

Correction Activities by French Supreme Courts and Control over their Dockets

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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, courts of appeal, judicial behavior, Administrative Law, Supreme Courts, judicial reform.

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1 Introduction

The existence of Supreme Courts is very often justified by the need to harmonize the application of the law. 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 enforcement of rules 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 wrong decision. Supreme Courts have therefore the opportunity to correct decisions of appeal courts due to improper application 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 made by appeal courts.

Although Supreme Courts contribute to harmonize the legal framework, they are not exempt from political and/or judicial preferences either. Several works have shown that Supreme Courts Justices can indeed be motivated by non-legal factors such as ideological preferences or political pressure; they can also be influenced by peer biases. In this respect, Supreme Courts may seek to take advantage of the opportunity to overrule appeal courts' decisions to align case law with their own preferences. In other words, Supreme Courts might correct lower courts' biases relatively to theirs.

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 regressions 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 behavior 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' biases, the more the latter are expected to engage in bias correction activity and, therefore, to reverse the appeal courts' decisions.

Previous investigations of Supreme Courts' decisions have emphasized the importance of considering case selection (Kastellec and Lax (2008)).¹ 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 al-

¹Biases 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 or knowledge of the legal system, 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.

lowed 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 *Conseil d'État* engages in more correction activities of the appeal courts' decisions than the *Cour de Cassation*. This allows to indirectly assess the relative distance in the courts' biases between the appeal courts and their Supreme Court. Second, we propose to investigate whether Supreme Courts, when they are allowed to select cases, change their correction behavior vis-à-vis lower courts' decisions.

The paper shows that, on the entire period, the administrative Supreme Court engages in more correction activity than the civil Supreme Court. However, decomposing the period according to the selection capacity of each court, we find that Supreme Courts' correction activities are similar in their intensity when both courts have 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 and presents some statistics. 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 has analyzed 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)) 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 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 before judges and juries significantly differ because 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, evidence suggesting a pro-defendant bias might also be explained by the fact that plaintiffs tend to appeal cases even when their chances of success are very low (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 Supreme Courts' 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.² 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.³ 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)).

²For 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) showed that the decision 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)).

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

Although a growing number of scholars recognize the importance of case selection (Eisenberg et al. (2012), Eisenberg et al. (2014)) and develop different statistical methods to control for it,⁴ the impact of selection bias on correction activities 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 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.⁵ In the absence of this administrative authority, a private company providing public services will be sued in civil courts.⁶ 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,⁷ 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 secu-

⁴For 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.

⁵Private 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*).

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

⁷*Installations classées pour la protection de l'environnement*.

rity⁸, and cannot claim that their level of care was limited by a budget constraint.⁹ Furthermore, the procedures are also different. The procedure before administrative courts is inquisitorial (“*inquisitoire*”) whereas the civil procedure is accusatorial (“*accusatoire*”). This means that administrative 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).¹⁰ Consequently, administrative judges have more room than civil judges to make their decisions. Hence, if judges were to be biased, the effect could virtually be greater in administrative courts.

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,¹¹ 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.¹² 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 bias.

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

⁸The 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).

⁹Cass. Crim., July 2nd 1998, *n.97-83.286*.

¹⁰These 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.

¹¹Indeed, 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.

¹²See About the Court, Cour de Cassation, www.courdecassation.fr/about_the_court_9256.html.

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.¹³ 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.¹⁴ 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 assures 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 courts of appeal.¹⁵ Similarly to the lower administrative courts, one could question the impartiality of these judges toward 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 toward the state: their choice to devote their career to the public administration might reflect a

¹³For 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.

¹⁴See 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.

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

strong propensity to see the state as a necessary key player for the country's welfare.

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 be actually treated.¹⁶

As explained in the introduction, due to this discretionary control, any interpretation of econometric results on Courts' decisions 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' bias 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 25th, 2001¹⁷ 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.¹⁸ Hence, for the period 1947 to 2001, the *Cour de Cassation* could not select the cases to be treated.¹⁹ The selection bias - at the *Cour de Cassation's* level - can only concern the cases after 2001.²⁰

Something similar has occurred for the *Conseil d'État* for which the preliminary screening of the cases has been restored on December 1987, 31st.²¹ Hence, cases judged before 1987 constitute a complete and non-biased sample of the cases treated by the *Conseil d'État*. Only the cases judged since 1987 can present the selection bias we isolate.

¹⁶See 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.

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

¹⁸See http://www.courdecassation.fr/cour_cassation_1/autres_publications_discours_2039/publications_2201/admission_pourvois_cassation_8424.html

¹⁹From 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.

²⁰The 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.

²¹*Art. L 822-1 du Code de la Justice Administrative.*

4 Database and preliminary results

4.1 Database

In order to investigate the bias 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 the *Cour de Cassation* and the *Conseil d'État* since 1956.²² 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,²³ Seveso, IPPC,²⁴ 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 noted "1" when present in cases and "0" otherwise.

The dependent variable is the decision of 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 otherwise. 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. **REPRENDRE : Indeed, as explained in the preceding Section, the career of administrative Judges favors the emergence of a pro-defendant bias. *A priori*, Judges from the *Conseil d'État* do not have any reason to favor one party rather than the other. They should thus be led to correct the pro-defendant attitude of lower administrative Courts by adopting a pro-plaintiff behavior.**

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.²⁵ From a statistical viewpoint, this variable coefficient measures the trend of Judges to satisfy the party who files the appeal, whoever is this party.²⁶ This variable allows to have some control over the "affirmative"

²²We have voluntarily excluded criminal cases, which are too different to be compared with civil and administrative cases.

