Criminals Can't Spell: Crime and the Guizot Law in 19th-century France

Étienne COMPÉRAT

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1 Introduction

"To open a school is to close a prison."

The idea of a causal link between education and crime has long occupied a prominent place in political debate, weel before it became a subject of empirical investigation in economics and other social sciences -as illustrated by this famous aphorism attributed to Victor Hugo. Suggesting that expanding access to schooling can reduce crime aligns with a broader view of education as a means of promoting civic responsibility, moral development, and social cohesion. This perspective is particularly resonant in the context of Hugo's quote: 19th-century France and the creation of its national school system, beginning with the Law on primary instruction of 1833 (Loi Guizot). Initiated under the direction of the Minister of Public Instruction, this first step was motivated less by economic development than by a desire to establish social order through the moralization of the lower classes. [1]

In this project, we will address whether a positive shock to primary education leads to a decrease in crime rates by studying the Guizot Law of 1833, which mandated every commune above 500 inhabitants in France to open and fund a primary school for boys. This law marked a pivotal moment in the erection of a national school system and provides a quasi-natural experiment to study the causal impact of early education policies. Thus, we propose to answer the following question: Did the implementation of the Guizot Law lead to a measurable decrease in crime rates in 19th-century France?

Understanding the determinants of crime is a central question in public policy. Among the many proposed levers to reduce criminal behavior, education has long been viewed as a powerful, non-coercive tool. In that regard, this project explores the non-pecuniary returns to education, highlightinh how "soft" policy instruments like schooling can influence individual behavior in ways that go beyond labor markets. Modern empirical studies consistently to find a negative relationship between schooling and crime, yet evidence from historical settings remains limited.

We believe the effect of education on crime in 19th-century France is relevant for several reasons. First, it would shed light how foundational public education policies shaped social outcomes during a time economic modernization and political liberalization. Secondly, it would help test theories of education and crime in a context free of modern confounders, as most the literature has focused on modern data. Thirdly, in many developing countries, debates continue over the role of education in promoting not just economic growth, but also civic behavior and social cohesion.

[1] Allier, 1976

We hope to contribute to the literature on the impact of education on crime by using historical data to answer one of its core question: What type of education policies help reducing crime?[2] Modern empirical studies have shown that an increase in educational attainment significantly reduces violent and property crime[3]. Several explanations have been put forward, such as human capital gains [4], learning patience and risk-aversion, or changes in social interactions[5]. However, these works often rely on recent data and tend to focus on high-school or college education. By focusing on primary schooling in the 19th-century, we hope to see whether basic-level education and literacy have an impact while avoiding modern confounders. Secondly, this project builds on papers studying the effects effect of the Guizot Law in 19th-century France. So far, they have emphasized on the economic and cultural outcomes of primary education[6]. We propose to go further by looking at other social benefits of such policy. Finally, we would add to the History and political science literature on the process of state-building and liberalization in 19th-century France, through the creation of a national school system and mass education.

Our empirical strategy compares crime rates across two adjacent birth cohorts - one exposed to the reform and one not -within a fixed effects framework that absorbs age, time, and département-specific confounders. Literacy is measured at age 20, reflecting early-life educational attainment, while crime outcomes are observed at multiple ages throughout adulthood. The analysis draws on rich administrative data from 19th-century France, combining archival sources on education, literacy, population, and criminal prosecutions at the cohort-départment level.

In the following sections, we will first discuss our empirical design, then we will describe the data we are using before casting a critical eye at this project.

2 Empirical design

We want to compare two births cohorts from the same département at different periods of time. One went to school before the Guizot law (1833) was passed, the other afterwards.

For reasons linked to data availability, the birth cohorts extend over 5 years. Our design focuses on the individuals born between 1816 and 1820, and those born between 1826 and 1830. Thus, if we consider children to go to primary school between the ages of 6 and 13, $cohort_{16-20}$ went to school between 1822 and 1833, and those from $cohort_{26-30}$ between 1832 and 1843. We exclude $cohort_{21-25}$ which went to school between 1827 and 1838 because we wanted the Guizot law to be effective during the whole primary school years of our cohorts.

We make the following assumptions:

1. Every increase in literacy rate over the period is due the positive education shock induced by the Guizot law.

2. Primary education drives literacy rates, such that variations in literacy rates are due to different levels of exposition to primary education.

3. There is no fundamental difference between two cohorts from the same département appart from their literacy rate and exposition to the primary schooling.

