

The Origins of Elite Persistence: Evidence from Political Purges in post-World War II France*

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Abstract

This paper studies a new mechanism that allows political elites from a non-democratic regime to survive a democratic transition: connections. We document this mechanism in the transition from the Vichy regime back to democracy in post-World War II France. The parliamentarians who had supported the Vichy regime were purged in a two-stage process whereby local courts, *Comités départementaux de libération* (CDLs) and a national court, the *Jury d'Honneur*, sequentially decided whether to uphold the ban on participation in politics for each defendant. First, we show that the *Jury* was more likely than the CDLs to clear defendants who were Law graduates, a powerful group in French politics at the time. The difference in clearance rates between Law graduates and other defendants was 10 percentage points higher in front of the *Jury* than in front of the CDLs. This Law graduate advantage was consequential and created elite persistence, as it mainly appeared when defendants intended on continuing their political careers. Second, a systematic analysis of the still-classified 17,589 documents contained in the *Jury* dossiers of the defendants is consistent with the hypothesis that the connections of Law graduates to the *Jury* was a major driver of their ability to avoid the purge. We consider and rule out alternative mechanisms.

Keywords: Purges, Political transitions, Elite persistence, Connections

JEL Codes: D73, K40, N44, P48

*This project receives financial support from the British Academy/Leverhulme Small Research Grants SRG 2018. Olivier Wieviorka contributed greatly at the project's early stages by sharing data, by offering guidance for our archival work and by discussing the functioning of the *Jury d'Honneur*. We thank Julia Cagé, Tommaso Giommoni, Tommy Krieger, Volker Lindenthal, Emiliano Grossman, Andreas Madestam, Jean-Laurent Rosenthal, Argyris Sakalis, Hans-Joachim Voth and Vincent Pons for comments and suggestions. We also benefited from comments by seminar participants at the Paris School of Economics, at the Université Paris-Saclay, at the London School of Economics, at the Université Paris Panthéon-Sorbonne, at the 2021 Economic History Association Meeting (EHA - Tucson) at the workshop on the "Political Economy of Wars and political transitions" (Cambridge), at the 2021 SIOE Conference, at the 2021 Spring Meeting of Young Economists (Bologna), at the 2021 French Economic Association Meetings (AFSE - Lille) at the 2021 European Public Choice Society Meetings (EPCS - Lille), at the 14th CESifo Workshop in Political Economy, at the 2021 ZEW Workshop in Political Economy, at the 2021 Political Economy of Democracy and Dictatorship workshop, at the 2021 Silvaplana Workshop and at the EPAB workshop. We would like to thank Camille Besse, Louise Dutertre and Victor Westergren for their wonderful work as research assistants. All remaining errors are ours.

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1 Introduction

Elites persist. In politics, elite persistence manifests itself through continuing selection of particular elites with direct control over or significant influence on government policy (Linz et al., 1996; O’Donnell and Schmitter, 2013). This creates political inequality (Van Coppenolle, 2020) and serves as a roadblock to policy reform (Acemoglu and Robinson, 2006). Whereas the reasons why the “iron law of oligarchy” (Michels, 1968) operates within *given* institutional structures with its strong tendency to subvert democracy and to enable small elites to dominate are reasonably well-established, how political elites can persist after a major institutional upheaval remains a puzzle.¹ Yet, examples of elite groups that survive major institutional changes are plentiful. For instance, the transition to democracy in Eastern Europe in the 1990s did not eliminate the political power of the old communist elite (Pakulski et al., 1996). In Indonesia, many of the mayors who had already served under the Soeharto regime were elected again after the transition to democracy and even stayed in office longer than other mayors (Martinez-Bravo et al., 2017). In post-Pinochet Chile, González et al. (2021) also observe that mayors appointed by Pinochet held an electoral advantage in municipal elections after the 1990 democratic transition. Historically, the sequence of franchise extensions in the United Kingdom between 1832 and 1885 did little to break the British aristocracy’s monopoly on power (Berlinski et al., 2014). Likewise, after the US civil war and the enfranchisement of African Americans, the Southern white elite managed to maintain *de facto* power for many years (Acemoglu and Robinson, 2008; Besley et al., 2010). How could those elites continue to dominate politics after a transition to democracy?

One possibility is that elite groups have a comparative advantage over larger groups due to their small size and because of concentrated benefits (Olson, 1965). This allows them to invest in *de facto* power that enable them to survive the loss of *de jure* power associated with a transition to a new political system (Acemoglu and Robinson, 2006, 2008). While elite groups have an incentive to invest in maintaining their political power, how they do it is less clear. Acemoglu and Robinson (2008) conjecture that they use wealth or weapons. Michels (1968) emphasized the control of communication technologies and political skills. Besley et al. (2010) focus on the elimination of political competition. In this paper, we document an alternative mechanism: connections.

We argue that members of a former, compromised elite can leverage connections to members of the new elite to survive the transition from autocracy to democracy. By connections we mean both shared social ties and the use of these social ties. Those connections, built before the transition among alumni, colleagues, friends, or relatives, can provide support when the new regime decides which members of the former elite are allowed to participate in politics under the new regime. In short, these connections determine who is purged and who is not.

We document the role of such connections in the purge that took place after World War II in France.² The liberation of France meant an abrupt transition from the Vichy regime, a dictatorship that had cooperated with Nazi Germany since the 1940 military defeat (Paxton, 1972), back to a republic, the Fourth Republic.

¹The classical argument of Michels (1968) is that any complex social organization will eventually be dominated by a small elite because leaders control resources that rank-and-file members do not: superior information, communication technologies, and political skills. In most Western democracies, incumbency advantage (Eggers et al., 2015) and internal legislative procedures (Berlinski et al., 2007) enables the same (type of) politicians to stay in power and these advantages can be passed on to family members (Querubin, 2015; Van Coppenolle, 2017; Fiva and Smith, 2017; Dal Bó et al., 2009). In autocracies, dynastic transitions develop as a norm to avoid the successor problem (Tullock, 1987; Kurrild-Klitgaard, 2000).

²Technically, this is a case of “lustration”. Lustration is a procedure whereby a legal body examines the actions of individuals in order to prevent those compromised with a previous regime to hold a category of positions in a new regime (see, e.g., Kaminski and Nalepa (2006), Nalepa (2008) and Bates et al. (2020)). After the war, France adopted an “accusation-based truth-revelation” lustration procedure that resembled usual court proceedings. For simplicity, we refer to this as a “purge” rather than as lustration.

To reinstate a republic, the post-war authorities had to purge politicians who had collaborated with the Vichy regime and determine who would be allowed to continue to pursue a political career. The role that connections played in the purge was clear to many of the participants at the time. Our primary archival research, for example, revealed a letter from a defendant to General De Gaulle claiming that the purges were “not about justice, but about connections”.³ Three features of the purge allow us to systematically investigate the role of connections in elite persistence. The first feature is that there was a well-identified group of politicians to purge. These were the parliamentarians who had endorsed, in a vote on July 10, 1940, the enabling act that cleared the way for the Vichy regime and those who had directly collaborated with the institutions of Vichy France. Yet, some of the parliamentarians who voted in favor of the act later took active part in the resistance, sometimes shortly after the vote. The new post-war authorities, therefore, had to sift away real supporters of the Vichy regime from those who had given in to pressure at the time of the vote on the enabling act but had later redeemed themselves.

The second feature of the purge is that it followed a structured legal process. To determine the parliamentarians who were to be purged and those who would be allowed to continue their political careers, the post-war authorities set up a two-stage process. Specifically, two bodies were tasked with reviewing the cases sequentially. A case was first reviewed by a local *Comité départemental de libération* (hereafter a CDL), established in each French department with members recruited from local resistance groups. In the second stage, each case was reviewed by the *Jury d'Honneur* (hereafter the *Jury*) in Paris, consisting of three prominent members of the resistance with a Law background. The *Jury* could either follow the judgment of the CDL or overrule it. This two-stage process meant that each case was heard twice: first locally by a CDL and, then, centrally by the *Jury*. Importantly, the process is well-documented, as the *Jury* maintained dossiers on each defendant. From this, we can document how defendants argued their case before the *Jury* and who supported them.

The third feature of the French post-war purge is that there was a well-identified elite group in French politics whose connections to the *Jury* we can study. Specifically, many French politicians were Law graduates. Lawyers, for instance, represented about 20 percent of all parliamentarians in the National Assembly between 1936 and 1940 and many held important government positions (Le Béguec, 2003). During the interwar period, 13 of the 19 presidents of the council of ministers were Law graduates and Le Béguec (2003), accordingly, refers to the Third French Republic (1870-1940) as the “Lawyers’ Republic”. After the War, the political influence of Law graduates remained substantial. For instance, two out of four presidents of the National Assembly were lawyers under the Fourth Republic (1946-58). Law graduates, thus, constitute a well-defined elite group that survived the post-war democratic transition. The members of that elite formed a cohesive social milieu, first established during their studies and subsequent training, and later in their careers, maintained through the Bar Association, clubs, and speech contests. We conjecture that those connections were instrumental in interceding with the three members of the *Jury*, who all had connections to Parisian faculties of Law.

Based on those three features – a target group to purge, a well-defined elite with connections to the *Jury*, and a two-stage legal process – we adopt the method developed by Anwar and Fang (2006) and Alesina and La Ferrara (2014) to study the racial bias of US courts to identify the advantage Law graduates had in front of the *Jury* using the decisions of the CDLs as a counterfactual. To test that hypothesis that the connections of Law graduates gave defendants with a Law degree a specific advantage in relation to the judgments of the *Jury* but not to those of the CDLs, we compare the acquittal rates across courts for different groups of

³“Il s’agit apparemment, non de justice, mais de politique”. Archives Nationales de France, Reference: AL//5324

parliamentarians. The difference-in-differences estimate of the acquittal rates across the two courts provides a measure of the relative advantage of Law graduates in front of one court compared to the other. The main result of this analysis is that the difference in acquittal rates between Law graduates and other defendants was 10 percentage points higher in front of the *Jury* than in front of the CDLs. The *Jury* overruled the decision of the CDLs to purge Law graduates in 26.36% of the cases whereas it only did so in 15.97% of the cases for other defendants. Accordingly, we find that Law graduates had an advantage in front of the *Jury* compared to other defendants and that this advantage did not materialize in front of the CDLs.

We argue that the Law graduate advantage before the *Jury* was due to the connections among Law graduates. We substantiate that claim by analyzing the dossiers of the defendants from the the *Jury*'s archives. From primary archival research, we created an inventory of the 17,589 documents in those dossiers. We collected information on the number and type of letters of support, on the identity of each defendant's supporters, and on the type of evidence that each defendant presented to the *Jury*. While the content of the dossiers of Law graduates and other defendants shared many similarities, they did differ in one important regard: the dossiers of Law graduates contained more letters of support from establishment figures who were well-connected to the *Jury* and many of them were written in an informal style that suggests a personal connection to the receiver of the letter. This is consistent with the claim that the Law graduate advantage in front of the *Jury* emerged because they benefited from connections that could be leveraged to lobby on their behalf.

Our analysis speaks to four strands of literature. First and foremost, we contribute to the literature on elite persistence. We document a new mechanism – connections – explaining why elites persist and survive political transitions that complements existing explanations (e.g., Higley and Burton, 1989; Acemoglu and Robinson, 2006; Martinez-Bravo, 2014; Martinez-Bravo et al., 2017; González et al., 2021). Second, we shed light on an understudied type of political purges: political purges during democratic transitions, thereby contributing to the literature on political purges. The focus of this literature is on purges in autocracies where purges or the threat thereof can protect an autocrat from coups originating from within the ruling coalition (Svolik, 2009; Bueno de Mesquita and Smith, 2017; Montagnes and Wolton, 2019; Goldring and Matthews, 2021). While political purges after a democratization also aim at consolidation, they are fundamentally different from those in autocracies because they are constrained by the rule of law and must happen within a legal framework. We provide a theoretical conceptualization of this process and new empirical evidence on how political purges can facilitate elite persistence that extend previous research hypothesizing that transnational justice might foster representation in new democracies (Ang and Nalepa, 2019). Third, we contribute to the literature on the effect of connections in economics and politics (e.g., Fisman, 2001; Dal Bó and Di Tella, 2003; Cohen and Malloy, 2014; Wolton, 2017). We show that connections can help elite groups surviving transitions from autocracy to democracy. Fourth, we contribute to the literature on bias in sentencing (Voeten, 2008; Shayo and Zussman, 2011; Alesina and La Ferrara, 2014; Lim et al., 2015; Park, 2017). We document that connections of defendants to a court result in more leniency and that biases can appear in transitional justice. Last but not least, we build a dataset featuring 798 decisions on the cases of 399 individual defendants along with the personal characteristics of the defendants and the content of their dossiers. To our knowledge, it is the first to record the decisions to purge politicians during a democratic transition along with detailed information on the defendants.

The rest of the paper is structured as follows. Section 2 presents the historical background of the purge. Section 3 presents a theoretical framework that illustrates how connections can influence court decisions. Section 4 describes the main dataset and reports the baseline results documenting the advantage of Law graduates. Section 5 presents evidence that the source of the Law graduate advantage is connections by

drawing on the information from dossiers of the defendants. Section 6 concludes.

2 Historical background: Political purges in post-World War II France

This section, firstly, describes the historical facts related to the transition from the Vichy regime to the Fourth Republic and to the political purge that took place during this transition. Secondly, it documents the prominent role that Law graduates played in French politics both before World War II and after and that they formed a cohesive and well-connected social group. Finally, based on the dossiers of the defendants from the archives of the *Jury*, it presents anecdotal evidence of the important role connections played in framing the cases put to the *Jury*.

2.1 The transition from the Vichy Regime to the Fourth Republic

On 10 July 1940, in the wake of the French military defeat in the Battle of France, the French Parliament passed an enabling act granting full power to Marshall Philippe Pétain. Until the liberation of France by the Allies, the Vichy regime was nominally in charge of the civil administration of the country, even though the country was first partly then fully occupied. It was located in the provincial city of Vichy in the so-called “free zone”, which Germany eventually occupied on November 11, 1942.

The regime was a dictatorship. It implemented a radical anti-modern reform program known as the “national revolution” (“révolution nationale”), based on Catholicism, political centralization, large capitalist corporations, coercion, and the persecution of Freemasons and Jews. It sided with Germany and Italy and collaborated with them in their fight against the resistance and in persecuting Jews, in fact (Paxton, 1972). The regime collapsed as the allied troops liberated France and the Vichy government was eventually forcibly moved to Germany to serve as a puppet government.

While the Vichy regime was nominally ruling over mainland France, a provisional government had emerged from the various branches of Free France led by General Charles De Gaulle and the French colonies (Paxton, 1972; Albertelli et al., 2019) and at the end of the war, the Provisional Government of the French Republic (“Gouvernement provisoire de la République française” or the “GPRF”) was established.

A key issue facing the GPRF was to dismantle the Vichy regime and restore a republic. In the order of 9 August 1944, the GPRF, therefore, declared void all the acts taken by the Vichy regime. The first elections held by the GPRF were municipal elections on 23 April 1945. Election of a Constituent Assembly followed on 21 October 1945 and, after a first failed attempt, a new constitution was adopted by referendum on 13 October 1946. The Fourth French Republic was officially born, marking the end of the transition away from the Vichy regime.

2.2 The purge

As large parts of French society had been compromised by the Vichy regime, a purge was needed at all levels, from civil servants and politicians to writers, journalists, and intellectuals.⁴ On 21 April 1944, the GPRF published an order rendering ineligible for election to public office various groups of individuals who had compromised themselves during the Vichy regime. The order explicitly singled out parliamentarians who had voted for the enabling act giving full power to Marshall Pétain and those having collaborated with the Vichy regime (Paxton, 1972).⁵

A total of 669 parliamentarians, both deputies and senators, had taken part in the vote on the enabling act. Out of them, 80 voted against the act, 20 abstained, and 569 voted for (Lacroix et al., 2019). By default, the order of 21 April 1944 banned the latter and all politicians belonging to the Vichy institutions from participating in politics. However, the ban could be waived if a parliamentarian could prove that he had taken active part in the resistance movement (Wieviorka, 2001). Until 6 April 1945, departmental prefects could waive the ban and did so for 51 parliamentarians for whom there was indisputable evidence of participation in the resistance (Wieviorka, 2001).

An order of 6 April 1945 describes in detail the procedure to be followed to waive the ban for all the remaining (banned) parliamentarians (henceforth referred to as the parliamentarians). Each case underwent a two-stage legal process and was assessed sequentially, first, by a local and, second, by a national court. At the first stage, the case was considered by Departmental liberation committees (“Comités départementaux de libération” or “CDLs”) which operated in each department, France’s main administrative unit. The CDLs had been created by the resistance and had their legal status confirmed by the French Committee of National Liberation on 21 April 1944. Their composition reflected the balance of power of local resistance groups and the CDLs were, therefore, heterogeneous in their makeup.⁶ They were initially created to organize the resistance movement politically and to administrate departments after their liberation until a formal administration could be appointed (Albertelli et al., 2019). However, as the state apparatus had not actually collapsed, the role of the CDLs was eventually limited and one of their tasks was to assess the dossiers filed by parliamentarians who wanted their ban from politics waived.

At the second stage, each dossier was assessed by the *Jury d’Honneur* (henceforth, the *Jury*), a national court established on 6 April 1945 specifically to purge former supporters of the Vichy regime from politics. The *Jury* was automatically in charge of judging all parliamentarians who had voted in favor of the enabling act or who belonged to the Vichy institutions. It could overrule the decisions of the CDLs. The *Jury* had three members. René Cassin, vice-President of the *Conseil d’Etat*, presided over the Court. He was assisted by Maxime Blocq-Mascart, representing the *Conseil National de la Résistance*, and by André Postel-Vinay, representing the *Ordre de la Libération*. The three members of the *Jury* used one criterion to acquit politicians: “an active (and direct) participation in resistant activities before November 1942”.⁷ Although none of the orders establishing the rules of the *Jury* defined how decisions were to be taken, they were collegiate and announced as a consensus decision.

A striking feature of the *Jury* is that it consisted of Law graduates. Its chairman, René Cassin, was a lawyer

⁴For more historical information on the purges, see Wieviorka (2001); Baruch (2003); Elster (2006).

⁵Politicians belonged to this second category if they had served either on the *Conseil national* or on the *Conseil Départementaux*.

⁶The precise composition of the CDLs is unknown but it is likely to have reflected the composition of resistance groups in the *département*.

⁷Minutes of the first meeting of the *Jury* (quoted in Wieviorka 2001, chap. 5).

at the Paris bar and a Law professor in Paris. He was a prominent figure in the legal milieu during and after the war. The *Dictionnaire historique de la Résistance* (Marcot, 2006) even defines him as “the jurist of Free France” (p. 383). André Postel-Vinay held a bachelor degree in Law and had studied at “Ecole libre des sciences politiques” in Paris, also known as Sciences-Po, where students study a blend of social sciences. The school, created in 1871 by a Professor of Law, Emile Boutmy, was, however, oriented towards the study of Law and had close connections to Law Faculties, as evidenced historically by the composition of its Faculty.⁸ Maxime Blocq-Mascart was a graduate of the same school. In addition, the *Jury* was assisted by rapporteurs assigned to each case and also by administrative staff. That staff mainly came from the *Conseil d’Etat*, the highest administrative court in France. This court is in charge of ruling in administrative disputes. Hence, most of the *Jury’s* members had a Law background. The *Jury* was, moreover, located in the building of the *Conseil d’Etat*. In short, the *Jury* had strong connections to one of the most influential groups in French politics at the time, Law graduates.

