The Impact of Attorneys’ Abundance on Litigation Quality. Evidence from France.

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Abstract

This study examines the impact of lawyer concentration on litigation outcomes in French civil courts. Using a novel dataset and a multidimensional approach, the paper suggest that higher lawyer concentration is negatively correlated with court outcome. In particular, the evidence suggests that higher attorneys’ concentration is associated with an increase number of rejections of plaintiffs' lawsuits, a proxy of frivolous lawsuits. In essence, if there are more lawyers competing for clients there is a rise in strategic litigation, with weaker cases introduced in court to secure fees even if rejection is likely. The results confirm theories about lawyer-stimulated demand and draw attention to potential quality issues in legal representation under excessive competition. Policy implications are drawn to address issues impairing judicial efficiency and access to fair outcomes.

**Keywords**: lawyers; supply induced demand; litigation; rejections

1. Introduction

The legal profession plays a crucial role in facilitating access to justice systems around the world (Heise, 2000). However, lawyers have been described as a group dominated by extroverted and sometimes unreliable individuals (Rydell et al., 2007). They are occasionally criticized for wasting valuable court time in pursuit of higher fees (Kalven, 1960). Citizens and litigants regularly express dissatisfaction with congestion, costs, and delays plaguing justice administration (Pal, 2021; Garoupa, 2008).

From a theoretical perspective, there are rational incentives for lawyers to prolong their involvement in cases. It is reasonable for attorneys to bill more hours in order to obtain higher fees (Myers, 1992) and increase their business costs (Bessen & Meurer, 2012). They may also seek to modify judicial interest rates to their advantage (Acciarri & Garoupa, 2013). Some literature argues lawyers are incentivized to intentionally delay justice through "an inexhaustible arsenal of dilatory techniques" focused on postponing trials for as long as profitable (Kidder, 1974, p. 31; Belli, 1981). This perspective posits procrastination stems from economic motives to generate highly idiosyncratic (Flanders, 1980) and lengthy outcomes benefiting legal fees (Economides, 2018). Scholars have characterized the legal profession as prone to exaggeration in evaluations (Monek, 1983), needless case initiation (Voigt, 2016), and filing frivolous petitions solely for litigation leverage (Lidge, 2009). Theories attribute problematic delays to intertwined relationships between attorneys, litigation markets (Posner, 1997), and financial returns from prolonged disputes (Tahura, 2022).

In terms of resolving matters efficiently, lawyers reportedly seek to prolong efforts over time in pursuit of greater compensation (Hadfield, 2000; Macey & Miller, 1991). They exacerbate settlement delays (Lederman & Hrung, 2006) by slowing negotiations (Helland & Tabarrok, 2003) and arbitration (Thornicroft, 1994). However, empirical analyses must reliably demonstrate how fee structures impact proceeding durations (Kritzer, 2002). Processing times involve pretrial opportunities (Johnson, 1997), interest rates (Spurr, 1997), data asymmetries (Fenn et al., 1999), complexity (Di Vita, 2010), and formalities (Djankov et al., 2003). Courts failing to manage swelling dockets rely on incoming (Felli et al., 2008), pending (Ariola et al., 1979), and lingering (Bielen et al., 2018) caseloads.

Robust literature assesses the lawyer-induced demand hypothesis, including work by Bielen & Marneffe (2017), Finocchiaro Castro & Guccio (2014), Carmignani & Giacomelli (2009), Ginsburg & Hoetker (2006), and Bar-Gill (2005). Notwithstanding, mixed findings counsel care with empirical data given endogeneity complicating causality determinations and methodological imperfections in analyzing competitive influences (Mora-Sanguinetti & Garoupa, 2015; Clemenz & Gugler, 2000; Rosales & Jimenez-Rubio, 2016).

France offers a unique case study due to lawyers historically shaping legal culture and social life while assisting justice and judges in upholding freedoms (Dewatripont & Tirole, 1999; Bell, 1994; Thouzellier, 2012). However, surging attorney numbers now overburden the profession (Deffains & Thierry, 2014). Justifications for supplier-induced demands relate to economic advantages from unwarranted lawsuits and complex trials obscuring information from paying clients (Brickman, 2011; Tabarrok & Helland, 2005).

Previous research has found extreme levels of competition among French lawyers, particularly in the sphere of labor law, can foster detrimental outcomes like excess litigation (Fraisse et al., 2015). Literature also postulates economic motivations may incentivize procedural delays given attorneys receive compensation correlating to resolution timeframes (Baudel, 2006).

Despite considerable theoretical and empirical work exposing potential inefficiencies of lawyer competition, this study homes in on institutional perspectives, hypothesizing monopolistic structures could stimulate better legal arguments and deter baseless cases (D'Agostino et al., 2012). Excess competition may engender adverse selection as lawyers exploit supplier-induced demand dynamics for financial gain rather than serving public interests (Abel, 2012).

This manuscript tests if concentrating lawyers territorially heightens rejection rates at the Tribunal de grande instance (TGI), France's initial court of civil jurisdiction presided by legal representation (Fricero, 2015). Hearings consist either of tripartite judicial panels or individual magistrates.

A dual analysis is undertaken. First, innovative 2012-2019 data and instrumental variables derived from archives address omitted biases in estimating competition's impact on dismissals. Second, advanced quantile regressions reveal how rejection determinants shift across the entire dispute distribution per evolving lawyer concentrations, as formalized by Chernozhukov and Hansen (2005).

Results show lawyer concentration uplifts rejections over all observations. This linkage presumably stems from demand stimulation theory but may parallel diminished representation quality. Additional evidence implies institutional distortions profoundly mold legal demand, necessitating policy remedies for enduring processing problems.

The remainder is organized as: Section 2 outlines methodology; Section 3 reports empirical outcomes and robustness testing; Section 4 discusses implications; conclusions complete the manuscript. Throughout, citations from the extensive literature are maintained to substantiate insights.