The model relies on the Guizot Law as a source of quasi-exogenous variation in literacy. On the one hand, $cohort_{16-20}$ is the baseline cohort, whose literacy levels

[2] Hjalmarsson & Lochner, 2012
[3] Lochner & Moretti, 2004
[4] Lochner, 2004
[5] Becker & Mulligan, 1997
[6] Montalbo, 2021a; Montalbo 2021b; Blanc, 2024

reflect pre-reform schooling access. Its members are not exposed to the Guizot Law and can be considered as untreated. On the other hand, $cohort_{26-30}$ is fully exposed to the reform, such that their increased literacy can be attributed to the reform; it forms the basis for identifying the treatment effect and can be considered as the treated group.

In our study, we propose a panel data regression model with multiple fixed effects in order to estimate how literacy rate affects crime rate across different cohorts, location, years, and ages. By doing so, we aim to isolate the relationship between early-life human capital (literacy) and later-life social outcomes (crime).

$$CrimeRate_{cdta} = \lambda_d + \lambda_t + \lambda_a + \beta Literacy_{cd} + \epsilon_{cdta}$$

Where:

- $CrimeRate_{cdta}$ is the crime rate of cohort c in département d in years t at age-range a.
- $Literacy_{cd}$ is the literacy rate of cohort in département d when its members reach 20 years old.
- λ_d absorbs time-invariant département characteristics: département fixed effect.
- λ_t absorbs any shock that hits all cohorts in a given year: time fixed effect.
- λ_a absorbs the baseline propensity to commit crime at age a (e.g. young people are more likely to commit crimes): age fixed effect.

In this model, β captures the causal effect of literacy on crime, identified through the quasi-exogenous variation induced by the refom. Specifically, it tells us how differences in literacy across cohorts and départements relate to differences in their crime rates, controlling for time, location, and age effects.

Our model is specifically designed so that the variation in literacy across cohorts and départements directly captures the intensity of treatment exposure to the 1833 Guizot Law, eliminating the need for an explicit treatment indicator. Because the reform applied uniformly across France but its impact varied depending on pre-existing educational infrastructure and local implementation capacity, the resulting increases in literacy differ across départements. By using cohort-départment-level literacy at age 20 as our key explanatory variable, measured after the treated cohort would have completed primary education, we exploit this continuous variation as a proxy for treatment intensity. This allows us to estimate the effect of actual gains in literacy, rather than simply contrasting treated and untreated groups in a heterogenous adoption design, and embeds the reform's quasi-experimental variation directly into the regression framework.

3 Data

3.1 Literacy

We gather data on literacy from the Statistique Générale de la France with the "Enseignement Primaire" reports from 1829 to 1897.[7] This database provides us with information on education in France throughout the 19th century with data at the département level. Thus, we retrieve the number of young male individuals (20 years old) that knew how to read between 1836 and 1840 and between 1846 and 1850, based on the information collected for local conscription lists. Divided by the number of people on these "listes de tirage", we obtain an approximation of men literacy rates for both

[7] Statistique Générale de la France, INSEE

cohorts in each département. Unfortunately, we do not have an equivalent of these data for women, but since the Guizot Law specifically targeted male education we do not expect notable changes within the 10 years that separate our cohorts. Eventually, we can also make the reasonable assumption that if there were changes in women literacy rates due to the reform, their sign and intensity are very likely correlated to variations in men literacy rates in the département.

3.2 Crime

Then, we retrieve data on crime from the Comptes Généraux de l'Administration de la Justice Criminelle, statistical yearbooks published by the Ministry of Justice. Starting in 1826, they were based on reporting by local court public prosecutors and clerks. They provide detailed informations on the number of people charged and acquitted of different types of crimes or offences in each département every year, as well as some socio-demographic characteristics. In this study, we are specifically interested in two of them: the number of people accused by département of origin and by age range. [8]

Because of the age-range structure of the data, we prefer to exclude the 21-25 years old individuals from our design. Indeed, this is the only age category that contains only 4 years (since 25 is excluded), which means we are leaving one year of the cohort out of the data. [9] We also remove the 16-21 category since literacy is evaluated at 20 years old among potential conscripts. Moreover, we decided to limit the age-range to 45 years old, given our empirical strategy does not allow us to account for the evolution of département-specific characteristics over time. This amounts to a total of 4 age-ranges for 2 birth-cohorts over 85 départements, as shown in the following table with the relevant yearly reports:

Cohort/Age	25-30	30-35	35-40	40-45
1816-1820	1845	1850	1855	1860
1826-1830	1855	1860	1865	1870

