"Enrichissez-vous !":
The Effects of Primary Schooling and Political Participation on Economic Development in 19th-century France

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Abstract

Education and democracy are thought to be potential drivers of economic growth, but relationship remains subject to debate. While some argue that democracy only emerges as a consequence of human capital accumulation, others contend it may independently contribute to long-run economic development through institutional improvements. However, there is still scarce causal evidence on either channel, and practically nothing on the interaction between the two. This paper exploits two policy shocks in 19th-century France -the Guizot Law of 1833 on primary education, and the Municipal Law of 1831- to examine the joint effects of primary education and democratic participation on local economic development. Enacted under the July Monarchy, both laws relied on population thresholds at the level of the communes. The Guizot Law of 1833 mandated every municipality with a population exceeding 500 inhabitants to open and fund a primary school for boys. Similarly, the Law on Municipal Organization of 1831 regulated the number of voters for municipal council elections, granted voting rights to millions of citizens while giving communes with fewer residents higher suffrage levels. Using a newly assembled dataset covering nearly all French arrondissements from 1830 to 1865, we implement two models: a static OLS design to estimate links before the educational reforms could affect labor markets, and a dynamic IV design exploiting pre-law population thresholds to instrument changes in schooling exposure and evaluate their effect on industrial wage growth. We find that higher levels of male primary education prior to the Guizot Law are significantly associated with higher industrial wages in the 1840s, but detect no evidence of an independent or interactive effect of local political participation in either model. This paper

contributes to the literature on the causes of growth by providing novel causal evidence on the economic consequences of early state-sponsored education and political inclusion, and highlights the challenges of identifying joint effects when institutional and demographic legacies are spatially correlated.

Contents

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 Public Instruction from 1832 to 1837

"Enrich yourselves!" Even though historians still debate whether François Guizot pronounced these words, the injunction remained famous to condemn the political doctrines of the French government under the July Monarchy (1830-1848), a regime of "notables" governed by a "bourgeois king". In reality, this aphorism was probably truncated from a speech made in front of the Chamber of deputies in March 1843, answering the criticism of the opposition as Minister of Foreign Affairs. And in the same way these two words illustrated the shortcomings of the constitutional monarchy, Guizot's original quote captures the essence of this period: the origins, objectives and struggles of this government in a time of important political and economic changes. Indeed, the July Monarchy has often been considered as a transitional phase in the history of 19th-century France. Between the absolutist attempts of the Restoration and the rising tide of republicanism, this liberal constitutionalist regime operated under the principles of the "juste-milieu", balancing limited political reform with the preservation of an elite rule. In effect, important reforms were carried out during Louis-Philippe's reign, which would have long-lasting institutional consequences, such as the creation of the mass-education system or the expansion of voting rights to millions of citizens. However this era is most importantly remembered for the beginning of France's First Industrial Revolution. Supported by a growing financial sector and the mechanization of agriculture, France would undergo deep transformations and decades of substantial economic development.

Few questions have received as much attention in the economic literature as the origins of growth, which have been debated since the discipline's inception. Among its various determinants that have been identified, two factors appear to be primary drivers of long-run economic growth: "good institutions" and human capital accumulation. In this study, we will focus on two specific channels associated with them, respectively democracy and education. On the one hand, a leading view in the institutionalist tradition, advanced by Acemoglu et al. (2005, 2019), holds that inclusive political institutions are a fundamental cause of sustained growth. According to this view, democracy fosters development by ensuring property rights, promoting rule of law, enabling citizen voice, and encouraging investment in public goods. Gerring et al. (2011) have documented how exposure to democratic institutions over time can shape long-run growth, adopting a historical perspective on the issue. Baum & Lake (2003) argue there are indirect effects of democracy on growth through public service provision, notably education. Thus, institutional quality would precede and shape economic outcomes, rejecting the idea that democracy is merely a byproduct of rising income or educational attainment. On the other hand, partisans of the human

capital perspective argue that education plays a fundamental role, not only in enhancing productivity directly, but also in shaping the emergence and stability of democratic institutions, for example. Glaeser et al. (2004, 2007) argue that better-educated populations are more likely to demand and sustain democracy. More recent studies have dealt with the relationship between these two factors. However, there is still scarce empirical evidence on either channel, and even practically nothing on their interaction.

This study leverages two legislative measures enacted under the July Monarchy in France, representing two shocks to democratization and education, both based on population thresholds. First, the Law on Municipal Organization of 1831 regulated the number of voters for municipal council elections, granting suffrage rights to 2.7 millions citizens, multiplying by thirteen the number of voters (Degrave, 2024). Second, the Guizot Law of 1833 required all municipality with a population exceeding 500 inhabitants to open and fund a primary school for boys. As a result, these laws introduced variations in the exposure to primary education and in democratization -through political participation- across France, contingent on municipal population sizes. This reform resulted in areas displaying higher suffrage rates due to variations in communes sizes; the law allowed a greater share of the population to vote in smaller municipalities, especially below 300 inhabitants. Thus, we propose to exploit these distinct but temporally proximate policy shocks as quasi-natural experiments to study the joint effects of education and democracy on economic development by answering the following question: How did the expansion of primary education and local political participation, introduced by 1830s legislation, interact to shape economic development in 19th-century France?

To answer this question, we will rely on a newly constituted dataset at the arrondissement level, covering nearly the entirety of the country from 1830 to 1865. Overall, we gather data from ten sources, both previous projects on 19th-century France and historical archives digitized. We estimate the exposure to the shocks to education and political participation based on the population rules they were built on and actual post-treatment data. Regarding economic outcomes, we rely on the two industrial surveys led during that period to retrieve information on average wage for industrial male workers.

For our empirical strategy, employ two complementary empirical strategies: a cross-sectional OLS model and an IV approach exploiting temporal variation. The first sudies the joint "static" effect of primary schooling and political participation on wages, before the educational benefits of the Guizot law affect the labor market. The second focuses on the "dynamic" effects of the education and democracy shocks on wage evolution. In the first design, we use a standard OLS regression, including demographic, industrial, institutional, and economic controls at the levels of the arrondissement and the département. In the second design, we implement an IV design, instrumenting the change in the number of pupils per ten thousand inhabitants using the share of the population living in municipalities above 500 inhabitants -therefore targeted by the Guizot law. In both models, we measure the degree of political participation by computing the share of eligible voters within the arrondissement (excluding the chef-lieu).

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Our results suggest that higher levels of male primary education prior to the Guizot Law are associated with higher average industrial wages in the 1840s, suggesting early schooling access contributed to local economic performance. However, we detect no evidence of an independent or interactive effect of local political participation on wages in either the static or dynamic models. The dynamic IV design, instrumenting schooling expansion with the share of population in municipalities above the 500-inhabitant threshold, also reveals no significant wage growth effect between 1839 and 1865, possibly due to lagged human capital returns. Nevertheless, on the contrary to the initial intuition behind this project, the distribution of communes by size poses a challenge for our identification strategy, as it might not be random, correlating with pre-existing regional disparities in development, rurality, and administrative structure. As a result, arrondissements with many small communes tend to have both higher suffrage exposure and higher initial education coverage, potentially confounding the effects of our treatments. These geographic considerations represent a risk of bias for our estimates. We attempt to control for these differences using observable characteristics, but we remain cautious with the interpretation of our findings. In the latest section, we propose a series of extensions to address this issue, improve our design, and further extend the analysis by incorporating new data or leveraging existing studies.

This study contributes to the economic history literature on 19th-century France, and builds on studies that examines similar policy-induced shocks. Hence, we join the works of Montalbo (2021) as well as Blanc & Kubo (2024) on the effects of the Guizot law on 19th-century France, even though our focus on economic outcomes relates more to the first. Similarly, we relate to Degrave et al. (2024) that study the effect of the expansion of suffrage rights on mass-politicization and political behavior, showing that people in municipalities with higher suffrage rates tended to display more democratic behaviors over the decades that followed the law on municipal organization. We add to this literature by introducing in our analysis another key phenomenon of that context: rural flight and the rise of internal migration across Europe in the 19th-century (Blanc, 2024). By switching our focus from the municipality to the arrondissement, we aim to capture variation linked to population movements, which were predominantly short-distance in nature.

Moreover, this study contributes to the "Institutions vs Human capital" growth debate by addressing the joint effects of primary education and local democratization on economic outcomes within a unified empirical framework. Leveraging two quasi-natural experiments in 19th-century France, it overcomes a key limitation in prior studies: the lack of causal evidence on the interaction between human capital formation and democratic inclusion. While earlier studies have speculated on the complementary of both channels, few have tested this empirically using historical policy shocks. By focusing on subnational variation and historical thresholds in a non-modernizing economy, this study offers a novel historical setting to examine whether and how these mechanisms jointly shaped early industrial development.

2 Historical Background

2.1 19th-century France & the July Monarchy

The July Monarchy (1830-1848) The July Monarchy lasted from 1830 to 1848 and represented a transitional phase in 19th-century France, positioned between the absolutist impulses of the Bourbon Restoration and the resurgence of republicanism that would culminate in 1848. Born from a Parisian uprising against Charles X, the regime of Louis-Philippe I, known as the "Citizen King," operated under the principles of liberal constitutionalism. It institutionalized a bourgeois form of governance, balancing limited political reforms with the preservation of elite rule, following the "juste-milieu" doctrine. The Charter of 1830 established a censal monarchy, expanding civil liberties and reducing the king's authority, yet restricted suffrage to property-owning males, maintaining the dominance of the "notables" class. The strategy of the conservative government, represented by key figures such as François Guizot, was to ensure stability and continuity within the realm, along with cautious innovations, to foster gradual progress and ensure France's recovery from previous periods of instability (Allier, 1976). Thus, it believed the best way to achieve this was by the political preponderance of the "middle-class", guaranteed by restricted censal suffrage.

Economic Growth in mid-19th-century France From the July Monarchy through to the end of the Second Empire (1870), France underwent a period of sustained but gradual economic modernization, driven by the twin engines of agricultural productivity and industrial expansion. (Montalbo, 2021) Industrial production, which had grown slowly during the late 18th and early 19th centuries, began to accelerate after 1815, reaching annual growth rates of around 3% (Lévy-Leboyer, 1968; Crouzet, 1996). This growth intensified particularly in the final years of the July Monarchy and under the Second Empire, when France's investment in railroads, banking, and infrastructure stimulated broader market integration and boosted output. However, the growth was neither uniform nor transformative in the British sense; as Mendels (1972) argue, France experienced no clear "industrial take-off," but rather a slow restructuring of production, with textiles and food industries continuing to dominate industrial value-added well into the 1860s (Verley, 1997).

