

# Correction Activities by French Supreme Courts and Control over their Dockets

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February 21, 2016

## Abstract

This paper aims at investigating correction activities of Supreme Courts *vis-à-vis* lower courts' decisions. More precisely, our paper deals with the experience of the two French Supreme Courts: the *Conseil d'État*, which supervises the administrative branch, and the *Cour de Cassation*, which deals with civil cases. Both courts have experienced, at different dates though, a reform that gave them discretionary control over their dockets. Our paper investigates whether the correction activities are similar across Supreme Courts, and whether these activities are affected when Supreme Courts can select cases. Using an original database of all environmental cases decided by Supreme Courts between 1956 and 2010, we rely on a counterfactual approach to compare cases before and after the reforms across courts. Our investigation concludes that correction activities do not differ across courts, as long as courts have the same selection rule. We also find that Supreme Courts use the possibility of selection to increase their pro-plaintiff correction activities.

JEL classification: K32, K41

Keywords: selection bias, bias correction, French cases, litigation, appeal process, Appellate Court, Supreme Court, Administrative Law, judicial reform.

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\*We thank participants in the *Mannheim Law and Economics Forum* (MaLEF seminar), University of Mannheim (November 2014), the *31st Annual Conference of the European Association of Law and Economics* (EALE), Aix-en-Provence, France (September 2014), the *First Annual Conference of the French Association of Environmental and Resource Economists* (FAERE), Montpellier (September 2014), the *64th annual Meeting of the French Economic Association* (June, 2015), the *2nd International Meeting in Law and Economics* (June 2015). The research leading to these results has received funding from the Institut Universitaire de France (IUF).

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# 1 Introduction

The existence of Supreme Courts is very often justified by the need to harmonize law enforcement. The heterogeneity of legal decisions within a single legal framework is often perceived as a threat to equality before law: litigants should expect to face the same rules enforcement independently of the judge they face at trial. In this regard, the hierarchical structure of the judiciary, dominated by Supreme Courts, can be considered as an appropriate solution: litigants can challenge decisions of lower instance courts, and this gives Supreme Courts' judges the possibility to reverse legally inappropriate decisions. Supreme Courts have therefore the opportunity to correct decisions of appellate courts due to improper enforcement of the law, to clarify unclear legal issues, and, sometimes, to limit judicial activism. In the light of this hierarchical structure, the goal of Supreme Courts is mainly to *correct* decisions taken by appellate courts.

To investigate the correction activities of Supreme Courts, we evaluate 614 Civil and Administrative Supreme Courts' decisions in the field of environmental accidents from 1956 to 2010, and test for a difference of treatment across courts. We run several probit estimations to assess the impact of Supreme Courts on defendants' win rates. Following Shavell (1995), we draw the hypothesis that Supreme Courts can engage in correction activities with respect to lower courts' errors and biases, especially if Supreme Courts have a discretionary control over their dockets and can focus on potentially biased decisions from Appellate Courts (Eisenberg et al. (2012)). Hence, the greater the discrepancy between the lower courts' and the Supreme Courts' preferences, the more the latter are expected to engage in correction activities and, therefore, to reverse the appellate courts' decisions.

Previous investigations of Supreme Courts' decisions have emphasized the importance of considering case selection (Kastellec and Lax (2008)).<sup>1</sup> Indeed, the case selection process, whereby Supreme Courts can choose the cases they hear, lead to biased observed samples. As case selection strategies are usually not publicized, any inference from case outcomes is thus potentially flawed. In our study, selection bias is a major issue as the two Supreme Courts have been allowed to select cases, though at different dates.

Our investigation consists in two steps. First, our study aims at comparing the correction behavior of both Supreme Courts. This first part seeks to figure out whether the Administrative Supreme Court (*Conseil d'État*) engages in more correction activities of the appellate courts' decisions than the Civil Supreme Court (*Cour de Cassation*). This allows to indirectly assess the relative distance in the courts' preferences between the appellate courts and their respective Supreme Court. Second, we propose to investigate whether Supreme Courts, when they are

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<sup>1</sup>Selection biases have different sources which can go back to the origins of a case. It is generally admitted that the set of trials which take place in a lower court is not necessarily representative of the whole set of potential trials. Some case categories can be favored by the plaintiffs if the latter estimate to have more chances of being successful. Even the set of plaintiffs (and thus of parties) may not be fully representative of the set of potential plaintiffs (and parties): the decision to go on trial can depend on the nature of the plaintiff (an individual, a firm), the level of education, the knowledge of the legal system and the level of wealth. Biases can be created at the origin of a trial, and propagate along the appeal processes until Supreme Courts' level: in theory, some biases can be created at each stage due to factors affecting the decision of appellants to file the case.

allowed to select cases, change their correction activities vis-à-vis lower courts' decisions.

The paper shows that, on the entire period, the Administrative Supreme Court engages in more correction activities than the Civil Supreme Court. However, decomposing the whole period according to the selection capacity of each court, we find that Supreme Courts' correction activities are similar in their intensity when both courts share the same selection mechanism.

The rest of the paper is organized in the following way. Section 2 reviews the relevant literature. Section 3 depicts the legal background and Section 4 describes the data, presents some statistics and preliminary econometric results. Section 5 develops our econometric strategy to assess the correction activities of each Supreme Court over time. Section 6 concludes.

## 2 Prior Literature

Two strands of literature relate to our paper. A first body of works has investigated whether judges' characteristics and their environment impact their decisions. We refer to this literature as the study of judges' biases. A second set of papers is dedicated to the methodology and the biases relative to the empirical study of case outcomes and courts' decisions. We refer to this literature as the study of selection biases.

Over the last decades, judges' biases have been abundantly studied. Indeed, some authors have investigated the effects of gender (Choi et al. (2011), Greiner and Rubin (2011), Bogoch and Don-Yechiya (1999)), religion (Gazal-Ayal and Sulitzeanu-Kenan (2010)), race (Shayo and Zussman (2011)), or judges' political preferences (Choi and Gulati (2008), Lambert-Mogiliansky et al. (2006), Ashenfelter et al. (1995), Hall (2010), Amaral-Garcia et al. (2009), Franck (2009), Martin et al. (2005), Spiller and Gely (1992), Espinosa (2015), Desrieux and Espinosa (2015)) on case outcomes. Some others have studied the impact of the socio-economic environment on judges' decisions. For instance, Ichino et al. (2003) observe that Italian Labor Courts are more favorable to employees when unemployment is high, whereas Marinescu (2003, 2011) concludes the opposite effect for the French and British Labor Courts respectively.

A second comprehensive stream of research has dealt with the inequality of treatment resulting from the composition of the courts or from the identity of the parties themselves. Clermont and Eisenberg (1992) show that plaintiffs' success rates significantly differ before judges and juries, since attorneys select more difficult cases when pleading before juries. In two other papers, Clermont and Eisenberg (2001), Clermont and Eisenberg (2002) show that defendants succeed more than plaintiffs on appeal from civil trials, explaining it by appellate judges' attitude. This difference has also been supported by Eisenberg (2004) and Eisenberg and Heise (2009). However, the fact that plaintiffs tend to appeal cases even when their chances of success are very low might also explain the apparent pro-defendant bias (Eisenberg and Farber (2003), Eisenberg and Farber (2013)). Eisenberg et al. (2011) confirm this last point by showing that there is no asymmetric reversal rates favoring defendants in Israel Supreme Court's appellate cases. The closest paper to ours, Amaral-Garcia and Garoupa (2012), tests for a pro-government bias in Administrative

Courts in the field of medical malpractice in Spain. Studying the Supreme Court's decisions to compare civil and administrative appellate courts' ones, they conclude that there is no evidence of such a pro-government bias in Administrative Courts.