2. Data and Conceptual FrameworkTop of Form

The dataset underpinning this analysis encompasses 157 jurisdictions across France and 1,256 observations spanning 2012 to 2019. Data originates from national sources including the INSEE, Ministry of Justice statistics bureau, and the BNF national library. Overseas territories are excluded owing to limited reporting.

Acknowledging constraints, the research relies solely on court-aggregated data due to prohibitions on studying individual judge behaviors in criminal matters. This necessarily precludes analyzing variance at the judicial level and influences findings to exclusively reflect macro tendencies.

Per economic postulation, consumer demand for professional services correlates positively with supplier quantity (Buonanno & Galizzi, 2012). Standard theory assumes amplified practitioners leverage informational asymmetries to stimulate superfluous demand that enriches profits over social welfare.

Lawyers hold potential not only to generate novel cases through expansive networks, but also introduce frivolous suits prejudicial to justice due their specialized acumen in discerning merit. Logically, increasing lawyer populations relative to legal need should correlate with higher dismissal rates as non-starters are pursued regardless of futility recognized among peers. Rational plaintiffs forego baseless actions forecasted to fail.

On these premises, a relationship between rising legal saturation points and concurrent rejection totals could evidence demand inducement dynamics outweighing social obligations to curtail litigation conducive only to remuneration, not resolution. Testing this is the aim of modeled associations between variables that account for endogenous influences through careful methodology respecting data constraints.

To test this hypothesis, we use a two-stage least squares method to estimate the empirical coefficients using the following simple model parameters:

*Rejectionsc,t = β1 lawyersc,t + β2 Xc,t + αct + εc,t*

The dependent variable *Rejectionsc,t* represents the rate of court cases introduced by the plaintiff and then rejected by courts of first instance (*c*) at each year of observation (*t*). The explanatory variable of interest is the variable *lawyersc,t* which is the number of lawyers registered with the competent bar association per 100.000 inhabitants. The law requires mandatory legal representation and limits the jurisdiction of lawyers to the court of appeal where the lawyer is registered (Fricero, 2021), allowing for the precise number of lawyers practicing in each TGI jurisdiction to be considered. The variable is instrumented as described in the following section.

Since institutional and socio-economic factors can stimulate inductive demand for litigation, we include a vector of control variables – *Xc,t*– in the model. This vector includes various indicators that may affect rejection rates.

The first judicial-related group includes data from the Ministry of Justice:

1. The rate of backlogged court cases – constructed as the ratio between the stock and ended cases (Bielen et al., 2018) – captures potential court inefficiencies (Voigt & El-Bialy, 2016) that might contribute to higher rejection rates due to rushed evaluations or overburdened judges. If courts with high lawyer concentration also have higher backlogs (Hossain, 2019), it's crucial to control for this factor to isolate the true impact of lawyer density.
2. The rate of withdrawn cases, that controls for cases resolved outside the court system due to weak arguments (Molot, 2014), settlements (Stipanowich, 2004), or strategic maneuvers (Goodpaster, 1992). This helps isolate the impact of lawyer concentration on cases actually reaching litigation and potentially influencing rejection rates.
3. Lawsuits’ duration – in months – reflects potential complexities in the legal stakes (Grajzl & Zajc, 2017) that might lead to rejections for procedural errors, time-related dismissals, or strategic delays by lawyers (Wistrich & Rachlinski, 2012).

The second group reflects socio-economic factors and includes:

1. Population density - measured as the ratio of population and judicial boundaries in the territory (INSEE, Ministry of Justice) – to controls for potential variations in caseload due to the number of people residing within the court's jurisdiction (Heydebrand & Seron, 1986). Higher population density might lead to more lawsuits (Eisenberg et al., 2012; Calhoun, 1962) potentially impacting rejection rates independently of lawyer concentration.
2. An inequality indicator, which is measured by INSEE has the difference between the first and ninth decile in population disposable income. It measures potential disparities in access to justice (Sandefur, 2008) and the quality of legal representation (Agmon, 2021; Gilman, 2014). Larger wealth gaps might indicate that individuals from poorer backgrounds are more likely to file weaker cases (Wexler, 1969) or have less effective representation (Kohler‐Hausmann, 2011), leading to higher rejection rates
3. The rate of individuals without any diploma[[2]](#footnote-2) – data from INSEE – controls for potential variations in legal awareness (Merry, 1990) and ability to navigate the legal system (Cantrell, 2001). Lower educational attainment might be linked to weaker case preparation or misunderstanding of legal procedures (Burke, 2002), potentially influencing rejection rates.
4. The unemployment rate is provided by INSEE and controls for the potential impact of economic hardship on litigation trends (Bachmeier et al., 2004). Higher unemployment rates might be associated with increased desperation (Shapiro & Ahlburg, 1982) leading to more lawsuits – potentially weaker ones (Chopra, 2010) – impacting rejection rates.

Finally, the model includes time indicators (αc,t) to restrict the correlation of the components of unobserved results in annual observations and the usual unobserved random term (εc,t). At the same time, error types are grouped into a single entity at the level of years and jurisdictions.

The paper outlines a sensitivity analysis that removes specific types of courts from the main analysis to assess the robustness of the findings. These exclusions aim to control for potential confounding factors and achieve cleaner results in understanding the relationship between lawyer concentration and rejection rates in French courts.

Firstly, to mitigate potential biases and unwanted variability arising from their different characteristics and data inconsistencies, courts with unique histories and operational structures are excluded in the robustness checks. These courts, like newly introduced, suppressed, and detached tribunals, might have undergone significant changes and lack information about various key variables like registered lawyers, case duration, and backlogs. Focusing on established, stable courts allows the analysis to isolate the impact of lawyer concentration within a more uniform legal system.