Meanwhile, agriculture remained the backbone of the French economy. In 1851, more than half of the population belonged to farming families, and even by 1881, nearly half the labor force was still concentrated in agriculture (Demonet, 1990). Indeed, this sector experienced strong growth over the period, largely driven by livestock farming and root crop cultivation, while rising demand from a growing urban population led to significant increases in per capita consumption (Duby and Wallon, 1976).
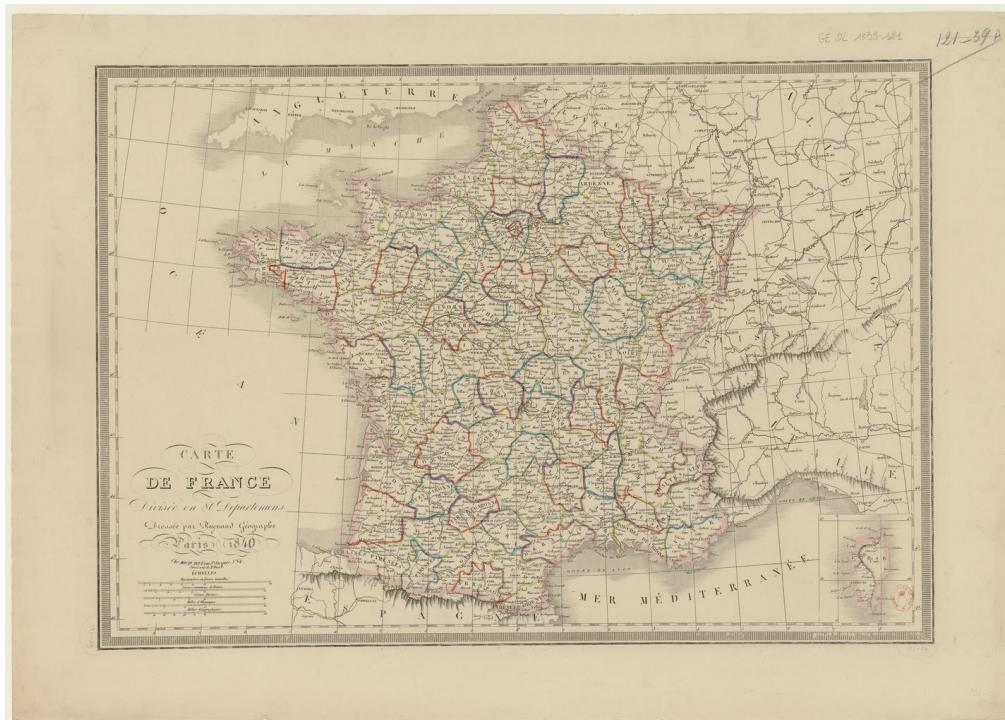
Figure 1: Carte de France divisée en 86 départements - Raynaud (source: Gallica)

Administrative division By 1830, the French kingdom was composed of 86 départements, each subdivided into 2 to 5 arrondissements, for a total of 356 administrative subdivisions. Created during the French Revolution to replace the fragmented and often overlapping jurisdictions of the Ancien Régime, the département system was designed to implement a rational, uniform administrative structure across the national territory. Each département functioned as a key intermediary between the centralized state and local governance, overseen by a prefect appointed by the government. The arrondissements, introduced in 1800 under the Consulate, further subdivided each département to facilitate judicial, fiscal, and educational administration. Administered by a sub-prefect, arrondissements did not possess autonomous governance but served as territorial units for state outreach, including tax collection, school inspection, and electoral coordination. The municipalities where the préfectures and sous-préfectures were established are referred to as "chefs-lieux". Between 1830 and 1865, the borders of départements and arrondissements remained relatively stable. However in 1860, the annexion of the provinces of Nice and Savoie increased the number to 89 and 373 respectively. Note that in the process, the Grasse arrondissement is transferred from Var to the new Alpes-Maritimes.

2.2 Primary schooling in early 19th-century France & the Guizot Law

Before the Guizot Law In the Ancien Régime, the presence and characteristics of primary schools across France was mostly shaped by local initiatives, rather than national legislation (Graff, 1987: Montalbo, 2021). Indeed, from the end of the 15th century to the French Revolution, primary schooling in France was organized in two distinct regional systems, reflecting profound geographic and institutional differences.

In the northern and northeastern of the country, it is the parochial or ecclesiastical system that structured primary education, and funded through tithes or parish contributions, with religious foundations playing an active part in the financing in the North-western area. Teachers were typically clerics whose educational role was secondary to their ecclesiastical duties. In contrast, the southern regions relied more heavily on municipal management and private financing, with little religious oversight. Thus, in regions such as Provence, schooling evolved as a civic rather than clerical function. Municipal authorities played a direct role in the recruitment and remuneration of teachers, who were contracted annually in much the same way as other local tradespeople. Despite municipal oversight, southern schools typically remained privately funded, as limited tax revenues and household reluctance to invest in formal education meant that schooling was often a market-provided good. Notably, this model entailed no clerical obligations; teachers were secular appointees and often had no religious affiliation.

The French Revolution marked the separation of primary education from the Church. After the abolition of the ecclesiastical taxes under the First Republic, teachers could no longer rely on church revenues and were instead compensated through a combination of schooling fees and municipal grants. To fund these expenditures, municipalities drew on their own budgets, and additional local taxes dedicated to such purpose. While departments could contribute financially, their role remained minimal until the Guizot Law formally enabled municipalities to request departmental support when local funds proved insufficient. It is only during the Restoration (1815-1830) that the state began to play a more active role in primary education, marked by a substantial increase in funding, which nearly doubled between 1816 and 1832.

But despite the growing state involvement, the longstanding regional disparities in educational provision, rooted in pre-Revolutionary schooling systems, persisted. Thus, from the 16th century onward, "regions north of the Loire river, such as Alsace and Normandy, had a dense network of primary schools while, in the South-West, only one parish out of five had a school". Hence, 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 (Montalbo, 2021). In his "Figurative map of popular education in France" (1826), Dupin drew a straight line from Saint-Malo to Geneva to illustrate the division between what he called "Northern" and "Southern France". This concept became very popular when it came to symbolize the opposition between industrial France in the North-east and rural France from the South-west, as the

differences were not restricted to education, but also population density, urbanization and economic development overall. Nonetheless, these regional patterns remained relatively stable until the 1860s (Montalbo, 2021).
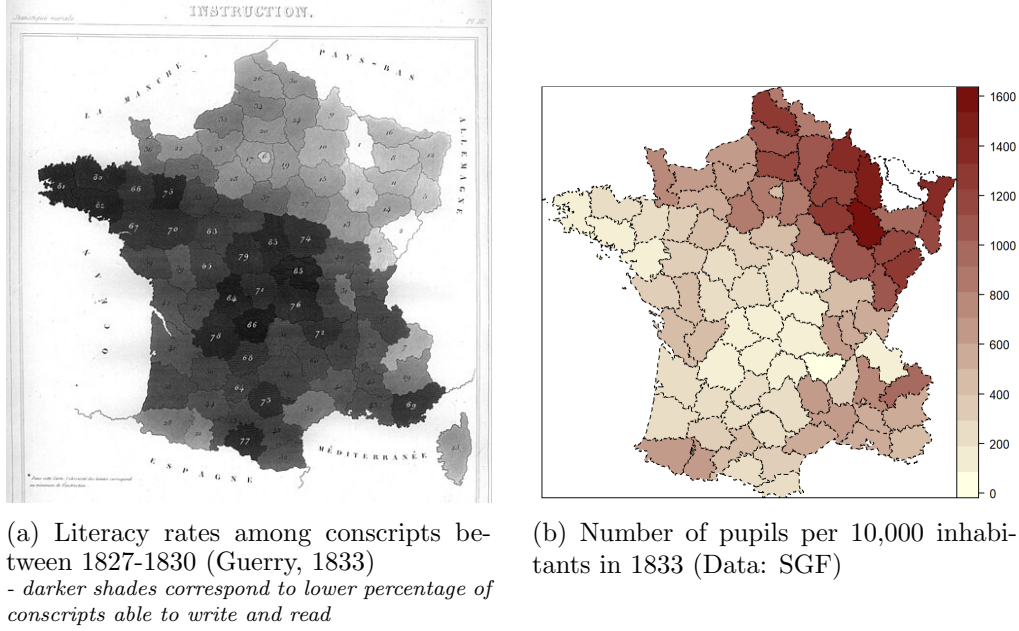

(a) Literacy rates among conscripts between 1827-1830 (Guerry, 1833)
- darker shades correspond to lower percentage of conscripts able to write and read

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

Figure 2: The unequal distribution of primary education

The Law on Primary Education of June 28, 1833. Thus, when François Guizot came into function as Minister of Public Instruction in 1832, most of the nation was illiterate, and the educational system "still in its early stages" (Blanc & Kubo, 2024; Furet & Ozouf, 1977; Montalbo, 2021). In 1833, alongside the passage of the Guizot Law, the French Ministry of Education launched a comprehensive national survey, known as the Enquête Guizot, to assess the state of primary education. Under the direction of the Minister, 490 inspectors were dispatched across nearly all departments of France (with the exception of Corsica) during the autumn of that year. Their task was to examine both public and private primary schools, though those exclusively serving girls were excluded, as the law did not yet apply to female education. The findings painted a bleak picture: classrooms were often open only part of the year, operated on irregular schedules, and were staffed by untrained individuals, many of whom were clergy or laypersons with no formal pedagogical training. As Meyers (1976) observes, the curriculum was overwhelmingly focused on religious and moral instruction, and in many municipalities, catechism was more common than structured academic instruction. Weber (1976) further highlights the improvised character of the teaching profession: instructors ranged from retired soldiers to local barbers or half-educated sons of peasants, underscoring the need for deep reforms.

The Guizot Law of June 28, 1833 established the legal and institutional framework for the French state-sponsored primary schooling system. It required

every municipality with over 500 inhabitants to open and fund a primary school for boys, within six years. While most of the new schools had been built by 1836 (Blanc & Kubo, 2024), this mandate nearly doubled their number within a decade (see Fig. 2). However, the motivations of the government were primarily political rather than economic, aiming to instill order, stability, and civic loyalty among the king's subjects. As Allier (1976) noted, the regime intended education "to moralize the working classes and encourage them to move away from revolutionary ideas," emphasizing the cultivation of obedient, patriotic citizens through moral instruction. Consequently, the law institutionalized a centralized, national curriculum, prioritizing the teaching of the French language, history, and geography, with materials designed and distributed by the state. Oversight was ensured by a newly created body of school inspectors, who worked alongside Académie officers and prefects to enforce compliance. Teachers were to receive regular salaries and standardized training through state-founded teacher schools (Furet and Ozouf, 1977). Municipalities that failed to comply could be compelled by the préfet through administriave orders though those with insufficient resources were then allowed to request financial support from the department or central government.