Notwithstanding the progresses done in the quantitative analysis of courts' decisions and judges' preferences, most of the authors fail to take into account biases that result from case selection.<sup>2</sup> Indeed, investigations of courts' decisions might be flawed by several biases. First, a great share of cases are settled: consequently, inferences on courts' decisions might under- or overestimate the impact of the exogenous variables if they also affect the settlement decision (Priest and Klein (1984), Eisenberg (1990)). In the same vein, decisions to appeal are not random and, therefore, case outcomes in Appellate Courts do not necessarily reveal the actual severity -and potential biases- of lower courts' judges. In other words, selection biases limit the possibility to infer from studies on case outcomes general conclusions on judges' decisions and preferences, especially if one only focuses on the identity of the winning party (Clermont and Eisenberg (1998)). Another important selection bias occurs when only a fraction of the whole cases is published (Donohue and Siegelman (1990), Merritt and Brudney (2001), Law (2005)) as the choice to publish a decision may not be random.

Finally, the study of Supreme Courts' decisions is even more sensitive to selection biases: Supreme Courts primarily focus on the most complex cases (Kritzer and Richards (2002)), which are not representative of the whole set of cases ruled by lower courts (Cross (1997), Friedman (2006)). Thus, one should be cautious when inferring general conclusions from the study of Supreme Courts' case outcomes. The reason for such selection biases is twofold. First, as for Appellate Courts, appeals to Supreme Courts are costly and not random. Second, most Supreme Courts have some discretionary control over their dockets and, therefore, can select the cases they review without explaining their selection strategy.<sup>3</sup> Hence, it is fundamental to cope with discretionary cases selection before drawing any inference about Supreme Courts' preferences and potential biases (Harvey and Friedman (2009), Eisenberg et al. (2012), Eisenberg et al. (2014)), as Supreme Courts' selection biases may have severe impacts on observed statistical results (Kastellec and Lax (2008)).

Although a growing number of scholars recognize the importance of case selection and develop different statistical methods to control for it,<sup>4</sup> the impact of selection bias on correction activities

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<sup>2</sup>For instance, concerning Labor Courts' analysis, Macis (2001) and Marinescu (2003) do not take into account *out-of-court settlements* and work with a sample biased by cases selection since the choice to settle is not random. In the same vein, in their study of judges' ideology and its impact on case outcomes, Sunstein et al. (2006) only focus on published decisions. However, Keele et al. (2009) show that the choice to publish a decision is not random and therefore samples restricted to published cases are biased. Finally, some authors do not even mention the existence of sample and selection biases (e.g. Epstein and Martin (2010), Amaral-Garcia and Garoupa (2012)).

<sup>3</sup>See for instance Eisenberg et al. (2011), Eisenberg et al. (2012), Eisenberg et al. (2014), and Eisenberg and Huang (2012).

<sup>4</sup>For instance, Eisenberg and Huang (2012) use a reform of Taiwan Supreme Court allowing for discretionary selection of cases as a quasi-natural experiment to observe judges' selection strategies. Eisenberg et al. (2014) compare discretionary and mandatory jurisdictions in Israel Supreme Court to assess case selection. Hall (2010) uses random judicial assignment to control for selection biases when assessing the effect of Appellate Judges' partisanship on their decisions. Eisenberg et al. (2012) use key covariates to account for non-random aspects of case assignment. Finally, in a recent paper, Boyd et al. (2010) use a more sophisticated method based on propensity score matching to deal with non-random assignment of cases among male and female judges.

has almost never been assessed. More importantly, no study, to our knowledge, analyzes how correction activities are affected by case selection rules. This is the gap we aim to fill in.

### 3 The French legal background: A quasi experiment for identifying selection biases

#### 3.1 The French legislation: Two different jurisdictions

In France, as in many civil law countries, cases involving the state as one of the litigants are dealt by separate courts. Indeed: disputes between private parties only are dealt by civil jurisdictions, whereas cases involving the state are decided by administrative courts. More precisely, in environmental cases, a dispute will be tried in administrative courts if the defendant is a public legal person, a state-owned company or a private company entitled to provide public services and exercising an administrative authority.<sup>5</sup> In the absence of this administrative authority, a private company providing public services will be sued in civil courts.<sup>6</sup> Hence, environmental administrative litigations may concern either a controversy over a decision of a state official (e.g. authorization of starting a potentially environmentally unfriendly activity, implementation of a controversial local regulation, or stringency of an environmental impact assessment) or a dispute over an action of a public defendant that resulted in damages to private plaintiffs.

The main reason of such a distinction between private and public defendants lies in the fact that "*public authorities have specific powers and obligations that require that their action should not be reviewed by ordinary courts*" (Frydman (2008)).

Consequently, the determination of liability is different in administrative and civil courts. In Civil Law, fault-based liability is the rule (except for the most environmentally unfriendly facilities called ICPE facilities,<sup>7</sup> which are subject to strict liability) and the criterion for negligence is the "reasonable man standard" (*Bon père de famille*). In Administrative Law, the standard of care is much higher as state-owned companies and state officials have an obligation of sanitary security<sup>8</sup>, and cannot claim that their level of care was limited by a budget constraint.<sup>9</sup> Furthermore, the procedures are also different. The procedure before administrative courts is inquisitorial ("*inquisitoire*") whereas the civil procedure is accusatorial ("*accusatoire*"). This means that ad-

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<sup>5</sup>Private companies with a public service mission are subject to Administrative Law if they are entitled to take administrative decisions, i.e. they benefit from a "*prérogative de puissance publique*" (see *Arrêt Magnier in CE January 1961, 13th, and CE May 1991, 15th, Association Girondins de Bordeaux FC*).

<sup>6</sup>*Tribunal des Conflits*, November 1995, 27th, *Le Troedec*, and *Arrêt Temier in CE February 1903, 6th*.

<sup>7</sup>*Installations classées pour la protection de l'environnement*.

<sup>8</sup>The first reference to this obligation appeared in 1902 (Law of February 1902, 15th, relative to the protection of public health). The criterion has become more stringent over the century as many public health and environmental scandals occurred. For instance, the State has been condemned for "public health deficiencies" in the HIV-contaminated blood affair (CE April 1992, 9th, *n.138653*) and in the asbestos affair (CE March 2004, 3rd *n.241153*). For a detailed analysis of the evolution of the severity of the precautionary principle in French environmental law, see Bentata and Faure (2012).

<sup>9</sup>Cass. Crim., July 2nd 1998, *n.97-83.286*.

ministrative courts direct the course of the procedure and are in charge of finding out the facts that may be relevant for their decisions (Frydman, 2008).<sup>10</sup> Consequently, administrative judges have more room than civil judges to take their decisions.

### 3.2 The appeal process: A common feature

Despite some different rules, civil and administrative jurisdictions have a similar organization. Both are pyramidal with the *Cour de Cassation* and the *Conseil d'État* at the apex of the civil and the administrative branches, respectively. Civil and administrative cases are first tried respectively in *Cours d'Instance* and in *Cours Administratives d'Instance*, and can be appealed in *Cours d'Appel* and in *Cours Administratives d'Appel*. As far as litigations are concerned,<sup>11</sup> the *Cour de Cassation* and the *Conseil d'État* share a common feature: both have to harmonize case law to ensure that texts are interpreted in the same way all over the country.<sup>12</sup> Moreover, they do not rule on the merits of a case, but rather on the proper application of the rules by lower courts (i.e. both Supreme Courts rule on the decisions of lower courts). Hence, even though administrative and civil rules might differ, the task of judges from both Supreme Courts is similar. From this common role, it follows that comparing decisions of both Supreme Courts makes sense to the extent that cases are comparable. Controlling for the characteristics of the case, a difference in the severity of across Supreme Courts could indicate some heterogeneous correction behaviors: Supreme Courts' judges may seek to correct, though at different intensities, the systematic mistakes of the lower courts resulting from their preferences.