²³*Installations classées pour la protection de l'environnement.*

²⁴For *Integrated Pollution Prevention and Control*. See European Directive EC 96/61 imposing the application of the "Best Available Technology" principle to polluting facilities.

²⁵In our database, there is only one appellant for each case.

²⁶Indeed, 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

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 the defendants and 36,48% by the 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), disagreement 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* (our reference variable), *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.²⁷

4.2 Preliminary results with simple Probit regressions

In order to observe the possible existence of a pro-plaintiff bias in the *Conseil d’État*, we conduct various Probit regressions. Table 9 presents the results of the regressions.

In regression (1), we observe the influence of the variable *Conseil d’État* (*CE*) on the defendant’s chances of success on the whole period, i.e. without any consideration for the selection bias in both Supreme Courts. The three other regressions take into account the reforms allowing both Courts to have control over their dockets. Regression (2) deals with cases before 1987, i.e. the period during which none of the Supreme Courts could select cases. Regression (3) deals with cases between 1987 and 2001, i.e. the period during which only the *Conseil d’État* could select cases. Last, regression (4) deals with cases after 2001, i.e. the period during which both Courts could select cases.

This division into periods allows to compare the effects of reforms and thus allows to have a first distinction between both types of biases. Indeed, during the first period (*Regression (2)*), there

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: $Pr(Prodef = 1|Defappeal = 1) - Pr(Prodef = 1|Defappeal = 0) = [1 - Pr(Prodef = 0|Defappeal = 1)] - [1 - Pr(Prodef = 0|Defappeal = 0)] = Pr(Prodef = 0|Defappeal = 0) - Pr(Prodef = 0|Defappeal = 1)$.

²⁷For a detailed description of each variable, see Bentata (013b), Bentata (013a).

cannot be any selection bias at the Supreme Court's level. The coefficient of the variable *Conseil d'État* (CE_1) allows to observe the difference in the behavior of Judges from both Courts when deciding in favor of plaintiffs or in favor of defendants. During the second period (*Regression (3)*), only the *Conseil d'État* could select cases. The comparison of the coefficients for the variable *Conseil d'État* between the first period (CE_1) and the second period (CE_2) allows to isolate the selection bias peculiar to the *Conseil d'État*. Last, in the third period (*Regression (4)*), the selection bias is present in both Courts: the difference between the coefficient of the variable *Conseil d'État* in this third period (CE_3) and the selection bias peculiar to the *Conseil d'État* already calculated ($CE_2 - CE_1$) allows to measure the selection bias peculiar to the *Cour de Cassation*.

In Table 9,²⁸ we observe that the coefficient of the variable CE varies widely between the four regressions. It is significant and negative for regressions (1) and (3), but non-significant in the other ones. Regression (1), in which the introduction of the reforms at different dates is not taken into account, lets us think that the chances of a pro-defendant judgment are lower in the *Conseil d'État* than in the *Cour de Cassation*. We are led to conclude that there exists a strong pro-plaintiff bias from Judges in the *Conseil d'État*. However, the absence of a division into periods is misleading. The three other regressions show that this essentially comes from a selection bias.

Indeed, only in the second period, in which a selection bias can occur in the *Conseil d'État* but not in the *Cour de Cassation*, the coefficient of the variable CE is significant. In other words, when the selection bias is not taken into account, the conclusions are distorted because selection biases and Judges biases are confused. Besides, it seems that there is no Judges bias since the variable CE is non-significant during the first period. Also, the same variable is non-significant in the third period: once the *Cour de Cassation* is also allowed to select cases, then any difference between the two Courts disappears.

However, we do not draw any definite conclusion at this stage of the study. The approach is relevant as long as: i) the structure of the cases in both Courts is identical; ii) decisions in both Courts do not systematically differ for a same case due to some intrinsic difference between Civil and Administrative Law. The control variables used until now may not suffice to account for the structure of the cases or a difference in the legal logic.²⁹ In such conditions, some unobservable variables could have some significant effect on our results and distort the conclusions. For this reason, we develop in Section 4 a strategy allowing to test whether cases in both Courts are identical on unobservable dimensions. We repeat this strategy within each period. The whole approach allows to take into account some structural differences between Civil and Administrative Law, and to distinguish the two types of biases under consideration.

²⁸To facilitate the highlighting of various biases, the results are presented separately for each period, as depicted earlier. However, results remain unchanged with a unique regression including some interaction variables representing each period. The results of such a regression are given in the Appendix A.

²⁹Comparing the structure of cases between both Courts using mean values is uneasy when all variables are binary.

5 Pro-plaintiff Correction Activities and Case Selection

The research question of this paper is twofold. First, we seek to determine whether Supreme Courts engage in the same level of correction behavior. Second, we aim at testing whether this finding is robust when Courts can select cases they must rule.

5.1 Bias Correction Activities by Supreme Courts

As stated in the institutional section, 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 refer to two kinds of mistakes judges can make when ruling a case. Errors of type 1 correspond to cases where a 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 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 appeal and Supreme Courts. We denote β the pro-defendant bias, with $\beta \in (-\infty, +\infty)$. A higher β 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 $\beta_{1,C}$ the average bias of the appeal courts in the civil jurisdictions, $\beta_{2,C}$ the bias of the Civil Supreme Court (*i.e.* the *Cour de Cassation*), $\beta_{1,A}$ the average bias of the administrative appeal courts, and $\beta_{2,A}$ the bias of the Administrative Supreme Court (*i.e.* the *Conseil d'État*).