2.3   The politics of suffrage

Under the July Monarchy existed a "dual-track" system of electoral participation, whereby some democratization at the local level coexisted with entrenched elite control at the national level. Despite its liberal constitutional framework, the regime remained a censal monarchy, where national suffrage was tightly restricted to approximately 200,000 adult males, which represented less than 1% of the population (Degrave et al. , 2024). This figure had modestly increased following a reduction of the tax requirement after 1830, but it ultimately left the legislative power in the hands of the "middle-class" -or wealthy bourgeoisie (Allier, 1976). It follows that the national polity remained exclusive to the wealthier citizens, reinforcing what has been often termed the "regime of the notables."

A more profound transformation occurred at the municipal level. The Municipal Law of 1831 established triennial elections and extended voting rights to approximately 2.7 million male taxpayers. Indeed, the number of eligible voters in a municipality would be determined as a function of the population. The percentage of voters would decrease with size, benefitting primarily rural municipalities. This asymmetry was a deliberate political strategy. Conservatives justified broader suffrage at the local level by arguing that rural voters were more trustworthy and less politicized than their urban counterparts. "The farmer and the artisan may struggle to judge complicated broader issues," one deputy noted, "but they are capable of weighing up the interests of their own communities" (Crook, 2021, p. 31). In contrast, urban centers were seen as breeding grounds for opposition, particularly in light of the July Revolution itself, which had been catalyzed by Parisian unrest. Limiting urban influence thus became a strategic constraint to mitigate anti-regime mobilization.

Municipal suffrage did not translate into legislative power, maintaining what

Degrave (2024) refers to as a system of mass politicization without full democratization. Nonetheless, it played a critical role in the politicization of rural France. As Tudesq (1982) notes, municipal elections served as "a first apprenticeship" in political life for peasants and artisans. Intra-communal competition often enabled rural voters to oust dominant landowners from local councils, catalyzing both class and political consciousness (Agulhon, 1983). Finally, the elections encouraged the emergence of mass-oriented local policies, including investment in education, public works, and redistribution of communal property (Tanchoux, 2013; Montalbo, 2021).

Building on these reforms, the July Monarchy institutionalized a hierarchical suffrage system that stimulated political engagement from below while preserving elite dominance from above. However, its municipal reforms ironically laid the foundations for mass political participation, even as its national restrictions helped provoke the very democratic crisis it sought to prevent (Degrave et al., 2024).

2.4   Migration & rural exodus

The early 19th century marked the onset of France's first rural exodus, as demographic pressure, early industrialization, and infrastructural improvements triggered a gradual shift of population from countryside to town. Duby and Wallon argue that at least 790,000 individuals migrated toward cities between 1831 and 1851, mainly from more agricultural regions: "Massif Central, Lorraine, Alsace, Normandie, Maine, Jura and from the alpine départements" (Montalbo, 2021). Between 1851 and 1856, a further 579,000 rural migrants were recorded, with the Parisian basin acting as the primary destination. Despite this transition, migration was mostly over short-distances: the average move at the century's start was 35 km, rarely exceeding 55 km by its end (Heffernan, 1989; Rosental, 2004).

Among the causes for the increase in internal migrations, scholars have emphasized the role of education as a transmission channel, linking human capital formation to migration patterns and economic geography. In particular, Tiebout sorting mechanisms (Tiebout, 1956) may have encouraged more literate and skilled parents to migrate to municipalities with better schooling infrastructure and economic opportunities. The hypothesis holds that such sorting would have reinforced spatial agglomeration of productivity and population, contributing to local economic development by gathering more educated -and more productive- people. However, recent evidence from Montalbo (2021) challenges this causal narrative, as he finds no no statistically significant effect of the education supply shock induced by the Guizot Law on population growth between 1836 and 1911. These results suggest that education-driven Tiebout sorting was only effective in large urban centers, not in the vast majority of smaller communes whose populations stagnated or declined.

Therefore, primary education may have facilitated rural depopulation. More educated individuals were more likely to have both the means and aspirations to migrate. They often associated life in the city with better job opportuni-

ties (Dupâquier, 1995). Eventually, municipalities that were early adopters of primary schooling often saw higher rates of emigration, especially among youth and the skilled. This suggests that while education increased individual mobility and enabled occupational upgrading, its aggregate demographic effect was not uniformly positive across space. Instead, it contributed to a concentration of productive capacity in urban centers, and a demographic hollowing of rural France. The nonlinear and spatial dependence of this effect complicate standard models linking human capital to local growth, calling for further inspection on the interdependence between urban centers and their rural peripheries.

3 Data

3.1 Data sources

Demographics For data on population and administrative units, we rely on the municipality-level data from the Cassini database (EHESS), based on the 18th-century topographic maps of the French kingdom (Pelletier, 1990, 2002). This project includes a list of all municipalities since 1789 with detailed information on administrative units and population over time. We extract historical arrondissements and départements from this database, while measuring the distribution of communes by size and the number of eligible voters across them.

Education Data on education come from the Statistique de l'Enseignement Primaire tables from the Statistique Générale de la France, provided by INSEE, and the results of the Guizot Survey of 1833 on primary education, digitized by CRH. The first source provides detailed information on the state of primary schooling in France between 1829 and 1897. For arrondissement-level data on primary education, we draw on the Guizot Survey, a nationwide inspection conducted in 1833 under the authority of Minister of Education François Guizot. The survey mobilized 490 inspectors across all departments of France (except Corsica) to document the condition of public and private primary schools, excluding institutions exclusively serving girls, which were outside the scope of the Guizot Law. Summary results were published in an official Report to the King (Guizot 1834), providing arrondissement-level aggregates on key educational indicators derived from the inspectors' original school-level forms. The survey aimed to capture the scale and organization of primary education infrastructure shortly after the introduction of the Guizot Law mandating primary schools in communes with over 500 inhabitants. Like Blanc and Kubo (2024) and Montalbo (2021), we consider the law had not taken effect by then and that it is a good estimation of pre-law levels. These data include information on the number of primary schools, the number of communes where there is at least one, or the number of (male) pupils who attended class during the year.

Industry Industrial data are obtained from the two industrial censuses conducted in 1839-47 and 1860-65 (Chanut et al., 2000). These two surveys conducted by the Statistique Générale de la France provide plant-level data across the country and offers rich empirical evidence on wages, production, labor force composition, sectoral disparities, and regional inequalities. In this study, we will

particularly be interested in the estimations of average daily wage for male industrial workers, as well as the number of industrial workers in each arrondissement. Indeed, based on the data extracted by Chambru, Henry, and Marx (2024), we were able to compile arrondissement aggregates for both surveys; that is 357 and 373 arrondissements respectively.

Institutions We gather data on administritive and religious institutions from different sources. First, we extracted the status of chefs-lieux (préfectures and sous-préfectures) from Cassini's history of each commune. Then, we retrieved data on religious presence from Montalbo (2021), measured by the number of presbyteries in every département, originally from the Statistique Générale de la France. Finally, we added data on old institutional functions under the Ancien Régime (evêchés, baillages, recettes, subdélégations) thanks to Chambru, Henry, and Marx (2024).

Economic resources We collect proxies for economic resources from different sources as well. Data on cereal production per hectare in 1815 comes from the Archives statistiques du Ministère des travaux publics de l'agriculture et du commerce published in 1837, and is obtained from Montalbo (2021). The same paper also provides the distance to coalfields in 1812 as a proxy for industrial potential. Note that this distance is computed at the département level, between the préfecture and the nearest coalfields, a feature that could be improved in later research. The amount of tax on doors and windows per capita in 1836[1] , originally from D'Angeville (1836), is also used as a proxy for the economic resources of individuals, as richer people were more likely to build bigger houses with more of them (Lepetit, 1986). We also draw from Montalbo (2021) for the share of rural population in 1836 -defined as the percentage of people living in towns above 3,000 inhabitants (a threshold defined by the Statistique Générale de la France)- as a proxy. Finally, we compute the median distance from Paris, from municipal-level data in Chambru, Henry, and Marx (2024), to control for both market potential and state-capacity.

3.2 Challenges

Data Harmonization The main challenge we faced throughout this project was the harmonization of data at the arrondissement level across ten distinct sources. Despite the abundance of databases on 19th-century France, unit identifiers have not been standardized: sources variously use modern INSEE codes, historical administrative codes, or project-specific ones. These inconsistencies made merging difficult and necessitate careful cross-validation, especially when dealing with datasets from previous research projects that might have made mistakes. Due to irregularities in arrondissement and département identifiers, we decided instead to rely on administrative names. Although not all datasets include them, we were able to reconstruct correspondence between all identifier systems and the 1836 arrondissement names used as our reference. This required extensive harmonization work, as names evolved over time and were

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[1]Implemented in 1798, this tax was indexed on the number and size of doors and windows per house.

sometimes spelled inconsistently or incorrectly. We then checked the integrity of our merges by comparing variables representing similar or related information across sources based on the established matches, whenever possible. Therefore, we are confident in the consistency of our final dataset at the arrondissement level.

Spatio-temporal Inconsistency A common concern when working with historical spatial data is spatio-temporal inconsistency due to administrative boundary changes. Our analysis focuses on the period between the passing of the first law and the end of the second Industrial Survey, that is between 1830 and 1865. While some municipalities were merged, dissolved, or reassigned to different arrondissements during that time, our focus on the arrondissement level mitigates most of these issues. The primary concern arises from large municipalities switching arrondissements, which could mechanically affect population or industrial aggregates. Such cases were rare, however, and arrondissement boundaries remained relatively stable through the mid-nineteenth century. The only significant changes—the 1860 annexation of Nice, Savoie, and Haute-Savoie, and the transfer of the Grasse arrondissement from Var to Alpes-Maritimes—are addressed by excluding the annexed territories and retaining Grasse within Var throughout for consistency. Moreover, département boundaries did not change significantly over the study period. Note that we exclude Corsica and the départements of Rhône and Seine due to data limitations and administrative irregularities.

To address spatial-temporal inconsistencies in administrative geography, we fix the 1836 arrondissement and département boundaries as the reference throughout the study period, extracted from the Cassini database. All municipal-level data (ie population) are harmonized to this baseline geography to ensure comparability over time. Also, we collected information on the location of administrative centers (préfectures and sous-préfectures) during the period 1830–1865. These locations remained largely stable, with the notable exception of the Loire département, where the préfecture moved from Montbrison to Saint-Étienne in 1855. We retain Montbrison as the administrative center for two main reasons. First, Saint-Étienne's designation as préfecture was a consequence of its rapid economic growth, not its cause, raising endogeneity concerns for our designs. Second, since the latest data we use are from the early 1860s, it is reasonable to assume persistence in the administrative and institutional advantages associated with Montbrison's long-standing status as préfecture, which may not have fully dissipated by the time of the shift; the change was only recent.

