design does not detect a statistically significant wage-growth effect of policy-induced schooling expansion between 1839 and 1865. In the dynamic specification, OLS and IV estimates both point to small, statistically insignificant effects, while the Wu-Hausman test fails to reject exogeneity; given the test's limited power, this should be read as convergence between estimators rather than as proof that OLS is unbiased. A central challenge for interpreting these findings is that exposure to both reforms depends on the distribution of commune sizes across arrondissements. Because this distribution is historically structured and correlated with pre-existing differences in development, rurality, and administrative organization, it creates a risk of residual confounding even after conditioning on observable characteristics. Therefore, estimates should be interpreted with caution, and this identification challenge is treated here as part of the substantive contribution of the paper rather than as a purely technical limitation.

This study contributes to the economic history literature on 19th-century France and builds on recent work exploiting the reforms of the July Monarchy as sources of policy variation. It relates first to Montalbo (2021) and Blanc & Kubo (2024), who study the effects of the Guizot Law, and to Degrave et al. (2024), who examine the expansion of municipal suffrage and its consequences for politicization and political behavior. The present paper studies schooling reform and suffrage expansion within a common empirical framework and examines how they relate jointly to local economic outcomes. It also brings into the analysis a broader feature of the period that is rising internal migration and rural exodus in 19th-century France (Blanc, 2024). By shifting the spatial focus from the municipality to the arrondissement, the paper aims to capture the short-distance population movements and spillovers linking rural communes to nearby urban centers.

More broadly, this study contributes to the institutions-versus-human-capital debate by examining education and democratization within a unified historical setting. Rather than claiming a single joint causal effect, the paper compares evidence across two complementary designs: a static association between pre-reform schooling and wages, and a dynamic specification that studies the wage effects of policy-induced schooling expansion while accounting for political participation. This makes it possible to ask whether education and democratization appear to reinforce one another in shaping local development, while also showing that the answer depends on the margin and time horizon considered. By focusing on subnational variation in a gradually modernizing economy with persistent regional inequality, the paper offers a historically grounded setting in which to reconsider the relationship between democratic institutions, human capital, and early industrial development.

A final contribution is methodological. Because exposure to both reforms depends on the distribution of commune sizes across arrondissements, the analysis highlights how historically persistent territorial structures can complicate identification in threshold-based historical designs. In this setting, commune-size distributions are not merely technical features of the data, but historically structured variables that may reflect path-dependent territorial trajectories. By making this identification challenge explicit, the paper shows that the key issue is not only whether the reforms generated useful policy variation, but also whether that variation can be interpreted independently of deeper territorial legacies, and identifies directions for future identification strategies that could better address this.

2 Historical background

2.1 The July Monarchy

Institutional centralization and limited liberalization The July Monarchy (1830–1848) was a transitional regime in 19th-century France: a liberal constitutional monarchy that expanded civil liberties but preserved elite political control through censal suffrage. Governed under the juste-milieu doctrine, it combined limited reform with a strong concern for order and continuity, especially after the upheavals of the Revolution and Restoration. As emphasized by Allier (1976), the governing strategy, associated with figures such as François Guizot, was to secure stability through cautious reform and the political preponderance of the propertied middle classes (the "notables"). This institutional setting is central to the paper because both reforms studied here, the Municipal Law of 1831 and the Guizot Law of 1833, were enacted within this centralized but reformist regime that expanded local participation and schooling while preserving a property-based distribution of political power.

Gradual economic modernization Economically, the period from the July Monarchy through the Second Empire was marked by gradual but sustained economic modernization rather than a sudden industrial “take-off” (Mendels, 1972). Industrial production accelerated after 1815 (Lévy-Leboyer, 1968; Crouzet, 1996), and growth strengthened further in the later years of the July Monarchy and under the Second Empire as investment in railways, banking and infrastructure promoted broader market integration. At the same time, this transformation remained uneven across sectors and regions, with textile and food industries continuing to account for a large share of industrial value added in the 1860s (Verley, 1997). Agriculture nevertheless remained the dominant sector for much of the century; in 1851, more than half of the population belonged to farming families, and rural economic activity continued to shape local labor markets well beyond the onset of industrial growth (Demonet, 1990). This mixed economic structure matters for the empirical analysis: the wage outcomes studied in this paper come from an economy in which industrial development was advancing, but where local labor markets remained deeply shaped by rural population patterns and uneven regional development.

Thus, the reforms examined in this paper took place in a context characterized by both institutional centralization and uneven economic transformation (Verley, 1997). This combination is important for interpretation: it helps explain why schooling reform could expand rapidly through state capacity, while the economic effects of local suffrage expansion may have remained limited under a regime that still constrained municipal autonomy (Tanchoux, 2013; Degrave et al., 2024).

Administrative division French territorial administration in the 19th century was structured around départements and arrondissements, with prefects and sub-prefects acting as intermediaries between the central state and local communities. This administrative hierarchy mattered directly for the reforms studied here: school inspection, electoral coordination, and municipal oversight were all implemented through these state channels. The centralized character of this structure is important for interpreting the results, especially because municipal political participation expanded under the 1831 law without a corresponding decentralization of local executive authority.

2.2 Primary schooling and the Guizot Law

Before the Guizot Law Before the Guizot Law, primary schooling in France was shaped mainly by local initiative rather than national legislation (Graff, 1987; Montalbo, 2021). Under the Ancien Régime, primary education developed through distinct regional systems that reflected broader institutional and geographic differences. In the North and North-East, schooling was largely organized through parochial and ecclesiastical structures, often financed through tithes or parish contributions, with teachers frequently drawn from the clergy. In much of the South,

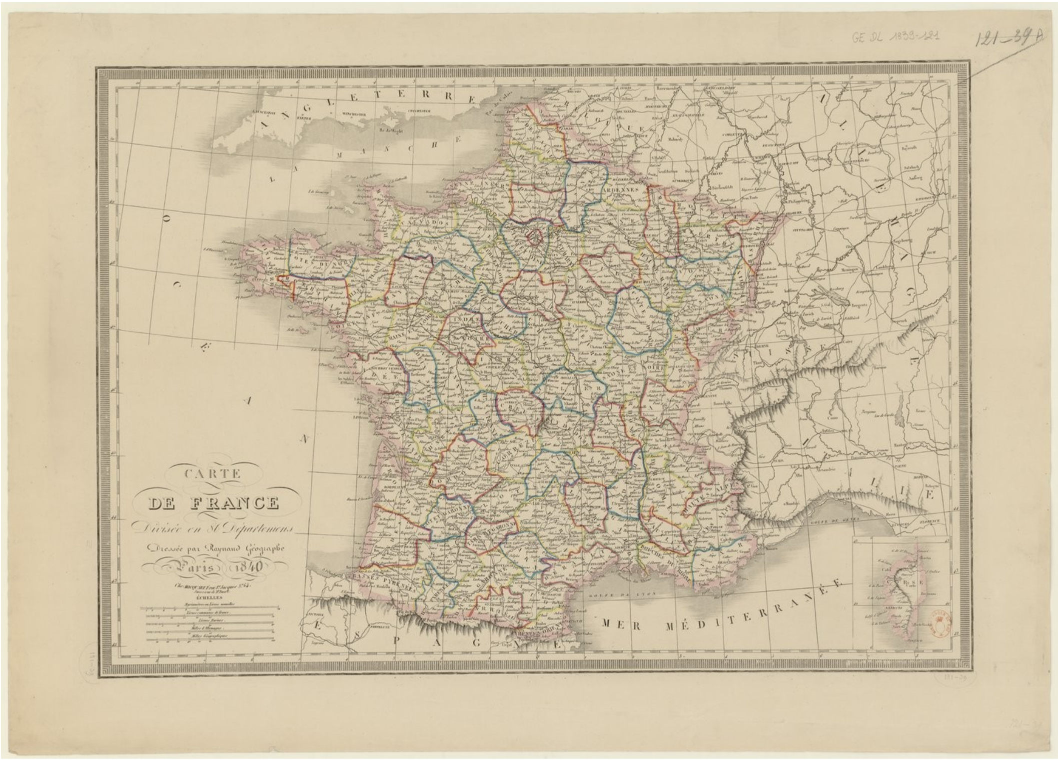


Figure 1: Carte de France divisée en 86 départements - Raynaud (source: Gallica)

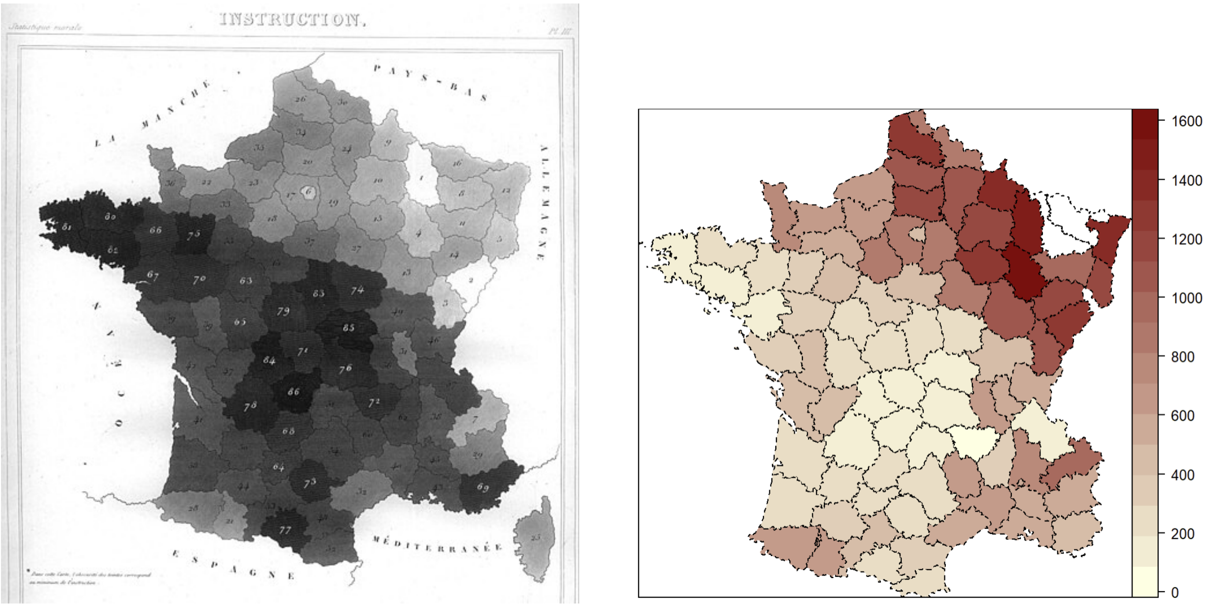
by contrast, schooling relied more on municipal management and private financing, with weaker religious oversight and a more civic organization of school provision. These differences mattered not only for how schools were funded and staffed, but also for the long-run geography of educational access.

The French Revolution severed the institutional link between primary education and the Church by abolishing ecclesiastical taxes, after which teachers were financed through a combination of school fees and municipal support. Although municipalities became more central to financing, state involvement in primary schooling remained limited until the Restoration, when public funding increased substantially between 1816 and 1832. Even so, this growing state role did not eliminate the deep regional disparities inherited from earlier schooling systems, which remained visible on the eve of the Guizot reform.

These disparities are directly relevant for the empirical strategy of this paper. As Montalbo (2021) emphasizes, schooling provision remained much denser north of the Loire than in the South-West, and these contrasts persisted into the 1820s and beyond. Indeed, through the 1820s, the average enrolment rate for children between 6 and 13 years old in the 32 départements north of the lines was 94%, against 53% in the 54 départements south. In his “Figurative map of popular education in France" (1826), Dupin drew a straight line from Saint-Malo to Geneva, capturing this divide in popular education, and reflecting broader differences in population density, urbanization, and economic development between "Northern" and "Southern" France. Historical geography is important for this paper, as it highlights the need to interpret later estimates conditional on persistent regional heterogeneity in education, settlement patterns, and development, a historical depth examined in Section 4.3.

The Law on Primary Education of June 28, 1833. In the autumn of 1833, the

Ministry of Public Instruction launched the Enquête Guizot, a national survey documenting the state of primary schooling shortly after Guizot entered office. The survey reveals that schooling provision was still limited and highly heterogeneous: many schools were irregularly operated, teachers were often poorly trained, and educational quality varied sharply across regions (Meyers, 1976; Weber, 1976). By then, most of the nation remained illiterate, while the educational system was still in its "early stages" (Furet & Ozouf, 1977; Montalbo, 2021; Blanc & Kubo, 2026). In this paper, the 1833 education data are used as a baseline indicator of schooling before the Guizot reform could fully reshape local education supply.