Unfortunately, the reports do not give information on crime rates per cohort with the département of origins. Using the data on natality for each département provided by the "Mouvement de la Population, 1800-1925" from the Statistique Générale, we propose two ways to reconstruct yearly cohort crime rates - defined as the ratio of the number of people of a cohort accused to the number of births within that cohort, in each département of origin:

1. $$CrimeRate_{cdta} \simeq \frac{\text{Number of people from cohort c accused in d at time t and age a}}{\text{Cohort c size in d}}$$

   The first "naive" option to retrieve cohort crime rates would be to divide the number of people in the age-range accused by the number of births in the département during the corresponding years. The issue with this approach is that it relies on a very strong assumption which is there is no mobility across départements. If this assumption might have been essentially true at the beginning of the 19th century [10], towards the end approximately half of the population died in a département different from their département of birth. Even though the Comptes Généraux show that the majority of crimes were committed by people born in the département, there were indeed a non-negligeable amount of them committed by individuals that were not born there.

[8] See Figures 1 and 2

[9] Note that we could also approximate the crime rate for 4 birth-years and keep the literacy level of the 5 years cohort, assuming it did not change much in the excluded birth-year.

[10] See Figure 3

2.

$$CrimeRate_{cdta} \simeq \frac{\text{Number of people from cohort c and departement d c accused at time t and age a}}{\text{Cohort c size in d}}$$

The second option is to estimate the number of convicted people in each département-cohort using data from Figure 2. First, we compute the ratio of the number of people born and accused in the same département to the number of people accused in this département, and multiply it by the number of accused people in that age range in the département in order to get an estimate of: the number of people in a cohort that were born and accused in the département. Second, we compute the ratio of the number of people born in a département but accused elsewhere to the number of people accused outside the département, and multiply it by the number of accused people in that age range outside the département in order to get an estimate of: the number of people in a cohort that were born in the département and accused elsewhere. Third, we add these two estimates to get the approximate number of people accused in a cohort born in a département.

Both options have issues, as the lack of more granular data forces us to find ways to estimate some key values. Here, we also makethe assumption that individuals will receive education from their département of origin, where their literacy level will be evaluated once they reach 20 years old will not leave it before they get drafted (20 years old), when we compute the literacy rate at the département level.

Furthermore, our method for constructing cohort sizes raises concerns related to mortality -especially child-mortality. To address this, we suggest to use département-level data on the population of children aged 5 to 15 in 1821, dividing it by the total number of births between 1806 and 1816 to estimate child mortality rates and adjust cohort sizes accordingly. [11] However, we are unable to account for other cohort or département-specific sources of mortality, such as epidemics, wars, or localized health shocks, for which no reliable data are available. Nonetheless, as long as mortality does not vary systematically across cohorts within départements, the département fixed-effect should absorb time-invariant mortality differences across départements, while time fixed-effects should absorb time shocks affecting all cohorts equally.

4 Comments

While we take several steps to ensure the credibility of our empirical strategy, our analysis is suject to a number of limitations, which we discuss in this section.

4.1 Data granularity

The first limitations of our project stem from the data sources we rely on. Tables from the Comptes Généraux have not yet been fully digitzed, which prevents us from presenting preliminary results at this stage. As previously mentioned, although this source is exceptionally rich -particularly regarding sociodemographic characteristics- it lacks sufficient granularity. As a result, we are constrained to working at the département level and must infer certain values, such as cohort-specific crime rates by département.

The same applies the "Mouvement de la Population" data, from which we construct cohort populations in the absence of reliable age-specific population figures. we are optimistic that this limitation could be addressed through further digitization of "Recensement" reports by the Statistique de la France. For example, the earliest digitized census (1851) provides a detailed breakdown of the population by age and département. If similar data were made available for earlier years, we could reconstruct cohort populations at the département level with much greater accuracy. We chose not to use the 1851 data in our current study because, by that year, the youngest individuals in our cohort would already be 21 years old - and many may have migrated from their département of birth by then.

[11] Mouvement de la Population, Statistique Générale de la France (INSEE)

Finally, although working at the département-level may raise concerns about the number of observations, our cohort-based design -where the dependent variable is observed over several periods- helps mitigates this issue.

4.2 Inference on Cohort populations and Crime rates

In order to reconstruct cohort-specific crime rates, we had to make assumptions about the composition and persistence of cohorts over time. A key concern is mortality bias, since we do not account for people who died over the years within the cohorts. While we propose a method to address child mortality using département-level data on youth population, other sources of mortality -such as famine, war, repression or epidemics- vary by cohort and location in ways we cannot fully observve. This may lead to overestimation of cohort sizes. However, we believe that because our model includes département fixed effects, time fixed effects, and age fixed effects, much of this residual variation is absorbed, limiting the potential for mortality-induced bias in our estimate of the effect of literacy on crime.