### 3.3 The potential for a pro-defendant bias in Administrative Courts

In France, Administrative and Civil Laws are considered as quite different branches of law, with their own logic and their own process. For this reason, administrative and civil judges often have very different backgrounds. Civil judges have a special statutory protection (referred to as *Magistrat*). To become civil judges, candidates have to attend the National School for the Judiciary (*École Nationale de la Magistrature*) for a period of 31 months. There are three different competitive examinations depending on the professional experience of the candidate: the first one is open to students with a Master degree in Law who are at least 27 years of age. The second and third ones are open to candidates who already have a strong legal experience, with at least seven years of experience. Most of the civil judges come from the first examination process.<sup>13</sup>

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<sup>10</sup>These differences between Administrative and Civil Law in the severity of the rules but also in the procedures renders the analysis quite uneasy, even with some control over a number of observable variables. For this reason, we do not give any definite conclusion in the end of Section 4, with the first regressions. Section 5 is precisely dedicated to offer some deepened analysis by providing some control over unobservable variables.

<sup>11</sup>Indeed, the *Conseil d'État* exercises two different roles: it is not only the Supreme Court of the administrative jurisdiction but also the most important legal advisor to the Government.

<sup>12</sup>See About the Court, Cour de Cassation, [www.courdecassation.fr/about\\_the\\_court\\_9256.html](http://www.courdecassation.fr/about_the_court_9256.html).

<sup>13</sup>For instance, in 2006, 88% of newly graduated judges entered the National School for the Judiciary through the first examination process. See the statistics provided by the *Cour de Cassation* on its website: [http://www.courdecassation.fr/IMG/File/pdf\\_2007/10-05-2007/10-05-2007\\_mcKee\\_fr.pdf](http://www.courdecassation.fr/IMG/File/pdf_2007/10-05-2007/10-05-2007_mcKee_fr.pdf).

As a result, the majority of successful candidates begin their professional careers as civil judges, and most of them remain civil judges until retirement.

The picture for the administrative branch is more complex. First of all, administrative judges are usual civil servants and not *Magistrats*. In this regard, they do not benefit from protections that guarantee the independence of *Magistrats* from the State. Second, except for judges from the *Conseil d'État* who, for the majority, attended the National School of Administration (*École Nationale d'Administration*) after a strong competitive examination, administrative judges are recruited among civil servants, lawyers and high level law graduates. This phenomenon is explained by the relative low number of students who intend to become administrative judges and the increasing need of administrative judges in the recent decades. As a matter of fact, over the last decade, only 19% of the new administrative judges did not exercise as civil servants in the past.<sup>14</sup> This means that 81% of the new administrative judges used to work in close relationship with state officials and local authorities, *i.e.* precisely the parties they may have to judge once in charge. This situation is favorable to the emergence of sympathy towards a group (the group of public agents) to whom administrative judges have previously pertained. Such a context can potentially give rise to a pro-defendant bias.

Unlike lower administrative courts, judges sitting at the *Conseil d'État* mainly come from a devoted school. Each year, five positions of auditors (*Auditeurs*) are made available to the top graduates of the National School of Administration. After four years, an auditor is promoted to master of petitions (*Maître des requêtes*) and, after twelve years, to the level of judge (*Conseiller d'État*). Promotion is based exclusively on seniority which ensures independence and impartiality in the promotion of members. Recruitment by external appointment accounts for one out of four masters of petitions and for one out of three judges. A number of external appointments, upon the nomination of the Vice-President of the *Conseil d'État*, is reserved for members of the administrative tribunals and the administrative appellate courts.<sup>15</sup> Similarly to the lower administrative courts, one could question the impartiality of these judges towards the state. Judges who were appointed from the lower courts potentially suffer from the same pro-state biases. Moreover, judges coming from the National School of Administration might also be biased towards the state: their choice to devote their career to the public administration might reflect preferences favorable to the state.

### 3.4 The potential for a selection bias in both Supreme Courts

As most of the Supreme Courts in other countries, the *Cour de Cassation* and *Conseil d'État* have some control over the cases filed that allow them to select the ones that will actually be treated.<sup>16</sup>

Considering this discretionary control, any interpretation of econometric results on courts' deci-

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<sup>14</sup>See the statistics provided by the *Cour de Cassation* on its website:  
[http://www.courdecassation.fr/IMG/File/pdf\\_2007/10-05-2007/10-05-2007\\_recrutement\\_adm.pdf](http://www.courdecassation.fr/IMG/File/pdf_2007/10-05-2007/10-05-2007_recrutement_adm.pdf).

<sup>15</sup>See the website of the *Conseil d'État*: <http://english.conseil-etat.fr/>.

<sup>16</sup>See Eisenberg et al. (2011). In France, 30% of the cases filed are declared "non-eligible" and rejected without publication of the underlying reasons in the legal databases.

sions should be done carefully. Indeed, judges' decisions can change as time passes, or because of external circumstances, or because selected cases have themselves changed. In the present study, the comparison of judgments by both courts can be distorted when they do not select similar cases. If both courts do not select cases in the same way, then a comparison of final decisions will not allow to determine whether a court is more severe than the other against defendants, or whether the initial selection was different. In other words, the possibility of selecting the cases to be treated increases the confusion between judges' preferences and selection bias.

For this reason, the selection bias is often ignored or simply mentioned in the empirical works analyzing Supreme Courts' decisions. However, the French situation is particularly interesting in this respect, since reforms on cases eligibility criteria have occurred that allow to distinguish - at least partly - different biases. Indeed, the suppression of the filter designated as *Chambre des Requêtes* in 1947 has forced the *Cour de Cassation* to motivate its decisions whether to treat or not the filed cases. This heavy process has triggered some blocking of the court and an increase in the delay for judgment. For this reason, the organic law of June 2001, 25th,<sup>17</sup> has restored the preliminary screening of files, allowing to declare as non-eligible a case without having to provide any detailed motivation for this rejection.<sup>18</sup> This law has been enforced in January 2002, 1st.<sup>19</sup> Hence, for the period 1947 to 2001, the *Cour de Cassation* could not select the cases to be treated.<sup>20</sup> The selection bias - at the *Cour de Cassation's* level - can only concern the cases after 2001.<sup>21</sup>

A similar reform has occurred for the *Conseil d'État* for which the preliminary screening of the cases has been restored on January 1989, 1st.<sup>22</sup> Hence, cases judged before 1989 constitute a complete and non-biased sample of the cases treated by the *Conseil d'État*. Only the cases judged since 1989 can present the selection bias we isolate.

## 4 Database

In order to investigate the correction behavior of the Supreme courts together with the potential selection effects induced by the reforms, we study the entire set of decisions of the *Cour de Cassation* and the *Conseil d'État* concerning environmental accidents and damages between 1956 and 2010. Our database is constructed on two French official legal engines that list all cases before

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<sup>17</sup>Loi 2001-539 révisant l'Art. L 131-6 du *Code de l'Organisation Judiciaire*.

<sup>18</sup>See [http://www.courdecassation.fr/cour\\_cassation\\_1/autres\\_publications\\_discours\\_2039/publications\\_2201/admission\\_pourvois\\_cassation\\_8424.html](http://www.courdecassation.fr/cour_cassation_1/autres_publications_discours_2039/publications_2201/admission_pourvois_cassation_8424.html).

<sup>19</sup>Art.28, Loi 2001-539.