Secondly, additional robustness checks remove courts with exceptionally high lawyer densities. These outliers, representing a small proportion of the sample but having significantly more lawyers per capita, could introduce excessive variability that obscures the overall relationship under investigation. Analyzing courts with more similar lawyer densities allows for a more focused examination of the specific effect within a controlled range.

It's important to acknowledge that excluding these courts might limit the generalizability of the findings to the entire French legal system, as they represent aspects of the national legal landscape. However, these controls are crucial for a cleaner understanding of the complex relationship between lawyer concentration and rejection rates, recognizing that specificities of certain tribunals might require further empirical exploration.

Eventually, to understand how the impact of lawyers’ density changes for different groups of courts, we use a special statistical technique called the Smoothed Instrumental Variable Regression Estimator (SIVQR). This method, developed by Chernozhukov & Hansen (2005) and adapted by Kaplan & Sun (2017), considers the entire distribution of our data, not just averages. The SIVQR aims to provide an overall view of a distribution (Fernandes et al., 2021) that performs appropriate approximations in calculations and inferences (He et al., 2021) and offers more precise estimation with bootstrap refinement (Kaplan & Sun, 2017). In our analysis, we use Bayesian bootstrapping with 1,000 repetitions to calculate error margins.

Descriptive statistics of the data are presented in Table 1.

Table 1: Descriptive statistics

|  |  |  |  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- | --- | --- | --- |
| Variable | n | Mean | S.D. | Min | .25 | Mdn | .75 | Max |
| Rate of rejected cases  | 1250 | 14.67 | 10.13 | 1.67 | 7.82 | 10.32 | 19.66 | 85.77 |
| Rate of withdrawn cases | 1250 | 6.89 | 6.35 | 0.58 | 4.17 | 5.13 | 7.05 | 78.71 |
| Rate of backlogged cases | 1250 | 98.42 | 45.98 | 30.59 | 69.09 | 83.55 | 109.87 | 349.82 |
| Population density | 1256 | 417.44 | 1917.17 | 14.77 | 59.45 | 108.91 | 197.25 | 21258.82 |
| Gap rich/poor (in euro) | 1256 | 24799.39 | 4774.91 | 19149.75 | 22006.45 | 23690.00 | 25918.50 | 58170.00 |
| Unemployment rate | 1256 | 9.50 | 1.85 | 4.93 | 8.18 | 9.31 | 10.48 | 15.25 |
| Rate of individuals with no diploma  | 1256 | 25.78 | 2.86 | 18.60 | 23.55 | 25.72 | 27.68 | 32.86 |
| Lawyers/100.000 inhabitants (2012-2019) | 1247 | 40.62 | 106.66 | 1.08 | 8.51 | 19.43 | 38.24 | 1340.06 |
| Lawyer/100.000 inhabitants (1865) | 1256 | 10.95 | 9.05 | 0.00 | 4.93 | 8.34 | 14.05 | 54.02 |

Note: The table provides descriptive statistics of the dataset, composed of a total sample of 141 courts, between 2012 and 2019, based on the data provided by the French Ministry of Justice, the Bibliothèque nationale de France, and the INSEE.

2.1. Potential endogeneity bias

The relationship between the number of lawyers in a region and the rate of litigation has piqued the interest of researchers for some time. Most studies suggest the existence of a relationship but acknowledge a form of intrinsic endogeneity in the lawyer concentration variable (Mora-Sanguinetti & Martínez-Matute, 2019; D’Agostino et al., 2012; Nakazato et al., 2010), based on simultaneity issues (Ginsburg & Hoetker, 2006). the presence of more lawyers might encourage frivolous lawsuits, while simultaneously, higher litigation rates could attract more lawyers to the region. This circularity can create biases in traditional statistical analyses, leading to misleading conclusions.

One promising strategy involves utilizing historical data as instruments. This builds on the logic that past lawyer concentration levels likely influenced current litigation rates, but wouldn't be directly affected by current litigation levels due to the time lag. Notably, we were able to trace all accredited legal professionals practicing in each imperial court found in the 1865 annual report on the judicial system of the Ministry of Justice in France ("Compte général de l'administration de la justice civile et commerciale") provided by the National Library of France. This data covers the period after the reunification of France with the Duchy of Savoy and the County of Nice, and before the German annexation of Alsace-Lorraine.

The long history of lawyers in France (Karpik, 1999) dates back to the thirteenth century, demonstrating long-standing traditions and stability in the legal profession. While lawyer distribution across departments shows inequality (Boigeol, 2003), this likely reflects a historical "sedimentation" of institutions and cultural factors (Karpik, 1988; Cohen-Tanugi, 1989). France's distinct legal culture developed over centuries (Garapon, 1995) within a historically stable profession (Osiel, 1990).

This historical path dependence suggests a positive link between lawyers registered with imperial courts and current legal professional concentration. As anticipated, our data demonstrates this connection (Figure 2).

Figure 2: The relationship between nowadays lawyers and imperial-era attorneys’ concentration.



Note: The figure represents the relationship between lawyers per 100,000 inhabitants registered by the French Ministry of Justice during the imperial era and the current number of attorneys (in logarithm).

This positive correlation, however, isn't enough to directly use historical lawyer concentration as an instrument. While examining lagged observations could help explain current lawyer distribution (Reed, 2015), several conditions must be met. Key among them is meeting the exclusion restriction, which means the instrument should not directly affect the dependent variable (rejection rate) in our model.

In this regard, we perform the D'Haultfoeuille et al. (2021) test for exclusion restrictions with a single instrument. This test acts as a simple check, assessing a small causal impact in small subgroups where the instrument's influence is likely minimal (Acemoglu et al., 2022). The test, using a large number of simulations, reveals we cannot reject the null hypothesis at the 1% significance level (KS statistic = 29.832). This signal that there is ground to consider that exclusion restrictions are met.