A second source of potential bias stems from migration, particularly involving individuals who (i) received their education in a different département than their place of birth, (ii) were registered on military draft lists in a different département, or (iii) were accused of crimes outside their département of birth. We made assumptions regarding the first two cases and proposed two ways to address the third when computing crime rates.

The first (naïve) method divides the number of accused individuals in a given age group by the number of births in the corresponding cohort and département. While simple and transparent, this approach assumes no migration and may overstate cohort size in départements with net out-migration and is therefore biased under mobility. The second (adjusted) method estimates the number of accused individuals born in a département -both those who commit crimes locally and those who do so elsewhere- using observed proportions of local-born accused. This method better reflects migration patterns, especially later in the 19th century, but relies on the assumption that these proportions are stable and representative across age groups, and is potentially noisy. Overall, we prefer this method in our main specification.

4.3 Clustering Standard Errors (Département)

One potential limitation of our empirical model is the possibility of correlated residuals within départements across cohorts, age groups, or years. For instance, unobserved département-level factors may persist over time and affect multiple observations. If such within-département correlation in the error term is not accounted for, it could lead to underestimated standard errors and inflated statistical significance. To address this concern, we propose to cluster standard errors at the département level, which allows for arbitrary forms of correlation in the residuals within each geographical unit and ensures valid inference in the presence of grouped dependence.

