

Connections During Democratic Transitions: Insights from the Political Purge in Post-WWII France*

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Abstract

We examine how connections shaped transitional justice during France's post-WWII democratic transition. Parliamentarians who had supported the Vichy regime faced a two-stage purge process involving two courts. Using a difference-in-differences strategy, we find that Law graduates – an influential group with ties to one of the courts – had a 10 to 14 percentage point higher acquittal rate. We analyze 17,589 documents in individual defendants' files to explain this difference. According to this analysis, indirect connections – connections through third parties – enabled transmission of information to the judges, highlighting how connected elite groups can navigate transitions despite institutional safeguards.

Keywords: Connections, Elite persistence, Purges, Political transitions, France, Vichy regime

JEL Codes: D73, K40, N44, P48

1 Introduction

Connections matter in many social contexts. They pay off in the labor market (Bayer et al., 2008), in schools (Fletcher et al., 2020), and in academia (Zinovyeva and Bagues, 2015). In politics, connections facilitate log-rolling (Cohen and Malloy, 2014) and can also be financially profitable (Fisman, 2001; Faccio, 2006; Acemoglu et al., 2016; Nian and Wang, 2023). However, connections can serve different purposes with varying welfare implications. For example, lobbyists use connections not only to provide special interests access to politicians but also to supply relevant information (Blanes i Vidal et al., 2012; Bertrand et al., 2014; Awad, 2020). The former use of connections is likely detrimental to social welfare, whereas the latter can be welfare-enhancing. Whereas previous research investigates the role of connections in stable political environments, this paper examines how connections can assist connected politicians during political transitions from autocracy to democracy and, therefore, contribute to elite persistence.

The paper makes two main points. First, it rigorously shows that connections can affect the outcome of transitional justice during democratic transitions. Second, it establishes how connections operate during democratic transitions. We propose a new distinction between direct connections – defined as a personal connection with those responsible for making a decision about you – and indirect connections – defined as a connection through a third party who knows the decision-makers. While both types of connections can theoretically convey decision-relevant information, indirect connections can be a particularly effective screening device because the reputation of a third party is at stake.

To make these points, we study the political purge that took place in France after World War II.¹ The liberation of France marked an abrupt transition from the Vichy regime, a dictatorship that had cooperated with Nazi Germany, back to a democratic regime, the Fourth Republic. To reinstate democracy, the post-war authorities had to purge politicians who had collaborated with the Vichy regime and determine who would be allowed to continue a political career.

Demonstrating who can leverage their connections and how is, in general, challenging because connections are not normally directly observed. However, three features of the purge in post-war France allow us to investigate the role of connections systematically and to make a distinction between direct and indirect connections. The first feature is that there was a

¹Technically, this is a case of “lustration.” Lustration is a procedure whereby a legal body examines individuals’ actions to prevent those compromised by a previous regime from holding certain categories 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 typical court proceedings. For simplicity, we refer to this as a “purge” rather than as lustration.

well-identified group of politicians to purge. It consisted of the parliamentarians who had endorsed, in a vote on July 10, 1940, the enabling act that cleared the way for the Vichy regime. Yet, some of these parliamentarians later actively participated 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 later redeemed themselves.

The second feature of the purge is that it followed a structured legal process: 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 *département* 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 that had considered the case first or overrule it. It sometimes did, and some of the politicians who were cleared by the *Jury* but not by the CDL went on to pursue successful political careers, some as ministers or even, like René Coty, as president of the Republic. This two-stage process meant that each case was heard twice: no selection into the second-stage rulings occurred. Moreover, the dossiers on each defendant in the archives of the *Jury* allow us to document direct connections between each defendant and the *Jury* and indirect connections via his supporters.

The third feature of the French post-war purge is that there was a well-defined elite group in French politics whose connections to the *Jury* we can examine. Specifically, both before and after the war, many French politicians were Law graduates (Le Béguet, 2003). Law graduates formed a tightly knit elite group, with connections first established during their studies, in faculties standardized by Napoleon I, further strengthened during their subsequent training, and later maintained through the Bar Association, clubs, and speech contests. Consistent with the evidence that alumni connections can have lasting political consequences (Cohen and Malloy, 2014; Battaglini and Patacchini, 2018; Battaglini et al., 2021), we argue that these connections were instrumental in interceding with the three members of the *Jury*, all of whom were Law graduates themselves with ties to Parisian faculties of Law.

Using those three key features – a target group to purge, a well-defined elite with connections to the *Jury*, and a two-stage legal process – we construct our econometric strategy. Our approach is grounded in an original theoretical framework that describes how a court seeking to minimize judgment errors can leverage information transmitted through the direct and indirect connections of defendants. Empirically, we proceed in two steps. In the first step, we build upon the empirical literature on connections. In most cases, this literature does not directly observe ties between individuals and infers them from the context. For example,

Jia et al. (2015), Fisman et al. (2020), and Fisman et al. (2018) argue that individuals from the same hometown or who attended the same university or college share ties and use that characteristic to measure connections empirically.² We use a similar approach and argue that, in the French post-war context, defendants who shared a Law background – Law graduates – belonged to a special group with a high probability of having connections to the *Jury*. Focusing on this group, we indirectly measure connections specific to the *Jury*. With this measure of connections, we can adopt the method developed by Anwar and Fang (2006) and Alesina and La Ferrara (2014) to study racial bias in US courts to identify the effect of connections causally. The idea is to use, in a difference-in-differences design, the decision of the lower courts (the CDLs) as a counterfactual for the decisions made by the upper court (the *Jury*) that was more likely connected with Law graduates. This difference-in-differences estimate of the difference in the acquittal rates between the two courts determines whether connections enabled the connected group of Law graduates to avoid the ban more than other unconnected defendants. We find that they did: the difference in the acquittal rate between Law graduates and other defendants was 10 to 14 percentage points higher in front of the *Jury* than in front of the CDLs.

In the second step, we document how connections drove this difference using an inventory of the 17,589 documents contained in the dossiers of the defendants in the *Jury*'s archives. This allows us to measure different types of connections between the defendants and the *Jury* directly and to operationalize the distinction between direct personal connections and indirect connections operating via a third party. We find that both types of connections were more common among Law graduates than among other defendants but that indirect connections mattered the most. Law graduates benefited from being connected to third-party supporters with close ties to the *Jury*. In contrast, we find little evidence in the archives of the *Jury* that direct connections to the *Jury* helped defendants. In short, the higher acquittal probability of Law graduates in front of the *Jury* primarily emerged because they benefited from connections that could be leveraged to advocate credibly on their behalf. This pattern is inconsistent with legal skills being the source of the differential acquittal probability of Law graduates. However, the difference in acquittal rates between Law graduates and other defendants could be attributed to in-group favoritism in addition to our theoretical mechanism. For example, members of the in-group of Law graduates may support one another in an effort to protect their collective reputation. While the archival data do not allow us to clearly distinguish between such in-group favoritism and more direct connections between Law graduates, they do highlight the importance of indirect connections as a means through which decision-

²This does, of course, not mean that *all* individuals from the same town or the same university share a tie, but the probability that they do is higher than for other individuals.

relevant information is transmitted and collective reputation is preserved.

Our results speak to three strands of literature beside the literature on connections. First, we contribute to the law and economics literature on bias in sentencing (Voeten, 2008; Shayo and Zussman, 2011; Alesina and La Ferrara, 2014; Park, 2017; Geerling et al., 2018; Feigenberg and Miller, 2021; Ahrsjö et al., 2024). This literature primarily focuses on biases in sentencing that arise from identity or race, due to taste-based discrimination. In contrast, we argue that connections between (some) defendants and a court can result in differential sentencing patterns – not because of taste-based discrimination or other forms of bias, but due to the information transmission these connections facilitate. In our theoretical model, connections give defendants an advantage that unconnected defendants lack when decision-relevant information about case circumstances can be credibly communicated to the court. Judges may be willing to consider such information if they believe it will reduce the risk of sentencing mistakes. As such, connections can improve the efficiency of court decisions. However, because only connected defendants benefit from this type of information transmission, this is contrary to the principle of equality before the law. Consequently, an efficiency-equality trade-off emerges.

Second, we contribute to the literature on transitional justice during the shift from autocracy to democracy (e.g., Nalepa, 2008, 2010; Ang and Nalepa, 2019). Like purges in autocracies³, the process of transitional justice serves to protect a new regime from coups and other challenges from within. However, since the rule of law constrains purges after a democratization – requiring that they respect a legal framework (Capoccia and Pop-Eleches, 2020) – it is tempting to conjecture that there is little room for connections to influence outcomes in this context (Huang et al., 2024).⁴ We show, however, that this conjecture is incorrect: connections play an important role in understanding the efficiency-equality trade-off inherent in political purges during democratic transitions.

Third, we contribute to the literature on elite persistence after democratic transitions (O’Donnell and Schmitter, 2013). We document a new mechanism – connections – explaining why members of a compromised elite persist and survive democratic transitions. Some of these defendants, such as René Coty, who became President of the Republic in 1953, played leading roles in the Fourth Republic. This complements existing explanations of elite persistence (e.g., Higley and Burton, 1989; Acemoglu and Robinson, 2006; Martinez-Bravo, 2014; Martinez-

³E.g., Svobik (2009), Shih et al. (2012), Bueno de Mesquita and Smith (2017), Sudduth (2017), Montagnes and Wolton (2019), or Goldring and Matthews (2021).

⁴This assumption may be further reinforced by the observation that democratization tends to empower non-elites, which, as argued by Acemoglu and Wolitzky (2021), leads to greater equality before the law.

Bravo et al., 2017; González et al., 2021).⁵

The rest of the paper is structured as follows. Section 2 presents the historical background of the purge. Section 3 summarizes our theoretical argument. Section 4 introduces the data, the difference-in-differences strategy, and reports the baseline results. Section 5 draws on the dossiers of the defendants in the archives of the *Jury* to measure direct and indirect connections in a novel way and shows that it was primarily indirect connections that mattered. Section 6 concludes.

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

2.1 The purge

As large parts of French society had been compromised by the Vichy regime, various groups, from civil servants and politicians to writers, journalists, and intellectuals, had to be purged at the end of WWII.⁶ On 21 April 1944, the Provisional Government of the French Republic (GPRF) published an order rendering ineligible for public office individuals who had compromised themselves. The order explicitly singled out parliamentarians who had voted for the enabling act that granted full power to Marshall Pétain and marked the end of the Third Republic (Paxton, 1972).

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., 2023). By default, the order of 21 April 1944 banned the latter from participating in post-war politics. 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 that was followed to waive the ban for all the remaining (banned) parliamentarians (hereafter, the parliamentarians). Each

⁵The sources of elite persistence within *given* institutional structures are reasonably well-established. The 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).

⁶For historical information on the purges, see Wieviorka (2001); Baruch (2003); Elster (2006).

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 *Comités départementaux de libération* (CDLs) which operated in each *département*, France’s main administrative unit. The CDLs emanated from the Resistance. Their exact composition is unknown but likely reflected the balance of power of local resistance groups, and the CDLs were, therefore, heterogeneous in their makeup (Albertelli et al., 2019). One of their tasks was to assess the dossiers filed by parliamentarians who wanted their ban from holding public office waived.

At the second stage, each case was assessed by the *Jury d’honneur*, a national court explicitly established to purge former supporters of the Vichy regime from politics, including all parliamentarians who had voted in favor of the enabling act. It could overrule the decisions of the CDLs. On average, the *Jury* decided on a case seven to eight months after the CDLs. The *Jury* had three members. René Cassin, vice-President of the *Conseil d’Etat*, the highest administrative court in France, presided over the *Jury*. He was assisted by Maxime Blocq-Mascart, representing the *Conseil National de la Résistance*, the body that coordinated the French Resistance movement, and by André Postel-Vinay, representing the *Ordre de la Libération*, the Order awarded to outstanding participants of the liberation of France. The *Jury* used one criterion to acquit the parliamentarians:⁷ “an active (and direct) participation in resistance activities before November 1942.” The *Jury* organized its investigations around this objective of assessing whether a defendant had participated in the Resistance early in the war. Although none of the orders establishing the *Jury* defined how decisions were to be taken, decisions were collegiate and announced as a consensus decision.

All the members of the *Jury* were Law graduates. Its chairman, René Cassin, was a lawyer 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, p. 383) refers to him as “the jurist of Free France”. André Postel-Vinay held a bachelor degree in Law and had studied at *Ecole libre des sciences politiques* (Sciences-Po) in Paris, where students study a blend of legal and social sciences. The school, created in 1871 by a Professor of Law, Emile Boutmy, was, 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

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

⁸Emile Boutmy, for example, published a study on Constitutional Law (<https://gallica.bnf.fr/ark:/12148/bpt6k235741/f1.item.texteImage> (accessed 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> (accessed February 25, 2021)).

to each case and also by administrative staff. That staff mainly came from the *Conseil d'Etat* – the highest administrative court in France – and the *Jury* was located in its building. In short, the *Jury's* members and its staff had a Law background and belonged to one of the most influential groups in French politics at the time, Law graduates.

2.2 The “Lawyers’ Republic”

Law graduates formed an influential social group for two reasons. First, its structure, with the Bar association, its clubs and speech contests, meant that Law graduates bonded during their training and later on cultivated these connections within the Bar association.

Second, Law graduates were historically tightly-linked to French politics. For example, many lawyers studied Law in combination with political science. In addition, 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 Presidents of the Council of Ministers, 13 of whom were lawyers⁹ (Le Béguec, 2003). During the 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). Moreover, from 1946 to 1958, two of the four Presidents of the National Assembly had a Law degree and the first President of the Council of the Republic, the Upper Chamber under the Fourth Republic, Auguste Champetier de Ribes, was a Law graduate.

The training and curriculum of Law graduates were standardized. Law schools, renamed Law faculties in 1808, had been created by Napoleon under the law of 13 March 1804 on the same model throughout France (Gleizal, 1980). The introduction in 1855 of the *agrégation en droit*, a national competitive exam to select and appoint Law professors, further homogenized the qualifications of Law professors. In that respect, Law faculties stood out from other disciplines in terms of both quality and homogeneity (Gleizal, 1980). R. Blanchard, a professor of geography at the University of Grenoble, is cited by (Gleizal, 1980, p. 151) as having declared in 1906 that “Law professors were, as a whole, the best of the university. [...] [They] emerged from a difficult competitive examination, the *agrégation*, which ensured both homogeneity and value.”

In short, in 1945, Law graduates were a powerful and homogeneous elite group with a well-established network of internal connections who assumed a prominent role in French politics.

⁹This is equivalent to the position of Prime Minister.

We conjecture that the capacity of Law graduates to survive the regime change after the war was related to their connections to the *Jury*, whose members, as noted, also had a Law background.

2.3 Connections and elite persistence after WWII

Besides the parliamentarians who voted for the enabling act, France purged large segments of its society from collaborators with Germany: the Church, writers' societies, unions, the police, the military, and the judiciary. Despite the breadth of these purges, Novick (1985, p. 225) notes that “all these different purges did not – as some have hoped for – transform French society.” In 1945, to explain this failed attempt at purging French society, Nobel laureate in literature Albert Camus (cited by Novick, 1985, p. 255) highlighted in the newspaper *Combat* the interventions of powerful figures: “politics in all its blindness got involved.” Rouquet and Virgili (2018, p. 402) also note that some suspected collaborators “benefited from the protection of pre-war networks – such as the business world.” Rouquet and Virgili (2018) highlight the example of Eugène Schueller, founder of the personal care company L'Oréal. Eugène Schueller was known for his extreme right-wing opinions in the late thirties and his support of the Vichy regime. After the war, he nevertheless benefited from many supporting testimonies – including one from François Mitterrand, who would later become president of the Republic. Despite being tried, he was never found guilty of collaboration and even received the *Légion d'Honneur* and the *Croix de Guerre*, two of the most prestigious decorations in France. Lacroix-Riz (2019) argues that the case of Eugène Schueller is not an isolated case but rather illustrates a pattern. As a reason for the *non-épuration* (non-purification) of France, she emphasizes the “constant pressure of the authorities on the police and the judiciary” (p. 397). Chapter 8 of her book is dedicated to the “great saviors”, members of the post-war public administration and government who had been great Resistance figures and who testified in favor of defendants during the various trials that followed the war.