<sup>20</sup>From a practical viewpoint, this means that legal databases collecting all the decisions taken in the *Cour de Cassation* do not present any selection bias - at the court's level - for this period. Appeals have been judged unacceptable or not motivated by serious reasons: in any case, the decision has been motivated by the court and codified in the databases.

<sup>21</sup>The selection bias that we manage to isolate in this paper is the one that occurs from the choice of cases to be treated by the Supreme Courts. This selection bias is potentially important, as explained earlier, but it is not the only possible one. Our approach allows to isolate and quantify this selection bias, but not the ones that take place earlier in the history of a particular case, i.e. before the appeal at the Supreme Court's level.

<sup>22</sup>Art.16, Loi 87-1127 du 31 décembre 1987.

the *Cour de Cassation* and the *Conseil d'État* since 1956.<sup>23</sup>

To collect the entire set of environmental cases, we have used the following keywords: pollution, *trouble de voisinage* (nuisance to neighborhood), environmental damages, environmental risk, environmental loss, ecological risk, ecological loss, ICPE,<sup>24</sup> Seveso, IPPC,<sup>25</sup> and risk prevention. We have obtained a total of 614 different cases.

In the following subsections, we present the variables of interest and a set of control variables. All variables are dummy variables coded "1" when present in cases and "0" otherwise.

The dependent variable is the decision of the court ("pro-defendant decisions", designated as *Prodef*), equal to 1 when judges rule in favor of the defendant and 0 otherwise. This variable measures the defendants' chances of success.

Among the explanatory variables, the identity of the court is the main variable of interest. It is designated as *CE*, and takes value 1 if the case is judged by the *Conseil d'État* and 0 if litigated by the *Cour de Cassation*. The econometric analysis greatly relies on this variable in order to determine whether the court's identity influences the outcome of the appeal, and thus, whether the correction behavior differs across courts.

In order to take into account the specificity of each case, we use various control variables. The most important is the identity of the appellant, designated as *Defappeal*, taking value 1 if the defendant filed the appeal before the Supreme Court and 0 if the victim did it.<sup>26</sup> From a statistical viewpoint, this variable coefficient measures the trend of judges to satisfy the party who files the appeal, whoever is this party.<sup>27</sup> This variable allows to have some control over the "affirmative" or "infirmative" behavior of the court. This variable has to be taken into consideration. Indeed, in our database, 63.52% of the cases are filed by defendants and 36.48% by plaintiffs. An "infirmative" behavior will have a significant positive effect on the variable *Prodef*, whereas a "confirmative" behavior will have a significant negative effect on *Prodef*.

Environmental lawsuits may be brought for different reasons. Legal grounds describe the legal basis used by the appellant to get her case to the *Cour de Cassation* or the *Conseil d'État*. In our database, we observe five different and recurrent legal grounds invoked by the appellants: disagreement on the amount of compensation (which we take as our reference variable), dis-

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<sup>23</sup>We have voluntarily excluded criminal cases, which are too different to be compared with civil and administrative cases. Indeed, once a case goes to Criminal Courts in France, then it will not be treated in Civil Courts. This can be different from countries where a same case can go both to Criminal and Civil Courts.

<sup>24</sup>Environmentally protected installations (*Installations classées pour la protection de l'environnement*).

<sup>25</sup>For *Integrated Pollution Prevention and Control*. See European Directive EC 96/61 imposing the application of the "Best Available Technology" principle to polluting facilities.

<sup>26</sup>In our database, there is only one appellant for each case.

<sup>27</sup>Indeed, the coefficient of the variable *Defappeal* measures the probability  $Pr(\text{Pro-defendant}|\text{Defendant appeal})$ , which is the probability that *Prodef* takes value 1 when *Defappeal* changes from 0 to 1. *Defappeal* takes value 1 when the defendant files the appeal and 0 when it is the plaintiff, and *Prodef* takes value 1 when the defendant wins and 0 when the plaintiff wins. Hence, observing the chances that the *Prodef* equals 1 when *Defappeal* changes from 0 to 1 is equivalent to measuring the chances that *Prodef* equals 0 when *Defappeal* changes from 1 to 0. It amounts to observe the probability  $Pr(\text{Pro-plaintiff}|\text{Plaintiff appeal})$ , meaning the probability to have a pro-plaintiff decision when the plaintiff appeals. To be fully convinced:  $Pr(Prodef = 1|Defappeal = 1) - Pr(Prodef = 1|Defappeal = 0) = [1 - Pr(Prodef = 0|Defappeal = 1)] - [1 - Pr(Prodef = 0|Defappeal = 0)] = Pr(Prodef = 0|Defappeal = 0) - Pr(Prodef = 0|Defappeal = 1)$ .

agreement on the relevance of the proof of wrongful or negligent behavior (*Proof*), disagreement with lower court's treatment of causation (*Causality*), disagreement with lower court's treatment of the uncertainty about the consequences - in the case of a lawsuit primarily brought by potential victims claiming that a given activity imposes an imminent risk of accident - (*Uncertainty*), and disagreement on the due process of law or on the legal procedure followed by the lower court (*Procedure*).

Environmental lawsuits may also concern different natural assets. Another group of control variables identifies the type of damaged natural resource: *Water, Soil, Air, Sea* or *Noise*.

Last, a variable takes into account the fact that the defendant had complied with regulation at the date of accident: *Compliance* with regulation is a dummy variable, noted 1 if the defendant complied with regulation and 0 otherwise.<sup>28</sup>

## 5 Pro-plaintiff Correction Activities and Case Selection

The empirical goal is twofold. First, we seek to determine whether the two Supreme Courts engage in the same level of correction activities. Second, we investigate the impact of case selection on these correction activities.

### 5.1 Bias Correction Activities by Supreme Courts

As stated in Section 2, the main objective of Supreme Courts is to ensure that legal provisions are enforced in the same way over the territory. Standard literature in Law and Economics usually refers to two kinds of mistakes judges can make when ruling a case. Errors of type 1 correspond to cases where an innocent party is wrongfully convicted, whereas errors of type 2 refer to cases in which a guilty party gets away with it. As both kinds of errors are almost inevitable, the trade-off between the two types of error is usually decided by the preferences of the court. These preferences can be expressed on a single dimension, *i.e.* a pro-defendant dimension. A pro-defendant court is less likely to convict a defendant when evidence is mixed.

In the following analysis, we propose to consider the relative pro-defendant preferences of the Appellate and Supreme Courts. We denote  $\gamma$  the pro-defendant bias, with  $\gamma \in (-\infty, +\infty)$ . A higher  $\gamma$  represents a stronger pro-defendant bias. Here, we understand the notion of bias in a very broad sense: it corresponds to the overall propensity of a court to decide in favor of the defendant when a case is not clear. We write  $\gamma_{1,C}$  the average bias of the Appellate Courts in the civil jurisdictions, the bias of the Civil Supreme Court (*i.e.* the *Cour de Cassation*),  $\gamma_{1,A}$  the average bias of the Administrative Appellate Courts, and  $\gamma_{2,A}$  the bias of the Administrative Supreme Court (*i.e.* the *Conseil d'État*).

The correction activities of each Supreme Court corresponds to the decisions it takes to correct

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<sup>28</sup>For a detailed description of each variable, see Bentata (013b), Bentata (013a).

for the relative bias of the Appellate Courts. The differences in biases between the Appellate and the Supreme Courts are given by:

$$\Delta_k = \gamma_{1,k} - \gamma_{2,k} \quad (1)$$

where  $k \in \{A, C\}$ . The correction activities of the Supreme Courts in favor of the plaintiff are given by  $h(\Delta_k)$ , where  $h(\cdot)$  is an increasing and monotonous function, and equal to zero at the origin. A positive  $\Delta_k$  reflects a greater bias toward the defendant of the Appellate Court than the Supreme court's bias. A positive  $\Delta_k$  is therefore associated with more correction activities in favor of the plaintiff, *i.e.* positive values of  $h(\Delta_k)$ . Because Supreme Courts have been able to select cases at different dates, we introduce a conditional level of correction activity given a selection rule:  $h(\Delta_k|S_k)$ . A  $S_k$  equal to 0 indicates that Supreme Court  $k$  has no control over its dockets, while a score equal to 1 reflects the fact that it can select cases.