5 References

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6 Figures

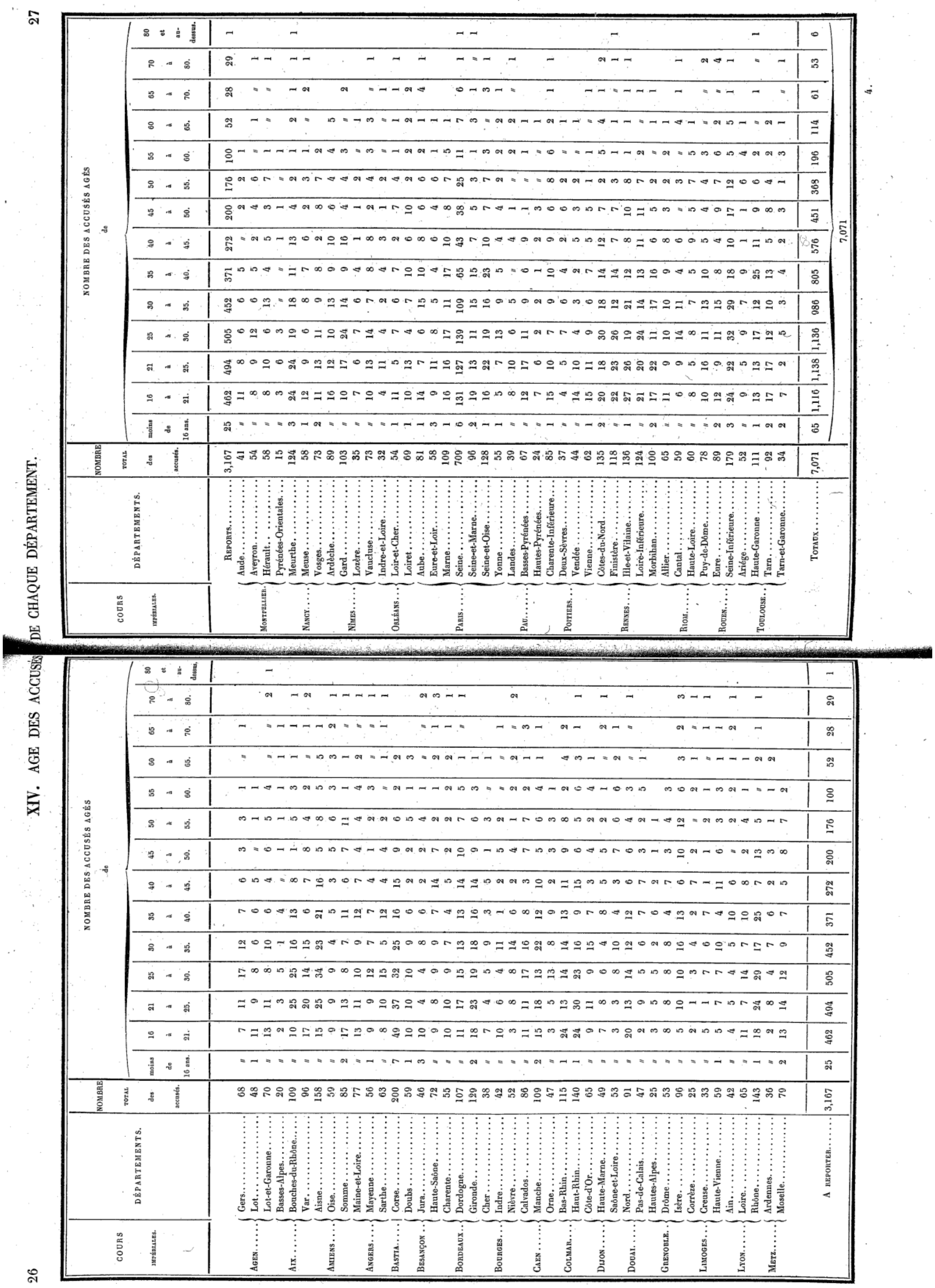
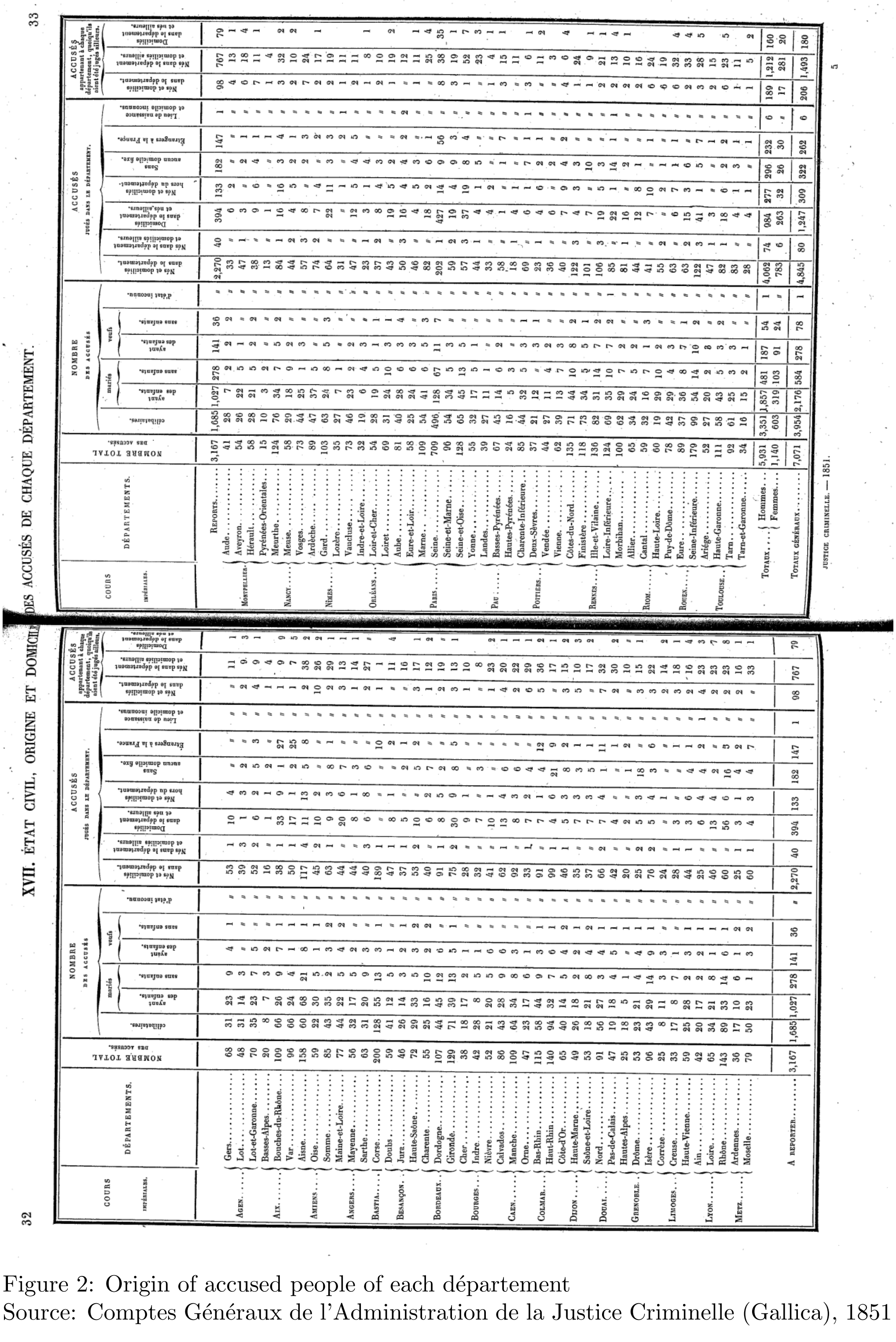