The correction behavior of each Supreme Court corresponds to the decisions it makes to correct for the relative bias of the appeal courts. The differences in biases between the appeal and the Supreme Courts are given by:

$$\Delta_i = \beta_{1,i} - \beta_{2,i} \quad (1)$$

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

Although our data do not allow to locate each $\beta_{j,i}$ 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

The next section aims at testing the relative levels of correction activities presented in the previous subsection. 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 decided 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, which introduced the possibility of case selection by Supreme Courts, we consider three periods in turn: when none of the courts could select cases (1956-1987), when only the *Conseil d'Etat* 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 1.³⁰

The objective of the probit estimation consists in comparing the level of correction activities of the two courts. A positive coefficient associated to CE would indicate that the administrative Supreme Court engages in more pro-plaintiff correction than the civil Supreme Court (case 1). On the contrary, a negative coefficient would suggest that the civil Supreme Court engages in

³⁰Estimations of the coefficients associated to the control variables are displayed in the appendix.

more pro-plaintiff correction activities than the administrative Supreme Court (case 3). A coefficient, which is not statistically different from zero, would depict a situation in which Supreme Court have the same intensity of pro-plaintiff correction activities.

Table 1: Results of the Probit regressions

	Full Period <i>Regression (1)</i>	Before 1987 <i>Regressions (2)</i>	Between 1987 and 2001 <i>Regression (3)</i>	After 2001 <i>Regression (4)</i>
<i>Conseil d'État (CE)</i>	-0.289** (0.147)	0.039 (0.228)	-0.753*** (0.238)	-0.270 (0.341)
Controls	Yes	Yes	Yes	Yes
Observations	614	130	315	169
Nagelkerke R^2	0.370	0.420	0.455	0.339

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 suggests that the *Conseil d'État* engages in more pro-plaintiff correction activities than the *Cour de Cassation*. Indeed, the coefficient associated to *CE* is statistically different from zero in the first regression, which covers the entire period. Decomposing the data into three sub-periods yields interesting results. First, prior to 1987, when no Supreme Court could select cases, we do not detect any difference in the intensity of the correction activities of the Courts. Second, between 1987 and 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, that we detect a strongly significant difference in the correction activities between 1987 and 2001, when both courts were able to select.

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 were allowed to select cases between 1987 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 remaining court's decisions. Second, we use permutation tests to ensure that the change in the correction activities is indeed driven by the two reforms and not by real changes in the Supreme Court preferences between 1987 and 2001.

5.3 Counterfactuals

In order to compare the correction activities of the courts, and thus to confirm or infirm results of the previous section, 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 decisions, and to capture the discrepancies between the predictions and the observations. To do so, we proceed in two steps:

- **Step 1.** Using a Multiple Correspondence Analysis (MCA),³¹ we create virtual cases for the *Conseil d'État* (designated as counterfactual cases), which are linear combinations of the cases from the *Cour de Cassation*.
- **Step 2.** We compare the results obtained from real and counterfactual cases of the *Conseil d'État* in order to detect the existence of a systematic difference between both Courts.

In this purpose, we carry out a Multiple Correspondence Analysis (MCA) using the whole set of variables that determine the nature of a case: the identity of the appellant (*Defappeal*), compliance with regulation by the defendant (*Compliance*), the legal ground of the appeal (*Compensation, Proof, Causality, Uncertainty, Procedure*) and the nature of pollution (*Water, Soil, Air, Sea, Noise*). For each case, the MCA gives a value for the first dimension, and we rank all the cases in the same Supreme Court according to this unique value. This allows to calculate the distance d_{ij} between any case j in the *Conseil d'État* and any case i in the *Cour de Cassation* on this first dimension. We then estimate, for each case j in the *Conseil d'État*, the probability of a pro-defendant judgment \tilde{p}_j using the distances of case j with all cases i in the *Cour de Cassation* together with the final decisions for all these cases in the *Cour de cassation*.

The prediction power of a case i in the *Cour de Cassation* for a case j in the *Conseil d'État* is reflected by a factor w_{ij} which depends on the distance d_{ij} that separates the two cases on the first dimension of the MCA.³² Hence, the probability of a pro-defendant judgment \tilde{p}_j for the counterfactual of case j in the *Conseil d'État* is:

$$\tilde{p}_j = \sum_i w_{ij} Prodef_i \quad (3)$$

with $Prodef_i$ the result of the variable *Prodef* for case i in the *Cour de Cassation* and w_{ij} a weighting of the distance from case j in the *Conseil d'État* to case i in the *Cour de Cassation*, such that

³¹The MCA method is depicted in Appendix C.

³²We have chosen to keep only the first dimension of the MCA since we observe that it always explains at least 50% of the variation. Results of MCA for each period are given in the Appendix C. Results remain unchanged when we repeat the whole approach with two or three dimensions. In the later cases, the distance d_{ij} is measured in the following way: $d_{ij} = \frac{\sum_k V_k Dim_{ki}}{\sum_k V_k}$, where V_k is the percentage explained by dimension k in the MCA and Dim_{ki} is the value of dimension k in case i .

$$w_{ij} = \frac{\frac{1}{d_{ij}}}{\sum_i \frac{1}{d_{ij}}} \text{ and } \sum_i w_{ij} = 1.$$

For each case j in the *Conseil d'État*, we thus obtain the probability \tilde{p}_j of a pro-defendant judgment, calculated thanks to a linear combination of characteristics of cases from both Courts and results of cases in the *Cour de Cassation*. This method minimizes the distance, in terms of observable variables, between results of counterfactuals of the *Conseil d'État* and actual cases in the *Cour de Cassation*.