Modifiable Areal Unit Problem Another challenge in working with historical and spatial data is when the geographical units at which it is recorded change over time. For instance, while education data in 1833 is available at the arrondissement level, subsequent data is reported at the more aggregated departmental level. This spatial misalignment creates difficulties for longitudinal analysis at the disaggregated level, also known as "modifiable areal unit problem" (Openshaw, 1984).

To address this, we developed a prediction model to estimate education data at the arrondissement level for later years. The model leverages both

département and arrondissement-specific characteristics, and département-level totals to impute arrondissement values, followed by a scaling procedure to ensure that estimates sum precisely to the departmental totals, thereby restoring comparability across time. The advantages of this method are that it incorporates arrondissement-specific features to restore spatial granularity while ensuring consistency with aggregates, and is easily adaptable. Nevertheless, it bears a few drawbacks; the estimations are model-dependent and rely on temporal stability assumptions. Most importantly, the absence of disaggregated data in the estimated periods means that no uncertainty estimates are available for the scaling procedure, and that our model might fail to capture spatial correlations across arrondissements. In that regard, we believe our estimates could be improved, for example by using Bayesian hierarchical models to incorporate uncertainty and spatial structure.

4 Identification strategy

4.1 Motivation

The original intuition behind this project was to exploit two mid-19th-century French laws as quasi-natural experiments, each of which introduced discontinuous institutional changes based on municipal population thresholds. Specifically, the Guizot Law of 1833, mandating the provision of primary education in municipalities above 500 inhabitants, and the Municipal Organization Law of 1831, expanding local voting rights especially in communes below 300 inhabitants, represent distinct but temporally proximate policy shocks. While recent studies have leveraged these thresholds at the municipality level to evaluate outcomes related to education and political participation (Montalbo, 2021; Degrave et al., 2024), they have largely neglected the migration dynamics that served as a key channel of economic transformation throughout the 19th century.

We build on this literature by shifting the unit of analysis from the municipality to the arrondissement, allowing us to capture short-distance rural-to-urban migration patterns that were typical of the period (Heffernan, 1989; Rosental, 2004). The second advantage of this approach is that it allows us to measure the joint impact of both laws, something rather difficult to implement at the municipality level since no commune can cross both thresholds. In effect, we initially intended to exploit potential exogenous variation in the distribution of municipalities by size to measure differential exposure to both laws in order to build our empirical strategy. Using suffrage rules and historical population data, we construct two key indicators: the share of eligible voters in each arrondissement, as a proxy for political participation, and the share of the population living in communes above 500 inhabitants, which we argue is a more precise measure of schooling shock exposure than a count of qualifying municipalities. We exclude chefs-lieux (administrative centers) to isolate the role of peripheral rural communes in shaping core urban dynamics through migration-induced spillovers. Ultimately, we use the share of the population subject to the Guizot Law as an instrument for changes in primary education, enabling us to examine how ed-

ucation and democratization jointly influenced mobility and long-run economic outcomes. In the following section, we discuss both laws and the measures of their impact, as well as the quality of our instrument.

4.2 Description of the two laws

The Guizot Law of June 28, 1833 The Guizot Law of 1833 laid the groundwork for the development of mass state-sponsored education in France. Its impact was both immediate and far-reaching; such that the number of primary schools grew by 81% between 1834 and 1837, while the number of municipalities without schools was nearly halved. (Blanc & Kubo, 2024). Moreover, state budgets for primary education expanded sixtyfold over the decade following the law's passage (Reboul, 1991). Far from being a narrow educational policy, the law represented a structural shock to the supply of schooling, fundamentally reshaping the role of the state in education. Thus, we believe it to be a relevant shock on education.
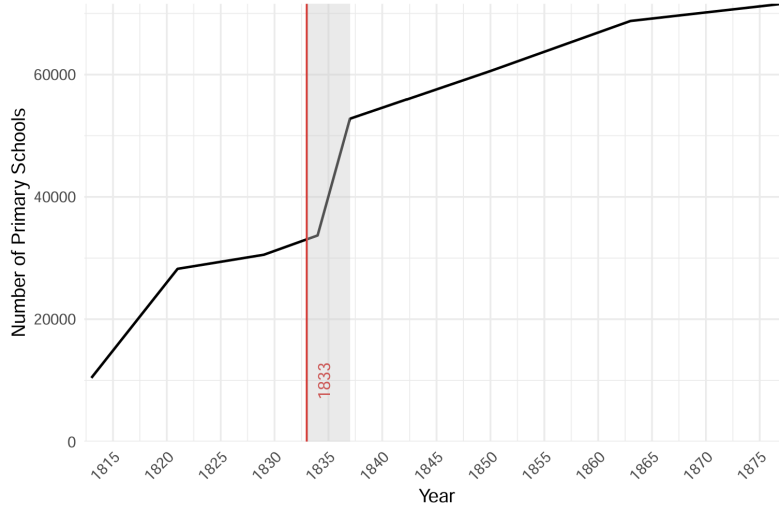

Figure 3: Total of Primary Schools in France (data: SGF)

The Guizot Law targeted primary education for boys, as girls were largely excluded from its provision. The law mandated that municipalities with over 500 inhabitants open and maintain a primary school for boys, but made no parallel requirement for female primary education. As a result, the evolution of schooling over the period we focus on (1830-1865) is mainly driven by male education (ie. number of pupils). Consequently, girls' schooling remained informal, fragmented, and largely delegated to religious congregations (Mayeur, 2004). Note however that having separate schools in one municipality was rare, and it was not uncommon for boys and girls to be taught together in mixed classes in economically constrained municipalities. Institutional parity would not begin until the Falloux Law of 1850, which introduced mandatory schooling for girls in municipalities above 800 inhabitants, Even then, progress remained limited until the Duruy Law of 1867 and the universal provisions of the Ferry Laws in the

1880s. One concern with our design could be that the effects of the 800-threshold on female education bias our estimates if not controlled for. However, we follow Montalbo (2021), arguing that the Falloux law is unlikely to bias our estimates because of its proximity to our outcome variables. Indeed, it is unlikely that the benefits of female primary education had kicked in by 1860, considering children went to school from 6 to 13 years old and that the law took several years to be enforced.
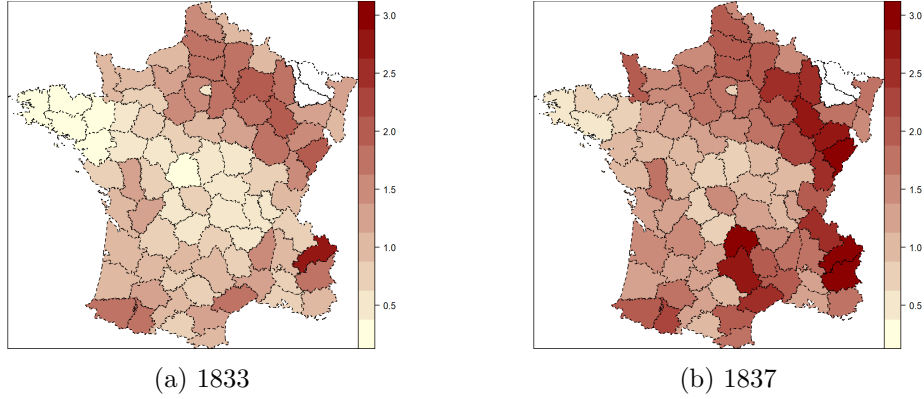

| (a) 1833 | (b) 1837 |

Figure 4: Number of primary schools per 1,000 inhabitants (data: SGF)

Moreover, when looking at these maps, we notice that the départements from the very South display nearly similar levels of education as in the Northeast, after the law is enforced. Indeed, during the first half of the century, the southern parts of the country start catching up (Rhone Valley, Languedoc, Garonne-Méditerranée axis). By 1850, the Saint-Malo-Geneva line has evolved into what Furet and Ozouf (1977) refer to as the "triangle d'arriération Brest-Guéret-Bayonne" (or triangle of backwardness): a base on the Atlantic seaboard and a more or less advanced tip in the Massif Central. This pattern can be easily identified and will remain consistent until relative convergence is reached, from the 1860s onward. This implies that the Guizot Law should have benefitted relatively more to départements below the Saint-Malo-Geneva line, with the biggest changes in the "méridionales" regions.
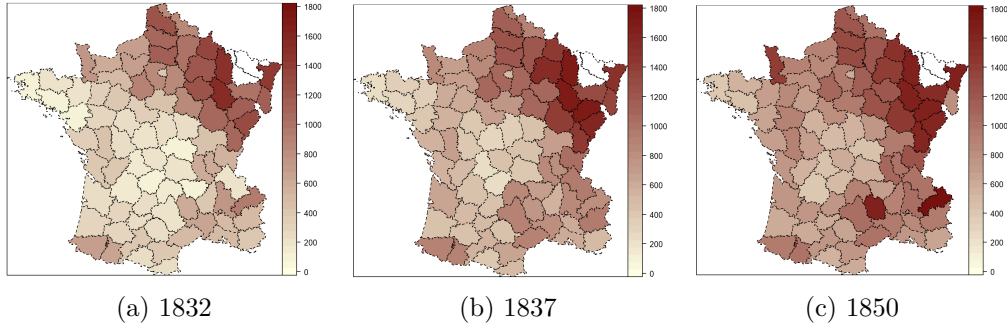

| (a) 1832 | (b) 1837 | (c) 1850 |

Figure 5: Number of pupils per 10,000 inhabitants (data: SGF)

The Law on Municipal Organization of March 21, 1831 To compute the share of eligible voters per arrondissement, we apply the suffrage function as provided in Degrave et al. (2024) to communes population data from Cassini:

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

In practice, in a municipality of n inhabitants, the right to vote for the triennal municipal council election would be given to the V(n) highest male taxpayers. They could then choose the members of the municipal council, which were prior to the law appointed by the prefect. Mayors were still appointed but had to be chosen from the elected council. However, as Tanchoux (2013) noted, their powers were still constrained in the centralized monarchy, and most of decisions had to get the approval of the prefect. Eventually, the effects of the expansion of suffrage rights has to be understood rather as a factor of mass-politicization, rather than a true democratization of the system (Degrave et al. 2024).
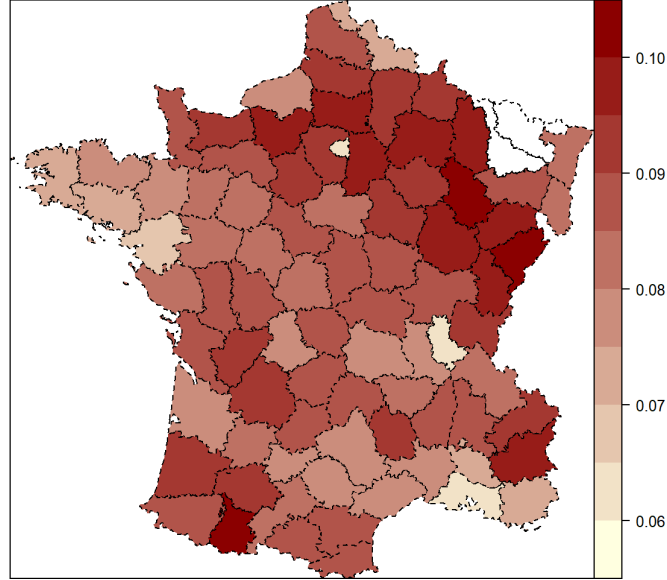
Figure 6: Share of eligible voters (data: Cassini)

The suffrage function was based on several thresholds, and the share of voters within the municipality decreased with population. The most notable discontinuity happened around 300 inhabitants, since below that threshold the number of voters would be fixed to 30. In other words, the percentage of voters did not exceed 10% in any municipality above 300 inhabitants, the suffrage level could reach 100% in smaller communes with 30 male taxpayers. These suffrage rules

led to an unequal distribution of voters across the country, with certain areas being more exposed to this shock on democracy -through political participation.

4.3 The distribution of French municipalitis by size

IV motivation Our study exploits two discontinuous policies tied to population cutoffs in order to estimate the causal effects of primary education and political participation on economic outcomes at the arrondissement level. The first law mandates the opening of primary schools for boys in municipalities above 500 residents, creating a shock to educational infrastructure. The second law, governing political participation, assigns voting rights based on population-based suffrage rules: municipalities below 300 inhabitants are allocated a fixed 30 male voters, while above this threshold suffrage is broadly scaled proportionally to population and declines as population increases.

To identify the effect of the education law, our intuition was to construct an instrument at the arrondissement level: the share (or population share) of municipalities above the 500-inhabitant threshold. The idea was to exploit potential quasi-random variation in treatment intensity due to the discontinuous policy assignment. For the political participation law, the suffrage rule mechanically determines the number of eligible voters per municipality, allowing us to use the resulting total or share of enfranchised individuals as a direct measure of political participation without requiring instrumentation.

In turn, our project from the outset relied on variations in the distribution of municipalities by population size across arrondissements. For education, the validity of the instrument would require that the share (and population share) of municipalites above 500 affected outcomes only via schooling increases. For political participation, causal identification would rely on the accuracy and exogeneity of voter counts as a measure of participation. However, since variation in both treatment intensities is ultimately driven by the distribution of municipal population sizes within arrondissements, this design requires careful attention to potential omitted variable bias.

The main concern would be that this distribution of communes was not random, but most importantly correlated with unobserved confounders. Indeed, an arrondissement with smaller municipalities -exhibiting both higher suffrage rates and lower incidence of the Guizot law- may differ systematically in characteristics such as rurality, economic development, infrastructure, or administrative structures. These underlying differences could confound the estimated effects of political participation and education

The origins of the French municipalities The French communes were created during the French Revolution as the smallest administrative units of the territory. The main objective at the time was to simplify and harmonize the local organization of the country by replacing the feudal structures from the Ancien Régime. It follows that the vast majority of the new municipalities were modelled on the former parishes. The way boundaries were set was not centralized: local assemblies, in consultation with parish priests and notables, proposed different borders that would then be submitted to département or national authorities.

As a result, communes often reflected pre-existing local realities, often based on natural boundaries (rivers, forests, hills), ancient community uses (pastures, woods, paths) or social and economic ties.

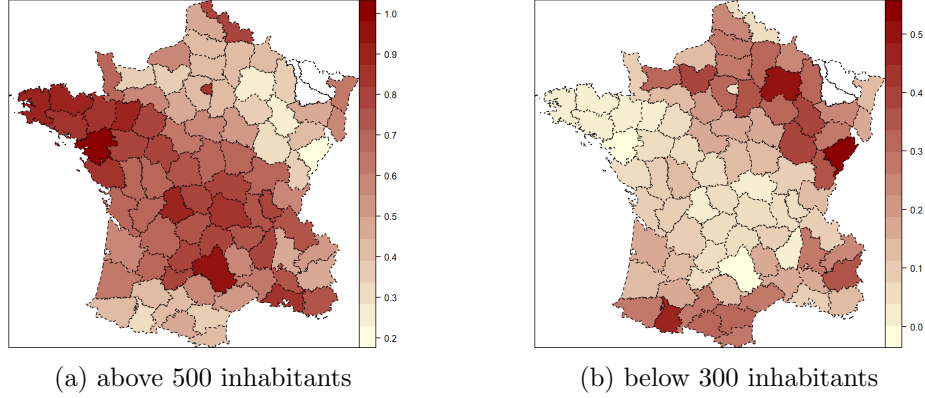(a) above 500 inhabitants   (b) below 300 inhabitants

Figure 7: Share of municipalities (data: Cassini)

Thus, the origin of French parishes, shaped centuries before the Guizot Law, directly influenced municipal boundaries. Understanding these determinants is crucial to evaluating the exogeneity of the law's population thresholds. Parishes were gradually established between the 6th and the 12th centuries, formed around a church, under the authority of the parish priest, with the only principle that every member of the community should be able to attend mass within half a day's walk. Yet, they exhibited important variations in size and demographic density across regions; these disparities were shaped by the interplay of geographic, demographic, economic and institutional factors that shaped the medieval landscape. First, topography seem to have played a fundamental role in determining parish configurations; parishes in challenging terrains (mountain, dense forests, marshlands) often encompassed larger territories to include sufficient population, while those in fertile plains could maintain smaller boundaries due to higher population density (Baldwin et al., 2005). Secondly, population density appeared to be the most important factor; for example, regions with intensive agricultural development and dense settlement could support smaller parishes (Duby, 1977). Accordingly, areas with nucleated villages typically developed compact parishes, while those characterized by dispersed settlement patterns often resulted in parishes with irregular boundaries encompassing multiple hamlets and isolated farms. In the same way, higher level of economic activity, often correlated with agricultural productivity, made it possible to maintain smaller parishes. Finally, secular and ecclesiastical authorities played decisive roles in parish formation and boundary determination. Seigneurial policies regarding settlement promotion, market establishment, and territorial control directly impacted parochial organization. Lords seeking to develop their territories often sponsored church construction and parish creation to attract settlers and increase revenues. As a result, all these elements would indirectly play a key role in the elaboration of the municipalities borders.
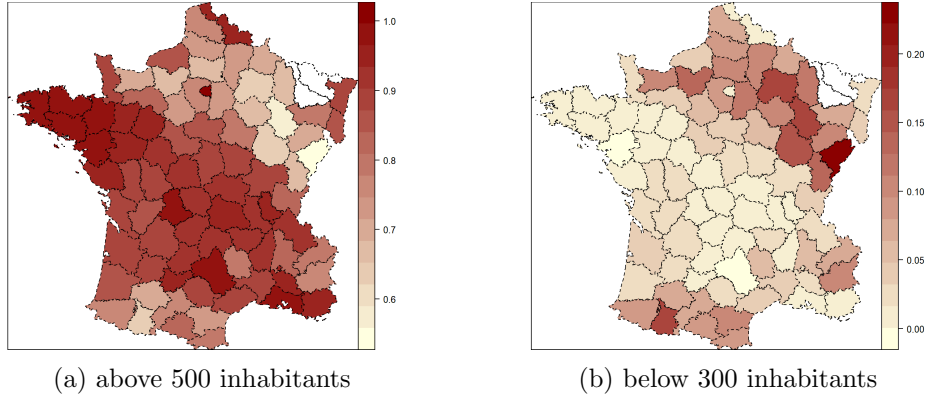
20

| (a) above 500 inhabitants | (b) below 300 inhabitants |

Figure 8: Share of the population in municipalities (data: Cassini)

Threat to instrument validity The variations in the distribution of municipality by population size across the territory in 1830 was therefore heavily influenced by pre-existing development differentials across arrondissements. The North-east is higher shares of small municipalities, with communes below 300 inhabitants representing half of them in some départements. In regions like Champagne, which was relatively developed, with vast plains and important economic activity thanks to trade, the multiplication of seigneureries and parishes led to a higher concentration of small municipalities. In contrast, in Bretagne or Massif Central, with very dispersed habitat and poor soils, municipalities had to cover large areas to gather enough people. As a result, nearly everyone in these regions lived in communes above 500 inhabitants.

The most striking observation when looking at the distribution of these municipalities on maps is that it closely aligns with Furet's and Ouzouf's "triangle of backwardness Brest-Guéret-Bayonne". This bears important consequences for our identification strategy, since there seems to be obvious correlations between the variation of municipality size and pre-existing levels of development, potentially hindering the validity of our instrument. Following these results, arrondissements falling in the triangle with lower levels of education will also be relatively more affected by the law because of their municipality configurations. On the other hand, arrondissements in the North-East and South, which were more developed economically and in terms of education would have relatively higher suffrage rates (see Fig. 6). Hence, before the Guizot Law takes place, there is a negative correlation between the share of communes (and their population) above 500 and schooling presence or rates. Nevertheless, it seems that the 500-inhabitants threshold had not been chosen with the idea of targeting the underdeveloped territories, since it was most likely inspired by a similar law passed in Switzerland a few decades before with the same cutoff (Blanc & Kubo, 2024).

The endogenous distribution of communes might external validity and local treatment effects, since our identification strategy relies on population thresholds used to assign treatment. However, it seems municipality size was not randomly assigned, and historically determined by a multitude of factors. As a result, mu-

nicipal population size is likely correlated with other variables such as historical wealth, urbanization, housing dispersion, etc. Hence, municipalities just above or below the threshold may not be representative of all French municipalities. For the same reasons, this raises a concern that our IV estimates capture the effect of marginal schooling expansion in undervelopped areas, not a universal education effect; the same treatment might have had different impacts in more urbanized or economically advanced regions. We try to mitigate this concern by including controls for observable arrondissement-level characteristics. By doing so, we try to ensure that, conditional on the relevant observables, the variation in treatment intensities generated by population thresholds becomes as good as random, and satisfy the conditional exogeneity assumption required for identification.