(a) Literacy rates among conscripts between 1827-1830 (Guerry, 1833)
- darker shades correspond to lower percentage of conscripts able to write and read

(b) Number of pupils per 10,000 inhabitants in 1833 (Data: SGF)

Figure 2: The unequal distribution of primary education

The Guizot Law of June 28, 1833 established the legal framework for state-sponsored primary education by requiring municipalities with more than 500 inhabitants to open and fund a boys' primary school within six years. The law expanded school provision quickly and strengthened centralized oversight through inspectors and prefects. By 1836, most of the new schools had been built (Blanc & Kubo, 2026) so that the number of schools nearly doubled within a decade (Figure 3). For the empirical strategy, its main importance is the 500-inhabitant threshold, which generates systematic variation in schooling exposure across communes and, once aggregated, across arrondissements. At the same time, the reform was not only educational but also political: as emphasized by Allier (1976), the regime viewed primary schooling as a tool for moralization, social order, and civic loyalty. The law therefore combined school expansion with stronger central oversight, including state inspectors, Académie authorities, and prefects, as well as more regularized teacher training and remuneration (Furet & Ozouf, 1977). Municipalities that failed to comply could be compelled by the préfet, while those with insufficient resources could request financial support from the département or the central government.

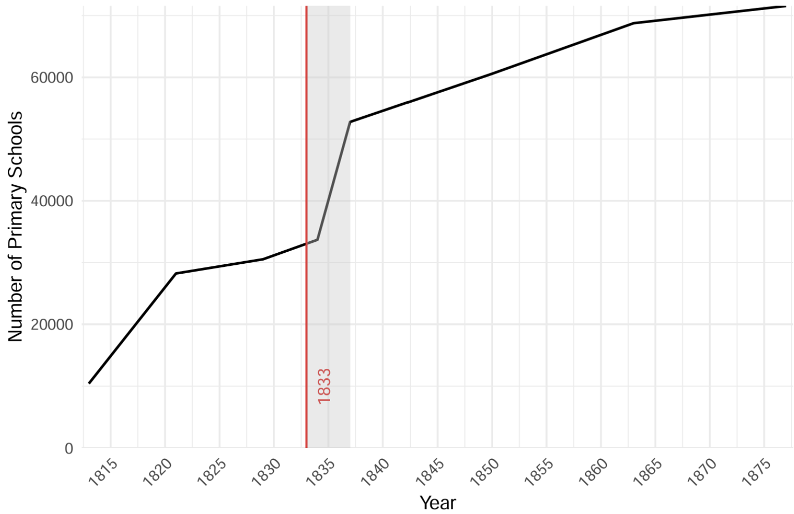


Figure 3: Total number of primary schools in France (data: SGF). The acceleration after 1833 reflects the Guizot mandate. Most new schools opened between 1834 and 1837.

2.3 The politics of suffrage and the Law on Municipal Organization

Suffrage rules Under the July Monarchy, electoral participation followed a "dual-track" logic: national political power remained tightly restricted under censal suffrage, while local political participation expanded through municipal reform. Despite the regime's liberal constitutional framework, national suffrage remained limited to a small share of adult men, roughly 200,000 voters, or less than 1% of the population, thereby preserving the dominance of the propertied elites (Allier, 1976; Degrave et al., 2024). At the municipal level, by contrast, the 1831 law extended the franchise to a much broader group of male taxpayers. This distinction is central to the paper because the political variable studied here captures municipal suffrage exposure, not national democratization.

The Law on Municipal Organization of 1831 The Municipal Law of 1831 introduced triennial municipal elections and extended voting rights toapproximately 2.7 million citizens. The number of eligible municipal voters was determined by a population-based suffrage schedule, with the voting share slightly declining as commune population increased. Formally, in a commune of n inhabitants, voting rights were granted to the $\mathrm{V}(n)$ highest male taxpayers, where V is defined by the following schedule, drawn from Degrave et al. (2024):

$$\mathrm{V}(n) = \begin{cases} 30 & \text{if } n < 300 \\ 0.1 \times n & \text{if } 300 \leqslant n \leqslant 1,000 \\ \mathrm{V}(1,000) + (n - 1,000) \times 5\% & \text{if } 1,000 < n \leqslant 5,000 \\ \mathrm{V}(5,000) + (n - 5,000) \times 4\% & \text{if } 5,000 < n \leqslant 15,000 \\ \mathrm{V}(15,000) + (n - 15,000) \times 3\% & \text{if } n > 15,000 \end{cases}$$

The most notable feature of this schedule is the fixed floor of 30 voters for communes below 300 inhabitants: in the smallest communes, where 30 voters might represent the entire adult male taxpaying population, the effective franchise share could approach 100%, while in communes above 300 the share never exceeded 10%. Thus, the suffrage rule gave relatively greater electoral weight to smaller, more rural communes and mechanically generated cross-commune and cross-arrondissement variation in local political participation. The rural bias of the rule also reflected a political strategy of the regime, which viewed local participation in smaller communes as less threatening than broader political mobilization in major urban centers (Crook, 2021).

Political participation without decentralization Municipal suffrage expansion, however, did not amount to full local self-government. Even after the reform, mayors remained appointed and many municipal decisions continued to require prefectural approval, leaving local councils embedded in a centralized administrative hierarchy. In that sense, the reform expanded political participation and local politicization without fully redistributing political authority, a pattern consistent with what Degrave et al. (2024) describe as mass politicization without full democratization. At the same time, municipal elections appear to have played an important role in the politicization of rural France. As Tudesq (1982) notes, they provided a "first apprenticeship" in political life for peasants and artisans, and local electoral competition could weaken the dominance of large landowners in communal politics, catalyzing both class and political consciousness (Agulhon, 1983). Finally, elections encouraged the emergence of mass-oriented local policies, including education, public works, or the redistribution of communal property (Tanchoux, 2013; Montalbo, 2021). This institutional configuration is important for interpreting the empirical results: municipal suffrage may have increased local political engagement and altered local priorities, even if its reach remained limited in a centralized institutional setting.

2.4 Migration and arrondissement-level spillovers

The early 19th century marked the beginning of France's first rural exodus, with rising migration from rural areas toward towns as demographic pressure, early industrialization, and infrastructure improvements gradually reshaped population distribution. Historical evidence indicates that these flows were substantial (Duby & Wallon, 1976) but mostly short-distance, often linking rural communes to nearby urban centers rather than generating immediate long-distance relocation. Average moves were around 35 km at the beginning of the century and rarely exceeded 55 km by its end (Heffernan, 1989; Rosental, 2004). This pattern is one reason the arrondissement is an appropriate unit of analysis for the paper, since it is better suited than the commune to capture rural-to-nearby-town linkages and local labor-market spillovers.

Migration also provides a plausible mechanism linking education to local economic outcomes. If schooling improved literacy, mobility, or occupational matching, it could affect industrial wages not only within the commune where schools expanded, but also in nearby labor markets through population sorting and spillovers. More educated individuals were more likely to have both the means and aspirations to migrate, as they often associated life in the city with better job opportunities (Dupâquier, 1995). Moreover, households that placed a higher value on education may have been more likely to move toward municipalities offering better school provision and stronger local opportunities, reinforcing spatial differences in human capital and productivity. This mechanism is especially relevant in a context of uneven urbanization and local labor-market integration.

At the same time, recent evidence suggests caution in interpreting this channel at the commune level. Montalbo (2021) finds no statistically significant effect of the Guizot-induced schooling shock on long-run commune population growth between 1836 and 1911. Eventually, municipalities that were early adopters of primary schooling often saw higher rates of emigration, especially among youth and the skilled. One implication is that education may have affected mobility and labor-market sorting without generating uniform commune-level population growth: demographic effects may have been spatially uneven, concentrated in larger urban centers, or transmitted through broader local systems rather than through sustained growth in treated communes. These patterns motivate the arrondissement-level approach adopted in this paper and support the emphasis on spillovers rather than purely local treatment effects.

3 Data

This section describes the construction of the arrondissement-level dataset used throughout the paper, including the unit of analysis, sample coverage, source data, and the main harmonization and measurement issues. Because the empirical strategy combines cross-sectional and dynamic specifications, particular attention is given to spatial comparability over time and to the construction of variables from sources reported at different administrative levels.

3.1 Sample

The unit of analysis in this paper is the arrondissement, which provides a useful intermediate scale between municipalities and départements. This choice is motivated by the empirical focus of the paper: arrondissements are large enough to capture spillovers between rural communes and nearby urban centers, but still sufficiently disaggregated to preserve meaningful regional variation in schooling, suffrage exposure, and industrial outcomes. Administrative and population data are harmonized to the arrondissement level using municipality-level information from the Cassini database (EHESS), which provides historical administrative affiliations and population counts over time.

By the early 1830s, metropolitan France was organized into 86 départements, each subdivided into 2 to 5 arrondissements, for a total of 356 arrondissements in the baseline geography. Arrondissements served as administrative subdivisions for state functions (including taxation, education, and electoral administration) and were centered on chefs-lieux (préfectures and sous-préfectures). This administrative structure is central to the dataset construction because treatment exposure, controls, and outcomes are harmonized to arrondissement boundaries.

The analysis covers the period from 1830 to 1865, corresponding to the implementation of the 1831 and 1833 laws and the timing of the two industrial surveys used to measure wage outcomes. The paper focuses on nearly all metropolitan French arrondissements over this period, with a few exclusions driven by data availability and administrative comparability.In particular, Seine is excluded because Paris dominates its industrial labor market to a degree that would make it an extreme outlier in any cross-arrondissement comparison; Rhône is excluded because the destruction of part of its industrial archives renders survey coverage unreliable, as noted by Chanut et al. (2000); and Corsica is excluded due to incomplete administrative data coverage in the Cassini database.

To ensure comparability over time, I use the 1836 arrondissement geography (and corresponding département affiliations) as the reference spatial framework for the full sample period. This choice minimizes distortions from boundary changes and aligns the spatial framework with the timing of the laws and the available demographic data. Municipal-level data are then aggregated or harmonized to this baseline geography. The main territorial changes during the period-most notably the 1860 annexation of Nice, Savoie, and Haute-Savoie, and the transfer of Grasse from Var to Alpes-Maritimes-are handled by excluding the annexed territories and retaining Grasse in Var throughout the analysis.

I also collect information on administrative centers (préfectures and sous-préfectures) and retain their historical locations as institutional controls. These locations are largely stable over the sample period, with one notable exception in the Loire département, where the préfecture moved from Montbrison to Saint-Étienne in 1855. I retain Montbrison as the administrative center in the baseline coding because Saint-Étienne's designation followed its rapid economic growth (raising endogeneity concerns), and because administrative advantages associated with long-standing prefectural status are likely to persist in the short run.

3.2 Data sources and variable construction

The dataset combines archival and secondary sources on demography, education, industrial outcomes, institutions, and local economic conditions. For each source, I construct arrondissement-level variables used either as outcomes, treatment-exposure measures, or controls in the static and dynamic specifications. Unless otherwise noted, variables are observed directly at the arrondissement level or aggregated from municipality-level data to the 1836 arrondissement geography.

Demographics Population and administrative-unit data come from the Cassini database (EHESS), based on historical topographic and administrative records (Pelletier, 1990, 2002). I use municipality-level population and affiliation data to reconstruct arrondissements and départements, measure the distribution of communes by size, and aggregate exposure to the municipal suffrage schedule and the Guizot threshold. These demographic inputs are central to both the political-participation measure and the schooling-exposure instrument used later in the paper.

Education Education data come from two main sources: (i) the Statistique de l'Enseignement Primaire tables from the Statistique Générale de la France (via INSEE), and (ii) the digitized results of the 1833 Guizot Survey from the CRH. The Guizot Survey provides arrondissement-level information on primary schooling shortly after the 1833 law, including the number of schools, the number of communes with a school, and the number of male pupils. Following the interpretation used in related work, I treat the 1833 schooling data as a proxy for pre-reform educational differences, since the law's effects on school creation and enrollment are unlikely to have fully materialized at that date, following Blanc and Kubo (2026) and Montalbo (2021). In the regression analysis, the main education variable is the number of male pupils per 10,000 inhabitants.

Industry Industrial outcomes are built from the two industrial surveys conducted in 1839–1847 and 1860–1865 by the Statistique Générale de la France (compiled by Chanut et al., 2000 and digitized by Chambru, Henry, and Marx, 2024). These surveys provide plant-level information on wages and labor-force composition. I use them to construct arrondissement-level measures of average daily wages for male industrial workers and industrial employment, which serve as the main outcome and key controls in the empirical specifications. Arrondissement-level average daily wages are constructed as a weighted average of plant-level daily male wages, where each plant is weighted by its number of male workers, so that larger plants contribute proportionally more to the arrondissement average. Arrondissement aggregates are available for 357 arrondissements in the earlier survey and 373 in the later one.

Institutions Institutional controls combine information from multiple sources. I code the administrative status of communes (préfecture / sous-préfecture) using Cassini historical records, include a département-level proxy for religious presence (number of presbyteries, from Montalbo, 2021), and add historical institutional indicators inherited from the Ancien Régime (such as bishoprics and administrative jurisdictions) from Chambru, Henry, and Marx (2024). These variables are used to absorb persistent institutional differences across territories.

Economic resources and geography I include several proxies for local economic conditions and structural characteristics: cereal production per hectare (1815)[1], distance to coalfields (1812)[2], tax revenue from doors and windows per capita (1836)[3], the share of rural population (1836)[4], and median distance from Paris[5]. Some of these variables are available only at the département level (for example, distance to coalfields), while others are constructed from municipality-level information and aggregated to the arrondissement level. In the regressions, these measures serve as controls for market potential, resource endowments, fiscal capacity, and the broader economic environment.