This historiography suggests that connections increased the likelihood of surviving the French purge after the war, allowing certain elements of the compromised elites to persist. The connection mechanism highlighted in our study of the parliamentarians who had supported the establishment of the Vichy regime likely represents a broader phenomenon at work throughout the transitional justice process in post-war France.

In our sample, some of the parliamentarians whose bans were overturned by the *Jury* went on to have successful political careers after the war and some of them became highly influential.

We highlight three examples of this.¹⁰ The first is Guy La Chambre. He served as a deputy from 1928 to 1942 and held several ministerial roles before the war. Although his CDL did not choose to acquit him, the *Jury* did. He was later elected mayor of the provincial town of Saint Malo in 1947 and returned as a deputy in 1951. Most notably, in 1954, he was appointed Minister for Relations with the Associated States, a pivotal role tasked with implementing the Geneva Agreements that marked the end of the First Indochina War. The second example is that of Emmanuel Temple, who was first elected as a deputy in 1936. The CDL that heard his case wanted to ban him, but after his clearance by the *Jury*, he regained his pre-war seat as a deputy. He later became Minister for Veterans in 1951, Minister for Defense in 1955, and, finally, Minister for Justice the same year. The most striking example, however, is that of René Coty. Elected as a deputy in Seine-Inférieure in 1923, Coty later became a senator in the same *département* in 1936. After his acquittal by the *Jury*, he was reelected as deputy and served as Minister of Reconstruction and Town Planning until 1948. In that year, he returned as a senator representing his pre-war constituency. The pinnacle of his political career came in 1953, when he was elected President of the Republic. These three politicians had endorsed the enabling act on July 10, 1940, they were acquitted by the *Jury* after the CDLs had upheld their ban, and they were all Law graduates.

Although it is difficult to demonstrate how the *Jury*'s tendency to overturn the ban on defendants who were Law graduates affected specific post-war policy decisions, it is important to remember that the aftermath of the war was a time of major political change. New leaders had risen through the ranks of the Resistance and were expected to play a dominant role in post-war French politics (Paxton, 1972). The program of the *Conseil national de la Résistance* – a secret manifesto issued in March 1944 outlining the guiding principles of post-war politics – reflected the left-wing orientation of this emerging elite (Albertelli et al., 2019). In contrast, many cleared politicians were relatively conservative and may have served as a counterbalance. For example, the three prominent politicians whose careers were discussed in the previous paragraph all became members of the *Centre National des Indépendants et Paysans*, a party that was founded after the war and which according to Cotillon (2003) gave “a discreet but excellent welcome to former Vichyists.”

¹⁰The examples draw on the biographies of French deputies retrieved from Jolly (1960) (database of French parliamentary biographies), accessible on the website of the Assemblée Nationale, <https://www2.assemblee-nationale.fr/sycomore/recherche>, accessed on January 2, 2025.

3 Theoretical framework

Our theory of connections and transitional justice, which we develop fully in Appendix A and sketched here, emphasizes two channels through which defendants may influence court decisions by providing information about mitigating circumstances. We refer to these two channels as direct and indirect connections, respectively. Direct connections are between a defendant and the judges; indirect connections run through a third party who is familiar with a defendant's case and connected to the judges. Connected defendants can use these two channels to transmit information about mitigating circumstances in ways that unconnected defendants cannot. However, the underlying logic that explains how this happens is fundamentally different in the two cases.

As in Alesina and La Ferrara (2014), our theoretical starting point is that the court's objective is to minimize judgment errors – in the case of the French post-war purge, to acquit only those defendants with sufficient evidence of participation in the Resistance. The judges, therefore, are interested in obtaining information about mitigating circumstances (which can help establish participation in the Resistance). The judges know that defendants and their third-party supporters have a strong incentive to claim that mitigating circumstances exist, whether that is, in fact, the case or not. Consequently, they do not take the information transmitted by defendants and third-party at face value. Formally, in our theory, the information transmitted to the judges is cheap talk, and it is reasonable to suppose that the overlap between the interests of the courts and the defendants is so limited that it is impossible to transmit information credibly; i.e., we can rule out informative perfect Bayesian equilibria of the type considered by Crawford and Sobel (1982).

In order to influence court decisions, defendants need to convince the courts that their claims about mitigating circumstances can be trusted. Conversely, the courts need an effective way to screen the claims. In our theory, this can be done in two ways.

First, all defendants can, in principle, communicate with the courts, but a defendant with a *direct* connection to a court has an advantage in doing so. We model this by assuming that communication is associated with a fixed cost and that these costs vary across defendants, generally differing from one court to another.¹¹ A direct connection to a court lowers a defendant's cost of communicating with *that* court. 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. Defendants with either very good or very poor connections to a court are unable to influence its decisions. It is defendants with communication

¹¹See Grossman and Helpman (2002) for a related approach to lobbying.

costs in the intermediate range who can leverage their connection to reduce their chances of conviction. The logic is that the cost of communication must be high enough to ensure that the defendant will not claim that there are mitigating circumstances when there are none, and low enough that they will have an incentive to report the circumstances when they are genuinely mitigating. This allows the court to screen the information it receives directly from the defendant effectively. This has the following empirical implication: if the population of defendants contains both directly connected and unconnected defendants, then, on average, the connected defendants are at least as likely as those unconnected to that court to be acquitted *by the court to which they are connected* and strictly more likely if they are not all “too” connected.

Second, a defendant may also have an *indirect* connection to a court through a third party who can submit claims of mitigating circumstances to that court on their behalf. We make three assumptions about the third-party supporters. Firstly, they are familiar with the circumstances related to the case against the defendant they support. Consequently, they can screen the information about mitigating circumstances and establish its credibility. Secondly, unlike the defendants, all third parties are well-connected and can communicate with the court to which they are connected at a low cost. Hence, judges do not assess the credibility of third parties from the cost they face to send information. Thirdly, we assume that the third-party supporters not only care about the welfare of the defendant they support (and want him acquitted), but they also, to varying degrees, put weight on the court’s objective of minimizing judgment errors or, equivalently, would suffer a larger reputational loss if the evidence they provided proved to be wrong. The reason why third-party supporters can influence the courts is, therefore, fundamentally different from direct connections. A claim from a third party who mostly cares about the defendant will not generally affect a court’s decision. Third-party interventions are influential when the third party is sufficiently concerned about the integrity of the legal process or their own reputation, thus having objectives that partly overlap with those of the court. It is the third feature that enables indirect connections to work. When a court understands that a third-party supporter sufficiently shares its objective, it can base its judgment on the information received through that channel and benefit from the information screening done by the third-party. The empirical prediction that flows from this is that defendants supported by third parties trusted by a court are more likely to be acquitted than those who are not.

While the two connection channels – in particular, the indirect channel – are novel to the law and economics literature, they have a parallel in Zinovyeva and Bagues (2015)’s study of academic promotions in Spain. In their theoretical framework, connections may impact decisions in two ways. On the one hand, they potentially generate biases; on the other hand,

they improve the quality of information. In our framework, the courts want to minimize judgment errors. Consequently, the judges are willing to accept information from third-party supporters with a stake in the integrity of the legal process because doing so improves the quality of their legal decisions. In contrast, they tend to discard the information channeled through *strong* direct connections because it is tainted with the self-serving bias of the defendant. However, if the direct connection is not too strong, they may take the information into account because it will also, in that case, improve the quality of their legal judgment.

4 Connections: A difference-in-differences approach

The objective of this section is to show causal evidence that connections affected the outcome of the post-war purge of French politicians. To this end, we associate connections with the group of Law graduates, based on the assumption that this group of defendants was more likely than others to have a connection to the *Jury* through which they could convey information about mitigating circumstances.¹²

To estimate the causal effect of connections – either direct or indirect – we use a difference-in-differences strategy, described in Section 4.1, which exploits the fact that the defendants were tried twice by two different courts – the *Jury* and a CDL. This allows us to isolate the effect of connections between Law graduates and the *Jury* on the probability of acquittal from statistical discrimination and court-specific differences. Section 4.2 reports the main results. Section 4.3 discusses how connections contributed to elite persistence. Section 4.4 evaluates and rules out other explanations.

4.1 Empirical approach

Out of the 569 parliamentarians who had voted in favor of the enabling act, 93 had died during the war, 51 well-known figures of the Resistance had been acquitted by prefects prior to the legal process began, 8 were facing criminal charges for collaboration with the Vichy regime, 8 were from overseas territories and not judged by a CDL, and in the dossiers of another 10 defendants, the decision of either the CDL or the *Jury* is missing. This leaves 399 parliamentarians who were tried by both courts and for whom the two judgments are preserved.

¹²This approach has been used to assess the value of connections in politics (Jia et al., 2015; Fisman et al., 2018, 2020) or academia (Zinovyeva and Bagues, 2015), how connections foster the diffusion of social media networks (Enikolopov et al., 2020), the value of connections in the lobbying industry (Blanes i Vidal et al., 2012), or CEO’s political connections (Bertrand et al., 2018).

To implement the first step of our empirical strategy, we combine information on the 798 decisions reached by the CDLs and the *Jury*, respectively, with biographical information on the 399 parliamentarians in our dataset.¹³ Table 1 cross tabulates the decisions of the *Jury* and the CDLs. The two agreed on 81% of the 399 defendants. Our tests leverage the cases where the CDLs and the *Jury* disagreed – the entries in boldface. These were mostly cases where the *Jury* acquitted defendants against the judgment of a CDL – 17% of all defendants – although in a few cases, the *Jury* banned defendants whom a CDL wanted to acquit – 2% of the defendants.

Table 1: Cross tabulation of the two courts’ decisions

	CDL for acquittal	CDL for ban	Total
<i>Jury</i> for acquittal	32	68	100
<i>Jury</i> for ban	8	291	299
Total	40	359	399

These data are the primary input to our difference-in-differences 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 the lower courts – the CDLs – and $c = U$ indexes the upper court – 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 if the judgment was reached by a CDL. We estimate the following linear probability model:

$$Acquit_{i,c} = \beta_0 + \beta_1 Jury_c \times LG_i + \beta_2 Jury_c + \beta_3 LG_i + \beta_4 Jury_c \times X_i + \beta_5 X_i + \varepsilon_{i,c}, \quad (1)$$

where $\beta_0, \beta_1, \beta_2$, and β_3 are (scalar) coefficients, and β_4 and β_5 are vectors of coefficients, and $\varepsilon_{i,c}$ is an error term. Equation (1) is estimated with ordinary least squares. The standard errors are clustered at the *département* level. Coefficient β_1 on the interaction term $LG_i \times Jury_c$ is the coefficient of interest. It captures 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 defendant characteristics and unobserved court-specific characteristics. In other words, β_1 measures how much more inclined than the CDLs the *Jury* was to acquit Law graduates specifically.¹⁴ It isolates the differential experienced of Law graduates before

¹³Most of the data are obtained from Wieviorka (2001) and Lacroix et al. (2023) and are consolidated with information from the still-classified individual dossiers of the defendants from the archive of the *Jury* (References AL//5295 to AL//5334). We sincerely thank Olivier Wieviorka for sharing his data with us.

¹⁴Our approach 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 that literature, this effect is referred to as “taste-based discrimination.” In our context, the source of the effect is connections rather than taste-based discrimination.

the *Jury* thanks to their connections.

Courts may, in different ways, base their decisions for acquittal on observable characteristics of the defendants. Some of these may overlap or correlate with being a Law graduate. To ensure that the estimate of β_1 does not capture other group characteristics correlated with the Law graduate dummy variable and with higher acquittal rates before the *Jury*, we control for a vector of individual (or group) characteristics (X_i) and their interactions with the *Jury* dummy variable ($Jury_c \times X_i$) in Equation (1). 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. The underlying analysis is reported in Appendix Table C.6. The variables that are added as controls are: age, being Jewish, being a journalist, being a President/Vice President or Secretary of the National Assembly, and a dummy equal to one if the defendant's constituency was in the part of France that Germany initially occupied. We note that the difference in acquittal rates before the CDLs and the *Jury* is not correlated with length of academic studies, length of the political career, or political orientation (see the bottom of Appendix Table C.6).

Importantly, we also include *département* of election fixed effects interacted with the *Jury* dummy variable. This isolates the effect of being a Law graduate (with access to a particular set of connections to the *Jury*) from geographical factors that could have affected the probability of acquittal by the *Jury* such as better access to the Resistance during the war, better knowledge of the judgment process, or access to relevant networks around the *Jury* due to geographic proximity to Paris, where the *Jury* sat. Critically, it also controls for unobserved differences in the composition and operation of the *département*-level courts (CDLs) faced by each defendant. In short, our difference-in-differences estimates, therefore, use within-defendant variation in acquittal across the two courts within the 86 *départements* in our sample.

We also include in X_i information on participation in the Resistance or of collaboration with the Vichy regime obtained from the dossiers of the defendants and coded by Wiewiorka (2001). Table 2 reports mean comparisons for the variables we use to measure objective facts about Resistance participation or collaboration with the Vichy regime for Law graduates versus other defendants. We see that there were no average differences. Controlling for these factors and their interactions with the *Jury* dummy variable ensures that our estimate of connections 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 related to their participation in the Resistance to the *Jury*.

Table 2: Evidence of participation in the Resistance in the defendants’ dossiers: Law graduates versus other defendants

Variable	Law graduates	Others	p-value (t-test)
Civilian Resistance	0.60	0.57	0.64
Military Resistance	0.18	0.20	0.67
Arrested by Vichy	0.04	0.06	0.31
Mayor under Vichy	0.21	0.29	0.10

Note: The dummy variables *civilian Resistance* and *military Resistance* are coded one if there was evidence of participation in the civilian or military Resistance in the dossier of the *Jury*, respectively, *arrested by Vichy* is a dummy variable equal to one if the defendant had been arrested by the Vichy regime, and *mayor under Vichy* is a dummy variable equal to one if the defendant had been a mayor under the Vichy regime. The classifications are based on the coding in Wieviorka (2001).