Although our data do not allow to locate each  $\beta_{1,k}$  or  $\beta_{2,k}$  on a pro-defendant axis, our empirical strategy aims at comparing the correction activities of the two courts. Comparing the correction activities, three cases can emerge:

- Case 1:  $h(\Delta_C|S_A = S_C) > h(\Delta_A|S_A = S_C)$  : The Civil Supreme Court engages in more pro-plaintiff correction activity than the Administrative Supreme Court.
- Case 2:  $h(\Delta_C|S_A = S_C) = h(\Delta_A|S_A = S_C)$  : Supreme Courts engage in a similar level of pro-plaintiff correction.
- Case 3:  $h(\Delta_C|S_A = S_C) < h(\Delta_A|S_A = S_C)$  : The Civil Supreme Court engages in less pro-plaintiff correction activity than the Administrative Supreme Court.

## 5.2 Econometrics

This subsection aims at testing the relative levels of correction activities depicted above. The goal is to determine whether courts engage in different levels of pro-plaintiff correction. To do so, we propose to run a Probit model to estimate the probability that a case is decided at the Supreme Court in favor of the plaintiff. The latent utility model we consider writes:

$$Prodef_i^* = \beta_0 + \beta_1 CE_i + \beta_2 X_i + u_i, \quad (2)$$

where  $Prodef_i^*$  is the latent utility of a pro-defendant decision,  $CE_i$  is a variable equal to 1 if case  $i$  is ruled by the *Conseil d'Etat*,  $X_i$  is a vector of control variables for the case  $i$ , and  $u_i$  is a normally distributed random term.

We propose to run the Probit model for four samples. First, we consider the entire set of data, running from 1956 to 2011. Second, because of the two reforms that introduced the possibility of case selection by Supreme Courts, we consider three periods in turn: when none of the courts could select cases (1956-1989), when only the *Conseil d'État* was able to select cases (1988-2001), and when both courts were able to select cases (2002-2011). Results of these estimations are displayed in table 1.<sup>29</sup>

The objective of the Probit estimation consists in comparing the level of correction activities of the two courts. A positive (resp. negative) coefficient associated to *CE* would indicate that the Administrative Supreme Court engages in less (resp. more) pro-plaintiff correction than the Civil Supreme Court: case 1 (resp. case 3). A coefficient not statistically different from zero would depict a situation in which Supreme Courts have the same intensity of pro-plaintiff correction activities.

Table 1: Results of the Probit regressions

	Full Period <i>Model (1)</i>	Before 1989 <i>Model (2)</i>	From 1989 to 2001 <i>Model (3)</i>	After 2001 <i>Model (4)</i>
<i>Conseil d'État</i> (CE)	-0.274* (0.147)	-0.152 (0.258)	-0.720*** (0.272)	-0.312 (0.351)
Controls	Yes	Yes	Yes	Yes
Observations	614	168	276	169
Nagelkerke $R^2$	0.395	0.432	0.466	0.342

Note: Standard errors in parentheses, \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

Table 1 yields interesting results. At the first sight, the estimation in model 1 suggests that the Administrative Supreme Court (*Conseil d'État*) engages in more pro-plaintiff correction activities than the Civil Supreme Court (*Cour de Cassation*). Indeed, the coefficient associated to *CE* is statistically different from zero in this regression covering the entire period. Decomposing the data into three sub-periods yields interesting results. First, in model 2, i.e. prior to 1989, when no Supreme Court could select cases, we do not detect any difference in the intensity of the correction activities of the courts. Second, in model 4, after 2001, when both Supreme Courts were able to select cases, we do not detect any difference between their correction activities. Third, one can note, however, a strongly significant difference in the correction activities in model 3, i.e. between 1989 and 2001, when only the Administrative Supreme Court was able to select cases.

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<sup>29</sup>Estimations of the coefficients associated to the control variables are displayed in the Appendix.

<sup>30</sup>In the appendix, we run two additional regressions, namely models 5 and 6. Running separate regressions to estimate the marginal impact of the *Conseil d'État* for each subperiod is the most intuitive -and the most commonly used- method. However, if one assumes that the control variables have a similar impact on the decision whatever the period at stake, it is more efficient to estimate the model on a unique sample and to distinguish the marginal effect of the variable of interest with interaction with dummy variables. Model 5 and 6 confirms the findings of models 2 to 4. With model 6, we are able to investigate whether the marginal effect of *CE* is different when courts had the same control possibilities over their dockets and when they did not. We are able to reject this hypothesis with

In short, the results can be summarized as follows:

- Over the entire period, we observe a stronger commitment of the Administrative Supreme Court to correct the pro-defendant bias of the lower courts than the Civil Supreme Court.
- This effect is driven by the fact that the Administrative Supreme Court was allowed to select cases earlier, between 1989 and 2001, and used case selection to increase its pro-plaintiff correction activities.
- When courts can (or cannot) select cases in the same way, we do not detect any difference in their correction activities.

In order to evaluate the veracity of these propositions, we propose several additional investigations. First, to compare the difference in the correction activities across Supreme Courts, we create counterfactual cases using one court's decisions to predict the other court's ones (Subsection 5.3). Second, we use permutation tests to ensure that the change in the correction activities is indeed driven by the two reforms and not by actual changes in the Supreme Courts' preferences between 1989 and 2001 (Subsection 5.4).

### 5.3 Counterfactuals

In order to compare the correction activities of the courts, and thus to confirm or infirm the previous results, we propose to rely on a counterfactual approach. The objective consists in creating cases using one Supreme Court's decisions to predict the other Supreme Court's ones, and to capture the discrepancies between the predictions and the observations. To do so, we rely on matching methods.

The underlying idea of matching estimations is to sort observations from two groups on a single axis (the *balancing score*), such that units with comparable scores from the two groups share the same observables (Rosenbaum and Rubin (1983)). The subsequent theoretical and applied literature has mainly used the *propensity score*, i.e. the probability of belonging to one group or another, as a balancing score (Rosenbaum and Rubin (1984)). We proceed in the same way and propose to use a case's probability to be decided by the *Conseil d'État* as a balancing score.

We estimate the following equation to compute the balancing score:

$$CE_i^* = \alpha_0 + \alpha_1 X_i + v_i \quad (3)$$

where  $X_i$  is the same vector of control variables as in equation (2) and  $v_i$  is a normally distributed random term.

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$p = 0.060$ . We believe that the weak significance mainly results from the low number of observations. All in all, these two additional regressions confirm our results.

We estimate equation 3 for three periods: before 1989, between 1989 and 2000, and after 2001. We then use matching algorithms to create counterfactuals. We take the cases dealt by the *Conseil d'État* as the reference group, and we use matching algorithms to create, for each case, counterfactuals that are combinations of the cases dealt by the *Cour de Cassation*. We use four algorithms: the nearest neighbor, the 3-nearest neighbors, the Epanechnikov kernel and the normal kernel.

Table 2 displays the average bias associated each matching algorithm and the original standardized bias. As it can be seen, the Epanechnikov outperforms the other algorithms and dramatically reduces the biases. The Gaussian (or Normal) kernel performs also well, but is slightly above the 5% threshold for the average standardized bias. We thus build the counterfactuals based on the Epanechnikov and Gaussian kernels.