3.3 Data limitations and harmonization challenges

This project combines data from roughly ten historical and modernized sources, which creates non-trivial harmonization and measurement challenges. The main issues concern identifier consistency, spatial comparability over time, and the mismatch between arrondissement-level and département-level reporting in some series. I address these issues systematically, but they remain important for interpreting the precision and scope of the empirical results.

Data harmonization A first challenge is that source datasets use different administrative identifiers (modern INSEE codes, historical codes, and project-specific conventions), and some contain inconsistent or incomplete labels. To merge these sources, I construct a correspondence system anchored on 1836 arrondissement names and reconcile spelling and naming differences across datasets. I then cross-check merge integrity by comparing related variables across sources wherever possible. These procedures substantially improve internal consistency and provide a high degree of confidence in the arrondissement-level harmonization. Nevertheless, as in most historical data work, residual matching error cannot be ruled out completely.

Spatial and temporal consistency Administrative boundaries and jurisdictional assignments can create spatio-temporal inconsistency in historical panel settings. The paper mitigates this risk by fixing the 1836 arrondissement and département geography and harmonizing municipal-level data to that baseline. This greatly reduces comparability problems, but not all

[1]Originally reported in the *Archives statistiques du Ministère des travaux publics, de l'agriculture et du commerce* (1837); data obtained from Montalbo (2021).

[2]Montalbo (2021). This measure is computed at the département level, as the distance between the préfecture and the nearest coalfield.

[3]Montalbo (2021). Originally from D'Angeville (1836). This tax was indexed on the number and size of doors and windows per dwelling and is used as a proxy for individual economic resources (Lepetit, 1986).

[4]Based on SGF population categories and obtained from Montalbo (2021). The SGF reports the rural share of the population living in towns below 3,000 inhabitants.

[5]Computed from municipality-level data in Chambru, Henry, and Marx (2024).

risks disappear: in principle, rare municipality transfers (especially involving large communes) could still affect population or industrial aggregates. The major territorial changes in 1860 are handled explicitly through sample restrictions and harmonization choices, as described above.

Modifiable Areal Unit Problem A more substantial issue for the dynamic specification is that education is observed at the arrondissement level in 1833 (Guizot Survey) but later education data are reported at the département level. This creates a classic modifiable areal unit problem (MAUP) for longitudinal arrondissement analysis (Openshaw, 1984). To restore comparability, I estimate arrondissement-level schooling values for later years using a prediction-and-scaling procedure that combines arrondissement characteristics with département totals, and then rescales predictions so they sum exactly to observed département aggregates. This approach preserves arrondissement-level variation while maintaining consistency with observed département totals, which makes the dynamic design feasible at the arrondissement level. At the same time, it is model-dependent and relies on the assumption that the relationship between predictors and schooling is sufficiently stable over time.

Uncertainty and remaining limitations The interpolation procedure preserves aggregate consistency, but uncertainty in the imputed arrondissement-level schooling values cannot be directly quantified because no arrondissement-level validation data are available in the imputed periods. In addition, the model may not fully capture spatial dependence across neighboring arrondissements. These limitations do not invalidate the analysis, but they imply that the dynamic IV estimates should be interpreted with caution; future work could improve this step by incorporating explicit spatial structure or hierarchical modeling in the interpolation procedure.

4 Empirical strategy

4.1 Motivation and design choices

Recent work has exploited the Guizot Law of 1833 and the Municipal Law of 1831 as sources of quasi-experimental variation at the commune level, comparing outcomes just above and just below each law's population threshold (Montalbo, 2021; Degrave et al., 2024). This paper takes a different approach, aggregating treatment exposure to the arrondissement level. Two considerations motivate this choice.

The first is the spatial unit of labor market integration. As documented in Section 2.4, 19th-century French migration was predominantly short-distance, with average moves of roughly 35 km at the beginning of the century and rarely exceeding 55 km by its end (Heffernan, 1989; Rosental, 2004). This pattern implies that the economic effects of schooling or political reforms in rural communes would have been transmitted partly to nearby urban centers rather than remaining contained within the originating commune. The arrondissement, centered on a chef-lieu and spanning the surrounding rural hinterland, is better suited than the commune to capture these rural-to-urban spillovers and the local labor market dynamics they produce.

The second is the need to study both laws jointly. No commune crosses both the 500-inhabitant Guizot threshold and the suffrage schedule of the Municipal Law in a way that allows their effects to be simultaneously identified at the commune level. Aggregating to the arrondissement, by contrast, generates meaningful cross-sectional variation in exposure to both reforms, since arrondissements differ in the share of their population living in communes above

500 inhabitants and in the share of their population eligible to vote under the suffrage schedule. This makes it possible to examine whether schooling expansion and political participation reinforced each other in shaping local economic outcomes, which is the paper's central question.

These advantages come at a cost that should be stated clearly. Commune-level regression discontinuity designs exploit the sharp discontinuity in treatment assignment at the population threshold, relying on local continuity of potential outcomes just above and just below the cutoff. Aggregating to the arrondissement level transforms this sharp discontinuity into a continuous share variable, losing the local randomization logic that justifies the RDD identifying assumption. The designs implemented in this paper therefore do not claim RDD-style identification. Instead, they rely on cross-arrondissement variation in treatment exposure, with identification resting on conditional exogeneity assumptions that are examined in Section 4.3.

The following subsection describes how the two key measures of treatment exposure are constructed from commune-level population data.

4.2 The two measures

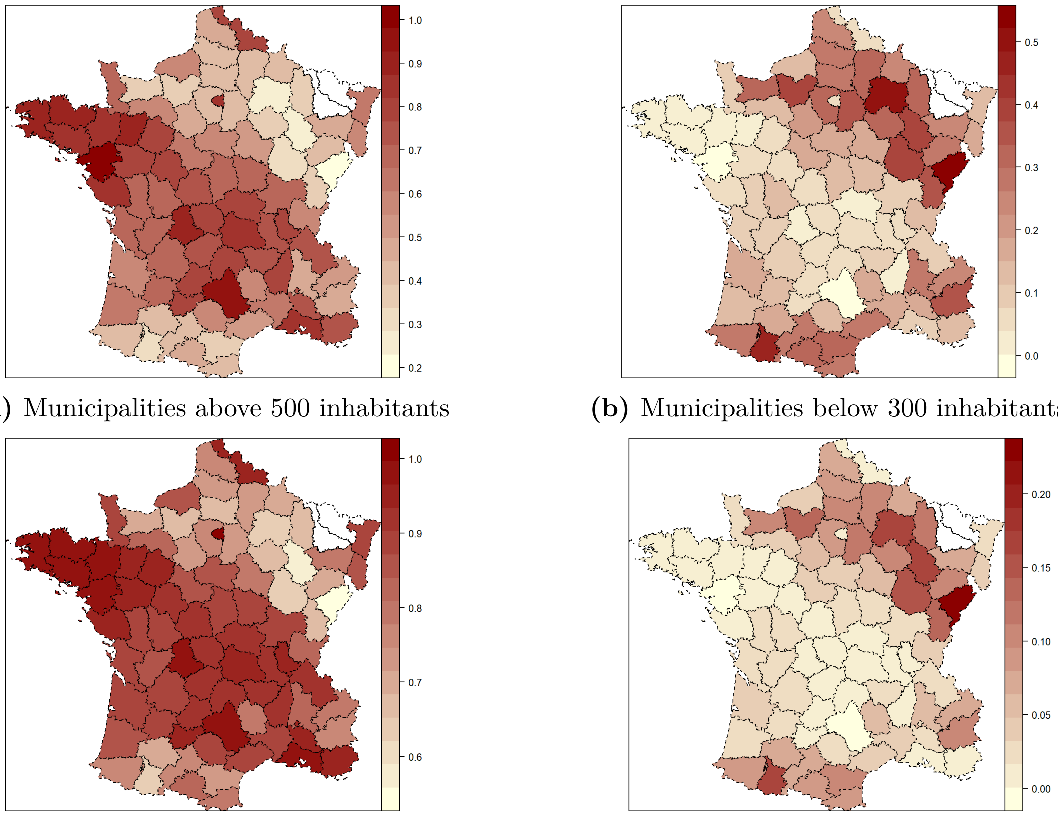
The Guizot schooling exposure measure The primary measure of exposure to the Guizot Law is the share of the arrondissement population living in non-chef-lieu communes above 500 inhabitants, computed from commune-level population data in the Cassini database for 1836. Chef-lieux are excluded from this construction for two reasons. First, as administrative centers with prefectural or sub-prefectural status, they disproportionately already had primary schools before the Guizot Law was enacted, meaning they were not meaningfully treated by its mandate; including them would inflate the treated share without capturing actual new school provision. Second, chef-lieux often account for a large share of arrondissement population, so their inclusion would allow the size of the urban center to drive the measure rather than the distribution of peripheral communes around the threshold. Excluding them ensures the instrument captures variation in Guizot-induced schooling expansion across the peripheral communes of the arrondissement, where the law's mandate generated actual new school provision. The 1836 population is used as the reference year because it aligns with the timing of the laws and the available demographic data, and because commune populations were sufficiently stable over the short period between the law's enactment in 1833 and the 1836 census to make this a reliable proxy for the distribution of communes at the time of the reform.

The municipal suffrage measure The measure of political participation is the share of the arrondissement population eligible to vote in municipal council elections under the Municipal Law of 1831. Following Degrave et al. (2024), I apply the suffrage schedule V(n), formally defined in Section 2.3, to commune-level population data from Cassini to compute the number of eligible voters in each commune. These are then aggregated to the arrondissement level and divided by total arrondissement population to produce the suffrage share. Chef-lieux are excluded from this construction for the same population dominance reason as the Guizot measure: larger communes have mechanically lower suffrage shares under the Municipal Law's schedule, and allowing the chef-lieu to dominate the arrondissement aggregate would obscure variation in political participation across peripheral communes. Unlike the Guizot measure, the suffrage variable is not used as an instrument but as a direct measure of political participation exposure, since the suffrage schedule mechanically determines eligible voters from population size without requiring a separate first stage.

Focus on male primary education Throughout the analysis, schooling is measured using male primary enrollment and pupil counts. This reflects the scope of the Guizot Law, which mandated boys' primary schools and made no parallel requirement for female primary education. As a result, the evolution of primary schooling over the 1830 to 1865 period studied here is driven predominantly by male education. The implications of this focus for the exclusion restriction, including the role of the Falloux Law of 1850, are discussed in Section 5.2.

4.3 The distribution of communes by population size

Figure 4 presents the distribution of French communes by population size in 1836, mapped at the arrondissement level. It shows both the share of municipalities and the corresponding population shares above 500 inhabitants and below 300 inhabitants. The geographic patterns are striking and systematic. Arrondissements where large communes dominate concentrate in the West, the Atlantic Southwest, and the Massif Central: in Brittany, Normandy, the bocage Vendéen, and much of the Auvergne and Limousin, the majority of communes are above 500 inhabitants. Arrondissements where small communes dominate concentrate in the Northeast: in Champagne, Lorraine, Alsace, Picardie, and the Paris Basin, communes below 300 inhabitants are the norm, representing in some départements more than half of all municipalities. These patterns are not the product of 19th-century administrative choices or economic conditions. They are the sediment of a territorial organization whose roots reach back to the medieval period.



(a) Municipalities above 500 inhabitants

(b) Municipalities below 300 inhabitants

(c) Population above 500 inhabitants

(d) Population below 300 inhabitants

Figure 4: Commune-size exposure in 1836 (data: Cassini)

To understand why, it is necessary to ask where communes come from. French communes were established by the National Assembly's law of December 14, 1789, which converted pre-existing parishes and village communities into the lowest level of administrative division. The process was not a clean redesign: local assemblies proposed boundaries that were submitted to departmental authorities, and the resulting communes broadly reflected the territorial organization that already existed. Before the Revolution, France had up to 60,000 parishes; the country ended up with approximately 44,000 communes, a figure that has remained roughly stable ever since. As the standard account puts it, French communes still largely reflect the division of France into villages or parishes at the time of the Revolution. The commune-size distribution of the early 19th century is therefore largely inherited from the parish geography of the Ancien Régime.

Parishes were gradually established between the 6th and the 12th centuries, formed around a church under the authority of the parish priest. The formation of parish territories was organized around the practical constraint of church accessibility: parishes were sized to encompass the community of inhabitants who could reach their church for regular sacraments, so that distance and topographic obstacles were structurally central to how parish boundaries formed (Iogna-Prat and Zadora-Rio, eds., 2005; Zadora-Rio, 2008). Topography mattered, but not because it mechanically produced a given population density. Parish boundaries reflected a church-catchment problem. A church had to be close enough for parishioners to attend mass, receive sacraments, and use the cemetery; at the same time, it had to draw on enough households, land, tithes, offerings, and seigneurial or ecclesiastical endowments to support the priest and maintain the church (Iogna-Prat and Zadora-Rio, eds., 2005; Germain, 1993). In fertile open-field plains, agricultural productivity and nucleated settlement made this constraint easier to satisfy at a small scale: households were already concentrated in village communities, and a smaller catchment could provide the material resources required for a parish. By contrast, mountains, forests, marshlands, bocage landscapes, and other zones of dispersed settlement often required larger or more irregular parish territories, since households and taxable resources were spread across hamlets, farms, and difficult terrain. Density is therefore better understood as a proximate expression of topography, settlement morphology, and agrarian productivity, not as a separate explanation. Secular and ecclesiastical authorities still mattered: lords and bishops promoted settlements, markets, churches, and territorial claims, but they operated within these constraints. The commune boundaries inherited by the Revolution consequently reflected a medieval geography of church catchments rather than a simple map of nineteenth-century population size.