After establishing the theoretical rationale for our instrumental variable (IV), the next crucial step is to rigorously evaluate its strength using various diagnostic tests. These tests help us identify potential weaknesses and underidentification issues that could compromise the validity of our results. Assessing the IV's strength is essential to ensure it credibly fulfills its role as an unbiased instrument that isolates any true causal effect.

3. Empirical Results

The first stage of the 2SLS regressions is reported in Table 2. The table presents the results of three regressions with different exclusion criteria for courts. Overall, the coefficient for the main variable of interest, "Lawyers/inhabitants (1865)", remains positive and significant across all three models, suggesting a positive relationship between historical lawyer concentration and current lawyer concentration. However, the magnitude and significance of other coefficients vary depending on the excluded courts. In effect, the coefficient magnitude decreases slightly when excluding new/merged courts and then more substantially when excluding tribunals having big attorneys’ bars. This suggests that these excluded courts might have higher historical lawyer concentration, influencing the overall relationship.

Table 2: First stage of the two stage least squared regressions (2SLS)

|  |  |  |  |
| --- | --- | --- | --- |
|  | (1) | (2) | (3) |
| VARIABLES | Lawyers/inhabitants | Lawyers/inhabitants | Lawyers/inhabitants |
| Lawyers/inhabitants (1865) | 1.593\*\*\* | 1.170\*\*\* | 1.411\*\*\* |
|  | (0.257) | (0.201) | (0.114) |
| Backlogs rate | -0.000132 | 0.0370 | -0.0676\*\*\* |
|  | (0.0659) | (0.0670) | (0.0233) |
| Withdrawal rate | -0.140 | -0.147 | -0.166\*\* |
|  | (0.148) | (0.147) | (0.0778) |
| Duration of cases (in months) | 1.056 | 1.555 | 2.478\*\*\* |
|  | (0.934) | (0.951) | (0.444) |
| Population density | 0.0433\*\*\* | 0.0437\*\*\* | 0.00816\*\*\* |
|  | (0.00383) | (0.00374) | (0.000425) |
| Uneployment rate | 1.303 | -0.331 | -1.251\*\*\* |
|  | (0.928) | (0.701) | (0.366) |
| Gap rich and poor revenues | -0.000245 | 0.000103 | -0.000753\*\*\* |
|  | (0.000634) | (0.000618) | (0.000194) |
| Rate of individuals without diploma  | -5.609\*\*\* | -6.145\*\*\* | -0.632\*\* |
|  | (0.928) | (0.951) | (0.258) |
| Kleibergen-Paap Wald F stat. | 38.41 | 34.04 | 153.2 |
| Cragg-Donald Wald F stat. | 50.93 |  35.28  | 341.9 |
| Montiel-Pflueger effective F statistic: | 38.41\*\*\* | 34.04\*\*\* | 153.2\*\*\* |
| Excluded new and merged courts | No | Yes | Yes |
| Excluded big bars (>500/100k inhabitants) | No | No | Yes |
| Yearly FE | Yes | Yes | Yes |
| Observations | 1,247 | 1,184 | 1,160 |
| Adjusted R-squared | 0.639 | 0.714 | 0.357 |

Bootstrapped – adjusted clustering at id and year level- errors in parentheses

\*\*\* p<0.01, \*\* p<0.05, \* p<0.1

The table shows strong F-statistics according to both the Kleibergen-Paap and Cragg-Donald tests across all three models. These statistics help assess the weakness of the instrumental variable ("Lawyers/inhabitants (1865)"). Generally, an F-statistic above 10 is considered acceptable (Staiger & Stock, 1997), and higher values are needed (Bun & De Haan, 2010) for an instrument valid in terms of bias size (Sanderson & Windmeijer, 2016). In your case, all models exceed this threshold comfortably, ranging from 34.04 to 341.9. This suggests that the instrument is not likely to suffer from weak instrument bias, which can lead to unreliable estimates. Furthermore, the Montiel-Pflueger robust weak instrument test (Olea & Pflueger, 2013) suggest, with a confidence level alpha of 1%, that the maximum worst-case bias is confined to 10% (the critical value is 30.12).

A notable difference across the models is the adjusted R-squared, which indicates the proportion of explained variance. Excluding newly introduced and merged courts leads to a slight increase in R-squared (0.639 to 0.714), while excluding big bars shows a more substantial jump (0.357 to 0.714). This suggests that both exclusions capture relevant factors impacting lawyer concentration.

The second-stage coefficients for the 2SLS and SIVQR regressions are presented across three different specifications to assess the robustness of the findings. Table3 reports the results for the complete dataset, providing insights into the overall relationship across all observed courts. As a measure of placebo population test (Eggers et al., 2023), Table 4 and Table 5 exclude some courts from the dataset. Table 4 does not account for newly introduced, suppressed, and detached tribunals. This allows for a comparison with potentially more established and stable court structures. Finally, Table 5 ignores the courts of Paris, Lyon, Strasbourg, and Toulouse. This focuses the analysis on courts with more similar lawyer densities, potentially reducing the influence of outliers.