XIV. AGE DES ACCUSÉS DE CHAQUE DÉPARTEMENT.

Figure 1: Age of accused people by département
Source: Comptes Généraux de l'Administration de la Justice Criminelle (Gallica), 1851



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XVII. ÉTAT CIVIL, ORIGINE ET DOMICILE DES ACCUSÉS DE CHAQUE DÉPARTEMENT.

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Cours impériales	Départements	Nombre total des accusés	Nombre des accusés: célibataires	Mariés: ayant des enfants	Mariés: sans enfants	Veufs: ayant des enfants	Veufs: sans enfants	D'état inconnu	Accusés jugés dans le département: Nés et domiciliés dans le département	Nés dans le département et domiciliés ailleurs	Domiciliés dans le département et nés ailleurs	Nés et domiciliés hors du département	Sans aucun domicile fixe	Étrangers à la France	Lieu de naissance et domicile inconnus	Accusés appartenant à chaque département, quoiqu'ils aient été jugés ailleurs: Nés et domiciliés dans le département	Nés dans le département et domiciliés ailleurs	Domiciliés dans le département et nés ailleurs
Agen	Gers	68	31	23	9	4	1	″	53	1	10	4	″	″	″	″	11	1
	Lot	48	31	14	3	″	″	″	39	3	1	3	2	″	″	2	9	3
	Lot-et-Garonne	70	35	23	7	5	″	″	52	2	6	2	5	3	″	4	9	1
Aix	Basses-Alpes	20	8	7	3	2	″	″	16	″	1	1	2	″	″	1	4	
	Bouches-du-Rhône	109	66	26	9	7	1	″	38	1	33	9	1	27	″	1	9	9
	Var	96	66	24	4	1	1	″	50	1	17	1	2	25	″	1	7	5
Amiens	Aisne	158	60	68	21	8	1	″	117	4	11	13	5	8	″	2	38	2
	Oise	59	22	30	5	1	1	″	45	2	10	2	″	″	″	10	26	2
	Somme	85	43	35	2	3	2	″	63	1	9	3	8	1	″	2	29	1
Angers	Maine-et-Loire	77	44	22	5	4	2	″	44	″	20	6	7	″	″	3	13	1
	Mayenne	56	32	17	5	2	″	″	44	″	8	1	3	″	″	1	14	1
	Sarthe	63	31	20	9	3	″	″	40	3	6	8	6	″	″	2	27	″
Bastia	Corse	200	128	55	13	3	1	″	189	1	″	″	″	10	″	1	1	
Besançon	Doubs	59	41	12	5	1	″	″	47	1	8	1	″	2	″	″	11	4
	Jura	46	26	14	3	2	1	″	37	1	5	″	2	1	″	″	16	
	Haute-Saône	72	29	33	5	3	2	″	53	2	10	″	5	2	″	3	17	1
Bordeaux	Charente	55	25	16	10	2	2	″	40	″	6	2	7	″	″	1	12	2
	Dordogne	107	44	45	12	6	″	″	91	1	8	5	2	″	″	2	19	″
	Gironde	129	71	39	13	5	1	″	75	2	30	9	8	5	″	3	13	1
Bourges	Cher	38	18	17	2	1	″	″	28	″	9	1	″	″	″	1	10	
	Indre	42	28	8	5	1	″	″	32	″	7	″	3	″	″	″	8	
	Nièvre	52	21	20	5	6	″	″	41	″	10	1	″	″	″	1	23	2
Caen	Calvados	86	43	28	9	6	″	″	62	1	13	4	6	″	″	4	20	1
	Manche	109	64	34	8	3	″	″	92	″	8	3	6	″	″	2	22	1
	Orne	47	23	17	6	1	″	″	33	1	7	2	4	″	″	6	29	1
Colmar	Bas-Rhin	115	58	44	9	3	1	″	91	″	7	1	4	12	″	5	36	2
	Haut-Rhin	140	94	32	7	6	1	″	99	1	4	6	21	9	″	″	17	1
Dijon	Côte-d'Or	65	40	14	5	4	2	″	46	1	5	3	8	2	″	3	15	2
	Haute-Marne	49	26	18	2	2	1	″	35	″	7	3	3	1	″	5	10	3
	Saône-et-Loire	53	18	21	8	4	2	″	37	″	7	3	5	1	″	″	17	2
Douai	Nord	91	56	27	3	4	1	″	66	2	7	4	1	11	″	7	32	
	Pas-de-Calais	47	19	18	4	5	1	″	42	″	4	″	″	1	″	2	30	2
Grenoble	Hautes-Alpes	25	18	5	1	″	1	″	20	″	2	″	1	2	″	″	10	″
	Drôme	53	23	21	4	4	1	″	25	2	5	3	18	″	″	3	15	1
	Isère	96	43	29	14	9	1	″	76	2	5	4	3	6	″	3	22	
Limoges	Corrèze	25	8	11	3	3	″	″	24	″	″	1	″	″	″	2	14	2
	Creuse	33	17	8	7	1	″	″	28	1	3	″	″	1	″	3	18	1