By using counterfactuals, we determine whether there exists a difference between the cases in the *Conseil d'État* and the *Cour de Cassation* that could come from unobserved variables. In this purpose, we carry out an hypothesis test to compare the results of actual and counterfactual cases in the *Conseil d'État*.³³

■ **Statistics.** For each case j in the *Conseil d'État*, where $j = 1, \dots, n$, there exists a positive probability $p_j > 0$ of a pro-defendant judgment. This probability p_j is unobservable. However, the realization of the random variable $Prodef_j$ (success or failure for the defendant) follows a Bernoulli distribution with probability p_j . Hence,

$$Prodef_j \sim \mathcal{B}(p_j). \quad (4)$$

Each random variable $Prodef_j$ has an expected value p_j and a variance $p_j(1 - p_j)$. The reduced centered variable x_j associated with $Prodef_j$ then writes:

$$x_j = \frac{Prodef_j - p_j}{\sqrt{p_j(1 - p_j)}}. \quad (5)$$

The sum s of these reduced centered variables, normalized by the square root of the number of random variables, writes:

$$s = \frac{1}{\sqrt{n}} \sum_j x_j. \quad (6)$$

According to the Central Limit Theorem, the distribution of this variable s tends towards a standard normal distribution when the number n of observations becomes infinite.

■ **Test.** The probability p_j is unknown. However, if the unobservable variables of cases from the *Conseil d'État* and from the *Cour de Cassation* were on average null, we would have:

$$\tilde{p}_j = \mathbb{E}(Prodef_j) = p_j.$$

Hence, the assumption of no difference between unobservable variables in the cases from both

³³The following method has first been tested using a simulation over 10000 observations repeated 1000 times. The results for this simulation are given in the Appendix B.

Courts is defined as:

$$H_0 : \tilde{p}_j = p_j.$$

Rejecting H_0 amounts to demonstrate that cases from both Courts only differ due to unobservable variables. In order to test H_0 , we just have to estimate the value \tilde{x}_j of reduced centered variables of counterfactuals,

$$\tilde{x}_j = \frac{Prodef_j - \tilde{p}_j}{\sqrt{\tilde{p}_j(1 - \tilde{p}_j)}}, \quad (7)$$

and calculate their normalized sum $|\tilde{s}|$

$$|\tilde{s}| = \frac{1}{\sqrt{n}} \sum_j \tilde{x}_j. \quad (8)$$

If the statistics $|\tilde{s}|$ also follows a standard normal distribution, this means that probabilities of pro-defendant judgments in the *Conseil d'État* and the *Cour de Cassation* do not differ for similar cases. In other words, our test amounts to the following decision rule:

$$\begin{cases} \text{if } |\tilde{s}| < 1.96 & \text{we do not reject } H_0 & \text{(judgments do not differ),} \\ \text{if } |\tilde{s}| > 1.96 & \text{we reject } H_0 & \text{(judgments differ).} \end{cases}$$

If H_0 is rejected, this entails that $h(\Delta_C|S_C) \neq h(\Delta_A|S_A)$. The last step consists in assessing the magnitude of the difference in correction activity across Supreme Courts. We thus measure the difference $D(p)$ between the probabilities of a pro-defendant judgment between actual and counterfactual cases of the *Conseil d'État*. We thus compare the mean p_j of the observed results for actual cases in the *Conseil d'État* with the expected value \tilde{p}_j of the results estimated from counterfactual cases. If the gap is negative (resp. positive), this means that counterfactual cases overestimate (resp. underestimate) the likelihood of a pro-defendant outcome, and, thus, that the *Conseil d'État* engages in more pro-plaintiff correction activities than the civil Supreme Court.. Consequently, quantifying the surplus of correction activities of the *Conseil d'État* amounts to observe:

$$D(p) = \sum_j p_j - \tilde{p}_j, \quad (9)$$

$$\text{such that } \begin{cases} \text{if } D(p) < 0, & \text{then } \bar{p}_j < \tilde{p}_j & h(\Delta_A) > h(\Delta_C) \\ \text{if } D(p) > 0, & \text{then } \bar{p}_j > \tilde{p}_j & h(\Delta_A) < h(\Delta_C) \end{cases}$$

■ **Results.** Results (table 2) confirm the observations from previous regressions.³⁴ First, we find that there is no difference in the correction activity of the two courts, when they both have the same control about their dockets. Indeed, the counterfactual analysis is not able to reject

³⁴A graphic analysis of the results is provided in the Appendix D.

the null hypothesis that both courts engage in the same level of pro-plaintiff correction for the two periods where the Courts faced the same selection rule (before 1987 and after 2001). Second, our estimations clearly reject the null hypothesis for the 1987-2001 period: the predictions of the *Conseil d'État's* decisions clearly overestimate the real probability of pro-defendant decisions. In other words, our results show that the *Conseil d'État* has been much more likely to take pro-defendant decisions than the *Cour de Cassation* over the 1987-2001 period. The average overestimation is equal to $\bar{p}_j - \widetilde{p}_j = 19\%$.

Few conclusions can be drawn from this second estimation. First, it confirmed the fact that both Courts engage in the same level of pro-plaintiff correction when they face the same selection rule. This is equivalent to: $h(\Delta_C|S_C = i) = h(\Delta_A|S_A = i)$ for $i \in \{0, 1\}$. Second, regarding the correction activity of the courts between 1987 and 2001, the estimation concluded that $h(\Delta_C|S_C = i) < h(\Delta_A|S_A = i)$, indicating that the *Conseil d'État* increased its pro-plaintiff correction activity during this time period.