4.4 Commune sizes, education, and identification

The previous subsection established that the commune-size distribution observed in 1836 is largely inherited from medieval parish geography. This subsection draws out the implications for identification. The argument has two parts: first, the commune-size distribution is the joint primitive that generates exposure to both the Guizot Law and the Municipal Law of 1831; second, the same distribution is spatially correlated with pre-reform schooling and broader development patterns. Together, these features mean that the variation this paper exploits is not as clean as a typical threshold-based design would assume, and that the identification challenge is itself part of what the paper has to say.

The commune-size distribution as a joint primitive. Both treatment measures are

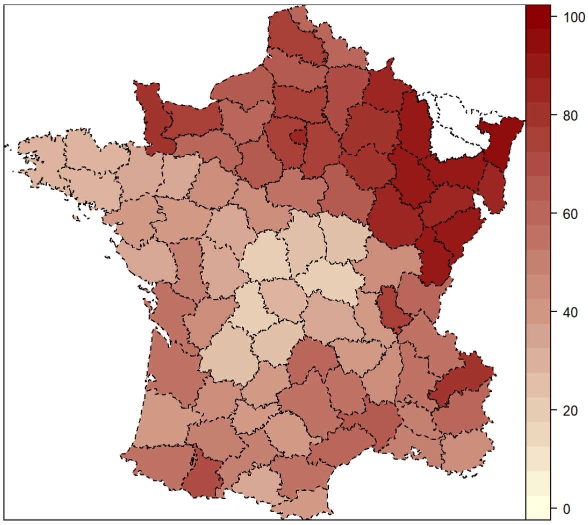


Figure 5: Percentage of conscripts (20 y/o) able to read and write, 1836–40 (school years: 1822–33). Data: SGF.

deterministic functions of the same underlying variable. The Guizot exposure measure aggregates commune populations above the 500-inhabitant threshold; the municipal suffrage measure applies the V(n) schedule, which has its central kink at 300 inhabitants and a 10% ceiling beyond. Arrondissements with many small communes therefore have, mechanically, lower Guizot exposure and higher suffrage shares. Conversely, arrondissements with many large communes have higher Guizot exposure and lower suffrage shares. The two treatment shares are not independent sources of variation: they are two views of the same distribution. This has two consequences. The first is statistical: in any specification that includes both treatments, the coefficient on each is identified off the residual variation in commune size after partialling out the other, which is limited. The second is conceptual: any unobserved variable that shapes the commune-size distribution is a potential confounder for both treatments simultaneously, so the usual move of arguing that one treatment is plausibly exogenous conditional on controls does not extend cleanly to the joint design.

Spatial alignment with pre-reform development. The commune-size distribution is not only structured by medieval parish geography; it is also spatially correlated with pre-reform levels of schooling and development. Comparing Figure 4 with Figure 5, the alignment is visible: arrondissements with high shares of population in communes above 500 inhabitants are concentrated in the West, the Atlantic Southwest, and the Massif Central, which are also the arrondissements with the lowest pre-reform conscription literacy. The 1836–40 conscript cohorts attended school between roughly 1822 and 1833, so this figure reflects the pre-Guizot schooling landscape. Northern and Northeastern arrondissements, characterized by many small communes, were also the most literate. The spatial pattern closely matches what Furet and Ozouf (1977) describe as the "triangle d'arriération Brest-Guéret-Bayonne", a band of comparatively low schooling stretching across western and central France that persisted into the 1860s. The implication is that the Guizot Law mechanically applied with greatest intensity in arrondissements that were also, on average, the least developed in educational terms prior to the reform.

Implications for the static and dynamic designs. For the static cross-sectional design, the alignment creates a direct threat to interpretation. A negative association between Guizot exposure and pre-reform schooling means that any cross-sectional positive correlation between schooling and outcomes will partly capture the contrast between (i) arrondissements with many small communes, high pre-reform schooling, and high baseline development, and (ii) arrondissements with many large communes, lower pre-reform schooling, and lower baseline development. Conditioning on observable correlates of development reduces this risk but cannot eliminate it: the commune-size distribution is correlated with characteristics, such as settlement dispersion and pre-modern institutional history, that are imperfectly captured in the available data.

For the dynamic instrumental-variables design, the same alignment poses a more specific threat. The instrument, the share of arrondissement population in non-chef-lieu communes above 500 inhabitants, is by construction the variable that determines Guizot exposure. Its first-stage relevance is straightforward: arrondissements with more population above the threshold should have seen larger schooling expansion under the law. But its exclusion from the second stage requires that, conditional on controls, the share above 500 affects wage growth only through schooling. If the share above 500 also captures persistent settlement and development characteristics that independently drive wage growth, the exclusion restriction is violated. The empirical question is whether the available controls absorb enough of this variation to make the residual variation in the instrument plausibly exogenous.

The identifying assumption. The empirical work that follows relies on the conditional-exogeneity assumption that, given the set of controls described in Section 3.2, the residual variation in the two treatment measures is unrelated to unobserved determinants of industrial wages. This assumption is strong, and the discussion above identifies two specific reasons it is not innocuous: the joint dependence of both treatments on the commune-size distribution, and the spatial alignment of that distribution with pre-reform development. Throughout the empirical sections, I treat the estimates as conditional on this assumption and acknowledge that residual confounding through the commune-size channel cannot be ruled out.

From limitation to contribution. The identification challenge is genuine but it is also informative. The fact that two of the most studied reforms of the July Monarchy turn on the same underlying variable, and that this variable is the sediment of medieval parish geography, is not a feature of the data that future work can easily design around. It is a structural feature of the setting. Subsequent work on these reforms, including at the commune level, faces the same constraint in a different form: the population of a commune in 1836 is itself a function of which medieval parish it descends from. Making this constraint explicit serves two purposes here. It calibrates the strength of the empirical claims this paper can defend, and it identifies a methodological challenge that any threshold-based study of 19th-century French institutions will need to confront. Section 6.3 returns to what this implies for future identification strategies.

4.5 The static and dynamic designs

The two empirical designs implemented in this paper rest on the framework laid out in Sections 4.1–4.4 and answer two different questions, with different identifying assumptions and different interpretations.

The *static* design is a cross-sectional regression of average male industrial wages in the 1840s on pre-reform schooling, baseline municipal suffrage exposure, their interaction, and a set of arrondissement and département controls. Pre-reform schooling is measured by the number of male pupils per 10,000 inhabitants in 1833, on the convention used elsewhere in the literature (Montalbo, 2021; Blanc and Kubo, 2026) that the Enquête Guizot of autumn 1833 captures the schooling landscape before the law could materially reshape it. The static design asks whether arrondissements with higher pre-reform schooling and higher municipal political participation also recorded higher industrial wages in the 1840s. It does not identify a causal effect of either variable: the interpretation is that of a conditional association, with the conditioning set discussed in Section 4.4. The wage outcome is measured in the 1840s precisely because, by that decade, the post-Guizot schooling expansion had not yet had time to reach the labor market, given the 6–13 schooling-age window and the timing of post-1834 school construction. The static estimates therefore speak to the pre-Guizot configuration, not to the law's policy-induced effects.

The *dynamic* design is a two-stage least squares regression of arrondissement-level wage growth between the 1840s and the 1860s on the change in male pupils per 10,000 inhabitants over the same window, instrumented by the share of population in non-chef-lieu communes above the 500-inhabitant Guizot threshold. The first stage isolates the component of the schooling change driven by the policy threshold; the second stage uses this component to estimate the medium-run effect of policy-induced schooling expansion on wage growth. The exclusion restriction, as discussed in Section 4.4, requires that the share above 500 affects wage growth only through schooling, conditional on the included controls; this assumption is examined alongside the results in Section 5.2. Unlike the static design, the dynamic design does identify a causal parameter, but a local one: a Wald-style estimate for the population of arrondissements whose schooling expansion was driven by the Guizot mandate. The estimate does not necessarily generalize to arrondissements where schooling expanded for reasons unrelated to the law.

Two design choices are common to both specifications. First, in both cases, the political-participation variable is the suffrage share constructed from the $\mathrm{V}(n)$ schedule, not an instrumented quantity: the suffrage share is mechanically determined by commune populations and the legal rule, so a separate first stage is not needed. Second, in both cases, the chef-lieu of each arrondissement is excluded from the construction of the Guizot and suffrage measures, for the reasons given in Section 4.2: chef-lieux are administrative centers that disproportionately had schools before the law and would dominate any population-weighted aggregate. The two designs therefore use the same treatment-exposure measures, applied to two different outcome margins (levels in the 1840s, log changes between the 1840s and the 1860s).

The two designs are complementary rather than redundant. The static design provides a description of how schooling, political participation, and industrial wages were related at the moment when the Guizot reform began to take effect. The dynamic design asks whether the part of the schooling expansion that was policy-induced moved wages over the following two decades. Comparing them makes it possible to ask whether the cross-sectional schooling-wage association reflects a causal margin reachable by educational policy in this period, or whether it captures longer-run features of arrondissement development that the policy did not move within the available time window.

5 Results

5.1 Pre-reform schooling, political participation, and wages

Empirical strategy The static design estimates the cross-sectional association between pre-reform schooling, baseline municipal suffrage exposure, and average male industrial wages in the 1840s. The outcome window is chosen so that the post-Guizot schooling expansion has not yet had time to reach the labor market: the law's implementation peaked between 1834 and 1837 (Blanc and Kubo, 2026), and given the 6–13 schooling-age window, cohorts educated under the new schools could not enter the industrial workforce in meaningful numbers before the 1850s. The 1839–47 wages therefore reflect the configuration of labor markets before the law had time to reshape them through its effect on enrollment. Political participation, by contrast, is taken as an active exposure: the Municipal Law of 1831 had been in force for nearly a decade by the time of the survey, and its effects on local political life, where present, would have had time to materialize.

For schooling, the variable is the number of male pupils per 10,000 inhabitants in 1833, drawn from the Enquête Guizot. As discussed in Section 3.2, this measure is treated as a baseline indicator of pre-reform educational provision, following the convention in Montalbo (2021) and Blanc and Kubo (2026) that the autumn 1833 survey predates the law's effect on schooling. For political participation, the variable is the share of arrondissement population (excluding the chef-lieu) eligible to vote under the V(n) schedule, applied to 1836 commune populations. The construction and rationale for excluding chef-lieux are described in Section 4.2; the structural concerns that link both measures to the underlying commune-size distribution are developed in Section 4.4.

The specification is

$$\begin{aligned}\overline{Wage\_3947_i} &= \beta_0 + \beta_1\, Pupils_i + \beta_2\, Voters_i + \beta_3\,(Pupils_i \times Voters_i) \\ &\quad + \beta_4\, Indus_i + \beta_5\, Demo_i + \beta_6\, Inst_i + \beta_7\, Econ_i + \varepsilon_i,\end{aligned}$$

where $\overline{Wage\_3947_i}$ is the plant-weighted average daily wage of male industrial workers in arrondissement i, computed from the Industrial Survey of 1839–47. $Pupils_i$ and $Voters_i$ are the pre-reform schooling and suffrage exposure measures defined above. The interaction $Pupils_i \times Voters_i$ tests for complementarity between human capital and local political participation. The vectors $Indus_i$, $Demo_i$, $Inst_i$, and $Econ_i$ contain the controls described in Section 3.2: industrial structure (share of industrial workers, number of male industrial workers, log distance to coalfields); demographics (share of rural population); institutional presence (presbytery counts, prefecture indicator); and economic resources (tax on doors and windows per capita, cereal returns per hectare). The error term ε_i captures unobserved arrondissement-level determinants of wages. Variables of interest are centered to ease interpretation of the interaction; unscaled specifications without an interaction yield similar coefficients on the main effects.

Results

The table below reports the baseline OLS estimates. Variables of interest are centered to make the interaction term interpretable; specifications without centering and without the interaction yield similar coefficients on the main effects. Standard errors clustered at the département level are reported in parentheses; this is the preferred specification because several controls are observed at the département level and within-département unobservables are likely correlated. Sensitivity to the standard-error choice is reported in the robustness table below.

Table 1: Static OLS: education, political participation, and male industrial wages (1839–1847)

	Male wage (1)
Male pupils per 10k (1833, cent.)	0.036**
	(0.012)
Share of voters (1836, cent.)	623.3
	(476.2)
Pupils × Voters	-0.912
	(0.817)
Industrial controls	Yes
Demographic controls	Yes
Institutional controls	Yes
Economic controls	Yes
Observations	341
Adjusted R^2	0.214

Notes: The dependent variable is the plant-weighted average daily wage of male industrial workers in arrondissement i. Variables of interest are centered at their sample means. Standard errors clustered at the département level are in parentheses. Intercept estimated but not reported. Full coefficient estimates are reported in Appendix Table A1.
Significance: ***$p < 0.001$, **$p < 0.01$, *$p < 0.05$, $\cdot\, p < 0.1$.