Unlike most studies of court decisions, we face no “selection-into-encounters” problem (Knox et al., 2020) 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*) can, therefore, not correlate with the defendants’ characteristics. Likewise no selection into the purge process could occur because all parliamentarians who had voted in favor of the enabling act were automatically selected.¹⁵

4.2 Difference-in-differences results

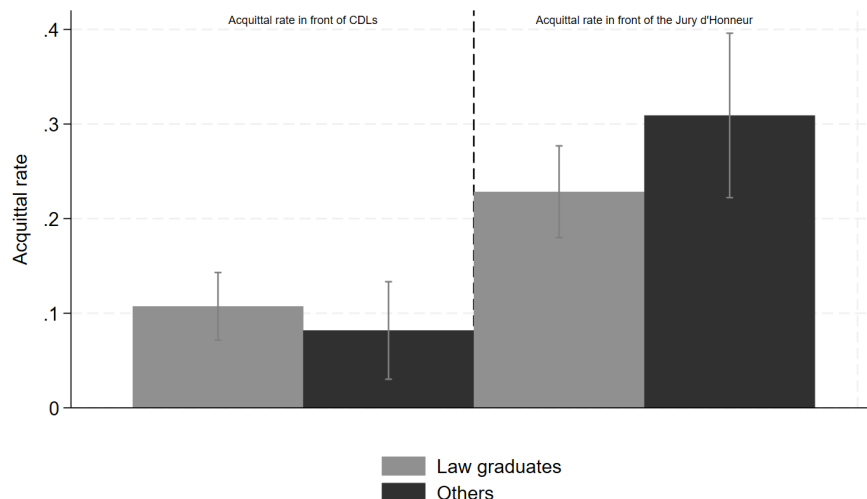
Figure 1 illustrates our approach. It compares the average acquittal rates of Law graduates and other defendants before the two courts. Before the CDLs, the acquittal rate was 8.2% for Law graduates and 10.7% for other defendants. This difference is not statistically significant, suggesting that the 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. The difference-in-differences estimate of this effect is $(30.9 - 22.8) - (8.2 - 10.7) = 10.6$ percentage points. This difference is statistically significant, suggesting that the *Jury* in contrast to the CDLs did treat the better connected Law graduates more leniently than other defendants.¹⁶

Table 3 reports the estimates of Equation (1). Column 3.1 shows a parsimonious specification with the Law graduate and *Jury* dummy variables and their interaction only. The

¹⁵As noted above there is, however, some attrition in our data, but it is random. Among the cases where information on the judgments is missing, the proportion of Law graduates is not statistically different from those of the defendants with information on both judgments (32% versus 28%). Moreover, Appendix Table C.1 shows that Law graduates represented 29% of the parliamentarians who voted for the enabling act in 1940. This is similar to the proportion of Law graduates in our sample of defendants (27.5%), but significantly lower than the proportion of acquitted Law graduate defendants (34%).

¹⁶Appendix Figure B.1 zooms in on the cases in which the two courts disagreed. The results are similar.

Figure 1: The two courts' decisions - Law graduates versus others defendants



difference-in-differences estimate of β_1 is significant at the five-percent level. The point estimate implies that the difference in acquittal rates between the connected group of Law graduates and other defendants is 10.6 percentage points higher before the *Jury* than before the CDLs. The estimates of β_1 are almost identical when we add controls for evidence of either collaboration with the Vichy regime or of participation in the Resistance, and the interaction between these and the *Jury* dummy variable (Column 3.2) and individual characteristics and their interaction with the *Jury* dummy variable (Column 3.3).¹⁷ In these specifications, the coefficient attached to the *Jury* dummy variable itself becomes insignificant. This suggests that the difference in acquittal rates between the CDLs and the *Jury* – observed from the raw data reported in Table 1 – can be explained by the evidence in the files of the defendants. When we add *département* fixed effects and their interaction with the *Jury* dummy variable (Column 3.4), which among other things control for unobserved CDL characteristics, the point estimate of β_1 increases to 14 percentage points and is significant at the one percent level. Lastly, in Column 3.5, we add occupation fixed effects to net out the effect of other professional networks from our estimate of the impact of connections among

¹⁷In a few cases, the interaction between a control variable and the *Jury* dummy variable is statistically significant. This suggests that, beyond the Law graduate effect, defendants with some other characteristics were treated differently by the two courts. An important example is mayors. Their acquittal probability was 7 to 10 percentage points higher before the CDLs than before the *Jury*. This may reflect, in line with our theory, that mayors could use their connections to the local courts to gain an advantage, but since we do not have access to the files of the CDLs, we cannot substantiate this conjecture further.

Law graduates.¹⁸ The results are in line with previous estimates. In terms of magnitude, recall that the *Jury's* average acquittal rate is 25.5%. The difference-in-differences estimates indicate that the acquittal rate for Law graduates before the *Jury* was 10 to 14 percentage points higher than that for other defendants. This represents an increase of between 39% and 55% relative to the average acquittal rate.

For the rest of this section, we treat the specification in Column 3.4 with *département* fixed effects as our baseline and we interpret the results in Table 3 as causal evidence of the role of connections before the *Jury*. To substantiate this causal claim, we use the approach developed by Altonji et al. (2005) and Oster (2019) to evaluate how important selection on unobservables would have to be in comparison to selection on observables to explain the positive estimate of β_1 . The smallest Altonji-Oster δ is equal to 1.46 in Column 3.5 and it is equal to 30.09 in Column 3.4 in our preferred specification with *département* fixed effects. This indicates that selection on unobservables would have to be at least 1.5 times more important than selection on the observables that we include for our estimate to be spurious and 30 times more important if we restrict the variation to be within *départements*. This is unlikely.

¹⁸The categories are: workers (mainly in manufacturing), industrialists, journalists, teachers, and agricultural workers. The omitted category combines professional politicians and independent workers (self-employed), which often overlap.

Table 3: Connections of Law Graduates to the *Jury*: the difference-in-differences estimate

	(3.1)	(3.2)	(3.3)	(3.4)	(3.5)
	Dependent Variable: $Acquit_{i,c}$				
Jury	.121 (.022)	-.0224 (.0243)	.143 (.103)		
LG	-.0254 (.0307)	-.0249 (.03)	-.0284 (.03)	-.0391 (.0333)	-.0232 (.0336)
Jury \times LG	.106 (.0479)	.108 (.0418)	.0996 (.0418)	.14 (.0454)	.12 (.0515)
Observations	798	798	798	790	790
Adjusted R ²	.0404	.271	.281	.292	.296
Altonji-Oster δ		-51.88	3.59	30.09	1.46
<u>Controls:</u>					
Resistance and collaboration	No	Yes	Yes	Yes	Yes
Age and Religion	No	No	Yes	Yes	Yes
Political mandates	No	No	Yes	Yes	Yes
Journalist	No	No	Yes	Yes	Yes
Included controls \times Jury	No	Yes	Yes	Yes	Yes
<i>Département</i> fixed effects	No	No	No	Yes	Yes
<i>Département</i> fixed effects \times Jury	No	No	No	Yes	Yes
Occupation fixed effects	No	No	No	No	Yes
Occupation fixed effects \times Jury	No	No	No	No	Yes

Note: This table presents estimates of variations on Equation (1). Standard errors are clustered at the *département* level (in parentheses). *Jury* is a dummy variable equal to one if the judgment was before the *Jury* and zero otherwise. LG is a dummy variable equal to one if the defendant was a Law graduate and zero otherwise. The controls are: Age and religion (Age, Jewishness); Journalist; Political mandates (Mayor, Special Role in the Assembly, parliamentarian of an occupied territory); Resistance and collaboration (Civilian resistance, Military resistance, Arrested by the Vichy regime, Mayor under *Vichy*). Each control is also interacted with the *Jury* dummy variable (coefficients shown in Appendix Table C.3). The reduction in the number of observations in Columns 3.4 and 3.5 is due to the fact that within some *départements*, there is no variation in sentencing. The *département* fixed effect is, therefore, perfectly collinear with the outcome in those cases and eight observations are dropped.

We find similar results with Logit and Probit estimators (Appendix Table C.2). Moreover, the difference-in-differences estimates reported in Table 3 are robust to adding control variables one by one (Appendix Table C.4), to controlling for the composition of resistance groups in a *département*, which we interpret as a proxy of the political composition of the CDLs (Appendix Table C.8), and did not emerge because the CDLs disregarded evidence put forward by Law graduates (Appendix Table D.7).

An alternative way to document the role of connections is to estimate the counterfactual probability of being acquitted, given the information about participation in the Resistance. Appendix C.4 explains this approach. We find that the counterfactual estimate aligns closely with the actual acquittal rate for non-Law graduates. In contrast, the counterfactual systematically underestimates the probability of acquittal for Law graduates. Law graduates,

therefore, benefited from factors beyond the evidence of participation in the Resistance reported in their dossiers. This further strengthens our interpretation of the positive estimate of β_1 as evidence of connections.

4.3 Connections and persistence

According to Table 3, Law graduates were 10 to 14 percentage points more likely than other defendants to avoid being barred from continuing their political careers after the war. Was this difference consequential for post-war French politics? Three conditions at least must be met for the answer to be affirmative: (1) acquitted politicians must run for election, (2) voters must elect them, and (3) acquitted politicians must eventually be in positions of influence (e.g. as mayors, in the Assembly, or as ministers). The three cases discussed in Section 2 clearly show that some of the politicians who had their ban overturned by the *Jury* met these conditions. To complement the anecdotal evidence, we study more systematically the post-war political careers of all the defendants. We do this in four ways.

First, we measure whether an acquittal by the *Jury* shaped a defendant's post-war political career. This could not have been the case if acquitted defendants had not run for election, had not been elected, or had only reached low-level positions. Moreover, in 1953, almost all defendants who had been banned by the *Jury* received an amnesty. Accordingly, after 1953, all defendants could continue or resume their political careers if they wanted to. This potentially limited the impact of the *Jury's* decisions. To assess this, we test whether defendants who were cleared during the purge were more likely than those who were not but were pardoned in 1953 to have a successful career after the war. If we find that the answer is no, this would suggest that the decisions of the *Jury* did not have any substantive effect on post-war French politics. However, the analysis reported in Appendix Table E.1 shows that the answer is yes: defendants acquitted by the *Jury* were 12 percentage points more likely than defendants who were later pardoned to become mayor, 22 percentage points more likely to win a seat in the National Assembly or the Senate, and seven percentage points more likely to become a government minister. These defendants also stayed longer in those positions (Appendix Table E.2).

Second, we compare the post-war political careers of Law graduate defendants to the careers of other defendants. Law graduates were about 10 percentage points more likely than other defendants to run in a national legislative election and, as a result, around eight percentage points more likely to become a deputy and three percentage points more likely to hold ministerial posts. In a nutshell, following the purge, Law graduates were more likely to stay in politics and obtain influential positions than other defendants.

Third, some defendants announced their participation in the 1945 elections before the legal process ended. We find that connections played a more important role for these defendants whom the *Jury* knew intended to pursue a political career, i.e., in the cases that really mattered (Appendix Table E.3).

Finally, we measure whether the cleared Law graduates differed from other cleared defendants in terms of their pre-war political careers or social recognition (WWI medals). Appendix Table E.4 reports that they did. In particular, cleared Law graduates were 21 to 25 percentage points more likely to have been awarded a war medal for their services during WWI than other cleared defendants, and they had held pre-war political mandates for, on average, four additional years. Importantly, these differences are evident both when we compare all acquitted Law graduates to other defendants acquitted by the *Jury*, and when we focus specifically on defendants who were not cleared by the CDLs but had their ban overturned by the *Jury*. This suggests that the connections between Law graduates and the *Jury* influenced the type of politicians who were allowed to participate in post-war politics.

4.4 Ruling out alternative explanations

We interpret the results in Table 3 as reflecting the effect of connections between a subset of defendants (Law graduates) and the court with the final say on their case (the *Jury*). However, one might be concerned that the interaction term, $Jury \times LG$, could instead capture other characteristics of Law graduates (such as legal skills) or reflect the *Jury*'s receptiveness to arguments presented by Law graduates.

To support our interpretation of the results, we leverage variation in access to the *Jury*'s network based on a defendant's place of birth. Naturally, a defendant's birthplace is unrelated to their legal skills – a Law graduate from Bordeaux should, on average, possess comparable abilities and qualifications to one from the Paris area. However, the latter likely had greater access to the legal milieu surrounding the *Jury*. To separate the effect of connections from legal skills, we constructed two measures of proximity to Paris at birth: first, the straight-line distance to Paris; and second, a binary variable indicating if the birthplace lies within a 100km radius of Paris.

If connections explain the positive estimate of the coefficient on $Jury \times LG$ in Table 3, then the triple interaction $Jury \times LG \times Paris$, where *Paris* is generic notation for one of the two measures of proximity of a defendant's birthplace to Paris, should be significant. This is because the strength of connections varies with geographical proximity to the legal milieu around the *Jury*. Conversely, if the difference-in-differences estimate captures legal skills

or the *Jury*'s receptiveness to arguments presented by Law graduates, then the estimate should not vary by birthplace, and the triple interaction should be insignificant. Table 4 reports the results. We find that the triple interaction, $Jury \times LG \times Paris$, is statistically significant (Columns 4.3 and 4.4). Defendants born close to Paris had a higher probability of being acquitted in front of the *Jury* only when they were Law graduates. In contrast, the interaction $Jury \times Paris$ is insignificant regardless of the measure of proximity to Paris at birth we use (Columns 4.1 and 4.2). This suggests that being born close to Paris *per se* gave no particular advantage in front of the *Jury*. Accordingly, proximity to Paris at birth only mattered among Law graduates. These pieces of evidence suggest that connections are the most plausible reason for the differential acquittal rate of Law graduates before the *Jury*. We attribute the added likelihood of having their ban overturned for Law graduates born near Paris to their greater access to the legal milieu surrounding the *Jury*. This interpretation is further supported by the fact that Law graduates from Parisian universities – arguably more integrated into this legal milieu – were more likely to be acquitted by the *Jury*, while other Law graduates did not get acquitted more frequently than non-Law graduate defendants (see Appendix Table D.5).

Table 4: Proximity to Paris and connections of Law graduates to the *Jury*

	(4.1)	(4.2)	(4.3)	(4.4)
LG	-.0506 (.0308)	-.0514 (.0309)	-.0448 (.0321)	-.0883 (.169)
Jury \times LG	.146 (.045)	.148 (.0447)	.114 (.0474)	.626 (.227)
Jury \times $\mathbb{1}(\text{Born}<100\text{km from Paris})$.0805 (.0529)		.00116 (.0668)	
Jury \times Distance to Paris		-.0201 (.0181)		.012 (.0214)
Jury \times LG \times $\mathbb{1}(\text{Born}<100\text{km from Paris})$.227 (.133)	
Jury \times LG \times Distance to Paris				-.0888 (.0412)
Observations	768	768	768	768
Adjusted R ²	.295	.295	.296	.298
Marginal Effects – LG before the <i>Jury</i>				
Outside buffer			0.07	
Within buffer			0.25**	
<u>Controls</u>				
Resistance and collaboration	Yes	Yes	Yes	Yes
Age and Religion	Yes	Yes	Yes	Yes
Political mandates	Yes	Yes	Yes	Yes
Journalist	Yes	Yes	Yes	Yes
Included controls \times Jury	Yes	Yes	Yes	Yes
<i>Département</i> fixed effects	Yes	Yes	Yes	Yes
<i>Département</i> fixed effects \times Jury	Yes	Yes	Yes	Yes

Note: The full regression (in Columns 4.3 and 4.4) is $Acquit_{i,c} = \alpha_0 + \alpha_1 Jury_c \times LG_i + \alpha_2 Jury_i + \alpha_3 LG_i + \alpha_4 Paris_i + \alpha_5 LG \times Paris_i + \alpha_6 Jury_c \times Paris_i + \alpha_7 Jury_c \times LG_i \times Paris_i + CONTROLS + \varepsilon_{i,c}$, where $Paris_i$ is either the direct-distance from the place of birth to Paris (*Distance to Paris*) or a dummy variable equal to one if the birthplace was within 100km of Paris (*Born<100km from Paris*). We include the same controls as in Table 3, Column 3.4, including *département* fixed effects. The sample includes defendants born in mainland France only. We report only the estimates of the variables of particular interest. Standard errors in parentheses are clustered at the *département* level.

Additional tests reported in the Appendix further bolster the connections interpretation. In particular, we find that our estimate of connections cannot be explained by the ability of Law graduates to leverage evidence in their files to their advantage (Appendix D.7). We also find that Law graduates were more likely to be acquitted if they had longer political careers during which their legal skills probably degraded (Appendix D.9). This suggests, again, that the higher acquittal rate of Law graduate before the *Jury* is explained by connections and not by any characteristics related to the legal profession.