Table 3: Two stage least squared and smoothed IV regressions (entire dataset)

|  |  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- | --- |
|  | 2nd stage 2SLS | SIVQR | SIVQR | SIVQR | SIVQR | SIVQR |
| VARIABLES | Rejections | Rejections | Rejections | Rejections | Rejections | Rejections |
| Lawyers/100.000 inhabitants  | 0.0295\*\*\* | 0.0291\*\*\* | 0.0296\*\*\* | 0.0348\*\*\* | 0.0295\* | 0.0485\*\*\* |
|  | (0.00986) | (0.00629) | (0.00793) | (0.0101) | (0.0152) | (0.0155) |
| Backlogs rate | -0.0294\*\*\* | -0.0200\*\*\* | -0.0212\*\*\* | -0.0166\*\* | -0.0269\*\*\* | -0.0269\*\*\* |
|  | (0.00729) | (0.00701) | (0.00664) | (0.00842) | (0.00907) | (0.00889) |
| Withdrawal rate | 0.507\*\*\* | 0.388\*\*\* | 0.404\*\*\* | 0.453\*\*\* | 0.495\*\*\* | 0.593\*\*\* |
|  | (0.0571) | (0.0488) | (0.0375) | (0.0398) | (0.0880) | (0.0992) |
| Duration of cases (in months) | 0.261\*\* | 0.187\* | 0.170\* | 0.131 | 0.248\*\* | 0.0688 |
|  | (0.110) | (0.0999) | (0.0871) | (0.112) | (0.122) | (0.117) |
| Population density | -0.00117\*\*\* | -0.00128\*\*\* | -0.00124\*\*\* | -0.000508 | -0.000609\*\* | -0.000834\*\* |
|  | (0.000447) | (0.000401) | (0.000475) | (0.000646) | (0.000275) | (0.000333) |
| Unemployment rate | 0.160\*\* | 0.176\*\*\* | 0.214\*\*\* | 0.245\*\*\* | 0.169\*\* | 0.221\*\* |
|  | (0.0749) | (0.0578) | (0.0533) | (0.0554) | (0.0793) | (0.0961) |
| Gap rich and poor revenues | 0.000257\*\*\* | 0.000186\*\*\* | 0.000181\*\*\* | 0.000202\*\*\* | 0.000262\*\*\* | 0.000335\*\*\* |
|  | (4.18e-05) | (3.10e-05) | (3.17e-05) | (2.83e-05) | (6.05e-05) | (8.85e-05) |
| Rate of individuals without diploma  | 0.156\* | 0.0741 | 0.0793\*\* | 0.0166 | -0.0164 | 0.00427 |
|  | (0.0819) | (0.0474) | (0.0400) | (0.0557) | (0.0983) | (0.0831) |
| Observations | 1,247 | 1,247 | 1,247 | 1,247 | 1,247 | 1,247 |
| Year indicators | Yes | Yes | Yes | Yes | Yes | Yes |
| Adjusted R-squared | 0.807 | - | - | - | - | - |
| Quantile level | - | 0.150 | 0.250 | 0.500 | 0.750 | 0.850 |

Bootstrapped – adjusted clustering at id and year level- errors in parentheses

\*\*\* p<0.01, \*\* p<0.05, \* p<0.1

Note: The table presents the first and second stage (2SLS) and the smoothed instrumental variable empirical coefficients based on a panel dataset of 157 Tribunals de Grande Instance from 2012 to 2019.

Table 4 takes a closer look at potential influences by excluding newly introduced, suppressed, and detached tribunals. Saint-Gaudens, Saumur, and Tulle tribunals were closed down. Hence, removing them could capture the effect of court closures on lawyer concentration and litigation patterns. Dôle, Guingamp, and Marmande were split, witnessing the creation of "detached chambers." This unique setup might lead to distinct dynamics compared to fully autonomous courts. Finally, Belley and Millau received specific offices for court registries. These hubs might impact case handling and lawyer interaction with the courts in unique ways. Excluding these courts might help isolate the influence of lawyers on traditional court setups, eliminating part of the “noise” created by the legislator’s choices to go “back and forward” in different occasions.

Table 4: Two stage least squared and smoothed IV regressions (Saint-Gaudens, Saumur, Tulle, Dôle, Guingamp, Marmande and Belley and Millau excluded)

|  |  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- | --- |
|  | 2nd stage 2SLS | SIVQR | SIVQR | SIVQR | SIVQR | SIVQR |
| VARIABLES | Rejections | Rejections | Rejections | Rejections | Rejections | Rejections |
| Lawyers/100.000 inhabitants  | 0.0462\*\*\* | 0.0444\*\*\* | 0.0370\*\*\* | 0.0445\*\*\* | 0.0399\*\* | 0.0530\*\*\* |
|  | (0.0140) | (0.00895) | (0.00701) | (0.00954) | (0.0155) | (0.0190) |
| Backlogs rate | -0.0310\*\*\* | -0.0222\*\*\* | -0.0216\*\*\* | -0.0237\*\*\* | -0.0230\*\* | -0.0271\*\*\* |
|  | (0.00809) | (0.00845) | (0.00720) | (0.00830) | (0.00965) | (0.00951) |
| Withdrawal rate | 0.509\*\*\* | 0.423\*\*\* | 0.406\*\*\* | 0.471\*\*\* | 0.508\*\*\* | 0.579\*\*\* |
|  | (0.0581) | (0.0470) | (0.0483) | (0.0455) | (0.114) | (0.107) |
| Duration of cases (in months) | 0.187 | 0.0931 | 0.131 | 0.133 | 0.175 | 0.0787 |
|  | (0.122) | (0.115) | (0.0864) | (0.123) | (0.139) | (0.164) |
| Population density | -0.00190\*\*\* | -0.00207\*\*\* | -0.00160\*\*\* | -0.00169\*\* | -0.000685\*\* | -0.000894\*\* |
|  | (0.000638) | (0.000449) | (0.000507) | (0.000812) | (0.000339) | (0.000350) |
| Unemployment rate | 0.228\*\*\* | 0.233\*\*\* | 0.265\*\*\* | 0.250\*\*\* | 0.193\*\* | 0.222\*\* |
|  | (0.0769) | (0.0677) | (0.0689) | (0.0618) | (0.0894) | (0.0984) |
| Gap rich and poor revenues | 0.000236\*\*\* | 0.000196\*\*\* | 0.000179\*\*\* | 0.000225\*\*\* | 0.000252\*\*\* | 0.000326\*\*\* |
|  | (4.62e-05) | (3.00e-05) | (2.77e-05) | (3.66e-05) | (6.45e-05) | (8.46e-05) |
| Rate of individuals without diploma  | 0.270\*\* | 0.134\*\* | 0.0783 | 0.175\*\* | -0.00542 | 0.0197 |
|  | (0.114) | (0.0574) | (0.0498) | (0.0736) | (0.103) | (0.0989) |
| Observations | 1,184 | 1,184 | 1,184 | 1,184 | 1,184 | 1,184 |
| Year indicators | Yes | Yes | Yes | Yes | Yes | Yes |
| Adjusted R-squared | 0.785 | - | - | - | - | - |
| Quantile level | - | 0.150 | 0.250 | 0.500 | 0.750 | 0.850 |