The coefficient on pre-reform schooling is positive and statistically significant at the 1% level under département-clustered standard errors. A one-unit increase in male pupils per 10,000 inhabitants in 1833, evaluated at the sample mean, is associated with an increase in average daily industrial wages of about 0.04 francs. The coefficient is stable across the inclusion of the interaction term and additional controls (shown in the robustness table below). The sign and magnitude are consistent with what the cross-sectional cliometric literature on 19th-century France has found: arrondissements with higher schooling provision had higher industrial productivity, as measured by wages (Squicciarini and Voigtländer, 2015). The coefficient should therefore be read more narrowly than a causal return to primary schooling. The timing argument rules out one channel, namely the post-Guizot expansion affecting 1840s wages through newly educated cohorts. It does not rule out reverse causality in the baseline stock itself: arrondissements with stronger industrial activity, higher expected wages, or greater municipal fiscal capacity may already have invested more in schooling before 1833. This is empirically plausible in the French setting. Montalbo (2020, 2021) shows that industrial activity and richer, growing municipalities shaped the provision and financing of primary schooling before the Guizot Law, while Franck and Galor (2022) find that early industrialization itself promoted human-capital formation. The static coefficient is therefore best interpreted as a reduced-form association between the inherited geography of schooling and the geography of industrial productivity, not as evidence that 1833 pupil density caused higher 1840s wages. Section 6.1 returns to this interpretation.

The coefficient on the suffrage share is positive but not statistically significant under département-clustered standard errors, and the interaction term between schooling and suffrage is negative and insignificant. Taken at face value, this is a null result for the local-political-participation channel in the cross-section. Two considerations matter for how to read it. First, the constitutional context constrains what the channel could deliver. As emphasized by Tanchoux (2013) and Degrave et al. (2024), municipal councils under the July Monarchy operated within a centralized administrative hierarchy: mayors were appointed, many council decisions required prefectoral approval, and the fiscal autonomy of communes was limited. To the extent that political-participation effects on industrial wages must transit through changes in local public-goods provision or institutional accountability, the mechanism was operating with reduced amplitude. Second, the channel need not have been institutional in this narrow sense to be present. Municipal elections in the July Monarchy were, as Tudesq (1982) and Crook (2021) document, organized political events that involved campaigning, mobilization, and brokered exchanges between local notables, including industrial employers, and the body of taxpaying voters. Where industrial patrons were themselves candidates or backed candidates, employment terms could plausibly enter the implicit electoral bargain. Such a channel would have differential reach across the franchise distribution: in communes below 300 inhabitants, where the $\mathrm{V}(n)$ floor of 30 voters could include taxpaying foremen and skilled workers, worker-voter bargaining would be possible; in larger communes, where $\mathrm{V}(n)$ limited the franchise to wealthier taxpayers, the relevant exchange would be among notables themselves. The cross-arrondissement specification averages over these heterogeneous configurations and is therefore not well-suited to detect them. The null result reported here is consistent with both the absence of an aggregate effect and the presence of a channel whose footprint is not visible in arrondissement-level industrial wage means. Section 6.2 returns to this distinction.

The control variables behave broadly as expected. The share of industrial workers in the arrondissement population is negatively associated with wages, consistent with downward pressure on average pay in labor-intensive local industries; the count of male industrial workers enters positively, consistent with agglomeration. The rural population share is negatively signed, and tax revenue from doors and windows per capita, a proxy for individual economic resources following Lepetit (1986), is positively associated with wages. Cereal returns and the log distance to coalfields are not statistically significant in this specification but are retained as controls for structural heterogeneity. The prefecture indicator is positive and significant. The specification explains roughly 21% of the cross-sectional variation in industrial wages.

Robustness Additional controls were retained on the basis of their contribution to model fit, measured by the Akaike Information Criterion and adjusted R^2, conditional on the inclusion of schooling, suffrage, and their interaction. Variance inflation factors indicate no problematic collinearity. The table below compares conventional OLS standard errors, heteroskedasticity-robust standard errors, and standard errors clustered at the département level. The latter accounts for spatial correlation in policy exposure and unobserved shocks within départements, which is the natural clustering unit given that several controls are observed at département level.

Table 2: Static OLS: inference robustness of key coefficients

	Conventional	HC1 robust	Clustered
Male pupils per 10k (1833, cent.)	0.0355***	0.0355***	0.0355**
	(0.0101)	(0.0094)	(0.0115)
Share of voters (1836, cent.)	623.3·	623.3	623.3
	(373.5)	(406.8)	(476.2)
Pupils × Voters	-0.912	-0.912	-0.912
	(0.741)	(0.722)	(0.817)
Industrial controls	Yes	Yes	Yes
Demographic controls	Yes	Yes	Yes
Institutional controls	Yes	Yes	Yes
Economic controls	Yes	Yes	Yes
Observations	341	341	341

Notes: Columns report the same specification under alternative standard-error estimators. Standard errors are in parentheses. Clustered standard errors are clustered at the département level. Full robustness estimates are reported in Appendix Table A2.
Significance: $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$, $^{\cdot}p < 0.1$.

The coefficient on pre-reform male pupils per 10,000 inhabitants remains positive and significant across all three inference choices, including under département-clustered standard errors that allow for within-département correlation in unobservables. The coefficient on suffrage share, marginally significant under conventional standard errors, loses significance once robust or clustered standard errors are used; the interaction is insignificant throughout. The point estimates themselves are stable across columns, so what changes is the precision of inference rather than the magnitude of the association.

Two limitations of this design should be carried into the dynamic specification that follows. First, the static design recovers a conditional association, not a causal effect: baseline schooling may itself reflect pre-1833 development and industrial demand, and, as noted in Section 4.4, the schooling-suffrage-wage relationship is also identified from variation correlated with the underlying commune-size distribution. Second, the suffrage null is silent on heterogeneity. The static specification averages over the full distribution of arrondissements, including those in which the franchise reached worker-voters and those in which it did not. The dynamic design in Section 5.2 turns to the question of whether policy-induced changes in schooling, isolated by the Guizot threshold, moved wages between the 1840s and the 1860s.

5.2 Policy-induced schooling expansion and wage growth

Empirical strategy The dynamic design asks whether the part of the schooling expansion that was driven by the Guizot mandate moved industrial wages between the 1840s and the 1860s. Two design choices follow directly from the structural concerns developed in Section 4.4. First, the regression is run in changes rather than in levels: the cross-sectional negative correlation between Guizot exposure and pre-reform schooling persists across the period (the share of population in non-chef-lieu communes above 500 inhabitants remains negatively correlated with average male industrial wages even in 1860–65), but the policy-induced change in schooling can plausibly be isolated from these persistent differences if the instrument is uncorrelated with arrondissement-specific wage trends conditional on controls. Second, the instrument used is the share of arrondissement population in non-chef-lieu communes above 500 inhabitants in 1836, the same exposure measure as in the static design, but here applied to the medium-run change in schooling rather than the level.

The outcome variable is the change in the logarithm of the average male industrial daily wage between the two industrial surveys, $\Delta \log(\overline{\text{Wage}_i}) = \log(\overline{\text{Wage\_6065}_i}) - \log(\overline{\text{Wage\_3947}_i})$. The endogenous regressor is the change in male pupils per 10,000 inhabitants between 1833 and 1850, ΔPupils_i, where the 1850 arrondissement-level pupil counts are constructed by the prediction-and-scaling procedure described in Section 3.3. The maintained assumption is that, across this window, the bulk of the schooling expansion was driven by Guizot-mandated school construction rather than by autonomous local demand: the law's enforcement peaked in the late 1830s, schools that opened in this period had time to fill, and the 1850 measurement predates the Falloux Law of 1850, whose implementation is discussed below. The two-stage least squares specification is

$$
\begin{aligned}
\text{First stage:} \quad & \Delta\text{Pupils}_i = \pi_0 + \pi_1\,\text{Sharepop500nc}_i + \pi_2\,\text{Vote}_i \\
& \qquad + \pi_3\,\text{Indus}_i + \pi_4\,\text{Demo}_i + \pi_5\,\text{Inst}_i + \pi_6\,\text{Econ}_i + \nu_i \\
\text{Second stage:} \quad & \Delta \log(\overline{\text{Wage}_i}) = \beta_0 + \beta_1\,\widehat{\Delta\text{Pupils}}_i + \beta_2\,\text{Vote}_i \\
& \qquad + \beta_3\,\text{Indus}_i + \beta_4\,\text{Demo}_i + \beta_5\,\text{Inst}_i + \beta_6\,\text{Econ}_i + \varepsilon_i,
\end{aligned}
$$

where Sharepop500nc_i is the share of arrondissement population in non-chef-lieu communes above 500 inhabitants in 1836, and Vote_i is the baseline 1836 suffrage share. The control vectors are slightly modified relative to the static design to suit a growth specification: Indus_i now includes the logarithm of average male industrial wage in 1839–47 and the initial industrial employment share, Demo_i includes the log change in population between 1836 and 1861, Inst_i contains the prefecture indicator and presbytery counts, and Econ_i contains the 1836 tax on doors and windows per capita, 1815 cereal returns, and the median distance from Paris (as a market-access proxy). The interaction between schooling and suffrage is omitted from the dynamic specification because the design instruments only one of the two variables, so identifying a heterogeneity in the schooling effect by suffrage exposure would require either a second instrument or strong functional-form assumptions; the static design provides the available cross-sectional evidence on this margin.

The instrument and the exclusion restriction. The first-stage relevance of the instrument is straightforward. As documented in Section 4.4 and visualized in the school-provision time series in Figure 3, the Guizot Law generated a sharp expansion of school construction concentrated in 1834–37, and that expansion was, by the law's design, mechanically tied to the share of population in communes above 500 inhabitants. The first-stage results below confirm this.

The exclusion restriction is the more demanding requirement: conditional on the controls, the share of population in non-chef-lieu communes above 500 inhabitants must affect wage growth only through its effect on policy-induced schooling expansion. Three potential violations deserve explicit treatment.

The first is the structural concern developed in Section 4.4: the instrument is a deterministic function of the commune-size distribution, which is itself spatially correlated with persistent settlement and development characteristics. If those characteristics independently drive wage growth, the exclusion restriction fails. The controls included absorb the most obvious correlates (rurality, fiscal capacity, market access, industrial structure, distance to coalfields), but residual correlation with unobserved arrondissement dynamics cannot be ruled out.

The second is compound treatment at the same population variable. The Guizot Law of 1833 used the 500-inhabitant threshold for boys' primary schools; the Falloux Law of 1850 introduced a separate 800-inhabitant threshold for girls' primary schools. As Eggers et al. (2018) emphasize, designs that exploit population thresholds are vulnerable to compound treatment whenever multiple policies are scaled in the same population variable. In this setting, the concern is that the share of population in non-chef-lieu communes above 500 inhabitants is mechanically correlated with the share above 800 inhabitants, so part of the variation attributed to the Guizot mandate may instead reflect post-1850 female schooling expansion. I follow Montalbo (2021) in arguing that this channel is unlikely to bias the second stage materially within the 1839–1865 outcome window: girls who entered school under the Falloux Law in the early 1850s would have completed primary education only in the early 1860s, and most would not have entered the male-only industrial labor market measured in the second survey. The argument is one of timing, not absence of mechanism. Indirect effects through household decisions and female labor supply cannot be ruled out, and a fuller exploration of female schooling effects would require an outcome measure not restricted to male industrial wages.

The third is selection or manipulation around the population threshold. Following Montalbo (2021), I treat this as unlikely in the period of enforcement: the law was enforced within years of the 1836 census, and commune populations were not actively manipulated by local authorities to enter or avoid the school-construction mandate. The commune-size distribution observed in 1836 is therefore taken as exogenous to the law's enforcement, though, as Section 4.4 develops, it is not exogenous to deeper territorial features.

Inference under weak instruments. The first-stage Kleibergen-Paap statistic reported below is above the Stock and Yogo (2005) conventional threshold of 10 but well below the more recent benchmark suggested by Lee, McCrary, Moreira, and Porter (2022), who show that, for the standard 5% size of a t-test on the second-stage coefficient, the appropriate first-stage F must exceed approximately 104.7. By this updated standard the instrument is borderline. The second-stage point estimate reported below is precisely close to zero, so the conclusion drawn from the IV does not turn on which critical value is used: the null result is robust to a more conservative weak-instrument benchmark and would not become significant under any plausible inference correction. Where this matters is for the interpretive language: I report the IV estimate as a precise null rather than as a confidence interval that could plausibly include large effects.

What the design identifies. Under the maintained assumptions, the IV identifies a local average treatment effect (LATE) in the Imbens and Angrist (1994) sense: the effect of an arrondissement-level increase in male pupils per 10,000 inhabitants on log wage growth, for the subset of arrondissements whose schooling expansion was actually driven by the Guizot mandate. Compliers, in this design, are arrondissements with many communes near the 500-inhabitant threshold whose 1840s–50s schooling growth reflects new schools opened under the law. The

LATE need not coincide with the effect that would obtain if schooling expanded in already-developed arrondissements with low Guizot exposure, or in arrondissements with the largest Guizot exposure (where most communes were already well above the threshold). Section 6.1 returns to what this scope condition means for the substantive interpretation of the null.

Results

The table below reports the OLS, 2SLS, and first-stage estimates of the effect of changes in male pupils per 10,000 inhabitants on log wage growth between 1839–47 and 1860–65. The OLS coefficient on the change in pupils is small, negative, and statistically insignificant. The 2SLS estimate is also small, slightly less negative, and similarly insignificant, with a larger standard error reflecting the loss of efficiency from instrumenting. Taken together, the two estimates indicate no detectable medium-run effect of policy-induced schooling expansion on industrial wage growth. The coefficient on the suffrage share is also insignificant in both columns, consistent with the static design.

Table 3: Dynamic IV: effect of male pupils on industrial wage growth (1839–1865)

	First stage ΔPupils (1)	OLS $\Delta \log(\overline{\text{Wage}})$ (2)	2SLS $\Delta \log(\overline{\text{Wage}})$ (3)
Share pop. $\geq$ 500 (1836, non-chef-lieu)	542.95***		
	(151.90)		
Δ Pupils per 10k (1833–1850)		-0.0000508	-0.0000314
		(0.0000537)	(0.000279)
Share voters (1836)	-1661.4	0.734	0.912
	(2414.4)	(1.287)	(2.825)
Industrial controls	Yes	Yes	Yes
Demographic controls	Yes	Yes	Yes
Institutional controls	Yes	Yes	Yes
Economic controls	Yes	Yes	Yes
Observations	341	340	340
Adjusted R^2	0.461	0.618	0.618
First-stage partial F		12.70	
Stock-Yogo 10% threshold		16.38	
Wu-Hausman p-value		0.944	

Notes: The instrument in columns (1) and (3) is the share of arrondissement population in non-chef-lieu communes above 500 inhabitants in 1836. Standard errors are in parentheses. Intercepts estimated but not reported. Full estimates are reported in Appendix Table A3.
Lee et al. (2022) imply a tF benchmark of approximately 104.7 for conventional 5% inference on the 2SLS coefficient.
Significance: $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$, $^{\cdot}p < 0.1$.

The first-stage results confirm that the instrument is a strong predictor of the change in male pupils per 10,000 inhabitants: the share of population in non-chef-lieu communes above 500 inhabitants enters positively and is significant at the 0.1% level, with a partial F-statistic of 12.7. As shown earlier in Figure 3, the national expansion of primary schools was concentrated in the late 1830s. The instrument captures the cross-arrondissement variation in exposure to this national expansion, and the first stage indicates that arrondissements with more population above the 500-inhabitant threshold did indeed see larger increases in pupils per 10,000 inhabitants over the 1833–1850 window.

As noted in the empirical strategy, the first-stage F exceeds the Stock and Yogo (2005) conventional rule-of-thumb of 10 but is well below the more recent Lee, McCrary, Moreira, and Porter (2022) benchmark of approximately 104.7 for valid 5%-level inference on the second-stage coefficient. The instrument is therefore borderline by modern standards. This matters for the interpretation of the second-stage standard error but does not change the second-stage conclusion: the IV point estimate is precisely close to zero, so the null is robust to a more conservative inference benchmark.

The remaining controls behave largely as expected. Log population growth between 1836 and 1861 is a strong positive predictor of wage growth, consistent with demographic dynamism in industrially expanding arrondissements. The baseline share of industrial workers is positively associated with subsequent wage growth, consistent with agglomeration. The initial log wage in 1839–47 enters negatively and significantly, indicating mean reversion: arrondissements with higher 1840s wage levels saw slower wage growth into the 1860s, consistent with a convergence pattern across the industrial landscape. Distance to Paris enters negatively, consistent with a market-access channel; the 1836 tax on doors per capita enters positively, consistent with the fiscal-capacity reading of this proxy. The prefecture indicator is positive but not statistically significant in this specification.

Robustness Additional controls are selected by the same Akaike Information Criterion and adjusted-R^2 procedure used in the static design, conditional on the inclusion of schooling, suffrage, and the instrument. The table below reports 2SLS estimates under conventional, heteroskedasticity-robust, and département-clustered standard errors. The clustering choice is again motivated by the fact that several controls are measured at the département level and that within-département unobservables are likely correlated.

Table 4: Dynamic IV: inference robustness of key coefficients

	Conventional	HC1 robust	Clustered
Δ Pupils per 10k (1833–1850)	-0.0000314	-0.0000314	-0.0000314
	(0.000279)	(0.000347)	(0.000373)
Share voters (1836)	0.912	0.912	0.912
	(2.825)	(3.430)	(3.525)
Industrial controls	Yes	Yes	Yes
Demographic controls	Yes	Yes	Yes
Institutional controls	Yes	Yes	Yes
Economic controls	Yes	Yes	Yes
Instrument	Yes	Yes	Yes
Observations	340	340	340
First-stage partial F		12.70	
Wu-Hausman p-value		0.944	

Notes: Columns report the same 2SLS specification under alternative standard-error estimators. Standard errors are in parentheses. Clustered standard errors are clustered at the département level. Full robustness estimates are reported in Appendix Table A4.
The instrument is the share of arrondissement population in non-chef-lieu communes above 500 inhabitants in 1836.

The coefficients on the change in male pupils and on the suffrage share remain statistically insignificant across all three inference choices, including under département-clustered standard errors. The point estimate on schooling is small and stable across columns; what changes is the precision of inference. Among the controls, log population growth, the baseline industrial share, the initial log wage, distance to Paris, and the tax-on-doors proxy retain their significance across specifications. The 1815 cereal-returns variable loses significance under robust and clustered standard errors, indicating that its baseline significance was driven by the homoskedasticity assumption.

Convergent OLS and IV evidence on the schooling-wage relationship. The Wu-Hausman test reported in the diagnostics fails to reject the null that the change in pupils is exogenous in this specification ($F(1, 329) = 0.005$, $p = 0.94$). The literature on 2SLS commonly reads such a result as license to prefer the more efficient OLS estimator, but two qualifications matter here. First, the Wu-Hausman test has low power in this setting: the comparison is between an OLS estimate that is small and a 2SLS estimate that is also small but considerably less precise, so the difference between them is hard to detect statistically regardless of whether endogeneity is present. Failure to reject is therefore weak evidence in favor of exogeneity, not a positive finding. Second, and more important for interpretation, OLS and 2SLS deliver convergent point estimates: both are small, negative, and statistically indistinguishable from zero. Under the maintained instrument validity, the IV identifies the LATE for arrondissements whose schooling expansion was driven by the Guizot mandate; the OLS, in contrast, exploits the full cross-arrondissement variation in schooling changes regardless of source. That these two estimators converge on a precise null suggests the wage growth between the 1840s and the 1860s did not respond meaningfully to medium-run changes in primary-pupil counts within the available data, on either the policy-induced margin or the broader margin of variation. The result is more informative as a description of the schooling-wage relationship in this period than as a verdict on the endogeneity of schooling.

Limitations from the MAUP interpolation. The change in male pupils per 10,000 inhabitants between 1833 and 1850 uses the 1833 Guizot Survey at the arrondissement level and an arrondissement-level estimate for 1850 constructed by the prediction-and-scaling procedure described in Section 3.3. The interpolation preserves département aggregates exactly but cannot be validated against held-out arrondissement-level 1850 data, which do not exist. Two limitations follow. First, uncertainty in the imputed 1850 pupil counts is not propagated through the 2SLS standard errors, which therefore understate the true standard error of the second-stage coefficient. Correcting this would require either a bootstrap that resamples the imputation step or an explicit measurement-error model; both are feasible extensions for future work. Second, the prediction model imposes that the relationship between arrondissement-level predictors and schooling is stable between 1833 and 1850, an assumption that the Guizot expansion may itself violate if the law disrupted the pre-reform relationship between arrondissement characteristics and schooling supply. A useful diagnostic, also for future work, would be to run the dynamic specification at the département level, where 1850 schooling is observed directly: department-level estimates would lose granularity but would not depend on the interpolation. The robustness of the central null result to this check is something I cannot establish here, and the current estimates should be read with this caveat in mind.

6 Discussion

6.1 Schooling and labor market outcomes

The static and dynamic designs deliver what is, on its face, an asymmetric pattern: a robust positive cross-sectional association between pre-reform schooling and industrial wages in the 1840s, and a precise null for the policy-induced change in schooling between the 1840s and the 1860s. Two readings of this pattern fit the broader literature on schooling and 19th-century industrialization.

The first reading is one of timing. Cohorts educated under the new schools opened in the late 1830s would have entered the industrial workforce at earliest in the late 1840s, with full cohort coverage requiring another decade. The 1860–65 outcome window captures the early stage of this process. Long lags between schooling investments and labor-market returns are a well-documented feature of human-capital expansions in 19th-century settings; the dynamic null is consistent with the policy-induced wage return materializing later than the available data can detect. This is the reading the original draft of this paper emphasized. It is consistent with the evidence but does not exhaust it.

The second reading is one of margin. Squicciarini and Voigtländer (2015), studying these same French industrial surveys, find that basic literacy predicts cross-sectional development but does not predict growth; only upper-tail human capital, proxied by Encyclopédie subscriber density, predicts industrial growth. Diebolt, Le Chapelain, and Menard (2021) reach a closely related conclusion for the second half of the 19th century: it was intermediate human capital, not primary schooling, that drove French industrialization. The pattern reported in this paper, a positive cross-sectional association combined with a null on the primary-schooling-expansion margin, is exactly what these accounts predict. Under this reading, the cross-sectional association reflects the pre-existing geography of human capital, of which primary enrollment is one indicator, rather than a causal margin reachable by primary-schooling policy in this period. The Guizot Law expanded literacy and primary enrollment at the bottom of the schooling distribution, but the marginal industrial wage was being set by skills the law did not directly target. Franck and Galor (2022) push this further, arguing that the causal arrow in 19th-century France ran from industrialization to primary schooling rather than the other way around: industrializing arrondissements adopted schooling, not the reverse. The static cross-sectional positive coefficient is observationally consistent with this direction as well.

These two readings are not exclusive. A long-lag story and a wrong-margin story can both be true. The data in this paper cannot adjudicate between them at the arrondissement level. What they can do is set boundaries on which interpretations are compatible with the evidence. A strong claim that primary-schooling expansion under the Guizot Law generated rapid industrial wage gains within the available window is not consistent with the dynamic null. A claim that the cross-sectional wage-schooling association reflects a deeper geography of human capital rather than a policy-movable margin is consistent with both the static positive and the dynamic null. The latter framing connects more cleanly to what is known about the French industrial revolution from the broader literature, and is the framing I prefer.

6.2 The limits of municipal democratization

The null result on municipal suffrage in both designs needs to be read against what the channel could plausibly deliver in this setting, and against the qualitative literature on what municipal elections in the July Monarchy were actually about.

The constitutional reading is the one already incorporated in the empirical setup. As Tanchoux (2013) and Degrave et al. (2024) document, the Municipal Law of 1831 expanded local political participation without expanding municipal authority: mayors remained appointed, council decisions required prefectoral approval, and the fiscal autonomy of communes was constrained. Under this reading, the suffrage null is unsurprising: the channel through which local political participation could have moved industrial wages, via local public goods, fiscal capacity, or labor-market regulation, was attenuated by the institutional architecture of the regime. This is consistent with what the broader political-economy literature finds about franchise extensions in mid-19th-century European settings, where institutional constraints often muted the economic consequences of formal democratization (Lizzeri and Persico, 2004; Aidt, Daunton, and Dutta, 2010).

The qualitative-history reading complicates the constitutional one. Municipal elections under the July Monarchy were organized political events: prefects acted as quasi-permanent electoral agents (Karila-Cohen, 2008), and the practice of official candidacy and prefectoral mobilization that the Second Empire institutionalized was already taking shape under the constitutional monarchy (Voilliot, 2005). Tudesq (1982) and Crook (2021) describe municipal elections as the "first apprenticeship" of political life for rural taxpaying voters, an apprenticeship that involved campaigning, brokered exchanges, and the gradual organization of local notables, including industrial employers, into political competitors. Where industrial patrons stood as candidates or backed them, employment terms could plausibly enter the implicit electoral exchange. The channel does not require formal council authority over wages to operate; it requires only that candidates have something to offer their voters, and that workers be among the voters or have informal influence over those who are.

The reach of this channel is heterogeneous by construction. The V(n) suffrage schedule limits the franchise to the highest male taxpayers, and the share that this represents falls steeply with commune size. In communes below 300 inhabitants, where the 30-voter floor could include most adult male taxpayers, foremen and skilled artisans could be voters. In larger communes, the franchise is confined to bourgeois and notable strata, and the relevant electoral exchange is among notables themselves, with workers as objects rather than parties. The empirical specifications in this paper, which use a single arrondissement-level suffrage share and average over arrondissements with very different commune-size compositions, are not well-suited to detect a channel that operates only within a subset of communes. A null on the arrondissement average is therefore silent about the existence of the channel in its intended scope.

Two implications follow. The first is that the suffrage null reported here should not be read as evidence that municipal democratization had no economic content under the July Monarchy. It should be read as evidence that no aggregate effect is visible at the arrondissement level on industrial wage means, conditional on controls. The second is that testing the campaigning-and-bargaining channel requires a different design: either an outcome more sensitive to local political activity than mean wages, or a heterogeneity specification that interacts the suffrage measure with industrial structure and commune-size composition. Section 6.4 returns to this as a future-work agenda.