5 The imprint of Direct and Indirect Connections in the *Jury*'s Archive

We go one step further than most of the existing literature on connections and measure connections directly – rather than simply inferring them from the context. This allows us to investigate the role played by different types of connections. We do this by drawing on the content of the defendants' dossiers from the archive of the *Jury*. If the positive estimate of the coefficient on the *Jury* \times LG interaction truly reflects connections between Law graduates and the *Jury*, then we should find imprints of these connections in the individual dossiers of the defendants. For example, we should find informal and personal letters from the defendants or supporting letters from powerful third-party figures. Any other explanation based either on legal skills, defendants efforts, or the “taste” of the *Jury* would leave different imprints such as, for example, long legalistic statements of defense submitted by the defendants. Thanks to this approach we can (1) develop direct measures of connections, (2) distinguish between direct and indirect connections, and (3) determine which type of connections mattered.

5.1 The dossier dataset

In our theoretical framework, presented in Section 3 and fully developed in Appendix A, connected defendants can, under certain conditions, influence the decisions of the court to which they are connected through two channels. Direct connections facilitate communication between defendants and the court, while indirect connections enable communication between third-party supporters of a defendant and the court. Both channels provide connected defendants with an edge that unconnected defendants lack when decision-relevant information about mitigating circumstances can be communicated credibly, and court officials are willing to consider this information if they are convinced that doing so will reduce the risk of making type I and type II errors.

To develop quantitative indicators of these two types of connections, we draw on the dossiers from the archive of the *Jury*. The *Jury* kept a detailed record of each case, including all internal and external correspondence. The court dossiers for each of the defendants contain all the pieces of information that the *Jury* used to reach its ruling. All these still classified dossiers are held by the *Archives Nationales de France* which granted us access to them.¹⁹ We created a full inventory of the 17,589 documents contained in these dossiers.²⁰

¹⁹Archives Nationales de France, References AL//5295 to AL//5334.

²⁰We were not allowed to scan the documents. This prevents digitization and OCR-based textual analysis.

These documents provide a wealth of contextual evidence that connections, not only were considered by many defendants to have influenced the *Jury*'s decisions, but also that connections were frequently leveraged to lobby its members. Firstly, a common line of defense used by defendants to challenge the decision of the *Jury* to ban them from politics was that undue pressure had influenced it. 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.²¹ On 18 October 1945, defendant C wrote in a letter to General de Gaulle: "It is not about justice, but about connections".²² Some of the defendants' supporters also used the rhetoric of partial decisions. For example, defendant D's dossier contains a letter from an anonymous supporter denouncing "a political plot against" him.²³

Secondly, many individual dossiers feature unambiguous evidence of how direct and indirect connections were leveraged. A perfect illustration is the letter defendant E directly sent to René Cassin on 6 June 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".²⁴ The dossiers also show how defendants mobilized their own connections to contact the *Jury* to lobby on their behalf. For example, the dossier of defendant F contains a letter from one of his supporters asking Fedia Cassin if he could help him reach out to his brother René Cassin, the president of the *Jury*.²⁵ 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.²⁶ A Resistance leader wrote to defendant G: "I would like to let you know that after learning of the injustice in your case, I personally went to see M. Bernard [Rapporteur on the case]".²⁷ Similarly, the dossier of defendant H, who had had his ban upheld, 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".²⁸

Thirdly, we observe various degrees of informality in the way the letters in the dossiers address the recipient, 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") which is indicative of a close connection between the two. For example, the dossier of defendant J contains a letter which

²¹As the archives are still classified, names are anonymized and we refer to defendants by letters. Archives Nationales de France, Reference: AL//5308 and AL//5309.

²²Archives Nationales de France, Reference: AL//5324.

²³Archives Nationales de France, Reference: AL//5321.

²⁴Archives Nationales de France, Reference: AL//5298.

²⁵Archives Nationales de France, Reference: AL//5298.

²⁶Archives Nationales de France, Reference: AL//5334.

²⁷Archives Nationales de France, Reference: AL//5298.

²⁸Archives Nationales de France, Reference: AL//5331.

a supporter wrote on 31 July 1945 to René Cassin with that salutation stating that not acquitting defendant J would be a mistake.²⁹ Similarly, the dossier of defendant K contains a letter from the defendant to the President of the Constituent Assembly from April 1946 asking for support. It is followed by a letter from the President of the Constituent Assembly to René Cassin in October 1946 starting with *Mon cher ami* and asking for a new assessment of defendant K’s case.³⁰

Our inventory makes it possible to systematically extract three types of information from the dossiers. 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, including whether they contain evidence of participation in the Resistance. Second, we can record documents containing correspondence between a defendant and the *Jury*. Third, we can record documents submitted by individuals external to the *Jury* who tried to intervene in favor of a defendant by writing to the *Jury* or to parties connected to the *Jury*. We refer to these as “letters of support”.³¹

The resulting dossier dataset shows that the dossiers of the Law graduates were at first glance similar to those of other defendants.³² Table 5 reports mean comparisons between Law graduates and other defendants for variables related to the general content of the dossiers (panel A), related to letters of support (panel B), and related to the information content of the dossiers (panel C). In all, but one aspect, the means are statistically indistinguishable.³³ In short, the dossiers of Law graduates are not thicker than other dossiers and do not contain more letters of support or more letters with information about participation in the Resistance than those of other defendants. If we interpret the size and content of the dossiers as an indicator of the effort defendants’ put into their defense, then the similarity of the dossiers of Law graduates and other defendants shows that the former did not put more effort into their defense than the latter. This suggests that Law graduates were not more eager to continue their political career after the war than other defendants.³⁴

²⁹ Archives Nationales de France, Reference: AL//5303.

³⁰ Archives Nationales de France, Reference: AL//5311.

³¹In a few cases, letters supported banning a defendant, but the vast majority of them were, in fact, letters advocating that the defendant be acquitted.

³²Appendix D.1 and D.2 define and describe all the variables related to the content of the dossiers. Appendix Table F.2 reports summary statistics related to the overall structure of the dossiers.

³³The exception is 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. That difference can, however, not explain the differential acquittal rate of Law graduate before the *Jury* because we control for it in the difference-in-differences estimations reported in Table 3.

³⁴Note that the inclusion of the Law graduate fixed effect in our baseline difference-in-differences regression already ruled out that the differential acquittal rate of Law graduates is driven by their greater willingness to continue their political career.

Table 5: Law graduates and the general content of the dossiers

	(5.1)	(5.2)	(5.3)
	Mean		
	LG	Others	Diff=0 (p-value)
Structure of the dossiers			
Nb Documents	40.96	40.59	0.90
Nb Pages	59.31	53.97	0.29
Nb Documents from the <i>Jury</i>	13.86	13.63	0.74
Nb Archival Documents	3.23	2.63	0.39
Nb Information requests	1.16	1.04	0.27
Letters of support			
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	1.23	0.94	0.39
Nb Letters of support - Others	2.08	2.88	0.26
Information content			
Nb Documents - Military Resistance	0.82	0.75	0.85
Nb Documents - Civilian Resistance	8.36	8.63	0.83
Nb Documents - Resistant Press	0.29	0.70	0.04
Nb Documents - Legal Arguments	5.59	5.37	0.62
Nb Documents - Political opinion	10.39	9.87	0.75
Nb Documents - Reelection	0.95	1.11	0.53
Nb Documents - Other topic	6.45	6.26	0.84

Note: “Nb” means “number of”. The top panel, labelled “Structure of the dossiers”, 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, or Administration). The bottom panel, labelled “Information content”, presents information on the topic covered by the documents. 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. Columns 5.1 and 5.2 display the mean value for the group of Law graduates (LG) and other defendants (Others), respectively. Column 5.3 reports the p-value of a two-sided t-test of equal means.

5.2 How connected were Law graduates? Measures and Results

From the dossier dataset, we construct quantitative measures of direct and indirect connections to the *Jury* for each defendant. We measure direct connections by the number of documents submitted by the defendant himself. Within these, we distinguish between documents related to legal arguments and documents related to non-legal matters, e.g., letters asking for an update on how the case is proceeding, etc. Moreover, we count the number

of “Informal letters from defendant”, defined as letters sent by defendants addressing the recipient as *Mon cher ami* in a dossier. We conjecture that defendants with a lower cost of communication communicate more frequently than others with the *Jury* about matters related to their case. We also conjecture that defendants who write about non-legal matters or in an informal style are better connected to the *Jury*.

We build three measures of indirect connections between the third-party supporters of a defendant and the *Jury*. First, we measure indirect connections by the number of letters of support in a defendant’s dossier originating from supporters in Paris and by the number of 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. We conjecture that Paris-based authors of letters of support, therefore, had a stronger connection to the *Jury* than other supporters, as they would interact in the same social and professional circles. Second, we built an index of the portfolio of letters of support for each defendant. 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) are grouped together and calculate the fraction of letters from each organization.³⁵ We then weight the share of letters in a defendant’s portfolio from a given organization with the total volume of correspondence originating from that organization in the entire dataset. We call this index “Indirect connections via supporters”. Appendix D.3 details the construction of the index. We conjecture that supporters have a stronger indirect connection to the *Jury* if they are primarily 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 argue that a letter from someone in the Ministry of Home Affairs would, therefore, carry more weight than a letter from someone from the Prefecture of the Morbihan *département*. Third, we record the number of documents from supporters of each defendant addressing the recipient as *Mon cher ami* and use these “informal documents from supporters” as an index of his indirect connections.

Their connections to the *Jury* set Law graduates apart. This is shown in Table 6. The

³⁵We classify “organizations” along two dimensions: their name (e.g., Ministry of Home Affairs) and their 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. Accordingly, for each *département* there is a fictitious “organization” of supporters 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*.

top panel reports mean comparisons of direct connections between Law graduates and other defendants. We see that Law graduates and other defendants send a similar number of documents overall. However, Law graduates send more documents *without* legal arguments than other defendants (row 1.1) while the dossiers do not differ with respect to documents *with* legal arguments (row 1.2). This is a strong indication that Law graduates used their connections to influence the members of the *Jury* rather than applying their legal skills to highlight particular points of their case. We find no difference with regard to informal letters (row 2). The bottom panel of the table reports mean comparisons for indirect connections. The evidence clearly shows that Law graduates had more such connections than other defendants. First, the *Jury* received more letters from Parisian supporters in support of Law graduates than in support of other defendants, and Law graduates had more Paris-based supporters than other defendants (rows 3 and 4).³⁶ Second, the supporters of Law graduates were better connected to the *Jury* than those of other defendants (row 5). Third, the dossiers of Law graduates contained more “informal documents from supporters” than those of other defendants (row 6). Together this shows that Law graduates could draw on more supporters and that the connections of those supporters to the *Jury* were stronger. We show that these traces of indirect connections are also more present in the dossiers of Parisian Law graduates than in those of other defendants (Appendix D.6), as one would expect given the evidence presented in Section 4.4.

³⁶Recall that the two groups have the same total number of letters of support (see Table 5). Accordingly, it is the composition of who sent them that differs.

Table 6: Connections between the defendants and the *Jury*: Law graduates versus other defendants

	(6.1)	(6.2)	(6.3)
	Mean		Diff=0 (p-value)
	LG	Others	
Direct connections			
1. Nb Documents from defendant	5.00	4.50	0.46
1.1 Nb Documents from defendant w.o legal content	1.80	1.32	0.05
1.2 Nb Documents from defendant with legal content	3.2	3.2	0.98
2. Nb Informal letters from defendant	0.17	0.16	0.89
Indirect connections			
3. Nb Letters of support from Paris	2.73	1.82	0.09
4. Nb Supporters from Paris	1.23	0.70	0.01
5. Indirect connections via supporters (excl. letters)	26.59	15.13	0.03
6. Nb Informal documents from supporters	1.99	1.26	0.03

Note: “Nb” means “number of”. In row 1, “Nb Documents from defendant” refers to the number of documents sent by the defendant to the *Jury*. In row 1.1, “Nb Documents from defendant w.o legal content” refers to the number of documents sent by the defendant with content unrelated to the legal aspect of his case. In row 1.2, “Nb Documents from defendant with legal content” refers to the number of documents sent by the defendant in which he presents legal arguments related to his case. In row 2, “Nb Informal letters from defendant” refers to the number of informal letters in the dossiers. In row 3, “Nb Letters of support from Paris” refers to the number of letters of support from a sender located in Paris. In row 4, “Nb Supporters from Paris” refers to how many different supporters from Paris were sending letters of support. In row 5, “Indirect connections via supporters (excl. letters)” refers to the portfolio index based on the total number of documents from each organization. In row 6, “Nb Informal documents from supporters” refers to the number of documents from supporters using the informal address *Mon cher ami*. Columns 6.1 and 6.2 display the mean value for the group of Law graduates (LG) and other defendants (Others), respectively. Column 6.3 reports the p-value of a two-sided t-test of equal means.

5.3 From connections to acquittal

We want to investigate the extent to which the connections to the *Jury* that we are able to quantify from the dossiers can account for the higher acquittal rate of Law graduates before the *Jury*. To this end, we sequentially augment Equation (1) with the five measures of connections (C_j , with j being the index for the different measures) for which we found a statistically significant difference between Law graduates and other defendants in Table 6. We include the interactions between these measures of connections and the *Jury* and LG dummy variables, respectively, as well as the triple interaction between each measure and these dummy variables. The augmented regression model is:

$$\begin{aligned}
 Acquit_{i,c} = & \gamma_0 + \gamma_1 Jury_c \times LG_i + \gamma_2 Jury_c + \gamma_3 LG_i + \gamma_4 C_j \\
 & + \gamma_5 Jury_c \times LG_i \times C_j + \gamma_6 Jury_c \times C_j + \gamma_7 LG_i \times C_j + \varepsilon_{i,c}. \quad (2)
 \end{aligned}$$

Table 7 reports the results for the parameters of interest and presents two tests. First, we can test whether the marginal acquittal probability of being a Law graduate depends on each measure of connections C_j in particular before *Jury*, i.e., if $\gamma_5 \neq 0$. Column 7.1 shows the

results for the measure of direct connections more common among Law graduates than among other defendants. The estimate of γ_5 is negative and significant at the 10 percent level. This suggests that *direct* connections to the *Jury*, if anything, tended to reduce a Law graduate’s advantage before the *Jury*. This type of connections was, therefore, not the main reason why the *Jury* acquitted Law graduates more frequently than other defendants. Columns 7.2 to 7.5 show results for the four types of indirect connections more common among Law graduates. We see that these connections did not give them a particular boost before the *Jury* (γ_5 is insignificant). Overall, this suggests that the effect of indirect connections was similar for Law graduates and other defendants facing the *Jury*. Importantly, however, Law graduates had more of these connections (see Table 6). Accordingly, even if indirect connections did not boost the acquittal rate before the *Jury* specifically for Law graduates, the fact that they had more of them help explain their higher acquittal rate.

Second, another way to gauge the role of connections is to ask whether the marginal impact of a particular type of connections (C_j) on the acquittal probability was stronger for Law graduates before the *Jury* than before the CDLs, i.e., to test if

$$\left. \frac{\partial^2 Acquit}{\partial C_j \partial Jury} \right|_{LG=1} = \gamma_5 + \gamma_6 \neq 0. \quad (3)$$

The results of this test are reported at the bottom of Table 7. We find in three cases –the number of letters of support from Paris, the number of supporters from Paris, and indirect connections excluding letters– that the marginal effects of indirect connections on acquittal are significantly larger for Law graduates in judgments made by the *Jury* than for those made by the CDLs (Columns 7.2, 7.3, and 7.4). Accordingly, indirect connections influenced the judgments of the *Jury* concerning Law graduates relatively more than the decisions involving the local courts. This was not the case for direct connections, again, highlighting the key role played by indirect connections.

Taken together, these results support our theoretical argument that Law graduates benefited from indirect connections to third parties – in particular Paris-based third parties – who could provide information to the *Jury* on their behalf. These results are important for two reasons. First, the connections of Law graduates to the *Jury* are evident in their dossiers. Thanks to this imprint, we can measure some markers of connections and show that they help explain the higher acquittal rate this group of defendants experienced in front of the *Jury*.

Second, indirect connections matter more than direct ones. Accordingly, the higher acquittal rate of Law graduates before the *Jury* probably did not result from any malicious plot by

Table 7: Unpicking the Law graduate effect: the role of direct and indirect connections

Dependent variable	(7.1) <i>Acquit_{i,c}</i>	(7.2) <i>Acquit_{i,c}</i>	(7.3) <i>Acquit_{i,c}</i>	(7.4) <i>Acquit_{i,c}</i>	(7.5) <i>Acquit_{i,c}</i>
	Direct connections		Indirect connections		
Measure of connections C_j	Nb doc from def w.o. legal arg.	Nb letters of support from Paris	Nb supporters from Paris	Indirect connections (excl letters)	Nb informal documents
Jury \times LG	.176 (.0584)	.0767 (.0467)	.0962 (.0476)	.0704 (.0531)	.124 (.057)
C_j (β_4)	-.021 (.0209)	-.0202 (.0129)	-.0233 (.02)	-.000219 (.0105)	.0025 (.0249)
Jury \times LG \times C_j (β_5)	-.12 (.0661)	.0115 (.0488)	-.0255 (.0673)	.0103 (.0245)	-.0576 (.0637)
Jury \times C_j (β_6)	.124 (.0416)	.0947 (.0325)	.143 (.0471)	.0385 (.016)	.113 (.0405)
LG \times C_j (β_7)	.075 (.0469)	.0557 (.0322)	.0904 (.0459)	.0104 (.0178)	.0122 (.0412)
Observations	798	798	798	798	798
Adjusted R ²	.051	.065	.0654	.0566	.0564
<u>Test: $\gamma_5 + \gamma_6 \neq 0$</u>	<u>Law graduates-<i>Jury</i> vs Law graduates-CDL</u>				
p-values	0.96	0.01	0.04	0.02	0.31

Note: The measure of connections (C_j) included in each regression is indicated in the column headings. The estimates of coefficients γ_2 and γ_3 are not shown. The test reported at the bottom compares the marginal effect of connections for Law graduates in front of the CDLs and Law graduates in front of the *Jury*, i.e., tests if $\gamma_5 + \gamma_6 \neq 0$. We estimate the models with Ordinary Least Squares. Standard errors are clustered at the *département* level (in parentheses).

either the (Law graduate) defendants or the members of the *Jury*. The more likely source of the higher acquittal rate is that interventions by Paris-based supporters of Law graduates – because of their proximity to the *Jury* – were considered more trustworthy by the members of the *Jury* than interventions by individuals and organizations outside this network. For that reason, these supporters could convey decision-relevant information that the *Jury* was willing to take into account.

6 Conclusion

We study the connections of a prominent elite group in French politics, Law graduates, to one of the courts in charge of purging the country’s political elite during the post-war transition back to democracy. We show that these connections helped Law graduates avoid being banned from holding public office. We find that Law graduates had a clearance rate that was 10 to 14 percentage points higher than other defendants. Our theory of connections and transitional justice emphasizes that connections provide connected defendants with an edge that unconnected defendants lack when decision-relevant information about mitigating

circumstances can be communicated credibly, and that court officials are willing to consider this information if they are convinced that doing so will reduce the risk of making errors. Archival evidence shows that it was primarily indirect connections that mattered. Specifically, Law graduates benefited from having third-party supporters close to the *Jury* who could screen claims of mitigating circumstances and communicate them truthfully to the court. In contrast, we find little evidence in the archives of the *Jury* that direct connections to the court helped defendants. The higher acquittal rate of Law graduates eventually helped them pursue post-war political careers.

Our analysis emphasizes the role of third parties by highlighting the importance of indirect connections in transmitting information to courts. Most jurisdictions prohibit direct connections between judges and litigants to avoid conflicts of interest. However, knowing someone indirectly connected to the court who has a reputation to protect and an interest in supporting particular litigants may benefit a litigant more than a personal connection to the court. This does not necessarily imply that bans on connections should systematically extend to indirect connections. One reason is that such a ban could be overly restrictive, as it might exclude too many judges from the process. More importantly, our theory suggests that indirect connections facilitate the transmission of decision-relevant information to the court, potentially reducing court errors. However, this transmission is possible only for a subset of defendants. Determining when the benefits of such information transfers outweigh the risks of breaching the equality of litigants before the Law is an avenue for future research. This question is particularly acute during political transitions. The trade-off between efficiency and equality before the law that we document complements previous research on the difficult balance between fairness and judicial errors in transitional justice documented in other countries and periods.³⁷ Our results are an invitation to study the political economy of democratic transitions to gauge the functioning of transitional justice.

Our analysis also speaks to the question of elite persistence. First, our results show that political selection in the transition to a new democracy can be distorted, despite the fact that the process used to purge collaborators with the previous autocratic regime respects the rule of Law: Because of their connections, some elite groups are more likely to persist than others. It, thus, becomes clear that connections are part of what Acemoglu and Robinson (2006) refer to as *de facto* power. Connections have been proven to be instrumental in distorting laws (Cohen and Malloy, 2014) and biasing political selection within political regimes (Dal Bó et al., 2009). Our analysis shows that connections also matter after major

³⁷Examples of the difficulties in striking this balance include denazification in Germany (Gimbel, 1960), various lustrations in Eastern Europe following the fall of the USSR (Nalepa, 2010), the post-Pinochet purge in Chile (González et al., 2021), and the Truth and Reconciliation process in South Africa (Gready, 2010).

institutional changes and can contribute to elite persistence. Second, our analysis adds to the understanding of what causes elite persistence. It stresses that in a democratic transition, the members of the authority overseeing the purge process may be connected to members of the previous autocratic institutions whom they have to judge. Those connections confer on this subset of the previous elite an advantage. The specifics of how connections help compromised elites in the transition to democracy will be context-specific and more research is needed on other cases than the French case we study here to fully understand this phenomenon.

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[Appendix for Online Publication]

A A theory of direct and indirect connections

This appendix develops a theory of how court decisions can be influence via connections. The theory is framed to match the procedure used in France at the end of World War II to judge the politicians who had voted for the enabling act. Accordingly, we consider two courts that have to decide if a ban on political participation already imposed on the defendants stands or is overturned. Each court has its own standard of proof required for acquittal. The defendants or their supporters can report mitigating circumstances with the aim of influencing the court decisions. Reporting 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. 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 a defendant and a court. The second is *indirect* connections operating via a third party (a supporter of the defendant). We show that both mechanisms can under certain circumstances help defendants with connections get acquitted by the court to which they are connected.

A.1 Courts decisions

Two courts, indexed by $c \in \{L, U\}$, are tasked with judging a fixed number of defendants indexed by $i \in D$. Before any evidence is heard, the two courts set their own bar for acquittal. After that, evidence is presented to the courts (related to whether the defendant had been sufficiently involved in the resistance), 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 I (convicting innocent defendants) and type II (not convicting guilty defendants) errors.³⁸ The weights that court c puts on type I and II errors are α_c and $1 - \alpha_c$, respectively. The evidence presented to the

³⁸This is consistent with the type of democratic purge that we study. A purge in an authoritarian regime would likely give little weight to type I errors, if any.

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) and the corresponding density functions are a_G and a_I . We assume that the defendants can be divided into subgroups based on fixed observable characteristics, such as profession, political affiliation, region of residence, age, religion, and other observable characteristics, and that the proportion of guilty defendants in those 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 subgroups. The two courts assume that the proportion of guilty among defendants belonging to subgroup g is π_g . As a consequence, each court sets N different bars – one for each group g . The objective function of court c can, then, be written as

$$\min_{x(c,g)} \sum_{g=1}^N \alpha_c (1 - \pi_g) A_I(x(c, g)) + (1 - \alpha_c) \pi_g (1 - A_G(x(c, g))). \quad (4)$$

Calculating the first order conditions, the optimal bar for court c for defendants belonging to sub-group g is the solution to

$$\frac{\alpha_c}{1 - \alpha_c} \frac{1 - \pi_g}{\pi_g} = \frac{a_G(x^*(c, g))}{a_I(x^*(c, g))} \quad (5)$$

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

$$Pr[c, g] = \pi_g (1 - A_G(x^*(c, g))) + (1 - \pi_g) (1 - A_I(x^*(c, g))) \quad (6)$$

$$\equiv 1 - H_g(x^*(c, g)), \quad (7)$$

where $H_g = \pi_g A_G + (1 - \pi_g) A_I$. After applying a first order linear approximation, we can write the probability of acquittal before court c of a defendant from subgroup g as

$$Pr[c, g] \approx a + b_c + b_g. \quad (8)$$

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 represents 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 (due to differences in π_g). This is a manifestation of

statistical discrimination and can explain systematic differences in acquittal rates between different sub-groups of defendants and is captured by b_g in equation (8).

A.2 Connections to the Courts and Mitigating Circumstances

For each defendant i , we assume that there may or may not be mitigating circumstances that he or one of his supporters can present to the courts with the aim of influencing the court decisions. We model this by θ which can take two values: θ_Y if there are mitigating circumstances and θ_N if not with $\theta_Y > \theta_N$. This is private information to the defendants and their supporters and not known to the courts and cannot be externally verified. Mitigating circumstances are communicated to the courts via letters. The content of a letter sent by defendant i either personally or via a supporter to court c is $l_{i,c} \in \{\theta_Y, \theta_N\}$. If a court c accepts a letter claiming that $\theta = \theta_Y$ for a defendant i belonging to subgroup g , then it is more likely that the defendant is acquitted and his acquittal probability increases by $\eta_{c,g} > 0$ and, using the linear approximation to the acquittal probability in equation (8), becomes

$$Pr[c, g] = a + b_c + b_g + \eta_{c,g}. \quad (9)$$

We can interpret a letter $l_{i,c} = \theta_N$ as not sending a letter containing arguments about mitigating circumstances to court c . Court c wants 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 avoid 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 a 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 regardless. 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 regardless, 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 each court c as a sequential game of asymmetric information where the defendant, firstly, after privately learning if there are mitigating circumstances, sends a letter to the court which, secondly, updates its beliefs about whether there are mitigating circumstances based on the content using Bayes rule when

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 ($\gamma > \frac{\theta_Y - \theta_N}{2} \equiv \bar{\gamma}$), i.e., we rule out informative perfect Bayesian equilibria of the type considered by Crawford and Sobel (1982). In order to influence a court, a defendant needs to use his direct or indirect connections to that court.

A.2.1 Direct connections

All defendants can, in principle, 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.³⁹ Each defendant i faces a fixed cost, $f_{i,c}$, to connect to court c . 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. 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 there are such circumstances 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. 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

³⁹See Grossman and Helpman (2002) for a similar approach to lobbying.

$f_{i,c}$. 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

$$f_{i,c} \leq (\theta_Y - \theta_N)(2\gamma + (\theta_Y - \theta_N)) \equiv \bar{f}. \quad (10)$$

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

$$f_{i,c} \geq (\theta_Y - \theta_N)(2\gamma - (\theta_Y - \theta_N)) \equiv \underline{f}. \quad (11)$$

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

The proposition shows that the defendants for each court 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 defendants connected to court c have relatively low cost of communicating with that court ($f_{i,c} \leq \bar{f}$), only some of them are successful at convincing the court that mitigating circumstances are relevant. If they are “too” connected to the court ($f_{i,c} < \underline{f}$), then the court will not trust their letters. Indeed, in these cases, the cost of sending information is so low that defendants will do it regardless of whether there are mitigating circumstances or not. As a consequence, receiving a letter from them is uninformative. The defendants who can influence the court are those with a moderately good connection to it ($f_{i,c} \in [\underline{f}, \bar{f}]$). 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 court c . This captures the first channel through which connections can influence court decisions. The objective of the court is to minimize judgement errors and it knows that defendants have an incentive to use their connection to the court to curry favors. This implies that defendants with “too good” a connection will not be trusted and it is only those for whom using their direct connection to the court is moderately costly who will be able to convey information about mitigating circumstances credibly and to allow the court to screen the information they receive directly from the defendant effectively. The

proposition has the following empirical implication: if the population of defendants contains directly connected defendants as well as unconnected ones, then, on average, the connected defendants are at least as likely as those who are unconnected to that court to be acquitted *by the court to which they are connected* and strictly more likely if they are not all “too” connected.

A.2.2 Indirect connections

A defendant may also have an *indirect* connection to court c through a third party (T) who can submit *letters of support* to that court on behalf of the defendant. The third party supporters observe θ and, thus, know whether there are mitigating circumstances or not: they have already screened the information about mitigating circumstance and established whether it is credibility or not. Unlike the defendants, all third parties can send letters at low cost (for simplicity, we set the cost at zero for both courts, i.e., $f_c^T = 0$ for all c). This means that they are all “too” connected and the reason why third party supporters can influence the courts is, therefore, fundamentally different from the mechanism behind proposition 1. The reason that supporters of a defendant may be able to influence court c is that while they care about the defendant they support (U_i), they also, to some extent, align with the objective that court c uses to judge mitigating circumstances ($U_{i,c}$). Specifically, we assume that the third party supporters of defendant i have the following objective in relation to court c :

$$U_{i,c}^T = \beta_{i,c}U_{i,c} + (1 - \beta_{i,c})U_i = -\beta_{i,c}(m_{i,c} - \theta)^2 - (1 - \beta_{i,c})(m_{i,c} - \theta - \gamma)^2 \quad (12)$$

where $\beta_{i,c}$ determines the relative weight on the two considerations which may vary for defendants across the two courts. From the point of view of the third party, the optimal decision of court c for defendant i is $d_{i,c}^T = \theta + (1 - \beta_{i,c})\gamma$. One interpretation of this is that the judges may trust more people they know better – in part because these people would lose more if the evidence they provided proved to be wrong. The next proposition shows how indirect connections to a court can help a defendant to get acquitted.

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 to court c can convince that court that there are mitigating circumstances for defendant i when that is the case. A letter to court c from a third party with $\beta_{i,c} \leq \bar{\beta}$ is not believed by that court.*

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 to 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

$$\beta_{i,c} > 1 - \frac{\theta_Y - \theta_N}{2\gamma} \equiv \bar{\beta}. \quad (13)$$

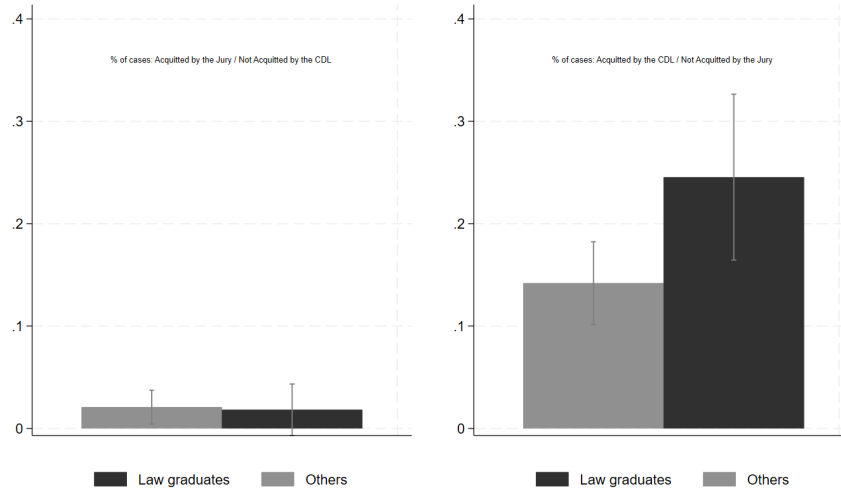
Clearly $\bar{\beta} < 1$. The cut-off $\bar{\beta} > 0$ because we assume that $\gamma > \frac{\theta_Y - \theta_N}{2} \equiv \bar{\gamma}$ for all i . Knowing this, court c will believe a letter of support claiming mitigating circumstances for defendant i coming from a third party with $\beta_{i,c}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

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. This splits the set of defendants 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 a 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 or by their own reputation and thus have objectives that partly overlap with those of the court. This is what makes their letters of support credible and influential. The connection channel is novel and requires that third-party supporters are able to screen the information about the defendant, communicate easily with the court, and align to some degree with the court's objective of minimizing judgment errors. While the first two features are necessary, on their own they are not sufficient. It is the third feature that enables indirect connections to work: when the court understands that the third party sufficiently shares its objective, it can benefit from the screening of information done by the third party and base its judgment on the information received through that channel.