We propose to note $P[h(\Delta_j|S_j)]$ the probability that a defendant faces when the Supreme Courts dealing with her case has a correction activity equal to $h(\Delta_j|S_j)$. $P[.]$ is a decreasing function, since a higher degree of pro-plaintiff correction decreases the probability of a pro-defendant decision. The first result implies:

$$P[h(\Delta_C|S_C = i)] = P[h(\Delta_A|S_A = i)], \forall i \in \{0, 1\} \quad (10)$$

The second result can be expressed in the following way:

$$P[h(\Delta_C|S_C = 0)] = P[h(\Delta_A|S_A = 1)] + 0.19 \quad (11)$$

Substituting 10 into 11, it follows:

$$P[h(\Delta_A|S_A = 0)] = P[h(\Delta_A|S_A = 1)] + 0.19$$

$$\underbrace{P[h(\Delta_A|S_A = 1)] - P[h(\Delta_A|S_A = 0)]}_{\text{Effect of the change in selection rule}} = -0.19 \quad (12)$$

$$(13)$$

We therefore estimate the effect of the change in the selection rule on the win rate of the defendants, that follows from the increase in the correction activity of the court: The introduction of the *selection rule* decreased by 19 percentage points the probability that the *Conseil d'État* rules

in favor of the defendant. This suggests that the *Conseil d'État* used the possibility of docket control to increase its pro-plaintiff correction activity. Moreover, given the first result, the second result also implies that the *Cour de Cassation* increased its pro-plaintiff correction activity about the same magnitude.

Table 2: Identification of Courts' biases

	Before 1987 <i>Period (1)</i>	Between 1987 and 2001 <i>Period (2)</i>	After 2001 <i>Period (3)</i>
Test ($H_0 : p_j = \tilde{p}_j$)			
\tilde{s}	0.30	-3.62	-0.71
$p(\tilde{s} < 1.96)$	0.61	0.001	0.24
Difference ($D(p)$)	$D_1 = 0$	$D_2 = -0.19$	$D_3 = 0$
$h(\Delta S)$	$h(\Delta_C S_C = 0) = h(\Delta_A S_A = 0)$	$h(\Delta_C S_C = 0) < h(\Delta_A S_A = 1)$	$h(\Delta_C S_C = 1) = h(\Delta_A S_A = 1)$
	Similar Correction Activity	Stronger correction by the CE	Similar Correction Activity

Robustness test. In order to confirm this last observation, we have repeated the identification strategy by considering each Court separately. For each Court, we have observed the differences between the trial outcomes before and after the reform. For these last two identifications, we have built counterfactuals of cases after the reform from cases judged before the reform and compared the estimated results based on these counterfactuals with the decisions on actual cases after the reform. Results show a significant difference between the two periods of the same magnitude in both Courts, i.e. a similar selection bias starting with the reform. Statistical estimations are the following: $\tilde{s} = -2.16$ and $p - value = 0.015$ for the *Conseil d'État*, $\tilde{s} = -3.06$ and $p - value = 0.001$ for the *Cour de Cassation*.

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 the fact that Courts have changed their preferences over time. In fact, our data would lead to the same conclusions if the *Conseil d'État* changed its views in the 80s, and the *Cour de Cassation's* position started changing at the end of the 90s.

In order to investigate this issue, we propose some tests to insure that our results are indeed driven by the two specific dates 1987 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 5 years before or 5 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 10 years before the reform to 10 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 ($\beta = -0.231$ and $p = 0.021$ for the *Conseil d'État*, $\beta = -0.113$ and $p = 0.023$ for the *Cour de Cassation*).

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 ($p - value > 0.05$). In this case, we cannot reject the assumption that the effect of the reform is simply random.

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 $p - value = 0.0014$ for the *Conseil d'État* and $p - value = 0.0018$ for the *Cour de Cassation*.

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

■ **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' bias. 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 5 years before then 5 years after the reform on the variable *Prodef*. For the *Cour de Cassation*, we thus measure the effects of two temporal variables, one starting in 1996 and the other in 2006 and compare the coefficients obtained with those of the variable *Reform*. For the *Conseil d'État*, we do the same with a variable starting in 1982 and the other in 1992. Table 3 provides the results for both Courts.

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

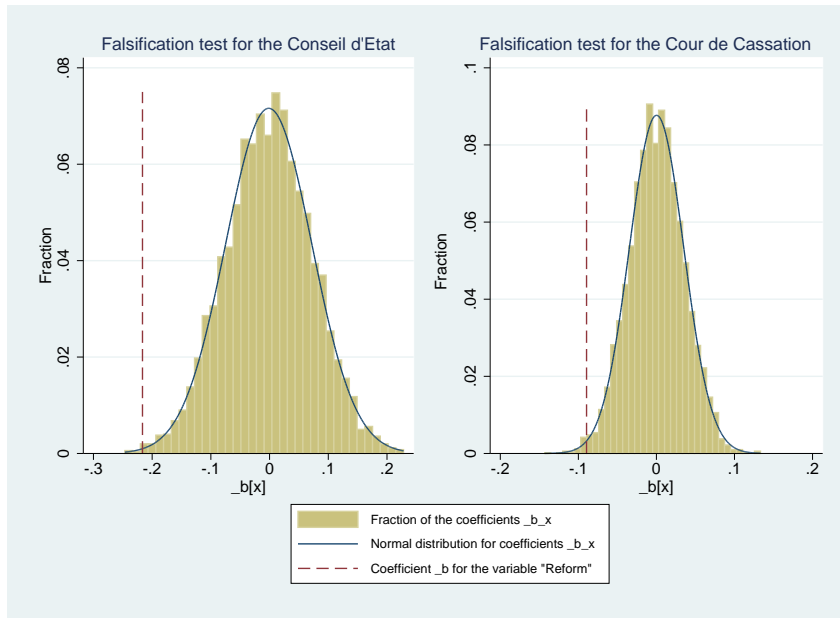


Figure 1: Graphic Results for the Falsification tests with Permutation

Table 3: Comparison of different time periods over Judges' decisions

	Reform	Reform <i>minus</i> 5 years	Reform <i>plus</i> 5 years
<i>Cassation</i>	-0.13*** (0.04)	-0.06 (0.04)	-0.10* (0.05)
<i>Conseil d'État</i>	-0.17** (0.07)	-0.06 (0.08)	-0.16** (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.