6.3 Identification and territorial legacies

The methodological contribution of this paper, foregrounded throughout but consolidated here, is that both reforms studied turn on the same underlying variable, the commune-size distribution, and that this distribution is not a neutral feature of the data. Section 4.4 developed the structural argument; this subsection draws out what it means for the empirical claims and for future work.

For the empirical claims, the structural dependence on commune size has two consequences. First, the static and dynamic estimates are conditional on the assumption that the included controls absorb the residual correlation between commune size and unobserved determinants of wage levels and growth. This assumption is testable only indirectly, through the visual/geographic diagnostics in Section 4.4 and through the convergence of OLS and IV under different inference choices in Section 5.2. Both checks are reassuring within their domains but neither is dispositive. The honest statement of what this paper identifies is therefore: a conditional association in the static case, a LATE in the dynamic case, both interpretable only under the maintained conditional-exogeneity assumption.

Second, the alignment between the commune-size distribution and pre-reform development means that the LATE itself is a local parameter in a stronger sense than the standard Imbens-Angrist framing allows. The compliers in this design are not arrondissements with a random demographic profile; they are arrondissements concentrated in the regions, the West, the Atlantic Southwest, and the Massif Central, that the Furet-Ozouf triangle identifies as having low pre-reform schooling and lower baseline development. The IV identifies a policy-induced schooling effect for exactly those arrondissements in which the Guizot mandate had the most administrative bite. It does not identify the effect that schooling expansion would have had in arrondissements with high pre-reform schooling and few communes near the 500-inhabitant threshold. External validity is therefore limited not only in the usual sense but in a setting-specific way that reflects the joint structure of the reform and the territorial geography.

What this implies for future work is twofold. The first implication is methodological: any threshold-based study of the Guizot Law at any spatial unit faces the same constraint in a different form. At the commune level, the 1836 population is itself a function of which medieval parish a commune descends from; sharp regression-discontinuity designs at the 500-inhabitant threshold therefore exploit variation in the commune-size distribution that is not independent of the historical correlates of development. The pattern is a structural feature of the setting, not an artifact of the arrondissement-level aggregation used here. The second implication is that improved identification strategies will need to break the dependence on commune size as the sole source of treatment variation. Three directions are plausible. One is to combine the Guizot threshold with a placebo population threshold (for example, 400 or 600) and report differential first-stage strength; weak placebo first stages would support the maintained interpretation that the variation comes from the law rather than from generic settlement features. A second is to exploit cross-time variation in implementation intensity, since the post-1834 enforcement of the law was not uniform across départements and prefects. A third is to design comparisons within narrow strata of the commune-size distribution, restricting the analysis to arrondissements with similar territorial profiles and exploiting the residual variation in policy uptake. None of these strategies is implementable within the scope of the present paper, but each is a defined research direction that the diagnosis offered here clarifies.

6.4 Extensions and future work

Beyond the identification work flagged above, several extensions would substantially strengthen the empirical content of the project.

Outcomes beyond male industrial wages. The industrial surveys carry additional information that has not been exploited here: the number of industrial workers per arrondissement, the breakdown by gender and age, plant-level value of output, intermediary input use, and steam-engine adoption. Using these outcomes would test whether the schooling and suffrage channels operate on margins other than mean wages. Three are particularly informative. Industrial employment growth would test whether the schooling expansion attracted industrial activity, even if it did not move local wages. The female and child workforce share would test whether household labor-supply decisions responded to the Falloux and Guizot expansions, and would also speak to the compound-treatment concern raised in Section 5.2. Steam-engine adoption would test whether human-capital-complementary capital accumulated differentially in arrondissements with higher schooling, a margin that the wage data cannot resolve directly. Value added per worker, recoverable from output and input data, would proxy productivity more directly than wages.

Additional controls and proxies. The most consequential omitted control is settlement dispersion. As discussed in Section 4.4, the commune-size distribution is partly a function of housing dispersion patterns inherited from medieval settlement; controlling for this directly would absorb a substantial part of the residual variation that currently threatens the conditional-exogeneity assumption. The Postal Survey of 1847, used by Montalbo (2021), provides commune-level housing-dispersion data that, with careful harmonization, could be aggregated to the arrondissement level. Encyclopédie subscriber density (Squicciarini and Voigtländer, 2015) and Maggiolo signature data (Furet and Ozouf, 1977) would provide additional pre-reform proxies for upper-tail and basic human capital respectively, helping disentangle the static cross-sectional association.

Testing the migration mechanism directly. The arrondissement-level design is motivated in Section 4.1 by the prevalence of short-distance migration in the period. The mechanism is invoked but not tested. A direct test would regress chef-lieu population growth or chef-lieu industrial employment growth on peripheral Guizot exposure: if the migration channel operates as theorized, arrondissements with higher peripheral Guizot exposure should see faster growth in their urban centers. A null on this test would weaken the unit-of-analysis justification; a positive result would strengthen the case for the aggregation while also clarifying which channel of arrondissement-level effects is doing the empirical work.

Sectoral heterogeneity. The Squicciarini and Voigtländer (2015) industrial nomenclature distinguishes “old” from “modern” technology sectors. Replicating their classification from the industrial-survey microdata and re-running the static and dynamic designs sector by sector would address the wrong-margin reading developed in Section 6.1: if the schooling channel operates differentially in modern, skill-intensive sectors but is averaged away in the full sample, sectoral heterogeneity should reveal it. The replication code for the Squicciarini-Voigtländer classification is not publicly available, but the classification can be reconstructed from the original criteria and the SGF data.

Improvements to the interpolation step. As noted in Section 5.2, the 1850 arrondissement-level schooling counts are constructed by a prediction-and-scaling procedure whose uncertainty is not propagated into the IV standard errors. A bootstrap that resamples the imputation step would deliver corrected standard errors; a hierarchical or spatial-prior interpolation model would tighten the imputation itself. A simpler robustness check, running the dynamic specification at the département level where 1850 schooling is observed directly, is the most informative single test of whether the central null is robust to the interpolation.

Inference under spatial dependence. The clustered standard errors used throughout treat arrondissements within the same département as correlated but treat across-département arrondissements as independent. This is unlikely to be exactly right: neighboring arrondissements share labor markets and shocks. Conley (1999) spatial standard errors, computed at plausible distance cutoffs (50, 100, and 200 km), would test whether the inference conclusions are sensitive to spatial dependence across département boundaries.

Each of these extensions is feasible within the data already available or with modest additional data collection. None is implemented here; each is flagged as a defined direction in which the project could be developed further.

7 Conclusion

This paper has examined the joint role of primary schooling and municipal political participation in shaping local economic development in mid-19th-century France, exploiting two population-threshold reforms of the July Monarchy. The empirical work combines a static cross-sectional design relating pre-reform schooling and baseline suffrage exposure to industrial wages in the 1840s, with a dynamic instrumental-variables design relating policy-induced schooling expansion to wage growth between the 1840s and the 1860s. The two designs are run at the arrondissement level, an intermediate spatial scale chosen to capture the short-distance rural-to-urban labor-market integration documented for the period and to permit joint analysis of both reforms within a single framework.

Three substantive findings emerge. First, pre-reform schooling is positively and robustly associated with industrial wages in the 1840s, with the association stable across robust and clustered inference. Second, municipal suffrage exposure has no robust independent effect on wages in either design, and no detectable interaction with schooling, in a cross-arrondissement setting. Third, the policy-induced expansion of male primary schooling between 1833 and 1850 has no statistically significant medium-run effect on industrial wage growth, with the IV point estimate close to zero and a precise null robust to more conservative weak-instrument inference. The convergence of OLS and IV on the same null, while not in itself a proof of exogeneity given the limited power of the Wu-Hausman test, is informative about the schooling-wage relationship: medium-run wage growth in this setting did not respond meaningfully to changes in primary-pupil counts, on either the policy-induced or the broader margin of variation.

These findings are most coherent when read against the broader literature on schooling and 19th-century French industrialization. The pattern of a positive cross-sectional association and a null on the policy-induced margin matches what Squicciarini and Voigtländer (2015) report for these same industrial surveys, what Diebolt et al. (2021) document for the second half of the century, and what Franck and Galor (2022) argue more broadly about the direction of the schooling-industrialization relationship in France. Under this reading, the cross-sectional positive correlation reflects the pre-existing geography of human capital and of correlated development rather than a margin reachable by primary-schooling policy in the available time window. The null on suffrage, in turn, is consistent with what the institutional literature on the July Monarchy would predict: a regime that expanded local political participation while preserving prefectoral oversight and limited municipal fiscal autonomy. The null does not rule out the campaigning and bargaining channels that the qualitative literature on municipal electoral practice documents (Voilliot, 2005; Karila-Cohen, 2008; Tudesq, 1982; Crook, 2021), but it indicates that these channels did not leave an aggregate footprint on arrondissement-level industrial wage means.

The fourth contribution of the paper is methodological. The two reforms studied turn on the same underlying variable, the commune-size distribution, and that distribution is itself the sediment of medieval parish geography, structured by topography, settlement patterns, and ecclesiastical authority centuries before the reforms were enacted. This is not a feature that future work can design around easily: it is a structural feature of the setting that any threshold-based study of 19th-century French institutions will need to confront. Making this constraint explicit calibrates the strength of the empirical claims this paper can defend and identifies a set of identification challenges that the literature has not previously characterized with this precision. Section 6.3 sketches three directions in which future work could break the dependence on commune size as the sole source of treatment variation, including placebo population thresholds, exploitation of cross-time variation in implementation intensity, and stratified comparisons within narrow bands of the commune-size distribution.

The empirical claims in this paper are therefore best read as conditional rather than definitive. They establish what is and is not visible in the available data, set boundaries on which interpretations are compatible with the evidence, and identify the specific assumptions on which sharper conclusions would depend. In doing so, the paper contributes both to the substantive literature on the joint role of education and political participation in local development, and to the methodological literature on identification under historically structured policy variation.

8 References

Acemoglu, Daron, Suresh Naidu, Pascual Restrepo, and James A. Robinson. "Democracy Does Cause Growth." *Journal of Political Economy* 127, no. 1 (2019): 47–100.

Agulhon, Maurice. *The Republican Experiment, 1848–1852.* Vol. 2. Cambridge University Press, 1983.

Aidt, Toke S., Martin Daunton, and Jayasri Dutta. "The Retrenchment Hypothesis and the Extension of the Franchise in England and Wales." *Economic Journal* 120, no. 547 (2010): 990–1020.

Allen, Robert C. "The Great Divergence in European Wages and Prices from the Middle Ages to the First World War." *Explorations in Economic History* 38, no. 4 (2001): 411–447.

Allier, Jacques. "Esquisse du personnage de Guizot." *Bulletin de la Société de l'Histoire du Protestantisme Français* 122 (1976): 27–45.

D'Angeville. *Essai sur la Statistique de la Population Française Considérée Sous Quelques-uns de ses Rapports Physiques et Moraux.* Bourg, 1836.

Baum, Matthew A., and David A. Lake. "The Political Economy of Growth: Democracy and Human Capital." *American Journal of Political Science* 47, no. 2 (2003): 333–347.

Blanc, Guillaume. "Demographic Transitions, Rural Flight, and Intergenerational Persistence: Evidence from Crowdsourced Genealogies." Working paper, 2024.

Blanc, Guillaume, and Masahiro Kubo. "The Making of France." Working paper, 2026.

Chambru, Cédric, Emeric Henry, and Benjamin Marx. "The Dynamic Consequences of State Building: Evidence from the French Revolution." *American Economic Review* 114, no. 11 (2024): 3578–3622.

Chanut, Jean-Marie, Jean Heffer, Jacques Mairesse, Gilles Postel-Vinay, Frédéric Boccara, Pierre Sicsic, André Strauss, and Patrick Verley. *L'industrie française au milieu du 19e siècle: les enquêtes de la Statistique générale de la France.* Paris: Editions de l'EHESS, 2000.

Conley, Timothy G. "GMM Estimation with Cross Sectional Dependence." *Journal of Econometrics* 92, no. 1 (1999): 1–45.

Crook, Malcolm. *How the French Learned to Vote: A History of Electoral Practice in France.* Oxford University Press, 2021.

Crouzet, François. "La première révolution industrielle." In *Histoire de la France industrielle*, 62–93. Paris: Larousse, 1996.

Degrave, Anne, Alejandro Lopez-Peceño, and Arturas Rozenas. "Peasants into Citizens: Suffrage Expansion and Mass Politics in France." Working paper, 2024.

Demonet, Michèle. *Tableau de l'agriculture française au milieu du 19e siècle: l'enquête de 1852.* Paris: École des Hautes Études en Sciences Sociales, 1990.

Diebolt, Claude, Charlotte Le Chapelain, and Audrey-Rose Menard. "Neither the Elite, nor the Mass: The Rise of Intermediate Human Capital during the French Industrialization Process." *Cliometrica* 15, no. 1 (2021): 167–202.

Duby, Georges. *L'économie rurale et la vie des campagnes dans l'Occident médiéval: France, Angleterre, Empire, IXe–XVe siècles.* Paris: Flammarion, 1977.