2.3 “The Lawyers’ Republic”

Law graduates were an influential interest group for two reasons. First, its structure, with the Bar association, its clubs and speech contests, ensured the cohesiveness of the group. Those characteristics meant that Law graduates bond during their training and later on cultivate these connections within the Bar association (Le Béguec, 2003). Second, Law graduates were historically tightly-linked to French politics. For example, many lawyers studied Law in combination with political science (Le Béguec, 2003). Specifically, a substantial share of parliamentarians in the National Assembly were lawyers. They represented 19.6 percent of parliamentarians in the 1936-1940 National Assembly. Furthermore, they often held positions of power. From January 1920 to March 1940, France had 19 Council Presidents.⁹ Among them, 13 were lawyers (Le Béguec, 2003). During the French Third Republic, Law graduates, hence, formed what Le Béguec (2003) calls the “Lawyers’ Republic”.

After the War, the proportion of lawyers in the Assembly decreased slightly but still amounted to 15.6 percent in 1958 (Le Béguec, 2003). In addition, their influence remained substantial. From 1946 to 1958, two of the four Presidents of the National Assembly had a Law degree. In addition, the first President of the Council of the Republic, the Upper Chamber under the Fourth French Republic, Auguste Champetier de Ribes, was a Law graduate.

Most important to our analysis, as a group Law graduates were more likely than other groups of parliamentarians to survive politically the transition from the Vichy regime to democracy. Specifically, Dogan (1967) estimates that 35.2 percent of the lawyers elected to the Assembly between 1945 and 1958 had served in the Parliament before the war. By contrast, Wieviorka (2001, p.412) estimates that only 20.65 percent of the 1945 National Constituent Assembly had held a political mandate before the war. Likewise, Novick (1985, p.182) estimates that 85 percent of politicians in the first post-war Assembly (1943-1944) had never held a political mandate before war.

⁸Emile Boutmy, for example, published a study on constitutional laws (<https://gallica.bnf.fr/ark:/12148/bpt6k235741/f1.item.texteImage> - Consulted February 25, 2021). A book celebrating the 25 years of the creation of the school (in 1896) shows that both the President of the Board (M. Léon Aucoc) and the General Secretary of Faculty members (M. C. Dupuis) were Law graduates (Source: <https://gallica.bnf.fr/ark:/12148/bpt6k96193204/f9.item> - Consulted February 25, 2021).

⁹This is equivalent to the position of Primer Minister.

In a nutshell, in 1945, Law graduates were a cohesive interest group that had historically assumed a prominent role in French politics. Although the proportion of lawyers in the Assembly decreased after the war, they were more likely than other parliamentarians to politically survive the post-war purge and still secured influential positions. We conjecture that the capacity of Law graduates to survive the regime change was related to their proximity to the *Jury*, whose members, as noted, had a Law background.

2.4 Connections and the post-war purge: Archival evidence

The Archives Nationales de France holds the dossiers of the defendants who were judged by the *Jury*. These documents provide a wealth of contextual evidence that connections, not only were considered by many defendants to have biased the *Jury*, but also that connections were indeed leveraged to lobby its members. Firstly, a common line of defense used by defendants to refute the decision of the *Jury* to purge them from politics was that undue pressure had biased the *Jury*'s decisions. For example, in their statement of defense, defendant A and B directly accused the *Jury* of being partial and the whole (purge) process to be biased.¹⁰ Some other defendants also tried to bypass the decision of the *Jury* by complaining about its partial nature. On October 18th 1945, defendant C wrote in a letter to General De Gaulle: "It is not about justice, but about connections".¹¹ Conversely, some supporters of defendants also used the rhetoric of biased decisions to get them acquitted. For example, defendant's D dossier contains an anonymous letter denouncing "a political plot against" him.¹²

Secondly, many individual dossiers have direct evidence of the intervention of connections. A perfect illustration is the letter defendant E sent to René Cassin on June 6th 1945 to ask for a reappraisal of his case. He writes "I took the liberty to ask you this favor, because many have advised me to use my contacts".¹³ In the dossier of defendant F, there is a letter from one of his supporters asking Fedia Cassin, brother of the president of the *Jury*, if he could help him reach out to René Cassin.¹⁴ The dossiers also show how defendants mobilized their own connections to contact the *Jury* to lobby on their behalf. For example, the leader of a resistance network wrote to defendant G: "I would like to let you know that after learning about the injustice concerning your case, I personally went to see M. Bernard (Rapporteur on the case)".¹⁵ Similarly, the dossier of defendant H contains a note from the cabinet of General De Gaulle forwarding a letter from the defendant to the Ministry of the Interior. The note states "It looks like the case of defendant H deserves some more attention" as the defendant has previously been purged from politics.¹⁶ Finally, in some cases, the dossiers also show defendants contacting their connections to seek help. In this vein, defendant I, in a letter to the president of his political group, wondered if he should go to the *Jury* with an introductory note from him.¹⁷

Thirdly, we observe various degrees of informality in the way the letters in the dossiers address the receiver, suggesting various degrees of familiarity with the members of the *Jury*. For instance, several dossiers include letters from influential figures addressed to René Cassin and with salutations such as "Mon cher ami" ("My dear friend"). For example, the dossier of defendant J includes a letter which a supporter wrote on July

¹⁰Names are anonymized as archives are still-classified. Archives Nationales de France, Reference: AL//5308 and AL//5309.

¹¹Archives Nationales de France, Reference: AL//5324.

¹²Archives Nationales de France, Reference: AL//5321.

¹³Archives Nationales de France, Reference: AL//5298.

¹⁴Archives Nationales de France, Reference: AL//5298.

¹⁵Archives Nationales de France, Reference: AL//5298.

¹⁶Archives Nationales de France, Reference: AL//5331.

¹⁷Archives Nationales de France, Reference: AL//5334.

31st 1945 to René Cassin with that salutation stating that not acquitting defendant J would be a mistake.¹⁸ Similarly, the dossier of defendant K contains a letter from the defendant to the President of the Constituent Assembly on April 1946 asking for support. It is followed by a letter from the President of the Constituent Assembly to René Cassin on October 1946 starting with “Mon cher ami” and asking for a new assessment of the case of defendant K.¹⁹

In conclusion, we observe clear evidence from the dossiers of the defendants that connections played a key role in the interactions with the *Jury*. It remains to determine whether and how such connections systematically influenced the decisions of the *Jury* and, more specifically, how they operated for different groups of defendants. That is the task of the rest of the paper.

3 Theory

3.1 Model overview

The model portrays a situation where a number of defendants who voted for the enabling act or cooperated with the Vichy regime are considered sequentially by a lower (the CDLs) and an upper court (the *Jury*). The courts have to decide if the ban on political participation already imposed on the defendants stands or is overturned. As in Alesina and La Ferrara (2014), each court defines the standard of proof, here evidence of participation in the resistance, required for acquittal. Thereafter, each court evaluates the evidence and the defendants or their supporters can report mitigating circumstances with the aim of influencing the decision of each court. This is the novel aspect of the model. The communication of mitigating circumstances is cheap talk, so there is no guarantee that it will, in fact, influence the courts. The fundamental problem is that the defendants and their supporters have an incentive to claim mitigating circumstances whether there are such circumstances or not. We model two mechanisms that can overcome this problem and allow defendants and their supporters to influence the courts’ decisions. Both mechanisms are related to connections between the courts, on the one hand, and the defendants and their supporters, on the other. The first mechanism is *direct* connections between the defendant and the courts. All defendants can communicate with the courts but at a cost. We assume that a direct connection lowers the cost of communication. Second, *indirect* connections operate via a third party (a supporter of the defendant). Specifically, the defendant is connected to a third party who, in turn, is connected to the courts. The third party cares about the legal process, but also about getting the defendant to whom it is connected acquitted. We show that both of these mechanisms can under certain circumstances help defendants with connections get acquitted and avoid the ban on political participation.

3.2 Setting the bar

Two courts, indexed by $c \in \{L, U\}$, are tasked with judging a fixed number of defendants indexed by $i \in D$. Court L – the lower court (the CDLs) – hears each case first and its judgment can be confirmed or overturned

¹⁸Archives Nationales de France, Reference: AL//5303.

¹⁹Archives Nationales de France, Reference: AL//5311.

by the upper court U (the *Jury*) that hears the case second. Before any evidence is heard, the two courts set their own bar for acquittal. After that, evidence is presented to the courts, they receive letters from the defendants and/or their supporters claiming mitigating circumstances and make their decisions.

Let x summarize the evidence presented to a court with $x \in (-\infty, \infty)$. A defendant who presents evidence stronger than the bar set by that court will be acquitted. To set the bar, we, assume as in Alesina and La Ferrara (2014), that the court's aim is to avoid making type 1 (convicting innocent defendants) and type 2 (not convicting guilty defendants) errors.²⁰ The weights that court c puts on type 1 and 2 errors are α_c and $1 - \alpha_c$, respectively. The evidence presented to the courts is drawn from the cumulative distribution function $A_G(x)$ if the defendant is guilty (did not participate in the resistance) and from $A_I(x)$ if innocent (participated in the resistance). We assume that the set of defendants can be divided into sub-sets based on fixed observable characteristics, such as profession, political affiliation, region of residence, age, religion etc., and that the proportion of guilty defendants in these subgroups may be perceived by the two courts to be different. We let $g \in \{1, 2, \dots, N\}$ with $\cup_g D_g = D$ index these sub-groups. The two courts assume that the proportion of guilty among defendants belonging to sub-group g is π_g . The objective function of court c can, then, be written as

$$\min_{x(c,g)} \sum_{g=1}^N \alpha_c (1 - \pi_g) A_I(x) + (1 - \alpha_c) \pi_g (1 - A_G(x)). \quad (1)$$

The optimal bar for court c for defendants belonging to sub-group g is the solution to

$$\frac{\alpha_c}{1 - \alpha_c} \frac{1 - \pi_g}{\pi_g} = \frac{a_G(x^*(c, g))}{a_I(x^*(c, g))} \quad (2)$$

and denoted $x^*(c, g)$. Given the bar, the probability that a defendant belonging to group D_g is being acquitted before court c is

$$Pr[c, g] = \pi_g (1 - A_G(x^*(c, g))) + (1 - \pi_g) (1 - A_I(x^*(c, g))) \quad (3)$$

$$\equiv 1 - H_g(x^*(c, g)), \quad (4)$$

where $H_g = \pi_g A_G + (1 - \pi_g) A_I$. Applying a first order linear approximation to this, we can write the probability of acquittal as

$$Pr[c, g] \approx a + b_c + b_g. \quad (5)$$

We observe that a defendant's chances of acquittal differ before the two courts for two reasons. First, the courts may weigh the risk of the two types of mistakes differently (differences in α_c). This would lead to systematic differences in the acquittal rates between the courts and is captured by b_c in the linear approximation. This captures a host of differences in the objectives and procedures of the two courts that can lead to differences in acquittal rates. Second, the two courts may perceive, based on observable characteristics, some groups of defendants to be more likely to be guilty than others (differences in π_g). This is a manifestation of statistical discrimination and can explain systematic differences in acquittal rates between different subgroups of defendants and is captured by b_g in equation (5). The next section models how the defendants and

²⁰This feature of the courts' objectives is consistent with the type of democratic purge that we study. A purge in an authoritarian regime would likely give little weight to type 1 errors, if any.

their supporters can claim mitigating circumstances to increase the chance of acquittal. We focus on the role of connections and show how this can lead to systematic differences in the acquittal rates for connected and unconnected defendants.

3.3 Connections

After the bars have been set, the defendants or their supporters can present evidence of mitigating circumstances to the courts. For each defendant i , we assume that there may or may not be mitigating circumstances that a court should take into account. We denote this by θ which can take two values: θ_Y if there are mitigating circumstances and θ_N if not. This is private information to the defendants and their supporters and not known to the courts and cannot be externally verified. The defendants or their supporters may present evidence of mitigating circumstances to the courts by sending letters. We denote the content of a letter sent by defendant i either personally or via a supporter to court c by $l_{i,c} \in \{\theta_Y, \theta_N\}$. If the court accepts a letter claiming that $\theta = \theta_Y$ for a defendant i , then it updates its belief about the cumulative distribution function from which the evidence for that defendant is drawn from $H_g(x)$ to the cumulative distribution function $\bar{H}_g(x)$. We assume that $H_g(x)$ is first-order stochastically dominated by $\bar{H}_g(x)$, i.e., $H_g(x) \leq \bar{H}_g(x)$ for all x with strict inequality over some interval. For simplicity, we write this as

$$\bar{H}_g(x_{c,g}^*) = H_g(x_{c,g}^*) - \eta \quad (6)$$

with $\eta > 0$. In other words, the mitigating circumstances make it more likely that defendant i is acquitted and the effect is increasing in η . We can interpret a letter $l_{i,c} = \theta_N$ as not sending a letter containing arguments about mitigating circumstances to court c . The objective of court c is to base its judgment on all the facts and it needs to decide if mitigating circumstances should be taken into account or not. Formally, the objective of court c in relation to the case of defendant i is to minimize mistakenly taking mitigating circumstances into account: $U_{i,c} = -(m_{i,c} - \theta)^2$ where m_i is interpreted as a decision to take mitigating circumstances into account ($m_{i,c} = \theta_Y$) or not ($m_{i,c} = \theta_N$) for defendant i . The optimal decision is $m_{i,c} = E(\theta|l_{i,c})$ where E is the expectation operator. If the court gets no letter for a defendant i , then its prior is that there are no mitigating circumstances and $m_{i,c} = \theta_N$. All the defendants want mitigating circumstances, if any, to be taken into account by the courts, but also to be acquitted. The objective of defendant i with information θ , therefore, is $U_{i,c} = -(m_{i,c} - \theta - \gamma)^2$, where $\gamma > 0$ captures the desire to be acquitted, i.e., the optimal decision by court c from the point of view of defendant i , $m_{i,c} = \theta + \gamma$, is biased in favor of taking mitigating circumstances into account whether there are, in fact, such circumstances. We model the interaction between a defendant i (or a supporter of defendant i , respectively) and court c as a sequential game of asymmetric information where the defendant, firstly, sends a letter to the court which, secondly, updates its beliefs about whether there are mitigating circumstances based on the content using Bayes rule where possible. The equilibrium concept is perfect Bayesian equilibrium. We assume that the motive to falsely claim mitigating circumstances is so strong that it is not possible, in general, for any defendant to write a letter that credibly claims mitigating circumstances. Formally, we assume

$$\gamma > \frac{\theta_Y - \theta_N}{2} \equiv \bar{\gamma} \quad (7)$$

The difference $\theta_Y - \theta_N$ can be interpreted as a measure of how much the defendants care about a fair trial. Accordingly, assumption (7) says that all defendants care so much about being acquitted relative to a fair trial

that they cannot communicate mitigating circumstances directly to the court. Formally, the assumption rules out informative perfect Bayesian equilibria of the type considered by Crawford and Sobel (1982) in which the defendants write a letter claiming mitigating circumstance only when there are, in fact, such circumstances. The court, therefore, sticks to its prior: there are no mitigating circumstances. In order to influence the court, a defendant needs to use his direct or indirect connections to the courts.

3.3.1 Direct connections to the courts

All defendants can communicate with the courts but a defendant with a *direct* connection to a court has an advantage in doing so. To model direct connections, we assume that communication is associated with a fixed cost.²¹ Connected defendants can communicate at low cost, while unconnected defendants can only do it at high cost. In other words, a direct connection lowers the cost of communication. There can be many reasons for this, ranging from personal or professional ties to shared knowledge about the proper etiquette for communication in the context. Each defendant i got a connection to court c indexed by a fixed cost of using it, $f_{i,c}$. The first proposition shows when and how direct connections can help a defendant get acquitted before court c .

Proposition 1. *There exist two values \bar{f} and \underline{f} with $\bar{f} > \underline{f}$ such that*

1. **Ineffective connected defendants:** Defendants with a cost of communicating $f_{i,c} < \underline{f}$ cannot convince court c and will not submit a letter claiming mitigating circumstances to that court.
2. **Effective connected defendants:** Defendants with a cost of communicating $\underline{f} \leq f_{i,c} \leq \bar{f}$ will submit a letter claiming mitigating circumstances ($l_{i,c} = \theta_Y$) to court c if and only if that is the case and the court will believe the claim.
3. **Unconnected defendants:** Defendants with a cost of communicating $f_{i,c} > \bar{f}$ will not submit a letter claiming mitigating circumstances to court c even though the court would believe such a letter.

Proof. See Appendix A.1 □

For each court, the proposition shows that the defendants are endogenously sorted into two subsets: the set of connected defendants, denoted K_c^d , and the set of unconnected defendants, denoted k_c^d , where the superscript d refers to direct connections. The set K_c^d of connected defendants consists of two types of defendants. While all connected defendants have relatively low cost of communicating with the court ($f_{i,c} \leq \bar{f}$), only some of them are successful at convincing court c that mitigating circumstances are relevant. If they are “too” connected ($f_{i,c} < \underline{f}$), then the court will not trust their letters. The defendants who can influence the court are those with a moderately good connection ($f_{i,c} \in [\underline{f}, \bar{f}]$). The court will believe what they have to say and so this group will submit letters claiming mitigating circumstances. For the set of unconnected defendants the cost of communication is too high ($f_{i,c} > \bar{f}$) and they do not submit letters claiming mitigating circumstances to the court.

The proposition has two empirical implications. First, if the population of defendants contains directly connected defendants as well as unconnected ones, then, on average, the theory predicts that connected

²¹See Grossman and Helpman (2002) for a similar approach to explaining lobbying.

defendants are at least as likely to be acquitted as the unconnected and strictly more likely if they are not all “too” connected. Second, if the population of connected defendants contains a mixture of “ineffective and effective” connected defendants, then the theory predicts that the number of letters submitted by the connected defendants will, on average, be larger than for the unconnected defendants. If, on the other hand, the number of letters from connected and unconnected defendants are the same, then the theory suggests that the cost of sending letters is either the same for the two groups and direct connections are not associated with different communication costs or that direct connections are irrelevant in the sense that it is “too” cheap for the connected defendants to use their connections, and it is not worth sending in letters that will not be trusted anyway.

3.3.2 Indirect connections

A defendant may have an indirect connection to the courts through a third party who himself is connected to them. The third party observes θ and thus knows if there are mitigating circumstances or not. We assume that the third party connected to defendant i has the following objective function

$$U_{i,c}^T = \beta_{i,c}U_{i,c} + (1 - \beta_{i,c})U_i = -\beta_{i,c}(m_{i,c} - \theta)^2 - (1 - \beta_{i,c})(m_{i,c} - \theta - \gamma)^2. \quad (8)$$

The assumption is that third party (T) linked to defendant i partly aligns with the objective that court c ($U_{i,c}$) uses to judge mitigating circumstances and partly with the objective of the defendant (U_i), with the weight $\beta_{i,c}$ determining the relative weight on the two which may vary for defendants across the two courts. The optimal decision of court c in case i from the point of view of the third party is $d_{i,c}^T = \theta + (1 - \beta_{i,c})\gamma$. The third party is, by definition, connected to the courts and can thus send a letter at low cost (for simplicity, we set the cost at zero for both courts, i.e., $f_c^T = 0$ for all c). One interpretation of this is that some third parties intrinsically care about the integrity of the legal process. Another is that the members of the Jury may trust more people they know better – in part because these people would lose more (in terms of reputation) if the evidence they provided proved to be wrong.