Table 2: Matching diagnosis: average standard bias per matching algorithm

	Before 1989	From 1989 to 2001	After 2001
Before matching	9.713	19.787	23.459
Nearest Neighbor	10.762	7.724	6.376
3-nearest neighbors	10.93	6.171	6.554
Epanechnikov kernel	3.99	2.463	3.469
Gaussian kernel	4.664	5.208	3.693

For each case  $i$  of the *Conseil d'État* we thus compute a counterfactual case, that is a linear combination of the cases of the *Cour de Cassation*. We then compute the difference between the actual decision of the *Conseil d'État* and the probability of a pro-defendant decision predicted by the counterfactual. If the counterfactual were a correct estimation of the real probability of a pro-defendant decision, the average difference between the actual and the predicted decisions should be close to 0. On the contrary, a systematic deviation, i.e. over- or underestimation, would reflect the higher propensity of one court to rule in favor of one party relatively to the other court.

Table 3 reports the average difference between the predicted and actual decisions of the courts for each of the three periods. It also shows whether these average differences are statistically different from 0.

Table 3: Average differences between the actual pro-defendant decisions of the *Conseil d'État* and the predictions based on the decisions of the *Cour de Cassation*.

	Before 1989	From 1989 to 2001	After 2001
Epanechnikov	-.0673 (-0.86)	-.1520** (-2.24)	-.1073 (-1.06)
Gaussian	-.0382 (-0.50)	-.1792*** (-2.72)	-.0813 (-0.82)

Note: Z-statistics, \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

Few conclusions can be drawn from this second set of estimations. First, it confirms that both courts engage in the same level of pro-plaintiff correction activity when they face the same se-

lection rule.<sup>31</sup> Using the notation introduced in subsection 5.1, this implies  $h(\Delta_C|S_C = S_A) = h(\Delta_A|S_C = S_A)$ . Second, regarding the correction activity of the courts between 1989 and 2001, the estimation concludes that  $h(\Delta_C|S_{C,0}) < h(\Delta_A|S_{A,1})$ , indicating that the Administrative Supreme Court increased its pro-plaintiff correction activity during this time period.

#### 5.4 Change in correction activity or change in preferences?

Previous results can be explained in two ways. First, they might be supported by our theory, which states that courts have increased their correction activity through case selection. Second, they might as well be explained by a change in courts' preferences over time. In fact, our data would lead to the same conclusions if the Administrative Supreme Court changed its views in the 80s, and the Civil Supreme Court's position started itself to change at the end of the 90s.

In order to investigate this issue, we propose some tests to ensure that our results are indeed driven by the two specific dates 1989 and 2001. Although we are not able to rule out the fact that Supreme Courts have changed their bias at the precise time they obtained case selection, we are willing to exclude the possibility that Supreme Courts changed their position steadily over time. To do so, we propose two falsification tests. First, we permute the *reform* status in the data. Second, we look at what the data would yield if we set the reform five years before or five years after the actual reform.

■ **Permutation test.** The first test deals with the null hypothesis that, in each court, the impact of the reform on the win rates of defendants (*Prodef*) is random. The underlying intuition for this test is the following: if the effect of the reform is random, then a random reallocation of values taken by this variable should have a similar or more important effect on the variable *Prodef*.

For each court, we create a variable *Reform* taking value 1 if the case is judged after the reform and 0 otherwise. We then observe the effect of the reform on the variable *Prodef* using an OLS regression on a period going from ten years before the reform to ten years after the reform (we keep all the control variables, as we did for the other regressions). For both courts, the variable *Reform* is significant with a negative coefficient ( $\beta = -0.163$  and  $p = 0.088$  for the Administrative Supreme Court,  $\beta = -0.115$  and  $p = 0.024$  for the Civil Supreme Court).

We proceed to a permutation of the variable *Reform*: we randomly reallocate the values of the variable *Reform* in the sample of interest and observe the effect of this random variable (denoted  $x$  on Figure 1) over the results. We repeat 5000 times this operation and compare the distribution of the coefficients of the random variable (denoted  $_b[x]$  on Figure 1) with the coefficient of the variable *Reform*. Under the null hypothesis, the coefficient of *Reform* is regularly found in the distribution ( $p - value > 0.05$ ): we cannot reject the assumption that the effect of the reform is simply random.

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<sup>31</sup>According to our results, the introduction of a *selection rule* decreases by 15.2 to 17.9 percentage points the probability that the Administrative Supreme Court rules in favor of the defendant. Yet, the figure have to be taken with caution as they are derived from a linear probability model.

Figure 1 shows the results of permutations for both courts. We observe that the value of the coefficient of the variable *Reform* is always in the extreme 5% of the normal distribution. The probability to randomly obtain an identical coefficient is  $p - value = 0.0146$  for the Administrative Supreme Court and  $p - value = 0.0032$  for the Civil Supreme Court.

We thus reject  $H_0$ : the above results are not driven by the randomness of the data but by the reforms.

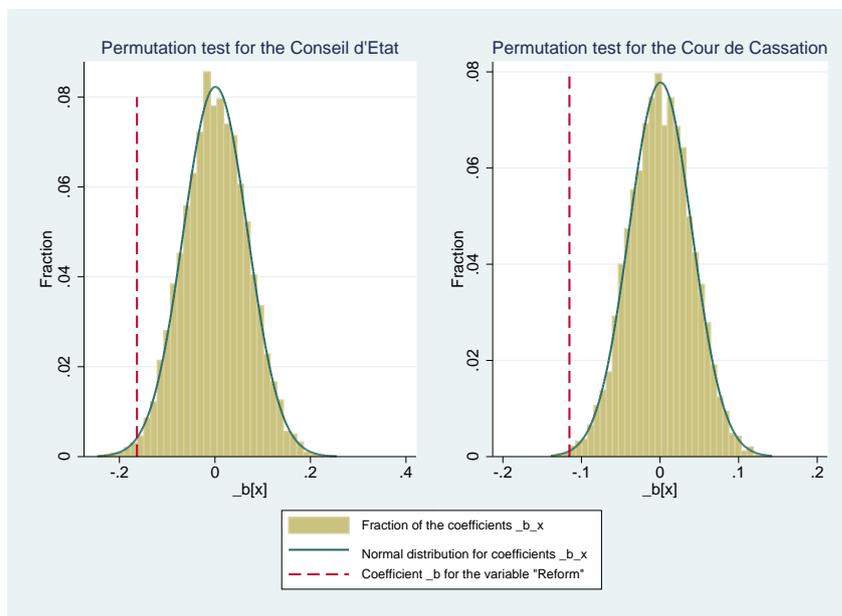


Figure 1: Graphic Results for the Falsification tests with Permutation

■ **Falsification of reform dates.** Although the reform has a significant effect on the observed results, we may wonder whether this effect is simply the result of a variation in the severity of judges over the same period, i.e. a variation in Supreme Courts' preferences. In order to ensure that it is not the case, we observe, for each court, the evolution in the severity of judges starting at a previous or later date with respect to the reform. We measure the effect of a temporal variable starting five years before then five years after the reform on the variable *Prodef*. For the Civil Supreme Court, we thus measure the effects of two temporal variables, one starting in 1997 and the other in 2007, and compare the coefficients obtained with those of the variable *Reform*. For the Administrative Supreme Court, we do the same with a variable starting in 1984 and the other in 1994. Table 4 provides the results for both courts.

Table 4: Comparison of different time periods over judges' decisions

	Reform	Reform <i>minus</i> 5 years	Reform <i>plus</i> 5 years
<i>Cassation</i>	-0.12*** (0.01)	-0.06 (0.04)	-0.10* (0.05)
<i>Conseil d'État</i>	-0.14** (0.07)	-0.06 (0.08)	-0.12* (0.07)

Note: Standard errors in parentheses, \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . The set of control variables used throughout the paper are also used in these regressions.