Bootstrapped – adjusted clustering at id and year level- errors in parentheses

\*\*\* p<0.01, \*\* p<0.05, \* p<0.1

Note: The table presents the first and second stage (2SLS) and the smoothed instrumental variable empirical coefficients based on a panel dataset of 148 Tribunals de Grande Instance from 2012 to 2019.

Table 5 tackles the significant lawyer concentration in major French cities. Paris, Strasbourg, Toulouse, and Lyon present unique environments compared to smaller towns. The disproportionate lawyer concentration in these cities might distort the general pattern observed in "average" courts, potentially leading to biased conclusions. In particular, Paris stands out with a staggering 1,300 lawyers per 100,000 inhabitants, dwarfing the national average of under 25, while Strasbourg, Toulouse, and Lyon also boast significantly higher lawyer densities – e.g., 20 times higher – compared to the rest of France. Hence, these cities could act as outliers, potentially skewing the results. Excluding them offers a cleaner picture of the relationship between lawyer concentration and your outcome variable in a more homogenous context.

Table 5: Two-stage least squared and smoothed IV regressions (Saint-Gaudens, Saumur, Tulle, Dôle, Guingamp, Marmande and Belley, Millau, Paris, Strasbourg, Lyon, and Toulouse excluded)

|  |  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- | --- |
|  | 2nd stage 2SLS | SIVQR | SIVQR | SIVQR | SIVQR | SIVQR |
| VARIABLES | Rejections | Rejections | Rejections | Rejections | Rejections | Rejections |
| Lawyers/100.000 inhabitants  | 0.0389\*\*\* | 0.0327\*\*\* | 0.0370\*\*\* | 0.0400\*\*\* | 0.0460\*\*\* | 0.0504\*\* |
|  | (0.0103) | (0.00824) | (0.00810) | (0.00946) | (0.0160) | (0.0201) |
| Backlogs rate | -0.0258\*\*\* | -0.0223\*\* | -0.0182\*\*\* | -0.0181\*\* | -0.0201\*\* | -0.0266\*\*\* |
|  | (0.00777) | (0.00886) | (0.00681) | (0.00870) | (0.00951) | (0.00917) |
| Withdrawal rate | 0.509\*\*\* | 0.388\*\*\* | 0.406\*\*\* | 0.459\*\*\* | 0.530\*\*\* | 0.581\*\*\* |
|  | (0.0590) | (0.0424) | (0.0408) | (0.0429) | (0.107) | (0.125) |
| Duration of cases (in months) | 0.163 | 0.149 | 0.130 | 0.119 | 0.121 | 0.0700 |
|  | (0.118) | (0.114) | (0.0815) | (0.124) | (0.127) | (0.108) |
| Population density | -0.000267\* | -0.000107 | -0.000186\* | -0.000243 | -0.000387\*\* | -0.000540\* |
|  | (0.000160) | (0.000117) | (0.000102) | (0.000148) | (0.000191) | (0.000305) |
| Unemployment rate | 0.260\*\*\* | 0.218\*\*\* | 0.261\*\*\* | 0.263\*\*\* | 0.209\*\*\* | 0.204\*\* |
|  | (0.0750) | (0.0576) | (0.0564) | (0.0592) | (0.0805) | (0.0998) |
| Gap rich and poor revenues | 0.000270\*\*\* | 0.000171\*\*\* | 0.000174\*\*\* | 0.000203\*\*\* | 0.000311\*\*\* | 0.000388\*\*\* |
|  | (3.88e-05) | (2.36e-05) | (2.29e-05) | (3.44e-05) | (7.75e-05) | (7.60e-05) |
| Rate of individuals without diploma  | 0.0195 | 0.0229 | 0.0126 | -0.0392 | -0.0821 | -0.0619 |
|  | (0.0535) | (0.0409) | (0.0321) | (0.0490) | (0.0851) | (0.0871) |
| Observations | 1,160 | 1,160 | 1,160 | 1,160 | 1,160 | 1,160 |
| Year indicators | Yes | Yes | Yes | Yes | Yes | Yes |
| Adjusted R-squared | 0.834 | - | - | - | - | - |
| Quantile level | - | 0.150 | 0.250 | 0.500 | 0.750 | 0.850 |

Bootstrapped – adjusted clustering at id and year level- errors in parentheses

\*\*\* p<0.01, \*\* p<0.05, \* p<0.1

Note: The table presents the first and second stage (2SLS) and the smoothed instrumental variable empirical coefficients based on a panel dataset of 145 Tribunals de Grande Instance from 2012 to 2019.

4. Discussion

The empirical coefficients in Section 3 tables support several interesting conclusions.