	Haute-Vienne	59	25	28	2	3	1	″	44	1	3	6	4	1	″	2	16	4
Lyon	Ain	42	20	17	2	2	1	″	25	″	6	4	4	2	1	4	23	3
	Loire	65	34	21	8	1	1	″	46	″	13	4	2	″	″	2	23	7
	Rhône	143	89	33	14	6	1	″	60	″	56	6	16	5	″	2	23	8
Metz	Ardennes	36	17	10	6	1	2	″	25	1	3	1	4	2	″	2	16	1
	Moselle	79	50	23	1	3	2	″	60	1	4	3	4	7	″	″	33	1
	À reporter	3,167	1,685	1,027	278	141	36	″	2,270	40	394	133	182	147	1	98	767	79
	Reports	3,167	1,685	1,027	278	141	36	″	2,270	40	394	133	182	147	1	98	767	79
Montpellier	Aude	41	28	7	2	2	2	″	33	″	6	2	″	″	″	4	13	1
	Aveyron	54	26	22	5	1	″	″	47	1	3	″	2	1	″	6	18	4
	Hérault	58	28	21	5	2	2	″	38	″	9	6	4	1	″	7	11	1
	Pyrénées-Orientales	15	10	3	2	″	″	″	13	″	1	″	″	1	″	1	4	
Nancy	Meurthe	124	76	34	7	5	2	″	84	1	16	16	3	4	″	3	32	2
	Meuse	58	29	18	9	2	″	″	44	2	4	5	2	1	″	2	10	2
	Vosges	73	44	25	1	3	″	″	57	3	8	″	2	3	″	7	24	
Nîmes	Ardèche	89	47	37	5	″	″	″	74	2	7	4	″	2	″	2	17	1
	Gard	103	63	24	8	5	3	″	64	″	22	11	3	3	″	2	19	
	Lozère	35	27	7	1	″	″	″	31	″	″	1	″	2	1	1	11	
	Vaucluse	73	46	23	2	2	″	″	47	″	12	5	4	5	″	2	11	
Orléans	Indre-et-Loire	32	19	6	4	3	″	″	23	1	3	1	4	″	″	1	8	1
	Loir-et-Cher	54	28	19	5	1	1	″	37	2	8	4	3	″	″	2	10	
	Loiret	69	31	24	10	3	1	″	43	″	19	5	2	″	″	1	19	2
Paris	Aube	81	40	28	6	3	4	″	50	3	16	4	4	2	2	″	12	
	Eure-et-Loir	58	25	24	6	3	″	″	46	″	4	5	3	″	″	1	11	1
	Marne	109	54	41	6	5	3	″	82	″	18	2	6	1	″	″	25	4
	Seine	709	496	128	67	11	7	″	202	1	427	14	9	56	″	8	38	35
	Seine-et-Marne	96	54	34	5	3	″	″	59	2	19	4	9	3	″	3	19	1
	Seine-et-Oise	128	65	45	13	5	″	″	57	3	37	19	8	4	″	1	52	7
	Yonne	55	32	17	5	1	″	″	44	1	4	1	5	″	″	″	23	3
Pau	Landes	39	27	11	1	″	″	″	33	″	4	2	″	″	″	1	4	1
	Basses-Pyrénées	67	45	14	6	2	″	″	58	″	1	″	1	7	″	3	15	1
	Hautes-Pyrénées	24	16	5	3	″	″	″	18	1	4	1	″	″	″	″	11	
Poitiers	Charente-Inférieure	85	44	32	5	3	1	″	69	″	6	1	7	1	1	3	6	1
	Deux-Sèvres	37	21	12	″	3	1	″	23	1	4	6	2	1	″	″	11	2
	Vendée	44	27	11	4	2	″	″	36	″	6	″	2	″	″	″	3	
	Vienne	62	39	13	7	3	″	″	40	″	7	9	4	2	″	4	6	4
Rennes	Côtes-du-Nord	135	71	44	10	8	2	″	122	3	4	3	3	″	″	1	24	
	Finistère	118	73	34	5	5	1	″	101	″	7	″	10	″	″	1	9	1
	Ille-et-Vilaine	136	82	31	14	7	2	″	106	3	19	5	3	″	″	2	21	1
	Loire-Inférieure	124	69	35	10	7	2	1	85	″	22	1	14	1	1	2	13	4
	Morbihan	100	62	29	7	2	″	″	81	1	16	″	2	″	″	2	10	1
Riom	Allier	65	34	24	5	2	″	″	44	″	12	8	1	″	″	2	16	
	Cantal	59	32	16	7	1	3	″	41	″	7	10	″	1	″	6	24	
	Haute-Loire	60	19	29	10	2	″	″	55	2	″	2	1	″	″	6	19	
	Puy-de-Dôme	78	42	29	4	3	″	″	63	″	6	7	1	1	″	6	32	4
Rouen	Eure	89	37	36	8	7	1	″	63	2	15	3	6	″	″	2	33	4
	Seine-Inférieure	179	99	54	14	10	2	″	122	3	41	1	5	7	″	3	28	5
Toulouse	Ariége	52	27	20	2	8	″	″	47	1	3	″	″	1	″	2	15	
	Haute-Garonne	111	58	43	5	3	2	″	82	1	18	6	2	2	″	6	23	5
	Tarn	92	61	25	3	3	″	″	83	″	4	1	3	1	″	1	11	
	Tarn-et-Garonne	34	16	15	2	1	″	″	28	″	4	1	″	1	″	1	5	2
Totaux	Hommes	5,931	3,351	1,857	481	187	54	1	4,062	74	984	277	296	232	6	189	1,212	160
	Femmes	1,140	603	319	103	91	24	″	783	6	263	32	26	30	″	17	281	20
Totaux généraux		7,071	3,954	2,176	584	278	78	1	4,845	80	1,247	309	322	262	6	206	1,493	180