Duby, Georges, and Armand Wallon. *Histoire de la France rurale. 3. De 1789 à 1914.* Paris: Éditions du Seuil, 1976.

Dupâquier, Jacques, ed. *Histoire de la population française*, Vol. 3. Paris: Presses Universitaires de France, 1995.

Dupin, Charles. *Effets de l'Enseignement Populaire de la Lecture, de l'Écriture et de l'Arithmétique, de la Géométrie et de la Mécanique Appliquée aux Arts, Sur la Prospérité de la France.* Paris, 1826.

Eggers, Andrew C., Ronny Freier, Veronica Grembi, and Tommaso Nannicini. "Regression Discontinuity Designs Based on Population Thresholds: Pitfalls and Solutions." *American Journal of Political Science* 62, no. 1 (2018): 210–229.

Franck, Raphael, and Oded Galor. "Technology-Skill Complementarity in Early Phases of Industrialisation." *Economic Journal* 132, no. 642 (2022): 618–643.

Furet, François, and Jacques Ozouf. *Lire et écrire: l'alphabétisation des Français de Calvin à Jules Ferry.* Paris: Éditions de Minuit, 1977.

Germain, René. "Revenus et actions pastorales des prêtres paroissiaux dans le diocèse de Clermont." In *Le clerc séculier au Moyen Âge*, edited by Société des historiens médiévistes de l'enseignement supérieur public, 101–119. Paris: Éditions de la Sorbonne, 1993. doi:10.4000/books.psorbonne.25194.

Gerring, John, Philip Bond, William T. Barndt, and Carola Moreno. "Democracy and Economic Growth: A Historical Perspective." *World Politics* 57, no. 3 (2005): 323–364.

Glaeser, Edward L., Rafael La Porta, Florencio Lopez-de-Silanes, and Andrei Shleifer. "Do Institutions Cause Growth?" *Journal of Economic Growth* 9, no. 3 (2004): 271–303.

Glaeser, Edward L., Giacomo A. M. Ponzetto, and Andrei Shleifer. "Why Does Democracy Need Education?" *Journal of Economic Growth* 12, no. 2 (2007): 77–99.

Graff, Harvey J. *The Legacies of Literacy: Continuities and Contradictions in Western Culture and Society.* Bloomington: Indiana University Press, 1987.

Heffernan, Michael. "Literacy and Geographical Mobility in Nineteenth-Century Provincial France: Some Evidence from the Département of Ille-et-Vilaine." *Local Population Studies* 42

(1989): 32–42.

Imbens, Guido W., and Joshua D. Angrist. "Identification and Estimation of Local Average Treatment Effects." *Econometrica* 62, no. 2 (1994): 467–475.

Iogna-Prat, Dominique, and Elisabeth Zadora-Rio, eds. "La paroisse, genèse d'une forme territoriale." Special issue, *Médiévales* 49 (2005). doi:10.4000/medievales.3132.

Karila-Cohen, Pierre. *L'État des esprits: l'invention de l'enquête politique en France (1814–1848).* Rennes: Presses Universitaires de Rennes, 2008.

Lee, David S., Justin McCrary, Marcelo J. Moreira, and Jack Porter. "Valid t-Ratio Inference for IV." *American Economic Review* 112, no. 10 (2022): 3260–3290.

Lepetit, Bernard. "Sur les dénivellations de l'espace économique en France, dans les années 1830." *Annales. Économies, Sociétés, Civilisations* 41, no. 6 (1986): 1243–1272.

Lévy-Leboyer, Maurice. "Les processus d'industrialisation: le cas de l'Angleterre et de la France." *Revue Historique* 239, no. 2 (1968): 281–298.

Lizzeri, Alessandro, and Nicola Persico. "Why Did the Elites Extend the Suffrage? Democracy and the Scope of Government, with an Application to Britain's 'Age of Reform'." *Quarterly Journal of Economics* 119, no. 2 (2004): 707–765.

Mendels, Franklin F. "Proto-Industrialization: The First Phase of the Industrialization Process." *Journal of Economic History* 32, no. 1 (1972): 241–261.

Meyers, Peter V. "Professionalization and Societal Change: Rural Teachers in Nineteenth-Century France." *Journal of Social History* 9, no. 4 (1976): 542–558.

Montalbo, Adrien. "Industrial activities and primary schooling in early nineteenth-century France." *Cliometrica* 14, no. 2 (2020): 325–365. doi:10.1007/s11698-019-00191-0.

Montalbo, Adrien. "Schools without a Law: Primary Education in France from the Revolution to the Guizot Law." *Explorations in Economic History* 79 (2021): 101364.

Openshaw, Stan. *The Modifiable Areal Unit Problem.* Concepts and Techniques in Modern Geography 38. Norwich: Geo Books, 1984.

Pelletier, Monique. *La carte de Cassini. L'extraordinaire aventure de la carte de France.* Paris: Presses des Ponts et Chaussées, 1990 (2nd ed., 2002).

Ridolfi, Leonardo, Carla Salvo, and Jacob Weisdorf. "The Effect of Mechanisation on Labour: Evidence from the Diffusion of Steam Engines in Nineteenth-Century France." CAGE Working Paper No. 689, University of Warwick, 2023.

Rosental, Paul-André. "La migration des femmes (et des hommes) en France au XIXe siècle." *Annales de Démographie Historique* 1 (107) (2004): 107–135.

Squicciarini, Mara P., and Nico Voigtländer. "Human Capital and Industrialization:

Evidence from the Age of Enlightenment." *Quarterly Journal of Economics* 130, no. 4 (2015): 1825–1883.

Stock, James H., and Motohiro Yogo. "Testing for Weak Instruments in Linear IV Regression." In *Identification and Inference for Econometric Models: Essays in Honor of Thomas Rothenberg*, edited by Donald W. K. Andrews and James H. Stock, 80–108. Cambridge University Press, 2005.

Tanchoux, Philippe. "Les 'pouvoirs municipaux' de la commune entre 1800 et 1848: un horizon chimérique?" *Parlement[s]* 20, no. 2 (2013): 35–48.

Tudesq, André-Jean. "Le monde paysan dans le système politique censitaire: un absent ou un enjeu?" *Annales de Bretagne et des Pays de l'Ouest* 89, no. 2 (1982): 215–228.

Verley, Patrick. *La Révolution industrielle.* Paris: Gallimard, 1997.

Voilliot, Christophe. *La candidature officielle: une pratique d'État de la Restauration à la Troisième République.* Rennes: Presses Universitaires de Rennes, 2005.

Weber, Eugen. *Peasants into Frenchmen: The Modernization of Rural France, 1870–1914.* Stanford: Stanford University Press, 1976.

Zadora-Rio, Elisabeth, ed. *Des paroisses de Touraine aux communes d'Indre-et-Loire: la formation des territoires.* Tours: Fédération Archéologique de la Région Centre / Revue Archéologique du Centre de la France, 2008.

A Full Regression Tables

This appendix reports the full regression tables corresponding to the compact estimates in Section 5.

Table A1: Full static OLS estimates: education, political participation, and male wages

Variable	Coef.	Std. Err.	p-value
Intercept	222.3***	36.41	< 0.001
Male pupils per 10k inhab. (cent., 1833)	0.0355***	0.0101	0.0005
Share of voters (cent., 1836)	623.3·	373.5	0.0962
$\text{Pupils}_i \times \text{Voters}_i$	-0.912	0.741	0.2191
Share industrial workers (1839–47)	-305.1**	109.3	0.0056
No. industrial workers (1839–47)	0.0022*	0.0011	0.0496
% Rural pop (1836)	-0.775*	0.320	0.0160
Cereal returns per ha (1815)	-0.554	0.709	0.4354
Tax on doors per cap. (1836)	70.87***	17.79	0.0001
Log dist. coal (1812)	3.75	2.59	0.1477
Presbytery presence	-0.0377·	0.0210	0.0735
Prefecture	12.65*	5.23	0.0161
Observations		341	
R-squared		0.239	
Adjusted R-squared		0.214	
Residual Std. Error		40.11 (df = 329)	
F-statistic		9.41*** (df = 11; 329)	

Notes: Standard errors and p-values are reported as shown.
Significance: ***$p < 0.001$, **$p < 0.01$, *$p < 0.05$, ·$p < 0.1$.

Table A2: Full static OLS robustness estimates

Variable	(1) OLS	(2) Robust SE	(3) Clustered SE
Intercept	222.3 (36.41)***	(38.71)***	(43.28)***
Male pupils (1833)	0.0355 (0.0101)***	(0.0094)***	(0.0115)**
Share voters (1836)	623.3 (373.5)·	(406.8)	(476.2)
$\text{Pupils}_i \times \text{Voters}_i$	-0.912 (0.741)	(0.722)	(0.817)
Share industrial workers (1839–47)	-305.1 (109.3)**	(124.6)*	(124.6)*
No. industrial workers	0.0022 (0.0011)*	(0.00099)*	(0.00103)*
% Rural pop (1836)	-0.775 (0.320)*	(0.331)*	(0.387)*
Cereal returns per ha (1815)	-0.554 (0.709)	(0.770)	(0.880)
Tax on doors per cap. (1836)	70.87 (17.79)***	(19.26)***	(23.49)**
Log dist. coal (1812)	3.75 (2.59)	(2.67)	(2.59)
Presbytery presence	-0.0377 (0.0210)·	(0.0224)·	(0.0294)
Prefecture	12.65 (5.23)*	(5.44)*	(4.86)**

Notes: Column (1) reports OLS estimates with standard errors in parentheses. Column (2) uses heteroskedasticity-robust (HC1) standard errors. Column (3) uses standard errors clustered by département.
Significance: ***$p < 0.001$, **$p < 0.01$, *$p < 0.05$, ·$p < 0.1$.

Table A3: Full dynamic estimates: effect of male pupils on wage growth

Dep. Var.	Log Change in Average Male Wage		
	OLS	2SLS	First Stage
Change in male pupils per 10k	$-5.08 \cdot 10^{-5}$	$-3.14 \cdot 10^{-5}$	542.95^{***}
	(5.37e-05)	(2.79e-04)	(151.90)
Share voters (1836)	0.734	0.912	-1661.38
	(1.287)	(2.825)	(2414.36)
Log change in pop. (1836–61)	0.473^{***}	0.476^{***}	$-221.70^{\cdot}$
	(0.118)	(0.128)	(118.46)
Share industry (1839–47)	0.680^{*}	0.693^{*}	-461.90
	(0.294)	(0.347)	(296.86)
Log avg. wage (1839–47)	-0.778^{***}	-0.777^{***}	-37.25
	(0.0362)	(0.0379)	(36.25)
Cereal returns (1815)	-0.00766^{**}	-0.00750^{*}	-6.09^{*}
	(0.00248)	(0.00333)	(2.51)
Dist. to Paris	-0.000223^{***}	-0.000228^{*}	0.272^{***}
	(0.000062)	(0.000090)	(0.0616)
Tax on doors per capita (1836)	0.274^{***}	0.280^{***}	-259.78^{***}
	(0.0651)	(0.1053)	(64.53)
Prefecture	0.0239	0.0247	$-39.08^{\cdot}$
	(0.0220)	(0.0242)	(21.96)
Instrument	–	Share pop. $\geq$500	–
Observations	340	340	341
Adj. R^2	0.6184	0.6182	0.461
First-stage F-stat.	–	12.70	12.70

Notes: Standard errors are in parentheses.
Significance: $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$, $^{\cdot}p < 0.1$.

Table A4: Full dynamic 2SLS robustness estimates

Variable	(1) 2SLS	(2) Robust SE	(3) Clust. SE
Intercept	$4.167\ (0.390)^{***}$	$(0.556)^{***}$	$(0.635)^{***}$
Male pupils (1833)	-0.0000314 (0.000278)	(0.000347)	(0.000373)
Share voters (1836)	0.912 (2.825)	(3.430)	(3.525)
Log change population (1836–61)	$0.476\ (0.128)^{**}$	$(0.128)^{**}$	$(0.143)^{***}$
Share industry (1839–47)	$0.693\ (0.347)^{*}$	$(0.341)^{*}$	$(0.371)^{\cdot}$
Log avg. male wage (1839–47)	$-0.777\ (0.0379)^{***}$	$(0.0723)^{***}$	$(0.0965)^{***}$
Cereal returns (1815)	$-0.00750\ (0.00333)^{*}$	(0.00485)	(0.00498)
Distance to Paris (med)	$-0.000228\ (0.0000899)^{*}$	$(0.0000893)^{*}$	$(0.000107)^{*}$
Tax per capita (1836)	$0.280\ (0.105)^{**}$	$(0.110)^{*}$	$(0.131)^{*}$
Prefecture	0.0247 (0.0242)	(0.0234)	(0.0237)

Notes: Column (1) reports 2SLS estimates with conventional standard errors in parentheses. Column (2) uses heteroskedasticity-robust (HC1) standard errors. Column (3) uses standard errors clustered by département. Weak instruments test: $F(1, 330) = 12.701^{***}$. Wu-Hausman test: $F(1, 329) = 0.005$, $p = 0.9435$.
Significance: $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$, $^{\cdot}p < 0.1$.