Firstly, the positive and statistically significant first-stage coefficients confirm the hypothesis that the French legal profession developed historically at the intersection of social and institutional factors (Houllemare, 2016). Even after excluding major or unusual courts, the number of lawyers per inhabitant in 1865 positively correlates with current lawyer concentration. This aligns with the literature highlighting the unique nature of the French legal profession (Gauthier, 2014), marked by long-standing monopolies (Lejeune, 2010; Willemez, 1999) that shaped French society (Assier-Andrieu, 2011). Notably, this historical feature demonstrates effectiveness as an instrumental variable, addressing the endogeneity inherent in lawyer concentration (Andriosopoulos et al., 2023; Poppe & Rachlinski, 2015) and showing a form of path of dependence (Wagner et al., 2014)

The results also suggest higher lawyer concentration in areas with dense populations and slower courts. This aligns with France's competitive spatial use (Darly, 2008), lower trust (Lieberman, 1981), and higher conflict potential in dense areas (Eisenberg et al., 2012). It suggests possible influences of court reputation (Zacharias, 2008), complex reasoning needs (Meijers, 1948), and anti-disturbance strategies (Chavent-Leclère et al., 2020) through legal means (Perrot et al., 2017).

Conversely, lower lawyer concentration occurs in areas with higher backlogs, judicial abandonment, unemployment, low education, and inequality, reflecting spatial injustice in France (Lévy et al., 2018). This may be due to regional differences in isolation (Reynès, 2018), disadvantage (Yates et al., 2001), and financial accessibility (Songer et al., 1995), leading to unequal access to legal rights (Bouquet, 2018).

Eventually, we tested the hypothesis of exogeneity (no correlation between the error term and the instrument) using the D'Haultfoeuille et al. test (2021) and we fail to reject, with a 99% confidence level, that exclusion restriction is satified. This supports our claim that the instrument is correlated with the outcome variable (current lawyer concentration) but not directly causing it, which is essential for an instrumental variable. Finally, we evaluated the strength of the instrument using the Kleibergen-Paap F-statistic, Cragg-Donald Wald F-statistic and the Montiel-Pflueger effective F statistic. The strength test results suggest the instrument operates as expected and is not significantly influenced by other factors, reducing concerns about biased estimates.

Interestingly, the results maintain their sign and statistical significance across all specifications, even when excluding courts with exceptionally high lawyer concentration or those recently modified by legislation. This consistency suggests that the observed relationship between lawyer’s concentration in 1865 and the current density of lawyers (2012-2019) is not driven by these specific court types or recent policy changes. It strengthens the argument that the legal profession reflects a broader and more persistent pattern in the French legal system (see the Appendix for some placebo test).

The relationship between lawyer concentration and rejection rates, which constitutes the central focus of our analysis, warrants a dedicated discussion.

Lawyer concentration has a positive and statistically significant association with rejection rates across all models. Excluding specific courts with high lawyer concentration or recent legislative changes does not affect the main findings regarding lawyer concentration's positive impact on rejection rates. Even when excluding major cities known for high lawyer density, the sign and significance of the coefficient for lawyer concentration remains consistent. This suggests the observed relationship is not driven solely by these specific urban contexts. Several explanations could underpin this observed relationship.

In the first place, the legal market is characterized by dispersed competition and high labor intensiveness (Jovanovič,2018). This creates an environment where intense competition may incentivize lawyers to translate even minor issues into legal claims (Sandefur, 2001), potentially contributing to an increase in filings that eventually face rejection. Additionally, research suggests that increased lawyer density might not translate to improved client outcomes (Worden et al., 2018), further supporting the link with higher rejection rates.

In the second place, concentrated legal markets might encourage the filing of frivolous lawsuits (Doriat-Duban et al., 2020). This could be due to the competitive pressure on lawyers to generate income or the belief that judges in such environments are more susceptible to pressure tactics. Higher rejection rates could thus reflect the court system filtering out these non-meritorious claims.

Eventually, concentrated legal markets can lead to various market failures, such as excessive litigation and inefficient resource allocation (Stucke, 2013). This can ultimately reduce social welfare (Cao,2008) as resources are spent on cases with low chances of success. The higher rejection rates you observe could be a symptom of these broader market inefficiencies.

The control variables suggest different important results.

The negative and consistent association between backlog rates and rejection rates suggests that areas with a higher backlog of cases tend to have fewer rejections. This could be due to several factors. Backlogs potentially indicate overburdened court systems, where judges prioritize clearing older cases rather than accepting new ones. This might lead to stricter scrutiny of new filings, resulting in higher rejection rates for cases deemed weak or lacking merit. Furthermore, in congested courts, lawyers might strategically delay certain cases, anticipating that backlogs will force their eventual dismissal (even without formal rejection). This behavior could contribute to lower observed rejection rates for newer cases within the system.

The positive and consistent association between lawsuit withdrawal rate and rejection rates seems counterintuitive. However, several explanations could come into play. On the one hand, in areas with high withdrawal rates, it's possible that many plaintiffs lose faith in the legal system due to factors like delays, costs, or perceived inefficacy. This may lead them to withdraw their cases before facing formal rejection, effectively contributing to the observed correlation. On the other had, high withdrawal rates could also reflect a strategic shift from plaintiffs. They might initiate lawsuits to pressure opponents or obtain concessions without aiming for a formal court judgement, leading to withdrawals before reaching rejection or resolution.

The positive and consistent relationship between unemployment rate, inequality indicators, and rejection rates presents a complex situation.

Higher unemployment and inequality might correlate with limited access to legal resources and support, leading to poorly prepared and ultimately rejected cases. Additionally, individuals from disadvantaged backgrounds might be more likely to pursue weak claims due to desperation or limited alternatives.

It's important to consider potential selection bias, where areas with higher unemployment and inequality might also have other characteristics affecting rejection rates (e.g., specific types of disputes, court practices). Further research is needed to disentangle these complex interrelationships, which might signal room for the reputational and compliance issues mentioned in the literature (Dothan, 2014).

5. Conclusions

This analysis has explored the relationship between lawyer concentration and rejection rates in the French legal system. We found a positive and statistically significant association, suggesting that areas with more lawyers per capita experience higher rates of case rejection. While this finding seems straightforward, its implications demand nuanced interpretation and consideration of broader contexts.

Several theories explain this link. Increased competition in a saturated legal market might incentivize lawyers to frame even minor issues as legal problems (Sandefur, 2001), potentially contributing to an influx of weaker claims ultimately rejected by the courts. This aligns with arguments by Krieger & Neumann (2015) regarding potential issues in legal strategies employed when lawyer concentration is high.

While French lawyers traditionally play a limited, technical role in proceedings (Blasi, 1995), and safeguards exist against professional misconduct (Motala, 2001), the competitive pressures of a large, dispersed legal market (Bessy, 2016) may still create challenges. Overzealous pursuit of cases fueled by competition, even if not fraudulent, could ultimately burden the justice system with frivolous claims and contribute to inefficiencies.

Our findings raise crucial questions about how to balance lawyer concentration with efficient and accessible justice. While lawyer density can offer benefits in terms of choice and expertise, excessively concentrated markets might present drawbacks. Further research could explore the factors underlying these relationships, investigate the specific types of rejected cases in high-lawyer areas, and evaluate potential policy interventions to mitigate potential negative consequences.

Ultimately, ensuring a healthy balance between lawyer density and a well-functioning justice system requires careful consideration of the economic, social, and legal contexts involved. This study provides a starting point for further exploration, aiming to contribute to a legal system that effectively serves the needs of its citizens while maintaining a healthy and ethical professional environment for lawyers.

While exploring the relationship between lawyer concentration and rejection rates has yielded valuable insights, acknowledging and discussing the limitations of our research is equally important. Transparency about these limitations builds trust in our findings and allows for a more nuanced understanding of their applicability and generalizability.

The dataset timeframe, restricted by large-scale geographical reforms affecting French tribunals, imposes a specific lens through which we view our results. It's crucial to consider how these reforms might have influenced lawyer distribution, court practices, and ultimately, rejection rates. At the same time, the need to comply with criminal law and protect individual privacy restricts the use of detailed, judge-level data. While aggregated data offers valuable insights, it necessarily masks individual variations and distinctions within the system.

Eventually, choosing the 2SLS and SIVQR methods to address endogeneity and quantile analysis was a well-considered decision based on current best practices. However, no model is without limitations. It's essential to acknowledge potential criticisms, such as the sensitivity of 2SLS to instrument validity and the assumptions underlying SIVQR.

Future studies could investigate longer timeframes, explore finer-grained data disaggregation while respecting privacy concerns, and experiment with alternative models to achieve a more comprehensive understanding of the complex relationship between lawyer concentration and rejection rates. Additionally, future studies could explore whether an increased number of lawyers can hinder courts' prompt decisions and whether judges' excessive workload affects legal proceedings.

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Appendix

Establishing the validity of the instrumental variable – the number of attorneys per 100,000 inhabitants in 1865 – is crucial for ensuring unbiased estimates in our model. In this analysis, we explore the use of alternative instrumental variables as placebo tests alongside our primary instrument, lawyer density in 1865. A placebo test is like a control experiment in the world of instrumental variables. It acts as a reality check to see if the instrument is truly influencing the outcome due to its intended effect, or if something else might be at play.

In comparison to the altered population test that was provided excluding different courts from the analysis, we test here whether a different instrumental variable has a relevant connection to the first stage outcome. If the alternative instrument shows a significant association, it raises red flags about the validity of our main instrument.

The rationale behind using alternative instrumental variables as placebo tests is twofold. On the one hand, these instruments should theoretically not have a causal relationship with the outcome variable (rejection rates) but share similar characteristics to the main instrument. If they predict the outcome significantly, it provides evidence that the main instrument might also be picking up on confounding factors, raising concerns about endogeneity. On the other hand, a strong main instrument with consistent results across different placebo tests strengthens its case for validity, while inconsistencies might suggest issues with the chosen instrument.

By incorporating multiple placebo tests, we aim to achieve a more comprehensive and rigorous assessment of our main instrument's validity, ultimately contributing to greater confidence and reliability in our findings regarding the relationship between lawyer concentration and rejection rates.

We introduce departmental indebtedness per inhabitant as an additional instrument for lawyers per capita. Increased local debt could incentivize individuals to pursue legal careers as a way to exploit monopolistic rents in a financially complex environment. Additionally, it might attract lawyers specializing in finance or debt collection practices, leading to higher lawyer density in high-debt areas. If the debt accumulation is largely driven by factors beyond individual control (e.g., economic downturns, natural disasters), it might not directly influence rejection rates, satisfying the exogeneity requirement for a good instrument.

The results of the 2SLS regressions suggest a positive (coefficient = 0.0144), but not statistically significant (p value = 0.453), correlation between the departmental indebtedness per inhabitant. The first stage F-statistic is 0.13, showing that the instrument is very weak and it is unlikely to predict the outcome variable.

We also test whether initial instrument – attorneys’ concentration in 1865 – is still valid after the introduction of the additional instrumental variable. The first stage F-statistic of the alternative instrument – departmental indebtedness per inhabitant – is 0.51, showing that the instrument remains weak and it is unlikely to predict the outcome variable, fulfilling the purpose of the placebo test.

On the contrary, attorneys per 100,000 inhabitants in 1865 remains a valid instrument as it has a high F-statistic (34.76), a positive (coefficient = 1.715) and statistically significant (p value = 0.000) coefficient. The strong relationship with lawyer concentration – in the timeframe 2012-2019 – suggests that the instrument might be a valid tool for isolating the causal effect of historical lawyer density on current legal professional concentration and, finally, on lawsuits’ rejection rates.

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2. This variable, provided by INSEE, captures the rate of individuals in France who are aged 15 or over, who are not in school, and who do not have any diploma or qualification, or who have at most a BEPC, brevet des collèges or DNB. The BEPC, brevet des collèges or DNB is a French national diploma that is awarded to students who have successfully completed the lower secondary school cycle. It is the equivalent of the General Certificate of Secondary Education (GCSE) in the United Kingdom. [↑](#footnote-ref-2)