JUSTICE CRIMINELLE. — 1851.

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Figure 2: Origin of accused people of each département
Source: Comptes Généraux de l'Administration de la Justice Criminelle (Gallica), 1851

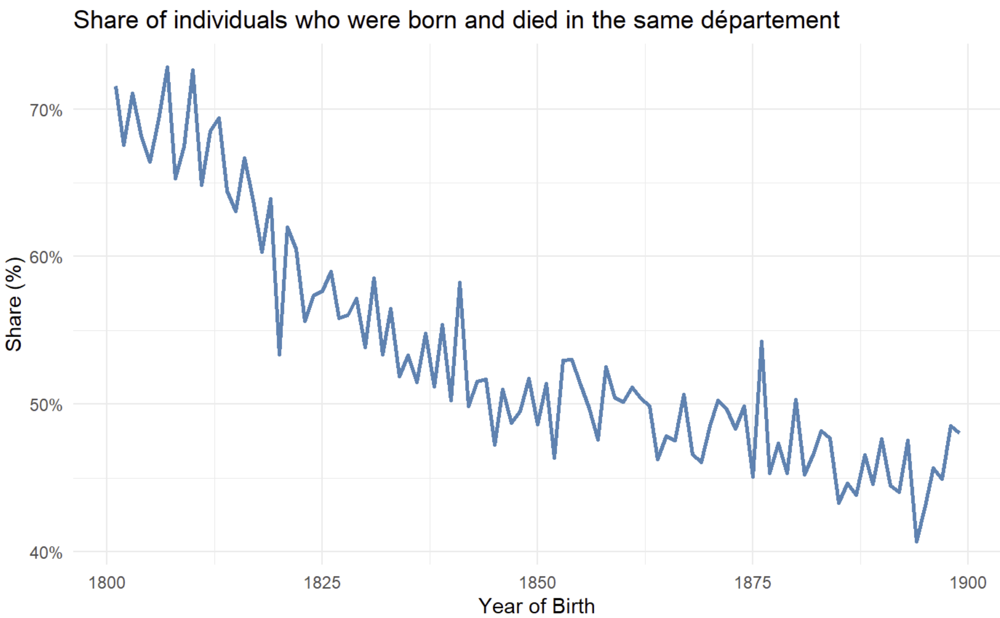

Figure 3: data from Blanc & Kubo (2024) for the French population