shock has influence over Judges' correction activity once the reform is adopted. As a consequence, we cannot reject the assumption that Judges' bias has remained constant over time: in other words, we accept the assumption that the pro-defendant preferences of Judges is constant over time.

5.5 How do Courts select their dockets?

Previous subsections have highlighted the existence of a correction activities of 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:

- **Case 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.

- **Case 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 *Conseil d'État* and the *Cour de Cassation* 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 4 depicts the results of the regression.

Table 4: Results of the Biprobit regression on *Reverse* and *Pro-defendant*

	Coefficients		Marginal effects	
	Reverse	Pro-defendant	Reverse	Pro-defendant
Reform in CE	0.529** (0.227)	-0.855*** (0.240)	-14	-19
Reform in CC	0.455** (0.192)	-0.598*** (0.199)	-11	-18
Def appeal*Reform	-0.837*** (0.241)	0.297 (0.252)	-18	
Defendant appeal	-0.240 (0.146)	-1.128*** (0.150)	-10	-32
Athrho		0.237*** (0.072)		
Controls			Yes	
Observations			614	
McFadden R^2			0.164	

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

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 Courts select cases in order to counter more often lower Courts' decisions. 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 (*Case 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 hypothesis test based on the construction of counterfactual cases) in order to distinguish and quantify the effects of bias correction activities of 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 plaintiff than their respective lower courts. We also showed that the correction activities of the Supreme Courts are similar when both have the same selection mechanism over their dockets. Our investigation showed that reforms giving the Courts control over their dockets increase the probability of a pro-plaintiff decisions by 19 percentage points. Finally, we found that this result was driven by the selection strategies of the Supreme Courts, which have been 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, regarding the correction activities of the Courts, 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 legal systems are similar in terms of difference of bias between Supreme Courts and their Appeal Courts. In both systems, 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 of probability of a pro-plaintiff decision resulting from the selection rule shows that Courts were highly willing to select cases, and thus 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 with regard to the

fact that recent Supreme Courts have undergone the same reforms (Taiwan in the early 2000s³⁵, Brazil in 2007).

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 Appeal Courts. We therefore ignore whether Supreme Courts decide on representative cases or on exceptional cases. 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 wrong 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 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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³⁵See Eisenberg et al. (2012)

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Appendix A.

Table 5 presents the results of a regression in which two interaction variables have been added (Column 3): a variable representing cases judged in the *Conseil d'État* after the 1987 reform and a variable representing cases judged in the *Cour de Cassation* 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 9). The regression without interaction variable lets us believe to a pro-plaintiff bias in the *Conseil d'État*. This effect is indeed related to an earlier introduction of the reform in the *Conseil d'État*. 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 χ^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 5: 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)	-0.289** (0.147)	0.005 (0.221)
CE after Reform only	-	-0.691*** (0.273)
CC after Reform only	-	-0.453*** (0.158)
Defendant appeal	-1.088*** (0.120)	-1.097*** (0.122)
Compliance	0.779*** (0.129)	0.872*** (0.133)
Proof	-0.464*** (0.175)	-0.475*** (0.177)
Causality	-0.171 (0.152)	-0.219 (0.154)
Uncertainty	0.008 (0.196)	0.013 (0.198)
Procedure	-0.038 (0.161)	-0.046 (0.163)
Soil	-0.391** (0.172)	-0.329* (0.178)
Air	-0.571*** (0.196)	-0.523*** (0.200)
Sea	-0.317 (0.329)	-0.065 (0.347)
Noise	-0.060 (0.152)	-0.107 (0.155)
Constant	0.190 (0.165)	0.269 (0.169)
Observations	614	614
LR test	192.441	206.926
Nagelkerke R^2	0.370	0.393
Cox Snell R^2	0.269	0.286
C-stat (%)	76.71	76.87

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

Appendix C.

■ **Multiple Correspondence Analysis (MCA).** Multiple Correspondence Analysis is an extension of Principal Correspondence Analysis which allows to analyze the patterns of relationships of several categorical or binary variables. The aim of this approach is to define a few number of variables (called dimensions) which summarize and explain the patterns of a large number of variables. In doing so, individuals and variables can be "mapped" on a graph (generally a one or two axis graph) according to their similarities, so that only one or two values will summarize the different characteristics of each individual.

Technically, MCA is obtained by analyzing the indicator matrix (matrix of individuals x variables) to determine the relationships (associations) between the different variables. These relationships measure the distance between different variables and different individuals, a small distance indicating that patterns are similar. Hence, variables sharing the same pattern (taking simultaneously the same value for the majority of individuals) are grouped and the difference between these groups are maximized in order to determine the vectors that best explain the pattern of each group of variables. Vectors are called dimensions, and their relative variance is called inertia. As in Principal Component Analysis, the first dimension explains the largest part of the patterns of each group of variables, the second dimension is the second most important to explain the patterns and so on. These dimensions allow to observe the degree of similarity between different individuals, by classifying them according to their value on the first dimension (or by a combination of their value on each dimension).