Proposition 2. *There exists a $\bar{\beta} \in (0, 1)$ such that a third party with $\beta_{i,c} > \bar{\beta}$ who submits a letter on behalf of defendant i can convince court c that there are mitigating circumstances for defendant i when that is the case. A letter from a third party with $\beta_{i,c} \leq \bar{\beta}$ is not believed by court c .*

Proof. See Appendix A.2 □

The proposition says that third parties are effective at intervening on behalf of a defendant before court c if they at least to some degree share the same objective as the court (if they are connected to the court ($f_c^T = 0$)). This splits the set of defendants before each court into two subsets: those with indirect connections K_c^{id} and those without k_c^{id} , where superscript id refers to indirect connections. A letter from a third party who mostly cares about the defendant will not, in general, influence the court’s decision. The advantage of having a third party intervening on behalf of a defendant is that third parties (to varying degrees) are concerned about the integrity of the legal process and thus have objectives that partly overlap with those of the court. This is what makes their letters of support credible and influential. The empirical predictions that flow from this is that defendants who are supported by connected third parties are more likely to be

acquitted than those who are not, and that these defendants would have more letters of support in their case files from third parties with a connection to the court.

From theory to empirics

To test the predictions and to estimate the causal effect of connections, we exploit the judgment of the two courts for each defendant. This enables us to use a difference-in-differences strategy to isolate the (relative) effect of connections on the probability of acquittal. To explain the logic, let us focus on direct connections. The logic is the same for indirect connections. Our aim is to estimate η in equation (6). Doing so requires that the 'connectedness' of the defendants varies by court so that we can separate the effect of 'connectedness' from other group characteristics that may induce statistical discrimination in the court decisions and from court-specific differences in acquittal rates. To see how this works, suppose that no one has a direct connection to the lower court (L), i.e., $f_{i,L} > \bar{f}$ for all i . Using equation (5), the probability of acquittal is $a + b_L + b_g$ for all $i \in D_g$ and all g . In contrast, assume for the upper court (U) that the cost of communication is such that the defendants, according to proposition 1, are endogenous sorted into the set of connected defendants K_U^d and the set of unconnected defendants k_U^d . Finally, to illustrate how we can eliminate the effect of statistical discrimination against or in favor of connected defendants, assume that there is a fixed characteristic observed by the two courts that correlates perfectly with being connected to the upper court. That is, we divide the defendants into two sets ($N = 2$) based on this feature such that $D_1 = K_U^d$ and $D_2 = k_U^d$. In the empirical application this characteristic is being a Law graduate. The probability of acquittal at the upper court, then, is $a + b_U + b_2$ for $i \in k_U^d = D_2$ (the unconnected) and $a + b_U + b_1 + I_i\eta$ for $i \in K_U^d = D_1$ where I_i is an indicator function equal to 1 if defendant i is, in the sense of proposition 1, effectively connected ($\underline{f} \leq f_{i,U} \leq \bar{f}$) to the upper court and zero if the defendant is ineffectively connected ($f_{i,U} \leq \underline{f}$). Given these assumptions, the differences in the expected acquittal rates for connected and unconnected defendants in the two courts are

$$\Delta_L = E_{i \in K_U^d = D_1} Pr(L, 1) - E_{i \in k_U^d = D_2} Pr(L, 2) = b_1 - b_2 \quad (9)$$

$$\Delta_U = E_{i \in K_U^d = D_1} Pr(U, 1) - E_{i \in k_U^d = D_2} Pr(U, 2) = b_1 - b_2 + E_{i \in K_U^d = D_1} I_i\eta \quad (10)$$

The difference-in-differences estimate, then, is $\Delta = \Delta_U - \Delta_L = E_{i \in K_U^d = D_1} I_i\eta$. We notice, firstly, that if the exact same defendants were connected to both courts, then we would not be able to identify η . Identification requires differential connections to the two courts. In practice, we can only observe connections to the upper court (the *Jury*), yet the estimate of Δ should be interpreted as the relative effect of connections to the two courts. Secondly, if the defendants are "too" connected in the sense of proposition 1, then $\Delta = 0$, i.e., being connected has no effect on the probability of acquittal: the connections are ineffective. By netting out both court characteristics and statistical discrimination, the difference-in-differences estimator allows us to identify connections specific to a court-defendant pair.

As we do not directly observe who is connected and who is not, we test the theory in two steps. First, in the next section, we establish that a particular group of defendants – Law graduates – who were differentially connected to the *Jury*, experienced a higher acquittal rate before the *Jury* than before a CDLs. In the following section 5, we use the detailed information derived from archival research on the content of the

defendants' dossiers to relate the advantage of Law graduates to direct and indirect connections between Law graduates and the *Jury*.

4 The Law graduate advantage before the *Jury*

This section introduces the data, translates the theoretical results from Section 3 into our empirical strategy, and reports estimates of the advantage of Law graduate before the *Jury*.

4.1 Political purges in post-war France: The data set

We combine two different types of observational data. The first type of data is biographical information on the parliamentarians who voted on the enabling Act in 1940. It includes data on age, profession, education, and other individual characteristics collected by Lacroix et al. (2019) and by Olivier Wieviorka in preparation for his book on the destiny of French parliamentarians during World War II (Wieviorka, 2001).²² The second type of data relates to the purge process itself. First, from Wieviorka (2001) and consolidated with information from the still-classified individual dossiers of the defendants from the archives of the *Jury*²³, we record for each of the parliamentarians who was tried, the decision reached by the CDLs and the *Jury*.

The combined data set comprises 798 decisions on the cases of 399 individual defendants along with the personal characteristics of the defendants we observe.

4.2 A first look at the data

To better understand the structure of our data, Table 1 cross tabulates the decisions of the *Jury* and the CDLs. The *Jury* and the CDLs agreed in 81.0% of the 399 cases in the data set. Our analysis leverages the cases where the CDLs and the *Jury* did not agree (the bold numbers in the Table). These are mostly cases for which the *Jury* acquitted defendants against the judgment of CDLs (17.0% of the cases). In the remaining 2.0% of cases, the *Jury* banned defendants that the CDLs wanted to acquit.

Table 1: Matrix - Distribution of the two-courts' decisions

	CDL for Acquittal	CDL not for Acquittal	Total
<i>Jury</i> for Acquittal	32	68	100
<i>Jury</i> not for Acquittal	8	291	299
Total	40	359	399

Armed with this dataset, Subsection 4.2 presents first evidence in line with our theoretical model whereas Section 4.3 presents a more thorough empirical strategy leveraging the panel structure of the data and Section 4.4 reveals the results of this approach.

²²We sincerely thank Olivier Wieviorka for sharing his data with us.

²³References AL//5295 to AL//5334.

Graphical representation - Acquittal rates

Figure 1(a) reports mean comparisons of the acquittal rate of Law graduates and of other defendants before the two courts, respectively. Before CDLs, the acquittal rate was 8,2% for Law graduates and 10,7% for other defendants. This difference is not statistically different, suggesting that the CDLs did not treat Law graduates differently from other defendants. Before the *Jury*, on the other hand, the acquittal rate of Law graduates was 30,9% compared to 22,8% for other defendants. This difference is statistically significant, suggesting that the *Jury* in contrast to the CDLs did treat Law graduates more leniently than other defendants. Following equation (9) in the theoretical section, the “difference-in-differences” estimate of the Law graduate advantage before the *Jury* is $(30,9-22,8)-(8,2-10,7)=10,6$ percentage points.

Figure 1(b) reports separately for Law graduates and other defendants the percentage of cases for which the decisions of CDLs and the *Jury* differed. The left-hand side panel displays the percentage of cases for which a CDL wanted to acquit a defendant but the *Jury* decided to ban him for Law graduates and other defendants. The percentages are similar: 1.8% for Law graduates and 2.1.% for other defendants. The right-hand side panel shows the percentages of decisions for which the CDLs’ decision to ban a defendant was overturned by the *Jury* for the two groups of defendants. The difference is striking: the *Jury* ended up acquitting a defendant a CDL wanted to ban in 24.5% of the cases for Law graduates but only in 14.2% of the cases for other defendants. Law graduates tended to have their ban overruled by the *Jury*.

That *relative* advantage of Law graduates before the *Jury* could reflect a disadvantage of Law graduates before CDLs instead of an advantage before the *Jury*. However, two additional findings suggest that the relative can, in fact, be interpreted as an absolute one. First, from Figure 1(a), we see that the positive difference-in-differences estimate results from a differential treatment of Law graduates before the *Jury* and not before CDLs. Second, from Figure 1(b), we see that the Law graduate advantage is due to the *Jury* overturning bans upheld by CDLs more frequently for Law graduates than for other defendants rather than to the *Jury* overturning acquittal decisions by the CDLs more frequently for non-Law graduates than for graduates. Law graduates therefore benefitted from a specific leniency of the *Jury*.

Counterfactual analysis - Standard of evidence and acquittal

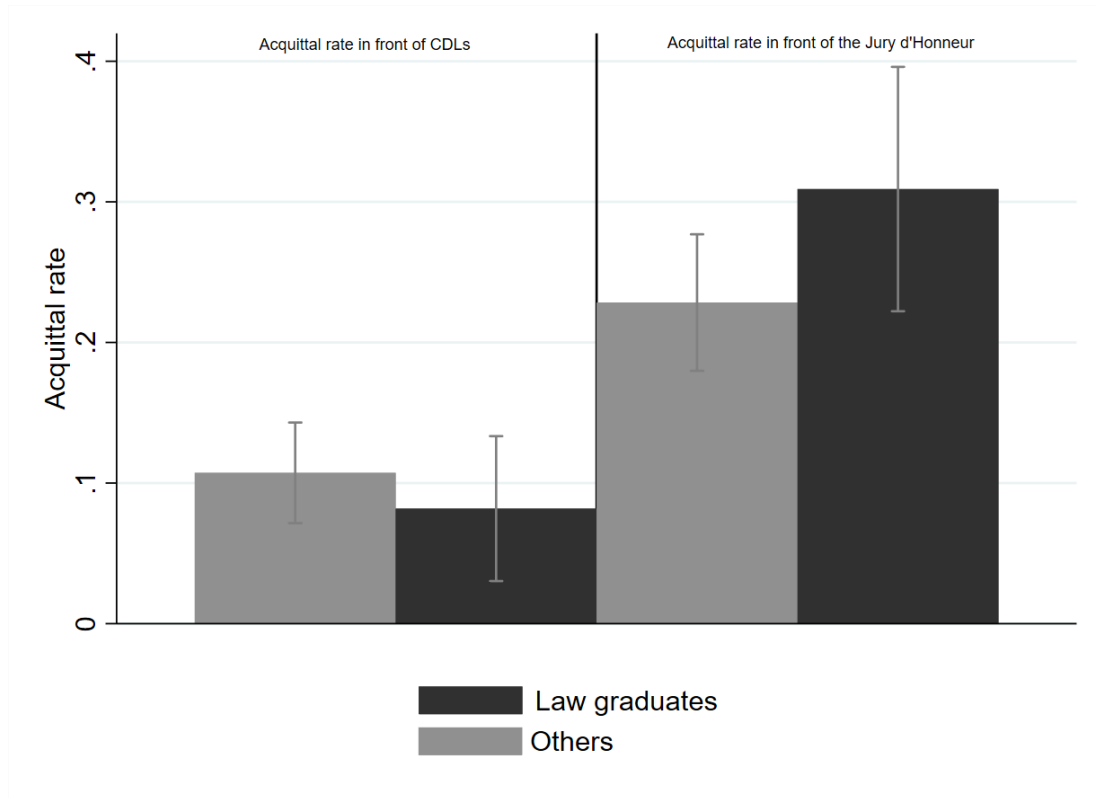
We further explore our data focusing on the criteria officially used by the *Jury* to decide whether to acquit a defendant or not. Appendix B.1 summarizes how we proceeded to construct a counterfactual acquittal rate at the individual-level. Echoing the theoretical model, we infer that both evidence (x) and group-specific connections (I_i) influence acquittal rates. We estimated a counterfactual probability of being acquitted based on the information in dossiers (x): participation in civilian resistance, participation in military resistance and judgement by the local courts. Should the functional relation between those criteria and the probability of acquittal be similar for Law graduates and for other defendants, their acquittal rates would be similarly approximated using the counterfactual we have constructed. Any deviation of a group from this counterfactual could result from differences in connections (I_i). Figure 2 summarizes our results.²⁴

The left panel of Figure 2 presents the counterfactual (in grey) and the actual acquittal rate (in black) for non-Law graduate defendants. They are not statistically different from each-other. Based on our counterfactual

²⁴Appendix B.1 provides the counterfactual and actual acquittal rates for both groups.

Figure 1: Courts' decisions - Law graduates vs Others Defendants

(a) Acquittal rates before the CDLs and the *Jury*
(Confidence intervals: 95%)



(b) Percentage of CDL decisions overruled by the *Jury*
(Confidence intervals: 95%)

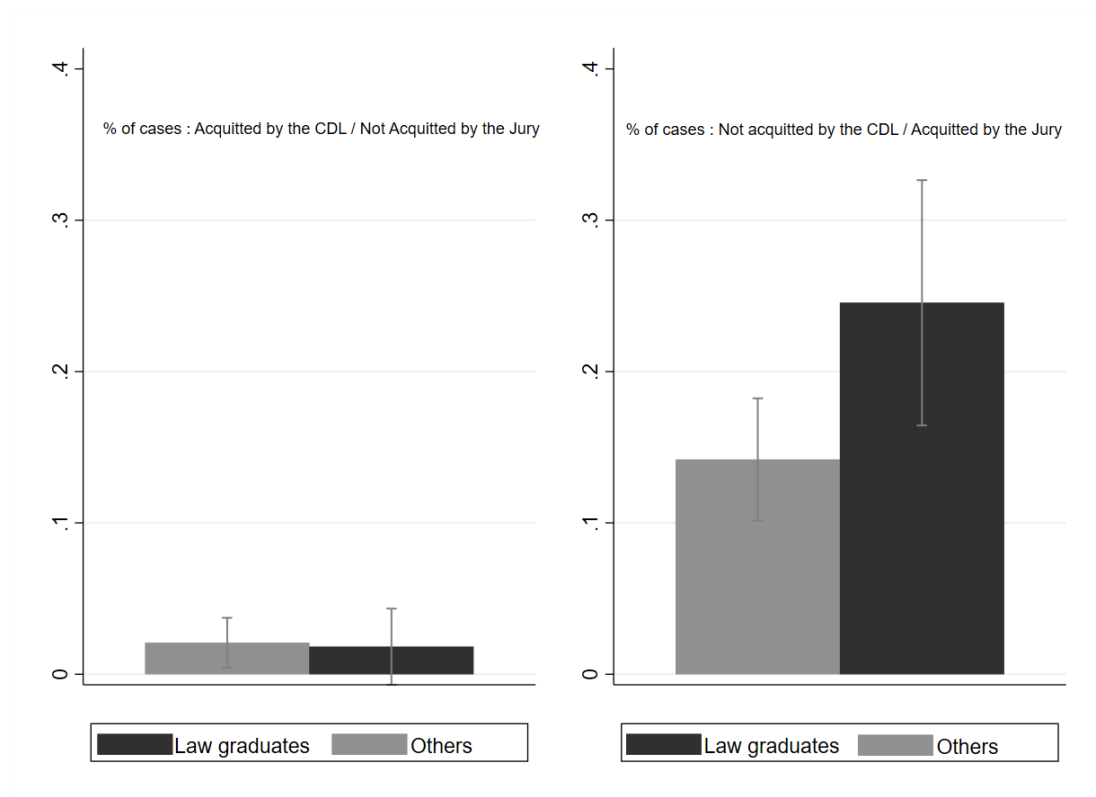
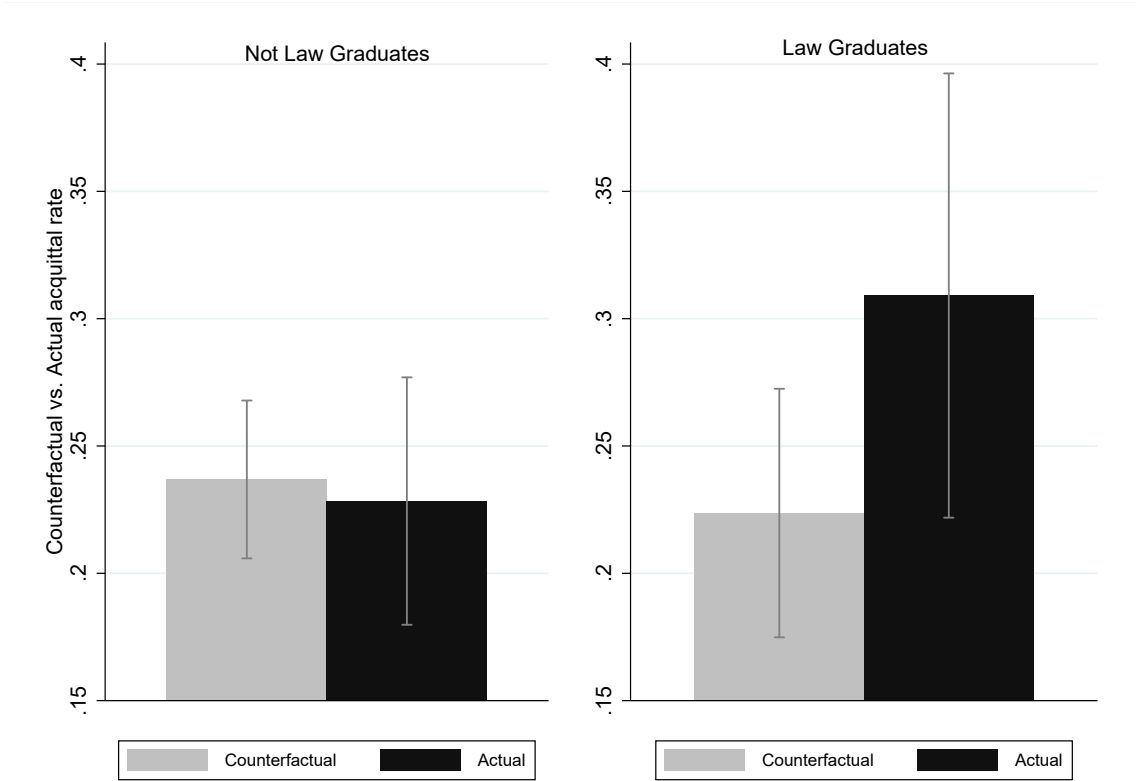


Figure 2: Counterfactual vs Actual Acquittal rates - Law graduates vs. other defendants



estimation, non-Law graduates had a 23.7% probability of being acquitted by the Jury when this was in fact 22.8%. Hence our counterfactual performs well on the set of decisions of the *Jury* concerning non-Law graduates, lending credibility to the specification and suggesting that participation in the resistance was the key information used by the *Jury* to make its decisions.

The right panel of Figure 2 displays the counterfactual and actual acquittal rates for Law graduates. The counterfactual probability to being acquitted is the same for Law graduates and for other defendants (22.4% vs. 23.7%). Based on their participation in the resistance and on the initial decision of CDLs, the probability of Law graduates to be acquitted should have therefore been the same as that of other defendants. Yet, the actual acquittal rate of Law graduates equalled 30.9%, which is 8.5 percentage points higher than the counterfactual. Accordingly, they benefitted from a greater clemency from the *Jury*.

4.3 Empirical strategy

Our objective in this section is to determine whether being a Law graduate affected the chances of being purged by the *Jury* relative to the CDLs in line with the model of Section 3. We go beyond estimates presented in section 4.2 to explicitly account for both statistical-discrimination and the possible advantage provided by other characteristics in front of the *Jury*. We leverage the fact that each defendant was tried twice – first, by a CDL and, then, by the *Jury* – and that he was either a Law graduate or not to develop a difference-in-differences identification strategy. Let $Acquit_{i,c}$ be a dummy variable equal to one if defendant i is acquitted by court $c \in \{L, U\}$, where $c = L$ indexes CDLs and $c = U$ indexes the *Jury*. We set the dummy variable LG_i to one if defendant i is a Law graduate and zero otherwise, and the dummy variable $Jury_c$ to one if the judgment was reached by the *Jury* and zero otherwise (i.e., the judgment was reached by a CDL). We estimate the following linear probability model:

$$Acquit_{i,c} = \alpha + \beta_1 Jury_c \times LG_i + \beta_2 Jury_c + \beta_3 LG_i + \beta_4 Jury_c \times X_i + \beta_5 X_i + \varepsilon_{i,c}, \quad (11)$$

where α , β_1 , β_2 and β_3 are coefficients, and $\varepsilon_{i,c}$ is the error term. Equation (11) is estimated with ordinary least squares.²⁵ Standard errors are clustered at the defendant level.