Two empirical predictions flow from this. First, defendants supported by third parties trusted by a court are more likely to be acquitted than those who are not. Second, these defendants have more letters of support in their case files from third parties with a connection to that court than others.

B Baseline results - Additional Figures

Figure B.1: Percentage of cases overruled



% of CDL decisions overruled by the *Jury*

C Baseline results - Robustness checks

C.1 Law graduates during purges: A summary

Table C.1: Share of Law Graduates throughout the purging process.

	Voted Yes in 1940	Faced Jury	Acquitted
Law Graduates	167	110	34
Total	569	399	100
Share	29.0%	27.5%	34%

C.2 Other Estimators

Table C.2: Connections of Law Graduates before the *Jury*: Difference-in-differences estimates

	(C.2.1)	(C.2.2)
	Dependent Variable: $Acquit_{i,c}$	
	Logit	Probit
Jury \times LG	1.2 (.408)	2.15 (.885)
Observations	441	441
Pseudo R ²	.416	.414
<u>Controls:</u>		
Resistance and collaboration	Yes	Yes
Age and Religion	Yes	Yes
Political mandates	Yes	Yes
Journalist	Yes	Yes
<i>Département</i> fixed effect \times Jury	Yes	Yes
<i>Département</i> fixed effect	Yes	Yes

Note: Standard errors are clustered at the *département* level (in parentheses). *Jury* is a dummy variable equal to one if the judgment was before the *Jury* and zero otherwise. 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). The controls are: Age and religion (Age, Jewishness); Journalist; Political mandates (Mayor, Special Role in the Assembly, parliamentarian of an occupied territory); Resistance and collaboration (Civilian resistance, Military resistance, Arrested by the Vichy regime, Mayor under *Vichy*). Each control is also interacted with the *Jury* dummy variable.

Table C.3: Coefficients of control variables included in the main estimation.

	(C.3.1)	(C.3.2)	(C.3.3)	(C.3.4)	(C.3.5)
Jury \times LG	.106 (.0479)	.108 (.0418)	.0996 (.0418)	.14 (.0454)	.12 (.0515)
Jury \times Civilian Resistance		.158 (.0344)	.166 (.0339)	.19 (.038)	.186 (.0389)
Jury \times Arrested by Vichy		.219 (.0969)	.226 (.0952)	.253 (.105)	.252 (.108)
Jury \times Military Resistance		.282 (.0729)	.257 (.0721)	.228 (.0809)	.227 (.0825)
Jury \times Mayor under Vichy		-.0613 (.0404)	-.0108 (.0466)	.0368 (.0569)	.0358 (.0574)
Jury \times Occupied Territory			-.0458 (.0349)	.111 (.0838)	.0771 (.0909)
Jury \times Age			-.00222 (.00173)	-.00256 (.0021)	-.00297 (.00202)
Jury \times Pre-War Mayor			-.0727 (.0459)	-.0138 (.0589)	-.0171 (.0585)
Jury \times Journalist			.119 (.0509)	.0966 (.0591)	.0889 (.0604)
Jury \times Jewish			.381 (.158)	.247 (.217)	.244 (.222)
Jury \times Special Role Assembly			-.0589 (.0501)	.0135 (.0638)	.012 (.0638)
Jury \times Workers					-.0448 (.0869)
Jury \times Industrialists					-.00526 (.0651)
Jury \times Agriculture					-.0421 (.063)
Jury \times Teachers					-.0631 (.0673)
Observations	798	798	798	790	790
Adj. R ²	.0404	.271	.281	.292	.296
<i>Département</i> fixed effects	No	No	No	Yes	Yes
<i>Département</i> fixed effects \times Jury	No	No	No	Yes	Yes

Note: Standard errors are clustered at the *département* level (in parentheses) *Jury* is a dummy variable equal to one if the judgment was before the *Jury* and zero otherwise. LG is a dummy variable equal to one if the defendant was a Law graduate and zero otherwise. This table presents estimates of the coefficients attached to control variables interacted with the *Jury* variable in Equation (1). The reduction in the number of observations in Columns 3.4 and 3.5 is due to the fact that within some *départements*, there is no variation in sentencing. The *département* fixed effect is, therefore, perfectly collinear with the outcome in those cases and eight observations are dropped.

C.3 Controls included one by one

Table C.4: Difference-in-differences estimates: including control variables one by one.

	(C.4.1)	(C.4.2)	(C.4.3)	(C.4.4)	(C.4.5)
	Dependent variable: $Acquit_{i,c}$				
Estimator	OLS	OLS	OLS	OLS	
Jury	.419	.106	.218	-.0224	
	(.12)	(.0231)	(.0397)	(.0243)	
LG	-.0264	-.0276	-.0273	-.0249	-.0322
	(.0301)	(.0311)	(.031)	(.03)	(.0343)
Jury \times LG	.0978	.11	.0958	.108	.13
	(.0462)	(.048)	(.0496)	(.0418)	(.0533)
Observations	798	798	798	798	790
Adj R ²	.0521	.0405	.049	.271	.105
<u>Controls:</u>					
Age and Religion	Yes				
Journalist		Yes			
Political mandates			Yes		
Resistance and collaboration WWII				Yes	
<i>Département</i> fixed effects \times Jury					Yes
<i>Département</i> fixed effects					Yes

Note: *Jury* is a dummy variable equal to one if the judgment was made by the *Jury* and zero if it was made by 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) but with the controls entered one by one. Controls are: 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 *Vichy*). Each control variable is also interacted with the *Jury* dummy variable. Standard errors are clustered at the *département* level (in parentheses)

C.4 Counterfactual analysis

C.4.1 The method

We model the decision by the *Jury* to clear a defendant as a function of variables capturing participation of the defendant in the civilian and military resistance, which was the criteria officially used by the *Jury* to decide each case, controlling for the decision by CDLs. Specifically, we estimate the following regression 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 use the estimates of $\beta_1, \beta_2, \beta_3$ ($\hat{\beta}_1, \hat{\beta}_2, \hat{\beta}_3$) to compute each defendant's individual probability to be acquitted given the information retrieved from his dossier and the decision by the CDL that considered case. The estimated probability or rate given by $Counterfactual_{Jury,i} = \hat{\beta}_1 Acquitted_{CDL,i} + \hat{\beta}_2 CivilianResistance_i + \hat{\beta}_3 MilitaryResistance_i$ provides a counterfactual benchmark based on the official criteria used by the *Jury* against which to compare actual acquittal rates. We average these counterfactual acquittal rates at the group level.

C.4.2 Comparison between counterfactual and actual acquittal rates

Table C.5: Counterfactual versus actual acquittal rates for non-Law graduates and Law graduates

	Non-Law Graduates	Law graduates
Counterfactual	23.7%	22.4%
Actual	22.8%	30.9%

C.4.3 Placebo tests and control variables

Table C.6: Difference-in-differences estimates for different groups (Part I)

	CDL			Jury			Diff-in-Diff		
	(C.6.1)	(C.6.2)	(C.6.3)	(C.6.4)	(C.6.5)	(C.6.6)	(C.6.7)		
	Treated	Control	Diff=0	Treated	Control	Diff=0	$\Delta\Delta$	p-value	
<u>Treated:</u>	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	0.06
	MP of an occupied department	0.09	0.11	0.52	0.20	0.31	0.01	-0.09	0.03
	MP elected in Paris	0.05	0.10	0.44	0.25	0.25	0.99	0.05	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	0.55	0.11	0.10
	Informational cues								
	Mayor under Vichy	0.07	0.11	0.16	0.15	0.29	0.00	-0.09	0.03
	Arrested by Vichy	0.14	0.10	0.56	0.50	0.24	0.01	0.23	0.03
	Military resistance	0.27	0.06	0.00	0.68	0.15	0.00	0.32	0.00
	Civilian resistance	0.13	0.05	0.01	0.38	0.08	0.00	0.22	0.00
	Age							-0.005	0.02

Table C.6 - Difference-in-differences estimates for different groups(PART II)

		CDL			Jury			Diff-in-Diff	
		(C.6.1)	(C.6.2)	(C.6.3)	(C.6.4)	(C.6.5)	(C.6.6)	(C.6.7)	
		Treated	Control	Diff=0	Treated	Control	Diff=0	$\Delta\Delta$	p-value
<u>Treated:</u>		Group	Group	(p-value)	Group	Group	(p-value)		
Placebo Tests	Politics and political mandates								
	Senator	0.10	0.10	0.95	0.25	0.25	0.92	0.002	0.95
	Rightwing	0.11	0.09	0.42	0.25	0.25	0.93	-0.03	0.42
	Center	0.06	0.11	0.15	0.21	0.26	0.35	0.003	0.95
	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	0.82
	WWII fighter	0.08	0.10	0.78	0.38	0.24	0.15	0.15	0.12
	Networks, clubs and religion								
	Free Masons	0.07	0.10	0.66	0.33	0.25	0.45	0.12	0.24
	Labour Unions	0.03	0.11	0.19	0.19	0.26	0.45	0.01	0.85
	Agr organization	0.13	0.10	0.50	0.26	0.25	0.85	-0.02	0.75
	War Medal	0.11	0.10	0.79	0.24	0.26	0.62	-0.03	0.46
	Légion d'Honneur	0.12	0.09	0.21	0.25	0.25	0.88	-0.03	0.40
	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	0.10	0.90	0.25	0.25	0.99	-0.01	0.90
	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	
National Mandate							0.00	0.80	
Conseiller général							-0.001	0.48	
Study Years							0.003	0.70	

Note: The table reports mean comparisons and difference-in-differences estimates for different treatment and control groups. In part I, we select, one at the time, as the treatment group, the defendants belong to the groups defined by the control variables including in equation (1). In part II, we define the treatment group along a number of other dimensions (as a type of placebo test). Column C.6.1 presents the average acquittal rate of the treated group (defined in the left column) in front of the CDL (Comité Départementaux de Libération) whereas Column C.6.2 presents the average acquittal rate of the control group (i.e., all individuals not in the treated group) in front of the CDL. Column C.6.3 displays the difference between these two means. Column C.6.4 presents the average acquittal rate of the treated group in front of the *Jury* whereas Column C.6.5 presents the average acquittal rate of the control group (i.e., all individuals not in the treated group) in front of the *Jury*. Column C.6.6 displays the difference between Column C.6.4 and C.6.5. Column C.6.7 reports the difference-in-differences estimate of the difference in acquittal between a specific group (defined in the left column) and others in front of the *Jury* from a regression similar to equation (1) (but without any controls). This estimate is also by construction equal to the difference between the difference estimate in column C.6.6 and C.6.3. Standard errors are clustered at the département level.

C.5 Court-level estimates

Table C.7: The acquittal rates for Law graduates by court

	(C.7.1)	(C.7.2)
Dependent variable	$Acquit_i$	$Acquit_i$
Samples	CDLs	<i>Jury</i>
LG	-.0391 (.0333)	.101 (.0369)
Civilian Resistance	.0173 (.0285)	.208 (.0423)
Military Resistance	.19 (.0482)	.417 (.0765)
Arrested Vichy	-.00646 (.0982)	.247 (.0983)
Vichy Mayor	-.106 (.0418)	-.0696 (.0489)
Observations	395	395
Adj R ²	.126	.33
Age and Religion	Yes	Yes
Journalist	Yes	Yes
Political mandates	Yes	Yes
<i>Département</i> fixed effects	Yes	Yes

Note: Column C.6.1 reports the results from a linear probability model where the acquittal probability before the CDLs is regressed on the Law graduate dummy variable and all baseline control variables. Column C.6.2 reports the corresponding estimates for the *Jury*. Standard errors are clustered at the *département* level. Standard errors in parentheses.

C.6 Controlling for the composition of resistance in each department

Table C.8: Controlling for the composition of resistance in each department

Dep variable	(C.8.1) <i>Acquit_{i,c}</i>	(C.8.2) <i>Acquit_{i,c}</i>
Jury	-.11 (.0679)	.0763 (.122)
LG	-.0226 (.0296)	-.0294 (.0296)
Jury × LG	.11 (.0425)	.101 (.0412)
Gaullist/Domestic Resistance	.453 (.662)	.25 (.823)
Jury × Gaullist/Domestic Resistance	.351 (.895)	-.145 (1.21)
Gaullist/Communist Resistance	-.122 (.145)	-.0839 (.15)
Jury × Gaullist/Communist Resistance	.0391 (.214)	.0483 (.235)
Foreign/Domestic Resistance	-.000214 (.000281)	-.00023 (.000347)
Jury × Foreign/Domestic Resistance	-.000718 (.000305)	-.000708 (.000339)
Communist/Domestic Resistance	-.39 (.149)	-.319 (.163)
Jury × Communist/Domestic Resistance	.191 (.187)	.155 (.184)
Observations	798	798
Adj R ²	.256	.279
Proof of resistance	Yes	Yes
Individual controls		Yes

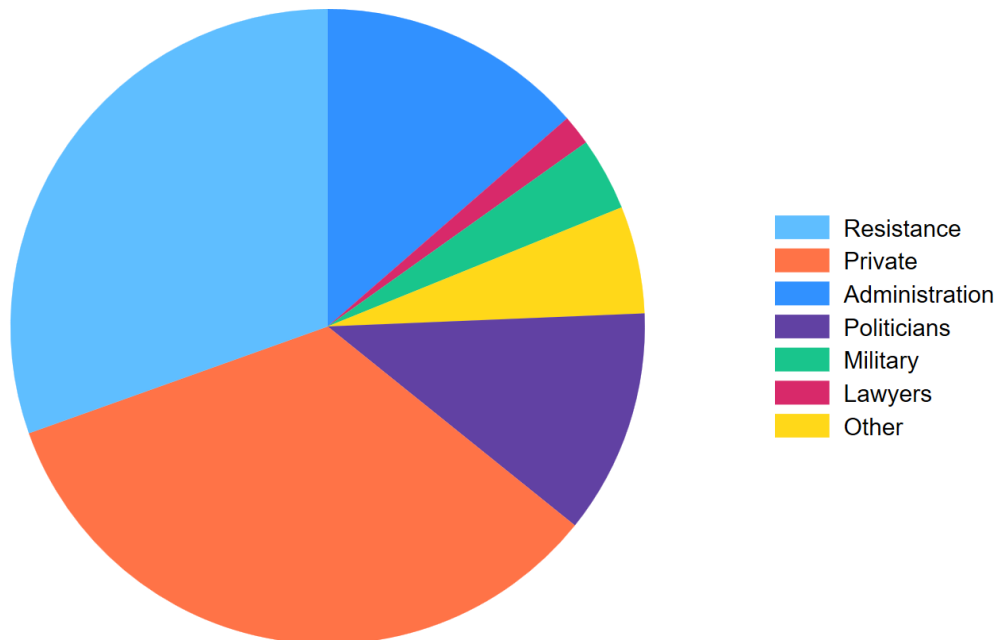
Note: The table reports two specifications of Equation (1) – with and without the baseline control variables and their interaction with the *Jury* dummy variable – augmented with proxies for the composition of the Resistance in the *département* and the interaction between these and the *Jury* dummy variable. The assumption is that the composition of the resistance in a *département* is highly correlated with the composition of the judges on the CDL in that *département*. We measure the composition of the Resistance in a *département* via different ratios of the membership of different resistance groups as defined by certificates of resistance. For example, Gaullist/Communist Resistance is defined as the ratio of the size of the membership of gaullist resistant factions and the membership of communist resistant factions in a *département*. Variables controlling for proof of participation in the civil or military Resistance are included in both specifications. Individual controls include: Age, Jewishness, Journalist, Mayor, Special Role in the Assembly, Arrested by Vichy, Mayor under Vichy, in occupied territory. Standard errors are clustered at the *département* level (in parentheses).

D Additional evidence on the mechanisms

D.1 Presentation - The structure of the dossier data set

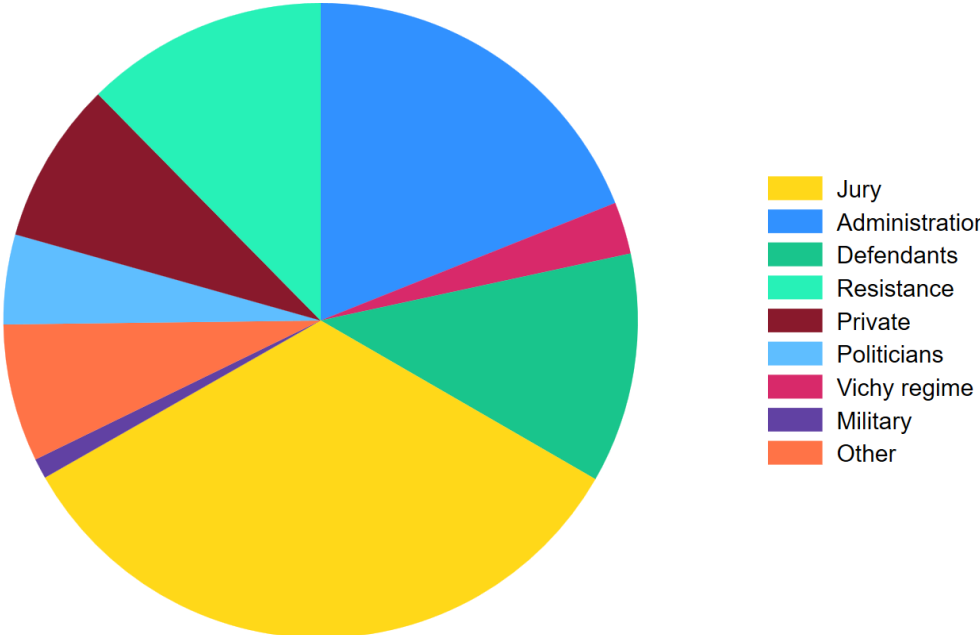
Figure D.1 presents information on the origin of letters of support. These letters represent 19.2% of the documents in a typical dossier. A third of them were private correspondence sent by the defendant’s friends, family, or by individuals in his constituency (“private”) and 30.5% originated from resistance organizations. Figure D.2 presents information on the origin of all documents in the dossiers.

Figure D.1: The origins of letters of support in the dossiers of the defendants



Note: The categories are defined as follows. Administration refers to documents produced by a ministry or a local administration. Vichy Regime refers to documents from the archives of the Vichy regime or from any Vichy-related institutions. Defendant refers to documents produced by the defendant himself. Jury refers to documents produced by the *Jury*. Military refers to documents produced by the French army. Private refers to documents 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 refers to documents sent by a lawyer using his/her title in the document sent.

Figure D.2: The origins of all the documents in the dossiers of the defendants



Note: The categories are defined as follows. Administration refers to documents produced by a ministry or a local administration. Vichy Regime refers to documents from the archives of the Vichy regime or from Vichy-related institutions. Defendant refers to documents produced by the defendant himself. Jury refers to documents produced by the *Jury*. Military refers to documents produced by the French army. Private refers to documents 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 refers to documents sent by a lawyer using his/her title in the document sent.

D.2 Definition and descriptive statistics: The dossier data set

Table D.1: Descriptive statistics - The dossier data set (Part I)

Variable name	Definition	Min	Max	Mean	s.d
General 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

Table D1 - Descriptive Statistics - Dossiers data set (Part II)

		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	0.84	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

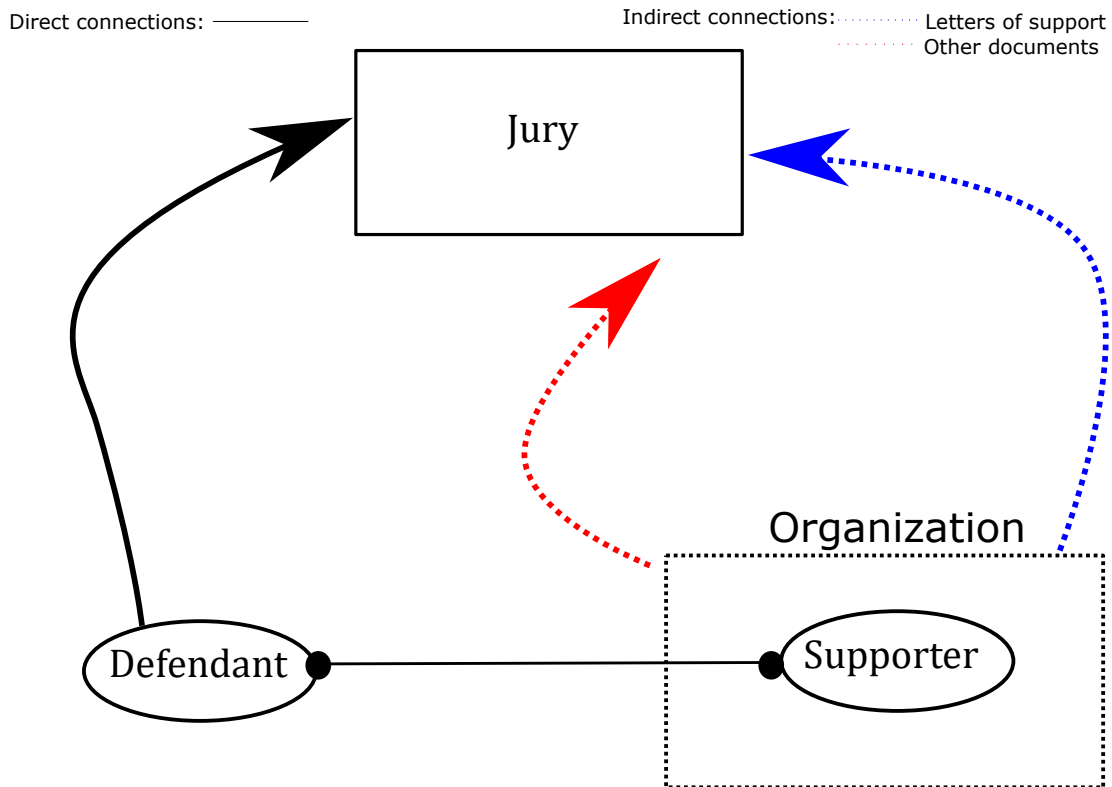
D.3 Measures of indirect connections - Definitions and illustrations

To construct the index of indirect connections, “Indirect connections via supporters”, we single out n organizations across the entire data set with $s = 1, \dots, n$ where n is the total number of organizations in the data set. We weight the share of letters in a defendant’s portfolio from organization s with the total number of documents ($NbDocs_s$) from organization s across all defendants in the entire data set. Formally, for defendant i , the index is defined as

$$\sum_{s=1}^n \left(\frac{NbLetters_{s,i}}{\sum_{s=1}^n NbLetters_{s,i}} \times weight_s \right), \quad (14)$$

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 $weight_s$ is the weight attached to organization s .⁴⁰ We set $weight_s = NbDocs_s - Letters_s$ to avoid capturing the efforts of supporters in sending letters but have experimented with alternative weights that add letters of support. It makes little difference. Figure D.3 illustrates the logic behind the index.

Figure D.3: Illustration - Measures of connections



⁴⁰In cases where a defendant did not get any letters of support, the index takes the value of 0.

D.4 Connections and difference in acquittal rate

D.4.1 The geography of the network

Figure D.4: Distance of the place of birth to Paris
Effect for LG and other defendants

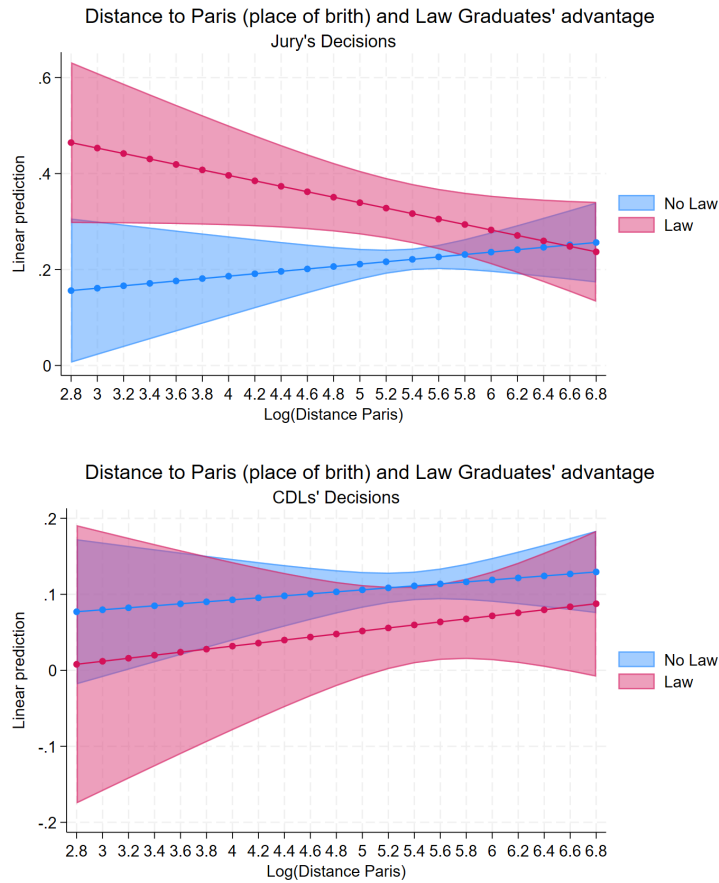


Table D.4: The probability of attending a Parisian Law University for Parisian versus non-Parisian Law graduates

Considered as treated =	T-test – Probability(Paris LG=1)		
	Treated Group	Control Group	Diff=0 (p-value)
Born <100km of Paris	0.94	0.46	0.00
Born <200km of Paris	0.84	0.44	0.00
Born <300km of Paris	0.63	0.46	0.08

Note: This table compares the probability of attending a Parisian Law University by geographical characteristics and where the MP was born. Born <100km of Paris, Born <200km of Paris, Born <300km of Paris are three dummy variables equal to one if the capital city of the birth *département* of the MP is within a 100km, 200km, 300km radius around Paris.

Table D.5: The differential acquittal probability for Parisian versus non-Parisian Law graduates

Panel A: Control group = All those considered non-treated								
Considered as treated =	CDLs			Jury			Diff-in-Diff	
	(D.5.1)	(D.5.2)	(D.5.3)	(D.5.4)	(D.5.5)	(D.5.6)	(D.5.7)	(D.5.8)
	Treated	Control	Diff=0	Treated	Control	Diff=0	$\Delta\Delta$	p-value
Group	Group	(p-value)	Group	Group	(p-value)			
All Law graduates	0.08	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 =	CDLs			Jury			Diff-in-Diff	
	Treated	Control	Diff=0	Treated	Control	Diff=0	$\Delta\Delta$	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	0.04	0.55

Note: The estimates in Panel A make use of the whole sample, i.e., they use all non-treated defendants as controls. The estimates in Panel B compares subgroups of Law graduates to non-Law graduates, i.e., they exclude non-treated Law graduates from the control group. Column D.5.1 reports the average acquittal rate of the treated group (defined in the left column) in front of the CDLs whereas column D.5.2 reports the average acquittal rate of the control group (i.e., all individuals not in the treated group) in front of the CDLs. Column D.5.3 displays the difference between these two means. Column D.5.4 presents the average acquittal rate of the treated group in front of the *Jury* whereas column D.5.5 presents the average acquittal rate of the control group (i.e., all individuals not in the treated group) in front of the *Jury*. Column D.5.6 displays the difference between columns D.5.4 and D.5.5. Column D.5.7 reports the difference-in-differences estimate from Equation (1) (without any control) of the difference in acquittal rate between each of the two subgroups of Law graduates defined in the left column and other defendants. Standard errors are clustered at the département level.

D.4.2 Connections - Parisian and non-Parisian Law graduates

Table D.6: Documents in the court dossiers: Parisian versus non-Parisian Law graduates

Control group = All considered as non-treated						
	Mean			Mean		
	(D.6.1)	(D.6.2)	(D.6.3)	(D.6.4)	(D.6.5)	(D.6.6)
	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	19.48	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

Note: The upper panel provides estimates on the whole sample. The bottom panel provides estimates of one subgroup of Law graduates compared to another. Column D.6.1 presents the averages of the various measures of document content for Law graduates from a Parisian University (Paris U); Column D.6.4 displays averages for Law graduates from other universities (not Paris U). Columns D.6.2 and D.6.5 show the averages for control groups. Columns D.6.3 and D.6.6 show the difference between averages of the treated and the control group. Standard errors are clustered at the département level.

D.4.3 Difference in the treatment of information between CDLs and the Jury - A Law graduate effect?

Table D.7: Did the CDLs treat information related to the actions and events during the war differently for Law graduate?

Dep Variable	(D.7 .1)	(D.7.2)	(D.7.3)	(D.7.4)	(D.7.5)
Sample	$Acquit_i$	$Acquit_i$	$Acquit_i$	$Acquit_i$	$Acquit_i$
	Decisions CDL	Decisions CDL	Decisions CDL	Decisions CDL	Decisions CDL
LG × Civil Res	.131 (.0723)				.133 (.0639)
LG × Military Res		.0155 (.118)			.0148 (.116)
LG × Arrested Vichy			.39 (.244)		.374 (.252)
LG × Mayor Vichy				.00883 (.0797)	.0307 (.0716)
Observations	395	395	395	395	395
Adj R ²	.0595	.11	.0566	.0657	.138
Individual controls	Yes	Yes	Yes	Yes	Yes
Dep FE + Interaction	Yes	Yes	Yes	Yes	Yes

Note: Linear probability models estimated on the sample of 395 decisions made by the CDLs. The dependent variable is the acquittal decision by the CDL for each defendant. The purpose is to assess if the CDLs reacted to information contained in the dossiers of the *Jury* in a different way for Law graduates (LG) than for other defendants. We interact the Law graduate dummy variable with four variables relating to war-related events recorded in the dossiers: participation in the civil or military resistance, arrest by the Vichy regime (Vichy), and served as mayor under the Vichy regime. Individual controls include: Age, Jewishness, Journalist, Mayor, Special Role in the Assembly, MP of an occupied territory. Standard errors are clustered at the département level.

Table D.8: Difference in the treatment of information between CDLs and the Jury

Dep Variable	(D.8.1)	(D.8.2)	(D.8.3)	(D.8.4)	(D.8.5)
Sample	All	All	$Acquit_{i,c}$	All	All
LG \times Jury	.143 (.0594)	.155 (.06)	.132 (.0532)	.147 (.0594)	.162 (.0543)
Civil Res \times Jury	.252 (.0421)				.201 (.0444)
LG \times Civil Res \times Jury	-.0401 (.0931)				-.0389 (.108)
Mili Res \times Jury		.269 (.0778)			.226 (.0775)
LG \times Mili Res \times Jury		-.00753 (.166)			.00326 (.168)
Arrested Vichy \times Jury			.239 (.11)		.238 (.108)
LG \times Arrested Vichy \times Jury			.0715 (.308)		.0782 (.278)
Mayor Vichy \times Jury				.0273 (.0584)	.0415 (.0545)
LG \times Mayor Vichy \times Jury				-.0436 (.119)	-.0148 (.114)
Observations	790	790	790	790	790
Adj R ²	.178	.237	.12	.112	.297
Controls	Yes	Yes	Yes	Yes	Yes

Note: This table estimates a version of equation (1) augmented with triple interactions of the Jury dummy variable (Jury), the Law graduate dummy variable (LG), and the different measures of participation (or non-participation) in the resistance. The sample include all decisions (i.e., both by the CDLs and the *Jury*). All regressions include LG, Jury and LG \times Jury (not reported). The purpose of the regressions is to investigate the specific reaction of the *Jury* to this type of information for Law graduates relative to other 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. Standard errors are clustered at the département level (in parentheses)

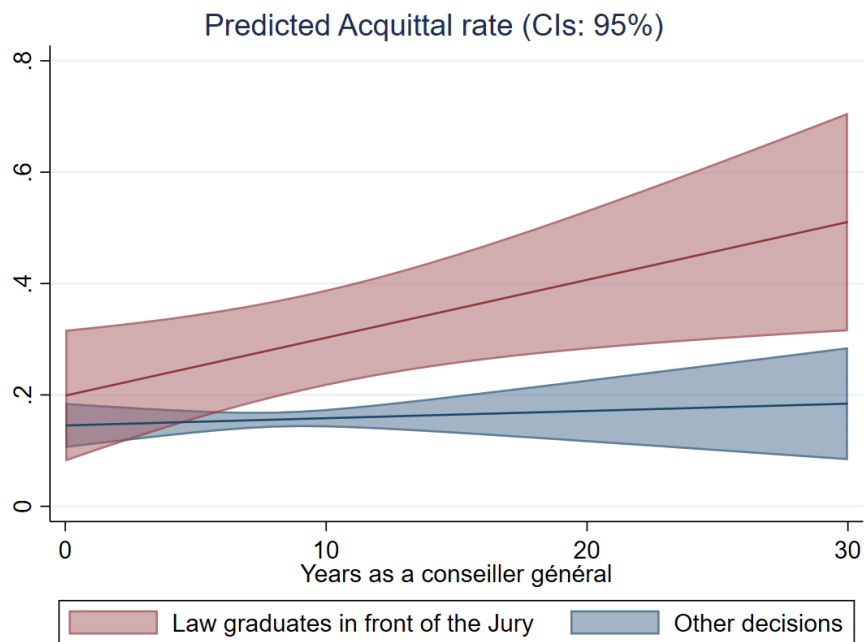
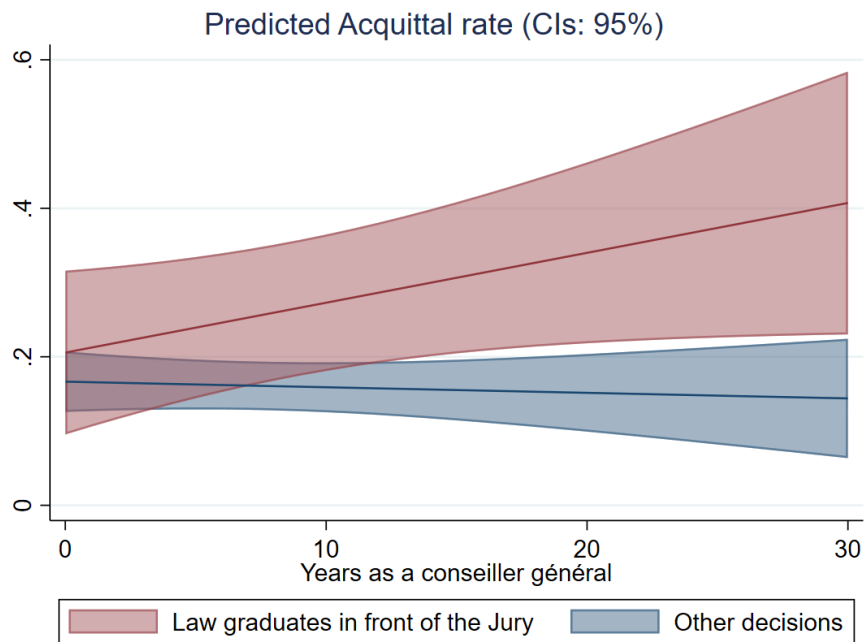
D.4.4 The higher acquittal of Law graduates is not explained by legal experience

Table D.9: Legal experience and acquittal rate

Dependent variable	(D.9.1)	(D.9.2)
	Diff-in-diff <i>Acquit_{i,c}</i>	Diff-in-diff <i>Acquit_{i,c}</i>
Jury	.155 (.0286)	-.17 (.169)
LG	-.0174 (.0383)	-.0201 (.0519)
LG × CG	-.00115 (.00309)	-.00265 (.00413)
LG × Jury	.0392 (.0598)	.0536 (.0662)
LG × Jury × CG	.00747 (.00385)	.00909 (.0047)
Observations	798	798
Adj R ²	.0399	.302
Full controls	No	Yes

Note: The table reports specifications of equation (1) augmented with a triple interaction between the length of the political career of a defendant before the war and LG × Jury. The hypothesis is that a long political career crowds out legal skills. Column D.9.2 adds the full set of 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. Standard errors are clustered at the département level. (in parentheses)

Figure D.5: Interaction plots: the probability of acquittal as a function of years of service as a local councilor



Note: The two figures exhibit interaction graphs based on the estimates in Table D.9, columns D.9.1 (upper graph) and D.9.2 (lower graph). The red line is for Law graduates in front of the *Jury*; the blue line is for other decisions (related to non-Law graduate defendants in front of the *Jury* and CDLs).

D.5 Correlation Matrix - Measures of indirect connections

Table D.10: 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 not Jury (“Dear” Letters)	0.54	0.52	0.37	0.35	0.98	\

E Elite persistence after the purges

E.1 Estimates of persistence

For the difference in acquittal rates between Law graduates and other defendants to have contributed to elite persistence, it must have helped the future careers of the politicians who benefited from it. Decisions of the *Jury* would have been inconsequential if acquitted Law graduates never ran for election or if voters did not elect them. In this section, we show that the decisions of the *Jury* were consequential for the post-war careers of defendants and that Law graduates were more likely than other defendants to pursue a career in politics after being cleared by the *Jury*. Table E.1 reports a series of regressions in which measures of each defendant's post-war political career (such as the number of times he ran in municipal and legislative elections, whether he was mayor, held a seat in parliament or had ministerial responsibilities) are explained by the court decisions (Panel A) or by being a Law graduate (Panel B). We find very similar results when looking at the intensive margin of the effect in Table E.2.

Table E.1: Elite persistence - the *Jury*'s decisions and Law graduates

Panel A - Independent variable: Acquitted _{Jury}					
	(E.1.A.1) Nb mayor election	(E.1.A.2) Nb deputy elections	(E.1.A.3) Mayor=1	(E.1.A.4) Parliament=1	(E.1.A.5) Minister=1
Acquitted _{Jury}	.189 (.071)	.253 (.0705)	.116 (.0515)	.225 (.0526)	.0701 (.0309)
Acquitted _{CDL}	.0431 (.097)	.0735 (.1)	.0115 (.064)	.0553 (.0771)	.00894 (.0399)
Observations	397	399	399	399	399
Adjusted R ²	.172	.156	.166	.157	.0671
<u>Control variables</u>					
Individual	YES	YES	YES	YES	YES

Panel B - Independent variable: Law graduate					
	(E.1.B.1) Nb mayor elections	(E.1.B.2) Nb deputy elections	(E.1.B.3) Mayor=1	(E.1.B.4) Parliament=1	(E.1.B.5) Minister=1
LG	-.0352 (.0438)	.105 (.0476)	-.0107 (.0348)	.0761 (.0348)	.0318 (.0181)
Acquitted _{CDL}	.121 (.0977)	.188 (.0961)	.0597 (.0614)	.156 (.0829)	.0408 (.0374)
Observations	397	399	399	399	399
Adjusted R ²	.153	.125	.151	.11	.0436
<u>Control variables</u>					
Individual	YES	YES	YES	YES	YES

Note: Level of observation: Defendant. In Panel A, Acquitted_{Jury} is a dummy variable equal to 1 if the defendant was acquitted by the *Jury*. In Panel B, Law graduate is a dummy variable equal to one if the defendant was a Law graduate. Acquitted_{CDL} is a dummy variable equal to one if the defendant was acquitted by a CDL. Dependent variables: Nb mayor elections is the number of times a defendant was a candidate in a municipal election after the war. Nb deputy elections is the number of times a defendant was a candidate in a legislative election after the war. Mayor=1 is a dummy variable equal to one if a defendant was elected mayor after the war and zero otherwise. Parliament=1 is a dummy variable equal to one if a defendant was a deputy or a senator after the war and zero otherwise. Minister=1 is a dummy variable equal to one if the defendant was a minister after the war and zero otherwise. Individual controls include: Age and 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 Vichy). Standard errors are clustered at the département level.

Table E.2: Persistence and the *Jury's* decision - the intensive margin

Panel A - Independent variable: Acquitted by the Jury			
	(E.2.A.1)	(E.2.A.2)	(E.2.A.3)
	Years as Mayor	Years in Parliament	Years as Minister
Acquitted _{<i>Jury</i>}	.271 (.135)	.366 (.104)	.0695 (.0336)
Acquitted _{<i>CDL</i>}	-.0373 (.163)	.117 (.149)	.0344 (.0611)
Observations	399	399	399
Adjusted R ²	.155	.137	.0508
<u>Control variables</u>			
Individual	YES	YES	YES

Panel B - Independent variable: Law graduates			
	(E.2.B.1)	(E.2.B.2)	(E.2.B.3)
	Years as Mayor	Years in Parliament	Years as Minister
LG	-.016 (.0847)	.181 (.071)	.0341 (.0247)
Acquitted _{<i>CDL</i>}	.0763 (.159)	.284 (.155)	.0662 (.0607)
Observations	399	399	399
Adjusted R ²	.142	.115	.0378
<u>Control variables</u>			
Individual	YES	YES	YES

Note: 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 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 Vichy). Standard errors are clustered at the département level.

E.2 Willingness to continue in politics and the differential acquittal rate of Law graduates in front of the *Jury*.

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. Table E.3, columns E.3.1 and E.3.2 report separate estimates of equation (1) for the two groups. We observe that the difference in acquittal rate between Law graduates and other defendants is only significant, at the ten-percent level, for the group of defendants who ran in the 1945 municipal election. Moreover, Column E.3.3 reports a specification estimated on the full sample in which equation (1) 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 13 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 an election. 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 difference in acquittal rate between Law graduates and other defendants before the *Jury* was larger 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 (1) with interaction terms between the pre- and post-reform dummies and $LG_i \times Jury_c$, respectively, to allow the coefficient β_1 to differ depending on when the case was heard. In Appendix Table E.3, columns E.3.4 to E.3.6 present the results. The result in column E.3.4 shows that the difference in acquittal rate between Law graduates and other defendants 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.⁴¹ 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 the effect is not driven by time trends in the sentences of the *Jury*.⁴² The difference in acquittal rate between Law graduates and other defendants before the *Jury* therefore facilitated elite persistence, as it materialized specifically when a Law graduate intended to continue his political career.

Table E.3: Higher acquittal of Law graduates when it matters: Electoral litigations

Dep variable	(E.3.1) <i>Acquit_{i,c}</i>	(E.3.2) <i>Acquit_{i,c}</i>	(E.3.3) <i>Acquit_{i,c}</i>	(E.3.4) <i>Acquit_{i,c}</i>	(E.3.5) <i>Acquit_{i,c}</i>	(E.3.6) <i>Acquit_{i,c}</i>
Sample	Ran for elections	Did not run	All	All	All	All
LG	-.221 (.448)	.000193 (.0405)	-.0111 (.04)			
Jury × LG	.888 (.146)	.0751 (.052)	.0733 (.0526)			
Jury × LG × Ran for elections			.558 (.195)			
Jury × LG × Pre-reform				.381 (.136)	.332 (.122)	.369 (.128)
Jury × LG × Post-reform				.0927 (.0517)	.114 (.0516)	.133 (.0496)
Observations	66	658	790	790	784	784
Adjusted R ²	.289	.311	.303	.308	.349	.359
Individual Controls	Yes	Yes	Yes	Yes	Yes	Yes
Dep FE + Interaction	Yes	Yes	Yes	Yes	Yes	Yes
Polynomial Date					3	3
Pre-reform dummy						Yes
Wald Test				0.06	0.12	0.10

Note: Column E.3.1 estimates equation (1) on the subset of defendants who ran for the first post-WWII mayoral elections. Column E.3.2 estimates equation (1) on the subset of defendants who ran for the first post-WWII mayoral elections. Column E.3.3 investigates how the magnitude of the coefficient β_1 varied with their participation in the first post-WWII elections by adding a triple interaction term (Jury × LG × Ran for elections) and controlling for the interaction Jury × Ran for elections. It controls for the variables not interacted in the estimation (Jury, LG, and Ran for elections). Columns E.3.4 to E.3.6 estimate equation (6) 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. This test assesses whether the reform of the remit of the Jury from mainly electoral litigations to investigations of all cases impacted the time trend of our estimates of the difference in acquittal rates between Law graduates and other defendants. Individual controls include: Age, Jewishness, Journalist, Mayor, Special Role in the Assembly, Civilian Resistance, Military resistance, Arrested by by the Vichy regime, Mayor under Vichy, parliamentarian of an occupied territory. Each individual control is interacted with the Jury dummy variable. Standard errors are clustered at the département level (in parentheses).

⁴¹To 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.

⁴²Part 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).

E.3 Differences between acquitted Law graduates and other acquitted defendants

Table E.4: Differences between acquitted LG and other acquitted politicians

	All cleared defendants			Not cleared by CDL		
	Cleared LG	Cleared Other	Δ	Cleared LG	Cleared Other	Δ
WWI War Medal	0.50	0.29	-0.21	0.52	0.27	-0.25
Mandate (years)	14.24	9.89	-4.34	13.67	9.71	-3.96
Conseiller général (years)	11.88	6.86	-5.01	11.26	5.66	-5.60
Deputé (years)	10.38	7.76	-2.62	9.44	8.15	-1.30
Senator (years)	3.85	2.14	-1.72	4.22	1.56	-2.66

Note: WWI War Medal is a dummy variable equal to one if the defendant was awarded a war medal for his action during World War II. Mandate (years) is the number a defendant was in parliament (either as a Député or as a Senator). Conseiller général (years) is the number of years a defendant has been a conseiller général (member of départemental assemblies). Deputé (years) is the number of years a defendant acted as Deputé and Senator (years) is the number of years a defendant acted as Senator.

F Descriptive statistics - Different data sets

F.1 Descriptive statistics related to defendant characteristics

Table F.1: Descriptive statistics - Defendant characteristics (Part I)

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
MP elected in Paris	0.05	0.22	0	1
MP 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 MP	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	0.49	0	1
War Medal	0.38	0.49	0	1
Veterans club	0.05	0.21	0	1

Table F.1: Descriptive statistics - Defendant characteristics (Part II)

Variable	Mean	s.d	Min	Max
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 Vichy	0.27	0.45	0	1
Arrested by Vichy	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

F.2 Descriptive statistics related to dossier data set

Table F.2: Descriptive statistics - the dossier data set

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	6.14
Defendant	2061	0	62	4.76	6.27
Administrations	3335	0	48	7.70	6.00
Politicians	804	0	26	1.86	3.26
Resistant Organizations	2176	0	45	5.03	5.6
Journal Officiel	441	0	3	1.02	0.38
Press	251	0	24	0.58	1.96
Private	1452	0	70	3.35	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	1.08	2.39
Others	54	0	5	0.12	0.52
Type of documents					
Letters of support	3385	0	90	7.82	12.23