The following tables provide the MCA results for the first two dimensions and for the three observed periods. For the three periods, the first dimension explains more than 50% of the general patterns of the variables. For this reason, we have restricted our MCA analysis to this first dimension. In our preliminary studies, we also ran MCA with two and three dimensions to create our counterfactuals and the results remained unchanged.

Table 6: Results of the MCA for period (1)

	Principal Inertia	Percent	Cumul. percent
Dimension 1	0.0105	50.94	50.94
Dimension 2	0.0030	14.56	65.50
Total	0.0205	100	

Table 7: Results of the MCA for period (2)

	Principal Inertia	Percent	Cumul. percent
Dimension 1	0.0119	56.16	56.16
Dimension 2	0.0028	13.41	69.57
Total	0.0252	100	

Table 8: Results of the MCA for period (3)

	Principal Inertia	Percent	Cumul. percent
Dimension 1	0.0236	82.75	82.75
Dimension 2	0.0001	0.24	61.99
Total	0.0285	100	

Appendix D.

■ **Simulations.** In order to check the validity of our statistical test, we simulate the results of trials and observe the statistical distribution of *pro-plaintiff* or *pro-defendant* outcomes under various assumptions. For each simulation, we create 10000 observations, which are only characterized by one variable defined as the probability to obtain a *pro-defendant* outcome. We assume that the realization of each random variable follows a Bernoulli distribution (with an expected probability p_j of obtaining a *pro-defendant* outcome) over the range $[0.2, 0.4]$.

We create three statistics:

- Statistic s_0 corresponds to the true value of p_j (we know the Bernoulli parameter for each observation). It follows a standard normal distribution.
- Statistic s_0^+ systematically overestimates the value of p_j by 0.05 ($\tilde{p}_j = p_j + 0.05$). This represents a situation where the *Conseil d'État* is less often *pro-defendant* than the *Cour de Cassation*.
- Statistic s_0^- systematically underestimates the value of p_j by 0.03 ($\tilde{p}_j = p_j - 0.03$). This represents a situation where the *Conseil d'État* is more often *pro-defendant* than the *Cour de Cassation*.

We simulate 1000 times the process in order to determine the distribution of our statistics. Figure 2 presents the results of this simulation.

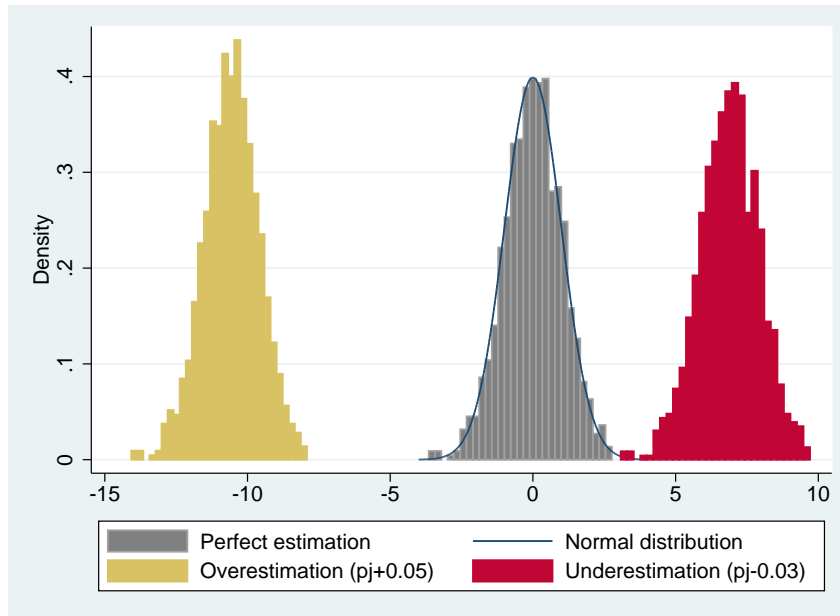


Figure 2: Graphic results of the simulation

Some few observations can be formulated:

- First, the distribution s_0 is a good approximation of a standard normal distribution. When H_0 holds true ($H_0 : p_j = \tilde{p}_j$), our statistic behaves as expected.
- Second, when the win rates of defendants are overestimated, the distribution of s_0^+ is on the

left of s_0 . Said differently, negative values suggest that cases in the *Conseil d'État* are less often favorable to defendants than cases in the *Cour de Cassation*.

- Third, when the win rates of defendants are underestimated, the distribution of s_0^- is on the right of s_0 . Hence, positive values indicate that cases in the *Conseil d'État* are more often favorable to defendants than cases in the *Cour de Cassation*.

■ **Results.** Figure 3 gives the results of the equivalence hypothesis test for outcomes of both Courts for each of the three periods. The null hypothesis (H0) is that there is no difference between actual and counterfactual cases of the *Conseil d'État* due to unobservable variables. If H0 is accepted, then there exists no difference between the mean of the normal distribution s of cases (where $s = \frac{1}{\sqrt{n}} \sum_j x_j$) and the value of \tilde{s} . In other words, if the value of the statistic \tilde{s} belongs to the middle 95% of the normal distribution (i.e. $|\tilde{s}| < 1.96$), then cases from both Courts do not differ due to unobservable variables.

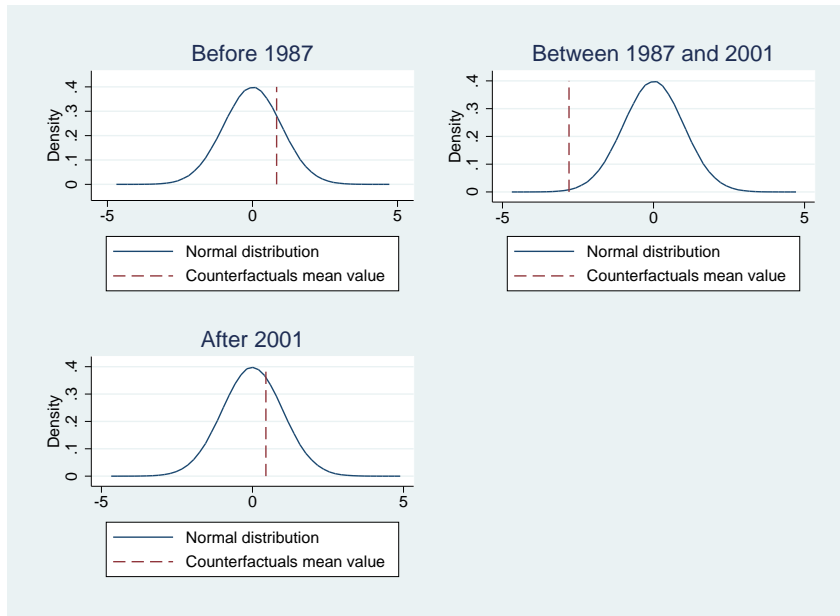


Figure 3: Graphic Results for the tests on Counterfactuals

Only the test corresponding to the second period shows a difference between the two distributions: the value of the statistic \tilde{s} is -2.79 ($p = 0.002$) and belongs to the extreme 5% of the normal distribution s . The null hypothesis of no difference between actual and counterfactual cases is even rejected at the 1% level. This means that the probability to observe similar decisions in both Courts during this period is very low and this low probability is due to unobservable variables. Hence, for this second period, counterfactuals lead to an overestimation of the chances of a pro-defendant decision ($\tilde{p}_j > p_j$). In other words, for this period (and only for this period), chances of a pro-defendant outcome are lower in the *Conseil d'État* than in the *Cour de Cassation*.

Tables and Figures

Table 9: Full results of the Probit regressions

	Full Period <i>Regression (1)</i>	Before 1987 <i>Regressions (2)</i>	Between 1987 and 2001 <i>Regression (3)</i>	After 2001 <i>Regression (4)</i>
<i>Conseil d'État</i> (CE)	-0.289** (0.147)	0.039 (0.283)	-0.753*** (0.238)	-0.270 (0.341)
Defendant appeal	-1.088*** (0.120)	-0.589** (0.292)	-1.348*** (0.175)	-0.861*** (0.240)
Compliance	0.777*** (0.129)	1.241*** (0.311)	0.710*** (0.184)	0.738** (0.293)
Proof	-0.464*** (0.175)	-0.265* (0.382)	-0.595** (0.267)	-0.483 (0.329)
Causality	-0.171 (0.152)	-0.679* (0.352)	-0.153 (0.221)	-0.182 (0.327)
Uncertainty	0.008 (0.196)	-1.116 (0.733)	0.173 (0.271)	-0.045 (0.381)
Procedure	-0.038 (0.161)	0.406 (0.358)	-0.090 (0.233)	-0.199* (0.331)
Soil	-0.391** (0.172)	-0.563 (0.451)	-0.124 (0.276)	-0.635** (0.300)
Air	-0.571*** (0.196)	0.072 (0.567)	-0.525* (0.272)	-0.813 (0.379)
Sea	-0.317 (0.329)	-0.343 (1.166)	na	-0.307 (0.399)
Noise	-0.060 (0.152)	0.004 (0.370)	-0.138 (0.211)	-0.289 (0.346)
Constant	0.190 (0.165)	-0.389 (0.379)	0.524** (0.227)	0.035 (0.399)
Observations	614	130	315	169
LR test	192.441	46.180	128.685	46.044
Nagelkerke R^2	0.370	0.420	0.455	0.339
Cox Snell R^2	0.269	0.299	0.336	0.238
C-stat (%)	76.71	80.00	78.34	76.92

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

Table 10: Results of the Biprobit regression on *Reverse* and *Pro-defendant*

	Coefficients		Marginal effects	
	Reverse	Pro-defendant	Reverse	Pro-defendant
Reform in CE	0.529** (0.227)	-0.855*** (0.240)	-14	-19
Reform in CC	0.455** (0.192)	-0.598*** (0.199)	-11	-18
Def appeal*Reform	-0.837*** (0.241)	0.297 (0.252)	-18	
Defendant appeal	-0.240 (0.146)	-1.128*** (0.150)	-10	-32
Compliance	0.243* (0.125)	0.883*** (0.134)	-8	29
Proof	0.169 (0.166)	-0.493*** (0.177)		
Causality	-0.220 (0.152)	-0.219 (0.155)		-9
Uncertainty	-0.199 (0.197)	0.027 (0.198)		
Procedure	0.089 (0.158)	-0.056 (0.163)		
Soil	-0.090 (0.164)	-0.336* (0.178)		-11
Air	-0.407** (0.188)	-0.492** (0.200)	-11	-13
Sea	0.006 (0.325)	-0.063 (0.341)		
Noise	-0.294* (0.152)	-0.096 (0.156)	-8	
Constant	-0.514*** (0.169)	0.293* (0.171)		
Athrho		0.237*** (0.072)		
Observations	614			
Log-likelihood	-621.796			
Wald χ^2	211.45			
McFadden R^2	0.164			
Rho	0.233 (0.068)			

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