Equation (11) is similar to the difference-in-differences estimation strategy that Anwar and Fang (2006) and Alesina and La Ferrara (2014) use to document a racial bias in judicial decisions in the USA. In our case, the treatment group consists of defendants who are Law graduates and the pre- and post-treatments are being judged by the CDLs and the *Jury*, respectively. To understand how we can use this strategy to causally identify the difference in the acquittal rate of Law graduates relative to other defendants in the judgments of the *Jury* relative to the CDLs, it is useful to consider each term in detail. Coefficient β_2 on the $Jury_c$ dummy variable captures the difference in acquittal rate across all defendants between the *Jury* and the CDLs. It captures all (unobserved) characteristics of the *Jury*, such as the weight it puts on type 1 relative to type 2 errors, that may result in it being on average more or less lenient than CDLs and corresponds to b_c in Equation (5). Coefficient β_3 on the LG_i dummy variable captures the difference between the acquittal rate of Law graduates and that of other defendants regardless of which court rules. This corresponds to b_g in Equation (5) for the group of Law graduates.

²⁵Our results are similar when using a Probit or a Logit estimation (Appendix B.4). We chose a linear probability model as the baseline model since this makes the interpretation of the interaction effect straightforward.

Coefficient β_1 on the interaction between $LG_i \times Jury_c$ is the coefficient of interest. It isolates the relative Law graduate advantage before the *Jury* by capturing the difference in the acquittal rate of the *Jury* from that of the CDLs specifically for Law graduates relative to other defendants after controlling for the effect of fixed defendant characteristics and unobserved court-specific characteristics. In other words, β_1 measures how much more inclined than CDLs the *Jury* was to acquit Law graduates. In the literature on sentence bias in court decisions, this effect is referred to as “taste-based discrimination” (e.g., Alesina and La Ferrara, 2014). We prefer to use the term “Law graduate advantage” and avoid the term discrimination.

Our theory suggests that the courts may, in different ways, base their bar for acquittal on observable group characteristics. Some of these may overlap with being a Law graduate. In Equation (11), we, therefore, control for a vector of individual (group) characteristics (X_i) and their interactions with the *Jury* dummy variable ($Jury_c \times X_i$) to ensure that the estimate of β_1 does not capture any other group characteristics correlating with the Law graduate dummy variable and higher acquittal rates before the *Jury*. To select these control variables, we investigate a large number of potential controls and retain those that are statistically significantly correlated with the difference in acquittal rates between the *Jury* and the CDLs.²⁶

We also include in X_i information on participation in the resistance or of collaboration with the Vichy regime from the dossiers of the defendants and coded by Wieviorka (2001). This includes dummy variables equal to one if there was proof of participation in the civilian or military resistance, if the defendant had been arrested by the Vichy regime, or if he had been a mayor under that regime. Controlling for these factors and their interactions with the *Jury* dummy variable ensures that our estimate of the Law graduate advantage is not driven by the fact that the *Jury* had better access to evidence of participation in the resistance than the CDLs or that Law graduates due to their legal training were better at conveying information on their participation in the resistance to the *Jury*.

Unlike other studies of bias in court decisions, we face no selection into encounters problems (Knox et al., 2018) because the exact same population of defendants automatically faced the two courts within a short period of time. Suspicion and selection into a second court hearing (here by the *Jury*) could therefore not correlate with defendants’ characteristics. Likewise no selection into the purging process could occur because all parliamentarians who had voted in favor of the 1940 enabling act and/or had collaborated with the Vichy institutions faced the purging process.²⁷

4.4 Baseline results: The Law graduate advantage before the *Jury*

Table 2 reports the estimates of Equation (11). Column 2.1 shows a parsimonious specification which controls for the Law graduate and *Jury* dummy variables only. The difference-in-differences estimate of β_1 is significant

²⁶The results are reported in Table B.2. The following defendants (group) characteristics were treated differently by the two courts and included as controls: age, being Jewish or not, being a journalist, being a President/Vice President or Secretary of the National Assembly, and a dummy equal to one if the parliamentarian’s constituency was in the part of France that was initially occupied by Germany. Conversely, the difference in acquittal rates before the CDLs and before the *Jury* is not explained by longer studies, longer political career, or political orientation (see the bottom of Table B.2).

²⁷There is some attrition in our data, but it is random and should not affect our results. Out of the 569 parliamentarians who voted in favor of the 1940 enabling act, 93 died during the war, 51 had been acquitted by prefects prior to the legal process because they were well-known figures of the resistance, 8 were facing criminal courts for collaboration with the Vichy regime, 8 were from overseas and we, therefore, do not have CDL judgments in their dossiers. Finally, in the dossiers of 16 defendants the decision of either the CDL or the *Jury* is missing. Those cases were, typically, cases on which the *Jury* and CDLs would have agreed given the level of evidence of participation in the resistance or they were lost randomly. Among those missing observations, the proportion of Law graduates is not statistically different from what it is for the defendants who are not missing (32% vs. 28%).

at the five-percent level and the point estimate implies that the difference in acquittal rates between Law graduates and other defendants is 10,6 percentage points higher before the *Jury* than before the CDLs. This is in line with the evidence from Figure 1(a) and of our counterfactual analysis. The estimate of β_1 remains statistically significant and its magnitude is stable across the specifications reported in the other columns, where we add controls for the characteristics of the defendants and the interaction between these characteristics and the *Jury* dummy variable. Controlling for all the characteristics increases the explanatory power of the model from an adjusted R^2 of 0.040 in Column 2.1 without any controls to 0.281 with all the controls in Column 2.6, but the difference-in-differences estimate remains between 9.6 and 11.0 percentage points. In particular, the estimate is hardly affected when we control for the presence of evidence of either collaboration with the Vichy regime or of participation in the resistance movement in the dossiers of the defendants (Column 2.5). Additionally, the specification in Column 2.7 controls for defendant fixed effects, which neither affect the magnitude nor the significance of the estimate of β_1 .

Table 2: The advantage of Law graduates before the *Jury*: Baseline estimates

Dep variable	(2.1)	(2.2)	(2.3)	(2.4)	(2.5)	(2.6)	(2.7)
Estimator	$Acquit_{i,c}$	$Acquit_{i,c}$	$Acquit_{i,c}$	$Acquit_{i,c}$	$Acquit_{i,c}$	$Acquit_{i,c}$	$Acquit_{i,c}$
	OLS	OLS	OLS	OLS	OLS	OLS	OLS
Jury	0.121*** (5.336)	0.419*** (3.283)	0.106*** (4.503)	0.218*** (5.000)	-0.0224 (-0.887)	0.143 (1.278)	0.143 (1.287)
LG	-0.0254 (-0.797)	-0.0264 (-0.839)	-0.0276 (-0.856)	-0.0273 (-0.831)	-0.0249 (-0.813)	-0.0284 (-0.915)	
Jury X LG	0.106** (2.143)	0.0978** (1.985)	0.110** (2.217)	0.0958* (1.890)	0.108** (2.385)	0.0996** (2.149)	0.0996** (2.165)
Constant	0.107*** (5.874)	0.0794 (0.907)	0.116*** (5.773)	0.101*** (3.405)	0.0498** (2.197)	-0.0797 (-0.829)	0.100*** (10.87)
Controls:							
Age and Religion		Yes				Yes	Yes
Journalist			Yes			Yes	Yes
Political mandates				Yes		Yes	Yes
Resistance and collaboration WWII					Yes	Yes	Yes
Individual FE							Yes
Observations	798	798	798	798	798	798	798
Adjusted R-squared	0.040	0.052	0.041	0.049	0.271	0.281	0.289

Robust t-statistics in parentheses: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Jury is a dummy variable equal to one if the judgment was before the Jury and zero if the judgment was before a CDL. LG is a dummy variable equal to one if the defendant was a Law graduate and zero otherwise. This Table presents estimates of equation (1) when focusing on law graduates and adding variables explaining a difference in sentencing patterns between the Jury and CDL. Individual controls include: Age and religion (Age, Jewishness); Journalist; Political mandates (Mayor, Special Role in the Assembly, parliamentarian of an occupied territory); Resistance and collaboration WWII (Civilian Resistance, Military resistance, Arrested by the Vichy regime, Mayor under Etat Francais). Each individual control is also interacted with the Jury dummy variable.

Table 2 therefore shows that Law graduates were about 10 percentage points more likely to be acquitted by the *Jury* compared to by the CDLs than other defendants. We interpret this effect as the Law graduate advantage before the *Jury*. To better substantiate, how much of this effect appears before each level of court, Appendix B.3 estimates how better Law graduates fare before both levels of courts. These estimates go exactly in the same direction as Figure 1(a). They show that most of the advantage of Law graduates comes from differences in sentencing before the *Jury* and not before CDLs: Law graduates had a 7.1 percentage points higher probability than other defendants to be acquitted by the *Jury* against a 2.8 percentage points lower probability to be cleared by a CDL. Accordingly, we find that around 71 percent of the relative advantage of Law graduates appears before the *Jury*. Appendix B.4, additionally, suggests that this effect is mostly driven by defendants whom CDLs were not in favor of acquitting but the *Jury* was. These results remain suggestive evidence but also point in the same direction as both anecdotal evidence and the theoretical model of Section

3. In the appendix, we show that baseline results are robust to alternative estimations methods (Appendix B.4), to controlling for the composition of resistance groups in a département, which we interpret as a proxy of the political composition of CDLs (Appendix B.5), and did not emerge because CDLs disregarded evidence put forward by Law graduates (Appendix B.6).

All the estimates in this section suggest that Law graduates benefited from a 8.5 to 11 percentage points bonus before the *Jury* even after taking evidence of participation in the resistance and decisions of lower courts into account. Accordingly between 9 and 12 parliamentarians might have directly benefited from the “Law graduate advantage” in front the *Jury*. The magnitude of the effect is not un conspicuous as it equals the size of the smallest parliamentary groups in the French Parliament in 1946.

4.5 From an advantage during purges to later elite persistence

For the behavior of the *Jury* to have contributed to elite persistence, it must have impacted the career of politicians. It would have been un consequential if it acquitted Law graduates had not intended to run for an election or if voters had not elected them. Here, we show that the decisions of the *Jury* was consequential for the careers of defendants and that Law graduates were more likely than other defendants to pursue a career in politics after facing the *Jury*.

Table 3 investigates how much the decisions of the *Jury* and the advantage of Law graduates influenced the careers of politicians after WWII. It reports a series of regressions where candidacies in municipal and legislative elections and mandates as mayors, as member of parliament and responsibilities as minister are explained by the decisions of the two courts or by being a law graduate. In each regression, we control for the decision to acquit the defendant by CDLs to capture the deviation from CDLs’ decisions as in our baseline estimation strategy. Panel A considers the impact of an acquittal by the *Jury* on post-World War II political career. All the coefficients attached to the acquittal variable are significant at the one or the five-percent level and positive. Defendants acquitted by the *Jury* filed more candidacies to municipal and legislative elections after World War II (Column 3.A.1 and 3.A.2). As a result, they were 12 percentage points more likely to be a mayor (Column 3.3), 23 percentage points more likely to seat in the Parliament (Column 3.A.4), and 7 percentage points more likely to become Ministers (Column 3.A.5).

Table 3: Elite persistence - Jury decision and Law graduates

Panel A - Independent variable of interest: Acquitted by the Jury					
	(3.A.1) Nb Candidacies Mayor	(3.A.2) Nb Candidacies Deputy	(3.A.3) Mayor=1	(3.A.4) Parliament=1	(3.A.5) Minister=1
Acquitted _{Jury}	0.189** (2.550)	0.253*** (3.458)	0.116** (2.210)	0.225*** (3.787)	0.0701** (2.293)
Acquitted _{CDL}	0.0431 (0.469)	0.0735 (0.739)	0.0115 (0.180)	0.0553 (0.780)	0.00894 (0.228)
Constant	0.595*** (4.590)	0.630*** (4.594)	0.379*** (3.842)	0.349*** (3.609)	0.0720* (1.681)
Observations	397	399	399	399	399
Adj R^2	0.172	0.156	0.166	0.157	0.067
<u>Control variables</u>					
Individual	YES	YES	YES	YES	YES

Panel B - Independent variable of interest: Law graduates					
	(3.B.1) Nb Candidacies Mayor	(3.B.2) Nb Candidacies Deputy	(3.B.3) Mayor=1	(3.B.4) Parliament=1	(3.B.5) Minister=1
LG	-0.0352 (-0.836)	0.105** (2.210)	-0.0107 (-0.348)	0.0761** (2.103)	0.0318* (1.691)
Acquitted _{CDL}	0.121 (1.303)	0.188* (1.953)	0.0597 (0.926)	0.156** (2.162)	0.0408 (1.072)
Constant	0.630*** (4.789)	0.626*** (4.554)	0.397*** (4.023)	0.351*** (3.654)	0.0699 (1.513)
Observations	397	399	399	399	399
Adj R^2	0.153	0.125	0.151	0.110	0.044
<u>Control variables</u>					
Individual	YES	YES	YES	YES	YES

Robust t-statistics in parentheses: *** p<0.01, ** p<0.05, * p<0.1. Level of observation: Defendant. In Panel A, the main independent variable is a dummy variable equal to 1 if the defendant has been acquitted by the Jury. In Panel B, the main independent variable is a dummy variable equal to one if the defendant was a law graduate. Dependent variables: Nb Candidacies Mayor is the number of times a defendant has been a candidate in a municipal election after the War. Nb Candidacies Deputy is the number of times a defendant has been a candidate in a legislative election after the WWII. Mayor=1 is a dummy variable equal to one if a defendant has been a mayor after WWII and zero otherwise. Parliament=1 is a dummy variable equal to one if a defendant has been a deputy or a senator after WWII and zero otherwise. Minister=1 is a dummy variable equal to one if the defendant has been a minister after WWII and zero otherwise. Individual controls include: Age and religion (Age, Jewishness); Journalist; Political mandates (Mayor, Special Role in the Assembly, parliamentarian of an occupied territory); Resistance and collaboration WWII (Civilian Resistance, Military resistance, Arrested by the Vichy regime, Mayor under Etat Francais).

Panel B assesses the political advantage of Law graduates after the war. The dependent variables are the same as in Panel A but the explanatory variable is the Law graduate dummy. In these estimations, we observe no effect of being a Law graduate at the municipal level, which echoes the finding that they enjoyed no particular advantage before local courts. The benefits from acquittal before the *Jury* could have been compensated by reputational costs at the local level limiting electoral prospects in municipal elections. By contrast, two coefficients attached to variables capturing a career at the national-level are positive and statistically significant at the five-percent level. Law graduates filed around 11 percent more candidacies for legislative elections than other defendants (Column 3.B.2). As a result, they were around 8 percentage points more likely than other defendants to stay in Parliament after World War II (Column 3.B.4). One coefficient

is significant at the ten-percent level suggesting that Law graduates were 3 percentage points more likely than other defendants to become Minister after World War II (Column 3.B.5).²⁸

The decision of the *Jury* to clear politicians was consequential for post-World War II politics in France. We observe that acquitted politicians did enjoy the possibility to pursue their career (Panel A). Moreover, Law graduates were more likely than other defendants to stay in the Parliament after World War II (Panel B). We infer that it is a consequence of their advantage before the *Jury*. Interestingly Law graduates did not persist at the local level in line with our results on their advantage in purges. In Appendix B.7, we furthermore show that the advantage of Law graduates appears when they sought to run for reelection in 1945. The activation of the advantage of Law graduates, hence, relates to their ambitions to stay in politics after the war.

All in all, the baseline results show that Law graduates had an advantage before the *Jury*: the court having the final say in the political purge of post-World War II France. Additional results confirm this intuition : Law graduates more likely persisted in the Parliament after World War II. In other words, an advantage in democratic purges distorted elite persistence – increasing the probability of some elites to persist in comparison to other elites. Now that we have established the existence of a substantial Law Graduate advantage before the *Jury*, we need to investigate the origin of that advantage. Section 5, more specifically, uses the content of the defendants’ dossiers to relate the advantage of Law graduates to connections.

5 What explains the Law graduate advantage?

In this section, we leverage the content of the dossiers of individual defendants kept in the archives of the *Jury* to investigate the role of connections in the advantage of Law graduates before the *Jury*. We begin by presenting the data set built from individual dossiers and then test if direct and indirect connections between the defendants or their supporters and the *Jury* can explain the Law graduate advantage. We also rule out that the advantage is caused by legal skills.

5.1 The dossier data set

We created a full inventory of the documents contained in the individual dossiers of the defendants facing the *Jury*.²⁹ The *Jury* kept a detailed record of each case, including all internal and external correspondence, and the dossiers contain all pieces of information the *Jury* used to reach its ruling. Overall, the inventory pertains to 17,589 documents. The inventory makes it possible to extract and quantify three types of information. First, we can quantify the volume and length of different types of documents in each of the dossiers and classify them according to their content. Second, we can record communication between defendants and the *Jury*: direct connections. Third, we can record letters from individuals external to the *Jury* who tried to intervene in favor or against a defendant by writing to the *Jury* or to third parties connected to the *Jury*: indirect connections. We refer to these as “letters of support”.³⁰ From the inventory data set, we code

²⁸Results are very similar using the number of years a politician spent in the different positions mentioned above (see Appendix B.8)

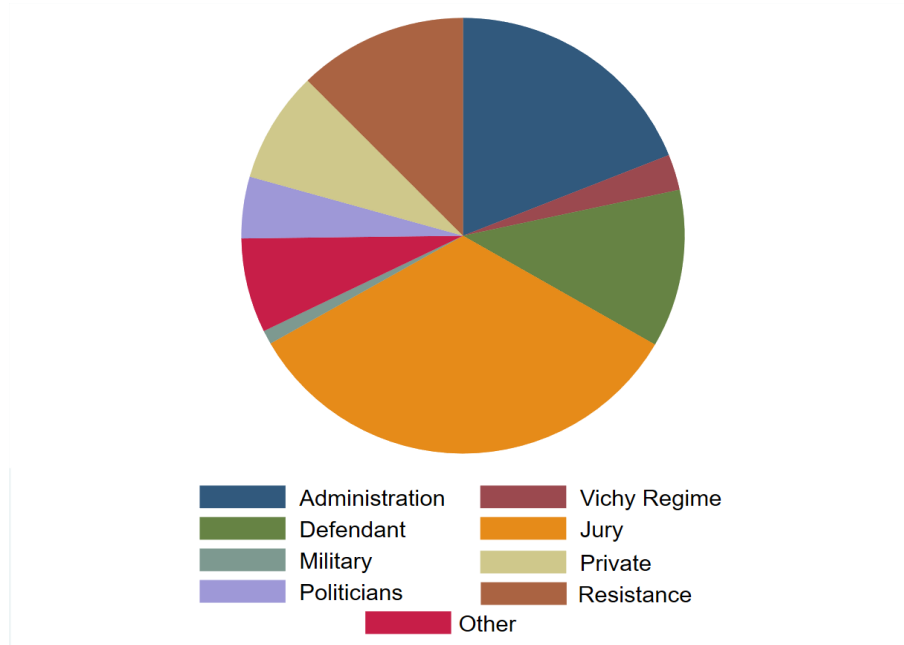
²⁹The dossiers are to be found in the French National Archives (References AL//5295 to AL//5334).

³⁰We note that in a few cases, opponents submitted letters in support of purging the defendant, but the vast majority of the letters were, in fact, letters advocating that the defendant be acquitted.

variables measuring for each defendant such aspects as the structure of the dossiers, the origin of letters of support, and the type and length of the documents in their dossiers as well as information on the connections between each defendant and the *Jury*. Appendix Table C.1 defines and presents summary statistics on the variables related to the content of the dossiers used in the analysis whereas Appendix Table D.2 reports summary statistics on the overall structure of the dossiers.

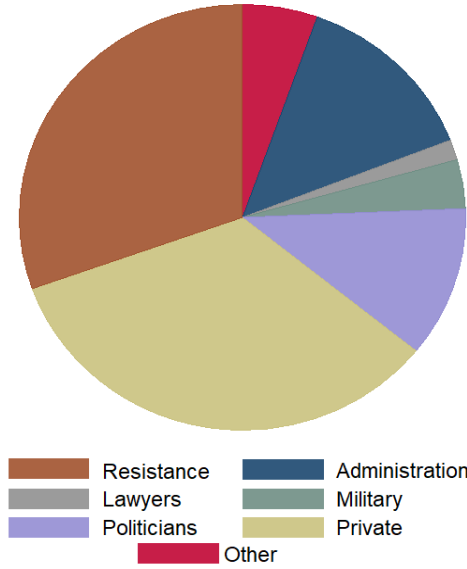
Figures 3 and 4 present information on the origin of the documents in the dossiers. In Figure 3, the entire set of documents is broken down by type of author. We see that most of the documents originated from the *Jury* itself (33.4%), while 19.0% of them came from within the public administration, and 12.4% were submitted by resistance organizations. Letters from the defendants themselves account for 11.7% of the documents. The dossiers contain 3,385 “letters of support”. These letters written by supporters (and in a few cases by opponents) of a defendant represent 19.2% of the documents in a typical dossier. Figure 4 shows the origin of those letters of support. A third of them were sent by the defendant’s constituents, friends, or family (“private”) and 30.5% originated from resistance organizations.

Figure 3: Summary - The origins of the all documents in the dossiers of defendants



Categories are defined as follows. Administration refers to document produced by a ministry or a local administration. Vichy Regime refers to archives of the Vichy regime or of any related collaboration institution. Defendant refers to document produced by the defendant. Jury refers to document produced by the Jury. Military refers to document produced by the French army. Private refers to document produced by an individual in his/her own name without stating an obvious relation to an organization, typically coming from family members or friends of the defendant or from individuals in his constituency. Politicians refers to documents produced by parliamentarians and local politicians. Resistance refers to documents produced by members of resistance networks. Lawyers are document sent by a lawyer using his/her title in the document sent.

Figure 4: Summary - The origins of letters of support in the dossiers of the defendants



Categories are defined as follows. Administration refers to document produced by a ministry or a local administration. Vichy Regime refers to archives of the Vichy regime or of any related collaboration institution. Defendant refers to document produced by the defendant. Jury refers to document produced by the Jury. Military refers to document produced by the French army. Private refers to document produced by an individual in his/her own name without stating an obvious relation to an organization. Politicians refers to documents produced by parliamentarians and local politicians. Resistance refers to documents produced by members of resistance networks. Lawyers are document sent by a lawyer using his/her title in the document sent.

Table 4 compares the dossiers of Law graduates with those of other defendants. The top panel reports mean comparisons for variables related to the structure of the dossiers; the middle panel reports comparisons for variables related to letters of support; and the bottom panel reports comparisons for variables related to the information content of the dossiers. We see that dossiers of Law graduates and other defendants are strikingly similar and in all, but one case, the means are statistically indistinguishable.³¹ In short, the dossiers of Law graduates are not “thicker” than other dossiers and do not contain more letters of support or letters with information about participation in the resistance than those of other defendants.

³¹ The exception is the number of documents referring to the “Resistant Press”, which are less common in the dossiers of Law graduates. This is probably due to the fact that Law graduates are rarely journalists. In any case, that difference cannot explain the Law graduate advantage because we control for that difference in the baseline estimations reported in Table 2.

Table 4: Law graduates and the content of dossiers

	(4.1)	(4.2)	(4.3)
	Mean		
	LG	Others	Diff=0 (p-value)
Structure of the dossiers			
Nb Doc	40.96	40.59	0.90
Nb Pages	59.30	53.97	0.29
Nb Doc from Jury	13.86	13.63	0.74
Nb Archival Docs	3.22	2.63	0.39
Nb Information requests	1.16	1.04	0.27
Nb Letters of support	7.16	8.06	0.52
Nb Letters of support - in Favor	6.78	7.65	0.52
Nb Letters of support - Against	0.22	0.22	0.98
Nb Letters of support - Neutral	0.16	0.19	0.74
Nb Letters of support - Resistance	2.09	2.54	0.35
Nb Letters of support - Military	0.24	0.28	0.72
Nb Letters of support - Administration	1.22	0.94	0.39
Nb Letters of support - Others	2.08	2.88	0.26
Nb Doc - Military resistance	0.82	0.75	0.85
Nb Doc - Civilian resistance	8.36	8.63	0.83
Nb Doc - Resistant Press	0.29	0.70	0.04**
Nb Doc - Legal Arguments	5.59	5.37	0.62
Nb Doc - Political opinion	10.39	9.87	0.75
Nb Doc - Reelection	0.95	1.11	0.53
Nb Doc - Other topic	6.45	6.26	0.84

"Others" is all defendants not being a Law graduate. The top panel presents statistics on the overall number of documents in the dossiers and the number of documents of various types. The middle panel labelled "Letters of support" presents statistics on letters of support broken down in subcategories. The first subcategory relates to the opinion expressed in these letters (in favor, neutral, against). The second subcategory relates to the affiliation of the sender (Resistance, Military, Administration). The bottom panel presents information on the topic covered by each document. For example, the first line of this panel headed Nb Doc - Military resistance should be read as: the average number of documents providing information on actions related to participation in military resistance. Results are similar when those measures are transformed using $\log(k+1)$ - Available upon request. Columns 4.1 and 4.2 display the mean value for the group of Law graduates (LG) and other defendants (others), respectively. Column 4.3 reports the p-value of a two-sided t-test of equal means with *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

5.2 Method for testing if connections explain the Law graduate advantage

To test if connections can explain the advantage of Law graduates, we augment equation (11) and estimate the following equation:

$$\begin{aligned}
Acquit_{i,c} = & \alpha + \beta_1 Jury_c \times LG_i + \beta_2 Jury_c + \beta_3 LG_i \\
& + \Gamma_1 (Court_c + Defendant_i) \times C_i + \beta_4 Jury_c \times NbDoc_i + \beta_5 NbDoc_i + \varepsilon_{i,c},
\end{aligned} \tag{12}$$

where $Acquit_{i,c}$ is the dummy variable equal to 1 if defendant i is acquitted by court c and the rest of the first line corresponds to the baseline specification in equation (11). The terms related to connections are shown

in the second line along with the error term $\varepsilon_{i,c}$. The matrix $(Court_c + Defendant_i)$ includes four dummy variables defined for pairs of defendants (Law graduate or another defendant) and court (*Jury* or CDL). These dummy variables are interacted with C_i which is a measure of either direct or indirect connections for a given defendant i . Γ_1 is the vector of coefficients measuring the effect of connections for different subcategories of decisions defined by the court/defendant-type pairs. $NbDoc_i$ is the number of documents in the dossier of defendant i . It is included directly and interacted with the Law graduate dummy variable to control for the possibility that the content of the dossiers can influence the difference in acquittal rates between CDLs and the *Jury*.

Equation (12) allows us to test if connections can explain the Law graduate advantage before the *Jury* in two ways. First, if a measure of connections explains the Law graduate advantage, then the coefficient associated with the interaction $Jury_c + LG_i$ will turn insignificant when that measure is controlled for.

Second, we can test if connections only worked in before the *Jury* and for Law graduates, as opposed to before CDLs and/or for other defendants. If so, only the interaction $(Jury + LG) \times C_i$ will be significant and a Wald-test will indicate that the coefficients on the interactions between the four court/defendant-types dummy variables and the measure of connections are statistically different.

5.3 Can direct connections explaining the Law graduate advantage?

We assess if direct connections can explain the Law graduate advantage. We measure direct connections by the number of documents presented and the type of communication contained in the dossiers between the defendant and the *Jury*. We distinguish between communication related to legal arguments and communication related to non-legal matters, e.g., letters asking for an update on how the case is proceeding, etc. First, we conjecture that defendants with lower cost of communication will communicate more frequently with the *Jury* about matters related to their case than others. Table 5 presents mean comparisons between Law graduates and other defendants for these measures of direct connections. Second, we measure informality in the letters sent by defendants as an alternative measure of defendant's proximity to the *Jury*. In particular we focus on letters' headings and count the number of letters addressing the recipient as "Dear X" in a dossier. The use of "Dear" in French signals some degree of informality and therefore a tighter link between the sender and the recipient. We refer to such documents as "Informal letters from Defendant".

Table 5: Direct connections between the defendants and the *Jury*: Law graduates versus other defendants

	(5.1)	(5.2)	(5.3)
	Mean		
	LG	Others	Diff=0 (p-value)
1. Nb Doc from defendant	5.00	4.50	0.46
1.1 Nb of Doc - Communication from defendant without legal content	1.80	1.32	0.05*
1.2 Nb of Doc - Communication from defendant with legal content	3.2	3.2	0.98
1.3 Nb of pages - Communication from defendant without legal content	2.62	1.59	0.02**
1.4 Nb of pages - Communication from defendant with legal content	11.6	8.95	0.28
2. Nb Informal letters from Defendant	0.17	0.16	0.89
2.1 Nb of Doc - Informal letters without legal content	0.08	0.05	0.32
2.2 Nb of Doc - Informal letters with legal content	0.09	0.11	0.68

"Others" refers to all defendants who are not Law graduates. In row 1, "Nb Doc from defendant" refers to the number of documents sent by the defendant to the *Jury*. In row 1.1, "Nb of Doc - Communication from defendant without legal content" refers to the number of documents sent by the defendant where the content is not directly related to the legal aspect of his case. In row 1.2, "Nb of Doc - Communication from defendant with legal content" refers to the number of documents sent by the defendant in which the defendant presents legal information related to his innocence. In row 1.3, "Nb of pages - Communication from defendant without legal content" counts the pages in "Nb of Doc - Communication from defendant without legal content". In row 1.4, "Nb of pages - Communication from defendant with legal content" counts the number of pages in "Nb of Doc - Communication from defendant with legal content". Results are similar when those measures are transformed using $\log(k+1)$ - Available upon request. Columns 5.1 and 5.2 display the mean value for the group of Law graduates and other defendants, respectively. Column 5.3 presents the p-value associated with a two-sided t-test for equal means. The level of significant is indicated with with *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

The top panel of Table 5 reports mean comparisons between Law graduates and other defendants for measures of direct connections. Row 1 of the Table shows that Law graduates, on average, sent 5 letters directly to the *Jury*, while other defendants sent only 4.5, but the difference is not statistically significant. Thus, it does not appear that Law graduates communicated more with the *Jury* by sending more letters in total than other defendants. However, the total number of letters masks differences in the type of arguments presented in them. In rows 1.1 to 1.4, we break down the letters into those with legal arguments and those without. On the one hand, Law graduates submitted more and longer letters without legal arguments than other defendants (rows 1.1 and 1.3). Specifically, they sent on average an extra page of communication not directly related to the legal aspects of their case to the *Jury* (row 1.3). On the other hand, while Law graduates sent more and longer letter with legal arguments than other defendants, the difference is not statistically significant (rows 1.2 and 1.4). When focusing on letters revealing informality, we also find no statistical difference between Law graduates and other defendants (rows 2, 2.1 and 2.2). To summarize, the evidence suggests that Law graduates had direct connections to the *Jury* but they did not use these connections more than other defendants to defend themselves using legal arguments. Instead, they used them to communicate with the *Jury* about non-legal aspects of their case.

To test if these direct connections can explain the Law graduate advantage, we estimate equation (12) with interactions between the four court-defendant-type pair dummy variables and the measures of direct connections. Table 6 reports the results.

Table 6: Direct connections as the origin of Law Graduate advantage

Dep Variable	(6.1) $Acquit_{i,c}$	(6.2) $Acquit_{i,c}$	(6.3) $Acquit_{i,c}$
Sample	All	All	All
Measure of Connections C_i	Nb Docs from Def	Nb Doc - Com Def without legal arg	Nb Pages - Com Def without legal arg
Jury X LG	0.158** (1.985)	0.176*** (2.832)	0.176*** (2.769)
(Jury + LG)X C	0.0491 (0.779)	0.0141 (0.208)	0.0108 (0.189)
(Jury + Others)X C	0.0375 (0.951)	0.0567 (1.300)	0.0463 (1.196)
(CDL + LG)X C	0.0401 (0.920)	0.0583 (1.341)	0.0525 (1.311)
(CDL + Others)X C	-0.0151 (-0.598)	-0.0164 (-0.613)	-0.0181 (-0.764)
Nb of Docs as control	Yes	Yes	Yes
Wald-test equality interactions	0.43	0.15	0.15
Observations	798	798	798
Adjusted R-squared	0.055	0.056	0.056

Estimates of equation (12) with OLS controlling for the number of documents in each individual dossier and its interaction with the Jury dummy variable. The specifications include interactions between the four dummy variables identifying “Court-defendant-type” pairs and a particular measure of direct connections C_i as indicated in the column headings. Robust t-statistics in parentheses: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

The regression results reported in columns 6.1 to 6.3 show that direct connections to the *Jury* do not explain the Law graduate advantage before the *Jury*. Firstly, the coefficient of the interaction $Jury_c \times LG_i$ remains significant at usual levels and its magnitude increases from 0.10 to 0.17 after we control for the effect of direct connections to the courts. Secondly, the coefficients on the measures of direct connections interacted with the four court-defendant-type dummy variables are all statistically insignificant.

The upshot of this section is that although Law graduates had direct connections to the *Jury* and communicated more with it and on matters unrelated to the legal aspects of their defense, these direct connections do not explain why Law graduates enjoyed an advantage before the *Jury*. Those findings can be interpreted in terms of our model by saying that Law graduates were possibly too connected to the *Jury* and that their non-legal arguments were, therefore, discounted. The next section investigates if the advantage, instead, can be explained by indirect connections.

5.4 Can indirect connections explain the Law graduate advantage?

In this section, we build three measures of indirect connections between the supporters of a defendant and the *Jury* and investigate if they can explain the Law graduate advantage. They capture indirect connections via a defendant’s supporters ($\beta_{c,i}$ in our theory) and are based on information from the defendants’ dossiers. First, we measure indirect connections by the number of the letters of support in a defendant’s dossier originating from supporters in Paris and by the number of different Paris-based supporters. The rationale is that the *Jury* and its members were located in Paris and that the three judges had studied Law at Parisian Universities. Paris-based authors of letters of support therefore had a stronger connection to the *Jury* than other authors, as they would interact in the same social and professional circles.

Second, we built an index of the portfolio of letters of support of each defendant where the letters in the portfolio are weighted by how connected to the *Jury* the supporters sending them were. To do this, we divide the letters of support in a defendant’s dossier into groups where letters originated from the same “organization” (such as, for example, a ministry, a prefecture or a bureau) and calculate the fraction of letters from each organization.³² We index these organizations with $s = 1, \dots, n$ where n is the total number of organizations in the data set. We argue that a supporter has a stronger (indirect) connection to the *Jury* if he is associated with an organization that sends more documents and letters of support to the *Jury* in general, i.e., in relation to all cases. For example, the Ministry of Home Affairs corresponded more with the *Jury* than the Prefecture of the Morbihan *département*. We hypothesize that a letter from someone from the Ministry of Home Affairs would carry more weight than a letter from someone from the Prefecture of the Morbihan *département*. We, therefore, weight the share of letters in a defendant’s portfolio from organization s with the total number of documents and letters of support from that organization in the entire data set. We calculate two different versions of this index. The first version is called “Indirect connections via supporters” and is defined as:³³

$$\text{Indirect connections via supporters}_i = \sum_{s=1}^n \left(\frac{NbLetters_{s,i}}{\sum_{s=1}^n NbLetters_{s,i}} \times NbDocs_s \right), \quad (13)$$

where $\frac{NbLetters_{s,i}}{\sum_{s=1}^n NbLetters_{s,i}}$ is the share of letters of support in defendant i ’s portfolio that originated from supporters associated with organization s and the weight $NbDocs_s$ is the total number of documents from organization s we have found in the archives of the *Jury* across all defendants. The second version excludes letters of support from the weight attached to each organization. The aim is to capture the connection between supporters of a defendant i and the *Jury* net of advocacy in support of defendants in general. For example, if the Ministry of the Interior sent X letters of supports (with arguments for why a defendant should or should not be banned) and Y administrative reports with factual information, then the weight is only based on Y. We call this measure “Indirect connections via supporters (excl. Letters)”. It is defined as:

$$\text{Indirect connections via supporters (excl. letters)}_i = \sum_{s=1}^n \left(\frac{NbLetters_{s,i}}{\sum_{s=1}^n NbLetters_{s,i}} \times (NbDocs_s - NbLetters_s) \right). \quad (14)$$

The variable $(NbDocs_s - NbLetters_s)$ measures how often an organization acted as an informant to the *Jury* (by providing factual reports or official documents).

Third, we observe the number of documents addressing the recipient as “Dear X” in a dossier. We refer to documents using this informal address as “informal documents” and we use the number of informal documents in the dossier of a defendant as an index of his indirect connections.

Table 7 reports mean comparisons of these proxies for indirect connections for Law graduates and other

³²We classify “organizations” along two dimensions: its name (e.g., Ministry of Home Affairs) and its location (e.g., Paris). For example, a bureau of the Ministry of Home Affairs located in Lyon is considered as a different entity than the Ministry itself located in Paris. Sometimes, supporters do not belong to any specific organization: so for each *département* there is a fictitious “organization” of people not affiliated to any organization (“individuals”). Among organizations, individuals from Paris, the Ministry of Home Affairs (Paris), the National Assembly (Paris), the Prefecture of the Seine *département* (Paris), the Prefecture of the North *département*, the Prefecture of the Morbihan *département*, and individuals from the Nièvre *département* are the ones that are more “connected” to the *Jury*.

³³In cases where a defendant did not get any letters of support, we let the “Average indirect connections via supporters” take the value of 0.

defendants. The results in rows 1 and 2 show that Law graduates received more letters of support from supporters in Paris and that they had more Paris-based supporters than other defendants.³⁴ Moreover, the two indexes of the portfolio of letters of support from equations (13) and (14) also indicate that the supporters of Law graduates were better connected to the Jury than those of other defendants (rows 3 and 4). Finally, the dossiers of Law graduates contained more “informal documents” than those of other defendants (row 5). Interestingly, the difference is not statistically significant for documents directly sent to the *Jury* (rows 5.1 and 5.2), but it is significant in letters addressed to third parties to the case (row 5.3). We interpret this as evidence that Law graduates were able to activate connections to indirectly influence the *Jury*.

Overall, the evidence presented in Table 7 shows that in the dossiers of Law graduates there were more letters coming from well-connected supporters and that Law graduates were able to leverage connections to third parties to seek influence the *Jury*. In short, Law graduates were connected to supporters with stronger indirect connections to the *Jury* than other defendants.

Table 7: The content of the dossiers: Indirect connections

	(7.1)	(7.2)	(7.3)
	Mean		
	LG	Others	Diff=0 (p-value)
1. Nb Letters of support from Paris	2.72	1.82	0.09*
2. Nb of Supporters from Paris	1.23	0.70	0.01***
3. Indirect connections via supporters	47.41	27.82	0.03**
4. Indirect connections via supporters (excl. letters)	26.59	15.13	0.03**
5. Nb Informal documents (“Dear” Letters)	1.99	1.26	0.03**
5.1 Nb Informal documents to Cassin	0.15	0.20	0.47
5.2 Nb Informal documents to Jury	0.22	0.22	0.96
5.3 Nb Informal documents not to Jury	1.77	1.04	0.02**

“Others” is all defendants not being Law graduates. In row 1, “Nb Letters of support from Paris” refers to the number of letters of support with a sender located in Paris appearing in a defendant dossier. In row 2, “Nb of Supporters from Paris” refers to the number of different supporters from Paris sending letters of support. In row 3, “Indirect connections via supporters” refers to the index defined in equation (13). In row 4, “Indirect connections via supporters (excl. letters)” refers to the index defined in equation (14). In row 5, “Nb Informal documents” refers to the number of documents including “Dear” in their headings. Rows 5.1 to 5.3 decompose the total number of informal documents from row 5 depending on the sender and receiver of such documents. Results are similar when those measures are transformed using $\log(k+1)$ - Available upon request. Columns 7.1 and 7.2 display the mean value for the group of Law graduates (LG) and other defendants (others), respectively. Column 7.3 reports the p-value of a two-sided t-test of equal means and its level of significance with *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

We now ask if these observed differences in indirect connections between a defendant’s supporters and the *Jury* can explain the Law graduate advantage. To investigate this, we estimate equation (12) and test if controlling for the various measures of indirect connections interacted with the four court/defendant-type dummy variables eliminates the Law graduate advantage. Table 8 reports the results.

We observe that the interaction $Jury_c \times LG_i$ is insignificant in three specifications: when indirect connections are measured by letters of support from Paris (column 8.1) or by the two indexes of indirect connections via supporters (columns 8.3 and 8.4). We can, accordingly, infer that these three measures of indirect connections at least partially explain the Law graduate advantage before the *Jury*. In contrast, in columns 8.5 and 8.6, the size of the coefficients on $Jury_c \times LG_i$ remain close to those of the baseline estimates and the coefficients are significant at the five percent level. This suggests that indirect connections as inferred

³⁴The two groups have the same total number of letters of support (see Table 4). Accordingly, it is the composition of who sent them that differs.

by informality in the communication between supporters of the defendants and the *Jury* cannot explain the Law graduate advantage. We also observe that the $Jury_c \times LG_i$ interaction remains marginally significant in the specification in column 8.2 where we proxy indirect connections with the number of different Paris-based supporters.

For the three cases in which controlling for indirect connections eliminates the Law graduate advantage, we can gain further insights by studying the interactions between these measures of indirect connections and the four court/defendant-type dummy variables. This is informative about the precise channel through which these connections operated. First, the results in column 8.1 and 8.4 show that letters of support from Paris-based supporters and “indirect connections via supporters (excl. letters)” mattered only for the decisions of the *Jury* in relation to Law graduates and not in the decisions of the *Jury* related to other defendants or in decisions by the CDLs. The result of the Wald-test reported at the bottom of the columns indicate that these differences are statistically significant in the case of the letters of support from Paris-based supporters. These results point to a specific effect of letters of support from Paris-based supporters for Law graduates in front of the *Jury*. They suggest that the Law graduate advantage came from the ability of Law graduates to leverage Paris-based connections. Second, the interactions between “indirect connections via supporters” and the $(Jury + LG)$ and the $(Jury + Others)$ court/defendant-type pair dummies (column 8.3), respectively, are both positive and significant. Accordingly, the *Jury* was more likely to acquit all types of defendants when it received letters of support from organizations with which it frequently interacted. The Wald-tests reported at the bottom of the column shows that we cannot reject that the coefficients on the four interaction terms are, in fact, the same.

Table 8: Indirect connections as the origin of Law graduate advantage

Dependent variable	(8.1) $Acquit_{i,c}$	(8.2) $Acquit_{i,c}$	(8.3) $Acquit_{i,c}$	(8.4) $Acquit_{i,c}$	(8.5) $Acquit_{i,c}$	(8.6) $Acquit_{i,c}$
Measure of Connections C_i =	Nb Letters from Paris	Nb Supporters from Paris	μ Indirect Connections	μ Indirect Connections (excl. letters)	Nb Doc Informal	Nb Doc Informal Not to Jury
$Jury \times LG$	0.0766 (1.518)	0.0949* (1.915)	0.0768 (1.376)	0.0707 (1.318)	0.123** (2.115)	0.127** (2.216)
$(Jury + LG) \times C$	0.119** (2.374)	0.154** (2.291)	0.0459** (2.003)	0.0453* (1.800)	0.0393 (0.660)	0.0372 (0.618)
$(Jury + Others) \times C$	0.0481 (1.188)	0.0813 (1.434)	0.0364** (1.966)	0.0221 (1.108)	0.0803* (1.764)	0.0829* (1.795)
$(CDL + LG) \times C$	0.0377 (1.139)	0.0746 (1.582)	0.0146 (1.095)	0.0129 (0.878)	0.0222 (0.603)	0.0166 (0.463)
$(CDL + Others) \times C$	-0.0177 (-0.869)	-0.0139 (-0.496)	0.0111 (0.825)	0.00298 (0.213)	0.0112 (0.368)	-0.0118 (-0.402)
Constant	0.137 (1.014)	0.171 (1.378)	0.206 (1.550)	0.164 (1.308)	0.169 (1.314)	0.130 (1.033)
Control Nb Docs	Yes	Yes	Yes	Yes	Yes	Yes
Control Nb Docs X Jury	Yes	Yes	Yes	Yes	Yes	Yes
F-test equality of Interactions	0.03**	0.05**	0.28	0.39	0.53	0.23
Observations	798	798	798	798	798	798
Adjusted R-squared	0.065	0.066	0.064	0.060	0.058	0.058

Estimates of equation (12) with OLS controlling for the size of each individual dossiers and its interaction with the Jury dummy variable. Each column includes the interaction between the four court/defendant-type dummy variables and C_i . The relevant measure of indirect connections C_i is defined in the heading to each column. Robust t-statistics in parentheses: *** p<0.01, ** p<0.05, * p<0.1

All in all, these results show that letters of support from Paris-based supporters specifically advantaged Law graduates. Indirect connections to the *Jury* advantaged all types of defendants but Law graduates were more frequently supported by third parties showing such connections to the *Jury*, as shown in Table 7. These results are consistent with the theoretical hypothesis that the Law graduate advantages was, at least in part,

caused by indirect connections.

5.5 Can the Law graduate advantage be explained by legal skills?

In the previous subsections, we have demonstrated that although the dossiers of Law graduates and other defendants were very similar (Table 4), they did differ in one important regard: Law graduates had better direct and indirect connections to the *Jury* than other defendants (Tables 5 and 7). While direct connections cannot explain the Law graduate advantage before the *Jury* (Section 5.3), we find consistent evidence that indirect connections can (Section 5.4). Indirect connections via Paris-based supporters played a key role as did connections via organizations with strong ties to the *Jury*.

There is, however, an alternative explanation that we need to rule out: the advantage that Law graduates enjoyed before the *Jury* could simply be explained by their legal skills. Although the dossiers of Law graduates, as documented in Table 4, did not look different from those of other defendants and Law graduates did not communicate with the *Jury* about legal matters more than other defendants (see Table 5), one may hypothesize that Law graduates because of their legal training were better at using the available evidence to defend themselves which, in turn, could explain the Law graduate advantage. We test and reject this hypothesis in three ways. In our first test, we distinguish Law graduates from Parisian Universities from those with a degree from another university outside Paris. The rationale is that the two groups would have similar professional and legal skills and should, therefore, if legal skills were the source of the Law graduate advantage, enjoy the *same* advantage. Finding that the two subgroups of Law graduates with similar legal skills were treated differently by the *Jury* would, in short, militate against the hypothesis that legal skills explains the Law graduate advantage.

Our data set includes 59 Law graduates from a Parisian university and 51 Law graduates from another university. Table 9 presents difference-in-differences estimates of the Law graduate advantage separately for Law graduates from a Parisian university and for Law graduates from another university. In panel A, we use all non-treated defendants as the control group, while in Panel B we excluded Law graduates from the control group. We observe in both cases that the difference in acquittal rates between Law graduates from a Parisian university and other defendants is around 16 percentage points larger in front of the *Jury* than in front of CDLs and is significant at the five-percent level. This is not the case for Law graduates from other universities, who do not display any advantage in front of the *Jury*. In Panel B, we only consider non-law graduates as the control group and reach the same results. This strongly suggests that legal skills cannot explain the Law graduate advantage and is consistent with the evidence that indirect connections related to Paris-based supporters presented in Section 5.4 can account for the advantage.

Table 9: The Law graduate advantage for Parisian vs non-Parisian Law graduates

Panel A: Control group = All those considered non-treated								
Considered as treated =	CDL			Jury			Diff-in-Diff	
	(9.1)	(9.2)	(9.3)	(9.4)	(9.5)	(9.6)	(9.7)	(9.8)
	Treated	Control	Diff=0	Treated	Control	Diff=0	$\Delta\Delta$	p-value
	Group	Group	(p-value)	Group	Group	(p-value)		
All Law graduates	0.081	0.11	0.45	0.31	0.23	0.097*	0.11**	0.03
Law graduates (Parisian U)	0.07	0.11	0.37	0.36	0.23	0.04**	0.16**	0.01
Other Law graduates	0.10	0.10	0.96	0.25	0.25	0.94	0.01	0.91
Panel B: Control group = All non-law graduate								
Considered as treated =	CDL			Jury			Diff-in-Diff	
	Treated	Control	Diff=0	Treated	Control	Diff=0	$\Delta\Delta$	p-value
	Group	Group	(p-value)	Group	Group	(p-value)		
	Group	Group	(p-value)	Group	Group	(p-value)		
Law graduates (Parisian U)	0.07	0.11	0.36	0.36	0.23	0.04**	0.17***	0.01
Other Law graduates	0.10	0.11	0.84	0.26	0.23	0.68	0.036	0.60

Panel A provides estimates on the whole sample, i.e., it uses all non-treated defendants as controls. Panel B provides estimates of one subgroup of lawyers compared to non-lawyers, i.e., excluded non-treated Law graduates from the control group. Column 9.1 presents the average acquittal rate of the treated group (defined in the left column) in front of the CDLs whereas column 9.2 presents the average acquittal rate of the control group (= all individuals not in the treated group) in front of the CDLs. Column 9.3 displays the difference between these two means. Column 9.4 presents the average acquittal rate of the treated group in front of the *Jury* whereas column 9.5 presents the average acquittal rate of the control group (= all individuals not in the treated group) in front of the *Jury*. Column 9.6 displays the difference between columns 9.4 and 9.5. Column 9.7 reports the difference-in-differences estimates from equation (11) (without any control) for the advantage towards each of the subgroups defined in the left column. Robust t-statistics in parentheses: *** p<0.01, ** p<0.05, * p<0.1

Our second test of legal skills as the source of the Law graduate advantage is based on the idea that Law graduates may have been better than other defendants to present information about their participation in the resistance to the *Jury*. We can test this by augmenting Equation (11) with a triple interaction “Information on participation in resistance \times Jury \times LG”. Estimates reported in Appendix Table C.3.1 show that this triple interaction is never significant. Accordingly, Law graduates were not better due to their legal training than other defendants at using information about participation in the resistance in front of the *Jury*.

Our third test is based on the idea that having a political career impairs the accumulation of legal skills. If so, Law graduates who had a longer political career would, therefore, have blunter legal skills, which would, in turn, result in a smaller advantage before the *Jury*. To test this, we augment Equation (11) with a triple interaction “Length of political career \times Jury \times LG”. The results reported in Appendix Table C.3.2 show that the Law graduate advantage was larger, rather than smaller, for defendants with longer political careers, measured by the number of years spent as a local representative (“conseiller général”). This, therefore, also suggests that the Law graduate advantage before the *Jury* is unrelated to legal skills.

Overall, the evidence from the various tests consistently militate against the Law graduate advantage being explained by differences in legal skills between Law graduates and other defendants.

6 Conclusion

Elite persistence has been an issue plaguing democratic transitions. Our results suggest that elite groups from the previous regime can leverage connections with members of the elite of the new democratic regime. Identifying this mechanism materializing at political transitions clarifies the mechanisms at play during similar transitional processes such as the Truth and Reconciliation commission in South-Africa, lustration in post-communist regimes, in post-Pinochet Chile and more recently in Afghanistan. The identification of

this paper relies on the comparison of decisions of two different transitional bodies whether to purge or not the exact same politicians. Our results hence speak of the advantage some elites might have in a system compared to another. More specifically, we compare the sentencing pattern of the *Jury d'Honneur* with the one of local courts (*Comités départementaux de libération* - CDLs) in the political purges happening in France after World War II. We focus on Law graduates as this group was/still is a powerful interest group in French politics (Le Béguec, 2003). Our baseline results indicate that the difference in clearance rates between Law graduates and other defendants was around 10 percentage points higher in front of the *Jury* than in front of CDLs. Put differently, using the decisions of CDLs as counterfactuals we observe that Law graduates have been treated more favourably than other defendants by the *Jury*. This advantage of Law graduates in the purging process was consequential as it appeared mainly in cases of electoral litigation.

In a second part of the paper, we investigate the source of this relative advantage of Law graduates in front of the *Jury*. We hypothesize that the advantage of Law graduates might result from shared connections between Law graduates defendants and the members of the *Jury* as they all had connections within the legal milieu. Additional results indeed show that the supporters of Law graduates were better connected to the *Jury*. We furthermore observe that this bias mainly emerged for highly connected law graduates, i.e. the ones graduating from Parisian universities.

What do these results imply? First, they show that transitional authorities are by definition connected to previous ones. So are the members of these transitional authorities. As a result, members of transitional authorities are connected to a subset of the previous elites. Even in the absence of any malicious plan, these connections confer on this subset of the previous elites an advantage in persisting through the transition and in keeping their political influence intact. We also show that this advantage of some elites in the transition will vary upon the design of the Court. In our case, Law graduates benefited from better connections in front of the *Jury* but apparently not in front of CDLs. The advantage of some elites in persisting is then evitable as it lies upon the shared connections between transitional bodies and the previous elites. Paying a systematic attention to these possible connections would be a way to avoid the perpetual overrepresentation of some elites throughout time. Second, these results also show how distorted political selection in new democracies might be. Beyond elite persistence, our results show that some elites are better than others in persisting. In our case, some politicians from previous regimes are cleared so elites persist. However, not only some elites persist, but a part of the elites is better at persisting. We show the importance of connections in explaining this pattern. Connections might be one dimension of the *de facto* power Acemoglu and Robinson (2006) refer to when investigating institutional persistence. They have been proven to be instrumental in distorting laws (Cohen and Malloy, 2014) and political selection within political regimes (Dal Bó et al., 2009). Our results show that connections also matter after major institutional changes. Connections, at least partially, explain elite persistence.

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A Proofs - Theoretical Section

A.1 Proof - Direct connections

Proof. Consider a given court c . We need to find the values of the fixed cost such that 1) a defendant wants to communicate that $l_{i,c} = \theta_Y$ when that is, in fact, the case and 2) a defendant do not want to claim that $l_{i,c} = \theta_Y$ when $\theta = \theta_N$. Assume that the court believes $l_{i,c} = \theta_Y$ and sets $m_{i,c} = \theta_Y$ in response to a letter with such a claim and consider the game between defendant i and court c . First, suppose that $\theta = \theta_Y$, i.e., there are mitigating circumstances for defendant i . Let the cost of communicating with court c for defendant i be $f_{i,c}$. He will then get $-(\theta_Y - \theta_Y - \gamma)^2 - f_{i,c}$ if he sends the letter $l_{i,c} = \theta_Y$ and $-(\theta_N - \theta_Y - \gamma)^2$ if he does not send a letter. Comparing these payoffs, we see that it is in the interest of defendant i to send the letter $l_{i,c} = \theta_Y$ if

$$f_{i,c} \leq (\theta_Y - \theta_N)(2\gamma + (\theta_Y - \theta_N)) \equiv \bar{f}. \quad (15)$$

Second, suppose that $\theta = \theta_N$, i.e., there are no mitigating circumstances for defendant i . If defendant i has cost $f_{i,c}$, then he will get $-(\theta_Y - \theta_N - \gamma)^2 - f_{i,c}$ if he sends the letter $l_{i,c} = \theta_Y$ and $-(\theta_N - \theta_N - \gamma)^2$ if he does not send a letter. Comparing these payoffs, we see that it is in the interest of defendant i not to lie and send a letter with $l_{i,c} = \theta_Y$ when $\theta = \theta_N$ if

$$f_{i,c} \geq (\theta_Y - \theta_N)(2\gamma - (\theta_Y - \theta_N)) \equiv \underline{f}. \quad (16)$$

Clearly, $\bar{f} > \underline{f}$. Given these strategies, the court will update via Bayes Rule its belief to “mitigating circumstances” if and only if $f_{i,c} \in [\underline{f}, \bar{f}]$. The three cases in the proposition follows immediately from this. \square

A.2 Proof - Indirect connections

Proof. This is a standard cheap talk game. Consider court c and suppose that it believes the third party if a letter saying $l_{i,c} = \theta_Y$ is received. There is no reason not send this letter if $\theta = \theta_Y$. Suppose, therefore, that $\theta = \theta_N$. In this case, the third party has an incentive to lie and write in the letter that $l_{i,c} = \theta_Y$. If he does write this, his payoff is $-\beta_{i,c}(\theta_Y - \theta_N)^2 - (1 - \beta_{i,c})(\theta_Y - \theta_N - \gamma)^2$. If he instead writes $l_{i,c} = \theta_N$, then his payoff is $-\beta_{i,c}(\theta_N - \theta_N)^2 - (1 - \beta_{i,c})(\theta_N - \theta_N - \gamma)^2 = (1 - \beta_{i,c})\gamma^2$. Comparing these two payoffs, we find that the third party will not be tempted to write a letter saying $l_{i,c} = \theta_Y$ when $\theta = \theta_N$ if

$$\beta_{i,c} > 1 - \frac{\theta_Y - \theta_N}{2\gamma} \equiv \bar{\beta}. \quad (17)$$

Clearly $\bar{\beta} < 1$. The cut-off $\bar{\beta} > 0$ because we assume that $\gamma > \frac{\theta_Y - \theta_N}{2} \equiv \bar{\gamma}$ for all i . Knowing this, court c will believe a letter of support claiming mitigating circumstances for defendant i coming from a third party with $\beta_{i,c} > \bar{\beta}$ and not otherwise. \square

B Baseline results - Robustness checks

B.1 Counterfactual analysis

B.1.1 Counterfactual - Method

We estimated a model where the decision by the *Jury* to clear a defendant was regressed on variables capturing the participation of defendants in civilian and military resistance, which were the criteria officially used by the *Jury*, controlling for the decision by CDLs. We therefore estimated the following regression equation on the sample of decisions of the *Jury*, :

$$Acquitted_{Jury,i} = \alpha + \beta_1 Acquitted_{CDL,i} + \beta_2 CivilianResistance_i + \beta_3 MilitaryResistance_i + \varepsilon_i$$

We then stored the estimators of $\beta_1, \beta_2, \beta_3$: $\hat{\beta}_1, \hat{\beta}_2, \hat{\beta}_3$ and computed an individual probability to be purged given the information retrieved from defendants' dossiers. The estimated probability given by $Counterfactual_i = \hat{\beta}_1 Acquitted_{CDL,i} + \hat{\beta}_2 CivilianResistance_i + \hat{\beta}_3 MilitaryResistance_i$ provides a counterfactual benchmark based on the official criteria used by the *Jury* against which to compare actual clearance rates. For simplicity, we present our counterfactual at the group-level.

B.1.2 Counterfactual - Comparison with actual acquittal rate

Appendix B.1: Counterfactual versus Actual acquittal rate for non-Law graduates and Law Graduates

	Not Law Graduates	Law graduates
Counterfactual	23.7%	22.4%
Actual	22.8%	30.9%

B.2 Placebo tests and control variables

Appendix B.2: Information in dossier and advantage before the *Jury*

	Treated:	CDL			Jury			Diff-in-Diff	
		(B.2.1)	(B.2.2)	(B.2.3)	(B.2.4)	(B.2.5)	(B.2.6)	(B.2.7)	
		Treated	Control	Diff=0	Treated	Control	Diff=0	$\Delta\Delta$	p-value
		Group	Group	(p-value)	Group	Group	(p-value)		
Control variables		Politics and political mandates							
	Mayor	0.12	0.09	0.31	0.22	0.28	0.14	-0.09**	0.02
	Pres/Vice-Pres or Sec Assembly	0.16	0.10	0.27	0.22	0.25	0.67	0.10**	0.05
	MP of an occupied department	0.09	0.11	0.52	0.20	0.31	0.01***	-0.09**	0.02
		Networks, clubs and religion							
	Jewish MPs	0.17	0.10	0.59	0.67	0.24	0.02**	0.35*	0.09
		Occupations							
	Journalist	0.04	0.11	0.14	0.29	0.25	0.455	0.11*	0.10
		Informational cues							
	Mayor under “Etat Fr”	0.07	0.11	0.16	0.15	0.29	0.00***	-0.09**	0.02
	Arrested by Etat Fr	0.14	0.10	0.56	0.50	0.24	0.01***	0.23**	0.03
	Militarian resistance	0.27	0.06	0.00***	0.68	0.14	0.00***	0.32***	0.00
	Civilian resistance	0.13	0.05	0.01***	0.38	0.08	0.00***	0.22***	0.00
		Continuous variables							
	Age							-0.005**	0.02
Placebo Tests		Politics and political mandates							
	Senator	0.09	0.10	0.95	0.25	0.25	0.92	0.002	0.95
	Rightwing	0.10	0.09	0.42	0.25	0.25	0.93	-0.03	0.49
	Center	0.06	0.11	0.15	0.21	0.26	0.35	0.003	0.95
	MPs elected in Paris	0.05	0.10	0.44	0.25	0.25	0.99	0.05	0.57
	Dynastic Politicians	0.06	0.11	0.29	0.27	0.25	0.70	0.07	0.27
		War experience							
	WWI veteran	0.11	0.09	0.63	0.25	0.25	0.89	-0.008	0.84
	WWII fighter	0.08	0.10	0.78	0.38	0.24	0.15	0.15	0.18
		Networks, clubs and religion							
	Free Masons	0.07	0.10	0.66	0.33	0.25	0.45	0.12	0.30
	Labour Unions	0.03	0.11	0.19	0.19	0.26	0.45	0.01	0.87
	Agr organization	0.13	0.10	0.50	0.26	0.25	0.85	-0.02	0.73
	War Medal	0.11	0.10	0.79	0.24	0.26	0.62	0.03	0.48
	Légion d'Honneur	0.12	0.09	0.21	0.25	0.25	0.88	-0.03	0.46
	Veterans club	0.11	0.10	0.94	0.32	0.25	0.50	0.06	0.60
		Occupations							
	Civil servant	0.08	0.10	0.73	0.32	0.25	0.41	0.10	0.28
	Workers	0.11	0.09	0.90	0.25	0.25	0.99	-0.01	0.91
		Informational cues							
	Excluded by his party	0.12	0.09	0.37	0.31	0.23	0.09*	0.05	0.28
	Signed Bergery motion	0.06	0.11	0.29	0.22	0.26	0.54	0.007	0.89
		Continuous variables							
	National Mandate							0.00	0.76
	Conseiller général							-0.001	0.49
	Study Years							0.003	0.66

Column B.2.1 presents the average acquittal rate of the treated group (defined in the left column) in front of the Comité Départementaux de Libération whereas Column B.2.2 presents the average acquittal rate of the control group (= all individuals not in the treated group) in front of the Comités. Column B.2.3 displays the difference between these two means. Column B.2.4 presents the average acquittal rate of the treated group in front of the Jury whereas Column B.2.5 presents the average acquittal rate of the control group (= all individuals not in the treated group) in front of the Comités. Column 4.6 displays the difference between Column B.2.4 and B.2.5. Column B.2.6 introduces the estimates of Equation 1 without any control for the bias towards each of the subgroup defined in the left column. This estimate is also by construction equal to the difference between Column B.2.6 and B.2.3. Robust t-statistics in parentheses: *** p<0.01, ** p<0.05, * p<0.1

B.3 Court-level estimates

Appendix B3: Relative bias and courts clearance rate		
	(B.3.1)	(B.3.2)
Dependent variable	$Acquit_i$	$Acquit_i$
Samples	CDLs	Jury
LG	-0.0284 (-0.915)	0.0712* (1.737)
Constant	-0.0797 (-0.829)	0.0636 (0.558)
Controls:		
Age and Religion	Yes	Yes
Journalist	Yes	Yes
Political mandates	Yes	Yes
Resistance and collaboration WWII	Yes	Yes
Observations	399	399
Adjusted R-squared	0.088	0.332
Column B.3.1 estimates a bivariate regression estimating the statistical advantage of law graduates before the CDLS and includes all baseline control variables to this estimation. Column B.3.2 estimates a bivariate regression estimating the statistical advantage of law graduates before the Jury and includes all baseline control variables to this estimation. Robust t-statistics in parentheses: *** p<0.01, ** p<0.05, * p<0.1		

B.4 Alternative estimations

Appendix B.4: Alternative specifications

	(B.4.1)	(B.4.2)	(B.4.3)	(B.4.4)	(B.4.5)	(B.4.6)	(B.4.7)	(B.4.8)	(B.4.9)	(B.4.10)	(B.4.11)
Dep Variable	$Acquit_{i,c}$	$Acquit_{i,c}$	$Acquit_i$	$Acquit_i$	$Acquit_i$	$Acquit_i$	$Acquit_i$	$Acquit_i$	$Acquit_i$	$Acquit_i$	$Acquit_i$
Sample	All	All	Jury All	Jury All	Jury All	Not Acquit CDL OLS	Not Acquit CDL Logit	Not Acquit CDL Probit	Acquit CDL OLS	Acquit CDL Logit	Acquit CDL Probit
Estimation Method	Logit	Probit	OLS	Logit	Probit						
Jury \times LG	0.977** (2.013)	0.542** (2.179)									
LG	-0.422 (-0.975)	-0.248 (-1.155)	0.0834** (2.051)	0.821** (2.224)	0.409** (2.060)	0.111*** (2.685)	1.052*** (2.846)	0.564*** (2.791)	-0.183 (-1.078)	-2.218 (-1.081)	-0.992 (-1.159)
Constant	-4.972*** (-3.732)	-2.603*** (-3.819)	0.0978 (0.949)	-3.014*** (-3.107)	-1.710*** (-3.169)	0.0136 (0.133)	-4.173*** (-3.819)	-2.380*** (-4.089)	1.373*** (3.897)	6.279** (2.293)	3.499** (2.359)
<u>Controls:</u>											
Baseline	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
CDLS decision	NO	NO	YES	YES	YES	NO	NO	NO	NO	NO	NO
Observations	798	798	399	399	399	359	359	359	40	30	30
Adjusted R^2			0.411			0.316			0.107		

Robust t-statistics in parentheses: *** p<0.01, ** p<0.05, * p<0.1. This Table estimates the baseline specification using various estimation methods (Logit and Probit). It also focuses on decisions by the Jury and control for the decision of CDLs various ways. Columns B.4.3 to B.4.5 control for the decision of the CDLs. Columns B.4.6 to B.4.8 estimate the specification within the subsample of cases for which the defendant has not been acquitted by a CDL. Columns B.4.9 to B.4.11 estimate the specification within the subsample of cases for which the defendant has been acquitted by a CDL. This Table presents estimates of Equation 1 when focusing on law graduates and adding variables explaining a difference in sentencing patterns between the Jury and CDL. Individual controls include: Age, Jewishness, Journalist, Mayor, Special Role in the Assembly, Civilian Resistance, Military resistance, Arrested by Etat Francais, Mayor under Etat Francais, Mp of an occupied territory. Each individual control is also interacted with the Jury dummy variable.

B.5 Controlling for the composition of resistance in each department

Appendix B.5: Controlling for the composition of resistance in each department

Dep variable	(B.5.1) <i>Acquit_{i,c}</i>	(B.5.2) <i>Acquit_{i,c}</i>	(B.5.3) <i>Acquit_{i,c}</i>	(B.5.4) <i>Acquit_{i,c}</i>	(B.5.5) <i>Acquit_{i,c}</i>	(B.5.6) <i>Acquit_{i,c}</i>	(B.5.7) <i>Acquit_{i,c}</i>	(B.5.8) <i>Acquit_{i,c}</i>	(B.5.9) <i>Acquit_{i,c}</i>	(B.5.10) <i>Acquit_{i,c}</i>
Jury	0.133*** (5.662)	0.123*** (4.895)	0.152*** (7.314)	0.114* (1.798)	0.134*** (5.707)	0.123*** (4.915)	0.118* (1.837)	-0.0775 (-1.030)	-0.110 (-1.447)	0.0763 (0.568)
Gaullist/Communist Resistance	0.0738 (1.039)				0.0801 (1.081)			-0.118 (-0.703)	-0.122 (-0.728)	-0.0839 (-0.479)
Jury X Gaullist/Communist Resistance	0.138 (1.277)				0.152 (1.372)			0.0172 (0.0695)	0.0391 (0.154)	0.0483 (0.172)
Gaullist/Domestic Resistance		0.0868 (0.283)				0.108 (0.341)		0.446 (0.609)	0.453 (0.617)	0.250 (0.282)
Jury X Gaullist/Domestic Resistance		0.775 (1.525)				0.849 (1.621)		0.385 (0.342)	0.351 (0.309)	-0.145 (-0.107)
Foreign/Domestic Resistance			-0.000265 (-1.242)		-0.000439 (-1.578)	-0.000319 (-1.223)	-0.000404* (-1.679)	-0.000237 (-0.865)	-0.000214 (-0.756)	-0.000230 (-0.649)
Jury X Foreign/Domestic Resistance			-0.000692*** (-5.750)		-0.00102*** (-3.685)	-0.00111*** (-3.770)	-0.000652*** (-4.462)	-0.000607 (-1.557)	-0.000718** (-2.270)	-0.000708* (-1.862)
Communist/Domestic Resistance				-0.305** (-2.519)			-0.311** (-2.540)	-0.339** (-2.292)	-0.340** (-2.298)	-0.268* (-1.651)
Jury X Communist/Domestic Resistance				0.106 (0.602)			0.100 (0.568)	0.184 (0.825)	0.191 (0.864)	0.155 (0.696)
LG									-0.0226 (-0.751)	-0.0294 (-0.950)
Jury X LG									0.110** (2.414)	0.101** (2.183)
Constant	0.0908*** (5.545)	0.0972*** (5.326)	0.101*** (6.642)	0.221*** (4.409)	0.0911*** (5.560)	0.0973*** (5.329)	0.223*** (4.402)	0.169*** (3.155)	0.176*** (3.171)	0.0526 (0.458)
Proof of resistance								Yes	Yes	Yes
Individual controls										Yes
Observations	798	798	798	798	798	798	798	798	798	798
Adjusted R-squared	0.045	0.043	0.039	0.045	0.046	0.044	0.045	0.251	0.254	0.279

All regressions test the effect of the composition of resistance, and so likely of CDL, on the bias as the ratio of the number of militants belonging to different groups as defined by their certificate of resistance. For example, Gaullist/Communist resistance is defined as the ratio of the number of members of gaullist resistant factions over the number of members of communist resistant factions in a department. Proof of resistance include: Civilian Resistance, Military resistance. Individual controls include the following control variables: Age, Jewishness, Journalist, Mayor, Special Role in the Assembly, Arrested by Etat Francais, Mayor under Etat Francais, In occupied territory. Robust t-statistics in parentheses: *** p<0.01, ** p<0.05, * p<0.1

B.6 CDLs and the treatment of information on Law graduates

Appendix B.6: The specific treatment of information on Law graduates in front of CDLs					
	(B.6.1)	(B.6.2)	(B.6.3)	(B.6.4)	(B.6.5)
Dep Variable	$Acquit_{i,c}$	$Acquit_{i,c}$	$Acquit_{i,c}$	$Acquit_{i,c}$	$Acquit_{i,c}$
Sample	Decisions CDL	Decisions CDL	Decisions CDL	Decisions CDL	Decisions CDL
LG X Civil Res	0.0831 (1.482)				0.0856 (1.647)
LG X Military Res		-0.00289 (-0.0242)			-0.0107 (-0.0904)
LG X Arrested EF			0.171 (0.711)		0.164 (0.687)
LG X Mayor EF				0.00183 (0.0282)	0.00468 (0.0753)
Constant	0.0529 (0.547)	-0.0612 (-0.656)	0.0971 (1.056)	0.0978 (1.069)	-0.0630 (-0.648)
Individual controls	Yes	Yes	Yes	Yes	Yes
Observations	399	399	399	399	399
Adjusted R-squared	0.012	0.079	-0.004	0.003	0.085

Estimations focus on the decisions of CDLs. They assess how the CDLs could have reacted to certain types of information contained in the dossier of the Jury and used by law graduates. They interact the law graduates dummy variable with variable assessing the information in the dossier of defendants. Individual controls include: Age, Jewishness, Journalist, Mayor, Special Role in the Assembly, Mp of an occupied territory. Each individual control is also interacted with the Jury dummy variable. Robust t-statistics in parentheses: *** p<0.01, ** p<0.05, * p<0.1

B.7 Persistence: When did the advantage of Law graduates appear?

We proxy the intentions of defendants in two ways. First, we use the information in their dossiers related to participation in the 1945 municipal elections and divide the defendants into two groups: those who ran for election and therefore intended to pursue a political career, and those who did not. Appendix B.7, columns B.7.1 and B.7.2 report separate estimates of equation (11) for the two groups. We observe that the Law graduate advantage is only significant, at the ten-percent level, for the group of defendants who ran in the 1945 municipal election. Moreover, Column B.7.3 reports a specification estimated on the full sample in which equation (11) is augmented with a triple interaction between the Law graduate dummy, the *Jury* dummy, and a dummy coding whether the defendant ran in the election. The coefficient on the triple interaction term is positive and significant at the one-percent level. Its magnitude indicates that the acquittal rate before the *Jury* was 40 percentage points higher for Law graduates who ran for election than for those who did not.

Second, as an alternative proxy for a defendant's intention to seek public office, we leverage a discontinuity caused by a change in the remit of the Jury. Until September 1945, the *Jury* was in charge of judging two types of cases: cases of electoral litigation brought by departmental prefects and cases brought by the defendants themselves. These cases were mainly about eligibility to run for election. An order of 13th of September 1945 expanded the remit of the *Jury* to include the cases of all parliamentarians who had voted in favor of the enabling act or had collaborated with the Vichy regime. Many of whom did not intend to run in the elections. We know from the dossiers of the defendants when a case was considered by the *Jury* and can, therefore, distinguish cases considered before and after the change in the remit and create a pre- and a post-reform dummy. In this way, we can use the discontinuity to test if the advantage of Law graduates before the *Jury* was bigger for defendants who wanted to continue their political career (as revealed by an early case related to eligibility for election) than for other defendants. Specifically, we augment equation (11) with interaction terms between the pre- and post-reform dummies and $LG_i \times Jury_c$, respectively, to

allow the Law graduate advantage to differ depending on when the case was heard. Appendix B.7, columns B.7.4 to B.7.6 present the results. The result in column B.7.4 shows that the Law graduate advantage before the *Jury* was around 29 percentage points larger for the defendants tried before the reform when the *Jury* focused on electoral litigation than for those tried after the expansion of its remit.³⁵ A Wald-test shows that this difference is statistically significant. The specifications in the other two columns are augmented with time polynomials and their interactions with $LG_i \times Jury_c$ and the reform dummy variables and show that that the effect is not driven by time trends in the sentences of the *Jury*.³⁶ The advantage of Law graduate advantage before the *Jury* therefore facilitated elite persistence, as it materialized specifically when a Law graduate intended to continue his political career.

Appendix B7: Law graduates' advantage appears when it matters: Electoral litigations						
Dep variable	(B.7.1)	(B.7.2)	(B.7.3)	(B.7.4)	(B.7.5)	(B.7.6)
Sample	$Acquit_{i,c}$ Ran for elections	$Acquit_{i,c}$ Did not run	$Acquit_{i,c}$ All	$Acquit_{i,c}$ All	$Acquit_{i,c}$ All	$Acquit_{i,c}$ All
Jury	0.536 (1.122)	0.169 (1.512)	0.165 (1.496)	0.170 (1.533)	0.200* (1.726)	0.231* (1.945)
LG	-0.131 (-1.437)	-0.00836 (-0.248)	-0.00575 (-0.170)			
Jury X LG	0.330* (1.913)	0.0569 (1.215)	0.0524 (1.112)			
Jury X LG X Ran for elections			0.404*** (2.610)			
Jury X LG X Pre-reform				0.338*** (2.985)	0.281** (2.349)	0.301** (2.462)
Jury X LG X Post-reform				0.0460 (0.960)	0.0661 (1.379)	0.0772 (1.638)
Individual Controls	Yes	Yes	Yes	Yes	Yes	Yes
Polynomial Date					3	3
Pre-reform dummy						Yes
Wald Test				0.016**	0.093*	0.088*
Observations	118	680	798	798	792	792
Adjusted R-squared	0.164	0.303	0.290	0.302	0.336	0.342

Column B.7.1 estimates equation (1) on the subset of defendants who ran for the first post-WWII mayoral elections. Column B.7.2 estimates equation (1) on the subset of defendants who ran for the first post-WWII mayoral elections. Column B.7.3 investigates how the magnitude of the advantage of Law graduates varied with their participation in the first post-WWII elections by adding a triple interaction term (Jury X LG X Ran for elections) and controlling for the interaction Jury X Ran for elections. It controls for the variables not interacted in the estimation (Jury, LG, and Ran for elections). Columns B.7.4 to B.7.6 estimate equation (1) in a manner akin to a RDD estimates using the cutoff of the September 13rd as a discontinuity. It therefore shows how the reform affected the bias of the Jury towards Law graduates after adding individual controls, a time-polynomial of order 3, and a pre-reform dummy variable. These estimates assess the break in time trend of the advantage of Law graduates due to the reform of the remit of the Jury from mainly electoral litigations to investigations of all cases. Individual controls include: Age, Jewishness, Journalist, Mayor, Special Role in the Assembly, Civilian Resistance, Military resistance, Arrested by the Vichy regime, Mayor under Etat Francais, parliamentarian of an occupied territory. Each individual control is interacted with the Jury dummy variable. Robust t-statistics in parentheses:

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

³⁵To determine the difference between the two groups we compare the coefficients attached to Law graduates tried before the reform and those tried after the reform, hence: 33.8-4.6.

³⁶Part of this difference in sentencing patterns over time might be captured by the comparison with the decision of CDLs (also varying over time). Adding time polynomials however allows to take time varying dynamics of the *Jury* into account as previous research has shown that time-dependence exists in sentencing (Bindler and Hjalmarsson, 2018).

B.8 Persistence and intensive margins

Appendix B.8: Persistence and Jury decision - Intensive margin

Panel A - Independent variable of interest: Acquitted by the Jury

	(B.8.A.1) Years as Mayor	(B.8.A.2) Years in Parliament	(B.8.A.3) Years as Minister
Acquitted _{Jury}	0.271** (2.006)	0.366*** (3.113)	0.0695** (2.032)
Acquitted _{CDL}	-0.0373 (-0.237)	0.117 (0.779)	0.0344 (0.571)
Constant	1.001*** (4.174)	0.738*** (3.643)	0.0694* (1.831)
Observations	399	399	399
Adj R^2	0.155	0.137	0.051
<u>Control variables</u>			
Individual	YES	YES	YES

Panel B - Independent variable of interest: Law graduates

	(B.8.B.1) Years as Mayor	(B.8.B.2) Years in Parliament	(B.8.B.3) Years as Minister
LG	-0.0160 (2.366)	0.181** (1.362)	0.0341
Acquitted _{CDL}	0.0763 (0.479)	0.284* (1.888)	0.0662 (1.088)
Constant	1.040*** (4.307)	0.722*** (3.638)	0.0664 (1.528)
Observations	399	399	399
Adj R^2	0.142	0.115	0.038
<u>Control variables</u>			
Individual	YES	YES	YES

Robust t-statistics in parentheses: *** p<0.01, ** p<0.05, * p<0.1.
Level of observation: Defendant. In Panel A, the main independent variable is a dummy variable equal to 1 if the defendant has been acquitted by the Jury. In Panel B, the main independent variable is a dummy variable equal to one if the defendant was a law graduate. Dependent variables: Years as Mayor is the number of years the defendant has spent as mayor after WWII (Log-transformed). Years in Parliament is the number of years the defendant has spent either as a deputy or as a senator after WWII (Log-transformed). Years as Minister is the number of years the defendant has spent as a minister after WWII (Log-transformed). Individual controls include: Age and religion (Age, Jewishness); Journalist; Political mandates (Mayor, Special Role in the Assembly, parliamentarian of an occupied territory); Resistance and collaboration WWII (Civilian Resistance, Military resistance, Arrested by the Vichy regime, Mayor under Etat Francais).

C Additional evidence on the mechanisms

C.1 Definition and descriptive statistics: Mechanisms dataset

Variable	Definition	Min	Max	Mean	s.d
Structure					
Nb Doc	Number of documents in the dossier	10	170	40.69	26.38
Nb Pages	Number of pages in the dossier	12	384	55.44	45.26
Nb Doc from Jury	Number of Document produced by the Jury	4	50	13.70	6.16
Nb Archival Docs	Number of Archives	0	50	2.79	6.23
Nb Information requests	Number of information requests sent by the Jury	0	4	1.08	0.95
Nb Letters of support	Number of letters of support	0	90	7.81	12.34
Nb Letters of support - in Favor	In favor of acquitting the defendant	0	90	7.41	11.89
Nb Letters of support - Against	Against acquitting the defendant	0	20	0.22	1.31
Nb Letters of support - Neutral	Neutral	0	10	0.18	0.71
Nb Letters of support - Res	from the Resistance	0	39	2.41	4.28
Nb Letters of support - Mil	from the Military	0	11	0.27	1.01
Nb Letters of support - Administration	from an administration	0	41	1.02	2.91
Nb Letters of support - Others	from other type of organizations	0	70	2.66	6.38
Nb Doc - Military resistance	Nb of Doc mentioning participation in military resistance	0	35	0.77	3.11
Nb Doc - Civilian resistance	Nb of Doc mentioning participation in civilian resistance	0	64	8.55	10.63
Nb Doc - Resistant Press	Nb of Doc mentioning participation in resistant press	0	13	0.59	1.79
Nb Doc - Legal Arguments	Nb of Doc mentioning legal arguments	0	27	5.43	3.95
Nb Doc - Political opinion	Nb of Doc mentioning political opinions of the defendant	0	90	10.02	14.34
Nb Doc - Reelection	Nb of Documents mentioning reelection prospects	0	18	1.07	2.37
Nb Doc - Other topic	Nb of Documents mentioning other topics	0	58	6.32	8.65
Direct connections					
1. Nb Doc from defendant	Nb of Docs sent by the Defendants	0	62	4.64	6.05
1.1 Nb of Doc - from Def without legal content	Nb of Doc sent by the Defendant - No legal content	0	19	1.45	2.23
1.2 Nb of Doc - from Def with legal content	Nb of Doc sent by the Defendant - Legal content	0	55	3.19	4.74
1.3 Nb of pages - from Def without legal content	Nb of Pages sent by the Defendant - No legal content	0	53	1.87	3.81
1.4 Nb of pages - from Def with legal content	Nb of Pages sent by the Defendant - Legal content	0	321	9.68	22.05
2. Nb Informal letters from Defendant	Nb of letters with headings “Cher”	0	6	0.17	0.62
2.1 Nb of Doc - Informal letters without legal content	Nb of letters with headings “Cher” - No legal content	0	3	0.06	0.30
2.2 Nb of Doc - Informal letters with legal content	Nb of letters with headings “Cher” - Legal content	0	6	0.11	0.50
Indirect connections					
1. Nb Letters of support from Paris	Nb of letters of support by a Parisian sender	0	38	2.07	4.77
2. Nb of Supporters from Paris	Nb of different supporters from Paris	0	14	0.85	1.81
3. Indirect connections via supporters	Average number of documents sent by different supporters (even outside the case)	0	583.83	33.22	78.38
3. Indirect connections via supporters (excluding letters)	Same as above excluding letters	0	341.67	18.29	46.80
5. Nb Informal documents (“Dear” Letters)	Nb of document with headings “Cher”	0	27	1.46	3.02
5.1 Nb Informal documents to Cassin	Addressed to R. Cassin	0	5	0.19	0.56
5.2 Nb Informal documents to Jury	Addressed to the Jury	0	5	0.22	0.61
5.3 Nb Informal documents not to Jury	Not addressed to the Jury	0	26	1.24	2.84

C.2 Additional evidence of connections being the factor

Appendix C.2: Documents and parisianism

Control group = All considered as non-treated						
	Mean			Mean		
	(C.2.1)	(C.2.2)	(C.2.3)	(C.2.4)	(C.2.5)	(C.2.6)
	LG	Control	Diff=0	LG	Control	Diff=0
	(Paris U)			(not Paris U)		
<u>Difference in:</u>	Group	Group	(p-value)	Group	Group	(p-value)
Nb Letters of support from Paris	3.63	1.80	0.01***	1.69	2.12	0.54
Nb of Supporters from Paris	1.59	0.71	0.00***	0.80	0.85	0.86
Indirect connections via supporters	57.01	29.09	0.01**	36.31	32.77	0.76
Indirect connections via supporters (excl. letters)	32.73	15.78	0.01**	19.49	18.11	0.85
Nb Informal documents ("Dear" Letters)	2.78	1.24	0.00***	1.08	1.52	0.33
Nb Informal documents to Cassin	0.19	0.19	0.98	0.12	0.20	0.34
Nb Informal documents to Jury	0.27	0.21	0.49	0.16	0.23	0.42
Nb Informal documents not to Jury	2.51	1.02	0.00***	0.92	1.29	0.39

Control group = All non law graduates						
	Mean			Mean		
	LG	Control	Diff=0	LG	Control	Diff=0
	(Paris U)			(not Paris U)		
<u>Difference in:</u>	Group	Group	(p-value)	Group	Group	(p-value)
Nb Letters of support from Paris	3.63	1.82	0.01**	1.69	1.82	0.85
Nb of Supporters from Paris	1.59	0.70	0.00***	0.80	0.70	0.65
Indirect connections via supporters	57.01	27.82	0.01***	36.31	27.82	0.46
Indirect connections via supporters (excl. letters)	32.73	15.13	0.01***	19.48	15.13	0.53
Nb Informal documents ("Dear" Letters)	2.78	1.26	0.00***	1.08	1.26	0.63
Nb Informal documents to Cassin	0.19	0.20	0.86	0.12	0.20	0.35
Nb Informal documents to Jury	0.27	0.22	0.58	0.16	0.22	0.49
Nb Informal documents not to Jury	2.51	1.04	0.00***	0.92	1.04	0.73

Upper panel provides estimates on the whole sample. Bottom panel provides estimates of one subgroup of law graduates compared to others. Column C.2.1 presents the averages in various measures of document contents for Law graduates from a Parisian University; Columns C.2.4 displays averages for Law graduates from other universities. Columns C.2.2 and C.2.5 show the averages for control groups, Columns C.2.3 and C.2.6 the difference between averages of the treated group and control group. Robust t-statistics in parentheses: *** p<0.01, ** p<0.05, * p<0.1

C.3 Advantage not explained by different legal skills

C.3.1 Difference in the treatment of information between CDLs and the Jury - A Law graduate effect?

Appendix C.3.1: Difference in the treatment of information between CDLs and the Jury						
Dep Variable	(C.3.1.1)	(C.3.1.2)	(C.3.1.3)	(C.3.1.4)	(C.3.1.5)	(C.3.1.6)
Sample	<i>Acquit_{i,c}</i>	<i>Acquit_{i,c}</i>	<i>Acquit_{i,c}</i>	<i>Acquit_{i,c}</i>	<i>Acquit_{i,c}</i>	<i>Acquit_{i,c}</i>
	All	All	All	All	All	All
Civil Res X Jury	0.141*** (3.534)	0.202*** (5.104)				0.148*** (3.815)
LG X Civil Res X Jury	0.0652 (0.772)	0.0636 (0.753)				0.0650 (0.787)
Military Res X Jury	0.260*** (3.629)		0.282*** (4.091)			0.243*** (3.463)
LG X Military Res X Jury	0.0908 (0.555)		0.0553 (0.358)			0.0628 (0.390)
Arrested EF X Jury	0.205* (1.943)			0.223** (2.156)		0.203** (2.045)
LG X Arrested EF X Jury	0.0935 (0.361)			0.117 (0.401)		0.144 (0.524)
Mayor EF X Jury	-0.0522 (-1.261)				-0.0239 (-0.487)	-0.00342 (-0.0726)
LG X Mayor EF X Jury	-0.0360 (-0.385)				-0.0380 (-0.370)	-0.0216 (-0.225)
Constant	0.0630** (2.522)	0.0529 (0.547)	-0.0612 (-0.655)	0.0971 (1.055)	0.0978 (1.068)	-0.0630 (-0.647)
Controls		Yes	Yes	Yes	Yes	Yes
Observations	798	798	798	798	798	798
Adjusted R-squared	0.273	0.137	0.226	0.072	0.065	0.284

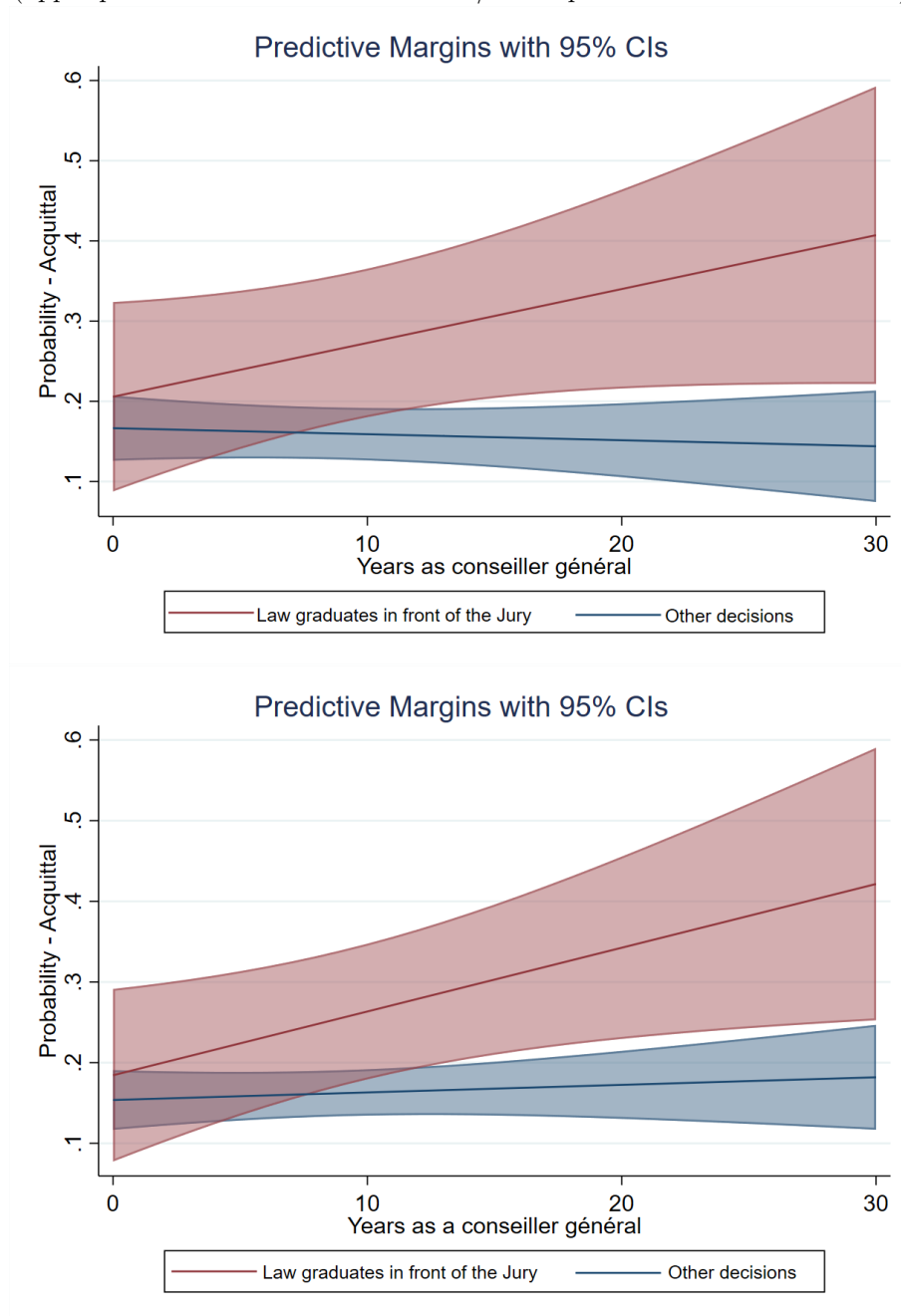
Columns C.3.1 to C.3.6 use the whole set of decisions (both CDL and Jury). They interact a dummy variable for the Jury to variables assessing the information in the dossier and add a triple-interaction of this first term with a law graduate dummy variable to assess the specific reaction of the Jury to this type of information for law graduates. Individual controls include: Age, Jewishness, Journalist, Mayor, Special Role in the Assembly, Mp of an occupied territory. Each individual control is also interacted with the Jury dummy variable. Robust t-statistics in parentheses: *** p<0.01, ** p<0.05, * p<0.1

C.3.2 Is the development of legal skills correlating with the bias ?

Appendix C.3.2: Political career and bias		
	(C.3.2.1)	(C.3.2.2)
	Diff-in-diff	Diff-in-diff
Dependent variable	$Acquit_{i,c}$	$Acquit_{i,c}$
Jury	0.158*** (5.344)	0.185 (1.610)
LG	-0.0174 (-0.439)	-0.0237 (-0.579)
LG X Jury	0.0392 (0.604)	0.0307 (0.511)
LG X Jury X CG	0.00747* (1.818)	0.00697* (1.835)
LG X CG	-0.00115 (-0.406)	-0.000988 (-0.362)
Constant	0.0938*** (4.310)	-0.0586 (-0.596)
Full controls		Yes
Observations	798	798
Adjusted R-squared	0.040	0.284

Table C.3.2 adds the interaction of the length of the political career with our baseline estimates of the advantage of law graduates in front of the Jury to estimate how renouncing to the development of legal skills affect this baseline effect. Column C.3.2.2 adds full individual control variables. Individual controls include: Age, Jewishness, Journalist, Mayor, Special Role in the Assembly, Mp of an occupied territory. Each individual control is also interacted with the Jury dummy variable. Robust t-statistics in parentheses:*** p<0.01, ** p<0.05, * p<0.1

Appendix C.3.2- Illustrative figure : Marginal effect interaction measure of bias with the length of the political career
(upper panel: without control variables / lower panel: with control variables)



C.4 Correlation Matrix - Measures of indirect connections

Appendix C.4: Correlation matrix - Measures of indirect connections

	1. Nb Letters of support from Paris	2. Nb of Supporters from Paris	3. Indirect connections via supporters	4. Indirect connections via supporters (excl. letters)	5. Nb Informal documents ("Dear" Letters)	5. Nb Informal documents ("Dear" Letters)
1. Nb Letters of support from Paris	\					
2. Nb of Supporters from Paris	0.82	\				
3. Indirect connections via supporters	0.55	0.34	\			
4. Indirect connections via supporters (excl. letters)	0.52	0.32	0.99	\		
5. Nb Informal documents ("Dear" Letters)	0.55	0.52	0.37	0.35	\	
5. Nb Informal documents ("Dear" Letters)	0.54	0.52	0.37	0.35	0.98	\

D Descriptive statistics - Different datasets

D.1 Description of Defendants characteristics

Appendix D.1: Description - Defendants' characteristics				
Variable	Mean	s.d	Min	Max
Law graduates	0.28	0.45	0	1
Dependent variable				
Acquittal in front of Jury	0.25	0.43	0	1
Acquittal in front of CDL	0.10	0.30	0	1
Politics and political mandates				
Senator	0.35	0.48	0	1
Rightwing	0.51	0.50	0	1
Center	0.21	0.41	0	1
Mayor	0.47	0.50	0	1
Pres/Vice-Pres or Sec Assembly	0.08	0.27	0	1
MPs elected in Paris	0.05	0.22	0	1
MPs of an occupied department	0.52	0.50	0	1
Dynastic politicians	0.16	0.37	0	1
War experience				
WWI Veteran	0.51	0.50	0	1
WWII fighter	0.06	0.24	0	1
Networks, clubs and religion				
Free Mason	0.04	0.19	0	1
Jewish MPs	0.02	0.12	0	1
Labour unions	0.08	0.27	0	1
Agricultural organizations	0.10	0.29	0	1
Légion d'Honneur	0.38	0.48	0	1
War Medal	0.38	0.49	0	1
Veterans club	0.05	0.21	0	1
Occupation				
Civil Servant	0.06	0.24	0	1
Workers	0.07	0.26	0	1
Journalist	0.12	0.33	0	1
Informational cues				
Excluded by his party	0.27	0.44	0	1
Signed Bergery motion	0.13	0.33	0	1
Mayor under "Etat Fr"	0.27	0.45	0	1
Arrested by Etat Fr	0.06	0.23	0	1
Militarian resistance	0.20	0.40	0	1
Civilian resistance	0.58	0.49	0	1
Continuous variables				
Age	60.25	10.43	34	84
National Mandate	11.19	8.07	1	38
Conseiller Général	8.63	10.67	0	42
Study Years	3.42	3.14	0	8

D.2 Mechanisms: documents dataset

Appendix D.2: Summary statistics - Documents dataset					
Documents	Total	Min per dossier	Max per dossier	Mean per Dossier	s.d
All type of document	17589	1	170	40.62	26.9
Produced by					
Jury	5882	0	50	13.58	6.13
Defendant	2061	0	62	4.76	6.27
Administrations	3335	0	48	7.70	6.00
Politicians	802	0	26	1.85	3.26
Resistant Organizations	2176	0	45	5.03	5.6
Journal Officiel	441	0	3	1.01	0.38
Press	251	0	24	0.58	1.96
Private	1449	0	70	3.34	7.28
Military	181	0	12	0.42	1.34
Intelligence Services	162	0	9	0.37	0.96
Lawyer	115	0	10	0.27	1.03
Court	131	0	6	0.30	0.96
Labor Unions	38	0	4	0.09	0.39
Veterans Association	41	0	7	0.09	0.56
Collaborationist administration	466	0	19	1.07	2.39
Others	58	0	5	0.12	0.54
Type of documents					
Letters of support	3385	0	90	7.54	12.09