We observe that the temporal variables starting before the reform are non-significant; those starting after the reform have a slightly weaker effect than the reform itself. Hence, for both courts, the variation in the severity of judges actually starts with the reform and no other temporal shock has influence over judges' correction activity once the reform is adopted. This confirms the above findings supporting the reform's effect.

## 5.5 How do Courts select their dockets?

Previous subsections have highlighted the existence of correction activities from both courts, that induce lower win rates for defendants before these courts. Such a correction activity can appear in only two ways: either judges tend to select appeals filed by defendants and uphold lower courts' decisions, either they tend to select appeals filed by plaintiffs and reverse lower courts' decisions. The two selection modes have different effects on the trial outcome:

- **Scenario 1.** In the first situation, the correction activity has a limited effect on the trial outcome. Indeed, Supreme Courts preferably select appeals filed by defendants (*i.e.* judgments in favor of plaintiffs in Appellate Courts) and uphold lower courts' decisions, while often declaring as "non-eligible" the appeals filed by plaintiffs (*i.e.* judgments in favor of defendants in Appellate Courts). Hence, Supreme Courts essentially select cases in order to uphold lower courts' decisions and the correction activity does not alter the outcome of the trial. From a legal viewpoint, the correction activity has no influence on final decisions.

- **Scenario 2.** In the second situation, the correction activity has a determining effect on the trial outcome. Supreme Courts preferably select appeals filed by plaintiffs (*i.e.* judgments in favor of defendants in Appellate Courts) and reverse lower courts' decisions, while often declaring as "non-eligible" the appeals filed by defendants (*i.e.* judgments in favor of plaintiffs in Appellate Courts). As a consequence, lower courts' decisions are reversed more often in favor of plaintiffs. From a legal viewpoint, the correction activity modifies the trial outcome in favor of plaintiffs.

In order to determine the selection process of Supreme Courts, we observe the influence of the reforms in the Administrative Supreme Court and the Civil Supreme Court over their decisions to reverse lower courts' decisions. In this purpose, we build a dummy variable (*Reverse*), taking value 1 when the Appellate Court's decision has been reversed and 0 otherwise. We carry out a *Biprobit* regression with the dependent variables *Reverse* and *Prodef*. Indeed, whatever the selection mode (*Case 1* or *Case 2*), the decision to reverse or uphold a judgment is clearly correlated with the identity of the winning party. We also introduce an interaction variable (*Defappeal\*Reform*) allowing to observe the effect of a defendant appeal after the reform on the probability of a judgment reversal. Last, we keep the whole set of previous control variables. Table 5 depicts the results of the regression.

In order to capture the selection strategy of Supreme Courts, we focus on the variable *Reverse*. In each court, the reform has significantly increased the chances of a reversal decision. Hence, it seems that Supreme Courts select cases in order to counter more often lower courts' decisions.

Table 5: Results of the Biprobit regression on *Reverse* and *Pro-defendant*

	Coefficients	
	Reverse	Pro-defendant
Reform in CE	0.485** (0.245)	-0.825*** (0.258)
Reform in CC	0.403** (0.193)	-0.573*** (0.200)
Def appeal*Reform	-0.769*** (0.245)	0.279 (0.256)
Defendant appeal	-0.276* (0.145)	-1.194*** (0.149)
Athrho		0.227*** (0.072)
Controls		Yes
Observations		614

Note. Standard errors in parentheses, \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

Besides, the interaction variable *Defendant appeal\*Reform* has a significant and negative effect on reversals. In other words, it seems that Supreme Courts select cases in order to counter lower courts' decisions, particularly when the latter are unfavorable to the plaintiffs. Indeed, defendants have fewer chances to obtain a reversal after the reform, though overall chances of a reversal have increased. The selection bias has thus clearly an effect on the overall trial outcome: courts select more often cases that they will counter in favor of plaintiffs (*Scenario 2*).

These results suggest an overall interpretation concerning the impact of the correction activity over the win rates of the litigants. A losing plaintiff (resp. defendant) in Appellate Courts has relatively more chances than a defendant (resp. less chances than a plaintiff) to see her case admitted by Supreme Courts and to obtain a reversal of the lower courts' decision. In other words, the pro-plaintiff effect observed when analyzing Supreme Courts is not a simple statistical artefact giving a truncated description (in favor of plaintiffs) of the whole set of courts' decisions. It is a genuine bias which modifies the actual win rates of litigants in favor of plaintiffs.

## 6 Conclusion

In this paper, we have adopted two different approaches (linear regressions and matching estimation) in order to distinguish and quantify the effects of bias correction activities of the two French Supreme Courts. Our study shows that both the Civil and the Administrative Supreme Courts correct Appeal Courts' in the same direction and in the same magnitude: both Supreme Courts are indeed more favorable towards plaintiffs than their respective lower courts. We have also shown that the correction activities of the Supreme Courts are similar when both have the

same selection mechanism over their dockets. Finally, we find that this result is driven by the selection strategies of the Supreme Courts, which are more likely to select cases that they will overrule in favor of the plaintiffs.

Our results have several implications for the public and academic debates on the role of Supreme Courts. First, one can observe that both courts engage in the same level of correction activity and that the correction goes in the same direction (pro-plaintiff correction). This result implies that both branches of the French legal system are similar in terms of the relative biases between Supreme Courts and their respective Appellate Courts. In both branches, plaintiffs face the same incentives to bring their claim to the highest court. Second, as far as the selection mechanism is concerned, we observe that preventing courts to select cases creates a strong pressure on them. Indeed, the great increase in the probability of a pro-plaintiff decision resulting from the selection rule shows that courts were highly willing to select cases to increase their correction activities. Regarding the harmonization of the legal system, giving Supreme Courts control over their dockets might be welfare improving: they seem to focus on their correction activity, sending signals to lower courts. This finding is especially important in the perspective of judicial reforms undertaken in other countries. Indeed, a few Supreme Courts have recently experienced similar changes (Taiwan in the early 2000s,<sup>32</sup> Brazil in 2007) and could be followed by other ones since the clogging of the courts and the resulting increased delays are a wide-spread public policy problem.

Our investigation is nevertheless limited in several ways. The most obvious limitation concerns case selection: cases, which are brought by litigants to Supreme Courts, might not be representative of the entire set of cases dealt by Appellate Courts. We therefore ignore whether Supreme Courts decide on representative cases or on exceptional ones. Second, still regarding the appeal process, it might be that the unobservables affecting the decision to bring the case to the Supreme Court are correlated with the unobservables of the Supreme Courts' decisions. Not accounting for the correlation between these two decisions may lead to a flawed estimation of the covariance matrix of our estimated coefficients, leading to "wrong" inferences. Third, we are limited in investigating the impact of the reform on Supreme Courts' behaviors: we do not have the pool of cases which have been denied any hearing by the Supreme Courts. Here again, unobserved factors leading Supreme Courts to hear a case are very likely to explain their final decision.