5 OLS Design (Static effect)

5.1 Empirical strategy

To estimate the impact of primary education and political participation in the "periphery" on average male daily wage in industry, we first propose a model before the educational benefits of the Guizot Law took effect. The dependent variable is the average male industrial wage at the arrondissement level, measured between 1839 and 1847 as part of a comprehensive national industrial survey. By this period, several years had passed since the implementation of municipal council suffrage, enabling meaningful exposure to political participation. In contrast, although the 1833 Guizot law mandated the creation of primary schools in municipalities with more than 500 residents, the vast majority of these schools were only established between 1834 and 1837. Given the schooling age window (6 to 13y/o), the direct returns to this educational expansion were unlikely to have materialized by the 1840s.

We therefore interpret political participation as a post-treatment exposure with potential short-run effects on wage -through local governance, resource allocation, or political games- while primary education functions as a pre-treatment measure. For education, we use the number of male pupils in 1833 per 10,000 inhabitants as a proxy for pre-law schooling intensity in each arrondissement. Although these data were collected shortly after the Guizot law was enacted, we make the assumption that school enrollment levels and geographic disparities reflect historical persistence, such that variation in 1833 reflects the pre-law educational landscape.

The choice to use the daily wage of male industrial workers as our outcome variable is motivated by both empirical and conceptual considerations. First, the Industrial Survey of 1839-1847 provide detailed and comprehensive information at the plant-level on salaries and labor force composition, which allowed us to compute this measure in 353 arrondissements. Similar information is given in the later 1860-65 survey, which makes the evolution of this variable easy to measure. Second, we believe industrial wages are a relevant indicator of local economic performance; they might reflect both the productivity of labor and

the structure of its demand within an arrondissement. Third, wages might be responsive to institutional and human capital shocks in the short to medium run. Education expands the supply of skilled labor, while political participation can influence local public goods provision, institutional accountability, and social demands, all of which may translate into differences in wage levels across areas. In this context, male industrial wages would serve as an indication of how these reforms translated into local economic returns, offering an interpretable, and policy-relevant proxy for economic development.

The objective of this static design is to understand whether education and political participation, as well as their joint effect, played a significant role in determining industrial daily wages for male workers. Thus, we use the following specification:

$$\overline{Wage\_3947_i} = \beta_0 + \beta_1\,Pupils_i + \beta_2\,Voters_i + \beta_3\,Interact_i$$
$$+ \beta_4\,Indus_i + \beta_5\,Demo_i + \beta_6\,Inst_i + \beta_7\,Econ_i + \varepsilon_i$$

Here, $\overline{Wage\_3947_i}$ denotes the average male industrial daily wage in arrondissement i, computed based on plant daily salaries for men and number of male workers from the Industrial Survey of 1839-1847. The variable $Pupils_i$ captures the number of male pupils per 10,000 inhabitants in 1833 in each arrondissement. Note that despite being interested in the relation between the urban center of the arrondissement and its periphery, we do not have access to municipality level data on education, which is why we cannot exclude the chef-lieux from our measure. The term $Voters_i$ reflects the share of the arrondissement population eligible to vote in 1836, excluding the *chef-lieu*, based on the law's suffrage allocation rule. We also include the interaction term $nbmalepupils \times sharevoters$ to capture potential complementarities between education and political participation.

The remaining terms correspond to a series of controls at the arrondissement or département levels. $Indus_i$ includes controls for industrial structure, such as the share of industrial workers in the arrondissement population in 1841, the number of male industrial workers, and the distance to the nearest coalfield. $Demo_i$ controls for the percentage of the population living in rural municipalities, $Inst_i$ for the administrative and religious presence (presence of a préfecture and number of presbytaries), and $Econ_i$ represents proxies for economic development (e.g., tax on doors and windows per capita in 1836, cereal productivity in 1815). Finally, the error term ε_i captures unobserved determinants of wages.

5.2   Results

In the following table, we display the results of our static OLS design. Note that because of multicollinearity concerns, we needed to center our variables of interest in order to add an interaction term; however, regression models without centering and interaction yield very similar results.

Table 1: Static OLS model -Interaction Effects of Education and Political Participation on Male Average Wages (1839-1847)

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
Pupils$_i$ × 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: ***$p < 0.001$, **$p < 0.01$, *$p < 0.05$, ·$p < 0.1$

We find a significant positive coefficient for education, such that a 1-unit increase in pre-treatment male pupil exposure (from its mean) is associated with a 0.036 francs increase in average daily wage for industrial workers. This effect is statistically significant and robust to the inclusion of the interaction term and additional controls. Under our assumptions, this seems to confirm that historical education levels are positively associated with industrial wages, even before post-reform cohorts entered the labor force. However, our model fails to identify a causal link between political participation (and its interaction with education levels) and salaries. Indeed, the positive coefficient for the share of voters outside the chef-lieu is only very marginally significant, and will lose all statistical significance during robustness checks. The interaction is statistically insignificant and negative. This implies that there is no strong evidence that the effect of political participation on wages depends on education, or vice versa, at least in this static setting.

We include a rich set of control variables to account for various differences across arrondissements that may confound the relationship between education, political participation, and industrial wages. Among these, the share of industrial workers in the arrondissement population is negatively associated with wages, supporting the idea that more precarious labor markets might exert downward pressure on average pay. The number of male industrial workers enters positively and marginally significantly, possibly reflecting agglomeration effects or

scale economies in industrial labor. The rural population share however is negatively signed and significant, indicating that more rural districts experienced lower wage levels. Indicators of economic development, like taxes per capita, exhibit strong positive association with wages, reinforcing its value as proxy for overall households wealth in the area (or local tax capacity). While some variables, such as cereal returns and distance to coalfields, might not be statistically significant, their inclusion controls for important historical a nd geographic heterogeneity. Finally, having the préfecture in the chef-lieu of the arrondissement is positively associated with industrial wages, possibly reflecting better institutions, administrative centrality, or enhanced economic connectivity.

Regarding the model fit, our specification seems to explain approximately 21.4% of the variation in average male industrial wages across arrondissements. We believe this level of explanatory power to be reasonable given the historical and cross-sectional nature of the data, but there remains scope for further model refinement. Moreover, the F-statistic seem to confirm that our set of explanatory variables jointly contribute to explaining wage variations. Altogether, the specification appears stable and reasonably well-fitted for the purposes of identifying broad institutional effects on early industrial wage outcomes.

5.3   Robustness

The selection of additional control variables was guided by their incremental contribution to overall model fit, as assessed by the Akaike Information Criterion (AIC) and adjusted R^2, conditional on the inclusion of our key explanatory variables—education, political participation, and their interaction. We chose this approach to ensure that controls were retained not simply for mechanical completeness, but for their relevance in explaining residual variation in industrial wages. Furthermore, multicollinearity diagnostics based on variance inflation factors (VIFs) revealed no indication of problematic collinearity among regressors, even suggesting that coefficient estimates were not unduly inflated and that the individual effects of included controls were reliably identified.

In the following table, we show how our baseline OLS results respond to different treatments of standard errors. Column (1) shows conventional OLS results; column (2) applies heteroskedasticity-robust (HC1) standard errors; and column (3) clusters standard errors at the département level to account for spatial correlation in policy exposure, institutional capacity, and unobserved shocks. Overall, The consistency of coefficient magnitudes and general patterns across standard, robust, and clustered standard errors indicates a high degree of model stability. The effect of male education is both statistically and substantively robust, reinforcing the idea of its role as a strong predictor of wage variation across arrondissements.

Table 2: Robustness of Estimates: Static OLS Design (1839–1847)

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)
Pupils$_i$ × 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.
***$p < 0.001$, **$p < 0.01$, *$p < 0.05$, ·$p < 0.1$

The coefficient on the number of male pupils per 10,000 inhabitants in 1833 remains positive and highly significant across all specifications. In the OLS estimate retains statistical significance under both heteroskedasticity-robust and département-clustered standard errors. This indicates a stable and robust relationship between pre-treatment education levels and industrial wages in the 1840s. In contrast, the coefficient on the share of voters in 1836 is only marginally significant in the standard OLS specification, and becomes statistically insignificant when robust or clustered standard errors are used. This suggests that the association between early political participation and wage levels is more sensitive to inference assumptions and may be more weakly identified in this static framework. The interaction term between education and voting remains insignificant across all specifications, indicating no strong evidence of complementarities between these two channels in shaping early industrial wage outcomes.

6 IV Design (Dynamic effect)

6.1 Empirical strategy

As previously discussed, the plausibility of our instrument for identifying changes in education levels after the Guizot law is challenged by what the endogenous distribution of municipality across arrondissements. Hence, a design similar to our previous OLS specification but applied for 1860-65 industrial outcomes would likely yield biased estimates. Indeed, it appears the negative correlation between the share of communes above the 500 threshold in 1836 and the average daily wage of male industrial workers remains after 30 years, even though it is

less important. However, we have reasons to believe our instruments remains valid to estimate not the level but the change in schooling supply within that period. Indeed, we find positive correlations between the number of communes above 500 in 1836 and the number of new schools per arrondissement, and, most interestingly, between the share of people in these municipalities and the change in the number of male pupils per 10,000 inhabitants between 1833 and 1850.

Hence, we propose a dynamic framework to assess the medium-run effects of education and political participation on economic development. Specifically, we estimate the impact of changes in primary schooling exposure on the growth of male industrial wages across arrondissements between the 1840s and 1860s. In this period, the educational system established by the 1833 Guizot Law had matured, and its effects were more likely to have materialized in the labor market. At the same time, the mass-politicization process continued at the municipal level.

Our outcome variable is the change in the logarithm of the average male industrial daily wage between the two national industrial surveys conducted in 1839-1847 and 1860-1865. We believe this wage change reflects both productivity growth and changes in labor market structure. The key explanatory variable is the change in the number of male pupils per 10,000 inhabitants between 1833 and 1850, capturing the net expansion in primary education induced by the Guizot Law. By doing so, we make the assumption that increases in schooling is mainly driven by the expansion of education infrastructure causes by the law, as confirmed by the correlation with our instrument. We also control for the level of political participation at baseline, measured by the share of the population eligible to vote under the suffrage law (as in the static model). Lastly, we try to control for other factors that might have influenced both municipality distribution and our variables of interest to minimize confounding errors.

Given that educational expansion was partly driven by the Guizot Law, which mandated school openings in municipalities with over 500 inhabitants, we instrument the change in schooling with the share of the arrondissement's population living in non-chef-lieu municipalities above the 500-inhabitant threshold. Indeed, the direct use of observed changes in male pupils per 10,000 inhabitants from 1833 to 1850 as a regressor might raise endogeneity concerns for several reasons. First, education levels are likely correlated with unobserved arrondissement-level factors, such as political will, fiscal capacity, early economic and institutional development, all of which could also directly influence wage growth (Omitted Variable Bias). Second, arrondissements experiencing rising wages or industrial growth might have expanded schooling access in response to labor market demands or in anticipation of future development, biasing the estimated effect upward. Finally, historical administrative data on schooling may contain noise or underrerporting, especially in less centralized or rural areas, which would bias OLS estimates. Because of these concerns, the OLS estimates of the effects of the change in education levels on wages might be biased and inconsistent. We did not implement such design in the static model in the absence of a plausible instrument, which we now have thanks to the Guizot law population threshold.

Indeed, the Guizot Law mandated school openings only in municipalities

with more than 500 inhabitants, creating a discrete, policy-induced threshold that generates plausibly exogenous variation in educational expansion, controlling for key arrondissement-level characteristics. The share of the arrondissement population living in such municipalities (excluding chef-lieux) can be used as an instrument for actual changes in male pupil enrollment; this instrument is appealing for the three following reasons. First, we believe it satisfies the relevance condition; the threshold rule induces sharp differences in the likelihood of school opening between municipalities just above and below 500 inhabitants. Therefore, the share of the population exposed to the treatment is likely predictive of education growth at the arrondissement level. Second, the assignment rule is mecahnical and (potentially) exogenous, based on fixed population cutoffs, not on economic conditions, political decisions, or administrative discretion. Conditional on controls, such as rurality, industrial composition, or economic development, we assume this share affects wage growth only through its effect on schooling (exclusion restriction). Finally, a common concern with population threshold policies is manipulation. But given the historical cotntext, we mobilize the same argument as Montalbo (2021) and argue taht it is unlikeley that municipalities could manipulate their population counts to fall above or below the threshold within the years the Guizot law was enforced; this assumption strenghtens the case for quasi-random variation in treatment intensity near the threshold.

This approach aims to isolate quasi-exogenous variation in educational expansion due to the policy's discontinuous assignment rule, and enhance the credibility of our empirical results. Our estimation strategy follows a two-stage least squares (2SLS) specification:

$$
\text{First stage:} \quad \Delta\text{Pupils}_i = \pi_0 + \pi_1 \, \text{sharepop\_ab500\_nc}_i + \pi_2 \, \text{Vote}_i \\
+ \pi_3 \, \text{Indus}_i + \pi_4 \, \text{Demo}_i + \pi_5 \, \text{Inst}_i + \pi_6 \, \text{Econ}_i + \nu_i
$$

$$
\text{Second stage:} \quad \Delta \log(\overline{\text{Wage}}_i) = \beta_0 + \beta_1 \, \widehat{\Delta\text{Pupils}}_i + \beta_2 \, \text{Vote}_i \\
+ \beta_3 \, \text{Indus}_i + \beta_4 \, \text{Demo}_i + \beta_5 \, \text{Inst}_i + \beta_6 \, \text{Econ}_i + \varepsilon_i
$$

In the first stage, we model the change in the number of male pupils per 10,000 inhabitants (ΔPupils_i) as a function of the share of the arrondissement's population living in non-chef-lieu municipalities above 500 inhabitants in 1836 ($sharepop\_ab500\_nc$). hich captures quasi-exogenous variation induced by the 1833 Guizot Law. We also control for the level of political participation at baseline (Vote_i) that same year and a set of arrondissement and département characteristics.

In the second stage, we estimate the effect of the instrumented change in schooling exposure on the log change in average male industrial daily wages: $\Delta \log(\overline{\text{Wage}}_i)$. The specification includes the same baseline suffrage variable $Vote_i$, as well as controls grouped into the same four domains as in the static model. First, we control for industrial structure ($Indus_i$) with the logarithm of average male industrial daily wage in 1839-1847 and initial industrial employment share. Then $Demo_i$ accounts for demographic characteristics such as the

log change of population between 1836 and 1861, to account for the population growth channel. $Inst_i$ represents administrative and religious controls with the presence of the préfecture and the number of presbyteries on the département. Finally, $Econ_i$ controls for economic structure with tax on doors and windows per capita in 1836 and cereal production per hectar in 1815. We also control for the distance from Paris, which can be interpreted as a proxy for market access and state-capacity. The error term ε_i captures unobserved determinants of wage growth.

This specification aims to isolate the short- and medium-run effects of educational expansion on industrial wage growth and strenghten the plausibility of the exclusion restriction, by accounting for political inclusion and baseline structural characteristics. The key identifying assumption is that, conditional on controls, the instrument affects wage outcomes solely through its impact on educational expansion and not through other unobserved channels. However, we have not included an interaction term in this specification for the joint effect of education and political participation. There are two reasons behind this choice. First, specifications including an interaction worsened our model fit, in addition to not yielding any significant result for this joint effect. Second, it raised multicollinearity concerns, which we will discuss in the following section.

6.2 Results

Table 3 presents the results from our two-stage least squares (2SLS) estimation of the effect of changes in male schooling on wage growth across arrondissement between 1839 and 1865. The OLS coefficient on the change in the share of male pupils is negative but statistically insignificant, while the 2SLS estimate is also negative, with a much larger standard error, and remains insignificant. Assuming our design successfully addressed endogeneity, these results suggest that there is no evidence that increases in male primary education between 1833 adn 1850 led to short- or medium-run increases in industrial salaries by the 1860s. While it seems surprising at first, this finding is consistent with the timing of the schooling reform and the maturation period required for human capital investments to affect the labor market, especially in a relatively underdeveloped context. In addition, the model fails to identify any statistically significant link between the share of voters and the outcome, a finding similar to the static specification. We did not add an interaction term in this specification, as explained previously, because it raised concerns regarding the model fit.

Table 3: 2SLS Estimation Results: Effect of Male Pupils on Wage Growth (1839–1865)

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·
	(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·
	(0.0220)	(0.0242)	(21.96)
Instrument:	–	Share pop. ≥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 in parentheses. ***$p < 0.001$, **$p < 0.01$, *$p < 0.05$, ·$p < 0.1$

The first-stage results confirm that the instrument (share of the population living in municipalities above 500 inhabitants, outside the chef-lieu), is strongly predictive of the endogenous regressor (change in the number of male pupils per 10,000 inhabitants). The coefficient on the instrument in the first stage is positive and significant, which confirms that the Guizot Law was actually enforced. Moreover, the F-statistic exceed the conventional rule-of-thumb threshold of 10, which suggests the instrument is strong enough to predict the education changes and validates our relevance assumption. The adjusted R^2 values are high in both the OLS and 2SLS models, indicating our model explains an important share of the variation in log wage growth. This value is lower in the first-stage regression but remains substantial, since it explains nearly half of the variation in the endogenous regressor. Overall, these statistics seem the point to a credible IV design, even though the second-stage estimate for education is not statistically significant.

The other control variables behave largely as expected and are stable across both specifications. Population growth between 1836 and 1861 appears to be a strong and significant predictor of wage growth, reflecting demographic dy-

namism and local economic expansion. The baseline share of industrial workers in the population is positively associated with wage growth, consistent with industrial clustering or agglomeration effects. Conversely, initial wage levels in the 1840s are negatively associated with wage growth, which seems to indicate certain convergence dynamics. Geographic and fiscal variables, such as distance to Paris, tax revenue per capita, and cereal yields, are significant and easily interpretable: greater distance to Paris is associated with slower wage growth (market acces, state capacity), while higher baseline fiscal capacity and economic resources is positively linked to wage increases. Finally, the presence of a prefecture is positively signed but not statistically significant.

6.3 Robustness

We determined our specification using the same method as in the static design. We selected our additional controls based on their contribution to the model fit, measured by the Akaike Information Criterion (AIC) and adjusted R^2, conditional on the inclusion of our key explanatory variables on education and political participation. This allowed us to keep the information that were relevant in explaining variation in average pay for male industrial workers.

In the following table, we test the robustness of our 2SLS results. Column (1) shows our baseline 2SLS results; column (2) applies heteroskedasticity-robust (HC1) standard errors; and column (3) clusters standard errors at the département level to account for spatial correlation. Overall, The consistency of coefficient magnitudes and general patterns across standard, robust, and clustered standard errors indicates a high degree of model stability. The effect of male education is both statistically and substantively robust, reinforcing the idea of its role as a strong predictor of wage variation across arrondissements.

Table 4: 2SLS Estimates with Robustness Checks

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)·
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.

***$p < 0.001$, **$p < 0.01$, *$p < 0.05$, ·$p < 0.1$

Diagnostic tests: Weak instruments test: $F(1, 330) = 12.701$***.
Wu-Hausman test: $F(1, 329) = 0.005$, $p = 0.9435$.

The estimated coefficients on male pupils change and share of voters remains statistically insignificant across all three specifications, whether using conventional, heteroskedasticity-robust, or clustered standard errors. This result reinforces the earlier conclusion: there is no evidence that increased exposure to primary education after the Guizot law or that higher suffrage levels in municipal elections led to short- or medium-run industrial wage growth by the 1860s. The consistently small and unstable coefficient for education, along with large standard errors, suggests either no real effect in this period or an effect too diffuse to be detected within the available sample. Meanwhile, several control variable retain their explanatory power, with the exception of cereal productivity in 1815 which loses significance when robust and clustered standard errors are used. The overall consistency of the estimates across the three specifications seems to indicate the model is stable, although the precision of inference does vary.

As mentioned previously, the weak instruments test yields an F-statistic above the conventional threshold. The low p-value associated seems to indicate that we can reject the null hypothesis that the instrument is irrelevant and does not explain variation in the change in male pupils per 10,000 people. This confirms the instrument provides sufficient explanatory power in the first stage, reducing the risk of weak instrument bias in the 2SLS estimate. Our main concern regarding our model arises from the Wu-Hausman test, which compares the consistency of OLS and 2SLS estimates by testing whether the suspected endogenous regressor (change in male pupils) is actually endogenous. Indeed, we fail to reject the null hypothesis, meaning that we do not find any statistical evidence that OLS estimates are biased due to endogeneity in the education vari-

able. These diagnostics suggest that while our instrument is valid and strong, the education variable may not be significantly endogenous in this context. This is surprising given the grounded theoretical justifications for endogeneity concerns that we listed earlier. In this context, we argue using 2SLS remains justified as conservative and credible identification strategy.

Altogether, these robustness checks confirm that the lack of significance for education and political participation remains consistent. Our data provide no evidence of a short-run wage return to educational expansion in the early Guizot era. By contrast, population dynamics, economic structure, and fiscal capacity emerge as consistent drivers of wage growth across the mid-19th century. These findings highlight the importance of interpreting education reform effects in light of timing, context, and complementary institutional conditions.

Lastly, it should be noted that our measure for the number of pupils by arrondissement in 1850 has been interpolated, based on available départements 1850 aggregates, as well as département- and arrondissement-specific characteristics. While we tried to make sure these figures were as accurate as possible and matched the département totals, we prefer to be careful with the interpretation of our results. We believe further extensions to this study would allow us to more closely inspect this relationship, and then confirm or not our findings.

7   Discussion

7.1   Interpretation in the light of the literature

This project intended to tackle the interaction effect of education and political participation through democratic institutions on economic development, which most canonical works have treated separately. While Glaeser et al. (2004, 2007) highlight education as a precursor to democracy, and Acemoglu et al. (2005, 2019) argue democracy fosters growth independently. Thus, we attempted to test both mechanisms in interaction using two simultaneous quasi-natural experiments, hoping to fill a gap in this literature: the lack of causal empirical studies on their combined effect.

Eventually, we find a positive and significant effect of primary education on industrial wages in a static setting, but not in our dynamic specification. This might reflect the absence of short- and medium-run effects of primary schooling, which potentially suggests long lags. In addition, we also fail to identify a significant link with suffrage expansion, as well as any robust evidence for an interaction effect between our measures for education and democracy. Overall, our results suggest that each mechanism might operate independently, at least in the short and medium run. Indeed, the temporal structure when dealing with historical data is crucial; even though the reforms might have happened, all the outcomes do not necessarily materialize immediately. Consequently, in mid-19th century France, education might have had more visible effects on economic development, whereas suffrage expansion and local political participation's effects were weaker, at least in the short- and medium-run.

Moreover, a reason why we do not identify a causal effect between suffrage

levels and wages could be that municipalities under the July Monarchy had very limited powers. Even if there is a "democratization" of the municipal administration, the difference between more or less democratic communes might be weak in terms of institutional differences. As a result, this setting might not be appropriate when studying the institutional growth channel under the July Monarchy. Instead, like Degrave et al. (2024), we might be capturing instead the change in political behavior, whose relationship with economic growth (at least in the short- to medium-run) is less documented.

While both the static and dynamic designs offer valuable insights into the relationship between institutional reforms and economic outcomes, their interpretation warrants caution. In the static design, observed wage levels may reflect long-standing structural differences (i.e. "triangle of backwardness"), raising concerns about omitted variable bias despite rich controls. In the dynamic design, although the IV strategy isolates policy-induced variation in education, the absence of significant effects may reflect timing issues rather than a true absence of impact. Additionally, both models rely on historical administrative data, which may be subject to measurement limitations or residual confounding. Together, these factors suggest that results should be viewed as suggestive, and interpreted in light of historical context and identification assumptions, opening the door for further extensions on the topic.

7.2 Extension propositions

First, we believe this project would greatly benefit from studying the effects of primary education and political participation on economic development using other outcomes. Unfortunately, the lack of reliable data at the arrondissement level over the period makes this task all the more difficult. However, the industrial surveys can provide other useful information regarding industrial development. For example, one could observe the impact of the two policies on labor market composition, using the number of industrial workers per arrondissement, or by looking at female and children employment. Indeed, we can expect a shock on primary education will have an effect on child labor, but also on women and children wages induced by changes in the labor force. Physical capital accumulation has also been identified as a key source of economic growth, the evolution of steam engine use could be a good proxy for such factor; it would then be possible to observe whether mechanization and the density of machines was related to education levels. In addition, the surveys give information on the total value produced per year in each plant, as well as information on intermediary goods used, which could allow to recreate a measure of the value produced per worker across firms. This measure might work as a better proxy for human capital than average wages, and leaves the door open to further inquiries. Last, one could use data on brevets (patents) to estimate whether mass education or limited democratization had an impact on technological innovation during that period.

Furthermore, we believe our models could be improved with new additional controls. Indeed, we discussed the concerns raised by the "triangle of backwardness" and the distribution of communes by size for our empirical strategy. While

we try to account for a few key characteristics, we did not include what seemed to be the main determinant of the exposure to both policies: population density. We use cereal productivity and the percentage of rural population as controls, but these measures remain at the département level and might not fully account for this factor. That being the case, we propose to include data from the Postal Survey of 1847, which provides detailed information the dispersion of housing per municipality. This variable has already been used in Montalbo (2021), but because we suspect the database to be prone to merging errors, we did not retrieve it for our design. Nevertheless, with enough time to clean and harmonize data, we are confident one could compute an index of population dispersion at the arrondissement level. For the same reasons, we recognize our specification lacks information on transportation networks and connectivity. We think the model fit could be improved by including data on canals, roads, and ports as further controls for infrastructure development and trade.

Last but not least, we suggest retrieving the industrial nomenclature established in Squicciarni & Voigtländer (2015) to distinguish "old" from "modern" technology firms. By doing so, we could observe whether primary education or political participation had an impact on the modernization of the industry, or even if their effects increased significantly in areas with a higher concentration of a particular type of firm. Note that, still inspired by the same paper, an additional approach would be to examine the joint effects across sectors across arrondissements. Unfortunately, the code to recreate the paper's main dataset is not available, but we are sure it could be recreated from our industrial survey data and careful inspection of Squicciarni's and Voigtländer's criteria.

8   Conclusion

This paper provides new evidence on the causal effects of primary education policy on local economic development by leveraging the implementation of the 1833 Guizot Law in France. Exploiting population-based assignment thresholds at the commune level and assembling a novel arrondissement-level dataset focusing on the period between 1830 and 1865, we find that higher pre-reform levels of male primary education are associated with greater industrial wage levels in the 1840s. These results underscore the potential of early human capital investments to shape regional labor market outcomes, even before policy-driven expansions in access to schooling took full effect.

In contrast, we find no evidence that local political participation, proxied by suffrage rates under the 1831 Municipal Law, had an independent or interactive effect on wages. While municipal suffrage expanded dramatically under the July Monarchy, its economic returns appear to have been limited, possibly reflecting the limited powers of local councils in the centralized and bureaucratic regime. Our results suggest that, in the short to medium run, democratization (without meaningful fiscal or administrative decentralization) may not especially offer gains in local economic performance.

Nonetheless, the interpretation of these findings warrants caution. Indeed, our identification strategy relies on several assumption, including that the dis-

tribution of municipal population sizes across arrondissements is exogenous to unobserved determinants of economic development (conditional on appropriate controls). Yet historical patterns of settlement and institutional legacies such as rurality, preexisting schooling infrastructure, and administrative fragmentation may have jointly influenced both treatment intensity and baseline economic conditions. Although we control for a rich set of covariates and implement robustness checks, the potential for residual confounding remains, particularly due to spatial correlation and the non-random structure of historical communes. These limitations underscore the importance of interpreting our estimated effects as local and conditional on the validity of the underlying assumptions.

By addressing the joint impact of early democratization and primary education within a unified empirical framework, this paper contributes to the literature on the determinants of growth and the debate on the roles of education and democracy. Our findings reinforce the role of schooling as a driver of wages, while casting doubt on the short-run economic efficacy of suffrage expansion in a centralized regime. Future work should explore the long-run political spillovers of mass education and assess whether broader institutional reforms might amplify the economic returns to democratic inclusion.

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10   Appendix

Note: Two départements are always white (NA) on our maps: Meurthe and Moselle. That is because the map of France we use as a reference uses post-1870 borders for these two départements. For the same reason, the Belfort territory does not appear as part of the Bas-Rhin département. We preferred not to include these départements in our maps since they do not correspond to the same areas.
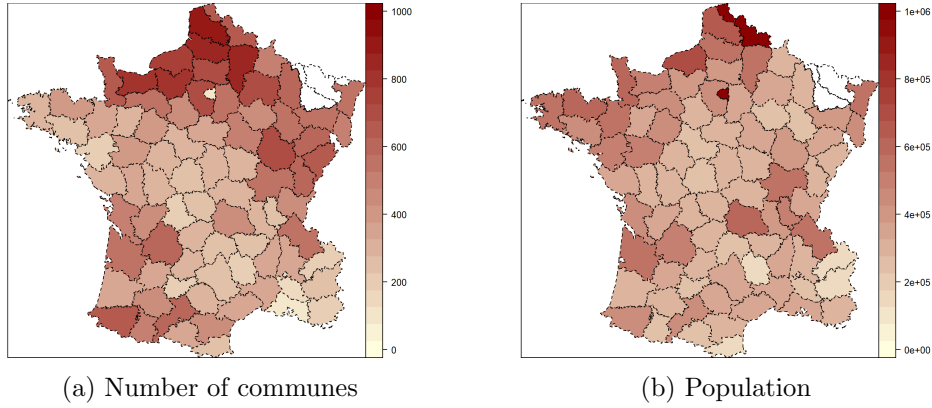
10.1   Population and municipalities density in 1836

(a) Number of communes

(b) Population

Figure 9: French départements in 1836 (data: Cassini)

10.2   The effects of the Guizot law on education

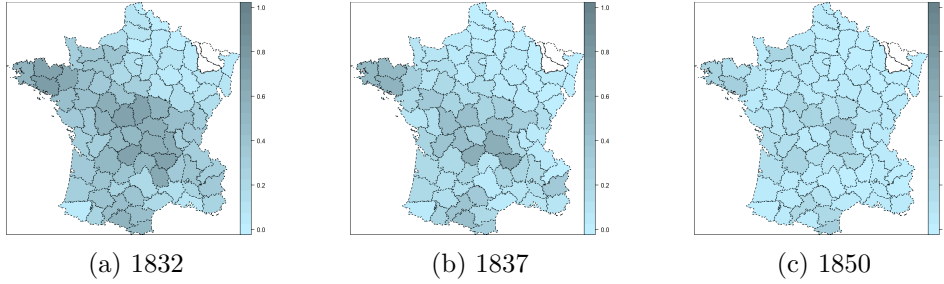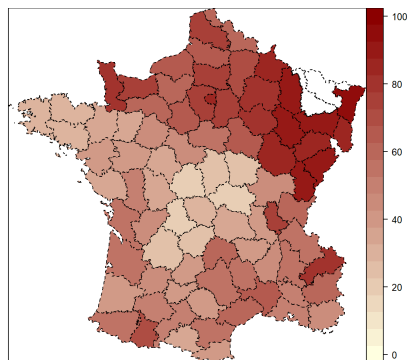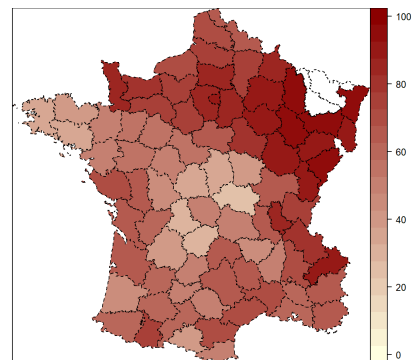
(a) 1832

(b) 1837

(c) 1850

Figure 10: Share of municipalities without a primary school (data: SGF)

41

(a) 1836-40
(school years: 1822-33)

(b) 1851-55
(school years: 1837-1848)

Figure 11: Percentage of conscripts (20y/o) able to write and read (data: SGF)