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<sup>32</sup>See Eisenberg et al. (2012)

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## Tables

Table 6: Descriptive Statistics

Variables	All Decisions	CE only	CC only	p-value
Defendant appeal	0.635	0.730	0.607	0.007
Compliance	0.539	0.489	0.554	0.177
Compensation	0.158	0.149	0.161	0.737
Proof	0.264	0.319	0.247	0.089
Causality	0.220	0.191	0.228	0.354
Uncertainty	0.153	0.128	0.161	0.339
Procedure	0.339	0.284	0.355	0.115
Water	0.350	0.369	0.345	0.598
Soil	0.171	0.163	0.173	0.777
Air	0.138	0.170	0.129	0.213
Sea	0.034	0.007	0.042	0.044
Noise	0.226	0.191	0.270	0.256

*Note: The p-values correspond to two-sample proportion tests.*

Table 7: Full results of the Probit regressions

	Full Period <i>Model (1)</i>	Before 1989 <i>Model (2)</i>	From 1989 to 2001 <i>Model (3)</i>	After 2001 <i>Model (4)</i>	Full Period <i>Model (5)</i>	Full Period <i>Model (6)</i>
<i>Conseil d'État (CE)</i>	-0.274* (0.147)	-0.152 (0.258)	-0.720*** (0.272)	-0.312 (0.351)		
CE × $\mathbb{1}_{t < 1989}$					-0.0595 (0.234)	
CE × $\mathbb{1}_{1989 \leq t \leq 2001}$					-0.725*** (0.268)	-0.724*** (0.268)
CE × $\mathbb{1}_{2001 > t}$					-0.193 (0.337)	
CE × (1 - $\mathbb{1}_{1989 \leq t \leq 2001}$ )						-0.104 (0.191)
Defendant appeal	-1.073*** (0.122)	-0.536** (0.249)	-1.517*** (0.200)	-0.868*** (0.239)	-1.077*** (0.123)	-1.077*** (0.123)
Compliance	0.683*** (0.145)	1.361*** (0.336)	0.527** (0.227)	0.732** (0.295)	0.759*** (0.150)	0.758*** (0.150)
Compensation	0.580* (0.319)	4.336*** (0.515)	-0.0795 (0.473)	-0.153 (0.737)	0.530 (0.325)	0.529 (0.325)
Proof	0.0360 (0.325)	3.784*** (0.496)	-0.644 (0.518)	-0.602 (0.696)	0.00854 (0.327)	0.00449 (0.327)
Causality	-0.00153 (0.180)	-0.402 (0.391)	-0.0944 (0.272)	-0.162 (0.372)	-0.0261 (0.179)	-0.0282 (0.179)
Uncertainty	0.487 (0.317)	3.939*** (0.594)	0.0515 (0.440)	-0.178 (0.733)	0.424 (0.325)	0.422 (0.324)
Procedure	0.454 (0.305)	4.257*** (0.487)	-0.0220 (0.450)	-0.335 (0.720)	0.400 (0.311)	0.398 (0.311)
Water	-0.0900 (0.200)	0.294 (0.513)	-0.132 (0.325)	-0.213 (0.364)	-0.171 (0.209)	-0.162 (0.208)
Soil	-0.442** (0.213)	-0.353 (0.582)	-0.205 (0.369)	-0.735** (0.357)	-0.429* (0.221)	-0.419* (0.217)
Air	-0.636*** (0.230)	0.0602 (0.638)	-0.571 (0.355)	-0.904** (0.395)	-0.650*** (0.236)	-0.641*** (0.234)
Sea	-0.362 (0.390)	0.0654 (0.745)		-0.426 (0.470)	-0.165 (0.402)	-0.152 (0.403)
Noise	-0.134 (0.210)	0.138 (0.540)	-0.192 (0.329)	-0.408 (0.397)	-0.236 (0.218)	-0.224 (0.213)
Constant	-0.212 (0.351)	-4.734*** (0.571)	0.802 (0.506)	0.287 (0.741)	-0.101 (0.369)	-0.0901 (0.367)
Period FE	No	No	No	No	Yes	Yes
Observations	614	168	276	169	614	614
Nagelkerke $R^2$	0.395	0.432	0.466	0.342	0.395	0.395
Cox Snell $R^2$	0.272	0.309	0.345	0.240	0.287	0.287

Note. Standard errors in parentheses, \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

Table 8: Results of the Biprobit regression on *Reverse* and *Pro-defendant*

	Reverse	Pro-defendant
Reform in CE	0.485** (0.245)	-0.825*** (0.258)
Reform in CC	0.403** (0.193)	-0.573*** (0.200)
Def appeal*Reform	-0.769*** (0.245)	0.279 (0.256)
Defendant appeal	-0.276* (0.145)	-1.194*** (0.149)
Compliance	0.132 (0.139)	0.791*** (0.149)
Compensation	0.528 (0.337)	0.488 (0.357)
Proof	0.609* (0.328)	-0.049 (0.357)
Causality	-0.047 (0.183)	-0.042 (0.186)
Uncertainty	-0.219 (0.340)	0.418 (0.360)
Procedure	0.524 (0.323)	0.360 (0.346)
Water	-0.202 (0.187)	-0.174 (0.198)
Soil	-0.205 (0.197)	-0.435** (0.212)
Air	-0.528** (0.225)	-0.617*** (0.238)
Sea	-0.101 (0.343)	-0.161 (0.361)
Noise	-0.442** (0.206)	-0.234 (0.211)
Constant	-0.739** (0.341)	-0.001 (0.357)
Athrho		0.227*** (0.071)
Observations		614
Log-likelihood		-615.46
Wald $\chi^2$		208.89
Rho		0.230 (0.068)

Note. Standard errors in parentheses, \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

## Appendix A.

Table 9 presents the results of a regression in which two interaction variables have been added (Column 3): a variable representing cases judged in the Administrative Supreme Court (*Conseil d'État*, CE) after the 1987 reform and a variable representing cases judged in the Civil Supreme Court after the 2001 reform.

Comparing the results of this regression with those of the regression without these interaction variables (Column 2), we observe that the variable *Conseil d'État* loses significance when taking reforms into account. This confirms the analysis conducted with separate regressions (Table 7). The regression without interaction variables lets us believe to a pro-plaintiff bias in the Administrative Supreme Court. This effect is indeed related to an earlier introduction of the reform in this court w.r.t. the Civil Supreme Court. This effect is linked to a selection bias by courts, which remains hidden when one does not take into account the reforms allowing the Supreme Courts to have control over their dockets.

Moreover, when comparing the coefficients of the two interaction variables using a Wald test, we see that these two variables have a similar effect. Indeed, under the null hypothesis of equal coefficients, we obtain a  $\chi^2$  value of 0.66 with a *p-value* of 0.4158. We thus accept the assumption that the reform has a pro-plaintiff effect of the same magnitude in the two courts.

Table 9: Probit regression with and without interaction variables

	Without interaction variables <i>Selection bias not taken into account</i>	With interaction variables <i>Selection bias taken into account</i>
<i>Conseil d'État</i> (CE)	-0.274* (0.147)	0.129 (0.201)
CE after Reform only	-	-0.547** (0.273)
CC after Reform only	-	-0.455*** (0.158)
Defendant appeal	-1.073*** (0.121)	-1.081*** (0.123)
Compliance	0.683*** (0.145)	0.768*** (0.149)
Compensation	0.580* (0.319)	0.533 (0.355)
Proof	0.036 (0.352)	0.003*** (0.354)
Causality	-0.00153 (0.183)	-0.035 (0.187)
Uncertainty	0.487 (0.357)	0.432 (0.358)
Procedure	0.454 (0.343)	0.401 (0.343)
Water	-0.090 (0.200)	-0.172 (0.197)
Soil	-0.442** (0.213)	-0.427** (0.212)
Air	-0.636*** (0.230)	-0.643*** (0.239)
Sea	-0.362 (0.390)	-0.166 (0.367)
Noise	-0.134 (0.210)	-0.235 (0.211)
Constant	-0.212 (0.351)	-0.032 (0.354)
Observations	614	614
LR test	195.243	207.365
Nagelkerke $R^2$	0.395	0.394
Cox Snell $R^2$	0.272	0.287
C-stat (%)	76.71	76.87

Note. Standard errors in parentheses, \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .