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Analyse Économique de la Norme Juridique : des Origines Constitutionnelles à la Mise en Œuvre par le Juge

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Analyse Économique de la Norme Juridique : des Origines Constitutionnelles à la Mise en Œuvre par le Juge

Résumé :

Les questions de légitimité et de stabilité des systèmes politiques ont longtemps été étudiées séparément des problèmes de mise en œuvre du droit en sciences économiques. L'objectif de cette thèse est concilier ces différentes approches afin de replacer la mise en œuvre de la norme juridique au centre du débat institutionnel. Ce travail se décompose en cinq investigations empiriques ou expérimentales portant chacune sur une des étapes du processus politique et judiciaire.

Le premier article s'intéresse à l'impact des droits constitutionnels sur les dépenses publiques. La seconde étude explore l'influence des biais d'auto-complaisance sur la demande et l'offre de redistribution. Le troisième travail analyse les décisions rendues par le Conseil Constitutionnel. La quatrième partie examine la réforme de la carte judiciaire des Conseils de Prud'hommes de 2008. Le dernier chapitre étudie la relation entre la composition syndicale des Conseils de Prud'hommes et les issues des litiges qui y sont portés.

Nos analyses reposent sur les outils économétriques et expérimentaux. Elles font usage de méthodes d'estimations classiques (*OLS, GLS, Probit, Logit, Within OLS*), de modèles à sélection (*Heckman, Triprobit*), des outils destinés aux problèmes d'endogénéité (*2SLS*) et des techniques d'estimation de systèmes d'équation (*3SLS*). L'approche expérimentale contient également des tests statistiques communément appliqués (*tests de permutation*, *tests de comparaison de moyenne*, *tests de proportion*) ainsi que de récentes méthodes pour traiter l'hétérogénéité (*wild clustering*).

Descripteurs :

Économie du droit, Économie politique, Économie constitutionnelle, Économétrie appliquée, Économie expérimentale, Norme juridique, Constitution, Conseil Constitutionnel, Biais d'auto-complaisance, Conseils de Prud'hommes, Justice, Délais, Résolution de litige.

Economic Analysis of Legal Norms: from the Constitutional Origins to the Enforcement by the Judge

Title and Abstract:

The legitimacy and the stability of political systems have very often been studied in economics separate from considerations about legal norms' enforcement. My objective is to combine these different approaches, and to place the question of the legal enforcement at the heart of the debate about institutions. This work is made of cinq empirical and experimental investigations that deal with each of the stages of the political and legal process.

This first paper analyzes the impact of constitutional rights on public expenditures. The second article explores the influence of self-serving biases on the demand and the supply of redistribution. The third analysis focuses on the decisions of the French Constitutional Council. The fourth work deals with the recent reform of the judiciary map of French labor courts. The last study investigates the relationship between the composition of the elected jurors in French labor courts and the way cases are terminated.

Our investigations rely on econometric and experimental techniques. They use standard estimation methods (*OLS, GLS, Probit, Logit, Within OLS*), selection models (*Heckman, Triprotibt*), techniques for endogeneity correction (*2SLS*), and methods to estimate systems of equations (*3SLS*). The experimental analysis makes use of standard statistical tests (*permutation tests, proportion tests, two-group mean-comparison tests*), and more recent methods to solve heterogeneity (*wild clustering*).

Keywords:

Law and Economics, Political Economy, Constitutional Economy, Applied econometrics, Legal norm, Constitution, Constitutional Courts, Self-serving bias, Labor Courts, Justice, Delays, Litigation.

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Introduction

> Celui qui fait la loi sait mieux que personne comment elle doit être exécutée et interprétée. Il semble donc qu'on ne saurait avoir une meilleure constitution que celle où le pouvoir exécutif est joint au législatif : mais c'est cela même qui rend ce gouvernement insuffisant à certains égards, parce que les choses qui doivent être distinguées ne le sont pas, et que le prince et le souverain, n'étant que la même personne, ne forment, pour ainsi dire, qu'un gouvernement sans gouvernement.
>
> Jean-Jacques Rousseau (1762), Du Contrat Social

Les changements constitutionnels récemment observés dans les pays du Maghreb ont réouvert les questions de la légitimité et de la stabilité des systèmes politiques. Dans la décennie qui a précédé les Printemps arabes, on avait pourtant assisté à la marginalisation du débat constitutionnel. Celui-ci avait perdu en vigueur après s'être imposé avec force dans les années 1990, au lendemain de la chute du bloc soviétique. L'exemple chinois d'une autocratie au développement économique florissant avait mis à mal les théories de fin de l'Histoire développées par Francis Fukuyama. Le regain d'autoritarisme en Russie voulu par Vladimir Poutine a conduit les courants de pensée les plus optimistes à s'interroger sur un éventuel reflux autocratique, venant contrecarrer les vagues de démocratisation post URSS.

Les Printemps arabes ont cependant porté une nouvelle fois sur le devant de la scène la question constitutionnelle. Ces soulèvements ont remis en cause la légitimité du système politique et institutionnel dans son intégralité, entraînant ainsi son instabilité. Malgré l'hétérogénéité des situations auxquelles ils faisaient face, les peuples arabes partageaient cependant des griefs communs à l'encontre de leur système politique. Les remises en cause de la légitimité de leur système politique ne se sont en effet pas opérées *par le haut*, à savoir par une remise en cause du système institutionnel tel que défini par la constitution, mais *par le bas*, à savoir par un rejet des normes produites par cet ensemble institutionnel. La corruption généralisée a créé une distorsion entre les aspirations populaires et les normes produites par l'Etat. Cette distorsion s'est agrandie au point que la population a retiré son consentement à être gouverné.

L'exemple singapourien démontre au contraire qu'un processus autoritaire peut demeurer stable dans la mesure où la production de normes répond aux attentes de la population. S'il est communément admis que la stabilité de l'appareil politique singapourien n'est rendu possible que par son extraordinaire croissance, les responsables politiques nationaux ont néanmoins entrepris plusieurs réformes en vue d'ouvrir le pays, de sécuriser les investissements étrangers et, ainsi, de rendre possible une telle croissance. Les récentes élections[1] ont conforté le système autocratique, matérialisant la satisfaction des citoyens avec un système

[1] `http://www.lemonde.fr/international/article/2015/09/12/a-singapour-le-parti-au-pouvoir-conforte-son-emprise-sur-le-pays_4754027_3210.html` (Dernier accès : Septembre 2015)

peu démocratique mais propice à la croissance. Ainsi, la stabilité du système institutionnel de Singapour s'opère *par le bas* : une grande majorité des citoyens ne remet pas ouvertement en cause la légitimité du système aussi longtemps que la production et la mise en œuvre des normes qu'il produit la satisfait.

Ces exemples montrent que la stabilité et la légitimité des systèmes politiques ne peuvent être appréciées qu'en embrassant l'intégralité du processus législatif et juridique : la satisfaction des citoyens avec le système politique dépend essentiellement des règles de droit produites et mises en œuvre. Les règles de droit sont le résultat d'un processus politique complexe sous-tendu par un ensemble de règles décisionnelles préalablement définies. Leur détermination présuppose l'existence d'un environnement institutionnel, i.e. une constitution[2], qui organise et délimite le processus de prise de décision. Dans ce environnement institutionnel[3], les acteurs politiques débattent et créent les règles de droit. Ces dernières sont mises en œuvre par un système juridique, également produit par le processus législatif.

Positionnement. Les analyses menées par la littérature en économie du droit (*Law and Economics*) se sont majoritairement concentrées sur l'application des normes juridiques et leur impact sur les comportements des citoyens. A contrario, les études conduites en économie politique (*Political Economy*) se sont principalement intéressées au processus politique et législatif conduisant à la détermination des règles de droit. Enfin, l'économie politique constitutionnelle (*Constitutional Political Economy*), initiée par Buchanan et Tullock, s'est focalisée sur les choix sous-tendant l'environnement institutionnel permettant la création de normes de droit.

Ces trois axes de recherche se sont ainsi intéressés à plusieurs étapes du processus politique et institutionnel déterminant la règle de droit, depuis la structure nécessaire à sa création à sa mise en œuvre par les juges. Ces champs de recherche sont restés, de fait, cloisonnés dans leurs approches respectives. Ceci s'explique principalement par la très grande difficulté à modéliser les systèmes légal et politique d'un seul bloc, eu égard aux nombreuses dimensions à intégrer. Ce cloisonnement a cependant conduit à découper les processus constitutionnel, législatif et juridique, excluant ainsi du débat les potentiels effets de retour (*feedback effects*). Les travaux empiriques en économie politique constitutionnelle ont montré que les choix constitutionnels sont contingents à de nombreux facteurs, tel que le mode même de prise de décision des règles constitutionnelles. Ainsi, une constitution peut représenter des intérêts déjà propres à certains partis politiques, et la compétition politique subséquente s'en trouve ainsi biaisée. Une constitution favorisant un parti lui

[2]Nous entendons le terme "constitution" au sens strict, à savoir l'ensemble des règles formelles, substantielles ou procédurales, écrites dans le texte appelé *Constitution* ou *Loi Fondamentale*. Les règles procédurales incluent généralement les compétences des pouvoirs législatif, exécutif et judiciaire, la définition d'une ou plusieurs institutions, telles que les Cours Suprêmes.

[3]Nous entendons ici le terme "environnement institutionnel" comme défini par les travaux de North. Ménard (2003) le synthétise ainsi : "L'environnement renvoie aux règles du jeu, règles politiques, sociales, légales, qui délimitent et soutiennent l'activité transactionnelle des acteurs." (p. 105)

donnera un avantage lors du vote des normes de droit. La recherche de rentes pour ce parti en sera aussi favorisée. Les travaux en économie politique ont bien perçu l'influence des règles institutionnelles sur les décisions politiques, mais très peu ont cherché à endogénéiser la décision de la règle institutionnelle vis-à-vis de la règle de droit elle-même. En effet, les règles institutionnelles déterminant partiellement les règles de droit, les citoyens insatisfaits de ces dernières sont en mesure de remettre en question les règles institutionnelles qui les déterminent : Les citoyens représentent le pouvoir souverain, et demeurent ainsi les décideurs ultimes des règles institutionnelles. La satisfaction des citoyens avec ces règles dépend en outre de la mise en œuvre de la norme : la question institutionnelle se trouve ainsi confrontée à la question de la nature de la norme mise en place, ainsi qu'à celle de l'application de cette norme. C'est cette dernière dimension qui a mobilisé une très grande partie de l'attention de la littérature en économie du droit : les effets des règles de droit ne peuvent s'apprécier qu'à la lumière de leurs possibilités de mise en œuvre par le système judiciaire. Une règle de droit conforme aux attentes de la population ne satisfera cependant pas les citoyens si ces derniers n'ont pas de moyens de la faire valoir par des mécanismes juridiques. Le processus institutionnel, législatif, puis juridique peut alors être perçu comme un cycle où les préférences des citoyens en forment les points de départ et d'arrivée.

Contribution. Les travaux présentés dans cette thèse s'intéressent à l'intégralité de ce processus de création et d'application des normes de droit. Chacun de ces travaux est une photographie d'une des étapes de ce processus que nous décomposons en plusieurs étapes : la création d'un cadre institutionnel préalable à la prise de décision, la création de normes juridiques dans ce cadre institutionnel, le contrôle de constitutionnalité de ces normes, la mise en place d'un système judiciaire permettant la mise en œuvre de ces normes, et l'application de ces normes par le système judiciaire.

Cette thèse propose d'analyser toutes ces dimensions, une à une, afin d'apporter un éclairage complet sur la vie d'une norme juridique. Elle n'a cependant pas pour ambition d'expliquer conjointement les décisions constitutionnelles, législatives, juridiques et politiques. L'objectif premier de cet ouvrage est d'apporter une meilleure compréhension de chacune des étapes du processus afin d'élargir des champs de recherche parfois trop cloisonnés. Notre travail replace ainsi les préférences des citoyens au cœur de la question juridique, en retraçant son origine à la constitution, et s'écarte ainsi de la perspective de l'économie du droit, qui considère le citoyen comme point d'arrivée. De telles propositions mettent ainsi en question des hypothèses largement —mais non unanimement— acceptées : les préférences des citoyens étant le point de départ, et ainsi l'objectif premier des systèmes légaux, la recherche d'efficacité des normes juridiques telle que formulée par Richard Posner est-elle réellement pertinente ?

Méthode. Bien que les problèmes étudiés ici aient reçu l'attention de nombreux champs disciplinaires, à commencer par la philosophie politique, notre approche relève de l'écono-

mie. Elle y emprunte les méthodes quantitatives, qui fournissent le cœur de l'analyse, et repose, sauf mentions contraires, sur les hypothèses standards des travaux en économie, telle que la rationalité des agents. Notre approche repose essentiellement sur deux champs techniques développés par les sciences économiques. D'une part, notre analyse est majoritairement empirique, et fait une utilisation extensive des outils économétriques. Elle fait appel, entre autres, à des méthodes de données de panel (*pooled* and *within* OLS), à des techniques de traitement d'endogénéité (variables instrumentales), à des méthodes de correction d'hétéroscédasticité (*robust standard errors*, *wild clustering*), à des outils d'évaluation de systèmes d'équation (*3SLS*), ainsi qu'à des mesures spécifiques aux modèles avec étapes de sélection (*Heckman estimates*, *triprobit*). D'autre part, le second article de cette thèse utilise des méthodes économie d'expérimentale. En sus des discussions méthodologiques propres à ce champ disciplinaire, on y retrouve des méthodes statistiques qui lui sont caractéristiques (*permutation tests*, *two-group mean-comparison tests*, *wild clustering*). Ces travaux ont pour but de contribuer aux trois courants de pensée évoqués ci-dessus. Parce qu'ils lient des sujets de préoccupation très divers, ces articles n'ont pas l'ambition de répondre à l'intégralité des problèmes soulevés par ces domaines de recherche. Toutefois, en créant des liens entre ces disciplines, cette thèse souhaite enrichir les débats et propose une "nouvelle vue de la cathédrale".[4]

Approche. Notre étude tire des trois champs disciplinaires dont elle s'inspire les présupposés méthodologiques qui font leur singularité. Tout d'abord, notre approche considère les règles de droit comme des objets pouvant faire l'étude d'analyses quantitatives. Dans la lignée de l'économie du droit, nous considérons en effet que ces règles de droit peuvent être anticipées par les agents qu'elles concernent et que ces derniers sont ainsi à mêmes de développer des comportements stratégiques en vue d'y répondre. De plus, nous inscrivons notre méthode dans le *réalisme juridique*, et refusons par là même le *formalisme juridique*. En d'autres termes, nous réfutons l'idée qu'un texte de loi contienne en soi une norme objective. À nos yeux, les règles de droit n'existent que par l'interprétation que leur donnent les juges qui les mettent en œuvre.

Nous faisons également nôtres les caractéristiques propres à l'économie politique telles qu'énoncées par Kornhauser (2011). Premièrement, nous admettons que les responsables politiques puissent avoir des intérêts propres qui divergent de l'intérêt général communément admis en économie du droit. Deuxièmement, nous évaluons la qualité du système politique et institutionnel à la lumière du consensus politique qu'il suscite dans la lignée des travaux de Buchanan. Enfin, nous ne supposons aucune volonté commune parmi les législateurs qui sous-tendrait les règles de droit, rejetant ainsi l'hypothèse d'instrumentalisme légal (*rule instrumentalism*) : les lois votées par le Parlement reflètent avant tout un équilibre politique aux intérêts et interprétations multiples, ne pouvant donner lieu à une seule volonté d'interprétation.

Enfin, notre approche s'inscrit entre l'économie politique constitutionnelle et l'écono-

[4]En référence à l'article de Calabresi & Melamed (1972).

mie politique radicale (selon les termes de Kornhauser (2011)) dans sa vision de l'instrumentalisme institutionnel. Nous ne supposons en effet nulle volonté commune derrière les règles institutionnelles, rejetant ainsi les doctrines originalistes d'interprétation des constitutions.[5] Nous ne rejetons cependant pas totalement cette hypothèse, en admettant qu'il puisse y avoir un consensus large et précis autour des institutions.

La suite de cette introduction présente les motivations propres à chacune des parties de cette thèse. Elle introduit également les méthodes et résume les résultats de chacune d'entre elles.

L'émergence des Systèmes Constitutionnels

De l'utilité des constitutions. La définition d'un cadre institutionnel pour le processus de création de règles juridiques représente la première étape du mécanisme aboutissant à la mise en œuvre de la norme de droit. La génèse du cadre institutionnel a été l'objet de nombreuses attentions en théorie du choix public (*Public Choice*), et davantage encore en philosophie politique.

La perception moderne de l'environnement institutionnel, aboutissant sur la notion de Constitution, prend ses racines dans la *Glorious Revolution*, par la remise en cause du pouvoir établi, et par la naissance du concept de contrat social.[6] Les travaux de Locke ont eu une importance capitale pour les travaux ultérieurs menés en économie politique constitutionnelle, dans la mesure où Buchanan fonde le choix constitutionnel sur le libre arbitre des constituants, concept développé par le philosophe britannique. Locke a en effet été un des premiers à lier la légitimité de l'Etat au consentement des citoyens à être gouvernés. Puisque la légitimité, et donc la stabilité, de l'Etat ne s'opèrent qu'au travers du consentement populaire, Locke en déduit qu'une plus grande participation des citoyens à la prise de décision est indispensable. Le débat qui focalise l'attention de Locke est celui de la redistribution, et c'est cette même question qui fera le lit de la Révolution Américaine : le prélèvement d'un impôt ne peut se faire qu'en associant les contribuables au processus décisionnel qui le détermine.[7] La boîte de Pandore ouverte par Locke sera reprise par les libertariens, à commencer par John Stuart Mill, qui considèrent l'argument du consentement dans sa dimension la plus extrême : un système n'est légitime que dans la mesure

[5] L'originalisme affirme que les constitutions doivent être interprétées selon la volonté qu'avaient les constituants à l'époque de leur ratification. Cette théorie présuppose ainsi qu'il n'y avait qu'une seule volonté déterminant l'écriture de la constitution.

[6] Nous entendons ici le *contrat social* comme l'accord implicite entre les gouvernants et les gouvernés sur la légitimité de l'Etat à gouverner. Les premières discussions d'un tel accord implicite se trouvent dans les travaux de Hobbes.

[7] Locke (1690) : "Ce sont là les bornes et les restrictions que la confiance qu'une société a prise en ceux qui gouvernent, et les lois de Dieu et de la nature ont mises au pouvoir législatif de chaque État, quelque forme de gouvernement qui y soit établie. [...] La troisième, qu'on n'imposera point de taxes sur les biens propres du peuple, sans son consentement, donné immédiatement par lui-même ou par ses députés." (§142)

où les règles qu'il impose ont été consenties par tous les individus concernés par ces règles.[8]

Le consentement aux règles de droit améliore la légitimité des systèmes, et contribue ainsi à leur efficacité, mais augmente par là même les coûts décisionnels. En considérant le système libertarien dans sa singularité la plus poussée, les gouvernements devraient limiter leurs politiques publiques à l'ensemble des politiques Pareto améliorantes, à savoir celles qui satisfont ou indiffèrent tous les citoyens concernés par ces mesures. Une telle limitation est de fait illusoire, dans la mesure où le fait politique consiste, dans la très grande majorité des cas, à s'accorder sur des problèmes où les intérêts des parties divergent. De plus, un système décisionnel unanime augmenterait considérablement les risques de rente de la part des minorités de blocages (Rae (1975)). Si la théorie du consentement a ouvert la voie à davantage de légitimité pour les systèmes politiques, les avancées démocratiques se sont néanmoins accompagnées de coûts décisionnels plus élevés.

L'arbitrage entre coûts décisionnels et légitimité du système politique s'est effectué au moyen des constitutions. S'il est impossible que les citoyens s'accordent unanimement sur chaque question politique, Levinson (2011) rappelle qu'il leur est possible de s'accorder sur des règles de décision qui détermineront la manière de trancher les débats politiques. En effet, Levinson rappelle qu'il est plus aisé d'obtenir un accord sur des règles de décision que sur des politiques publiques, car un environnement institutionnel ne correspond pas nécessairement à une politique publique précise, mais à un ensemble de politiques publiques atteignables. Ainsi, alors qu'un consensus sur une politique donnée demeure difficilement atteignable, il est possible que toutes les parties s'accordent sur le processus décisionnel.

Contenu et fonctions des constitutions. Les constitutions matérialisent ainsi une partie du contrat social, et permettent aux institutions de disposer d'une large légitimité au sein de la population, tout en permettant aux gouvernants de décider des politiques publiques à la majorité, et non à l'unanimité. Les constitutions peuvent être perçues comme une émanation écrite du contrat social qui comporte des règles tant procédurales que substantielles. Les règles procédurales concernent les environnements institutionnels qui définissent l'organisation des pouvoirs publics ainsi que les modalités de prise de décision. Rentrent dans cette catégorie les règles de majorité qualifiée, la responsabilité du gouvernement face au Parlement, ou encore les procédures accélérées de prise de décision. Les règles substantielles se réfèrent aux dispositions imposant une nature substantielle aux politiques publiques mises en place par l'Etat. Les droits de l'homme représentent la majorité des règles substantielles, mais certaines dispositions, à l'instar du principe d'indivisibilité de la République, peuvent aussi être catégorisées comme règles substantielles.

Dans cette perspective, l'élaboration d'une constitution correspond au premier maillon

[8]Mill (1859) déclare : "Une société quelle que soit la forme de son gouvernement n'est pas libre, à moins de respecter globalement ces libertés ; et aucune n'est complètement libre si elles n'y sont pas absolues et sans réserves. L'humanité gagnera davantage à laisser chaque homme vivre comme bon lui semble qu'à le contraindre à vivre comme bon semble aux autres".

de la chaîne aboutissant à la mise en œuvre d'une norme juridique. Les choix constitutionnels dans les pays démocratiques se font à la recherche d'un équilibre entre un consensus populaire large et des coûts décisionnels faibles. Les environnements constitutionnels dans les pays autocratiques répondent à d'autres objectifs, et cherchent souvent à obtenir non pas un large soutien populaire, mais l'appui d'une élite privilégiée soutenant le régime (Albertus & Menaldo (2012)). Ainsi, la violation de la constitution dans les pays autocratiques est dangereuse pour le dictateur dans la mesure où il menace l'équilibre des intérêts qui a conduit à son arrivée au pouvoir.

Travaux précédents sur l'analyse empirique des constitutions. Le choix constitutionnel est ainsi primordial afin de comprendre le processus de création de règles juridiques. L'influence des règles procédurales sur les politiques publiques a fait l'objet d'études de plusieurs travaux quantitatifs. D'une part, Persson & Tabellini (2004) et Persson & Tabellini (2005) ont étudié l'impact des règles électorales et des formes de régime (présidentiel ou parlementaire) sur les dépenses des Etats. Ces auteurs mettent en évidence un impact significatif des règles électorales et des formes de régime sur les dépenses publiques, tandis que Blume et al. (2009) ne trouvent pas de relation causale entre les formes de régime et les dépenses publiques mais confirment un impact des règles électorales. Blume et al. (2009) montrent en effet que les systèmes majoritaires ont des dépenses publiques moins élevées que les systèmes proportionnels. D'autre part, certains travaux se sont intéressés à l'impact des règles constitutionnelles substantielles. Bjørnskov & Mchangama (2013) ont analysé les conséquences des droits sociaux inscrits dans les constitutions. Les auteurs ne trouvent aucun effet positif à long terme à l'introduction de ces droits sur les dépenses publiques. Law & Versteeg (2013) analysent aussi l'impact des droits constitutionnels sur la situation des droits de l'homme. Les auteurs montrent que la mise en place effective de ces droits dépend fortement de la région dans laquelle se situe le pays et de la catégorie de droit étudié : les droits socio-économiques sont moins souvent respectés, et les pays africains et asiatiques ont tendance à promettre plus de droits qu'ils n'en mettent en place.

Cadre théorique de l'analyse. Le premier article de cette thèse contribue à cette littérature en apportant des éléments de discussion sur l'impact des règles substantielles sur les dépenses publiques. Cet article explore le lien causal entre le nombre de droits constitutionnels et le montant des dépenses publiques. Ce travail prend comme point de départ le cadre théorique proposé par Voigt (2011) qui décrit les règles constitutionnelles procédurales comme des biens publics au sens économique du terme : ils sont non-rivaux (les bénéfices qu'en tirent les uns n'affectent en aucun cas les bénéfices qu'en tirent les autres) et non-exclusifs (tous les citoyens bénéficient d'institutions transparentes et démocratiques). Notre analyse étend l'argumentaire aux règles substantielles, en considérant que les droits de l'homme peuvent aussi être considérés comme des biens publics. Ainsi, un Etat fournissant de nombreux droits constitutionnels devrait avoir à intervenir plus souvent dans l'économie, et devrait, de fait, avoir un budget plus élevé. Cette grille de lecture

peut aussi être rapprochée de celle de Buchanan et Tullock qui considèrent la construction de l'Etat par le bas. Dans cette théorie, les citoyens partent d'une situation où l'Etat est cantonné à des fonctions régaliennes, et débattent ensuite des domaines d'intervention supplémentaires à déléguer à l'Etat. Les domaines d'intervention ainsi délégués peuvent être vus comme des règles substantielles inscrites dans la matérialisation du contrat social, *i.e.* la constitution. Une constitution comprenant davantage de droits devrait ainsi être le reflet d'un Etat avec un niveau d'intervention plus élevé, ce qui devrait induire des dépenses publiques plus élevées toutes choses égales par ailleurs.

L'objectif du premier article consiste à tester cette dernière prédiction : un Etat avec davantage de droits constitutionnels devrait avoir des dépenses publiques plus élevées. D'un point de vue légal, le papier distingue deux types de droit selon la typologie standard en droit constitutionnel : les droits *positifs* et les droits *négatifs*.[9] Les droits positifs regroupent les droits sociaux, à l'instar du droit à l'éducation, du droit au logement, ou du droit à la sécurité sociale. Les droits négatifs regroupent quant à eux les droits politiques, tels que la liberté d'expression, le droit à un procès équitable, ou encore la liberté de religion. L'article argue que les deux types de droits sont susceptibles d'induire des dépenses plus élevées pour les Etats qui les respectent. Les droits positifs appellent de fait à davantage d'intervention : pour garantir le droit à l'éducation, l'Etat doit investir dans les écoles publiques, ou, à défaut, subventionner les écoles privées. Les droits négatifs appellent eux aussi à davantage d'intervention de la part de l'Etat. D'une part, l'Etat doit garantir le respect des libertés énoncées dans la constitution, et doit pour ce faire entretenir un système policier et judiciaire permettant de prévenir ou, le cas échéant, de sanctionner les atteintes portées à ces libertés. De plus, afin de réaliser ses politiques publiques, l'Etat doit prendre en considération ces contraintes et doit, parfois, déployer davantage de moyens pour atteindre les mêmes objectifs.[10] Sunstein (2001) partage cette conception et affirme même que les droits négatifs sont intrinsèquement des droits positifs.[11]

De nombreux facteurs peuvent néanmoins perturber cette relation injective entre droits constitutionnels et dépenses publiques. Dans le cas le plus pessimiste, on peut aisément imaginer que les pays inscrivent des droits dans leur constitution à but uniquement déclaratif et que les dirigeants n'ont en réalité pas l'intention de les respecter ou de les mettre

[9] Aharon Barak définit la distinction par ces mots : "Negative rights define the limitations on a constitutional right that the state is precluded from imposing. Positive rights define the actions that the state is obligated to take in order to protect a constitutional right." (Barak (2012), p. 742).

[10] Le papier cite à ce titre un arrêt de la Cour Suprême Américaine dans lequel Justice Scalia affirme que, si la propriété privée est garantie constitutionnellement, l'Etat ne peut placer de dispositif de géolocalisation sur les véhicules de suspects, mais qu'il peut néanmoins déployer plusieurs agents de force de l'ordre pour faire suivre un individu. Ainsi, afin d'atteindre un objectif social déterminé (prendre un criminel en flagrant délit), un Etat devant respecter davantage de contraintes devra déployer plus de moyens.

[11] Sunstein (2001) écrit : "Même les droits individuels conventionnels, comme le droit d'expression et le droit à la propriété privée nécessite un degré d'intervention publique. [...] Les droits dits négatifs sont de fait des droits positifs." Citation originale : *"Even conventional individual rights, like the right to free speech and private property, require governmental action. [...] So-called negative rights are emphatically positive rights."* (p. 2)

en œuvre. En d'autres termes, les droits constitutionnels peuvent être des signaux envoyés par des Etats qui ne disposent pas de la volonté ou des moyens nécessaires à leur réalisation mais qui, pour des raisons stratégiques ou idéologiques, décident de les inscrire dans leur constitution. Une analyse empirique de l'impact des droits constitutionnels se confronte aussi aux problèmes du fédéralisme (la charge de l'application des droits constitutionnels peut être supportée par les Etats fédérés), des développements jurisprudentiels (la jurisprudence peut développer d'autres droits à partir de ceux existants, créant ainsi plus de droits que ceux contenus par la constitution), ou encore des pouvoirs des cours de justice quant à l'applicabilité des règles constitutionnelles.

Résumé du travail. L'analyse quantitative de l'impact des droits constitutionnels sur les dépenses des Etats pose de multiples problèmes que ce premier travail tente de résoudre. Le principal problème réside dans la faible variation des règles de droit au cours du temps. Ce problème, appelé la *règle de fer* (*iron law*, Persson & Tabellini (2004)), constitue un obstacle majeur dans la mesure où l'utilisation de données de panel s'en retrouve handicapée. Certains travaux en économie politique constitutionnelle ont utilisé des modèles à erreur composée (*random effect*) tandis que d'autres ont transformé les données de panel en données longitudinales. L'article critique ces deux méthodes, et propose d'utiliser des modèles à effets fixes (*fixed effects*). Afin de circonscrire le problème des variations limitées, l'article utilise un indicateur composite créé par Elkins et al. (2013) qui regroupe 74 droits constitutionnels. Les autres problèmes incluent : la disponibilité des données, la sélection des pays ayant changé leur nombre de droits constitutionnels sur la période observée (1960-2011), l'hétérogénéité inobservée, et l'endogénéité résultant de l'effet de retour des dépenses de l'Etat sur la détermination du nombre de droits constitutionnels.

Afin de répondre à ces différents problèmes, l'analyse se décompose en plusieurs étapes. Premièrement, nous montrons que se concentrer sur les pays pour lesquels les données macroéconomiques sont disponibles ne crée pas de distorsion en ce qui concerne l'étude du nombre de droits constitutionnels. Ensuite, nous constatons que, sur la période observée, les pays membres de l'OCDE ont majoritairement modifié leur constitution à l'aide d'amendements, tandis que les pays non-membres ont davantage changé de système constitutionnel. En ce qui concerne les règles substantielles, l'article montre, par des méthodes de moindres carrés généralisés, que les pays qui ont diminué leur nombre de droits constitutionnels ont connu une augmentation importante des dépenses publiques avant la modification constitutionnelle. Il montre également que les pays qui ont le plus augmenté le nombre de droits constitutionnels sur la période 1960-2011 sont aussi ceux qui ont connu la plus grande augmentation de dépenses après le changement constitutionnel.

L'article s'intéresse ensuite aux choix constitutionnels. Il montre ainsi que les droits constitutionnels ont connu une logique de rattrapage depuis 1960 : les pays qui ont changé le nombre de droits sont ceux qui avaient le moins de droits en 1960. De plus, les pays les plus susceptibles de changer leur nombre de droits ont été les ceux ayant connu les plus grandes avancées démocratiques. Nos travaux montrent en outre que la taille des dépenses

publiques n'influence pas la probabilité de changer le nombre de droits constitutionnels mais le nombre de droits lui-même : les pays avec de fortes dépenses publiques ont inscrit moins de droits dans leur constitution. Ces analyses reposent sur des méthodes de moindres carrés généralisés ainsi que des modèles avec sélection (Heckman).

En raison de ce possible effet de retour, l'analyse de l'impact du nombre de droits constitutionnels sur les dépenses publiques repose sur une approche à variables instrumentales. Le papier considère trois instruments qui peuvent influencer le nombre de droits et non les dépenses de l'Etat : le nombre moyen de droits constitutionnels à la date de la dernière modification constitutionnelle (amendement ou nouvelle constitution), la situation démocratique des pays ayant la même origine légale, et la nature de l'autorité ayant créé la constitution. Goderis & Versteeg (2012) ont montré en effet un fort phénomène de mimétisme constitutionnel entre les pays partageant la même origine légale. Ginsburg et al. (2009) ont de plus prouvé que les constitutions rédigées par le pouvoir législatif ou le pouvoir exécutif possèdent moins de droits.

En considérant ces instruments conjointement, puis tour à tour, le travail conclut que le nombre de droits constitutionnels n'a pas d'impact significatif sur les dépenses publiques des Etats. Il montre également que ce résultat reste valide conditionnellement au niveau démocratique : le nombre de droits constitutionnels n'a pas d'impact sur les dépenses publiques dans les démocraties ni dans les autocraties. De plus, ce résultat demeure inchangé quand on restreint l'analyse aux droits positifs uniquement, à savoir ceux le plus susceptibles d'augmenter les dépenses des Etats.

La Production de Normes Juridiques

De la question institutionnelle au jeu politique. La deuxième étape du processus aboutissant à la mise en œuvre de la règle de droit est celle de la production de normes juridiques dans le cadre institutionnel défini par la constitution ; cadre introduit par la discussion précédente. Les règles procédurales définissent les institutions qui produisent les normes juridiques, et les règles substantielles en définissent partiellement la nature. Le jeu institutionnel se confond alors avec le jeu politique, où les partis s'affrontent pour maîtriser les mécanismes de création de normes. L'affrontement politique y est défini par des règles constitutionnelles ou par des lois organiques, à l'instar des lois électorales.

Cette étape de création de normes a été principalement étudiée par les théoriciens du choix public et de l'économie politique. Suivant les travaux d'Arrow, de nombreuses investigations ont eu pour ambition de comprendre les mécanismes d'agrégation des préférences, et l'impact des règles électorales sur les décisions de politiques publiques (par exemple Benoît & Kornhauser (2010)). Les travaux d'économie politique se sont quant à eux intéressés au marché politique, étudiant ainsi la demande et l'offre du marché politique. Ces travaux ont étudié la création de normes sous forme de signal dans un jeu de recherche de rentes (Downs (1957), Acemoglu et al. (2013)).

La question de la genèse des normes légales est centrale pour la légitimité et la stabi-

lité du système politique. La légitimité de la norme juridique se définit en grande partie à cette étape. Les citoyens considèreront une loi comme légitime, si celle-ci a été votée par un gouvernement représentatif, à l'écoute de ses citoyens, et après un processus démocratique transparent. Certains travaux ont ainsi exploré l'*expressivité* des lois (*e.g.* Carbonara et al. (2010)).[12] Les citoyens sont plus à même de respecter des lois expressives, et la stabilité du système politique en sera renforcée. De plus, les citoyens peuvent également changer d'opinion et soutenir les lois votées par le gouvernement à condition que ces dispositions aient été votées par un gouvernement démocratiquement reconnu. L'abolition de la peine de mort en France en est le meilleur exemple.

La redistribution au centre du jeu politique. Le second article de cette thèse se concentre sur la question de la redistribution qui a mobilisé le plus de ressources en économie politique. Les modèles standards d'économie politique classent généralement les candidats sur un axe unidimensionnel, qui correspond généralement à un axe de demande de redistribution (Acemoglu et al. (2013)). La question de la redistribution a aussi été centrale en théorie politique, et les questions institutionnelles ont souvent été étudiées à l'aune de leurs impacts sur les politiques redistributives. Les discussions de Madison dans les *Federalist Papers* reflètent clairement ces questionnements. Madison proposait un concept de République dans laquelle les intérêts individuels seraient divisés de telle sorte à limiter l'émergence d'une majorité tyrannique. Une telle division de la société aurait ainsi pour conséquence de limiter la redistribution effective.[13]

Ce deuxième travail part du constat que le marché politique s'est, jusqu'à aujourd'hui, révélée incapable de résoudre la résurgence des inégalités dans les démocraties occidentales (Atkinson (2003), Piketty & Saez (2003, 2006), Dustmann et al. (2009)). Les précédents travaux en économie politique ont cherché à comprendre la persistance d'inégalités. La question sous-jacente à cette littérature consiste à comprendre pourquoi les institutions peinent à atténuer les inégalités croissantes dans nos sociétés. En d'autres termes, pourquoi le processus de création de normes décrit précédemment n'arrive-t-il pas à enrayer l'opposition croissante entre les riches et les pauvres tel que décrit par Madison ?

Travaux sur la redistribution. Les précédents travaux en économie politique se sont intéressés à trois sujets : la demande du marché politique, l'offre du marché politique et les mécanismes institutionnels. La demande du marché politique correspond aux préférences des citoyens en matière de politiques publiques. Le choix constitutionnel discuté précédemment correspond aux préférences des citoyens en termes de règles institutionnelles. Si le consensus constitutionnel cherche à être le plus large possible, le consensus politique ne

[12]Les règles de droit sont dites expressives lorsqu'elles correspondent aux normes sociales en vigueur parmi la population.

[13]Madison souligne l'importance de la question redistributive dans ce débat par ces mots : "Those who hold and those who are without property have ever formed distinct interests in society.". (Madison (1787))

partage pas forcément cet objectif.[14]

Les études menées sur la demande politique de redistribution ont exploré quatre pistes de recherche. Une première série de travaux s'est intéressée au rôle des intérêts égoïstes dans la demande de redistribution. La théorie microéconomique standard postule en effet que les fonctions d'utilité des agents sont exclusivement centrées sur leur propre bien-être. En d'autres termes, les travaux théoriques ont pendant très longtemps considéré que l'*homo œconomicus* était dépourvu d'altruisme. Bien que cette hypothèse soit une simplification de la réalité, les économistes souhaitaient surtout souligner que les individus pondèrent de manière plus importante leur propre bien-être que celui de leurs proches. C'est ainsi que plusieurs travaux macroéconomiques empiriques ont eu pour ambition de comprendre si la demande de redistribution était motivée par les intérêts propres aux citoyens. Les résultats de cette littérature ont été partagés : certaines études affirment que les individus répondent à des motivations égoïstes (Corneo & Grüner (2002)) tandis que d'autres affirment que cet effet est limité (Milanovic (2000), Fong (2001), Boarini & Le Clainche (2009)). Une seconde série de travaux s'est fixée pour objectif d'éclaircir le rôle des valeurs sociales dans la demande de redistribution. Ces études s'intéressent aux valeurs éthiques des individus, ainsi qu'aux considérations d'efficacité (maximisation du surplus) des citoyens. Les éléments empiriques apportés par Fong (2001) suggèrent que l'altruisme et/ou la réciprocité[15] incitent à davantage de redistribution. La réciprocité est aussi mise en avant par Boarini & Le Clainche (2009). Alesina & Angeletos (2005) montrent que la perception des déterminants de la pauvreté, qu'ils soient endogènes ou exogènes, influencent la demande de redistribution. Bernasconi (2006) trouve des éléments similaires pour la perception du rôle de la famille dans l'ascenseur social. Une troisième vague de travaux a exploré le rôle de la compétition sociale, ou, en d'autres termes, l'importance de la richesse relative : un individu serait contre la redistribution car, quand bien même celle-ci lui bénéficierait, elle amoindrirait son écart de revenu avec les personnes plus pauvres que lui. Corneo & Grüner (2002) trouvent des éléments empiriques soutenant cette théorie. Keely & Tan (2008) apportent aussi des éléments qui montrent que les citoyens prennent en considération la situation de leurs concitoyens quand ils décident de la redistribution. Enfin, les études sur la demande du marché politique ont travaillé sur l'hypothèse *Prospect Of Upward Mobility* d'Hirschman. L'hypothèse *POUM* suppose que les citoyens anticipent leurs revenus futurs quand ils émettent leurs souhaits de redistribution : un individu optimiste demandera ainsi des taux de redistribution plus faibles. En utilisant des données états-uniennes de sondage, Keely & Tan (2008) trouve un soutien empirique mitigé pour cette hypothèse.

Un autre corps de recherches a dévolu son attention à l'offre du marché politique. Plusieurs travaux se sont démarqués du modèle downsien standard pour reconnaître que

[14]Lijphart (1999) distingue deux types de démocratie. Il oppose les démocraties consensuelles aux démocraties majoritaires. Les premières sont caractérisées par des institutions et une culture politique qui fonctionnent par négociation et par compromis, tandis que les dernières règlent les différends politiques par décisions majoritaires.

[15]Fong (2001) est incapable de déterminer quel facteur est déterminant.

les agents politiques sont motivés par des préférences intrinsèques pour certaines politiques publiques (par exemple Acemoglu et al. (2013)). D'autres articles ont reconnu l'influence des lobbys. Harms & Zink (2003) arguent que les élus se tiennent à l'écart des politiques trop redistributives, dans la mesure où les groupes les mieux dotés sont capables d'investir davantage dans les activités de lobbying, conduisant de fait à une influence plus grande pour ces groupes.

D'autres études se sont intéressées à la structure du marché politique en vue de comprendre la raison de l'accroissement des inégalités. Harms & Zink (2003) partent du constat que l'abstention crée une distorsion entre la distribution réelle des préférences et la redistribution exprimée lors des élections. Les auteurs en déduisent ainsi l'existence d'un électeur politiquement décisif, qui est potentiellement différent de l'électeur médian. En supposant une corrélation négative entre l'abstention et le niveau de richesse, il en résulte un électeur décisif ayant une demande de redistribution inférieure à l'électeur médian, conduisant ainsi à l'élection de représentants demandant moins de redistribution. Dans une autre étude, Roemer (1998) s'intéresse à la multidimensionalité du marché politique. Les électeurs sont en effet souvent appelés à choisir des représentants, et non à voter sur des politiques publiques précises.[16] La compétition politique s'effectue alors sur un axe unidimensionnel, souvent dénommé gauche-droite, les candidats s'opposant néanmoins sur un espace à plusieurs dimensions. Analysant le cas états-unien, Lee & Roemer (2006) cherchent à estimer l'impact du racisme sur la redistribution. Pour ce faire, les auteurs mesurent ce qu'ils appellent le *policy bundle effect*, à savoir l'impact en termes de redistribution des votes républicains motivés non pas par des préférences redistributives mais par des préférences racistes qui ne trouvent pas leur pendant dans les revendications démocrates. Ils estiment ainsi que, si les considérations racistes n'avaient pas existé, le taux de taxation du revenu aurait été de 11 à 18 points de pourcentage plus élevé.

Motivation de l'article. Cette littérature considérable a eu pour objet de comprendre comment le résultat du processus de création de normes redistributives, e.g. le taux d'imposition du revenu, peut s'écarter des préférences de la population. Le second article de cette thèse contribue à ce champ disciplinaire, et propose un rapprochement avec la psychologie. Notre travail propose en effet de considérer l'impact du bias d'auto-complaisance (*self-serving bias*) sur la demande et l'offre de redistribution. Le biais d'auto-complaisance a été mis en lumière par des travaux en psychologie (Miller & Ross (1975)) et affirme que les agents ont tendance à attribuer leurs succès à des facteurs endogènes (e.g. effort, motivation) et leurs échecs à des facteurs extérieurs (e.g. manque de chance). Notre article établit un lien avec les travaux de Konow (2000) qui ont montré que les agents choisissent de redistribuer en fonction de leur perception des déterminants de la pauvreté : plus un individu perçoit les facteurs endogènes comme déterminant le résultat à une situation, moins un individu souhaitera corriger les inégalités *a posteriori*. Konow (2000) nomme ce phénomène le principe de responsabilité (*accountability principle*). L'ambition de ce second

[16] Les cas de référendums récurrents à l'instar de ceux organisés en Suisse font figure d'exception.

travail consiste ainsi à explorer dans quelle mesure le biais d'auto-complaisance, associé au principe de responsabilité, peut influencer la demande et l'offre de redistribution.

Résumé du travail. Afin de répondre à cette question de recherche, notre analyse propose un cadre expérimental qui permet d'isoler l'impact du biais d'auto-complaisance. L'expérience est divisée en deux jeux. La première partie cherche à induire un bias d'auto-complaisance parmi les candidats et d'observer leur offre de redistribution. La second partie vise à obtenir leur demande de redistribution.

Afin d'induire un biais d'auto-complaisance parmi les participants, l'expérience procède comme suit. Au début de l'expérience, les individus sont informés qu'ils vont être appelés à remplir une tâche qui peut être facile ou difficile avec des probabilités équivalentes. Les sujets sont alors aléatoirement assignés à une tâche qu'ils doivent effectuer de manière chronométrée. Les participants observent uniquement leur tâche, et ne sont pas capables d'inférer la nature de la tâche reçue (facile ou difficile). A la fin de leur tâche, les individus sont informés de fait qu'ils aient mieux ou moins bien réussi que le participant médian. Cette première étape consiste à induire le biais d'auto-complaisance parmi les candidats. En théorie, aucun participant n'est capable de déterminer l'influence de son effort sur leur succès ou son échec dans la mesure où les candidats ignorent la difficulté de la tâche à laquelle ils n'ont pas été assignés. L'induction du biais d'auto-complaisance est vérifié par des questions posées aux participants sur les attributions causales[17] de leur résultat.

Une fois le biais-d'autocomplaisance induit aux candidats, l'expérience se poursuit en sélectionnant aléatoirement deux participants et en affichant la différence de profit entre les deux candidats aux participants restants. Ces derniers ont alors la possibilité de redistribuer le surplus de l'individu le plus riche vers l'individu le plus pauvre (en partie ou en intégralité). Les participants sont *a priori* informés qu'une de leurs décisions sera aléatoirement sélectionnée pour être mise en œuvre. Leur décision est analysée comme reflétant leur *offre* de redistribution, dans la mesure où leurs décisions individuelles n'affectent pas leur propre profit. Une fois les décisions prises, une solution est choisie pour être mise en place, et le premier jeu se finit.

Le second jeu commence alors et les participants apprennent qu'ils vont devoir effectuer une nouvelle série de tâches dont ils ignorent la nature. Les participants savent que cette tâche s'effectuera de manière individuelle, mais que leurs profits individuels seront affectés par des chocs aléatoires (positifs ou négatifs), et que leurs profits feront l'objet d'une redistribution dans des groupes de quatre participants (groupes fixes jusqu'à la fin du jeu). Les sujets sont ainsi invités à choisir entre trois règles de redistribution. La règle égalitarienne assure une redistribution totale des profits individuels du groupe en parts égales. La règle libertarienne laisse les individus avec leurs profits individuels (après le choc). La règle sociale-libérale redistribue le profit total du groupe après le choc en fonction des profits

[17]Le terme *attribution causale* sert à désigner le processus par lequel les individus expliquent les situations auxquelles ils font face en inférant les causes de cette situation (influence des facteurs endogènes ou exogènes).

individuels après le choc (redistribution proportionnelle). Les participants doivent assigner des poids entre 0 et 10 à chacune de ces règles et sont informés que le système de redistribution pour le groupe sera défini en fonction des poids choisis par un des individus du groupe (tiré au hasard). En laissant les candidats dans l'incertitude quant à la nature de la tâche qu'ils devront effectuer, les sujets sont placés sous un voile d'ignorance. Leur décision reflète leur *demande* de redistribution, dans la mesure où leurs voix pourront affecter leur revenu futur.

Fort de ce *design* expérimental, l'article conclut sur plusieurs résultats. D'une part, le papier montre que l'offre de redistribution est affectée par le biais d'auto-complaisance : les individus ayant mieux réussi au premier jeu, sont plus susceptibles de penser que les facteurs internes ont déterminé leur résultat, et redistribuent moins de l'individu le plus riche vers l'individu le plus pauvre. D'autre part, le papier montre que le biais d'auto-complaisance affecte aussi la demande de redistribution : les individus ayant mieux réussi à la première partie demande davantage du système égalitarien (redistribution totale) et moins du système social-libéral (redistribution proportionnelle).

Le Contrôle de Conformité à la Constitution

La troisième étape du processus aboutissant à l'application de la norme de droit est le contrôle de la norme créée à la seconde étape avec le contrat social. La discussion de la première partie du processus a rappelé que les constitutions contiennent des règles procédurales et substantielles. Le contrôle de conformité des normes juridiques à la constitution peut s'établir sur ces deux dimensions. Le contrôle de conformité en France s'est d'abord établi autour des règles procédurales. Créé sous la IVème République, le Comité Constitutionnel avait pour objectif de vérifier le respect des règles procédurales dans la création des normes juridiques. Leur violation induisait la nullité de l'acte législatif incriminé. La Vème République a introduit pour la première fois dans l'histoire juridique française la possibilité d'un contrôle de conformité du contenu des lois avec les règles substantielles de la constitution, et, plus largement, avec les principes de droit français. Ce renforcement du contrôle de conformité s'est poursuivi en 1974 avec l'introduction de la possibilité de saisines par les parlementaires, confortant ainsi un phénomène observé dans de nombreux pays (Ginsburg & Versteeg (2014)).

D'un point de vue de philosophie politique, le contrôle de constitutionnalité assure un respect des règles fondamentales du contrat social tel que matérialisé par la constitution. Les bénéfices à un tel contrôle sont ceux énoncés plus haut : le respect de la constitution permet de préserver la cohésion du système politique quand bien même le gouvernement ferait face à une très forte opposition en dehors du Parlement.

Les approches empiriques et théoriques en économie du droit et en économie politique ont exploré une tension inhérente au contrôle de constitutionnalité, i.e. la volonté de mettre en place des politiques précises, parfois inconstitutionnelles, et l'avantage à respecter l'indépendance de la Cour Constitutionnelle. La théorie politique présentée ci-dessus présente

les avantages du gouvernement à un contrôle de constitutionnalité : il assure sa légimité. Plus encore, les travaux sur l'indépendance judiciaire y voient la source de sa crédibilité (Feld & Voigt (2003), Hayo & Voigt (2007)). Les coûts associés au contrôle de constitutionnalité se reflètent dans la réduction de la marge d'action du gouvernement : ce dernier doit limiter la portée des politiques mises en place à l'ensemble des politiques constitutionnellement acceptables. Dès lors, le gouvernement peut chercher à entretenir une image d'indépendance de la part de la Cour Constitutionnelle, tout en essayant de l'influencer de manière détournée.

Travaux américains. Une abondante littérature s'est développée en sciences politiques ainsi qu'en économie du droit pour rendre compte de ce délicat équilibre. Les mécanismes de nomination, la durée des mandats ou encore les protections associées à la fonction de juge constitutionnel jouent un rôle essentiel dans la mise en place d'un contrôle de constitutionnalité impartial. Les premiers travaux empiriques ont vu le jour aux Etats-Unis, avec l'analyse des décisions de la Cour Suprême. Des travaux similaires ont par la suite été réalisés en Europe et en France.

Les premiers travaux empiriques traitant des décisions de Cours Suprêmes ont été effectués aux États-Unis, menés de manière conjointe par les départements de sciences politiques et de statistiques. Ces travaux y ont bénéficié d'une audience considérable, principalement due à l'intérêt réel des départements de droit pour les approches quantitatives. Les principaux acteurs de cette littérature ont contribué à populariser ce type d'approche (L. Epstein, W. Landes, R. Posner, A. Martin, J. Segal, K. Quinn).

Les recherches américaines ont principalement traité deux questions. D'une part, une première série d'études s'est intéressée aux préférences idéologiques des juges de la Cour Suprême. En opposition aux théories formalistes, qui supposent que les juges prennent des décisions fondées uniquement en droit, ces travaux partent des prédictions développées par les théories *attitudinales*, qui considèrent au contraire que les juges sont également motivés par un ensemble de préférences idéologiques qui déterminent leurs décisions. Cette littérature a eu pour ambition de classer les juges selon leurs préférences idéologiques. Pour cela, les travaux statistiques ont construit un axe libéral-conservateur, sur lequel discriminer les juges afin d'obtenir une représentation spatiale des préférences idéologiques. L'intérêt de cette approche est de faire émerger un juge médian, considéré comme le juge pivot dans la mesure où il détermine la majorité de la Cour. Les débats techniques ont alors essentiellement porté sur la construction de l'axe libéral-conservateur et sur la détermination du positionnement de chaque juge. Différentes méthodes statistiques appliquées sur les décisions rendues par la Cour Suprême ont été utilisées pour obtenir ce type d'axe et le classement des juges qu'il implique. Ces approches n'aboutissent pas toujours au même type de classement, faisant ainsi changer l'identité du juge pivot.

Les universitaires américains se sont en outre intéressés à la propension individuelle des juges de la Cour Suprême à censurer des dispositions légales. Deux articles influents se sont focalisés sur la question de l'activisme judiciaire de la part de la Cour Suprême.

L'objectif de ces études a été de comprendre dans quelle mesure les juges font preuve de retenue judiciaire (*judicial self-restraint*), i.e. une déférence vis-à-vis des décisions du pouvoir démocratiquement élu. D'une part, L. Epstein et A. Martin ont étudié l'ensemble des décisions de la Cour Suprême de 1969 à 2009 (Epstein & Martin (2012)). Leur étude a montré que les juges conservateurs (respectivement libéraux) faisaient preuve d'une plus grande retenue pour les lois conservatrices (resp. libérales). La probabilité de censure d'une loi conservatrice sur cette période était de 27% pour les juges conservateurs, contre 76% pour les juges démocrates. A contrario, la probabilité de censurer une loi libérale était de 17% pour les libéraux et de 46% pour les juges conservateurs. L'analyse statistique de ces décisions montre également que l'effet idéologique reste le plus à même d'expliquer une décision de la Cour (84%), et ce même en prenant en compte d'autres facteurs tels que l'environnement politique, les caractéristiques des lois concernées, ou le processus de sélection des lois abordées. D'autre part, L. Epstein et W. Landes se sont interrogés sur l'existence même d'une telle retenue judiciaire (Epstein & Landes (2012)). Pour ce faire, ils ont étudié un total de 647 décisions de la Cour Suprême dans lesquelles des lois fédérales étaient contestées (1937-2009). Cette seconde étude confirme les résultats précédents, et conclut que le marquage idéologique est si fort qu'il en vient à remettre en cause l'existence même d'une retenue judiciaire. L'étude affirme néanmoins que la politisation de la Cour a évolué au cours du temps : sur les 12 juges nommés avant 1952 et considérés dans l'étude, seuls 5 pouvaient être qualifiés d'idéologues, contre 14 des 18 juges nommés après 1953.

Travaux européens. Plusieurs études européennes ont eu pour objectif de transposer les débats américains aux Cours Constitutionnelles continentales. Ces travaux ont été principalement menés sous l'égide de Nuno Garoupa qui, avec ses co-auteurs, a analysé les décisions des Cours espagnole, portugaise et italienne.

Dans un premier article, Amaral-Garcia et al. (2009) ont analysé les décisions de la Cour Constitutionnelle portugaise. Leurs travaux ont cherché à comprendre la pertinence des débats américains dans les systèmes de droit civil. Les auteurs notent en effet que « dans le modèle continental, les Cours Constitutionnelles sont faites pour être plus conservatrices en interprétant les constitutions et les risques d'un gouvernement des juges y sont très limités ».[18] Contrairement au système américain où les nominations s'effectuent à l'initiative du Président –sous le contrôle du Sénat–, la nomination des juges portugais s'effectue par le Parlement à la majorité des deux tiers pour dix d'entre eux, tandis que trois autres juges sont élus par leurs pairs. De manière similaire, Amaral-Garcia et al. (2009) s'interrogent sur la division droite-gauche (5 des juges élus par le Parlement étant marqués à gauche et 5 marqués à droite). Leur étude porte sur les cas traités entre 1983 et 2007, et conclut que les juges portugais sont eux aussi sujets à un vote idéologique et/ou politique, tout en contrôlant pour les caractéristiques des lois étudiées par la Cour. En d'autres termes, les

[18]Citation originale : "In the Continental model, constitutional courts are intended to be more conservative in interpreting constitutions and are restrained from embracing active law making." (Amaral-Garcia et al. (2009))

juges nommés par la gauche (respectivement la droite) sont moins susceptibles de voter contre des lois de gauche (resp. de droite) comparativement aux lois votées par la droite (resp. par la gauche).

Une seconde étude, menée en Espagne, a confirmé ces résultats. La Constitution espagnole établit une Cour Constitutionnelle composée de douze juges dont huit sont élus à la majorité des trois cinquièmes par le Parlement (quatre par chambre), deux sont nommés par le gouvernement, et deux sont mandatés par le Conseil Judiciaire. Considérant que, en raison de leur processus de nomination, les juges siégeant à la Cour Constitutionnelle ont de fortes chances d'être influencés par des motivations politiques et/ou idéologiques, Garoupa et al. (2013) étudient les votes individuels des juges en fonction du parti politique qui les a nommés. Cette étude conclut, elle aussi, que les juges votent en grande partie selon les intérêts des partis qui les ont nommés. Les auteurs estiment en effet que la probabilité qu'un juge vote en faveur d'une requête à la Cour augmente de 39 points de pourcentage lorsque cette dernière est portée par le parti qui l'a nommé.[19]

La Cour Constitutionnelle de la République italienne a aussi fait l'objet d'une récente étude par Garoupa & Grembi (2013). Celle-ci s'est principalement intéressée aux effets du changement institutionnel du système politique italien de 1992, qui, selon les auteurs, a transformé ce dernier, passant d'une démocratie consensuelle (avant 1992) à une démocratie majoritaire (à partir de 1994). Les auteurs se fondent sur les théories développées par Lijphart (1999), qui oppose deux modèles de démocratie. Il définit d'une part le modèle consensuel dans lequel la polarisation politique est faible, et qui fonctionne principalement sur la négociation et le compromis. Il décrit le modèle de démocratie majoritaire comme un système où les problèmes politiques sont tranchés par la majorité et caractérisé par un fort affrontement entre la majorité et l'opposition. L'étude de Garoupa & Grembi (2013) est la plus proche du cas français, dans la mesure où les votes individuels des juges ne sont pas publiés. La Cour Constitutionnelle de la République est composée de 15 juges, nommés en proportions égales par le Président de la République, le Parlement, et des représentants du pouvoir judiciaire. Le travail prend pour hypothèse que les juges sont soumis à une pression moins forte dans un système parlementaire, que les auteurs assimilent à un système consensuel, dans la mesure où la majorité parlementaire y est plus fragile. En revanche, les auteurs étudient la possibilité qu'un système majoritaire accentue la concentration du pouvoir (démocratie majoritaire), augmentant les risques de pression auprès des juges, et amenant, de fait, à des jugements politisés. L'étude conclut que la proportion de juges nommés par le parti du requérant augmente la probabilité de censure, pour les lois contestées après 1994. *A contrario*, l'analyse statistique ne permet pas d'affirmer qu'un effet similaire ait eu lieu pour les lois portées avant 1992.

Travaux français. Deux récents travaux menés par Raphaël Franck ont initié l'étude quantitative des décisions du Conseil Constitutionnel français.

[19] L'étude souligne néanmoins que ce résultat est vérifié pour les questions soulevées par les partis au niveau national et non au niveau local.

Dans une première étude, Franck (2009) propose une analyse statistique des *décisions constitutionnelles* rendues de 1956 à 2006. Partant du constat que le Conseil Constitutionnel est caractérisé par un manque d'indépendance, du moins formel , notamment par son processus de nomination, R. Franck tente de mettre en exergue les déterminants des décisions du Conseil Constitutionnel. R. Franck s'intéresse essentiellement à deux dimensions : les périodes de cohabitation et la composition du Conseil. L'auteur avance d'une part que, lors des périodes de cohabitation, le gouvernement est moins susceptible d'influencer les décisions du Conseil. Selon l'auteur, le gouvernement et le Président de la République étant de sensibilités politiques différentes, la pression exercée sur le Conseil serait plus faible. D'autre part, Franck analyse l'impact de la composition du Conseil sur ses décisions. L'auteur suggère que la proportion de juges nommés par la droite (respectivement par la gauche) diminue le risque de censure pour les lois portées par les partis de droite (respectivement de gauche).[20] Franck postule aussi que le cursus des conseillers peut avoir un impact sur la décision finale : les juges ayant exercé des fonctions politiques seraient plus susceptibles de voter de manière politique que les juges ayant une formation juridique. Cette première étude conclut que les périodes de cohabitation sont associées à de plus forts taux de censure, surtout pour la législature 1997-2002, ce que Franck interprète comme un signe de la faiblesse de la gauche plurielle face au Conseil Constitutionnel, assurant au Conseil une plus grande marge de manœuvre pour rendre ses décisions. Elle apporte aussi quelques éléments suggérant que la proportion de juges nommés par la droite augmente le risque de censure pour les majorités de gauche.

Une seconde étude (Franck (2010)) s'intéresse aux décisions du Conseil Constitutionnel en matière électorale. Plus précisément, ces travaux analysent 2 229 décisions portant sur la validité d'élections parlementaires. De manière similaire, l'article s'intéresse à la composition du Conseil ainsi qu'aux périodes de cohabitation pour expliquer les décisions en matière électorale. En outre, l'article analyse l'impact du différentiel du nombre de voix lors de l'élection contestée sur les décisions du Conseil, ce dernier étant supposé trancher les cas de fraude selon le principe de *minimis non curat praetor*.[21] Ces travaux, bien que peu détaillés d'un point de vue méthodologique, débouchent sur plusieurs résultats intéressants. Premièrement, l'analyse statistique suggère que le différentiel de voix entre les candidats n'influence pas la décision du Conseil. Deuxièmement, Franck suggère que les juges ne favorisent pas les partis qui les ont nommés. Troisièmement, cette étude affirme que les juges décident de manière indépendante du gouvernement, à savoir qu'ils ne favorisent pas les candidats de la majorité siégeant à l'Assemblée Nationale et que les périodes

[20] À cet égard, Franck reprend le cadre théorique développé dans les travaux précédents. Les travaux européens ont repris à leur profit la distinction droite-gauche développé dans le cadre du bipartisme Américain. Le cas français est plus proche du cas Américain dans la mesure où le système politique français est caractérisé par un bipartisanisme poussé – du moins au niveau institutionnel. Ces travaux, ainsi que les nôtres, passent ainsi sous silence des configurations plus subtiles, telles que les tensions au sein d'un même bloc politique, à l'instar de l'opposition Debré-Sarkory.

[21] Le principe de *minimis non curat praetor* stipule que les fraudes électorales ne doivent être considérées que dans la mesure où elles ont pu affecter le résultat de l'élection

de cohabitation n'influencent pas leurs décisions. Quatrièmement, Franck conclut que les candidats d'extrême droite ont, toutes choses égales par ailleurs, une plus grande chance de voir leur élection invalidée par le Conseil. Étant donné que l'analyse statistique de Franck prend en considération les caractéristiques de chaque élection, ce résultat semble indiquer un traitement différencié pour les candidats d'extrême droite. Ce résultat est à considérer avec prudence, dans la mesure où les élections de candidats d'extrême droite peuvent présenter des caractéristiques inobservées qui n'ont pas été prises en compte dans l'analyse statistique (effet de sélection).

Cadre théorique. L'objectif du troisième article de la thèse est d'analyser la pertinence de trois théories de prise de décision décrites ci-dessus dans le cadre du Conseil Constitutionnel. A cet égard, ces travaux s'inscrivent dans une approche de réalisme juridique selon laquelle "la validité des décisions judiciaires ne dépend pas de leur conformité à la logique ou l'idée que l'interprétation est une fonction non de la connaissance, mais de la volonté" (Troper (2007)). La première théorie analysée dans l'article reprend le modèle américain ainsi que les travaux de Franck sur le Conseil Constitutionnel et s'intéresse au modèle attitudinal. Le Conseil Constitutionnel est en effet potentiellement sujet à de grandes influences politiques étant donné le processus de nomination politique de ses membres et la présence d'anciens Présidents de la République. Notre approche est fondamentalement réaliste dans la mesure où "elle prétend faire apparaître la réalité du pouvoir du juge derrière les justifications, qui ne sont que de façade, des décisions judiciaires" (Troper (1993)). La seconde théorie étudiée par l'article est celle de la retenue judiciaire. Nos travaux testent empiriquement certaines prédictions formulées par Dominique Schnapper à ce sujet, à commencer par l'existence d'un "quota de censures", au delà duquel le Conseil n'aurait plus la possibilité (politique) de censurer les dispositions du gouvernement. Enfin, notre étude s'intéresse à la théorie de l'indépendance opportuniste qui considère les décisions du Conseil Constitutionnel dans un cadre politique plus large, à savoir dans les périodes de cohabitation et les périodes d'opposition entre les deux chambres du Parlement.

Résumé du travail. Afin de tester empiriquement ces trois théories, ce troisième article analyse les décisions constitutionnelles rendues entre 1974 et 2013. L'objectif est de comprendre les facteurs qui influencent la décision de validité ou d'invalidité prise par le Conseil Constitutionnel. L'article repose sur une base de données constituées par nos soins qui comprend l'intégralité des demandes de contrôle de constitutionnalité adressés au Conseil Constitutionnel sur les textes juridiques avant leur promulgation. La procédure de code s'est effectuée de manière informatisée, les variables codées n'étant pas sujettes à interprétation.

L'investigation empirique se concentre ainsi sur la variable d'invalidation, définie égale à 1 si une loi est censurée (en partie ou en intégralité) par le Conseil et à 0 si la loi est confirmée (en partie ou en intégralité). La procédure de code de cette variable est reprise à partir de la catégorisation du Conseil, qui, sur son site internet, répertorie les décisions

comme « conformité » ou « non-conformité ». Cette variable est alors mise en perspective avec des variables représentant chacune des trois théories. Deux variables cherchent à capturer la validité de la théorie attitudinale : la proportion de juges nommés par la droite et la couleur politique de l'autorité ayant nommé le Président du Conseil. Concernant la théorie de retenue judiciaire, la partie empirique se focalise sur trois dimensions : le nombre de censures prononcées le même jour, le fait que la décision précédente soit une censure, et le nombre de censures prononcées depuis le début de la session parlementaire. Enfin, suivant les travaux de Franck (2009), la théorie d'indépendance opportuniste est matérialisée par une variable capturant les périodes de cohabitation et une variable d'opposition gouvernementale au Sénat.

Ce travail conclut sur la pertinence des théories attitudinales et de retenue judiciaire. D'une part, les résultats montrent une corrélation nette entre la proportion de juges nommés par un parti et la probabilité de censure des lois passées par ce même parti : les lois de droite (resp. de gauche) ont moins de chance d'être censurées lorsque davantage de juges nommés par la droite (resp. par la gauche) siègent lors de la décision. D'autre part, les éclairages empiriques montrent que le Conseil Constitutionnel est moins susceptible de censurer lorsque sa décision précédente est une censure et, surtout, lorsqu'il a déjà censuré plusieurs fois pendant la session parlementaire en cours.[22]

Ces deux résultats doivent être interprétés avec prudence, dans la mesure où ils résultent d'un équilibre politique. L'analyse économétrique d'équilibres stratégiques s'avère toujours délicate dans la mesure où les parties anticipent les comportements des autres acteurs. Ainsi, il est difficile de déterminer si les décisions des juges sont affectées par leur volonté, ou bien si la nature intrinsèque des questions qui leur sont posées change. Le taux de censure plus élevé en période de cohabitation peut ainsi être dû à davantage de liberté de la part des juges mais peut aussi être attribué à un travail parlementaire plus important et moins souvent encadré par les conseillers d'Etat.

Ces conclusions soulèvent cependant de nombreux doutes sur l'indépendance du Conseil Constitutionnel. Il se trouve assurément au cœur d'un jeu politique, et fait l'objet d'une forte attention par le pouvoir politique. Le fait que les juges soient convaincus de leur faible marge de manœuvre en termes d'interprétation de la norme constitutionnelle n'empêche pas des interprétations opportunistes.

Définition et Tracé du Système Juridique

Les trois premiers articles présentés ci-dessus s'intéressent au processus aboutissant à la création de la norme et à la validation de sa conformité avec les règles de droit supérieures. Une fois la norme ainsi créée et validée, le pouvoir politique doit se doter d'un système

[22]Nous contrôlons éventuellement pour un effet temporal au sein de chaque session parlementaire afin de vérifier que l'effet observé n'est pas du à un changement monotone de la qualité des lois au fil de la session parlementaire. Un tel effet linéaire n'est pas significatif.

juridique capable d'assurer la mise en œuvre et le respect de cette règle de droit. La définition du système juridique et les moyens mis en place pour assurer son fonctionnement relèvent du débat politique, et font ainsi l'objet du jeu politique tel que défini par la constitution et la culture politique du pays.

Les premières pages de cette introduction ont rappelé l'importance de la mise en œuvre des normes de droit. La contestation des systèmes politiques peut en effet débuter par l'insatisfaction des citoyens, non pas des institutions en tant que telles, mais de la mise en œuvre des normes qu'elles produisent. De nombreux dictateurs ratifient des traités sur les droits de l'homme tout en refusant de mettre en œuvre les normes qu'ils contiennent : bien que ces dictateurs se servent de ces traités comme vernis démocratique sur la scène internationale, ils n'en tirent aucune légitimité interne. De manière similaire, comme l'a souligné le premier article, les constitutions peuvent comporter de nombreux droits à titre purement déclaratif. La capacité de mise en œuvre des normes juridiques est ainsi l'étape la plus importante pour la légitimité du système : c'est le résultat du processus politique directement appréciable par les citoyens, résultat sur lequel ils jugeront les responsables politiques et, plus globalement, la qualité du système en place.

Origine et raison d'être des Conseils de Prud'hommes. Le système juridique français moderne tire ses origines de la Révolution Française et de la volonté centralisatrice des constituants des premières Républiques. Cet objectif de centralisation cherchait principalement à réduire le pouvoir des juges qui étaient considérés, à tort ou à raison, comme complices de l'Ancien Régime.[23] Cette vision centralisatrice s'est accélérée sous les premières Républiques et les Empires qui leur ont succédé[24], mais l'aversion française envers un système juridique jouissant d'une grande indépendance était déjà présente dans les travaux de Montesquieu, pour qui les juges devaient se cantonner à demeurer, selon l'expression célèbre, "la bouche de la loi".

La centralisation du pouvoir juridique présente de nombreux avantages pour le pouvoir politique, à commencer par celui de lui donner le contrôle sur l'application des normes qu'il établit. Cette centralisation intervient cependant à un coût, à savoir celui de réduire la légitimité du système juridique auprès de la population. C'est dans cette perspective que les Conseils de Prud'hommes ont été créés en 1806 par Napoléon I^{er} sur le modèle des tribunaux de commerce. L'objectif de cette mesure consistait à déléguer la mise en œuvre des règles régissant le droit du travail à des professionnels des métiers concernés afin d'accroître la légitimité des décisions de justice et d'améliorer ainsi leur acceptation par les justiciables.[25] La création des Conseils de Prud'hommes s'est ainsi effectuée à contre-

[23] A propos des considérations sur le système judiciaire pendant la Révolution : "[...] les membres de l'Assemblée constituante définissent ce dont ils ne veulent plus : ressorts enchevêtrés des tribunaux, décisions arbitraires, vénalité des offices, « chicane » et privilèges – en particulier les tribunaux différents selon le statut social des justiciables." (Lemercier (2007))

[24] Un exemple représentatif de cette méfiance est l'interdiction faite aux juges d'entreprendre sur les fonctions administratives ; les constituants préférant une juridiction spécialisée. (Voir Mestre (2002))

[25] A propos des Conseils de Prud'hommes : "Leur caractère électif est en valorisé par les commentateurs

courant de la centralisation du système juridique, mais dans la continuité de la suppression de la vénalité des offices.

Les Prud'hommes aujourd'hui. Les Conseils de Prud'hommes demeurent aujourd'hui une exception dans le système légal français, et suscitent de nombreux débats passionnés.[26] Alors que le système prud'homal avait pour objectif initial d'améliorer la légitimité des décisions de justice, ces dernières années ont vu l'essor de critiques de plus en plus virulentes à son encontre. Les différents rapports rendus cette dernière décennie attestent de l'ampleur des réformes à mettre en place pour moderniser une institution qui crée aujourd'hui plus de polémiques qu'elle n'en éteint (Lacabarats 2014, Marshall 2013, Richard et Pascal 2010). Le faible nombre de réformes menées à terme démontre l'étendue des blocages qui caractérisent l'institution : l'opposition entre syndicats employés et organisations patronales rend difficile toute évolution, au détriment des employés et des employeurs.

Les Prud'hommes jouissent néanmoins en France d'une très grande popularité, et sont aujourd'hui la juridiction la mieux connue des Français (Cretin (2014)). La simplicité des procédures, telle que l'oralité des débats, contribue à cette popularité. Néanmoins les Conseils de Prud'hommes sont devenus, malgré eux, l'archétype des problèmes de mise en œuvre des normes de droit. Les délais de résolution de litiges y sont extrêmement longs, plus de deux fois supérieurs aux tribunaux de commerce ou autres juridictions civiles. L'encombrement des Conseils de Prud'hommes sont aussi le reflet d'une justice disposant de faibles moyens et d'un taux de conflit très élevé entre les employeurs et les salariés. Le taux d'appel y est en outre très élevé (62,1% en 2012)[27], et le taux de renversement en appel considérable.

Contextualisation du travail. La dernière évolution majeure qu'ont connue les Conseils de Prud'hommes a été amenée par Rachida Dati en 2008 (Garde des Sceaux), dans le cadre d'une refonte de la carte judiciaire. Eu égard aux politiques de restrictions budgétaires mises en place par son gouvernement, l'objectif de la ministre était de redéfinir les compétences géographiques des juridictions afin d'en réduire le nombre. Cet objectif de rationalisation a concerné plusieurs types de juridiction civile (tribunaux d'instance, chambres de commerce, et Conseils de Prud'hommes) mais les débats se sont majoritairement focalisés sur les Conseils de Prud'hommes. La concentration du débat autour des cours du droit du travail s'explique par la popularité et la proximité de ces cours d'une part, et le lourd tribut payé par cette juridiction d'autre part. En effet, la réforme de 2008 a supprimé plus de 20% des Conseils de Prud'hommes : parmi les 271 Conseils d'avant

du XIXe siècle : les critiques ne portent que sur la nécessité de rapprocher le corps électoral de la population des justiciables, de contrôler les modalités du vote ou de l'accompagner d'autres garanties." (Lemercier (2007))

[26] Les tribunaux de commerce, desquels s'est inspirée la création des Conseils de Prud'hommes, continuent cependant à partager ces singularités.

[27] Voir Lacabarats (2014)

réforme, 62 ont été supprimés.[28]

La réforme de la carte judiciaire s'est accompagnée d'une polarisation du débat entre les gagnants et les perdants de la réforme. Ainsi, les responsables politiques touchés par la réforme judiciaire ont considéré que la suppression des Conseils de Prud'hommes était susceptible de menacer l'accessibilité de la justice sociale pour les salariés : l'accroissement de distance pour accéder au Conseil de Prud'hommes risquait de décourager des salariés, licenciés sans faute réelle ni sérieuse, de faire valoir leurs droits et obtenir l'indemnisation de leur préjudice.

Ces dernières considérations sont extrêmement importantes eu égard aux questions soulevées dans les sections précédentes de cette thèse : la légitimité du système politique dépend de sa capacité à mettre en œuvre les normes de droit considérées comme légitimes — et souvent désirées — par la population. Une réduction de l'accessibilité aux cours menace le système politique, dans la mesure où les justiciables sont à même de penser que l'appareil juridique — et donc politique — n'est plus à même à assurer les fonctions qui lui sont dévolues. Alors que les travaux traditionnels en économie du droit se focalisent sur le bien fondé des plaintes portées aux cours, notre approche nous permet de souligner l'importance prépondérante à donner aux plaintes légitimes, non pas en termes de gain d'efficacité pour la société, mais en termes de stabilité du système institutionnel.

Les décisions en matière de carte judiciaire prennent alors toute leur importance : l'accessibilité à la justice détermine les possibilités de mise en œuvre du droit, et affecte ainsi la légitimité du système politique. Le système institutionnel français montre l'importance donnée à ce principe au travers du respect de la continuité du service public sur tout le territoire. Ces considérations ont toutefois été contrebalancées par la nécessité de réduire les coûts du système juridique, et la volonté de supprimer des cours à faible activité. La problématique qu'explore ce quatrième article est double : il s'intéresse d'une part aux choix effectués quant aux cours à supprimer, et étudie d'autre part les conséquences de ces choix sur la demande de résolution de litige (i.e. l'accessibilité des cours) et sur les durées de procédure.

Résumé du travail. La première partie de l'article s'intéresse aux choix effectués par le gouvernement. La question de la rationalisation de la carte judiciaire s'est posée dans le cadre de réduction des dépenses publiques et s'est avérée très polémique dans un contexte de crise du travail. De telles réformes ont néanmoins été discutées, et parfois mises en place, dans de nombreux pays d'Europe dans le cadre de réduction des dépenses publiques. Le gouvernement avait annoncé avoir pour objectif de supprimer les Conseil de Prud'hommes à faible activité, afin de recentrer les ressources sur les tribunaux traitant davantage d'affaires. L'opposition avait alors craint que de nombreux justiciables soient de fait exclus du système judiciaire, en aggravant les difficultés géographiques d'accès aux Prud'hommes. Le gouvernement avait aussi été suspecté de favoriser les membres de sa majorité lors de la suppression des tribunaux, en ciblant davantage les tribunaux dans les villes tenues par

[28]Un Conseil de Prud'hommes a aussi été créé, portant le nombre de cours à 210 après la réforme.

les partis d'opposition.[29] Afin de comprendre les arbitrages effectués par le gouvernement, la première partie de l'article utilise les données du ministère de la Justice sur l'activité des Conseils de Prud'hommes en 2007 et les croise avec des données socio-économiques locales de l'INSEE (PIB, chômage, population). Nous estimons empiriquement la probabilité qu'un Conseil de Prud'hommes soit supprimé en fonction de son activité et de la situation politique et économique au niveau local (modèle probit). Cette première partie conclut que le gouvernement a supprimé en premier lieu les tribunaux ayant une faible activité (peu de cas à traiter par an), avec de faibles durées de procédure, et où le taux de chômage était plus élevé. Elle montre aussi que le gouvernement s'est astreint à ne pas supprimer de Conseil de Prud'hommes dans les départements qui n'en possédaient qu'un seul. L'analyse empirique ne trouve cependant aucune indication selon laquelle le gouvernement aurait favorisé ses alliés politiques lors de la refonte de la carte judiciaire.

Dans un second temps, ce travail analyse l'impact de la réforme sur deux dimensions : la demande de résolution de litige et la durée des cas traités. Pour ce faire, notre étude utilise les données du ministère de la Justice sur l'activité des cours entre 2004 et 2012. Cette base de données contient des informations sur les quatre années qui ont précédé et les quatre années qui ont suivi la réforme. D'un point de vue technique, nous cherchons à estimer un système d'équations, dans la mesure où le nombre de cas et les délais sont susceptibles de s'influencer mutuellement : des requérants plus nombreux induiront une charge de travail plus importante, et donc des délais plus grands. De manière similaire, des délais plus longs sont susceptibles de décourager les justiciables, induisant une diminution de la demande de résolution de litige. Afin d'estimer l'impact de la réforme, l'approche empirique prend en compte cette interdépendance et repose sur des méthodes de triples moindres carrés. D'une part, en ce qui concerne la durée des litiges, l'analyse ne détecte aucune augmentation significative au niveau national. Nous détectons néanmoins des augmentations de durée pour les tribunaux qui ont dû supporter une très forte surcharge de travail suite à la réforme. L'effet est d'autant plus marqué à partir de 2011, ce qui suggère que les effets à long-terme peuvent conduire à une augmentation globale de la durée de résolution des litiges. D'autre part, l'article détecte une diminution significative du nombre de cas portés devant les Conseils de Prud'hommes suite à la réforme, et ce dès 2009. La contraction de la demande de résolution de litige est d'autant plus prononcée lorsque les tribunaux supprimés étaient éloignés des nouveaux tribunaux d'affectation. On observe que cette contraction est davantage prononcée dans les Conseils de Prud'hommes ayant récupéré une large charge de travail.

[29] En 2008, Rue89, journal en ligne classé à gauche, titrait "Réforme de la carte judiciaire : Dati a bien doté ses amis". `http://rue89.nouvelobs.com/2008/08/07/reforme-de-la-carte-judiciaire-dati-a-bien-dote-ses-amis`

Mise en Œuvre de la Norme Juridique

La mise en œuvre des règles de droit constitue la dernière étape du processus étudié, et est par nature l'objectif premier des étapes précédentes. Les systèmes constitutionnel et législatif ne sont que des préalables nécessaires en vue de mettre en place des politiques publiques, et donc, l'application de normes de droit.

La question de la mise en œuvre des règles de droit a été partiellement introduite par le troisième article de cette thèse. Le travail des juges de première instance est en effet similaire au travail des juges constitutionnels, dans la mesure où les questions d'interprétation y sont très présentes. Les questionnements liés à l'interprétation juridique demeurent aujourd'hui un tabou pour le système juridique français : de nombreux juristes arguent en effet que les textes de loi contiennent une norme objective qu'ils s'efforcent de chercher et de mettre en œuvre. Le mythe de l'existence d'une norme juridique objective a cependant été mise à mal par de nombreux travaux empiriques. Les travaux cités dans la section dévouée au troisième article ont montré que les juges essaient de concilier, consciemment ou non, l'interprétation de la règle de droit avec leurs préférences dans les Cours Suprêmes. D'autres articles ont néanmoins constaté les mêmes phénomènes pour les tribunaux de première instance.[30]

Composition Paritaire. Les Conseils de Prud'hommes ne sont pas exempts de ces polémiques, et sont devenus la cible privilégiée de nombreuses critiques concernant la partialité des conseillers prud'homaux. Les cas jugés aux Conseils de Prud'hommes sont en effet traités par des jurys composés à parts égales de représentants élus par les employés (deux conseillers) et de représentants élus par les employeurs (deux conseillers). Jusqu'à aujourd'hui, tous les conseillers prud'homaux ont été élus sur des listes syndicales au niveau de chaque section de chaque Conseil de Prud'hommes.

Cette parité représentative avait pour objectif initial d'accroître la légitimité des décisions en droit du travail, en conciliant les points de vue des employeurs et des employés. La composition *paritaire* a néanmoins échoué dans cette mission. Alors que le paritarisme devait assurer une minorité de blocage aux conseillers employeurs, on observe aujourd'hui une croyance généralisée selon laquelle les Conseils de Prud'hommes sont très favorables aux employés. S'il est vrai que 3 affaires sur 4 jugées par les conseillers prud'homaux sont tranchées en faveur des salariés selon la catégorisation établie par le Ministère de la Justice[31], la composition du jury montre cependant qu'il est impossible qu'une condamnation des employeurs se fassent à la majorité des représentants employés seuls. La peur des employeurs

[30] A titre d'exemple, Shayo & Zussman (2011) ont montré que les décisions des juges israéliens étaient influencées par l'ethnie des demandeurs. Cet article incarne l'étendue des facteurs qui influencent la prise de décision des juges.

[31] De tels chiffres doivent être interprétés avec prudence dans la mesure où les décisions prud'homales jouent essentiellement sur une dimension *intensive* : les cas gagnés par les employés peuvent être ainsi très éloignés de leurs demandes initiales.

pour l'institution n'en demeure pas moins très vive dans l'esprit général.[32] A ce titre, la composition paritaire des Prud'hommes a échoué à donner confiance aux justiciables sur l'impartialité de la cour. D'autre part, la composition *paritaire* des jurys crée également de nombreux blocages lors des décisions et de nombreux cas nécessitent l'intervention de juges professionnels. On estime ainsi que 18,3% des cas traités en 2012 ont nécessité une procédure de départage (Lacabarats (2014)). Le coût des procédures de départage pèse très fortement sur la justice prud'homale, car elles occupent une place croissante, et le faible nombre de juges départiteurs (professionnels) augmente considérablement les délais.

L'interprétation de la Norme. La question de l'interprétation de la norme est ainsi centrale dans le système prud'homal : les justiciables français anticipent une interprétation biaisée en faveur de l'employé et la composition paritaire des conseillers est source de départage. Le premier point reflète le fait que la population pense que les conseillers prud'homaux interprètent la norme juridique à la faveur de l'employé. Le second argument caractérise le fait que les conseillers interprètent la norme de droit à la lumière des intérêts des parties qui les ont élus. Ces deux éléments représentent de claires remises en cause des modèles kelseniens d'interprétation et soutiennent au contraire une vision réaliste du fait juridique.

Le cinquième travail de cette thèse s'intéresse ainsi à la composition des Conseils de Prud'hommes, et lie cette dernière à l'issue des cas traités auxdits Conseils. Notre étude part du constat que la composition des représentants employeurs est relativement homogène entre les Conseils de Prud'hommes : les listes soutenues par le MEDEF obtiennent une très grande part des sièges aux élections. Nos travaux s'intéressent alors aux variations de la composition des collèges employés où la représentation est plus hétérogène. Plus précisément, cinq grandes centrales syndicales se partagent les sièges : la CFDT, la CFE-CGC, la CFTC, FO, et la CGT. La diversité des opinions de ces syndicats est un phénomène établi en sciences politiques, et laisse supposer des comportements ou bien des anticipations différentes aux Conseils de Prud'hommes de la part des conseillers eux-mêmes ou bien des justiciables en fonction de la composition de la cour.

Les cinq syndicats employés sont généralement catégorisés en deux groupes : les syndicats *réformistes* d'une part, incluant la CFDT, la CFE-CGC et la CFTC, et, par opposition, les syndicats *non-réformistes* d'autre part, i.e. la CGT et FO. Nos travaux cherchent ainsi à comprendre dans quelle mesure la proportion de sièges allouée à chacun de ces deux groupes influence (i) la probabilité qu'un cas soit concilié au bureau de conciliation, (ii) la probabilité qu'un cas soit abandonné, à savoir qu'il disparaisse entre le bureau de concilia-

[32] À titre d'exemple : "Et pourtant, les usagers des prud'hommes ne manquent pas de motifs de plaintes : incertitude sur les décisions rendues, suspicion, de la part des employeurs, d'un préjugé favorable au salarié, lenteur de la procédure..." `http://www.lefigaro.fr/social/2015/06/17/09010-20150617ARTFIG00223-un-journee-ordinaire-au-conseil-des-prud-hommes.php` Dernier accès : 9 septembre 2015.

tion et la décision des conseillers prud'homaux, (iii) la probabilité qu'un cas soit jugé en faveur d'un salarié par les conseillers prud'homaux, (iv) la probabilité qu'un cas nécessite l'intervention d'un juge départiteur, et (v) la probabilité qu'un cas soit tranché en faveur du salarié par le juge départiteur.

Ces travaux ont ainsi pour objectif de comprendre comment la mise en œuvre de la norme par des juges élus peut avoir un impact sur le système judiciaire ainsi que sur les stratégies des justiciables.

Résumé du travail. Afin de comprendre la relation entre le niveau de non-réformisme des cours et les résultats des procédures ouvertes aux Conseils de Prud'hommes, nous utilisons une base de données mise à disposition par le Ministère de la Justice. Nous analysons ainsi l'intégralité des cas ouverts par des salariés et ayant une tentative de conciliation obligatoire. Nous croisons ces données avec les trois vagues d'élection déterminant la composition de chaque section de chaque Conseil de Prud'hommes (1997, 2002, 2008).

Le premier objectif consiste à déterminer le degré de non-réformisme de chaque section de chaque Conseil de Prud'hommes. En reprenant la dichotomie réformiste / non-réformiste énoncée en sciences sociales, nous calculons la proportion de juges élus appartenant à la CGT ou à FO. Cette première mesure est un indicateur du niveau de non-réformisme de la cour : plus cette proportion est élevée, plus les syndicats non-réformistes sont présents. Cette mesure suppose cependant que tous les syndicats d'une même catégorie (réformiste ou non-réformiste) partagent le même niveau de réformisme. Nous relâchons cette hypothèse en autorisant les syndicats à avoir des préférences hétérogènes au sein de chaque groupe. Nous construisons alors un axe de non-réformisme sur lequel nous classons les syndicats. Nous utilisons la propension de chaque syndicat à ratifier des Accords Nationaux Interprofessionnels (ANI) pour les discriminer sur cet axe : les syndicats ratifiant le plus d'accords avec les organisations patronales sont, par définition, les syndicats les plus réformistes. Les méthodes bayesiennes appliquées ici sont celles utilisées pour le classement des juges de la Cour Suprême aux Etats-Unis, présentées lors de la discussion du troisième chapitre de cette thèse. Cette technique livre une seconde mesure du niveau de non-réformisme de chaque cour. Enfin, nous relâchons une hypothèse supplémentaire, en autorisant les syndicats à changer de niveau de non-réformisme au cours du temps. Cette dernière estimation délivre ainsi une troisième mesure du niveau de non-réformisme au niveau local.

Dans un premier temps, nous croisons ces mesures de non-réformisme avec la probabilité de chacun des résultats possibles de résolution de litige (conciliation, abandon, décision par les conseillers prud'homaux, décision par le juge départiteur). Nos variables explicatives incluent des variables macroéconomiques (taux de chômage local, niveau de PIB par habitant), des variables caractéristiques des cas (genre du demandeur, représentation du demandeur, représentation du défendeur), la durée moyenne des cas terminés l'année précédente, et des effets fixes locaux (section $\times$ conseil de prud'hommes) et temporels (années). Nos estimations en probit simple et probit ordonné indiquent que les cours non-réformistes

sont associées à davantage de conciliation, d'abandon et de départage.

Notre étude se confronte cependant à un problème d'endogéneité : il est possible que les cours non-réformistes soient associées à des populations aux préférences plus contestataires. Les préférences (inobservées) de la population affecteraient ainsi la composition des cours ainsi que les comportements des parties. Nous proposons de corriger ce biais de variable omise en approximant les préférences de la population : nous utilisons le pourcentage de voix reçues par les partis de gauche au premier tour de des élections présidentielles comme proxy pour les préférences de la populatin. Les estimations intégrant cette nouvelle dimension confortent les résultats précédents.

Troisièmement, notre article explore la question du volume des litiges. Il est en effet possible que les cours non-réformistes attirent davantage de cas que les cours réformistes. Un tel phénomène biaiserait nos estimations en introduisant un effet volume. Nous estimons ainsi de plusieurs manières la relation entre la demande de résolution de litige et la composition des cours afin de déterminer si le volume de cas est constant. Nos résultats n'indiquent aucun impact du niveau de non-réformisme sur le nombre de cas ouverts.

Enfin, notre travail s'intéresse au processus de sélection des cas : les cas jugés par les conseillers prud'homaux sont en effet ceux qui n'ont pas été conciliés ni abandonnés. Cette double sélection peut ainsi faire disparaître une relation pourtant significative entre le niveau de non-réformisme des cours et les décisions rendues par les conseillers prud'homaux. Pour répondre à cette question, nous construisons un modèle de double sélection (triprobit) que nous estimons sur la conciliation, l'abandon et sur les décisions des conseillers prud'homaux. Nos résultats montrent que les cas conciliés sont généralement défavorables aux employés (ils auraient eu davantage de chance de perdre ces cas), et que les cas abandonnés leur sont davantage favorables (les salariés auraient eu plus de chance de les gagner). Puisque les salariés n'ont pas intérêt à abandonner des cas qui leur sont favorables, nous en concluons que les cas abandonnés sont en réalité négociés en dehors du système juridique. En contrôlant pour ces deux étapes de sélection, nous ne détectons aucune relation significative entre la probabilité pour un salarié de gagner aux Conseils de Prud'hommes et le niveau de non-réformisme de la cour.

Notre étude interprète ces résultats en termes d'anticipations rationnelles. Les cours non-réformistes créent davantage de départage que les cours réformistes. Anticipant des durées de procédure plus longues, et donc plus coûteuses, les justiciables sont davantage prêts à négocier avant la décision des conseillers prud'homaux. Les cas défavorables à l'employé sont ainsi plus souvent conciliés, tandis que les cas favorables sont davantage négociés en dehors des cours. Ainsi, seuls les cas réellement litigieux sont maintenus jusqu'au bureau de jugement, où les conseillers prud'homaux décident. Ces derniers font alors face à davantage de cas difficiles, et vont ainsi faire plus souvent appel au juge départiteur.

Chapitre	Étape du Processus	Domaine D'application	Question de Recherche
(1)	Création du système institutionnel	Droits Constitutionnels	Davantage de droits constitutionnels créent-ils une augmentation des dépenses publiques ?
(2)	Création de normes de droit	Redistribution	Le biais d'auto-complaisance induit-il des distorsions dans la demande et l'offre de redistribution ?
(3)	Contrôle de conformité	Conseil Constitutionnel	Quels facteurs non-juridiques influencent les décisions du Conseil Constitutionnel ?
(4)	Définition du système juridique	Réforme de la carte judiciaire	Quelles ont été les effets de la suppression de 20% des Conseils de Prud'hommes ?
(5)	Mise en œuvre des normes	Conseillers prud'homaux	Les cas ouverts dans les cours non-réformistes connaissent-ils une issue différents de ceux des cours réformistes ?

Tableau Récapitulatif : Résumé des chapitres, des domaines d'application et des questions de recherche.

1 State Provision of Constitutional Goods

This paper has been accepted for publication in *Constitutional Political Economy.*

Abstract

This paper investigates the impact of constitutional rights on the level of public expenditure in a large sample of countries. To do so, we construct a panel of 73 countries from 1960 to 2011. We first investigate factors that drive constitutional changes regarding constitutional rights. To address potential endogeneity concerns in the choice of constitutional rules, we rely on an instrumental variable within estimation (country and time fixed effects) to estimate the impact of constitutional rights on government size. We find that larger governments tend to inscribe fewer rights in their constitutions, but we do not detect any impact of constitutional rights on the government size.

JEL: E60, H50, K10

Keywords: constitutional rights, constitutional economics, government expenditure, government size, endogenous constitutions.

1.1 Introduction

Recent developments in the Constitutional Political Economy literature have studied the relationship between constitutional rules and state behavior. These works aimed to assess the impact of constitutional provisions on economic outcomes. So far, two sets of constitutional rules have received most of the attention, i.e., rules defining de jure judicial independence (Feld & Voigt (2003), Hayo & Voigt (2007), Hayo & Voigt (2013), Melton & Ginsburg (2014), Holcombe & Rodet (2012)), and electoral/governmental rules (Persson & Tabellini (2004), Persson & Tabellini (2005), Blume et al. (2009)).

Strikingly, the impact of constitutional rights on public expenditure has not been discussed. Constitutional rights have more than once been the subject of close theoretical

scrutiny (Sugden (1993)). For instance, Bose (2010) highlighted the central role that constitutional rights play in the institutional game. Elkins et al. (2013) have documented the rise of constitutional rights since the end of WWII. Law & Versteeg (2013) have investigated the relationship between constitutional rights and human rights violations.

Our work builds on these previous lines of research and proposes to investigate the economic impact of constitutional rights on state expenditure. To do so, we rely on previous discussions introduced by Voigt (2011), who considers constitutions to be public goods. Similarly, we propose to address constitutional rights as goods the state is required to provide. Indeed, constitutional rights are, by their very nature, substantive provisions that constitution-makers ask the state to respect and/or to implement. Because constitutional rights are, as a matter of principle, publicly provided, more inclusive constitutions are expected to induce larger governments. To put it bluntly: the more constitutional goods are inscribed in a constitution, the more the state will have to intervene in the economy. Our paper aims to test this last implication.

To answer this research question, we need to address some empirical challenges. First, we need to construct an index, which accounts for the number of rights inscribed in each country's constitution. To do so, we use the data introduced by Elkins et al. (2013). Second, to address the unobserved heterogeneity of our data, we are forced to consider fixed effects models, which limits our analysis to countries that have effectively changed their number of constitutional rights since 1960. Because sample selection might result from such restrictions, we determine which countries were the most likely to incur constitutional changes. We then investigate which factors influenced the determination of the number of rights inscribed in a constitution. Finally, we use IV-techniques to estimate the causal effect of the number of constitutional rights on the size of government.

The paper is organized as follows. We first discuss how constitutional rights can be expected to induce larger governments (Section 1.4.3). Second, we introduce the data and show some stylized facts (Section 1.3). Then, we discuss which countries were likely to change their number of rights since 1960 and which factors affected their choice when they selected the constitutional rights. Because of the potential endogeneity concerns we detect, we rely on an instrumental variable approach to estimate the causal impact of constitutional rights on government size (Section 1.4). Finally, we summarize our results and put them in perspective with the existing literature (Section 1.5).

1.2 The Relationship between Constitutional Rights and the Size of Government

Constitutional rights have received a significant attention by both public actors and legal scholars since WWII. The UN declaration of Human Rights (Elkins et al. (2013)), together with the two international covenants on human rights and the subsequent international treaties, have led to a worldwide recognition, at least virtually, of individual and

social rights.[1] As a consequence, human rights have become increasingly integrated into national constitutions and therefore have been given an internal legal value in numerous legal systems.

The question of enforcing constitutional rights, however, has become more complex. On the one side, it has been argued that constitutional rights may improve human rights only if states are willing to enforce them. In this view, constitutional rights mainly pursue an expressive purpose. For instance, states that inscribe rights in their constitution may lack the financial resources to enforce them (Bjørnskov & Mchangama (2013)). On the other side, the growing expansion of judicial review that took place with the succeeding waves of democratization (Ginsburg & Versteeg (2014)) has reinforced the legal scope of constitutional rights. Constitutional courts have developed a comprehensive set of mechanisms to make constitutional promises real.

The mechanisms used by courts to enforce constitutional rights are very diverse, and few courts adopt trans-substantive approaches.[2] Enforcement techniques, and thus enforcement consequences, are likely to vary with the nature of the right at stake. However, all rights, to one point or another, are likely to call for more state intervention. The extent to which the state is required to intervene depends on both the nature of the right and the enforcement mechanism of the national courts.

1.2.1 Constitutional Rights Calling for Larger Governments

Several categorizations have been proposed to discuss constitutional rights, including political versus social rights, individual versus group rights, and positive versus negative rights. We use the latter—and the most popular—categorization to discuss how constitutional rights may call for greater state intervention and, therefore, may induce larger governments.

The classical dichotomy between positive and negative rights focuses on their substantive implication for the relationship between citizens and the state. In Justice Barak's view, "negative rights define the limitations on a constitutional right that the state is precluded from imposing". On the other hand, he explains positive rights as those that "define the actions that the state is obligated to take in order to protect a constitutional right" (Barak (2012), p. 742). *Prima facie*, a restrictive approach would conclude that only positive rights lead to an increase in public expenditure. One can argue, however,

[1] Although many countries have ratified the International Covenant on Economic, Social, and Cultural Rights (ICESCR), only a few of them have actually ratified the UN protocol aimed at enforcing the Economic, Social, and Cultural Rights. States seem to agree on which rights should be promoted, but some of them are reluctant to ensure their *justiciability* at the national level.

[2] A trans-substantive approach considers that all constitutional rights necessitate the same degree of scrutiny, whatever the nature of the right at stake. The alternative consists of taking different interpretative approaches for different rights. For instance, racial discrimination and sex discrimination receive different levels of scrutiny in the United States (respectively, strong and medium).

that both types of rights are likely to increase the size of government.

Positive Rights. Standard examples of positive rights include the right to housing, the right to employment, and the right to health care. Regarding the latter, one expects the state to intervene in the economy to ensure that citizens have effective access to health care. In some cases, it imposes on the state a duty of health care provision for those who would be excluded from the private health care system, mainly because of financial constraints. It is then obvious that, if positive constitutional rights are indeed enforced by the state, they increase the burden of the government and, therefore, increase the size of the government.

A useful illustration of positive rights can be found in the jurisprudence of the Indian Supreme Court. In addition to individual rights, the Indian constitution provides a list of social-economic rights known as the *Directive Principles*. These social rights were originally meant to be purely declarative because the Constitution explicitly excludes them from judicial review. Nevertheless, the Indian Supreme Court has integrated them over time into the realm of judicial review by arguing that they were inherent to the concept of human dignity (which is included in the individual rights section). Several decisions have compelled the state to more intervene in more issues and, therefore to increase its expenditure. In *Bandhua Mukti Morcha v. Union of India*, the Supreme Court of India decided that the state was responsible for the infringement of constitutional rights in so far as it did not undertake measures to monitor the enforcement of the legislation by private parties (in this instance, the right to work in decent conditions). In *Daily Rated Casual Labour [...] v. Union of India*, the Supreme Court of India decided that "[t]he Government should be a model employer" and that it could not "compel any worker to work even as a casual labourer on starving wages". In its decision, the Court relied on the *Directive Principles* (Article 38(2)), which require the state to seek to minimize inequalities in the society. The aforementioned decisions best illustrate how positive rights may induce larger governments.

Negative Rights. Contrary to positive rights, assessing the impact of negative rights on state expenditure is a more complex task. Negative rights indeed aim at reducing the intervention of the state in areas where its intervention could harm individual liberties. In proportionality terms, the violation of these rights is so costly that no social benefits could outweigh the social costs of their limitation. Rights contained in the first ten Amendments of the US Constitution usually fall into this category. These rights, however, do not necessarily preclude the state from any intervention. As is the case in a proportionality framework, the state remains under the obligation to enact any alternative measure that would be less intrusive.[3] This, in turn, implies that states are forced to rely on more costly

[3]Proportionality inquiries in constitutional law seek, among other things, to figure out whether the government's actions are the least right-infringing measures that effectively achieve the pursued social goal. If courts conclude that other measures would have somehow reached a similar objective and would have been less intrusive, they are very likely to strike down the government's decision.

policies when these allow for achieving similar objectives while being less intrusive.

A good illustration of this reasoning can be found in *United States v. Jones*. In this case, the US Supreme Court was asked to decide on the constitutional validity of evidence obtained by the police through the use of GPS tracking devices. Justice Scalia, delivering the opinion of the Court, concluded that attaching a GPS tracking device to a suspect's vehicle was unconstitutional because it violated the Fourth Amendment. Reacting to the concurring opinion of Justice Alito, Justice Scalia recognized that the Fourth Amendment would not have been violated if the police had followed the suspect on the street instead of attaching a device to his vehicle. In his words: "Thus, even assuming that the concurrence is correct to say that traditional surveillance of Jones for a 4-week period would have required a large team of agents, multiple vehicles, and perhaps aerial assistance our cases suggest that such visual observation is constitutionally permissible". In other words, the Fourth Amendment, by imposing limitations on the state, forces public authorities to take more costly measures to achieve equivalent social objectives.

1.2.2 Factors Mitigating the Impact of Constitutional Rights

Despite the above arguments, some factors can mitigate the impact of constitutional rights on government size.

Rights as Signals. First, constitutional rights might lead to larger governments only if governments are willing to fulfill their constitutional duties when legislating. This is especially the case in autocratic countries, where leaders' preferences take precedence over constitutional provisions (Posner & Young (2007)).[4] Several concerns emerge and raise questions regarding the government's willingness to enforce constitutional rights. Constitutional rights might serve as *signals* for some countries: rights might embody goals they aspire to reach but that they are unable to enforce; others, such as autocratic regimes, may only seek to mitigate the international pressure regarding domestic human rights violations or as part of an effort to receive international investments.

Enforcement by Courts. To force governments to undertake measures to fulfill their constitutional duties, constitutions have been increasingly likely to empower constitutional courts. However, the great diversity of judicial review has led to very heterogeneous situations: some courts have developed strong mechanisms to coerce governments to undertake measures, while others clearly lack the independence or the judicial power to oppose the governments' decisions. Controlling for judicial review, however, is a difficult exercise because the scope and power of judicial review is often defined by courts themselves through

[4]Posner & Young (2007)) investigate violations of constitutional provisions by African leaders. They state the following: *"These cases remind us that many African leaders still possess the power to shape outcomes to suit their preferences, even when those preferences conflict with formal limitations on what they are legally permitted to do."* (p. 134)

case law. [5] At the present time, no dataset is available to control for the de facto level of enforcement of constitutional rights. Estimating the impact of constitutional rights would therefore lead to an average effect, estimated at the average situation of constitutional enforcement.

Provision By Third Parties. Some states delegate the provision of public goods to private companies. In such cases, even fully enforced constitutional rights might not affect the size of the government. It is true that, compared to a counterfactual state, which would provide public goods by itself, an outsourcing state would be less affected by constitutional rights. Other mechanisms, however, have been developed by the judiciary to bind private parties, which provide public goods to enforce constitutional rights. A useful illustration can be found in the *Kadaan vs. Katzir* case, decided by the Israeli Supreme Court. An Arab family that was willing to settle in a Jewish neighborhood called Katzir was denied authorization to move into the neighborhood by the Jewish Agency. This agency was partly delegated the authority of land allocation in the country by the state of Israel. The agency was intentionally discriminating against Arab families to promote Jewish culture in Israel. The Court ruled that, even though the agency was not a governmental agency, the very fact that the agency had been delegated this authority by the state made it subject to the same constitutional constraints as the state itself.

Similar mechanisms can be found in other countries. They aim to bind private or semi-public parties to enforce constitutional rights. Naturally, the introduction of new constitutional duties will have a stronger impact in insourcing states. However, mechanisms constraining delegated authorities are also likely to increase public expenditures: the state is ultimately responsible for the well-functioning provision of public goods, and may be asked to invest more or to intervene more to ensure that constitutional rights are effectively enforced. In this regard, a state with a given outsourcing level is also expected to incur a growth in public expenditure after the inscription of new rights in the constitution. However, we expect this increase to be lower than that for insourcing states because part of the cost is directly transferred to the citizens. Ultimately, the impact of constitutional rights on the government expenditure will depend on the allocation of costs decided by the government. The state might decide either to partially fund the provision of a right by private parties or to leave it to the market. In any case, the level of public expenditure is very likely to increase when the state inscribes a right in its constitution.[6]

Legal Scope. Constitutional rights may have a conditional impact depending on the

[5] The U.S. Supreme Court defined its scope of intervention in *Marbury v. Madison*. In India, the Supreme Court proclaimed that it was competent to assess the constitutionality of constitutional amendments in *Minerva Mills v. Union of India*.

[6] Bjørnskov & Mchangama (2013) write: "Whether the government produces or merely finances the access to, e.g., free education and health care services and provides substantial social security, [...], government expenditures are likely to be permanently larger if no other expenditures are cut." (p. 10)

legal scope courts give them. The two concepts of verticality and horizontality define the scope of the enforcement of constitutional rights. A right is said to have a vertical effect if the state is forced to enforce it when enforcing its own legislation. On the other side, horizontality refers to situations where courts recognize that a constitutional right also affects inter-citizen relationships. Vertical enforcement of constitutional rights is recognized in all countries with substantive judicial review because the very nature of constitutional review is to ensure that rights are well enforced by the state. On the contrary, horizontality has been recognized in fewer states because constitutions have mainly been seen as documents regulating the relationship between the state and its citizens. It is obvious, then, to see how vertical enforcement of constitutional rights is likely to induce larger governments: they ask for a direct intervention of the state for both *positive* and *negative* rights. The effect of horizontality, however, is less obvious. States with horizontal enforcement of constitutional rights might have larger governments because some understandings of horizontality require the state to take measures to ensure that constitutional rights are respected in inter-citizen relationships. An example is given by the German Constitutional Court, which recognized an indirect effect of horizontal enforcement (*Drittwirkung*), i.e., the state's duties to undertake measures to ensure horizontal enforcement of constitutional rights. Similar to the enforcement possibilities available to constitutional courts, no data have been collected to account for the legal scope given to constitutional rights by courts. Measuring the impact of constitutional rights on the size of government would therefore lead to an average effect. The estimated effect would be lower than the full effect in the case of complete *horizontality*, and greater than the effect in the case where horizontality is not recognized.

To put it in a nutshell, constitutional rights are expected to have a positive impact on the size of government. The great diversity of legal mechanisms available to judicial review, together with the very structure of each economy, implies that looking at the effect of constitutional rights without considering the specific institutional arrangement of each country can only yield an average effect estimation. Note, however, that estimates are robust to other institutional factors, such as federalism, to the extent that they do not vary over the period considered or that they are not correlated with the number of constitutional rights.

1.3 Data and Stylized Facts

The above discussion about the economic impact of constitutional rights concludes that constitutional rights induce larger governments. To assess the empirical validity of this claim, we build a panel dataset, which contains information about both government size and constitutional rights.

1.3.1 Presentation of the dataset

The above discussion about the economic impact of constitutional rights concludes that constitutional rights induce larger governments. In order to assess the empirical validity of this claim, we build a panel dataset, which contains information about both government size and constitutional rights.

Government Size. First, as far as the government size is concerned, we compute the share of the government expenditure in GDP as the share of government consumption of the total GDP. We derive these data from the national accounts of the Penn World Table (PWT, Feenstra et al. (2013)), which covers countries from 1960 to 2011. In addition to these data, we also consider World Bank (WB) data about government consumption in GDP. Although the WB data are more limited, we use them as robustness checks to ensure that our results are not driven by specific features of the PWT data. We thus obtain two variables accounting for the size of government: $SIZE^{PWT}$ and $SIZE^{WB}$.

Rights. Second, concerning the number of constitutional rights, we take over the variable constructed by Elkins et al. (2013). In their article, the authors introduce a unique worldwide dataset, which covers 74 constitutional rights. Following their work, we construct a variable $RIGHTS$, which is equal to the sum of rights written in a constitution (each right counts for 1 if it is mentioned in the constitution). By doing so, we obtain a variable that varies both across time and across countries.[7]

Determinants of Government Size. The literature on the size of government has investigated some theories hypothesizing a causal effect of social, economic, and political variables on the level of public expenditure. Bergh & Karlsson (2010) investigated the relationship between government size and economic growth. Their investigation relies on Bayesian Averaging of Classical Estimates (BACE) and finds no relationship between the two variables. Benarroch & Pandey (2008) focus on the potential impact of trade openness on government size. Using a panel dataset from 1979 to 2000, they conclude that trade openness does not impact government size, unlike in the original study of Rodrik (1998). Alesina & Wacziarg (1998) explore both the relationship between country size and trade openness, on the one side, and government size, on the other. They find that governments tend to be relatively smaller in larger countries and that smaller countries tend to be more open to international trade. In a more recent study, Ram (2009) uses a very broad dataset to investigate the same research question. The paper relies on a fixed effects approach and concludes that there is a positive relationship between government size and both trade openness and country size. Furthermore, in a very detailed investigation, Shelton (2007) explores several theories that aim to explain government size. The paper takes into consid-

[7] To assess the validity of this aggregate indicator, we computed the Cronbach's alpha for the indicator in 2012. We obtained a score of 0.926, which indicates a very good internal consistency.

eration the trade-off between integrating more variables into the estimation equation and the scarcity of the data, which usually leads to reduced samples. The author concludes that government size is (i) positively associated with trade openness, (ii) positively associated with the share of population over 65, (iii) negatively correlated with majoritarian electoral systems, (iv) not related to income, and (v) positively related to political rights. Finally, Holcombe & Rodet (2012) show that the rule of law is positively correlated with the size of government. All of these papers use static models and, if possible, integrate fixed effects in their estimations.

Constitutions and Government Size. Following previous works on constitutional rules (Persson & Tabellini (2004), Persson & Tabellini (2005), Blume et al. (2009)), our investigation of the relationship between constitutional rights and the size of government takes into consideration several dimensions. First, we control for some demographic and economic factors: the logarithm of the real GDP per capita ($LGDP$), the logarithm of the population ($LPOP$), the openness of the country ($OPEN$), the share of the population below 14 years of age ($POP14$), and the share of the population above 65 years of age ($POP65$).[8] Second, we take into account some political factors: the level of *de facto* democracy (POL), a variable that accounts for the membership to the International Covenant on Civil and Political Rights ($ICCPR$), and the number of years the current constitution has been in force (DUR).[9]

1.3.2 Frequency of Constitutional Changes and Rights Variations Since 1960

Figure 1.1 displays both the number of new constitutions and the number of constitutional amendments per year since 1950. As we can see, the number of new constitutions varies from 1 to 20 per year. In addition, the number of constitutional amendments ranges between 10 and 45 annually. The relatively high number of constitutional modifications (i.e., amendments and new constitution together) creates some room for change in the number of constitutional rights. In total, 122 countries have changed their number of constitutional rights between 1960 and 2011.

Because of the availability of data on the control variables, the sample size of the empirical investigation (section 1.4.3) is reduced to 73 countries between 1960 and 2011 for the PWT data. Figure 1.2 in the appendix displays the average number of constitutional rights for three samples: the *Full Sample*, which includes all countries coded by Elkins et al. (2013), the *Variation Sample*, which corresponds to the sample of countries whose number of rights has changed since 1960 (122 countries), and the *Data Sample*, which is the sample

[8] *LGDP, LPOP* and *OPEN* were obtained from the Penn World Tables (Feenstra et al. (2013)). *POP14* and *POP65* were obtained from the World Bank Database.

[9] *POL* was imported from the Polity 2 index computed by the Polity IV Project. We recoded the variable into a 0 to 20 scale.

of countries for which we have all explanatory variables, including the government size of the PWT data, and which and that have experienced variations in the number of rights. Figure 1.3 displays the same graph for the World Bank Data.

With regard to the three samples, we can observe that they share a common trend. Indeed, we do not detect any distortion in the number of rights by considering only countries that experienced variations and for which control variables are available. We present descriptive statistics of the *Data Samples* in the appendix (table 1.3) for both PWT and World Bank datasets.

Remark 1: Focusing on the countries that have changed their number of constitutional rights since 1960 and for which we have additional data does not create a distortion in the number of constitutional rights compared to the full set of countries.

1.3.3 Choosing How to Change the Constitution

Countries that alter their constitution might not be a random sample of the entire set of countries. Indeed, more stable countries may be less likely to modify their constitution because their constitutional system is more entrenched and therefore more costly to modify (Levinson (2011)). On the contrary, unstable countries may write new constitutions more frequently because new constitutions might serve as de jure legitimations of the new empowered authority. For instance, Albertus & Menaldo (2012) have shown the importance of establishing constitutions in autocratic countries.

Looking at constitutional changes since 1960, we observe two distinct patterns for OECD and non-OECD countries. OECD members in 2012 have been much more likely to amend their constitution and much less likely to write a new constitution than non-OECD countries.[10] Table 1.1 displays some statistics about constitutional amendments and new constitutions. First, it appears that OECD members were much less likely to write a new constitution between 1960 and 2012. Indeed, only 43.8% of them did so, while approximately 90.7% of the non-OECD countries did it in the same period. Second, it seems at the first sight that both OECD and non-OECD countries were equally likely to amend their constitution: approximately 93.8% of the OECD members amended their constitution at least once, compared with 91.4% for the non-OECD countries. However, OECD members amended their constitution much more often: on average, OECD members amended their constitution in 27.7% of the periods considered, i.e., almost once every four years. In contrast, non-OECD countries amended their constitution once every seven years (14.4%).

Remark 2: Non-OECD countries were more likely to change their constitution by writing a new constitution. On the contrary, constitutional changes in OECD countries were mostly achieved through constitutional amendments.

[10] We compare the set of countries that were OECD members in 2012 and the set of countries that were not OECD members in 2012.

Table 1.1: New constitutions and constitutional amendments between 1960 and 2012 per OECD status in 2012.

	OECD Status	
	Member in 2012	Not Member in 2012
% which wrote at least one constitution since 1960	43.8%	90.7%
% which amended at least once their constitution since 1960	93.8%	91.4%
Average frequency a new constitution was written	1.6%	4.9%
Average frequency the constitution was amended	27.7%	14.4%

1.3.4 Constitutional Rights Since 1960

We now investigate which rights have been the most likely to be adopted or removed between 1960 and 2012. Table 1.2 in the appendix provides three sources of data. The first column indicates the percentage of constitutions in force in 1960 that specified each of the 74 rights. The second column displays the same information for constitutions in force in 2012. The third column computes the difference between the two percentages to show the adoption trend of each right. Positive scores reflect rights that were more likely to be adopted between 1960 and 2012, while negative scores are associated with rights that were more likely to be dropped.

First, one can note that no right has been massively dropped during this time period. This indeed reflects the global trend toward rights inflation depicted by Elkins et al. (2013). A few rights have lost influence over time: the right to bear arms was inscribed in less than 2% of the constitutions in force in 2012, while it was mentioned in 5.8% of them in 1960; the right to be judged by a jury was written in 26.7% of the constitutions in force in 1960, but was mentioned in only 17.2% of those in force in 2012.

On the contrary, a great number of rights have gained in influence in the last five decades. The most popular rights have been negative rights as defined in the above section: the prohibition of cruel, inhuman, or degrading treatment (+48.7 percentage points), the right to a fair trial (+37.2), and the freedom of movement (+38.5). Nevertheless, positive rights have also experienced a fair increase: the state's duty to protect or promote culture (+30.3), the right to counsel if one is indicted (+43.7), the right to health care (+26.8), the right to a safe/healthy working environment (+17.4), and the right to shelter or housing

(+17.4).

In 2012, the most popular rights were negative rights: the freedom of expression or speech (93.8% of the constitutions in force), the freedom of assembly (92.8%), the freedom of religion (92.2%), the freedom of association (92.2%), the freedom of movement (85%), and the right of privacy (83.9%). On the other hand, the most popular positive rights were less consensual: the state duty to protect or promote culture (61.7%), the right to health care (48.9%), or the right to just remuneration, fair compensation (47.2%).

1.3.5 Constitutional Changes and Government Size

To take an initial look at the relationship between the number of rights and government size, we consider the level and the evolution of government size at the time the number of rights in a constitutional system is changed. To do so, we consider periods of constitutional changes that led to variations in the number of constitutional rights, and we analyze the level of public expenditure before and after the change for three categories of countries.

Figure 1.4 displays the evolution of the level of expenditures of the central government where there was a change in the number of constitutional rights. We consider the size of the state five years before and five years after a constitutional change (either due to a new constitution or to an amendment) that induced a change in the number of constitutional rights. To isolate the effect of the targeted change, we keep only countries that did not modify their constitution in the five years preceding and five years following the change. To insure cross-country comparability, we normalized the level of expenditures at 100 five years before the constitutional change. Moreover, to avoid averages with different samples, we keep only countries for which we have data during the entire period.

The graph displays the evolution of government expenditure for three groups of countries: countries that decreased their number of rights (group 1), countries that slightly increased their number of rights (from 1 to 6 additional rights—group 2), and countries that greatly increased their number of rights (more than 7—group 3).[11]

Figure 1.4 yields interesting results. First, one can observe that the second and the third groups, i.e., countries that increased their number of constitutional rights, display similar trends prior to the constitutional change: both groups have increased, on average, their public expenditure by approximately 5% in five years. On the contrary, countries in the first group, i.e., those that decreased their number of rights, have experienced a much larger growth in the level of public expenditure in the five years preceding the constitutional change.

Second, one can note that all categories have increased their expenditures in the two years following the constitutional change. Surprisingly, countries that decreased their number of rights experienced, on average, a constant growth in public expenditure. This result tends to be in contradiction to the hypothesis discussed in the section above. Nevertheless, countries that considerably increased their number of rights (group 3) endured the greatest

[11] The threshold of 7 was defined to have balanced groups.

increase in public expenditure five years after the reform.[12] The greater increase in the level of expenditure incurred by group 3 is in line with the hypothesis we aim to test.

To confirm that these initial results are not solely driven by the considered range (i.e., *five* years before and *five* years after the change), we also display the same figure for the period *four* years before and *four* years after the change. Looking at figure 1.5,it appears that the two preliminary observations are reinforced: the increase in public expenditure for countries that increased their number of rights is even more obvious.

Preliminary observations: (i) Countries that decreased their number of constitutional rights experienced a considerable increase in their level of expenditure prior to the constitutional change. (ii) Countries that considerably increased their number of constitutional rights experienced the greatest increase in public expenditure after the constitutional change.

1.4 Econometrics

To investigate the relationship between the number of constitutional rights and the size of government, we proceed in three steps. First, we analyze states' decisions of whether to change their number of constitutional rights. Second, we address states' choices to increase or decrease their number of constitutional rights. Finally, we propose to estimate the causal impact of constitutional rights on the size of government.

1.4.1 Choosing to Change the Number of Constitutional Rights

In the first step, we analyze which countries have changed their number of constitutional rights since 1960. This investigation addresses the issue of sample selection because more stable constitutions might be correlated with some important factors, such as greater political stability or better provision of public goods.

To answer this question, we run a probit estimation on the probability of a country to have changed its number of constitutional rights between 1960 and 2012 (subject to the availability of data for the control variables). We find that 122 of the 182 countries in our database have changed their number of constitutional rights at least once since 1960. However, due to the limited availability of data on the independent variables, we are only able to run probit estimations for 114 countries, among which 76 have changed their constitution since 1960.

The latent utility model associated to the probit estimation is expressed as follows:

[12]The average score of public expenditure for group 1 went from 120.3 to 127.8 between the year of constitutional change and five years after the change, which corresponds to an increase of 5.4%. Concerning the third group, the score increased from 103 to 113.8, which is equivalent to an increase of 10.5%.

$$CH_i^* = \beta_1 \overline{SIZE_i} + \beta_2 \overline{RIGHTS_i} + \beta_3 \overline{X_i} + u_i \tag{1.1}$$

where CH stands for *change* (1 if country i changed its number of constitutional rights at least once, 0 otherwise), $SIZE_i$ is a measure of government size, $RIGHTS_i$ corresponds to the number of rights, X_i is a vector of control variables, including a constant[13], and u_i is a normally distributed random term. The bar above the independent variables indicates that the variables have been transformed.

We present three sets of results in table 1.4 depending on the transformation of the independent variables we consider. First, we consider the country average value of the independent variables for all available observations between 1960 and 2012. Second, we use the value of the first time we observe the independent variable in our dataset for each country. Third, we use the latest value of the independent variables for each country. Moreover, for each specification, we estimate the probit model by using both the PWT and the World Bank data separately.

Three main results emerge from table 1.4. . First, considering the second specification (columns 3 and 4), we can observe that countries that had many rights at the beginning of the period were less likely to change the number of rights during this same period. This observation might reflect a catching-up effect: because of the overall increase in human rights during the second half of the 20th century (as shown by Elkins et al. (2013)), countries with few constitutional rights might have been pressed to catch up, and thus to increase their number of constitutional rights. Second, we also observe that countries that have been the most likely to change their number of constitutional rights were also the least democratic countries in 1960. Given that the effect of POL is not significant in the third specification, it follows that the current level of democracy does not matter. Altogether, these two observations might suggest that countries that have changed their number of constitutional rights have also *improved* their democratic situation. To confirm this prediction, we compute the change in the democratic score between the first and the last year of observation for countries that changed their number of rights and those that did not. A two-group mean comparison test on 157 countries rejects the null hypothesis, and confirms the previous finding: countries that changed their number of rights also *improved* their democratic situation.[14] Third, we also observe that the current population

[13]It includes: the number of years the constitution has been in force, the ratification status of the ICCPR, the log of the GDP per capita, the log of the population, the level of openness, the polity2 score, a proxy for human capital, the share of the population below 14 years old, and the share of the population above 65 years old.

[14]The bilateral two-group mean comparison test rejects the null hypothesis at the 99% confidence level. Countries that did not change their number of rights improved their democratic situation by 1.83 points, on average, against 4.89 for those that did change their number of rights.

is positively associated with the probability of a change in the number of constitutional rights. It indicates that more populated states have been more likely to change their number of rights. This result *per se* is difficult to interpret because the coefficient associated with the average and the first values are not significant.

Results 1: Countries that have changed their number of constitutional rights:
(i) had fewer constitutional rights in 1960, which suggests that they have sought to catch up on the number of constitutional rights; and
(ii) improved their democratic situation between 1960 and 2012.

1.4.2 Choosing the Number of Constitutional Rights

The previous section has shown that the countries that changed their number of rights are not random. A second question arises regarding the direction and the intensity of these changes: which factors influence the number of constitutional rights?

Constitutional changes are costly political events, as they require either a large consensus to modify the constitution in democracies or strong resources to impose new institutions by force in authoritarian regimes. Factors that may call for a high or low level of constitutional rights might also influence the opportunity for constitutional change itself. In other words, we observe changes in the number of constitutional rights only when the underlying factors affecting the number of constitutional rights are strong enough to lead to a modification of the constitution (either by amendment or by creating a new constitution).

It follows that estimating the impact of some independent variables on the choice of the number of constitutional rights only when changes occurred would suffer from sample selection issues. Therefore, we propose to explain choices of the number of constitutional rights by controlling for the selection step, i.e., the probability of modifying the constitution. To do so, we use a Heckman procedure. The equation of the number of rights is as follows:

$$RIGHTS_{it} = \gamma_1 SIZE_{i,t-1} + \gamma_2 X_{i,t-1} + v_{it} \tag{1.2}$$

where $SIZE_{i,t-1}$ is the size of the government at time $t-1$, $X_{i,1t}$ is the lag value of the same vector of control variables as in equation (1.1) plus a linear time trend, and v_{it} is a zero-mean random term. We consider lag values of the control variables to avoid endogeneity issues.

The latent utility model of the selection equation is expressed as follows:

$$CH^*_{it} = \phi_1 SIZE_{i,t-1} + \phi_2 X_{i,t-1} + \phi_3 PRESS_{i,t-1} + \epsilon_{it} \tag{1.3}$$

where ϵ_{it} is a normally distributed random variable.

Identification of the Heckman model is achieved by having at least one independent variable included in the selected step, which is not included in the main equation. In our case, it requires a variable that influences the probability of modifying the constitution but that does not determine the number of constitutional rights. Our specification uses the variable $PRESS_{i,t-1}$ as an instrument. This variable is equal to the difference in the number of rights between country i's constitution and the average number of rights in the world. It represents the *pressure* for constitutional modification: the greater the value of this variable, the more *outdated* a constitution might be and, therefore, the more likely the country is to modify its constitution. Therefore, we expect this variable to be correlated with the selection step. Moreover, we believe that this variable does not directly affect the number of rights. When choosing the number of rights, constitution-makers do not consider the previous gap between the former constitution and the previous average number of rights in other countries; rather, they take into account the entire set of rights that are 'available': the larger the set of rights that have been acknowledged by other constituencies, the more rights there are available to inscribe in a constitution. The linear trend in our specifications aim at capturing this inflation of rights. As we comment below, this trend is indeed highly significant and is in line with Elkins et al. (2013).

Table 1.4 displays the results of a series of estimations with the PWT data.[15] The first column displays the results of an OLS estimation of equation (1.2) considering all observations of countries that changed their number of constitutional rights at least once since 1960. The second column shows the result of the same estimation, but considers only observations when the constitution was amended or when a new constitution was written. The third column corresponds to the previous estimation, but considers only observations when the number of rights was modified. The fourth and fifth columns correspond to the first and second steps of the ML Heckman estimation, where the number of rights is estimated only when constitutional change occurs and when the number of rights is changed.

First, comparing the general performance of our regressions, we can observe that the first two columns yield very similar results. The third and the fourth columns also display findings close to the two previous columns, except for DUR, $ICCPR$, $LPOP$, and $POP65$. The Heckman estimation finds a very high correlation coefficient between the error terms of the selection and the main equations (resp. equations 1.3 and 1.2). This suggests that selection does occur, i.e., that changing the number of constitutional rights is not independent of the number of constitutional rights a country decides to inscribe in the

[15] We run the estimations for the World Bank data as well. The results for the OLS estimations were nearly identical. Because of the reduced availability of the data and the high number of censored data in the Heckman estimation, convergence was very difficult to obtain for the Heckman estimation. However, subspecifications (excluding some independent variables) indicated similar results.

constitution.

The results of these estimations are very similar with regard to the main variable of interest, i.e., the size of government. The three OLS specifications detect a negative impact of the size of government on the number of rights. The Heckman specification confirms that this effect is not affected by the selection step: the size of the government does not influence the *probability* of changing the number of constitutional rights, but does influence the *number* of constitutional rights one inscribes in the constitution. This finding suggests that endogeneity might be a major concern when estimating the impact of the number of constitutional rights on government size. We can note, however, that the specifications in table 1.4 do not include country fixed effects because of the small number of changes in the number of rights. It might be that countries with a strong involvement of the state (such as Norway) inscribe fewer rights in their constitution (for instance because of their well-developed case law), but that variations of wealth within a country might not influence the number of constitutional rights. In all, our results suggest that endogeneity might be an issue, but we cannot draw that conclusion too rapidly.

As far as the remaining variables are concerned, we observe that wealthier countries inscribe more rights in their constitution. The coefficient associated to $TREND$ is positive and statistically significant: it captures the fact that constitutional rights have inflated over the second half of the 20th century. The two first specifications also detect that younger constitutions, i.e., those that have been in place for only few years, contain fewer rights. The specifications also reveal that countries that ratified the ICCPR and those that have a small share of old citizens are more likely to have constitutional rights. Finally, the selection step of the Heckman specification shows that more populated countries are more likely to change their number of constitutional rights.

Results 2: The size of government (i) does not affect the probability of changing the number of constitutional rights, (ii) but determines the number of constitutional rights: countries with larger governments tend to put fewer rights in their constitution.

The negative impact of the size of government on the number of rights might appear surprising at first sight. Indeed, countries with larger governments should be used to state intervention, and citizens should therefore trust the state to undertake further duties. One could also think that larger governments are *de facto* providing some constitutional rights, and citizens should seek to recognize *ex post* the state's duties in their constitution. Both propositions should induce a positive relationship between the size of government and the number of rights. On the contrary, we observe that larger governments are associated with fewer rights. From an economic perspective, this observation might result from decreasing returns on state intervention: political actors might seek to reduce the intervention of the state (which is costly because of increasing organizational costs) by limiting the number of rights in their constitution. This would account for the negative relationship depicted by the data. Similarly, one could also argue that larger governments already result from an

extensive case law developed by higher courts and that such governments do not see the necessity of inscribing in their constitution rights they already provide.

1.4.3 Impact of the Number of Constitutional Rights on Government Size

The discussion of section predicts a positive impact of the number of constitutional rights on the size of government. To assess the empirical validity of this claim, we need to tackle some empirical challenges, i.e., sample selection and two endogeneity issues (unobserved heterogeneity and feedback effect).

1.4.3.1 Estimation Strategy

The investigation of the impact of constitutional rights on the size of government must address three empirical issues. First, it might be that countries have different inherent propensities to have constitutional rights and to have large governments, which might lead to unobserved heterogeneity. This problem is equivalent to an omitted variable problem, which may lead to endogeneity concerns. To isolate the effect of the number of constitutional rights, we need to focus on the *changes* in the number of rights in each country. To do so, we exploit the panel structure of our dataset and include fixed effects. Second, it may also be that countries that have changed their number of rights are a specific subsample of countries: evaluating the impact of constitutional rights on this subsample might be misleading. The first subsection of the current section investigated this issue (see results 1). Sample selection might also come from the independent variables used in our estimations. We discuss the potential bias that arises from this below. Third, the last subsection showed that the size of government might also determine the number of constitutional rights, leading to potential feedback effects (endogeneity) if the number of rights has an effective impact on government size. In the following paragraphs, we develop an empirical strategy to address both of these endogeneity concerns (unobserved heterogeneity and feedback effects).

Unobserved heterogeneity. To address unobserved heterogeneity, we rely on fixed effects specifications. Previous works in constitutional political economy have also taken advantage of the panel structure of their data to cope with unobserved heterogeneity. However, these works have faced a strong econometric challenge: constitutional rules experience very few variations over time. In constitutional political economy, and more globally in political science, this problem is known as the *iron law*. Past works have addressed this issue in three ways. First, some authors have used random effects models, which all assumed strict exogeneity between the individual effects and the regressors (Gutmann & Voigt (2013)). Such investigations usually lead to doubtful results because they often fail at fulfilling the exogeneity condition, leading to biased and inconsistent estimators. Sec-

ond, some authors have averaged data over time and have run cross-section regressions (Melton & Ginsburg (2014)). Such analyses do not take advantage of the panel structure of the data and are therefore as informative as simple cross-section investigations. Third, some other papers have dropped observations to implement fixed effects procedures. The main disadvantage of this last strategy lies in the potential selection bias between countries that experienced constitutional changes and countries that did not.

Our empirical strategy relies on a fixed effects approach. While the iron law turns out to be a major concern when one focuses on some specific constitutional rules (e.g., whether the government can be dismissed by the Parliament), it has a much more limited impact on our *rights* variable, which aggregates several rights. In fact, although the probability of changing *one* particular right may be very low, the total probability of changing *at least one* right in the 50-year period is much higher.

Sample Selection. To run fixed effects estimations, we consider only countries whose number of constitutional rights varied in the period for which we have data. To do so, we selected countries whose constitution has been either amended or changed between the first year of observation of the dependent variable and 2012, such that the number of rights has varied in the time period we consider (subject to data availability). Because our investigation relies on two dependent variables, we obtained two samples of countries.[16]

Section 1.3 has presented some stylized facts about constitutional changes since 1960. It showed that 122 countries have changed their number of constitutional rights between 1960 and 2011. Because of the availability of data on the control variables, the sample size was reduced to 73 countries between 1960 and 2011 for the PWT data and to 69 countries for the WB data. We present descriptive statistics of the two regression samples in the appendix (table 1.3).

General Specification. Our general econometric model is as follows:

$$SIZE_{it} = T'_{it}\beta + RIGHTS_{it}\gamma + \alpha_i + \mu_t + \epsilon_{it} \tag{1.4}$$

where $SIZE_{it}$ denotes the relative size of the government as the share of GDP, T_{it} is the vector of control variables for country i at year t, including a constant[17], $RIGHTS_{it}$ is the number of rights in country i's constitution at year t, α_i is the country fixed effect, and μ_t is the year fixed effect.

Feedback Effects. The main challenge of our investigation concerns the potential source of a feedback effect. In fact, a global consensus has emerged in constitutional po-

[16]Because our estimation relies on instruments, we need to get rid of countries that did not experience variations in the number of constitutional rights. The first stage estimation would otherwise create artificial variations for the instrumented variable.

[17]It includes variables contained in X (previous subsection) and variable LOPOL (see below).

litical economy and, more globally, in the new institutional economy, to recognize that institutions are endogenously selected.[18] Constitutions are often written either by constituent assemblies or by the executive or legislative branch themselves (Ginsburg et al. (2009)), which *de facto* excludes quasi-natural experiments.

As far as our investigation is concerned, feedback effects would arise if government size and the number of constitutional rights were influencing each other. The above subsection showed that the lag value of government size is associated with a negative and significant effect on the number of constitutional rights. To determine whether the number of rights effectively affects the level of public expenditure, we need to anticipate such potential feedback effects.

The general solution for such feedback effects consists of relying on instrumental variables, which must fulfill two criteria: they must first influence the number of constitutional rights, but they also need to be uncorrelated with the degree of state intervention.[19]

We propose three instruments that, in our view, fulfill these two criteria.

First, we propose two instruments that account for international pressure when amending or changing a constitution. As Elkins et al. (2013) shown, worldwide practices greatly influence domestic constitutional design.

On the one side, we claim that constitutions are written in the light of previous and contemporary constitutional systems. We believe, therefore, that the date at which a constitution is written has an impact on the number of constitutional rights: when one amends a constitution in 2015, one is more likely to introduce constitutional rights than one century ago because the very notion of human rights has expanded to include many additional dimensions. We propose to capture this idea by considering the worldwide average number of rights in constitutions the year before country i modified its constitution. We denote the corresponding variable CH_WAR (WAR stands for *worldwide average rights*, and CH for *change* because its value is set one year before the last constitutional modification).

On the other side, we aim to capture a second dimension of international pressure, namely, the de facto situation of human rights in influencing countries. In our view, when countries decide to change the content of their constitution, they are influenced by the democratic situation in similar legal systems. As Goderis & Versteeg (2012) have shown, countries are more likely to follow constitutional choices made by countries that share the same legal origin. Their analysis even concluded that legal origin is the most important predictor of constitutional diffusion. They argue that "constitution-makers borrow from states with which they already share important pre-existing legal similarities" (Goderis & Versteeg (2012), page 2). For this reason, we consider the influence between countries

[18] As Blume et al. (2009) state: "Institutions are man-made which means that there is always a potential endogeneity problem that needs to be dealt with." (p. 203)

[19] A more rigorous statement would be that instruments must be uncorrelated with the error term of equation (1.4), i.e., with government size that is not explained by the remaining independent variables.

that share the same legal origin (Glaeser & Shleifer (2002), Glaeser et al. (2004)). We postulate that the de facto democratic situation in countries that share the same legal origin influences the choices to inscribe constitutional rights in a constitution: the more democratic similar countries are, the stronger the democratic pressure is, and therefore, the more likely the countries are to include constitutional rights. To account for this phenomenon, we generate a variable CH_LOPOL (where LO stands for *Legal Origin* and POL for the *polity2* score). Because the democratic situation of the similar countries might have an impact on the level of public expenditure, we control in the main equation and in the first stage by the *contemporaneous* value of $LOPOL$.

Third, we consider the nature of the authority that drafted the constitution. Ginsburg et al. (2009) found that the drafters' identity matters for constitutional design. We thus use the nature of the authority that wrote the constitution to proxy the number of constitutional rights. In our view, the identity of the drafters is very likely to influence the number of constitutional rights because of the strategic behaviors of the political actors. Indeed, we believe that whenever the constitution is written by one of the existing branches of power (i.e., the executive or legislative branch), it will include fewer rights because such drafters would be reluctant to constrain themselves in the future. We create a variable $DRAFTER$, which is equal to 1 when the constitution was not drafted by the executive or the legislative in place (e.g., when a constituent assembly was elected), and 0 otherwise.[20]

Because these two variables aim to instrument the number of rights, we freeze the instruments at the date of the last constitutional modification (amendment or new constitution). Using contemporaneous values of the instruments would indeed lead to biased estimations because the instruments would approximate the number of rights that would have been in the constitution if it were revised in the present year. For instance, Albania changed its constitution in 1998. To instrument the number of rights in 1999 and the following years, we take the value of our instruments in 1998.

Altogether, the first stage of our estimation for the number of rights is:

$$RIGHTS_{it} = T'_{it}\phi + Z'_{it}\sigma + \xi_i + \nu_t + u_{it} \tag{1.5}$$

where T_{it} contains the same variables as in the second stage equation, and Z_{it} includes CH_WAR, CH_LOPOL and $DRAFTER$. We rely on both 2SLS and GMM estimates.

To further assess the validity of our instruments, we run a series of specifications and, for each of them, report the results of the *underidentification test*, the *weak identification test*, and the *overidentification test*. We reject a test's null hypothesis when the test's

[20]The variable DRAFTER focuses only on the drafting procedure of new constitutions. It is therefore not modified when a constitution is amended. For instance, a constitution originally drafted by a constituent assembly but modified by the legislative branch will keep a DRAFTER score equal to 1.

p-value is lower than 0.05. For the overidentification test, we report the p-value when it is higher than 0.05. Concerning the weak identification test, we present both the highest Stock-Yogo statistics and the corrected F-statistics.[21]

1.4.3.2 Results

Table 1.6 shows the results of the within OLS and IV estimations of equation 1.4 for the Penn World Table Data. Column 1 displays the results of the OLS estimation. Columns 2 to 5 include the estimates of the IV estimation for both GMM and 2SLS procedures. The specification of column 2 includes all instruments, and columns 3 to 5 each exclude one instrument. The exclusion of instruments is motivated by the tradeoff between the overidentification test, which requires a large number of instruments, and the weak identification test, which prefers fewer but stronger instruments. All specifications pass the underidentification test, which indicates that our instruments are indeed correlated with the instrumented variable. The specifications of columns 3 and 5 both yield Kleibergen-Paap F-statistics that are above the 15% Stock-Yogo statistics, which suggests that the set of instruments in these two specifications is sufficiently correlated with the instrumented variable, i.e., the number of rights. No specification rejects the null hypothesis of the overidentification test, which indicates that our instruments satisfy the exclusion restriction. Finally, and more surprising, our estimations fail at rejecting the null hypothesis of the endogeneity test, which suggests that the IV estimates are statistically not different from the OLS estimates. Table 1.7 displays the same estimates for the World Bank Data. Table 1.8 and 1.9 display the estimated coefficients associated with the instruments of the first stage IV within estimations of tables 1.6 and 1.7.

Results displayed in tables 1.6 and 1.7 are similar and fail at detecting any impact of the number of rights on government size. None of the coefficients associated to $RIGHTS$ is found to be significant. The 2SLS and GMM estimates are very close and conclude in the same way. Moreover, the fact that the PWT and the WB datasets yield estimates with opposite signs seems to confirm the null hypothesis, i.e., that the number of rights does not have any impact on government size.

Differentiation By Democratic Situation. Past works on international human rights treaties have determined that the marginal impact of human rights treaties on the human rights situation was conditional based on the level of de facto democracy in the ratifying countries. We investigate the relevance of such theories in the case of constitutional rights. To do so, we compute the conditional impact of constitutional rights with respect to the level of democracy in the country under consideration. We start by computing country i's average level of *de facto* democracy in the three past years ($AVPOL$). We then

[21] A specification passes the weak identification test if the corrected F-statistics is higher than the reported Stock- Yogo statistics.

multiply this variable with $RIGHTS$ to obtain an interaction variable INT_RIGHTS. Third, we multiply our instruments with $AVPOL$ to obtain interaction instruments (see Balli & Sørensen (2013)).

The results of this new set of specifications are displayed in tables 1.10 and 1.11. Because of the duplication of our instruments, the F-statistics associated with the weak identification test decreased in the specification including all instruments. Columns 6 and 7 reduce the number of instruments, as to maximize the F-statistics and to decrease the Stock-Yogo requirements. The specification of column 7 appears to be the most suitable specification.

Figures 1.6 and 1.7 propose graphical interpretations of these results. For both figures, the upper graphs correspond to the marginal effect when using 2SLS techniques, while the two lower graphs display the marginal effect of constitutional rights when estimated with GMM techniques. The graphs on the left are associated with the three-instrument specification (column 6 of the above table), while the graphs on the right show the results for the two-instrument specification (column 7).

Here again, the results of the different specifications yield the same conclusion: the impact of constitutional rights is not significantly different from zero, regardless of the democratic situation of the country.[22] In addition, all specifications depict a positive impact of the democratic level on the marginal effect of the number of constitutional rights; however, this increase is statistically significant in only a few specifications (specification 5 of table 1.10, specifications 2, 3, 4 and 5 of table 1.11).

Robustness Check: Focus on More Demanding Rights. Focus on More Demanding Rights The above estimation concludes that the number of constitutional rights does not have a significant effect on the size of government. Several explanations may account for these findings. More particularly, one potential concern could result from the scope of rights considered by our $RIGHTS$ variable. One could argue, in fact, that our independent variable of interest includes too many dimensions, which might even have opposite effects on government size. Indeed, although it is quite straightforward that the enforcement of positive rights necessitates a greater involvement of the state, the picture is more complex regarding negative rights. We therefore consider a second indicator, denoted SUB_RIGHTS, which considers only a *subset* of rights included in the $RIGHTS$ variable, i.e., *positive* rights only.[23]

The estimation of the previous specification with this new variable yields similar results: the number of constitutional rights does not increase the size of the government. Previous comments on the quality and relevance of our IV estimates also hold for this new estimation. Tables 1.12 and 1.13 display the results of this new series of estimations.

[22]The marginal effect is not statistically significant at the 90% confidence level either.

[23]The list of constitutional rights in our dataset is displayed in table 1.2 in the appendix. Variable SUB_RIGHTS includes: asylum, conright; couns, cultrght, falseimp, healthf, healthr, provhlth, provwork, remuner, safework, rghtapp, shelter, speedtri, standliv trilang.

1.5 Conclusion and Discussion

This article aimed to investigate the potential impact of constitutional rights on the level of public expenditures. To do so, we used a panel dataset, which includes information about constitutional rights for constitutional systems since 1960. We measured the relationship between constitutional rights and the relative size of the state. We used the share of the government in terms of total GDP as a proxy for the size of the government. First, we investigated which countries were the most likely to have changed their number of constitutional rights since 1960. We found some evidence for a catch-up effect: countries that had fewer rights in 1960 were the most likely to change their number of rights during the subsequent fifty years. Moreover, we also found that countries that improved their democratic situation were also the most likely to have changed their number of constitutional rights. Second, we analyzed whether the size of government influenced the *probability* that a state changes its number of constitutional rights and whether it affects the *number* of constitutional rights the state decides to put in the constitution. We found that (the lag value of) the size of government does not affect the probability of changing the number of rights, but negatively affects the number of constitutional rights. Considering this feedback effect, we used an instrumental variable approach to estimate the impact of the number of constitutional rights on government size. To do so, we used a set of three instruments and estimated the causal impact with both GMM and 2SLS methods. We compared our results using two dependent variables.

Our estimations did not detect any causal effect of the number of constitutional rights on government size. Indeed, we did not find any evidence supporting the theory that more constitutional rights induce a stronger commitment by the state, leading to a stronger intervention in the economy. The lack of significance of the coefficient associated with the number of rights was robust to all specifications and to both sources of data. To investigate the lack of significance, we allowed for a conditional causal impact according to the level of de facto democracy. We did not find any evidence of an overall positive impact of constitutional rights on government size. At best, we found some fragile evidence suggesting that constitutional rights are more likely to induce larger governments in democratic countries. Finally, we reduced the number of rights considered in our analysis, to focus exclusively on those that are the most likely to induce a greater intervention by the state. Here again, we failed at detecting an impact of constitutional rights on the level of public expenditure.

Several factors may account for the lack of evidence supporting a positive causal impact of the number of constitutional rights on government size. First, it might be that countries inscribe rights in their constitutions when they already provide these rights, e.g., through statutory laws. The inscription of rights in the constitution would only aim at insulating existing policies. However, this alternative explanation would hardly account for the negative impact of government size on the number of constitutional rights. Second, it might also be the case that constitutions mainly serve signaling purposes: countries do

not necessary seek to enforce constitutional rights, but decide to inscribe them in their constitution for other purposes. For instance, autocratic countries might try to mitigate the international pressure regarding human rights violations by changing their constitutions.

The lack of significance might also be due to technical reasons. It might be that our dependent variable ($SIZE$) is not appropriate to capture the effect of constitutional rights. The discussion in section 1.4.3 has shown that the final consumption of outsourcing governments may not be affected by an increase of constitutional rights due to the private enforcement of these rights. Considering the government's budget rather than its final consumption might help to resolve this issue. The scarcity of data regarding the government's budget, however, would raise other methodological issues.

Appendix 1.A: Figures

Figure 1.1: Number of Constitutional Changes since 1950 (both new constitutions and amendments).

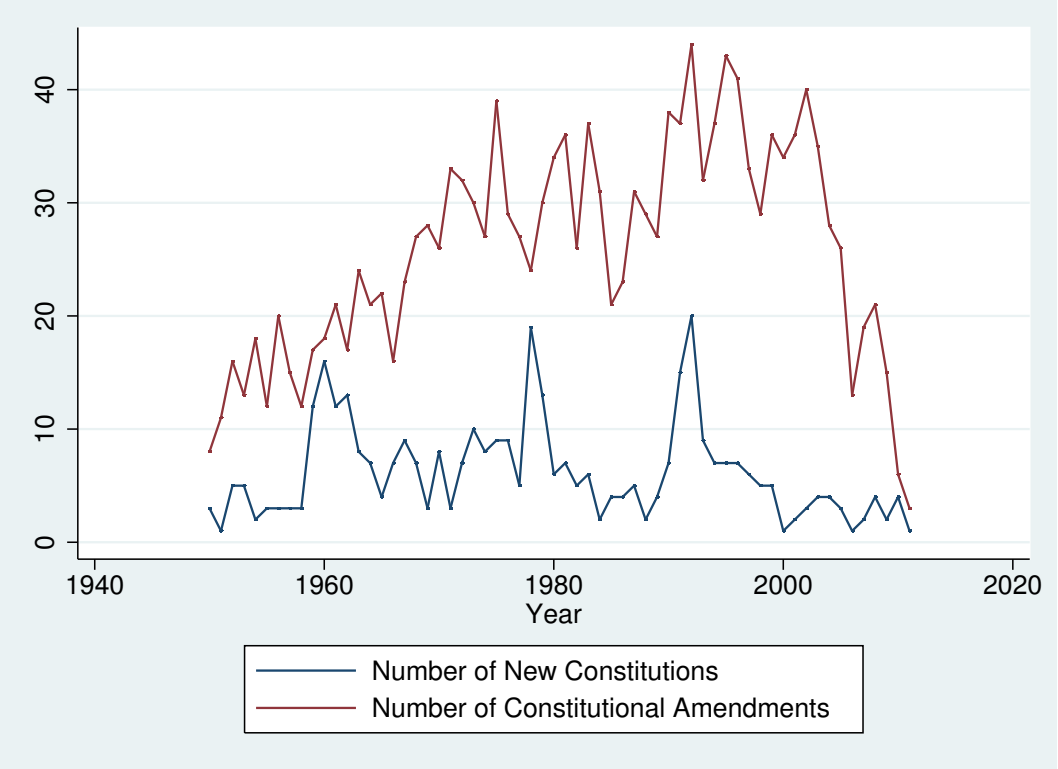


Figure 1.2: Mean of Constitutional Rights for *Full*, *Variation* and *Data* samples since 1960: PWT Data.

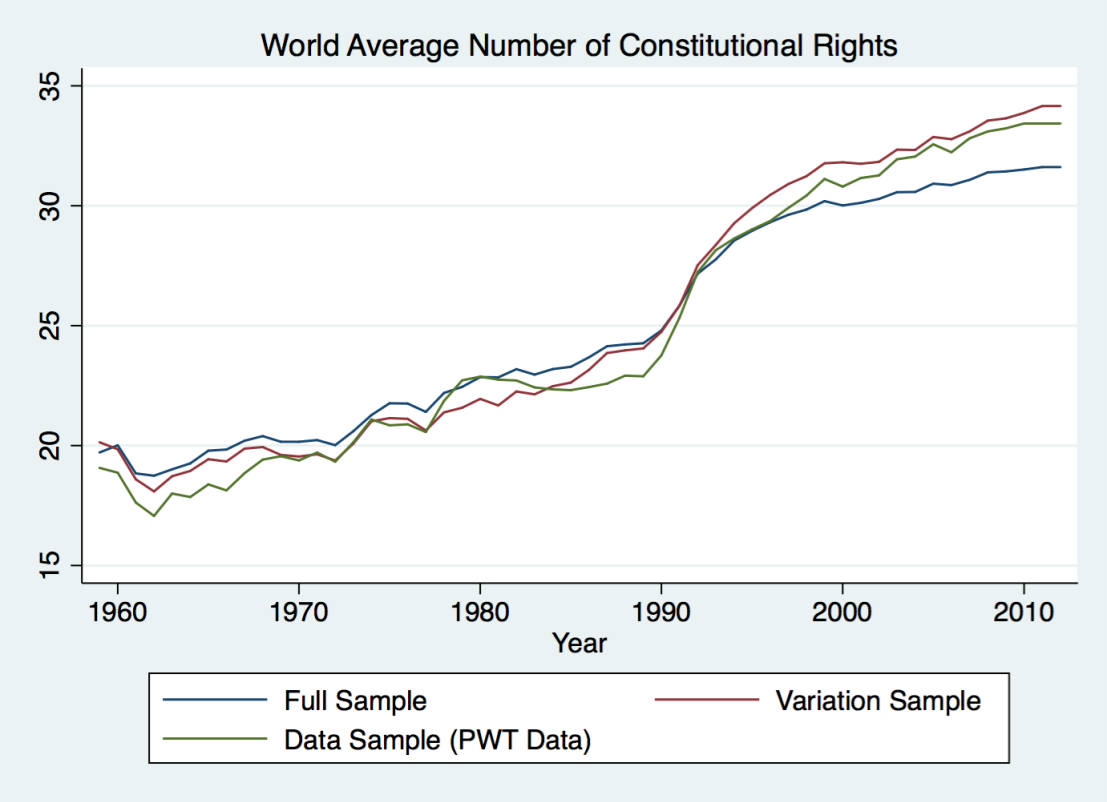


Figure 1.3: Mean of Constitutional Rights for *Full*, *Variation* and *Data* samples since 1960: World Bank Data.

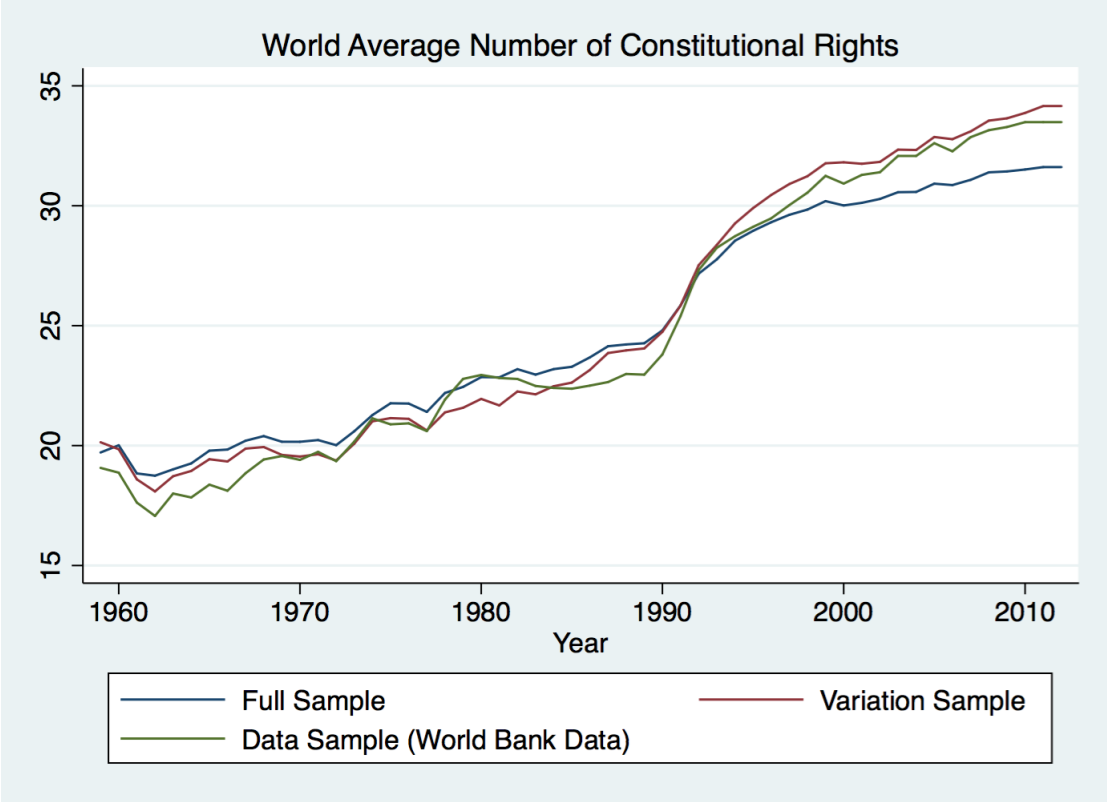
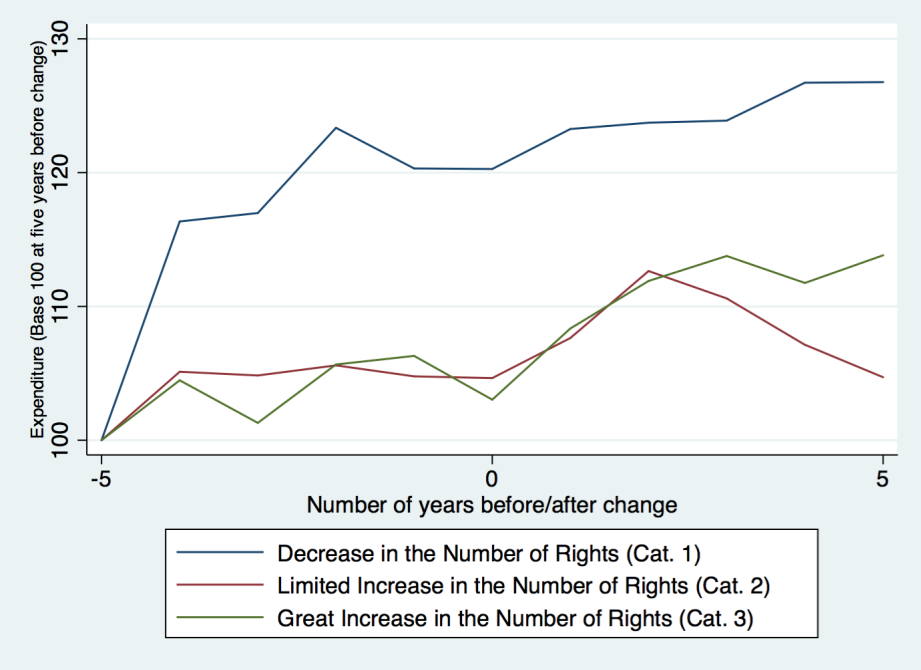
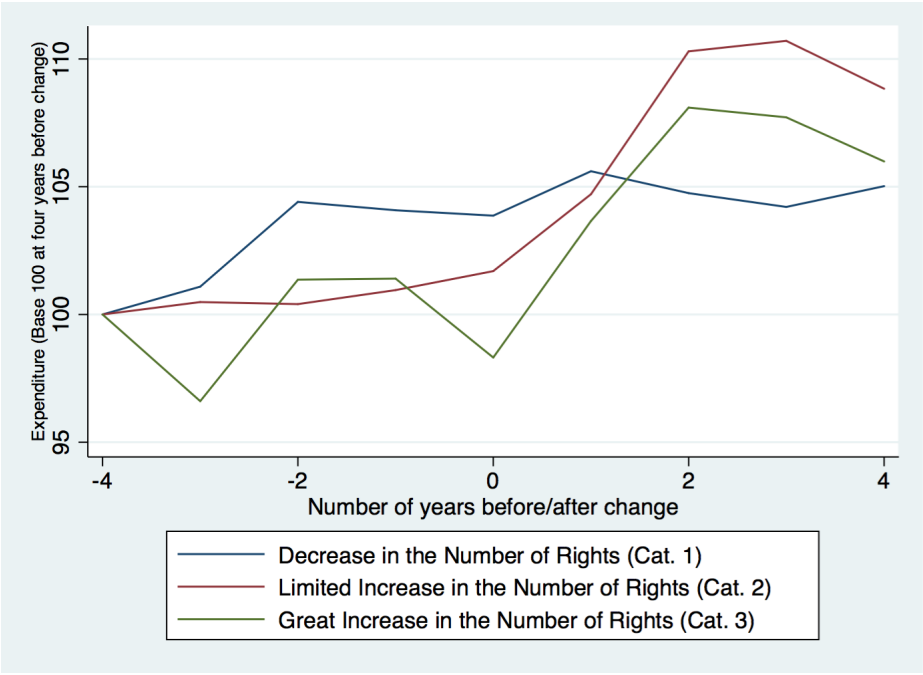


Figure 1.4: Evolution of the size of government **5 years before and 5 years after** a constitutional change according to the evolution of constitutional rights.



Notes: Constitutional modifications without changes in the number of constitutional rights are excluded.

Figure 1.5: Evolution of the size of government **4 years before and 4 years after** a constitutional change according to the evolution of constitutional rights.



Notes: Constitutional modifications without changes in the number of constitutional rights are excluded.

Figure 1.6: Marginal Effect of Constitutional Rights on Government Size (PWT Data).

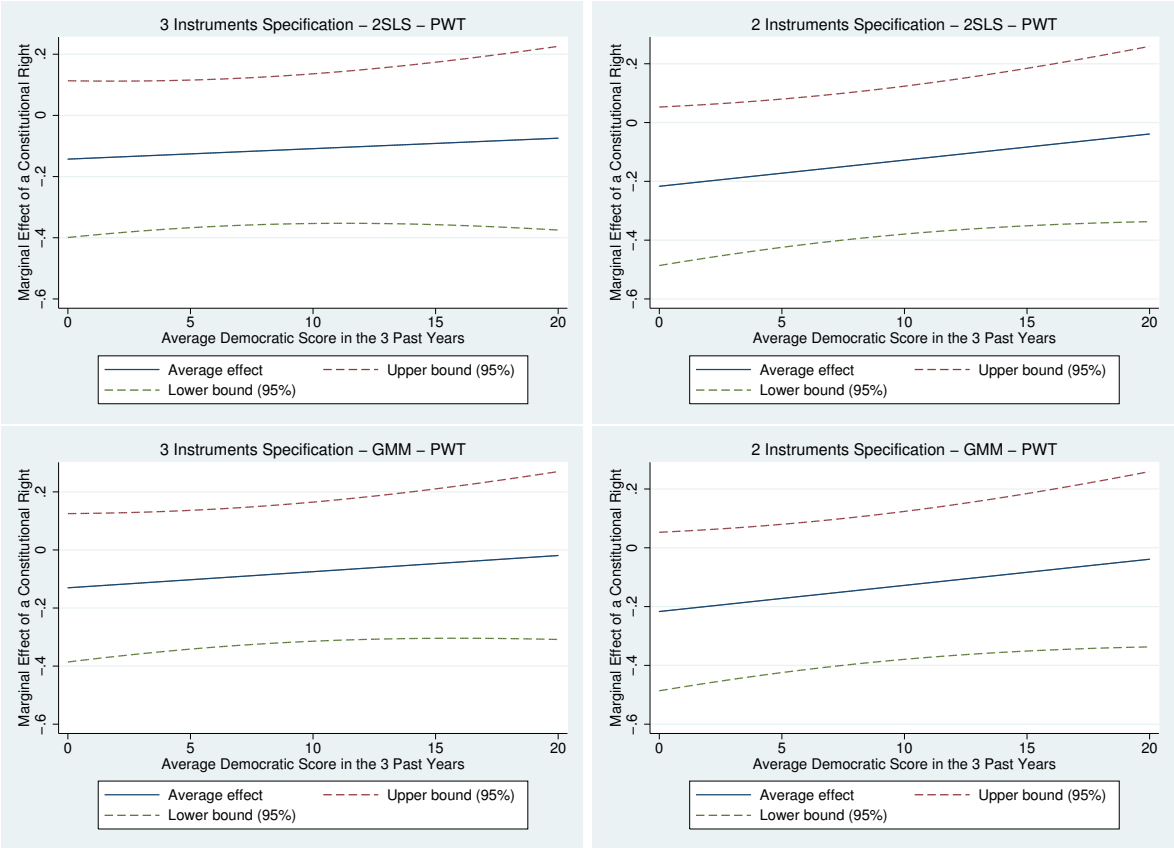
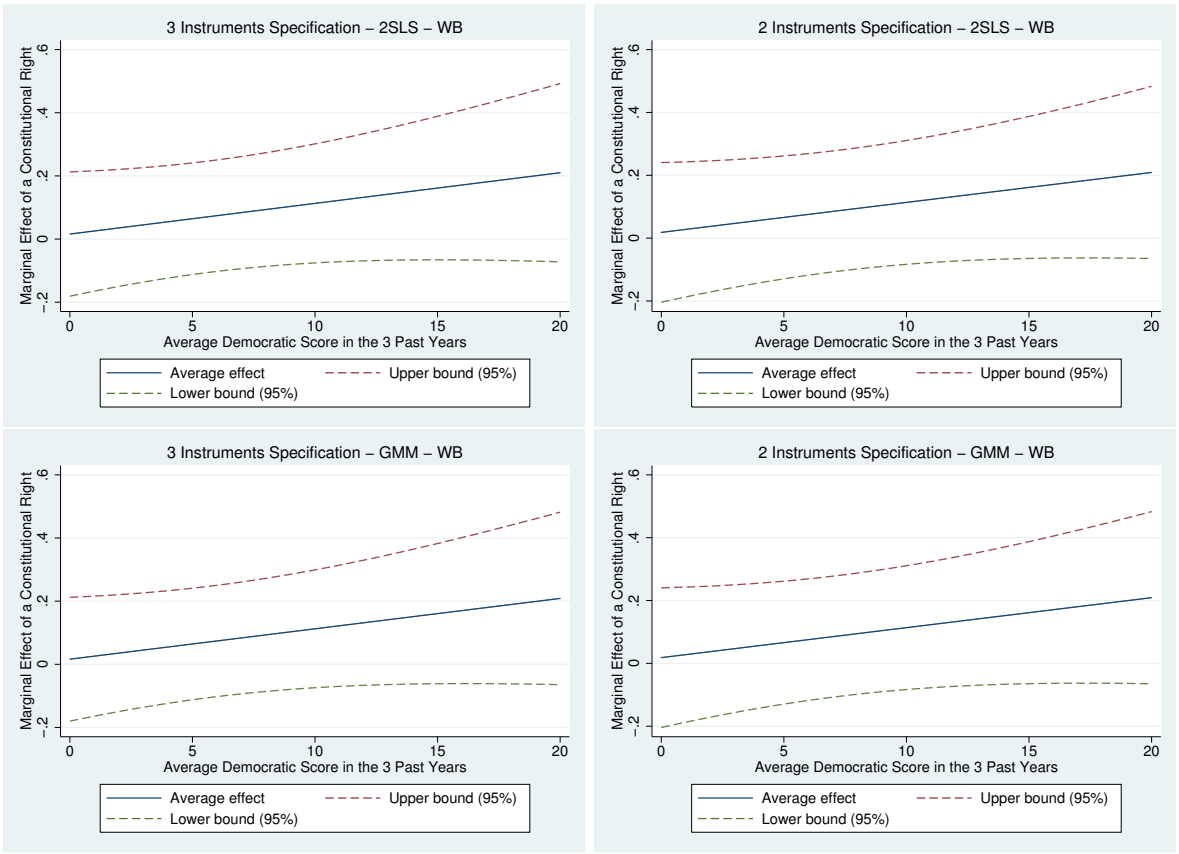


Figure 1.7: Marginal Effect of Constitutional Rights on Government Size (World Bank Data).



Appendix 1.B: Tables

Variable	% in 1960	% in 2012	Evolution
ARMS: The right to bear arms	.058	.017	-.041
ASSEM: Freedom of assembly	.756	.928	.172
ASSOC: Freedom of association	.733	.922	.189
ASYLUM: Provisions for the protection of stateless persons	.326	.45	.124
BUSINES: The right to conduct/establish a business	.14	.378	.238
CAPPUN: Restrictions on the use of capital punishment	.198	.328	.13
CENSOR: Prohibition of censorship	.256	.311	.055
CHILDPRO: Guarantee of the rights of children	.209	.406	.197
CHILDWRK: Limits on child employment	.221	.322	.101
CITDEP: Grant to the government of the right to deport citizens	.291	.572	.281
CITREN: The right of citizens to renounce their citizenship	.047	.261	.214
CITSUFF_1: Jus soli citizenship	.306	.331	.025
CONRIGHT: Mention of consumer rights or consumer protection protection	.047	.233	.186
CORPPUN: Prohibition on the use of corporal punishment	.047	.033	-.014
COUNS: The right to counsel if one is indicted	.291	.728	.437
CRUELTY: Prohibition of cruel, inhuman, or degrading treatment	.291	.778	.487
CULTRGHT: Reference to a state duty to protect or promote culture	.314	.617	.303
DEBTORS: Prohibition on the detention of debtors	.163	.139	-.024
DEVLPERS: Provision for an individual's right to self development	.047	.15	.103
DOUBJEP: Prohibition of double jeopardy	.209	.511	.302
EXAMWIT: The right to examine evidence or confront all	.116	.317	.201
EXPOST: Prohibition of punishment by laws enacted ex post	.477	.8	.323
EXPRESS: Freedom of expression or speech	.756	.939	.183
FAIRTRI: The right to a fair trial	.128	.5	.372

Variable	% in 1960	% in 2012	Evolution
FALSEIMP: The right to some redress in cases of false imprisonment	.209	.339	.13
FNDFAM: The right to found a family	.163	.306	.143
FREECOMP: The right to a free and/or competitive market	.093	.211	.118
FREEMOVE: Freedom of movement	.465	.85	.385
FREEREL: Freedom of religion	.849	.922	.073
HABCORP: The right to protection from unjustified restraint	.651	.761	.11
HEALTHF: Specification that healthcare should be provided at state expense	.145	.238	.093
HEALTHR: Mention of the right to health care	.221	.489	.268
INFOACC: Right of an individual to view government information	.081	.317	.236
INHERIT: Inheritance rights	.209	.294	.085
INTPROP: Intellectual property rights	.25	.231	-.019
JOINTRDE: The right to form or to join trade unions	.477	.739	.262
JURY: Requirement of a jury or any form of citizen participation in criminal trials	.267	.172	-.095
JUVENILE: Provision for juveniles of special rights/status in criminal process	.035	.139	.104
LEISURE: A right of rest and leisure	.349	.4	.051
LIBEL: The right of protection of one's reputation	.163	.289	.126
LIFE: A right to life	.384	.75	.366
MARRIAGE: The right to marry	.105	.089	-.016
MATEQUAL: Provision for matrimonial equality	.174	.322	.148
MIRANDA: A right of the accused to silence or protection from self-incrimination	.256	.517	.261
NOMIL: A right to exemption from military service for conscientious objectors	.07	.2	.13
OCCUPATE: The right to choose one's occupation	.337	.55	.213
OPINION: Freedom of opinion, thought, and conscience	.512	.806	.294
PETITION: A right of petition	.616	.55	-.066
PREREL: The right/possibility of pretrial release	.221	.294	.073
PRESINOC: A presumption of innocence in trials	.233	.733	.5
PRESS: Freedom of the press	.535	.633	.098
PRISONRG: Requirement that the names of those imprisoned be recorded in a register	.023	.022	-.001

Variable	% in 1960	% in 2012	Evolution
PRIVACY: A right of privacy	.64	.839	.199
PROPRGHT: A right to own property	.593	.844	.251
PROVHLTH: Mention of a state duty to provide health care	.256	.428	.172
PROVWORK: Mention of a state duty to provide work employment	.186	.128	-.058
PUBTRI: A general requirement of public trials	.453	.656	.203
REMUNER: The right to just remuneration, fair compensation, etc.	.349	.472	.123
RGHTAPP: The right of defendants to appeal judicial decisions	.128	.306	.178
SAFEWORK: The right to safe/healthy working environment	.198	.372	.174
SAMESEXM: The right for same-sex marriages	0	0	0
SCIFREE: A right to enjoy the benefits of science	.058	.117	.059
SELFDET: A people's right of self determination	.058	.144	.086
SEPREL: An explicit decree of separation of church and state	.279	.261	-.018
SHELTER: The right to shelter or housing	.093	.267	.174
SLAVE: Prohibition of slavery, servitude, or forced labor	.279	.539	.26
SPEEDTRI: The right to a speedy trial	.186	.456	.27
STANDLIV: A right to an adequate or reasonable standard of living	.186	.211	.025
STRIKE: A right to strike	.244	.444	.2
TESTATE: A right of testacy, or the right to leave property to one's heirs	.093	.078	-.015
TORTURE: Prohibition of torture	.326	.739	.413
TRANSFER: The right to transfer property freely	.163	.167	.004
TRILANG: Specification that a trial has to be in a language of the accused	.128	.372	.244
WOLAW: Mention of nulla poena sine lege or equivalent	.535	.639	.104

Table 1.2: Percentage of constitutions including each right in 1960 and 2012, and the evolution between 1960 and 2012.

Table 1.3: Descriptive Statistics

	CH_i			
	PWT		World Bank	
	Mean	Standard Deviation	Mean	Standard Deviation
$SIZE$	14.715	5.84	14.622	5.477
$RIGHTS$	25.807	13.373	26.293	13.664
DUR	19.036	24.076	19.473	24.831
$LOPOL$	23.11	8.269	23.503	8.334
$ICCPR$	.586	.493	.606	.489
$LGDP$	8.118	1.092	8.16	1.1
$LPOP$	2.338	1.459	2.38	1.472
$OPEN$	.402	.351	.417	.341
POL	10.635	7.262	11.37	7.117
HC	2.016	.588	2.039	.593
$POP14$	36.6	9.905	36.203	10.036
$POP65$	5.744	4.101	5.871	4.22

The descriptive statistics correspond to the samples of the OLS estimations of tables 1.6 and 1.7.

Table 1.4: Constitutional Change since 1960: Probit Estimates

	CH_i					
	Average Values		First Values		Lastest Values	
	PWT	WB	PWT	WB	PWT	WB
	(1)	(2)	(3)	(4)	(5)	(6)
$SIZE$	-0.0169	-0.0183	0.011	0.0039	-0.00088	-0.0093
	(-0.53)	(-0.58)	(0.42)	(0.20)	(-0.04)	(-0.36)
$RIGHTS$	-0.021	-0.021	-0.029**	-0.030**	0.012	0.012
	(-1.46)	(-1.46)	(-2.38)	(-2.45)	(0.91)	(0.89)
DUR	-0.0047	-0.0047	0.0024	0.0022	-0.0067	-0.0065
	(-0.90)	(-0.91)	(0.53)	(0.51)	(-1.27)	(-1.21)
$ICCPR$	-0.048	-0.084	.	.	0.921	0.889
	(-0.07)	(-0.13)	.	.	(1.27)	(1.23)
$LGDP$	-0.070	-0.085	-0.042	-0.050	0.044	0.033
	(-0.31)	(-0.37)	(-0.21)	(-0.24)	(0.18)	(0.14)
$LPOP$	0.070	0.067	0.051	0.050	0.199**	0.191**
	(0.70)	(0.68)	(0.55)	(0.53)	(2.13)	(2.04)
$OPEN$	-0.864*	-0.850*	-0.921*	-0.876*	-0.219	-0.215
	(-1.72)	(-1.72)	(-1.80)	(-1.73)	(-1.54)	(-1.61)
POL	-0.015	-0.013	-0.050**	-0.051**	-0.010	-0.0095
	(-0.47)	(-0.41)	(-2.20)	(-2.21)	(-0.34)	(-0.31)
HC	0.599	0.605	0.350	0.385	0.433	0.446
	(1.12)	(1.14)	(0.84)	(0.91)	(0.87)	(0.90)
$POP14$	0.053	0.050	0.096*	0.098*	0.023	0.022
	(0.89)	(0.84)	(1.93)	(1.95)	(0.64)	(0.64)
$POP65$	0.063	0.060	0.163	0.170	-0.027	-0.023
	(0.56)	(0.53)	(1.17)	(1.21)	(-0.51)	(-0.44)
# obs.	114	113	114	113	114	113
Pseudo R^2	0.125	0.123	0.186	0.183	0.142	0.140
Log Likelihood	-63.475	-63.293	-59.080	-58.953	-62.261	-62.053

Z-values in parentheses. Robust standard errors. $ICCPR$ is not included in the second specification, since no country ratified the ICCPR in 1960.

* Significant at the 10-percent level.

** Significant at the 5-percent level.

*** Significant at the 1-percent level.

Table 1.5: Number of Rights: OLS and Heckman estimates

Technique	OLS	OLS	OLS	Heckman	
Dependent variable	$RIGHTS$	$RIGHTS$	$RIGHTS$	$RIGHTS$	CH
	(1)	(2)	(3)	(4)	(5)
$SIZE_{t-1}$	-0.362***	-0.266***	-0.397***	-0.453**	-0.0104
	(-12.45)	(-4.860)	(-3.189)	(-2.435)	(-1.589)
DUR_{t-1}	-0.111***	-0.0715***	-0.00622	-0.00611	-0.00165
	(-16.37)	(-5.728)	(-0.174)	(-0.147)	(-1.295)
$ICCPR_{t-1}$	2.038***	2.545***	0.457	-0.0505	-0.0102
	(4.016)	(2.592)	(0.201)	(-0.0169)	(-0.101)
$LGDP_{t-1}$	2.787***	5.576***	4.846***	4.175**	0.0252
	(8.987)	(8.811)	(3.423)	(2.505)	(0.447)
$LPOP_{t-1}$	0.654***	0.682**	-0.978	0.248	0.0740***
	(4.688)	(2.451)	(-1.519)	(0.299)	(2.766)
$OPEN_{t-1}$	-2.065***	-3.761***	-8.244**	-5.863*	0.157
	(-3.713)	(-2.855)	(-2.547)	(-1.799)	(1.441)
POL_{t-1}	0.494***	0.132**	0.198	0.280	0.00739
	(16.76)	(2.048)	(1.444)	(1.431)	(1.136)
$POP14_{t-1}$	0.0137	0.116	0.0451	-0.109	-0.00502
	(0.259)	(1.087)	(0.154)	(-0.333)	(-0.481)
$POP65_{t-1}$	-0.529***	-1.241***	-0.965	-1.297*	-0.0171
	(-4.120)	(-4.796)	(-1.288)	(-1.779)	(-0.726)
$TREND_t$	0.278***	0.422***	0.459***	0.377***	-0.00439
	(14.73)	(10.53)	(5.019)	(3.218)	(-1.198)
$PRESS_{t-1}$					0.0127***
					(4.90)
# obs.	3,564	904	177	3,511	
Pseudo R^2	0.337	0.362	0.352		
Log Likelihood				-1370.671	
Lambda				20.949	(3.383)
Rho				0.928	(0.0310)
LR $\rho = 0$ (p-value)				0.000	

Z-values in parentheses, except for Lambda and Rho (standard errors). Robust standard errors. HC is not included because convergence of the Heckman estimation was not obtained when included. OLS estimates were similar when included.

* Significant at the 10-percent level.

** Significant at the 5-percent level.

*** Significant at the 1-percent level.

Table 1.6: Estimation of the impact of the number of constitutional rights on the Government Size (Penn World Table Data).

Specification	(1)	(2)	(3)	(4)	(5)
PWT data	OLS	IV	IV	IV	IV
RIGHTS	-0.00902				
	(0.727)				
RIGHTS (GMM)		-0.0767	-0.0613	0.149	-0.118
		(0.524)	(0.620)	(0.444)	(0.440)
RIGHTS (2SLS)		-0.0223	-0.0943	0.286	-0.00498
		(0.872)	(0.456)	(0.225)	(0.976)
Observations					
Number of countries	73	73	73	73	73
Country FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
CH_WAR	.	Yes	No	Yes	Yes
CH_LOPOL	.	Yes	Yes	Yes	No
DRAFTER	.	Yes	Yes	No	Yes
Underident. Test p-value	.	Reject (1%)	Reject (1%)	Reject (1%)	Reject (1%)
Overident. test p-value	.	0.202	0.216	0.304	0.0835
Endogeneity Test	.	Don't Reject	Don't Reject	Don't Reject	Don't Reject
Weak Ident. F-Test	.	10.36	15.37	4.932	14.92
Stock-Yogo 10%	.	22.30	19.93	19.93	19.93
Stock-Yogo 15%	.	12.83	11.59	11.59	11.59
Stock-Yogo 20%	.	9.54	8.75	8.75	8.75
Stock-Yogo 25%	.	7.80	7.25	7.25	7.25

Within estimation of *SIZE*. Robust and clustered standard errors. P-values in parentheses. Control variables include: LGDP, LPOP, OPEN, HC, POP14, POP65, POL, ICCPR, DUR, LOPOL.
* Significant at the 10-percent level.
** Significant at the 5-percent level.
*** Significant at the 1-percent level.

Table 1.7: Estimation of the impact of the number of constitutional rights on the Government Size (World Bank Data).

Specification	(1)	(2)	(3)	(4)	(5)
World Bank data	OLS	IV	IV	IV	IV
RIGHTS	-0.000363				
	(0.989)				
RIGHTS (GMM)		0.0665	0.0864	0.0196	0.0798
		(0.490)	(0.376)	(0.872)	(0.504)
RIGHTS (2SLS)		0.0991	0.0782	0.120	0.113
		(0.358)	(0.439)	(0.402)	(0.372)
Observations	1,986	1,651	1,654	1,851	1,651
Number of countries	72	68	68	68	68
Country FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
CH_WAR	.	Yes	No	Yes	Yes
CH_LOPOL	.	Yes	Yes	Yes	No
DRAFTER	.	Yes	Yes	No	Yes
Underident. Test p-value	.	Reject (1%)	Reject (1%)	Reject (1%)	Reject (1%)
Overident. test p-value	.	0.710	0.749	0.189	0.426
Endogeneity Test	.	Don't Reject	Don't Reject	Don't Reject	Don't Reject
Weak Ident. F-Test	.	9.451	14.04	5.118	12.66
Stock-Yogo 10%	.	22.30	19.93	19.93	19.93
Stock-Yogo 15%	.	12.83	11.59	11.59	11.59
Stock-Yogo 20%	.	9.54	8.75	8.75	8.75
Stock-Yogo 25%	.	7.80	7.25	7.25	7.25

Within estimation of *SIZE*. Robust and clustered standard errors. P-values in parentheses. Control variables include: LGDP, LPOP, OPEN, HC, POP14, POP65, POL, ICCPR, DUR, LOPOL.
* Significant at the 10-percent level.
** Significant at the 5-percent level.
*** Significant at the 1-percent level.

Table 1.8: First Stage Estimates of table 1.6.

Specification	(2)	(3)	(4)	(5)
CH_WAR	0.0672		0.0807	.01073
	(0.470)	.	(0.265)	(0.177)
CH_LOPOL	0.2027	0.2388**	0.2044*	
	(0.129)	(0.025)	(0.083)	.
DRAFTER	6.170***	5.982***		6.187***
	(0.000)	(0.000)	.	(0.000)
Partial-R^2	0.0926	0.0855	0.0272	0.0813

First stage estimates of the IV within estimation of *SIZE*. Robust and clustered standard errors. P-values in parentheses. Control variables include: LGDP, LPOP, OPEN, HC, POP14, POP65, POL, ICCPR, DUR, LOPOL.
* Significant at the 10-percent level.
** Significant at the 5-percent level.
*** Significant at the 1-percent level.

Table 1.9: First Stage Estimates of table 1.7.

Specification	(2)	(3)	(4)	(5)
CH_WAR	0.0816		0.0959	0.1246
	(0.433)	.	(0.231)	(0.193)
CH_LOPOL	0.2782*	0.3055**	0.3176**	
	(0.083)	(0.028)	(0.038)	.
DRAFTER	6.564***	6.346***		6.687***
	(0.000)	(0.000)	.	(0.000)
Partial-R^2	0.1038	0.0937	0.0397	0.0856

First stage estimates of the IV within estimation of *SIZE*. Robust and clustered standard errors. P-values in parentheses. Control variables include: LGDP, LPOP, OPEN, HC, POP14, POP65, POL, ICCPR, DUR, LOPOL.
* Significant at the 10-percent level.
** Significant at the 5-percent level.
*** Significant at the 1-percent level.

Table 1.10: Estimation of the impact of the number of constitutional rights on the Government Size (Penn World Table Data).

Specification	(1)	(2)	(3)	(4)	(5)	(6)	(7)
PWT data	OLS	IV	IV	IV	IV	IV	IV
RIGHTS	-0.0547						
	(0.168)						
INT_RIGHTS	0.00482						
	(0.109)						
RIGHTS (GMM)		-0.228**	-0.164	0.177	-0.187	-0.130	-0.217
		(0.0305)	(0.142)	(0.446)	(0.189)	(0.317)	(0.115)
INT_RIGHTS		0.00529	0.00695	-0.00194	0.0181*	0.00556	0.00889
		(0.378)	(0.265)	(0.841)	(0.0767)	(0.404)	(0.187)
RIGHTS (2SLS)		-0.0903	-0.174	0.256	-0.0549	-0.143	-0.217
		(0.519)	(0.123)	(0.414)	(0.734)	(0.274)	(0.115)
INT_RIGHTS		0.00448	0.00581	-0.00299	0.00818	0.00342	0.00889
		(0.512)	(0.396)	(0.775)	(0.486)	(0.618)	(0.187)
Observations	2,152	1,889	1,892	2,149	1,889	1,892	1,892
# countries	73	72	72	72	72	72	72
Country FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
CH_WAR	.	Yes	Yes	Yes	No	No	No
inter_CH_WAR		Yes	Yes	Yes	No	No	No
CH_LOPOL	.	Yes	Yes	No	Yes	Yes	No
inter_CH_LOPOL		Yes	Yes	No	Yes	Yes	Yes
DRAFTER	.	Yes	No	Yes	Yes	Yes	Yes
inter_DRAFTER		Yes	No	Yes	Yes	No	No
Underidentification	.	Rej. (1%)	Rej. (1%)	Rej. (5%)	Rej. (5%)	Rej. (1%)	Reject (1%)
Overidentification	.	0.230	0.364	0.395	0.228	0.182	.
Weak Ident. F-Test	.	3.972	6.030	2.447	2.254	8.019	11.60
Stock-Yogo 10%	.	21.68	16.87	16.87	16.87	13.43	7.03
Stock-Yogo 15%	.	12.33	9.93	9.93	9.93	8.18	4.58
Stock-Yogo 20%	.	9.10	7.54	7.54	7.54	6.40	3.95
Stock-Yogo 25%	.	7.42	6.28	6.28	6.28	5.45	3.63

Within estimation of *SIZE*. Robust and clustered standard errors. P-values in parentheses. Control variables include: LGDP, LPOP, OPEN, HC, POP14, POP65, POL, ICCPR, DUR, LOPOL.

* Significant at the 10-percent level.

** Significant at the 5-percent level.

*** Significant at the 1-percent level.

Table 1.11: Estimation of the impact of the number of constitutional rights on the Government Size (World Bank Data).

Specification	(1)	(2)	(3)	(4)	(5)	(6)	(7)
WB data	OLS	IV	IV	IV	IV	IV	IV
RIGHTS	-0.0890**						
	(0.0427)						
INT_RIGHTS	0.00893**						
	(0.0139)						
RIGHTS (GMM)		.	-0.0285	-0.125	-0.0126	0.0162	0.0183
		.	(0.744)	(0.455)	(0.897)	(0.871)	(0.872)
INT_RIGHTS		.	0.0143**	0.0187*	0.0213**	0.00962	0.00954
		.	(0.0199)	(0.0848)	(0.0268)	(0.201)	(0.222)
RIGHTS (2SLS)		0.00567	-0.0444	0.0566	0.0219	0.0158	0.0183
		(0.960)	(0.616)	(0.784)	(0.847)	(0.875)	(0.872)
INT_RIGHTS		0.0132*	0.0128*	0.0142	0.0133	0.00971	0.00954
		(0.0562)	(0.0557)	(0.209)	(0.255)	(0.216)	(0.222)
Observations	2,152	1,889	1,892	2,149	1,889	1,892	1,892
# countries	73	72	72	72	72	72	72
Country FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
CH_WAR	.	Yes	Yes	Yes	No	No	No
inter_CH_WAR		Yes	Yes	Yes	No	No	No
CH_LOPOL	.	Yes	Yes	No	Yes	Yes	No
inter_CH_LOPOL		Yes	Yes	No	Yes	Yes	Yes
DRAFTER	.	Yes	No	Yes	Yes	Yes	Yes
inter_DRAFTER		Yes	No	Yes	Yes	No	No
Underidentification	.	Rej. (5%)	Rej. (1%)	Rej. (5%)	Rej. (10%)	Rej. (1%)	Rej. (1%)
Overidentification	.	.	0.504	0.265	0.403	0.968	.
Weak Ident. F-Test	.	3.270	4.938	2.662	2.067	6.712	13.13
Stock-Yogo 10%	.	21.68	16.87	16.87	16.87	13.43	7.03
Stock-Yogo 15%	.	12.33	9.93	9.93	9.93	8.18	4.58
Stock-Yogo 20%	.	9.10	7.54	7.54	7.54	6.40	3.95
Stock-Yogo 25%	.	7.42	6.28	6.28	6.28	5.45	3.63

Within estimation of *SIZE*. Robust and clustered standard errors. P-values in parentheses. Control variables include: LGDP, LPOP, OPEN, HC, POP14, POP65, POL, ICCPR, DUR, LOPOL.

* Significant at the 10-percent level.

** Significant at the 5-percent level.

*** Significant at the 1-percent level.

Table 1.12: Robustness Check: Estimation of the impact of the number of constitutional rights on the Government Size (Penn World Table Data).

Specification	(1)	(2)	(3)	(4)	(5)
PWT data	OLS	IV	IV	IV	IV
SUB_RIGHTS	0.151				
	(0.200)				
SUB_RIGHTS (GMM)		-0.291	-0.205	0.299	-0.439
		(0.398)	(0.572)	(0.504)	(0.322)
SUB_RIGHTS (2SLS)		-0.139	-0.267	0.698	-0.0964
		(0.723)	(0.467)	(0.199)	(0.844)
Observations	2,089	1,853	1,856	2,086	1,853
Number of countries	69	68	68	69	68
Country FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
CH_WAR	.	Yes	No	Yes	Yes
CH_LOPOL	.	Yes	Yes	Yes	No
$DRAFTER$	.	Yes	Yes	No	Yes
Underident. Test p-value	.	Reject (1%)	Reject (1%)	Reject (1%)	Reject (1%)
Weak Ident. F-Test	.	11.67	17.20	6.430	15.27
Stock-Yogo 10%	.	22.30	19.93	19.93	19.93
Stock-Yogo 15%	.	12.83	11.59	11.59	11.59
Stock-Yogo 20%	.	9.54	8.75	8.75	8.75
Stock-Yogo 25%	.	7.80	7.25	7.25	7.25
Overident. test p-value	.	0.219	0.201	0.196	0.0970
Endogeneity Test	.	Don't Reject	Don't Reject	Don't Reject	Don't Reject

Within estimation of *SIZE*. Robust and clustered standard errors. P-values in parentheses. Control variables include: LGDP, LPOP, OPEN, HC, POP14, POP65, POL, ICCPR, DUR, LOPOL.
* Significant at the 10-percent level.
** Significant at the 5-percent level.
*** Significant at the 1-percent level.

Table 1.13: Robustness Check: Estimation of the impact of the number of constitutional rights on the Government Size (World Bank Data).

Specification	(1)	(2)	(3)	(4)	(5)
World Bank data	OLS	IV	IV	IV	IV
SUB_RIGHTS	0.207*				
	(0.0728)				
SUB_RIGHTS (GMM)		.	0.333	0.292	0.213
		.	(0.269)	(0.467)	(0.552)
SUB_RIGHTS (2SLS)		0.308	0.259	0.648	0.281
		(0.342)	(0.407)	(0.177)	(0.442)
Observations	1,889	1,586	1,589	1,736	1,586
Number of countries	68	63	63	63	63
Country FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
CH_WAR	.	Yes	No	Yes	Yes
CH_LOPOL	.	Yes	Yes	Yes	No
DRAFTER	.	Yes	Yes	No	Yes
Underident. Test p-value	.	Reject (1%)	Reject (1%)	Reject (1%)	Reject (1%)
Weak Ident. F-Test	.	12.41	17.76	5.566	14.60
Stock-Yogo 10%	.	22.30	19.93	19.93	19.93
Stock-Yogo 15%	.	12.83	11.59	11.59	11.59
Stock-Yogo 20%	.	9.54	8.75	8.75	8.75
Stock-Yogo 25%	.	7.80	7.25	7.25	7.25
Overident. test p-value	.	.	0.370	0.176	0.367
Endogeneity Test	.	Don't Reject	Don't Reject	Don't Reject	Don't Reject

Within estimation of *SIZE*. Robust and clustered standard errors. P-values in parentheses. Control variables include: LGDP, LPOP, OPEN, HC, POP14, POP65, POL, ICCPR, DUR, LOPOL.
* Significant at the 10-percent level.
** Significant at the 5-percent level.
*** Significant at the 1-percent level.

2 Political Self-Serving Bias and Redistribution

This paper has been co-written with Bruno Deffains and Christian Thöni.

Abstract

In this article, we explore the impact of *self-serving biases* on the supply and demand for redistribution. We present results from an experiment in which participants vote on redistribution after performing a real effort task. Dependent on individual performance, participants are divided into two groups, successful and unsuccessful. Participants' success is exogenously determined, because they are randomly assigned to either a hard or easy task. However, because participants are not told which task they were assigned to, there is ambiguity as to whether success or failure should be attributed to internal or external factors. Our results confirm and expand previous findings on the self-serving bias: successful participants ask for less redistribution because they are more likely to attribute their success to their effort rather than luck. This result holds for both supply and demand of redistribution. The innovative contribution of our work consists in mimicking political debates, in which participants have limited information on the exogenous factors at stake. We also discuss the implications of the self-serving bias for the institutional framework.

JEL: K10, H3.

Keywords: Redistribution, self-serving bias, experimental, veil of ignorance.

2.1 Introduction

The last decades have seen resurgence and worsening of inequalities in democratic countries. Various works have documented this phenomenon (World: Atkinson (2003), Piketty & Saez (2006); US: Piketty & Saez (2003); Germany: Dustmann et al. (2009)). In this article we seek to understand the failure of the political market in developed countries

to fight the rising inequalities. Previous works in political economy have addressed this issue from different perspectives. Few works have sought to understand why voters do not ask for higher redistribution levels (public preferences, efficiency concerns, prospect of upward mobility). Other scholars have dealt with the structure of the political market to explain the persistence of inequalities (multidimensionality of the political space, low turnout). A third set of works investigated the reasons which drive representatives to limit redistribution (lobby, policy preferences). In this article, we are mainly interested in the latter aspect. In particular we study whether success reduces the supply and the demand for redistribution.

Our primary objective is to investigate how individuals who are asked to decide on the level of redistribution may be affected by their identity in an unconscious manner. Our goal is to develop the literature on redistribution by investigating the impact of psychological biases due to the very identity of the decision-maker. In other words, we propose to explore to which extent wealthy individuals (resp. non-wealthy individuals) advocate against (for) redistribution because they believe that they deserve their wealth (do not deserve their poverty).

The subsequent analysis builds on previous works on the self-serving bias (SSB). Theories about the SSB postulate that individuals are more likely to attribute their failure to situational factors than their success. In other words, the SSB claims that, when an individual succeeds at a task, she tends to congratulate herself for her efforts, while she is more prompt to blame the situation when she fails. The SSB predicts therefore a tight relationship between income and the perception of the causes of poverty: wealthier individuals are more likely to believe that they deserve their wealth. Considering the above discussion, this might have two effects on the political market. First, the self-serving bias may affect voters whenever they believe that they are successful in life: because people are not willing to recognize that their success is due to random events, they are more likely to support low tax rates. Second, the SSB might also be at play on the supply side of the political market: because politicians are elected, they can see their electoral success as the result of their efforts, which is likely, in turn, to make them believe that redistribution is unfair. In this work, we investigate both dimensions of redistribution. On the one hand, we explore how participants are affected by the SSB when they decide redistribution for other individuals, having no personal interests at stake (supply side). On the other hand, we analyze how participants' preferences toward redistribution are modified by the SSB when they must decide for a redistribution rule that will affect their unknown future own payoffs (demand side).

Recent works in experimental economics have started to explore the relationship between the SSB and the demand for redistribution. Our study goes one step further in this direction. Previous researches have reproduced games in which participants face different situations (tasks) to observe how the relative success influences their demand for redistribution. In such experiments, luck components were introduced to disturb the one-to-one relationship between effort and outcome. These works gave however full information about

the set of situations participants could possibly face, which, we believe, is far from the real political process where citizens have very limited information about their compatriots' experience.

Our research question is the following: Does belonging to the group of succeeding people affect the supply and the demand of redistribution by changing one's perception of the causal factors which determine success? Our paper concludes on two main findings. First, individuals who are successful at a task ask for less redistribution among two randomly selected participants: the supply of redistribution is affected by the SSB. Second, participants who are successful in one task display stronger preferences for non-redistributive systems for future unknown tasks: the demand of redistribution is also affected by the SSB.

These findings have considerable consequences for the public choice literature, since the self-serving bias impacts both the demand and the supply sides of the political market. Our work shows that such psychological biases affect the perception of reality, and the result of the decision-making process. What is the impact on the production of legal rules if biased voters are electing biased representatives?

The rest of the paper is organized as follows. Section 2.2 introduces previous experiments on the self-serving bias, and redistribution systems. Section 2.3 describes the experiment and presents some predictions. Section 2.4 displays the results. Section 2.5 concludes.

2.2 Literature Review

2.2.1 Literature on the self-serving bias

In a very influential paper, Miller & Ross (1975) summarized previous studies in psychology that attempted to assess the self-serving bias. In their paper, they defined the *self-serving bias* as the fact to "attribute success to our own dispositions and failure to external forces". Following this definition, they decomposed the *self-serving* bias into two phenomena: on the one hand, the fact that some people indulge in self-protective attributions under conditions of failure, and, on the other hand, the fact that some others indulge in self-enhancing attributions under conditions of success. They further argue that the combination of these two phenomena leads to a difference in causal attributions between successful and disappointed individuals.

The self-serving bias has become a broadly recognized cognitive mechanism in Western countries, although some papers have questioned its universality (Mezulis et al. (2004)). The law and economic literature has devoted a strong attention to the self-serving bias and more specifically to its role for the definition of fairness. Some authors showed indeed that a low rate of settlement could emerge from litigants affected by self-serving biases (Loewenstein et al. (1993), Babcock et al. (1995), Babcock & Loewenstein (1997)).

2.2.2 Theories of the Veil of Ignorance

Our experiment is closely related to the works of Rawls and Buchanan, who both defined their own version of the veil of ignorance. In our experiment, we use the veil of ignorance to estimate the impact of the self-serving bias on the demand of redistribution net of egoistic interests.

The starting point of the Rawlsian theories of redistributive justice is the fiction of the *veil of ignorance*. In Rawls' eyes, redistribution should be decided under a veil of ignorance, that is to say in complete ignorance about one's own identity, in order to get rid of selfish interests. Rawls argues indeed that, if redistribution decisions were to be made in this way, the implemented level of redistribution would correspond to the *fair* level of redistribution, and everyone would agree on the notion on fairness.

This first version of the veil of ignorance has received harsh criticisms. Although this veil of ignorance was primarily meant to be a thought experiment, many scholars have pointed out that individuals are not able to abstract from their identity, and to think in a disinterested manner. Buchanan recognized that individuals are not able to abstract from their identity, and proposed an augmented version of the veil of ignorance. Buchanan proposes to introduce uncertainty by considering future positions in life. By doing so, he argues that individuals will come to think in a disinterested manner.

This augmented version of the veil of ignorance has however found a limited empirical support. Several reasons can explain the failure of Buchanan's veil of ignorance. First, it might be that individuals form expectations about their future identity relying on their current identity. Second, it may be that agents display very high discount rates, and value therefore the present –in which they know their identity- much more than the future.

Our paper uses Buchanan's veil of ignorance to analyze the effect of the self-serving bias on the demand of redistribution net of the egoistic interests (second game). Our conclusions tend however to support the critiques against the veil of ignorance as an efficient political devise to make emerge a *fair* level of redistribution. We show indeed that successful individuals are more likely to believe that success is due to effort: they are therefore likely to believe that their future efforts will help them to maintain their relatively high social status.

2.2.3 Experimental Literature on the Determinants of Redistribution Preferences

Since the end of the 70s, several works in the economic literature have studied the determinants of redistribution choices. Three pioneer experimental works have aimed at investigating concepts of distributive justice.

First, Hoffman & Spitzer (1985) built on previous experimental results that showed a surprisingly high propension of individuals asking for equal redistribution in case of random

assignment of entitlements. They proposed to investigate subjects' concept of redistributive justice by analyzing three redistribution principles: utilitarianism, egalitarinism, and the Lockean theory. They concluded that most individuals behave according to the Lockean theory, i.e. that their demand of redistribution depends on their perception of whether individuals *deserve* their outcome.

Then, Frohlich et al. (1987) sought to determine which redistribution scheme people tend to consider as fair. To do so, they constructed an experiment in which they reproduced Rawls' *Veil of Ignorance* (VoI), and in which participants had limited information about their own situation. Under this VoI, they asked people to choose between four justice principles, among which Rawls' *difference principle* (maximization of the floor income). They concluded that Rawls' difference principle was the least preferred redistribution mechanism.

In a subsequent work, Frohlich & Oppenheimer (1990) proposed to investigate whether the acceptance of justice principles was determined by economic experience. The results of their experiment showed that the level of satisfaction with redistributive system was increasing over time, i.e. the more participants experienced it, for both tax payers and tax receivers.

This high level of satisfaction for both tax payers and tax receivers was however contested in subsequent studies. As a matter of fact, in a recent experiment Cabrales et al. (2012) showed that wealthy and non-wealthy participants display different redistribution preferences. More explicitly, they found that wealthy participants typically vote *against* redistribution, while non-wealthy participants, on the contrary, usually vote *for* redistribution.

Two recent works have contributed to expand the knowledge on the determinants of redistribution preferences.

First, Schildberg-Hoerisch (2010) proposed to disentangle two effects of the VoI on redistribution choices. In her article, Schildberg-Hoerisch claims indeed that decisions made under the VoI result from two factors, namely risk-aversion and social preferences. Her experimental protocol consisted in investigating whether uncertainty created by the VoI increases the demand for redistribution as a consequence of selfish or altruistic interests. She found that selfishness is the main factor driving the increase in redistribution following the introduction of the VoI.

Second, Gerber et al. (2013) proposed an experiment with various degrees of ignorance. In their experiment, the authors considered several treatments with different sets of information with regard to participants' future identity. They partly confirmed Rawls' assertion by showing that the level of redistribution was decreasing with the level of information.[1]

[1]Gerber et al. (2013) have three treatments which affected the level of information participants had regarding their future identity. When choosing on redistribution, participants could (i) know nothing about their future identity, (ii) have a noisy signal about their future identity, or (iii) be aware of their future identity.

Several results can be retained from the existing literature. First, the literature has extensively showed that preferences over redistribution systems are very heterogenous among the population (Gerber et al. (2013), Cappelen et al. (2007), Frohlich et al. (1987), Frohlich & Oppenheimer (1990), Loukas et al. (2013)). Second, it has also been emphasized that redistribution is mainly determined by self-interests, but *not entirely* (Hoffman & Spitzer (1985), Cappelen et al. (2007), Kataria & Montinari (2012), Klor & Shayo (2010), Durante et al. (2014)). Third, Kataria & Montinari (2012) have showed that the demand for redistribution depends on the perception of the causes of poverty. In an influential experimental work, Konow (2000) showed indeed that disinterested observers act according to the accountability principle, i.e. they are more likely to reward individuals based on their efforts, and to compensate back luck. Eisenkopf et al. (2013) also showed that individuals tend to compensate back luck in settings with unequal opportunities. This result has also been confirmed by Balafoutas et al. (2013), who show that participants are more likely to redistribute when they believe that inequalities are arbitrary.

2.2.4 Closest Works

Kataria & Montinari (2012) and Gerber et al. (2013) are the two papers closest to our work.

First, Kataria & Montinari (2012) have showed that (i) when income is determined by luck, the demand for redistribution is higher than when it is generated by ability, (ii) when people do not know their relative position, they ask for more redistribution, and (iii) people are affected by the self-serving bias even when they act as third parties. Our experiment differs in several dimensions. First, in their setting, participants have a perfect information about the determinants of the individual payoffs (luck vs. effort). However, in every day debates about redistribution, it is rarely known to which extent luck objectively determined each individual situation. On the contrary, our protocol aims at mimicking these debates, and leaves people uninformed about the extent to which relative success is exogenously determined. Second, in the above setting, participants are asked to act as third parties just after they have been asked to express their preferences for themselves. This set-up may contaminate the data since individuals are more likely to adjust their declaration of what is fair to what they have declared for themselves (to avoid cognitive dissonance). Wealthy participants are therefore likely to declare that low taxation levels are fair, not because of the SSB, but because of consistency with their previous answer (in which they had interests at stake). Third, in the unequal opportunities treatment, participants are able to form posterior beliefs about their relative position after the production phase (and before the shock). In our protocol, participants cannot form any posterior about the difficulty of the task they were assigned to, and cannot therefore update their beliefs about the exogenous determination of their situation.

Second, our paper relates to Gerber et al. (2013) from which it takes the redistribution

systems. In their paper, the authors analyze the choices of redistribution systems under different informational sets. First, they define the *libertarian rule*, under which no redistribution occurs, and participants keep their own production. Second, they introduce the *egalitarian rule*, under which the total production is shared equally among participants whatever their effort level. Third, they present the *proportional rule*, under which participants receive a share of the total production proportional to their investment. Our protocol keeps these three systems, and refers to them respectively as the *libertarian*, the *egalitarian* and the *social-liberal* systems.

2.2.5 Contribution

The main innovation of our paper consists in creating a framework with total uncertainty about the causes of relative success. In everyday situations, when citizens debate on the level of redistribution, they only know (i) the situation they have faced, and (ii) their relative success. Because they do not know situations other citizens have faced, nor do they observe others' efforts, they are not able to infer what is due to luck and what is due to effort in their society. In such a context of total uncertainty (probabilities and states of the world are unknown), people should, as Rawls assumes, have similar beliefs about the determinants of poverty. However, as our article shows, even in case of total uncertainty, people still update their beliefs about the determinants of success. As we show, this change in beliefs affects the supply and the demand for redistribution.

2.3 The Experiment

Our experiment explores the potential consequences of the self-serving bias on redistribution issues. Our protocol aims at generating a self-serving bias among participants, and capturing the effects of this bias on both the supply and the demand of redistribution.

2.3.1 Design

The experiment started with subjects earning money in a real effort task. The purpose of this task was to allocate the status of either 'overachiever' (to the subjects with an above median performance among the subjects in a session), or 'underachiever' (to the remaining subjects). This stage was followed by a manipulation check. After that we elicited our two main measures of interest. First, subjects played the Disinterested Dictator Game (DGG), providing us with a measure of supply of redistribution. Second, we conducted the Redistribution System Game (RSG) as a measure for the demand for redistribution. All interaction was anonymous and computerized. We used z-tree (Fischbacher, 2007) to program the interface, and ORSEE (Greiner, 2015) for recruitment.[2]

[2]See appendix 2.E for the instructions and screen shots.

Real Effort Task. The real effort task consisted of a simple task of counting the ones in lines of binary digits. The screen contained 20 to 25 lines, with four to thirteen digits each. Subjects had to indicate the number of ones occurring in each line. There were five consecutive screens and there was a time limit of 25 seconds per screen. Correct answers were rewarded by a certain number of tokens, depending on the condition they were assigned to. Half of the subjects were randomly assigned to the *hard* condition, the other half to the *easy* condition. The maximum number of tokens was identical in both conditions. However, the tasks were designed such that it was very unlikely that a subject in the *hard* condition would earn more tokens than a subject in the *easy* condition. After completion of the five screens we used the number of tokens earned in the real effort task to perform a median split of the subjects within sessions. Subjects who earned more tokens than the median were told that they performed above median (in the article we label them as overachievers), the other subjects were told that they performed below median (underachievers). The fact that the difficulty of the two tasks was sufficiently different ensured that the allocation of the task determined to a large extent whether a participant was an over- or underachiever. Any differences between over- and underachievers in the DDG or the RSG must then be caused by the allocation of the task, and not by self selection of subjects into treatment.[3]

Subjects were aware of the procedures. In the instructions we informed them that they could be assigned either to an *easy* or to a *hard* task with equal probability. Participants were also told that the maximum possible earnings were the same in both tasks. However, at no point in the experiment participants were told which task they were assigned to. While the two tasks clearly differed in difficulty, even the easy task was designed such that none of the participants managed to solve it perfectly, given the time limit. In addition, participants could not observe other participants' task. Consequently, participants were unable to deduce which task they were actually assigned to.

After the completion of the real effort task, participants were informed about their relative position compared to the median participant (above or below). After that we preformed a manipulation check. Subjects answered six questions as to which extent they believed that their relative achievement (success or failure) was due to one of the following factors: (i) the task's difficulty (*Diff*), (ii) the introduction of the exercise (*Intr*), (iii) the clearness of the exercise (*Clear*), (iv) their effort (*Eff*), (v) their will (*Will*), and (vi) their attention and focus (*Focus*). The first three questions identify situational factor, the last three questions individual factors.

[3]In the two first sessions (STANDARD sessions hereafter) four subjects in *hard* condition managed to become overachievers. Results from these sessions might be influenced by selection. In the results section we will show that all our results remain the same if we exclude these two sessions. For the remaining sessions we increased the difference in the difficulty between *hard* and *easy*, and we observed a perfect separation. We will refer to the latter as GAP-sessions. For a comparison of the two versions see appendix 2.D.

Disinterested Dictator Game. For the DDG two participants (the 'targets') were randomly selected among all participants of the session.[4] The remaining participants (the 'disinterested dictators') were informed about the *difference* between the two targets' incomes of the real effort task. The disinterested dictators had then the possibility to redistribute tokens from the wealthier to the poorer target. All participants were told that the decision of one disinterested dictator would be randomly selected and implemented. Participants were also explicitly told that redistribution would concern only the two *targets*, and that all others would not be affected by any redistribution mechanism in this task.[5] Prior to the decision, disinterested dictators were reminded that targets may have faced different tasks. After every disinterested dictator made her choice, one redistribution proposal was randomly selected, and implemented. Disinterested dictators received their payoff from the real effort task, while targets received their real effort task payoff corrected for redistribution. Disinterested dictators did not learn the implemented redistribution decision, and targets only learned their post-redistribution payoff.

We refer to this game as the *Disinterested Dictator Game*, because the dictator has the power to redistribute, but–different from the dictator game–does not have his own profit at stake. The game is also different from the so-called third party dictator game (Fehr & Fischbacher (2004)), in which the classic dictator game is enriched by a third party who can punish the dictator. A game similar to ours is presented by Konow (2000), who studies the accountability principle. Konow investigates the redistribution choice of a dictator who is either exterior to the real effort task and has no stake in the redistribution, or who participates to the game and has direct stakes in the redistribution. Konow refers to the two treatments as the *Benevolent Dictator Treatment* and the *Standard Dictator Treatment*. In our case, dictators have taken part in the real effort task but have no stake in redistribution. Previous works in the literature, such as Durante et al. (2014), also used disinterested decision-makers to investigate redistribution decisions net of selfish interests.

Redistribution System Game. For the Redistribution System Game participants were given new instructions. In these instructions, participants were told that they were going to be matched into groups of four, and that they were going to perform another series of real effort tasks that were substantially different from what they did in the beginning of the experiment. Participants were also informed that they were going to earn tokens in these real effort tasks, but that their payoffs would also be affected by *random shocks*, which could be either payoff increasing or payoff decreasing. Finally, the instructions said that, after each task and after each shock, redistribution was going to occur within each

[4] In order to ensure comparability among our sessions, the selection process was set as follows. First, we randomly selected the first target. Second, we computed the difference of tokens between the first target and the remaining participants. We then selected a participant such as to have a difference of tokens equal to twenty (or, if there was no exact match, as close to twenty as possible).

[5] Figure 2.13 in the appendix shows a screen shot of this stage.

group according to the group's redistribution system.

Participants were also informed that, prior to the real effort game, they would vote on redistribution systems. We presented three canonical redistribution systems to the participants. The *libertarian* system leaves each participant with her *after-shock* payoff (no redistribution). The *egalitarian* system sums up all individual *after-shock* payoffs within the group, and redistributes the sum in equal shares to the group members (full redistribution). Finally, the *social-liberal* system sums up all individual *after-shock* payoffs within the group, and redistributes the sum proportionally to the individual *pre-shock* payoffs (effort-based redistribution). Apart from these three 'pure' systems, participants could also implement a mixture of the systems. To aid understanding we gave participants an example showing how each redistribution system could potentially affect their final payoffs for given pre-shock and after-shock payoffs. Before turning to the vote, we presented the participants with control questions to ensure that the three redistribution principles were well understood. In four of the six sessions participants were presented with three statements about the redistribution systems and had to indicate whether they were correct or not. In the two remaining sessions, we implemented extended control questions. Participants were asked to compute hypothetical after-redistribution payoffs for a given set of pre-redistribution payoffs of four group members. The examples comprised four redistribution systems: 100% libertarian, 100% egalitarian, 100% social-liberal, and 50% libertarian 50% social-liberal.[6]

After the presentation of the redistribution systems, participants were asked to assign weights w_i between 0 and 10 to each of the three redistribution systems. Participants were told that one group member's set of choices would be randomly chosen and implemented for the group. Given three weights w_1, w_2 and w_3 we computed a triplet of relative weights $v_i = \frac{w_i}{w_1+w_2+w_3}$, $i = 1, 2, 3$. For each of the real effort tasks a participant's final payoff is equal to $v_1\%$ (resp. $v_2\%$ and $v_3\%$) of the payoff that she would have earned under the canonical system 1 (resp. 2 and 3).

After the vote and the determination of the redistribution system participants were informed about the redistribution system selected for their group. A screen displayed the composition in terms of percentages of the three canonical system. After that, participants proceeded with the real effort tasks. They had to read a short text (approx 140 words) and count the number of misspelled words. The individual (pre-shock and pre-redistribution) profit of the task was equal to the 20 tokens minus four times the absolute difference between the reported number of mistakes and the real number of mistakes in the text. After each real effort task participants learned their initial profit, their profit after the shock, and their final profit (including redistribution). Then participants were then asked (1) whether they were satisfied with the implemented redistribution system, and (2) whether they felt reinforced in their original choice. The second experiment ended after four real

[6]Using control questions presumably enhances subjects' understanding of the mechanisms, but it might have the disadvantage that the experimenter has to pick specific actions of the game as examples, which might influence subsequent behavior, see Roux & Thöni (2015).

effort tasks.

The RSG is inspired by the experiment reported in Frohlich & Oppenheimer (1990), where subjects choose a redistribution system without knowing the nature of the task they are about to perform. Once a redistribution system has been selected, subjects are given a series of texts to correct (spelling mistakes). The choice of redistribution systems follows Gerber et al. (2013).

2.3.2 Hypotheses

Our experimental protocol aimed at investigating how the self-serving bias may impact the supply and the demand of redistribution. Our goal consisted in reproducing real-life debates on redistribution, in which citizens have limited information about situations other citizens have faced in their life, but are aware of their relative status in society and the difficulties they faced themselves.

The real effort task in the beginning of the experiment creates two kinds of participants: those who performed better than the median participant (*overachievers*), and those who performed worse than the median participant (*underachievers*). The goal of this categorization is to induce a self-serving bias among participants, by artificially creating a group of successful and a group of unsuccessful participants. The Disinterested Dictator Game (DDG) measures the impact of the change of the perceptions of causality on the supply of redistribution towards third parties. Indeed, as Konow (2000) showed, people decide on redistribution according to the *accountability principle*, i.e. they reward people proportionally to their level of effort. By affecting the perception of the role played by effort in the final outcome, we expect the self-serving bias to affect the supply of redistribution: successful (resp. non-successful) participants will be more likely to believe that efforts (resp. random factors) play a great role in determining one's success, and will therefore be less (resp. more) likely to redistribute. Because decision-makers are not directly affected by redistribution, our protocol allows us to isolate how the redistribution is changed by the perception of the causes of success without any interference of egoist interests. Our prediction with regard to the DDG is:

Prediction 1 Overachievers will redistribute less than underachievers.

In the Redistribution System Game participants are asked to express their preferences over three redistribution systems. Unlike in the first game, participants' redistribution decisions at the beginning of the second game are designed to affect their own future (unknown) payoff. The *ex-ante* choice about the redistribution systems ensures that participants expresses their *demand* for redistribution behind a veil of ignorance, i.e., not knowing the nature of the real effort task. Following the same argument as for prediction 1, if the self-serving bias changes one's perception of the determinants of success, overachiev-

ers should be more likely to believe that their future payoffs will be determined by their efforts than underachievers, and would therefore express a lower demand for redistribution.

At the beginning of the second game, participants consider three factors that might affect their future revenue: their level of effort, their ability to the (unknown) task and the random shocks. The first factor is obviously endogenous, while the two latter are exogenous. The two exogenous factors result in uncertainty and risk. First, participants face *uncertainty* regarding the nature of the task they are about to perform (ability). Second, they face *risk* concerning the shocks that they know to happen after each task. The libertarian system corresponds to a situation without insurance, where both risk and uncertainty may affect the payoffs. On the opposite, the egalitarian system insures against both risk and uncertainty: if an individual faces a task at which he/she is very bad, he/she will receive transfers from other participants more capable at this task. Moreover, in the egalitarian system, shocks are fully compensated. However, the egalitarian system comes at a cost: it generates incentives to free ride. The social-liberal system stands in-between: it redistributes according to the pre-shock payoff, which is determined by the participants' abilities, but not by the shocks. The social-liberal system provides an insurance against risk, but not against uncertainty.

Due to the self-serving bias a successful participant is more likely to see the outcome as resulting from his/her own effort than a less successful participant. The self-serving bias is therefore likely to impact the demand for redistribution in cases where causation is not clearly determined. In our setting, random shocks are clearly determined in so far as participants know that they are purely exogenous. The self-serving bias is therefore not expected to have an impact on the demand for insurance against *risk*. However, the nature of the task is unknown to participants, and they can therefore hardly anticipate their relative ability. Thus, the self-serving bias should increase the demand for insurance against *uncertainty* for participants who performed relatively worse, because they expect the nature of the task to play a predominant role in the determination of their payoff.

Three predictions follow from this discussion. First, we expect overachievers to have a stronger preference for social-liberalism than underachievers, since it does not insure against uncertainty (but does for risk). Similarly, we anticipate that underachievers will display a stronger demand for the egalitarian system, because it insures against risk and uncertainty. In case of the libertarian system things are less clear. A priori, one should expect this system to be more preferred by overachievers than underachievers, since it insures against neither uncertainty nor risk, and that underachievers are more willing to insure against uncertainty than overachievers. However, choices about the redistribution systems were made simultaneously, which obviously rules out independence of alternatives. The difference between the social-liberal and the libertarian system is that the former insures against the purely exogenous shocks, while the latter does not. Given that the shocks are mean-preserving and variance-increasing, risk averse individuals will always prefer the social-liberal system to the libertarian system. Assuming that participants are risk averse,

we should observe no difference between the overachievers and the underachievers regarding the libertarian system. If participants are risk neutral, we should expect a higher demand for the overachievers.

Prediction 2-A Overachievers will opt for less *egalitarianism* than underachievers.

Prediction 2-B Overachievers will opt for more *social-liberalism* than underachievers.

Prediction 2-C Overachievers will opt for at least as much *libertarianism* as underachievers.

2.4 Results

We ran six sessions with 24 participants each. All sessions were run in Strasbourg (January and February 2014, July 2015). The sessions lasted about 45 minutes, and participants earned on average 13.66 euro. We present our results in the order in which they were elicited, starting with real effort task, followed by the Disinterested Dictator Game and the Redistribution System Game.

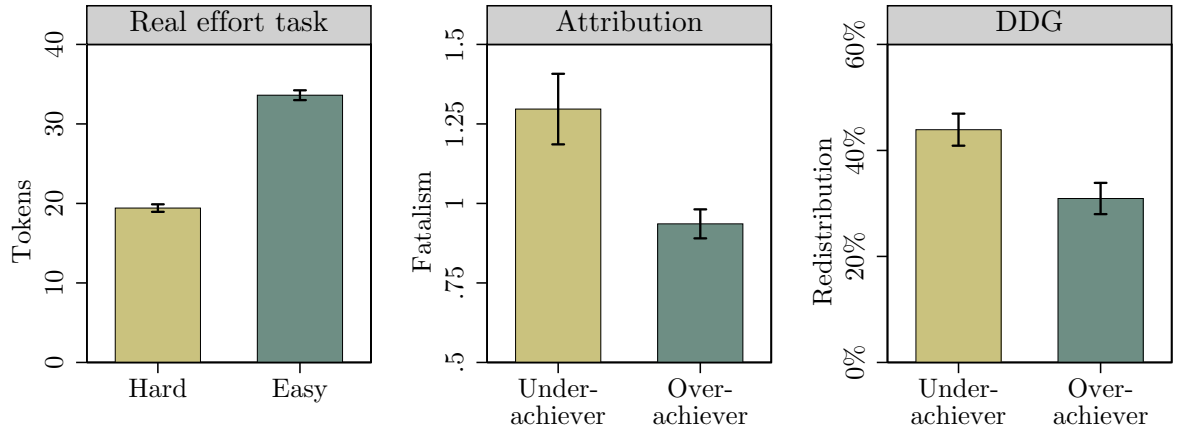
2.4.1 Real effort task

The experiment starts with a real effort task, in which subjects count ones on the screen. This provides us with a measure for individual performance. The left panel of Figure 2.1 shows the results of the real effort task. Subjects randomly allocated to the hard task scored on average 19.4 tokens (sd: 4.02), while subjects in the easy task scored 33.6 (sd: 5.25) tokens. Spikes in the figure are standard errors, indicating that the difference between the hard and easy task is highly significant.

Based on the performance measure we classify our subjects into overachievers (above median performance), and underachievers. In four sessions the hard/easy task was perfectly separating the population, i.e., all participants randomly allocated to the easy task turned out to be overachievers, and vice versa. In the other sessions four participants with the hard task managed to perform better than the median participant, and became overachiever. Consequently, four participants with the easy task became underachiever. Note that our protocol induced the same level of information for both underachievers and overachievers regardless of their original task. It follows that participants were not able to deduce whether they were assigned to the hard or the easy task, such that presumably only the labeling as 'above the median' or 'below the median' affected their attributions. Consequently SSB can occur whatever the original task a participant was assigned to.

Before looking at the redistribution decisions, we perform a manipulation check to see whether our protocol did induce a self-serving bias among participants. To do so, we compare answers to the six questions as to whether subjects attribute their success (or

Figure 2.1: Left panel: Average number of tokens earned in the *hard* and *easy* real effort task. Middle panel: Levels of *Fatalism*, defined as the ratio between external and internal factors, for over- and underachievers. Right panel: Percentage redistributed in the disinterested dictator game. Spikes show standard errors.



failure) to effort or luck. Comparing the average scores of overachievers and underachievers shows that the former gave systematically higher scores to all questions (see Table 2.1 in the appendix). To compare the relative weight of situational factors to the factors related to effort (individual), we define a measure *Fatalism* as the ratio between the sum of the scores for the three situational factors and the sum of the scores for the individual factors. The middle panel of Figure 2.1 shows the results. We find a clear and significant difference in *Fatalism* between the two groups: With a ratio of 1.30 underachievers put a higher relative weight on situational factors than overachievers (0.94). The difference is significant at $p = .003$ (two-sample t-test).[7] To conclude, the experience of being an under- or overachiever systematically affects the way they attribute the outcome to internal and external factors. While overachievers tend to emphasize their own contribution, underachievers tend to focus on external factors, i.e., develop a more fatalist attitude. We see this as a clear indication for a self-serving bias. In a next step we investigate whether the differences between over- and underachievers affect redistribution decisions.

2.4.2 DDG: Supply of Redistribution

Recall that in the disinterested dictator game redistribution affects only targets' payoffs. Dictators were specifically told that no redistribution would affect their own payoff in this game. Since we have some variation in the differences between the two targets' profits

[7]The results are very similar if we consider only the GAP sessions (1.21 vs. 0.90); and the difference remains significant ($p = .012$).

across sessions we calculate the percentage of the payoff difference to be redistributed form the richer to the poorer target. Zero corresponds to leaving the incomes unchanged, while reallocating 50 percent of the difference means that the two profits are equalized. Overall we observe a redistribution of 37.4 percent; 17.4 percent of the subjects do not redistribute at all, while 41.7 percent of the subjects implement a solution which equalized payoffs.

Prediction 1 links a person's status after the real effort task to the supply of redistribution. The right panel of Figure 2.1 shows the average redistribution percentage chosen by underachievers and overachievers (*RedSupply*). The difference is substantial and significant: Underachievers redistribute 43.9 percent while overachievers redistribute 30.9 percent ($p = .003$, two-sample t test).[8] In the appendix we provide additional analyses to check the robustness of our results on *Fatalism* and the redistribution decision. First, we perform a permutation test (two-sided $p = .001$); second we run OLS estimates with cluster robust standard errors, controlling for individual characteristics such as gender, political orientation, age or the practice of competitive sport (table 2.2).[9] This leads to our first result:

Result 1 Overachievers of the real effort game redistribute less money from the richer to the poorer target than underachievers.

2.4.3 RSG: Demand for redistribution

We now turn to the analysis of the preferences over the redistribution systems in the second game. Because participants were told that the real effort tasks of the second game would be substantially different from the first real effort task, they were *a priori* not able to predict their relative abilities in the task. Thus, it is unclear to them to which extent their future outcomes would in fact depend on their effort. In this regard, the decisions made at the beginning of the first game were made under a *veil of ignorance*.

Overall the social-liberal system is clearly the most popular among our participants, with an average relative weight of 46.3 percent, followed by the egalitarian system (28.7 percent), and the libertarian system (25.0 percent).[10]

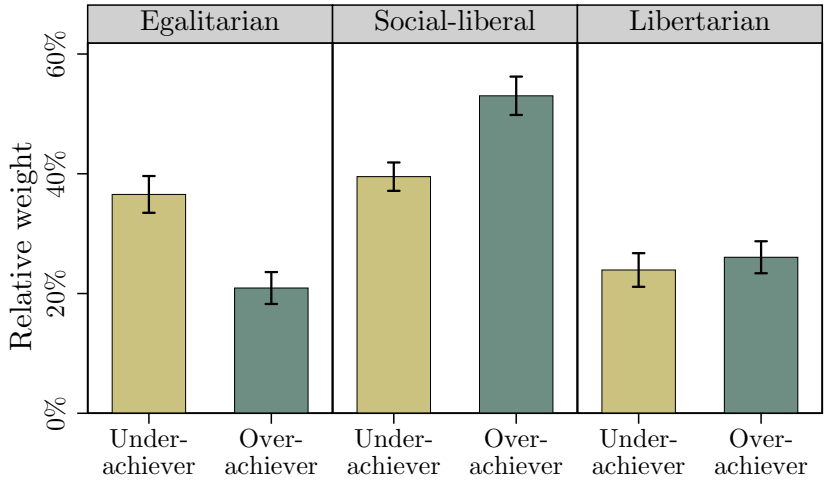
Overachievers' preferences over redistribution systems are systematically different from those of underachievers. Figure 2.2 illustrates the main results. Most pronounced are differences in the support for the egalitarian system, which receives on average 36.6 percent of the relative weight among underachievers and only 20.9 among overachievers ($p < .001$, two-

[8]The effect size is almost identical if we consider only the GAP sessions (42.2 percent vs. 29.9 percent); and the difference remains significant ($p = .015$).

[9]Because of the small number of sessions, we implemented wild bootstrapping to obtain robust p-values such as suggest by Cameron & Miller (2015)

[10]The results from the two sessions with the extended control questions are similar to the results of the other four sessions. The relative weight of the social-liberal system is almost identical (46.5 percent vs. 45.8 percent , $p = .878$); the libertarian system receives somewhat stronger support in the sessions with the extended control questions (29.7 percent vs. 22.6 percent, $p = .085$).

Figure 2.2: Results from the Redistribution System Game. Bars show average relative weight given to the respective redistribution system. Spikes show standard errors.



sample t test). Most of the difference is offset by a stronger support for social-liberalism, which is more popular among overachievers (53.0 percent), than among underachievers (39.5 percent, $p < .001$). With regard to the libertarian system we do not find significant differences.

In order to determine the validity of predictions 2-A, 2-B, and 2-C, we investigate preferences over the redistribution systems for both overachievers and underachievers. Because participants know they will have future (unknown) interests at stake, their decisions reflect their *demand* for redistribution.

The redistribution system chosen by each participant was a composition of the three theoretical redistribution systems given the importance level given to each of them. Because scores were normalized before implementation, we analyze the relative weights given to each system. We denote them *Libert* (libertarianism), *SocialLib* (social-liberalism), *Egal* (egalitarianism).[11]

Few results can be derived from the investigation of the preferences over the three redistribution systems.[12] First, the two-group mean-comparison tests show that overachievers assign (i) significantly more weight to the social-liberal system (*SocialLib*, p-value=0.026),

[11]Results with the absolute weights were identical. Results of the two-group mean-comparison tests are also available in the appendix, and the regression tables are displayed in the appendix. Absolute weights are labelled *absLibert*, *absSocialLib* and *absEgal*.

[12]Note that we do not exclude the 'target' participants. Indeed, since the target participants were only aware of their final payoff in the first game (i.e. they did not know whether they benefited or whether they were damaged by redistribution), the effect of the self-serving bias is not altered compared to the rest of the group. We also computed the statistics excluding these observations, and results were nearly identical.

and (ii) significantly lower weight to the egalitarian system (*Egal*, p-value=0.000). The OLS estimations confirm these two results (tables 2.5 and 2.6 respectively). The OLS estimates show that the choice for the libertarian system solely depends on the political orientation of the participants. The coefficient associated to *overachiever* is positive but not statistically significant.

Preferences for the libertarian system do not seem to be affected by the self-serving bias, but is only correlated with political orientation: more rightist participants ask for a stronger proportion of libertarianism (see table 2.4). The instructions given to the participants and the instructions displayed on the screens labelled each of the three systems by their own name. We cannot therefore rule out the possibility that the preferences for the libertarian system by the rightist participants are driven by a labelling issue (i.e. that libertarianism is badly connoted for left-wing participants). But given the random assignment of the easy and hard tasks, political orientation should be identically distributed across groups (overachievers vs. underachievers)[13], and the lexical stigma associated to the labelling of the systems should be also identically distributed. It follows that the lack of significance of the *overachiever* status cannot be attributed to labelling issues.

Given the empirical evidence, it appears that the self-serving bias affects the demand of insurance against uncertainty, but not the demand of insurance against risk. Underachievers are more likely to prefer full insurance than overachievers (egalitarianism). This confirms Prediction 2-A. On the contrary, overachievers display a stronger preference for a system that insures against risk only compared to underachievers (social-liberal). This validates Prediction 2-B. Overachievers tend to show a stronger preference for the system without insurance compared to underachievers (libertarian), but this difference is not statistically significant. This result is in line with Prediction 2-C.

Result 2 Overachievers have a stronger preference for the social-liberal system than underacherachievers, who, in turn, have a stronger pref for egalitarianism. We find no difference across treatments for the libertarian system.

2.5 Conclusion

Our paper aimed at investigating the consequences of the self-serving bias on redistribution choices. To do so, we run an experiment in which participants had no information about the situation other participants faced, but had some information about their relative success status. This set-up was enough to induce self-serving biases among participants. To isolate the effects of the self-serving bias from selfish interests, we made participants choose on the level of redistribution in a disinterested manner or under a veil of ignorance. This allowed us to assess the impact of the self-serving bias on both the supply and the demand of redistribution.

[13]We observe indeed no statistical difference between the two groups (two-sample t-test: $p = .595$)

We came up with two far-reaching results. We showed that participants with a good relative success status display a lower *supply* of redistribution, because they are on average more likely to believe that their outcome result from their efforts compared to participants with a low relative success status. Second, we showed that the self-serving bias also affects the demand of redistribution in the same manner, i.e. by reducing the *demand* for redistribution for relatively successful participants.

Our findings have significant implications for political debates on redistribution, because the self-serving bias affects both the supply and the demand of redistribution.

As far as the supply side is concerned, when deputies discuss wages or capital taxes at the Congress, they often make a trade-off between the necessity of insurance against random events (luck) and the risks of moral hazard (effort). More globally, national debates on redistribution are usually made in the light of these two arguments, and Alesina & Angeletos (2005) have showed how common beliefs on these topics indeed affect redistribution policies. The main problem lies in the fact that policy-makers deal with limited information about the situation people in need have faced in the past: it is therefore impossible to figure out whether poverty resulted from a lack of effort or from bad luck. In this regard, our findings show that, in addition to selfish interests, such debates may be biased by personal experience, i.e. relative success. Our results show that, even in this precise case of limited information, individuals don't share the same beliefs on the determinants of individual situations. In simpler words: successful and less successful individuals do not share the same reality. Our results lead to pessimistic implications for the institutional question. Indeed: because of the very nature of the political process, in which elected officials are *winners* of previous elections, politicians are likely to be subject to the self-serving bias, and to overestimate the role of effort.

Second, our results also create some concerns regarding the demand for redistribution. The self-serving bias may indeed exacerbate the egoistic interests in the political debates. Assuming that the demand for redistribution is decreasing with wealth, the self-serving bias deepens the gap between wealthy and non-wealthy individuals. Because wealthy individuals are more likely to believe that their economic success is due to their efforts, they will feel comforted in their egoistic interests, and will ask for lower redistribution levels. On the contrary, non-wealthy individuals will be more likely to believe that their situation is due to misfortune, and this will strengthen their (relatively) high demand for redistribution. In this respect, the dichotomous nature of our treatment (overachiever or underachiever) prevents from drawing further inference about the self-serving bias' impact for continuous relative success status. It might be that its effect is non-monotonous nor symmetric.

It follows, from a theoretical perspective, two implications for the political economy. First, the distribution of the preferences for redistribution for the elected representatives is shifted compared to the general population toward less redistribution (supply side). Second, within the population, the distribution of preferences is affected by contingent

experiences: poorer individuals may ask for even more redistribution, and richer individuals for even less redistribution, leading to fatter tails of the distribution and increased variance (demand side). This increased polarization of the public debate may reduce the likelihood to reach a broad compromise on redistribution issues, making extreme choices more attractive.

Appendix 2.A: Summary statistics

Variable	Source	All Participants	Underachievers	Overachievers	p-value
Diff	MC	3.903	3.597	4.208	0.052
		(1.893)	(1.998)	(1.744)	
Intr	MC	4.604	4.472	4.736	0.412
		(1.922)	(2.143)	(1.678)	
Clear	MC	3.639	2.125	5.153	0.000
		(2.295)	(1.695)	(1.758)	
Eff	MC	3.951	3.333	4.569	0.000
		(1.682)	(1.601)	(1.537)	
Will	MC	4.042	2.653	5.431	0.000
		(2.099)	(1.567)	(1.582)	
Focus	MC	4.993	3.917	6.069	0.000
		(1.83)	(1.782)	(1.105)	
Fatalism	MC	1.116	1.297	.936	0.003
		(.74)	(.942)	(.387)	
RedSupply	DDG	.374	.439	.309	0.003
		(.25)	(.246)	(.24)	
absLibert	RSG	4.035	3.861	4.208	0.559
		(3.545)	(3.562)	(3.544)	
absSocialLib	RSG	7.021	6.431	7.611	0.026
		(3.194)	(3.223)	(3.074)	
absEgal	RSG	4.556	5.681	3.431	0.000
		(3.747)	(3.626)	(3.544)	
Libert	RSG	.25	.239	.261	0.584
		(.232)	(.238)	(.226)	
SocialLib	RSG	.463	.395	.53	0.001
		(.248)	(.201)	(.272)	
Egal	RSG	.287	.366	.209	0.000
		(.255)	(.261)	(.226)	

Notes: Means and standard errors (in parentheses); p-values correspond to bilateral two-group mean-comparison tests.
MC stands for *Manipulation Check*, DDG for *Disinterested Dictator Game* and RSG for *Redistribution System Game*.

Table 2.1: Summary Statistics

Appendix 2.B: Additional Analyses

In this section, we present the estimates of the econometric specifications discussed in the paper. We propose a multivariate analysis of the degree of *Fatalism* and the redistribution decision of the first game, and the importance levels of the redistribution systems of the second game. Our set of explanatory variables includes a dummy variable for male participants, since it has been found that male participants are more likely to be risk-averse or/and altruistic than male. Second, we control for political orientation, since redistribution is obviously a very salient political issues for left-right opposition. Third, we include the age of participants. Finally, we also propose to consider the impact of practicing sport regularly at competitions. It might be indeed that high skilled athletes are more likely to perceive their results as the fruit of their efforts.

We run standard OLS regressions for eight dependent variables: the degree of fatalism, the supply of redistribution in the first game, the normalized importance levels given to each redistribution system in the second game and the associated absolute importance levels. Using the *best* selection method, we present different specifications, which progressively include additional independent variables, based on their explanatory power. We impose that our specifications include the overachiever status and two dummies for the GAP sessions (i.e. session 3 to 6) and the second set of control questions (i.e. sessions 5 and 6). The best selection method then includes additional variables according to their explanatory power. We present the C, the AICC and the BIC statistics. Tables 2.2 and 2.3 display the results for the *Fatalism* and *RedSupply* variables respectively. Tables 2.4, 2.5 and 2.6 display estimates of the normalized importance scores (*Libert, SocialLib, Egal*). Tables 2.7, 2.8 and 2.9 show respectively the results for *absLibertarian*, *absSocialLib* and *absEgalitarian*. Note that we cluster all standard errors by session.

Since we run our experiment on six sessions, the clustering of the standard errors can be made on six clusters only. Recent works in the econometric literature have investigated the issue of the low number of clusters. Standard corrections for the standard errors still underestimate the true standard errors. In order to deal with this issue, we rely on wild cluster-bootstrapping to compute a robust p-value for our parameter of interest (*overachiever*). We follow the guidelines given by Cameron and Miler (2015).[14] In this present appendix, we display three elements of this method for each regression table. On the one hand, we present the Rademacher and Webb p-values. These statistics were calculated for the regressions including all covariates. They must be interpreted similarly the p-values of the statistical test associated to the null hypothesis for the *overachiever* variable (two-sided test). In other words, we claim that the *overachiever* variable has a significant impact when these p-values are below 5%. On the other hand, we also show the distributions of the Webb and Rademacher t-statistics from which we derived the p-values. Note that, in our case, the wild cluster-bootstrap technique yields similar results to the standard cluster

[14]Cameron, A. C. & Miller, D. L. (2015), 'A practitioner's guide to cluster-robust inference', Journal of Human Resources 50(2), 317–372.

correction.

Model	1	2	3	4	5
overachiever	-0.361***	-0.368***	-0.377***	-0.380***	-0.387***
	(0.0783)	(0.0833)	(0.0838)	(0.0855)	(0.0862)
GAP	-0.195*	-0.194*	-0.204*	-0.214*	-0.222*
	(0.0896)	(0.0897)	(0.0927)	(0.0897)	(0.0939)
male		-0.0774	-0.0707	-0.107	-0.101
		(0.140)	(0.141)	(0.123)	(0.126)
sport				0.0805	0.0817
				(0.0926)	(0.0905)
polit_orient				-0.0220	-0.0195
				(0.0390)	(0.0407)
age			0.0104		0.00854
			(0.0111)		(0.0129)
Constant	1.427***	1.553***	1.319**	1.459**	1.250
	(0.0953)	(0.231)	(0.422)	(0.432)	(0.666)
Observations	144	144	144	144	144
R-squared	0.075	0.078	0.081	0.084	0.085
C		1.999	3.544	5.241	7
AICC		317.515	319.22	321.11	323.10
BIC		328.96	333.45	338.11	342.82
Rademacher p-value					0.035
Webb p-value					0.126

Notes: Clustered standard errors in parentheses. ***p<0.01, ** 0.01<p<0.05, *0.05<p<0.10

Table 2.2: OLS regression of *Fatalism*.

Model	1	2	3	4	5
overachiever	-0.130***	-0.129***	-0.127***	-0.125***	-0.125***
	(0.0134)	(0.0135)	(0.0144)	(0.0168)	(0.0173)
GAP	-0.0409	-0.0372	-0.0324	-0.0312	-0.0311
	(0.0435)	(0.0449)	(0.0459)	(0.0442)	(0.0435)
polit_orient		0.00546	0.00634	0.00578	0.00572
		(0.00587)	(0.00558)	(0.00501)	(0.00500)
sport			-0.0312	-0.0325	-0.0313
			(0.0486)	(0.0467)	(0.0559)
age				-0.00185	-0.00191
				(0.00590)	(0.00666)
male					-0.00314
					(0.0662)
Constant	0.466***	0.438***	0.532**	0.579**	0.582**
	(0.0363)	(0.0445)	(0.155)	(0.161)	(0.194)
Observations	132	132	132	132	132
R-squared	0.074	0.076	0.079	0.080	0.080
C		1.476	3.074	5.004	7
AICC		5.915	7.687	9.845	12.107
BIC		16.97	21.43	26.24	31.12
Rademacher p-value					0.015
Webb p-value					0.010

Notes: Clustered standard errors in parentheses. ***p<0.01, ** 0.01<p<0.05, *0.05<p<0.10

Table 2.3: OLS regression of *RedSupply*.

Model	1	2	3	4	5
overachiever	0.0213	0.0292	0.0326	0.0346	0.0352
	(0.0230)	(0.0196)	(0.0202)	(0.0207)	(0.0218)
GAP	0.0414	0.0400	0.0424	0.0413	0.0423
	(0.0387)	(0.0414)	(0.0415)	(0.0413)	(0.0408)
newControls	0.0499	0.0830	0.0850	0.0865	0.0866
	(0.0391)	(0.0430)	(0.0449)	(0.0447)	(0.0443)
polit_orient		0.0318***	0.0307***	0.0311***	0.0313***
		(0.00386)	(0.00306)	(0.00313)	(0.00335)
age			-0.00396	-0.00366	-0.00367
			(0.00277)	(0.00301)	(0.00303)
male				0.0218	0.0242
				(0.0421)	(0.0401)
sport					-0.00799
					(0.0171)
Constant	0.195***	0.0336	0.124*	0.0795	0.100
	(0.0124)	(0.0233)	(0.0504)	(0.115)	(0.142)
Observations	144	144	144	144	144
R-squared	0.028	0.118	0.121	0.124	0.124
C		2.907	4.356	6.032	8
AICC		-20.70	-19.07	-17.17	-14.93
BIC		-6.465	-2.076	2.551	7.488
Rademacher p-value					0.164
Webb p-value					0.146

Notes: Clustered standard errors in parentheses. ***p<0.01, ** 0.01<p<0.05, *0.05<p<0.10

Table 2.4: OLS regression of *Libert*.

Model	1	2	3	4	5
overachiever	0.135***	0.142***	0.141***	0.141***	0.142***
	(0.0118)	(0.00951)	(0.0102)	(0.0111)	(0.0143)
GAP	-0.0664***	-0.0696***	-0.0692***	-0.0680***	-0.0676***
	(0.00775)	(0.00851)	(0.00820)	(0.00928)	(0.0102)
newControls	0.0265	0.0312	0.0243	0.0243	0.0246
	(0.0214)	(0.0230)	(0.0314)	(0.0323)	(0.0344)
male		0.0757	0.0735	0.0763	0.0759
		(0.0526)	(0.0545)	(0.0550)	(0.0527)
polit_orient			-0.00650	-0.00625	-0.00642
			(0.00826)	(0.00805)	(0.00753)
sport				-0.00952	-0.00962
				(0.0176)	(0.0178)
age					-0.000625
					(0.00488)
Constant	0.431***	0.307**	0.344**	0.368**	0.383**
	(0.00742)	(0.0831)	(0.116)	(0.131)	(0.106)
Observations	144	144	144	144	144
R-squared	0.087	0.110	0.113	0.113	0.113
C		2.552	4.051	6.012	8
AICC		-.412	1.269	3.471	5.735
BIC		13.824	18.264	23.193	28.150
Rademacher p-value					0.0150
Webb p-value					0.008

Notes: Clustered standard errors in parentheses. ***p<0.01, ** 0.01<p<0.05, *0.05<p<0.10

Table 2.5: OLS regression of *SocialLib*.

Model	1	2	3	4	5
overachiever	-0.156***	-0.162***	-0.172***	-0.176***	-0.177***
	(0.0254)	(0.0224)	(0.0220)	(0.0254)	(0.0271)
GAP	0.0251	0.0261	0.0302	0.0276	0.0252
	(0.0362)	(0.0386)	(0.0390)	(0.0369)	(0.0337)
newControls	-0.0764	-0.101*	-0.109*	-0.111*	-0.111*
	(0.0442)	(0.0447)	(0.0451)	(0.0513)	(0.0515)
polit_orient		-0.0240**	-0.0257**	-0.0245**	-0.0249**
		(0.00797)	(0.00881)	(0.00841)	(0.00842)
male			-0.0979**	-0.0948**	-0.100**
			(0.0341)	(0.0331)	(0.0364)
age				0.00426	0.00430
				(0.00674)	(0.00687)
sport					0.0176
					(0.0264)
Constant	0.374***	0.496***	0.665***	0.562**	0.517**
	(0.0161)	(0.0504)	(0.109)	(0.145)	(0.185)
Observations	144	144	144	144	144
R-squared	0.110	0.152	0.187	0.191	0.191
C		8.604	4.701	6.137	8
AICC		1.480	-2.398	-.749	1.383
BIC		15.72	14.60	18.97	23.80
Rademacher p-value					0.020
Webb p-value					0.0126

Notes: Clustered standard errors in parentheses. ***p<0.01, ** 0.01<p<0.05, *0.05<p<0.10

Table 2.6: OLS regression of *Egal*.

Model	1	2	3	4	5
overachiever	0.347	0.473	0.456	0.445	0.451
	(0.466)	(0.452)	(0.461)	(0.489)	(0.509)
GAP	0.771	0.750	0.698	0.697	0.701
	(0.611)	(0.658)	(0.645)	(0.650)	(0.626)
newControls	-0.0625	0.463	0.470	0.463	0.466
	(0.607)	(0.695)	(0.721)	(0.738)	(0.760)
polit_orient		0.504***	0.497***	0.494***	0.492***
		(0.0951)	(0.104)	(0.109)	(0.0927)
sport			0.359	0.395	0.393
			(0.479)	(0.519)	(0.528)
male				-0.0961	-0.101
				(0.527)	(0.528)
age					-0.00699
					(0.0729)
Constant	3.368***	0.805	-0.293	-0.238	-0.0671
	(0.244)	(0.579)	(1.148)	(1.277)	(2.632)
Observations	144	144	144	144	144
R-squared	0.012	0.109	0.111	0.111	0.111
C		2.333	4.031	6.007	8
AICC		766.10	768.00	770.22	772.49
BIC		780.34	784.99	789.94	794.90
Rademacher p-value					0.432
Webb p-value					0.446

Notes: Clustered standard errors in parentheses. ***p<0.01, ** 0.01<p<0.05, *0.05<p<0.10

Table 2.7: OLS regression of *absLibert*.

Model	1	2	3	4	5
overachiever	1.181**	1.227**	1.175*	1.185*	1.178*
	(0.420)	(0.411)	(0.467)	(0.477)	(0.504)
GAP	-0.562***	-0.582***	-0.618***	-0.625***	-0.638**
	(0.0878)	(0.0798)	(0.136)	(0.129)	(0.176)
newControls	-0.938**	-0.908**	-0.951**	-0.897**	-0.898**
	(0.273)	(0.289)	(0.259)	(0.305)	(0.297)
male		0.474	0.510	0.533	0.503
		(0.510)	(0.461)	(0.480)	(0.453)
age			0.0593	0.0652	0.0654
			(0.0909)	(0.0830)	(0.0835)
polit_orient				0.0551	0.0525
				(0.112)	(0.119)
sport					0.0999
					(0.588)
Constant	7.118***	6.345***	5.016**	4.572**	4.313
	(0.195)	(0.862)	(1.516)	(1.236)	(2.330)
Observations	144	144	144	144	144
R-squared	0.072	0.077	0.082	0.083	0.083
C		2.863	4.228	6.025	8
AICC		741.11	742.65	744.68	746.93
BIC		755.35	759.65	764.40	769.34
Rademacher p-value					0.051
Webb p-value					0.044

Notes: Clustered standard errors in parentheses. ***p<0.01, ** 0.01<p<0.05, *0.05<p<0.10

Table 2.8: OLS regression of *absSocialLib*.

Model	1	2	3	4	5
overachiever	-2.250***	-2.380***	-2.459***	-2.550***	-2.566***
	(0.468)	(0.458)	(0.438)	(0.458)	(0.475)
GAP	0.0625	0.118	0.134	0.0617	0.0357
	(0.182)	(0.181)	(0.207)	(0.203)	(0.155)
newControls	-1.521***	-1.605***	-1.900***	-1.950***	-1.951***
	(0.322)	(0.314)	(0.295)	(0.437)	(0.451)
male		-1.342***	-1.435**	-1.353**	-1.411**
		(0.319)	(0.365)	(0.393)	(0.456)
polit_orient			-0.278*	-0.245	-0.250
			(0.116)	(0.126)	(0.124)
age				0.116	0.116
				(0.0728)	(0.0748)
sport					0.195
					(0.541)
Constant	6.146***	8.336***	9.899***	7.126**	6.620*
	(0.273)	(0.625)	(1.240)	(1.949)	(2.914)
Observations	144	144	144	144	144
R-squared	0.126	0.157	0.183	0.195	0.195
C		8.418	6.012	6.078	8
AICC		774.09	771.77	771.98	774.18
BIC		788.32	788.77	791.71	796.59
Rademacher p-value					0.000
Webb p-value					0.004

Notes: Clustered standard errors in parentheses. ***p<0.01, ** 0.01<p<0.05, *0.05<p<0.10

Table 2.9: OLS regression of *absEgal*.

Figure 2.3: Distribution of the t-values using wild cluster-bootstrapping for *Fatalism*.

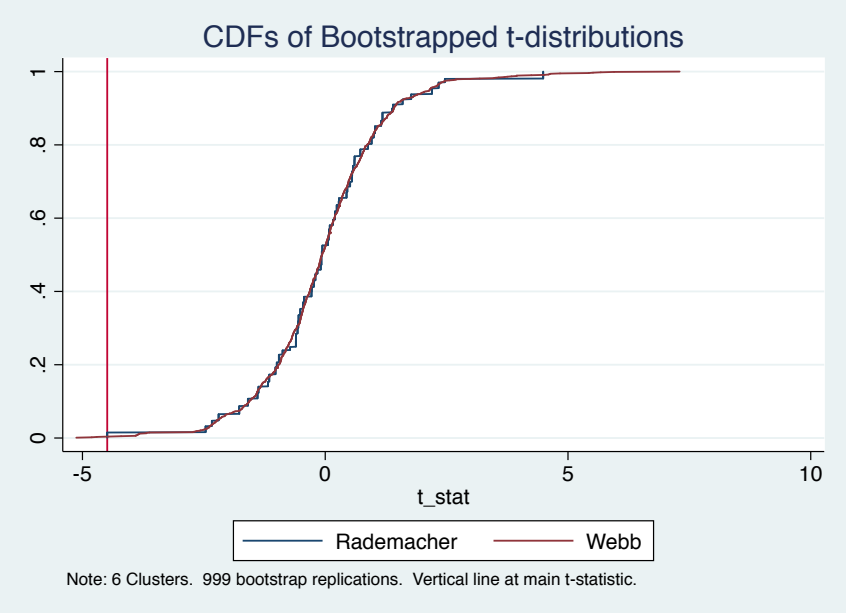


Figure 2.4: Distribution of the t-values using wild cluster-bootstrapping for *RedSupply*.

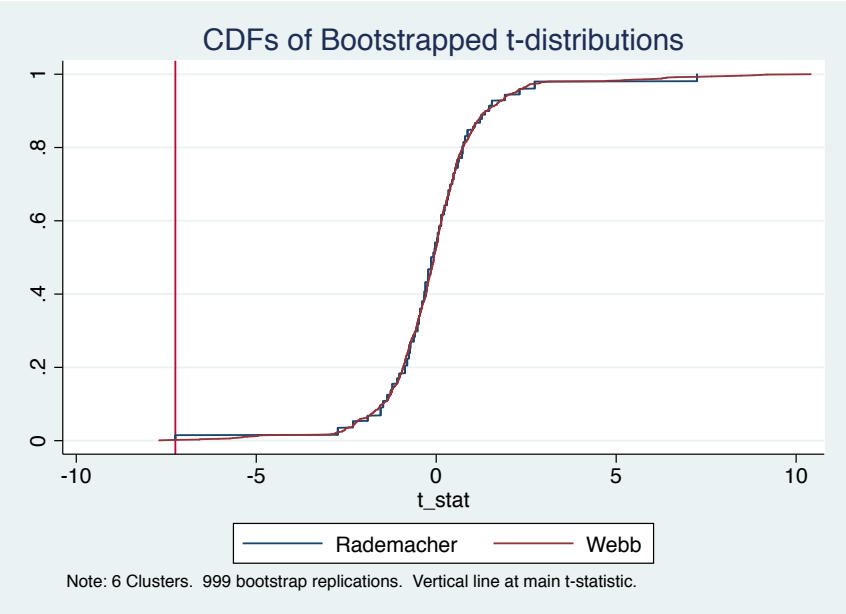


Figure 2.5: Distribution of the t-values using wild cluster-bootstrapping for *Libert*.

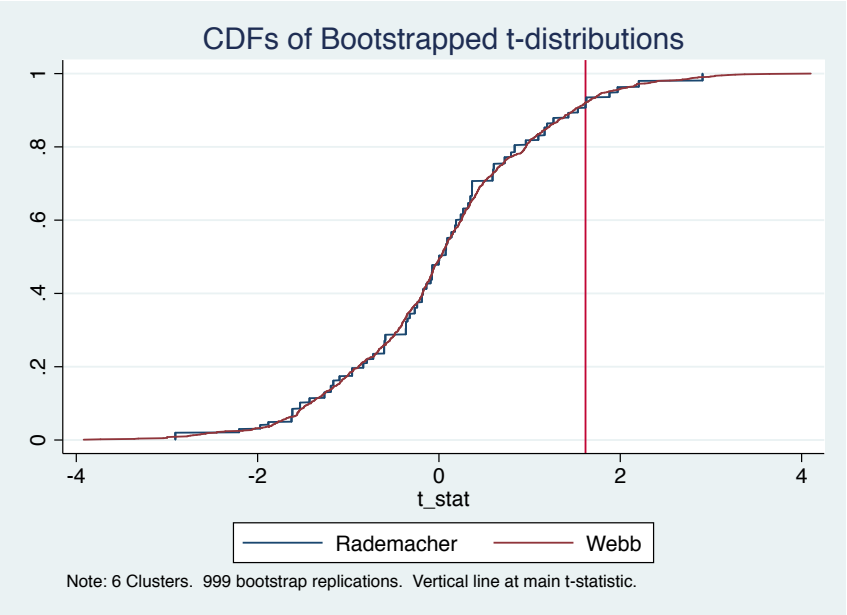


Figure 2.6: Distribution of the t-values using wild cluster-bootstrapping for *SocialLib*.

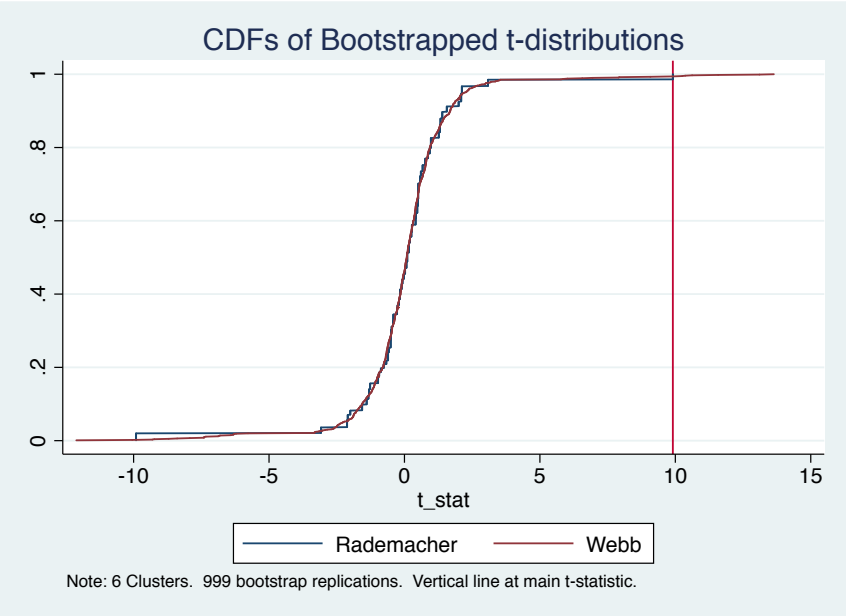


Figure 2.7: Distribution of the t-values using wild cluster-bootstrapping for *Egal*.

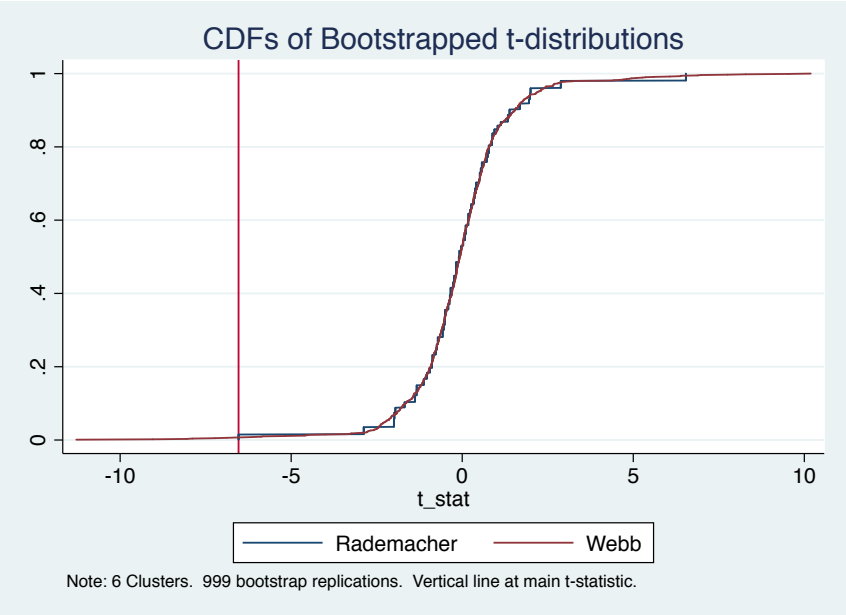


Figure 2.8: Distribution of the t-values using wild cluster-bootstrapping for *absLibert*.

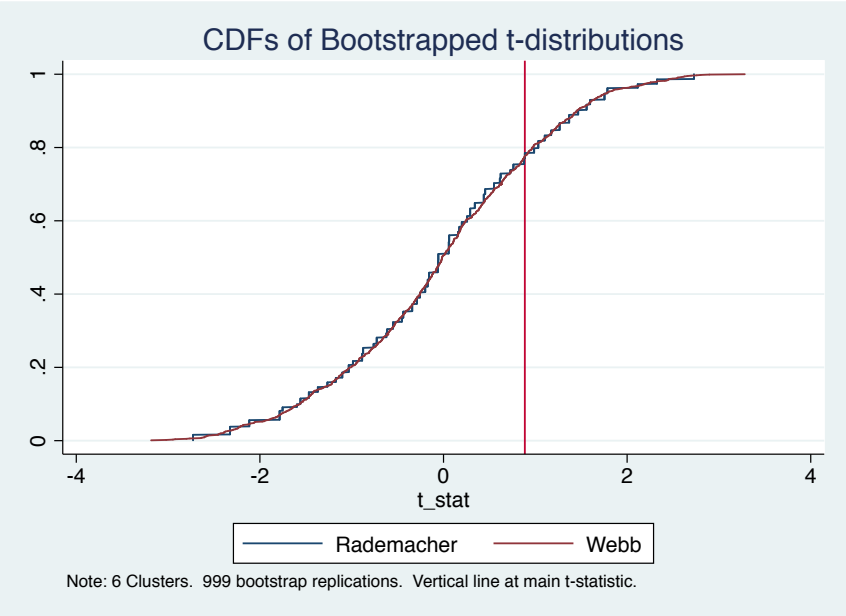


Figure 2.9: Distribution of the t-values using wild cluster-bootstrapping for *absSocialLib*.

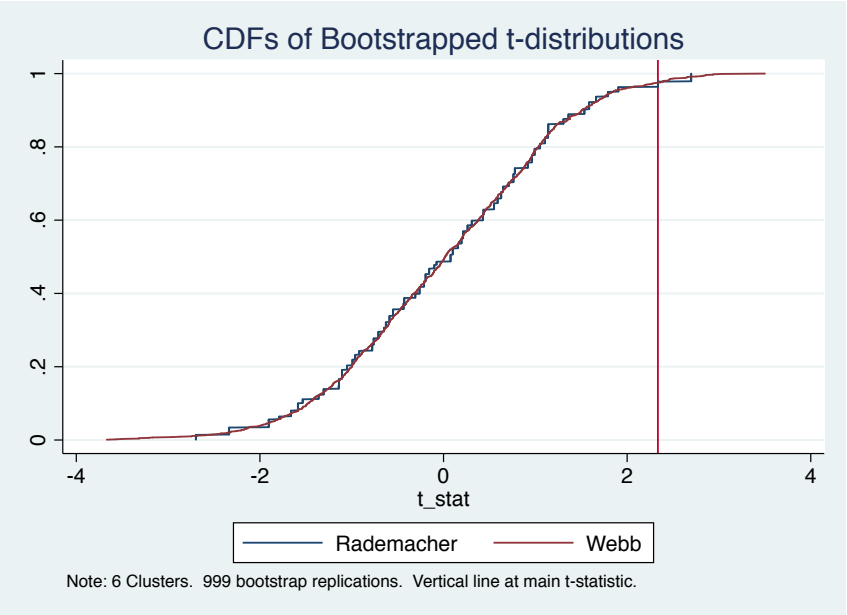
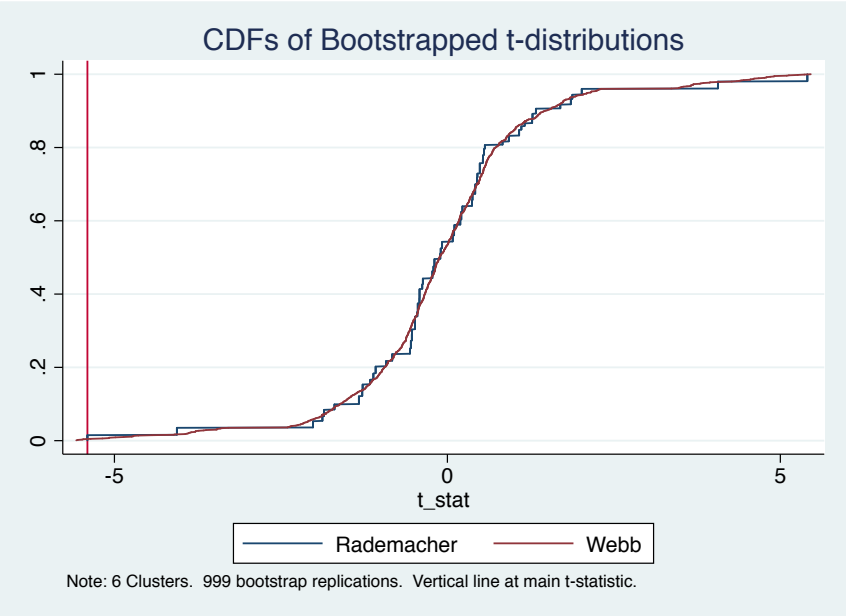


Figure 2.10: Distribution of the t-values using wild cluster-bootstrapping for *absEgal*.



Appendix 2.C: Permutation Tests

Figure 2.11: PDF of the permutation test for *Fatalism*.

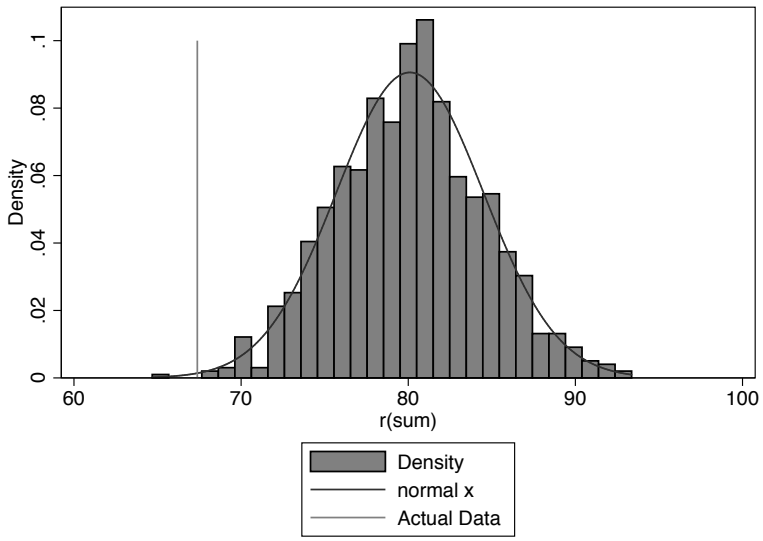
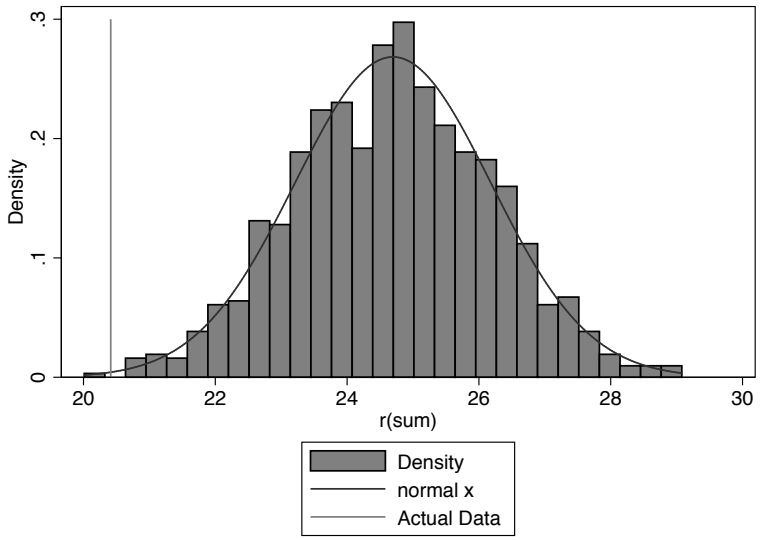


Figure 2.12: PDF of the permutation test for *Supply*.



Appendix 2.D: Comparison of STANDARD and GAP

Our protocol contained two versions of the first real effort task: STANDARD (first two sessions) and the GAP (remaining four sessions). The difference between the two versions is the difference in the difficulty between the real effort tasks (the *easy* and the *hard* task). Compared to the STANDARD, the GAP task contained more simple *easy tasks* and more complex *hard tasks*, leading to a better separation of the profits in *hard* and *easy*.

As we have mentioned in the main text, in the *STANDARD* sessions the separation of subjects in over- and underachievers was not perfectly exogenous: four participants (three in the first session, one in the second session) in the *hard* task succeeded in performing better than the median and became overachievers. In GAP the task was perfectly separating in all sessions: participants who were assigned to the *hard* task became *underachievers*, while those who were assigned to the *easy* task turned out to be *overachievers*.

In Table 2.10 we compare overachievers and underachievers across versions of the experiment. As one can see, we observe no statistical difference across comparable groups at the 95% confidence level.

	All Overachievers			All Underachievers		
	STANDARD (mean)	GAP (mean)	p-value	STANDARD (mean)	GAP (mean)	p-value
RedSupply	.329	.299	.64	.474	.422	.419
Fatalism	1.017	.895	.213	1.476	1.207	.257
Libert	.202	.29	.123	.209	.254	.452
SocialLib	.565	.513	.446	.431	.377	.284
Egal	.233	.197	.537	.36	.368	.892

Table 2.10: Comparison of comparable groups across versions of the experiment. All variables are normalized. P-values correspond to the two-group mean comparison tests.

Appendix 2.E: Instructions and Screen Shots

First Part - Instructions

Hello everyone!

You are about to take part to an experiment, and we are very thankful for your participation. This experiment is made of two parts. This instruction sheet refers to the first part of the experiment only. Instructions for the second part will be given to you at the end of this first part. If instructions turn to be unclear, or if a question remains unanswered, please raise your hand and wait for an instructor to come.

This experiment is made of both individual decisions and group interactions with other individuals in the room. At some points in the game, your decisions may affect others' payoffs, and reciprocally. For this reason, it is strictly forbidden to communicate during the entire experiment. In case of breach of this rule, we will be forced to expel you from the room.

During this experiment, you will earn ECU (*Experimental Currency Unit*). At the end of the experiment, the total number of ECU that you will have gained will be converted into EURO. The conversion rate is 5 ECU for 1 EURO.

The first part of the experiment proceeds in several steps. First, you will be randomly assigned to a task. In this task, you will be asked to count the number of 1 in series of 0 and 1. Your performance will be timed, and your gains will depend on the number of correct answers you will give. The assigned task will possibly be either *easy* (50% chances) or *hard* (50% chances). Both kinds of tasks contain the same number of ECU to win.

Example: 011010. This series of digits contains 3 ones. The correct answer is 3.

Once all participants have completed their task, you will be asked to answer few questions. Then, two participants (the *targets*) will be randomly selected. The first part of the experiment will end at this point for the two *targets*. The remaining participants (the *judges*) will learn the difference of ECU between the two target participants, and will have the opportunity to transfer ECU from the wealthiest to the poorest participant.

Example: Participant A owns 8 ECU more than B. How many of these ECU are you willing to transfer to B? The answer must lie between 0 (no redistribution) and 8 (total transfer).

Once all *judge* participants have chosen a level of redistribution, a solution will be randomly drawn, and will be implemented for the two *targets* only.

To sum up, the first part of the experiment unfolds as follows:

1) All participants are randomly assigned to a task;
2) All participants do their task;
3) Participants answer few questions;
4) Two participants are randomly selected (*target* participants);
5) The difference of ECU between the two *targets* is displayed to the *judges* who decide on the allocation these ECU;
6) One redistribution proposal is randomly selected;
7) All participants learn their final payoff. It is equal to their performance to the task for the *judges*, and equal to the performance affected by the randomly selected redistribution solution for the *targets*.

Second Part - Instructions

The second part of the experiment is about to start. In this part, you will be asked to complete a series of tasks. These tasks are substantially different from what you have done in the first part of this experiment. Your profit for each task will depend on both your effort (the number of ECU received at the task) and a random component (a positive or negative *choc*). Each task can give you individually 0, 4, 8, 12, 16 or 20 ECU. Individual shocks may affect your own payoff by the following amounts: -5, -4, -3, -2, -1, 0, +1, +2, +3, +4, +5 (all shocks have the same probability of occurrence). Shocks affecting other participants of your group are independent of the shock that affect your own payoff.

At the beginning of this part, you will be randomly grouped with 3 other participants. Once the group is formed, it will remain identical until the end of the experiment. Before the beginning of the second task, your group will be asked to choose a redistribution rule. The retained redistribution rule will be implemented after each task: it will determine the way to reallocate the sum of all profits made by the group at each period. Note that tasks are played individually.

You will be asked to choose among the three following redistribution systems:

1) The **libertarian** rule corresponds to a situation without redistribution: each participant keeps the ECU he obtained at each task and after the shock affected him.
2) The **egalitarian** rule corresponds to a situation with full redistribution: all ECU obtained by the group members after the individual shocks are reallocated in equal shares among group members.
3) The **social-liberal** system corresponds to a situation with partial redistribution: all ECU obtained by the group members after the individual shocks are reallocated proportionally to individual efforts (i.e. according to the individual pre-shock payoffs).

Each participant will be asked to decide on the **importance** he wants to give to each of the three systems presented above to define the group redistribution system. You will be able to give a score ranging from 0 to 10 to each of the three systems. These scores will define a redistribution system. For instance, if you allocate 8 points to each system, the corresponding redistribution rule will be made of 1/3 (one third) of the libertarian system, 1/3 of the egalitarian system, and 1/3 of the social-liberal system. If you decide to unequally weight the three systems, the resulting system will be proportional to the allocated scores. For example, if you allocate 8 points to the libertarian system, 4 points to the egalitarian system, and 0 point to the social-liberal system, the resulting redistribution system will be made of 2/3 (two thirds) of the libertarian system and 1/3 of the egalitarian system.

Once all group members will have made their choice, one and only one solution among the group members will be randomly drawn and implemented for the group until the end of the experiment. For instance, if one of your group member chose 20% of the libertarian system, 50% of the egalitarian system, and 30% of the social-liberal system, and that his/her solution has been selected to be implemented, the redistribution that will occur after each task will be made in the following proportions: 20% of the after-shock payoff will not be modified (20% libertarian), 50% will be redistributed equally

(50% egalitarian), and 30% will be redistributed proportionally to the pre-shock payoff (30% social-liberal).

Example: One group of four participants obtained the following outcomes at the assigned task: participant A, B and C obtained 4 ECU each, while participant D obtained 6 ECU (second column in the table below). After a random shock, participant A obtains 3 ECU, B and D obtain 5 ECU each, and C obtains 7 points (third column).

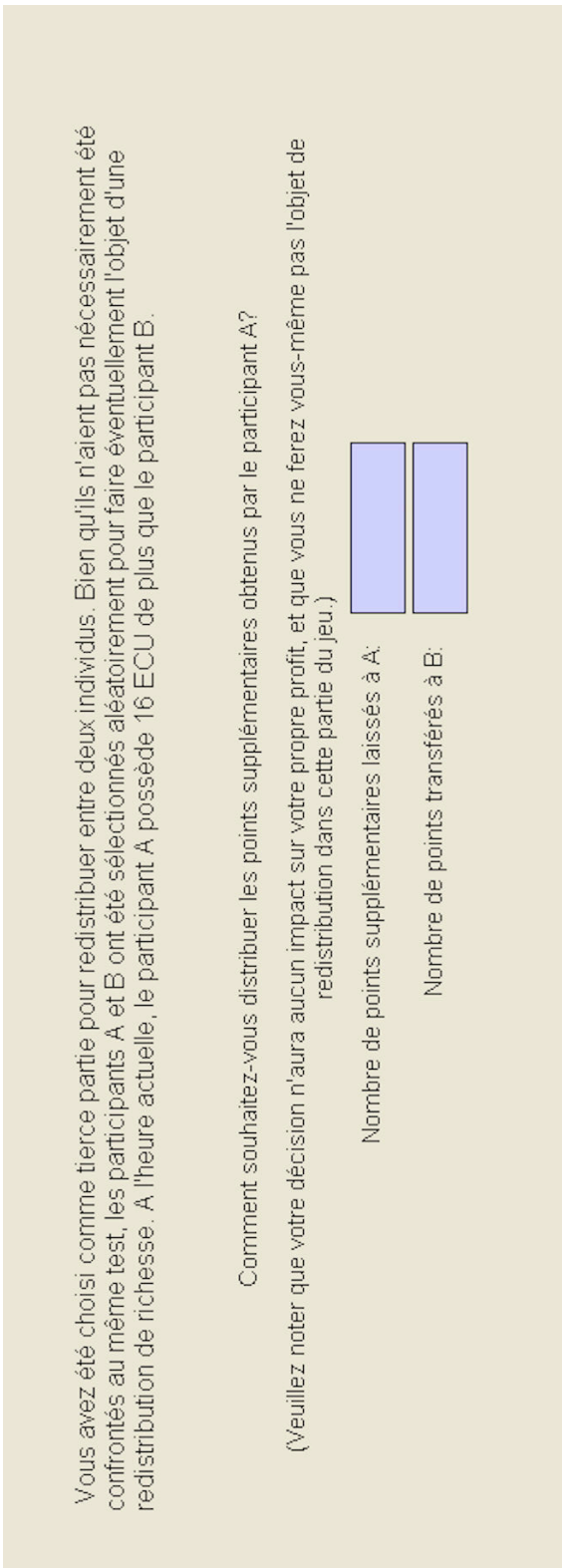
*In the libertarian system, each individual keeps his/her own after-shock payoff. Individual A would therefore keep his/her 3 ECU (fourth column). In the egalitarian system, all individuals receive the same share of the total after-shock revenue, i.e. one fourth of 20 ECU. Individual A would therefore receive 5 ECU (fifth column). In the social-liberal system, participants receive a share proportional to their pre-shock payoff. Individual A received 4 ECU prior to the shock and the group totalized 18 ECU. The total revenue after the shock is 20 ECU. Using a rule of three, the payoff of individual A in a social-liberal system would be equal to: (4/18)*20=4.5 (rounded at 0.5) (sixth column).*

Participant	*Number of ECU obtained at the task*	*Number of ECU after the shock*	*Payoff if full* ***libertarian*** *redistribution system*	*Payoff if full* ***egalitarian*** *redistribution system*	*Payoff if full* ***social-liberal*** *redistribution system*
A	*4*	*3*	***3***	***5***	***4.5***
B	*4*	*5*	*5*	*5*	*4.5*
C	*4*	*7*	*7*	*5*	*4.5*
D	*6*	*5*	*5*	*5*	*6.5*
Somme	*18*	*20*	*20*	*20*	*20*

*Imagine that the system that has been randomly selected to be implemented is as described in the above example: 20% libertarian, 50% egalitarian, and 30% social-liberal. The after-redistribution payoffs of individuals A, B, C and D are given in the table below. Participant A's final payoff is computed by taking 20% of his/her libertarian payoff (0,20*3) (see above table), 50% of his/her egalitarian payoff (0,50*5), and 30% of his/her social-liberal payoff (0,30*4.5).*

Participant	*Number of ECU after redistribution (before rounding)*	*Final payoff (rounded)*
A	*20% * **3** + 50%*5 + 30% * **4,5** = 0.6+2.5+1.35 = 4.45*	*4*
B	*20% * 5 + 50%*5 + 30% * 4,5 = 1+2.5+1.35 = 4.85*	*5*
C	*20% * 7 + 50%*5 + 30% * 4,5 = 1.4+2.5+1,35 = 5.25*	*5*
D	*20% * 5 + 50%*5 + 30% * 6,5 = 1+2.5+1.95 = 5.45*	*5*

Figure 2.13: Screen Shot: Redistribution Decision in the *Disinterested Dictator Game*



3 Constitutional Judicial Behavior: Exploring the Determinants of the Decisions of the French Constitutional Council

Abstract

This article empirically assesses the relevance of three theories of judicial decision-making for the French Constitutional Council. Our empirical analysis follows previous works by integrating more recent observations, and proposes a new methodology by exploiting new data for cases posterior to 1995. After analyzing the 612 cases published between 1974 and 2013, we focus on cases posterior to 1995 for which we know the exact composition of the court. Our results suggest that (1) political/ideological voting occurs, (2) Justices restraint themselves from invalidating laws, and (3) the court's independence suffers from political power concentration in other institutions. All in all, these results urge for a reform of the Constitutional Council to strengthen its independence.

JEL: D71, D72, K40

Keywords: constitutional court, attitudinal model, judicial self-restraint, opportunistic independence, judicial decision-making, politicization, judicial independence.

3.1 Introduction

In the two past decades, a growing empirical literature has analyzed the determinants of Supreme Courts' rulings. These studies started from the widely spread observation that Justices do not decide solely on legal grounds, but are also influenced by personal, inter-personal and out-of-court matters. These studies have explored numerous theories of judicial decision-making. The goal of this paper is to assess the relevance of three of them

for the French Conseil Constitutionnel.

First of all, a considerable number of empirical investigations have aimed at determining whether judges are influenced by political or ideological factors when deciding on a case. These works have mainly sought to question the relevance of the Kelsenian theories of courts supported by legalists, who state that judges purely decide on legal grounds. These legalist views follow a very long tradition in civil law countries, which usually consider that law-making is done exclusively by the Parliament, and that courts rule in a very narrow interpretative framework. In Montesquieu's words, legalists typically consider that judges are the "mouth of the law", and must stick to statutory provisions when enforcing the law. Many works in the United States have been conducted during the past decades, and have emphasized, unlike the legalist theory predicts, that ideological beliefs and political matters influence Justices' decisions (Epstein et al. (2007), Martin et al. (2005), Spiller & Gely (1992), Epstein & Landes (2012), Epstein & Martin (2012)). Similar studies have been conducted for European Constitutional Courts, *i.e.* in civil law countries, and tended to support a similar conclusion: Justices are less likely to strike down laws passed by the party that appointed them (Amaral-Garcia et al. (2009), Garoupa et al. (2013), Hoennige (2009)). More globally, these studies have contributed to validate the *attitudinal theory*, which claims that Justices are not only motivated by legal concerns, but also by political and ideological matters.

A second branch of research has explored the existence of judicial self-restraint that is to say Justices' reluctance to strike down laws voted by the Parliament. Judge Posner defined the *judicial self-restraint theory* as: "the reluctance of judges to declare legislation or executive action unconstitutional out of deference to the judgements of the elected branches of government".[1] Judicial self-restraint may therefore occur whatever the political and/or ideological considerations of the Justices. Politically indifferent Justices may also be subject to judicial self-restraint: even Justices who leave aside their political views may be reluctant to invalidate laws passed by elected representatives. This aversion against invalidation might be driven either by personal preferences (weak perception of the Constitutional Court's legitimacy), or by social concerns (Constitutional Courts are not expected to invalidate bills passed by elected representatives). Judicial self-restraint is especially relevant in the French case, where there exists a well-established tradition against judicial activism. Following Montesquieu, French legal scholars and politicians often publicly criticize decisions of the judiciary when they fill a legislative gap. In the American case, Epstein & Landes (2012) aimed at disentangling judicial self-restraint from ideological voting, but found no evidence for judicial self-restraint.

At last, a third category of studies has analyzed the role of political division for institutional pressure on Supreme Courts' decisions. From a theoretical perspective, Lijphart (1999) claimed that Supreme Courts are more likely to function properly in consensual than in majoritarian democracies. To his view, institutions in majoritarian systems exist

[1] As quoted in Epstein & Landes (2012).

to serve the interests of the political majority, unlike those in consensual democracies, which seek to maximize the support to the public policies. Lijphart predicts therefore that a higher degree of power concentration in majoritarian democracies will induce a stronger pressure of the majoritarian party on other institutions. According to this theory, Supreme Courts would be less likely to function properly when power is concentrated. On the contrary, when elected branches are politically divided, the theory predicts that Supreme Courts are more likely to arbitrate between majorities, and therefore to fulfill its duty of constitutional review. Garoupa & Grembi (2013) analyzed the shift from a consensual to a majoritarian democratic system, and report some evidence for the change in the role played by the Supreme Court in line with Lijphart's theory. In the same direction, Franck (2009) investigates whether the division of the political power in the French elected institutions affects the censorship likelihood by the Constitutional Council.

In addition to these three lines of research, the present paper proposes to contribute to a growing literature on judicial behavior (Carroll & Tiede (2011)). Judicial independence has been extensively discussed in the past decades, since economists have argued that economic growth is fostered when litigants are confident in the independence of the legal system (Holcombe & Rodet (2012)). La Porta et al. (1999) argued that differences in economic growth between civil law and common law countries were mainly explained by the lack of judicial independence in civil law countries. Aside from the civil law vs. common law debate, Feld & Voigt (2003) showed that *de jure* judicial independence does not matter for economic growth, but that *de facto* judicial independence does. Hayo & Voigt (2007) showed however that institutional provisions remain the most important determinants of the *de facto* judicial independence. Serious doubts have therefore been raised about *de jure* judicial independence in institutional frameworks, where the judiciary and the political branches are too interconnected. Melton & Ginsburg (2014) find indeed that judicial independence is enhanced when both the selection and the removal processes of judges ensure that they are independent from other political actors. In the Japanese case, Ramseyer & Rasmusen (1997) showed indeed that the hierarchical structure of the judiciary, together with the political appointing process of the Supreme Court's Justices, can create great threats to judicial independence.

The aim of this paper is to investigate the extent to which the three preceding theories apply to the French case. In the following analysis, we shall refer to the *attitudinal model* when the outcome of the decisions of the Constitutional Council (CC) depends on the political and ideological linkages between Justices and other political actors. Moreover, we will call the *judicial self-restraint theory* the fact that Justices limit their censorship decisions because they are reluctant to strike down laws voted by elected branches. Finally, we shall refer to the *opportunistic independence theory* when the CC's independence is greater when institutions are politically divided. Here, we understand *judicial independence* as "*the amount of discretion that judges have at their disposal vis-à-vis representatives of*

other government branches" (Feld & Voigt (2003)).

From a pragmatic perspective, the *attitudinal model* usually implies that the likelihood to invalidate a law is decreasing with the number of Justices who were appointed by the government's party. The *judicial self-restraint theory* typically entails that Justices are less likely to invalidate a law if they already stroke down laws recently. With regard to the *opportunistic independence theory*, a natural consequence is that the CC invalidates more laws when other institutions are politically divided.

These three theories are *a priori* highly relevant for the French case. Regarding the *attitudinal model*, the CC is indeed frequently criticized for its (assumed) political activism. The recent opposition between the socialist government and the President of the CC, a former right-wing politician, is representative of the accusations of political / ideological voting against the CC.[2] Moreover, the French legal tradition is characterized by a well-establish deference of the judiciary to the elected bodies: following Montesquieu, legal scholars believe that courts should not engage in law-making, and should stick to a strict enforcement of laws. This deference is regularly expressed by Justices themselves, who claim to resist to the international trend of a uniformization of constitutional courts toward the American system.[3] Some legal scholars, such as Troper (2007), recognize that the CC is part of a broader political game, and must adapt its strategies to those of other actors.[4]

The two closest studies to our work are Franck (2009) and Franck (2010). Studying the French Constitutional Council's rulings, Franck (2009) has shown that the level of independence of the Constitutional Council is higher when institutions are politically divided. This work also showed some evidence suggesting that Justices without legal background may engage in political/ideological voting. Franck (2010) also found that the Constitutional Council's decisions on contested elections were also affected by political/ideological considerations: far-right candidates are, *ceteris paribus*, more likely to have their elections invalidated. Our study builds on these two previous works, and completes them in three ways. First, the current paper proposes to consider a broader framework of decision-making: we test the relevance of three theories at the same time (*the attitudinal model, opportunistic independence theory* and *the judicial self-restraint theory*). Franck (2009) devoted his attention to the two first theories. Second, our dataset includes more recent data: while Franck (2009) considered decisions up to 2006, our investigation deals with all

[2]http://www.lemonde.fr/politique/article/2014/01/06/le-rappel-a-l-ordre-de-jean-louis-debre-a-l-executif_4343687_823448.html (Last Access: April, 2015)

[3]http://www.lepoint.fr/politique/conseil-constitutionnel-jospin-oppose-a-l-idee-d-une-cour-supreme-francaise-15-12-2014-1889977_20.php (Last Access: April, 2015)

[4]Troper (2007) says : "It is clear that the Constitutional Council, like all other Supreme Courts [...], is subject to many constraints coming from the political and judicial system and that it must adapt its strategies when exerting its powers." (In French: "Il est incontestable que le Conseil Constitutionnel, comme toutes les juridictions suprêmes [...], subit des contraintes diverses provenant du système juridico-politique et qu'il est conduit à adopter des stratégies dans l'exercice de son pouvoir.")

cases from 1974 to 2013. Third, we exploit a new source of information on the data to better assess the impact of *political/ideological voting*. Our results complete and strengthen the previous findings found by Franck (2009) and Franck (2010).

To answer these research questions, we construct a dataset that comprises all CC's rulings from 1974 to 2013 (August).[5] We then run a series of logit estimations, including fixed effect specifications. We also explore the time component of our observations. The empirical investigation tends to confirm that the three theories have some relevance for the French case: Justices seem to be more likely to support laws passed by the party that appointed them; Justices are reluctant to invalidate too many laws; and the CC is more likely to strike down a law when institutions are politically divided.

The rest of the paper is organized as follows. Section 3.2 presents the CC and the data. Section 3.3 presents the empirical strategy. Section 3.4 displays the results. Section 3.5 discusses the implications of our results. Section 3.6 concludes.

3.2 The *Conseil Constitutionnel*

3.2.1 Description of the *Conseil Constitutionnel*

The *Conseil Constitutionnel* was created together with the Fifth Republic (1958). The regular part of the Constitutional Council is composed of 9 Justices renewable per third and appointed for 9 years. Every three years, three Justices leave the CC; the President of the Republic and each President of the two Chambers appoint one Justice each for nine years. The President of the Constitutional Council is designated by the President of the Republic among the Justices sitting at the CC. Justices can be appointed only once for a full mandate (9 years), but those who are appointed to replace a Justice who left the bench before the end of his/her mandate can be reappointed for one full mandate. Between 1974 and 2013, four Justices were appointed twice according to this procedure.[6] In addition to the nine regular Justices, former Presidents of the Republic may sit as ex-officio members when they desire to do so. In practice, these members have had a limited role in the CC's decisions.[7]

[5]Few cases were dropped, either because the CC declared not to be empowered to rule on them, or because they were constitutional revisions.

[6]Joxe was first appointed to end Rey's mandate, and was then appointed for a full mandate. He seated 12 years at the CC. Lecourt was called to replace Coste-Floret, and was then appointed for a complete mandate. He sat for 10 years. Bazy-Malaurie was first appointed in 2010 to carry on Pezant's mandate, and she was reappointed in 2013. Finally Simonnet was appointed twice (once for replacement, and once for a full mandate), but died before the end of his mandate. He sat about 4 years at the CC.

[7]Three former Presidents of the Republic (De Gaulle, Pompidou, Mitterrand) did not sit at the Council at all. Chirac took part to few decisions for a limited time period, since he had to retire for health reasons. Sarkozy has not taken part to abstract review so far (but did for concrete review). All in all, only Giscard d'Estaing has attended the Council on a regular basis since the end of his presidential mandate.

The Constitutional Council was the first institution empowered to substantially review laws.[8] The 1958 Constitution gave the CC the possibility to review laws in abstract, *i.e.* after the vote of the Parliament and before the promulgation by the President of the Republic. Until 1974, judicial review had to be requested either by the President of the Republic, the Prime Minister, the President of the Senate (Upper Chamber) or the President of the National Assembly (Lower Chamber).

Two main constitutional reforms have contributed to change this institutional framework. The 1974 constitutional reform has extended requests of judicial review to Deputies and Senators. In fact, this reform has introduced for major opposition parties the possibility to challenge laws passed by the majority.[9] Since then, the CC has gained importance, and abstract judicial review has been extensively used by the opposition. The 2008 reform has introduced the possibility of concrete judicial review. Since this date, the CC has been able to strike down laws, which are already in force, if litigants contest their constitutionality during a trial (*Questions Prioritaires de Constitutionnalité*). This procedure has become very popular in the past few years, which has considerably increased the power - and the amount of work- of the CC.

In the rest of this article, we shall focus solely on abstract reviews, which are called *décisions constitutionnelles*. We shall refer to these rulings as *decisions*. Invalidation decisions may concern only some specific features of the laws at stake. The distinction between partial validation and partial invalidation is not our concern, since we consider the outcome of the decision as reported by the CC itself. Because of the very small number of partial validation, this is however not a concern. We categorize the CC's decisions as either *conformity* decisions or *censorship* decisions.

The main institutional difference between the French CC and other Constitutional Courts lies in the fact that the debates of the CC are private, and, therefore, neither individual votes nor individual opinions are available. Privacy of the deliberations is also applied at the Italian Constitutional Court, and is motivated by the will to reinforce the legitimacy and the strength of the Court's decisions.

3.2.2 Data

To analyze the determinants of the CC's decisions, we construct a dataset that comprises the entire set of decisions from 1974 to 2013 excluding one constitutional revision and few cases where the CC was not competent. The final dataset contains 612 cases. The choice

[8]The fourth Republic created an institution able to strike down laws only if procedures had been violated.

[9]Requests by parliamentarians must be done jointly. In fact, Deputies and Senators must be at least 60 in their own Chamber to ask the CC to challenge a law. However, considering that the National Assembly comprises more than 500 seats, this requirement has been set low enough to ensure that major opposition parties are always able to meet the criterion.

to focus on decisions made after 1974 is due to the substantive changes introduced by the 1974 constitutional reform.

The dataset contains information about decisions on laws that were passed either under left-wing or under right-wing legislatures. During this time period, France has been ruled by six right-wing legislatures and four left-wing coalitions.[10] Our investigation focuses solely on the so-called *décisions constitutionnelles*, that is to say judicial review of laws voted by the Parliament but not yet promulgated by the President of the Republic. It includes regular bills, organic laws, resolutions, and treaties. For each decision, we collected both information about the content of the decision and information about the challenged law. Table 3.4 summarizes variables of our dataset.

First, regarding information about the decision itself, we define a *censorship* variable, which is equal to 1 when the decision is reported as *non-conformité* (invalidated) and equal to 0 when the decision is reported as *conformité* (conform to the Constitution).[11] We create a series of variables about the political context in which the decision is made: the proportion of Justices appointed by a right-wing party who are supposed to attend the decision (*compRW*), a dummy variable for the fact that the Chief Justice was appointed by a right-wing party (*presRW*), a dummy variable if the Senate's majority is aligned with the National Assembly's majority (*senate*), and a dummy variable if the Prime Minister is not from the same party as the President of the Republic (*cohabitation*). We also indicate whether the previous decision made by the CC was an invalidation decision (*previous*), the number of invalidation decisions since the beginning of the parliamentary session (*parliamentary*), a dummy variable that accounts for the fact that other invalidation decisions have been published on the same day (*sameDay*)[12], and the time the CC took to review the law (*delay*). Moreover, we control for the authority, which asked for the review (*senators*, *joint*, *presRep*, *primeMin*, *presSenate*, *presAssem*).

Second, we also collect information about the challenged law. We create a series of dummy variables which account for the area of the challenged law (*economics, electoral, finances, institution, justice* and *territorial*). We categorize each challenged law into four groups and create corresponding dummy variables: regular statutes (reference group), organic laws (*organic*), resolutions (*resolution*) and treaties (*treaty*).

Third, we dispose of additional variables for the 1995-2013 time period. For this time period, we are indeed able to disentangle exactly which Justices attended the decision. We exploit these case-to-case variations to create three variables. First, we compute the exact share of Justices appointed by right-wing parties over the all set of appointed Justices

[10]For right-wing legislatures: 1973-1978, 1978-1981, 1986-1988, 1993-1997, 2002-2007, and 2007-2012. For left-wing legislatures: 1981-1986, 1988-1993, 1997-2002, and 2012-today.

[11]The CC has also the possibility to validate a law, and to give some guidelines for the interpretation of this law. This is called *interprétation avec réserves*. While this legal tool may have some importance, this is not the focus of our article, since these guidelines usually precise the original goal of the government.

[12]The Constitutional Council sometimes issues several decisions on the same day. The variable *sameDay* counts the number of other invalidation decisions made on the same day.

(*shareRW*). We are therefore able to replace *compRW* by the more precise variable *shareRW* for the subset of laws passed between 1995 and 2013. Second, we create two variables that account for the presence of the two former Presidents of the Republic (*giscard* and *chirac*).[13]

Our main variable of interest is *censorship*. As we mentioned above, the classification of the decisions as validation or censorship is achieved by the CC itself.[14] As one can see in table 3.3, the number of decisions per legislature has remained relatively stable from the end of the 70s until the beginning of the 2000s. In the two past legislatures (2002-2007 and 2007-2012), the number of decisions has increased (from 70-80 to 90-100 decisions decisions per five year legislature). This increase in the work of the CC is mainly due to the CC's growing political role. In fact, in the past decades, opposition parties have become more and more prompt to challenge laws before the CC as a last attempt to block the government's projects.

More interestingly, the rate of censorship has considerably varied across legislatures. The highest censorship rate was found during Jospin's legislature (58.2%). The fact that this high censorship rate occurred during a cohabitation period would tend to support the *opportunistic independence* theory. In French politics, cohabitation refers to legislatures during which the President of the Republic and the Prime Minister are not from the same political wing. Franck (2009) has found that censorship rates were higher under cohabitation (for the subsample of left-wing legislatures only).

Note however that cohabitation cannot be the only factor accounting for the high censorship rates. In fact, one can see that the second highest censorship rate was found under Sarkozy's presidency, where the Prime Minister was from the same party. Moreover, the 1986-1988 right-wing legislature, which was also a cohabitation phase, reported a relative low censorship rate (43.2%).

All in all, these results suggest that legislatures may have been heterogenous in the quality of laws they produced, inducing *de facto* different censorship rates.

For an overview of the database, table 3.5 displays the summary statistics of the variables listed in table 3.4. Several remarks can be made in the light of these numbers. First, one can note that the censorship rates are roughly constant across left-wing and right-wing legislatures. Second, we observe that the average proportion of Justices appointed by right-wing parties is higher under right-wing legislatures than under left-wing legislatures. This comes from the fact that, when a party wins the elections, it appoints its own Jus-

[13] As we show in the appendix, former Presidents potentially select cases they attend. To avoid selection problems and to distinguish their role from the behavior of appointed Justices we include these two dummy variables in the set of controls for 1995-2013 decisions.

[14] The decisions can either be: total validation, partial validation, partial censorship, or total censorship. Since partial validation and total censorship concern less than 5% of the cases, we consider only two categories: validation and censorship.

tices in the years following its election. Third, it can be mentioned that, according to the subsample we consider, the most frequent kind of request is either the *joint* request (when both 60 senators and 60 deputies ask for judicial review) or the request by 60 deputies only. In the econometric investigation, we will take requests by 60 deputies as the reference group.

In the appendix, we propose a discussion about the potential time components that may affect our regressions. We show the necessity to include a linear time trend in our analysis.

3.3 Empirical Strategy

We now turn to the empirical investigation of the CC's decisions from 1974 to 2013. Our econometric strategy consists in a series of logistic regressions that aim at explaining censorship decisions (0 if validation, 1 if censorship) in the light of the three theories explained above.

Four categories made up our independent variables: variables for each of the three theories on judicial decision-making presented above, and control variables. Discussion of the expectations implied by the three theories we aim to test is summarized in table 3.1.

Attitudinal Model. First, we include variables that account for ideological or political voting. To assess the relevance of the attitudinal theory, we include several variables.

If Justices vote on a political or ideological basis, we can expect the proportion of Justices appointed by a right-wing party to increase the censorship probability of laws passed by left-wing coalitions. Reversely, we can expect it to have a negative impact on the invalidation likelihood of laws passed under right-wing legislature. To capture this phenomenon, we include in our set of regressors the proportion of Justices appointed by right-wing parties (*compRW* for the full set of decisions, *shareRW* for the 1995-2013 subperiod), and an interaction term with the dummy variable for right-wing laws ($\mathbb{1}_{Right}$). The attitudinal model predicts that this interaction term will be significant and negative: the more Justices appointed by right-wing parties attend a case, the less likely a right-wing law will be censored.

Another prediction of the *attitudinal model* concerns the role played by the Chief Justice. If the Chief Justice is also affected by political or ideological concerns, it may be that he uses his authority to favor laws of the party that appointed him/her. To capture this second effect, we include in our set of explanatory variables a dummy variable for the fact that the Chief Justice was appointed by a right-wing party (*presRW*) and an interaction term with the dummy variable for right-wing laws ($\mathbb{1}_{Right}$). If the attitudinal model holds, we expect the interaction term to be significant and negative (right-wing laws are less likely

to be invalidated when the Chief Justice was appointed by a right-wing party).

Judicial Self-Restraint. Second, we propose to test the relevance of the judicial self-restraint theory in three ways. In a first step, we propose to investigate whether a previous decision has an impact on the censorship likelihood. Indeed, if Justices refrain themselves from invalidating too many laws, they should avoid to censor a law when their last decision was already an invalidation. To capture this phenomenon, we include *previous* in our set of explanatory variables. If the coefficient associated with this variable turns to be negative, it will mean that Justices are reluctant to invalidate a law when their last decision was already a censorship decision.

A second way to test the validity of the judicial self-restraint theory is explained by Schnapper (2010). In her testimony of her stay at the CC, she mentioned the existence of a *censorship quota* per parliamentary session, meaning that Justices are reluctant to invalidate a law if they have already invalidated many laws in the parliamentary session.[15] Following this statement, we propose to test for the significant impact of the number of censorship decisions since the beginning of the parliamentary session (*parliamentary*) on future decisions. This variable aims at representing the pressure described by Schnapper. The judicial self-restraint theory predicts a negative coefficient associated with this variable: the more Justices have censored previously in the parliamentary session, the less likely they will censor future laws of the same parliamentary session.

A third method aiming at testing the judicial self-restraint theory consists in looking at the effects of invalidation on decisions issued on the same day. Indeed: in some cases, the CC can deliberate, and then publish several decisions on the same day. Following the judicial self-restraint theory, one could hypothesize that self-restrained Justices are reluctant to censor too many laws on the same day. This would result in a negative dependence across decisions given the same day: the more Justices have already censored in other decisions, the less likely they will invalidate the law at stake. The judicial self-restraint theory predicts therefore a negative and significant coefficient associated to the *sameDay* variable.

Opportunistic Independence. Third, following previous works on the French CC, we propose two ways to test whether the CC invalidates more laws when the polity is divided. To begin with, our set of independent variables includes a dummy variable, which accounts for the fact that the Senate's majority is aligned with National Assembly's majority (*senate*). Following the opportunistic independence theory, we expect this variable to have a negative coefficient, meaning that the censorship rate decreases when the polity is united (*i.e.* the two Chambers support the government).

[15] "A Chief Justice declared once with humor that the Council had run out of censorship decisions for the year or the parliamentary session, and, therefore, could not afford any additional one [...]" (In French: "Un président avançait parfois, avec humour, que le Conseil avait épuisé son 'quota' de censure pour l'année ou pour la session parlementaire en cours, qu'il ne pouvait plus se permettre d'en imposer une nouvelle [...]") (p.256) Schnapper (2010).

Similarly, we include a dummy variable that accounts for cohabitation periods (*cohabitation*). If the opportunistic independence theory holds, we will expect cohabitation phases to face higher censorship rates. This theory predicts therefore a positive and significant coefficient associated with *cohabitation*.

Finally, our set of explanatory variables comprises some control variables that are likely to influence the CC's decisions. We control for the nature of the law at stake (*organic*, *resolution*, *treaty*), the area of the challenged law (*economics*, *electoral*, *finances*, *institution*, *justice* and *territorial*), the year the law was passed (time trend component or time fixed effects), the number of days between the date judicial review was requested and the date the decision was made (*delay*), and we introduce a dummy variable for the 2008 constitutional reform (*reform*).

We present a series of regressions, which focus on two time periods (1974-2013, and 1995-2013). The general specification writes as follows:

$$Pr(Censorship_i) = F(\beta_0 + \beta_1 \mathbb{1}_{Right_i} + \beta_2 X_i + \beta_3(\mathbb{1}_{Right_i} \times X_i) + \beta_4 W_i + \beta_5 Z_{t(i)}) \quad (3.1)$$

where F(.) is a logistic function, $\mathbb{1}_{Right_i}$ is a dummy equal to 1 when the challenged law was passed under a right-wing legislature, X_i is the vector of political variables, $Z_{t(i)}$ is a vector of time variables, and W_i contains the remaining variables.

Variables included in the vector X_i change from one specification to another. In all specifications, X_i includes $presRW$. In specifications 1 to 3, X_i also includes $compRW$. In specifications 4 to 7, $compRW$ is replaced by $shareRW$. The vector $Z_{t(i)}$ also changes across specifications. In specifications 1 and 4, it includes a linear time trend. In specifications 2 and 5, $Z_{t(i)}$ contains legislature fixed-effects. In specifications 3 and 6, the vector $Z_{t(i)}$ is made of both legislature and year fixed-effects. Finally, in specification 7, $Z_{t(i)}$ contains legislature, year and court fixed effects.[16]

The vector of control variables W_i includes: a dummy variable for the cohabitation (*cohab*), a dummy variable for the alignment of the Senate (*senate*), the CC's previous decision (*previous*), the CC's other decisions made on the same day (*sameDay*), the number of previous censorships since the beginning of the parliamentary session (*parliamentary*), the time the CC took to make the decision (*delay*), a series of dummy variables for the area of law (*economics, electoral, finances, institution, justice* and *territorial*), a series of dummy variables for the nature of the law (*organic, resolution* and *treaty*), and a series of dummy variable for the institution which asked for judicial review (*senators*, *joint*, *presR*, *primeM*, *presS*, and *presA*). Finally, specifications 4 to 7 include two additional variables for the 1995-2013 time period. Indeed, in order to account for the potential effects of the presence of former Presidents of the Republic, we control for the attendance of both

[16]Note that we are unable to run specification 7 with the variable *compRW*, the court fixed effects are collinear with *compRW*.

Giscard d'Estaing and Chirac (*giscard* and *chirac*). As we show in the appendix about case attendance, their presence may however raise some concerns, that is why we propose to include in the specifications 4 to 7.

Theory	Variable	Expected Sign
Attitudinal Model	$\mathbb{1}_{Right} \times compRW$	-
	$\mathbb{1}_{Right} \times shareRW$	-
	$\mathbb{1}_{Right} \times presRW$	-
Self-Restraint Theory	previous	-
	parliamentary	-
	sameDay	-
Opportunistic Independence	senate	-
	cohabitation	+

Table 3.1: Predictions of the impact on censorship regarding the three theories.

3.4 Results

Table 3.6 displays the results of seven specifications (described above). Specifications 1 to 3 deal with all cases from 1974 to 2013. Specifications 4 to 7 focus only on cases decided from 1995 to 2013. Under all specifications, we have reported two statistics: the log-likelihood, and the McFadden's pseudo-R square.

Attitudinal Model. As far as political/ideological voting is concerned, our results point out serious doubts about Justices' political neutrality.

In order to interpret the impact of the composition of the CC on the censorship likelihood, we decompose the effect between laws passed under left-wing legislatures and those passed by right-wing majorities. The coefficient associated to the composition of the CC for left-wing laws is equal to the coefficient of $compRW$, $shareRW$ or $presRW$ without the coefficient associated to the interaction variable. The coefficient for right-wing laws is the sum of the coefficients associated to the previous variables plus the coefficient associated to the interaction term.[17] Table 3.2 summarizes the results.

As far as the proportion of right-wing Justices is concerned, we observe two sets of results. First, when we consider $compRW$ on the full period 1974-2013, we do not detect any impact of the proportion of Justices appointed by right-wing officials on the censorship likelihood whatever the coalition which passed the challenged law. On the contrary,

[17] See Balli & Sørensen (2013) for the interpretation of interaction variables.

Table 3.2: Impact of the CC's composition.

		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(4e)
compRW	Left-wing laws	.	.	.					.
	Right-wing laws	.	.	.					.
shareRW	Left-wing laws				.	.	+	.	
	Right-wing laws				-	-	-	-	
presRW	Left-wing laws	.	+	.	+	+	-	+	.
	Right-wing laws	.	+	-	+	+	-	-	.

Note: We report a plus or minus sign when the p-value is lower than 5%. Otherwise, we report a dot.

specifications 4 to 7 all find evidence supporting the *attitudinal model*: the proportion of Justices who take part to the decision significantly decreases the censorship risks for right-wing laws. This result is robust to the fixed-effects (FE) specifications. Specification 6 which includes year and legislature FE but no court FE also detects a positive impact on left-wing laws: the more Justices appointed by right-wing coalitions attend the decisions for left-wing laws, the more likely these laws are to be censored. This also supports the *attitudinal model*.

In order to test whether the difference between the two sets of results is driven by the reduced period considered in specifications 1 to 7 or by the gain information that is included in *shareRW* compared to *compRW*, we run specification 4 by substituting *compRW* by *shareRW*, which yields specification 4e. This new specification does not reject the null hypothesis for *compRW* unlike specification 4 did with *shareRW* for right-wing laws. This finding indicates that the above results supporting the *attitudinal model* are not driven by the reduced time period but by the additional information contained in *shareRW* compared to *compRW*.

Second, regarding the effect of the fact that the President of the CC was appointed by a right-wing official (*presRW*), we find mixed results. Some specifications fail at detecting any effect (1 and 4e). Some others detect an increase in the censorship likelihood for both right-wing and left-wing laws (specifications 2, 4 and 5). Specification 6 concludes that the variable *presRW* has a negative impact on both types of laws. Only specifications 3 and 7 are aligned with the *attitudinal model*.

Running a *Variance Inflation Factor* analysis, we detect one source of collinearity which may affect previous results, *i.e.* the correlation between *shareRW* and *presRW*.

First, to investigate whether our results are not driven by collinearity between *shareRW* and *presRW*, we compute the correlation coefficient, which turns to be equal to 0.804 (p-value: 0.000). The strong correlation between these two variables can be explained by

the fact that the Chief Justice is appointed by the President of the Republic, and that Justices are also more likely to be appointed by right-wing parties when the Chief Justice is himself/herself chosen by a right-wing President of the Republic.

To discriminate between the respective roles of these two variables, we run a series regressions excluding one of them in turn. Specification 4a excludes $presRW$ and $\mathbb{1}_{Right} \times presRW$, while specification 4b excludes $shareRW$ and $\mathbb{1}_{Right} \times shareRW$. As one can see, results of specification 4 associated to $shareRW$ remains qualitatively identical, and the magnitude of the coefficients is not affected. Results associated to $presRW$ substantially change when $shareRW$ is dropped: they are now consistent with specification 7, and support the *attitudinal model*. Indeed, $presRW$ is associated with significantly more censorship for left-wing laws and with less censorship for right-wing laws.

All in all, these results tend to go in the direction of the *attitudinal theory*. Separate regressions indicate that both the proportion of Justices appointed by right-wing parties and the party that appointed the Chief Justice reduce the censorship probability for right-wing laws. We are however not able to distinguish between the two effects because of the collinearity of our variables. We do not know therefore whether the right-wing appointed Chief Justice or the other right-wing appointed Justices vote in a political or/and ideological manner. Our estimations however suggest that some political/ideological voting happens.

Judicial Self-Restraint. Our estimations seem to confirm the relevance of the *judicial self-restraint theory*. In fact, we observe that two out of the three variables of interest (*previous* and *parliamentary*) have a negative impact on the censorship likelihood (in almost all specifications). These results suggest that Justices are respectively (1) less likely to censor when their last decision was an invalidation, and (2) less likely to censor the more they have censored since the beginning of the parliamentary session. Nevertheless, we don not detect any effect of the number of invalidations given on the same day.

The negative and significant impact associated with *parliamentary* might however be driven by two other factors in addition to judicial self-restraint. First, it might result from the linear time trend specification that we impose in specification 4. One can nevertheless remark that this effect is robust to the other specifications which include year fixed effects instead of a linear time trend. Second, this result might be due to the change in the quality of law *within* a parliamentary session. In such a case, the variable *parliamentary* would simply depict the decrease in quality over months of the parliamentary session. To investigate this issue, we run specification 4 including a linear trend (the number of months since the beginning of the parliamentary session). The resulting estimation rejects the hypothesis that our results were driven by the change in the quality of law within the parliamentary session.[18]

Altogether, these results support the *judicial self-restraint theory*, in the sense that Jus-

[18]The coefficient associated to *parliamentary* remains significant, but the coefficient associated to the linear time of the parliamentary session is not statistically different from zero.

tices are more reluctant to censor the more they have censored in the past. These results seem to confirm Schnapper's theory about the existence of a "censorship quota" (*i.e.* a maximum number of censorship decisions).

Opportunistic Independence. We now turn to the analysis of the two variables related to opportunistic independence: *cohabitation* and *senate*.

First, regarding *cohabitation*, we find that cohabitation periods are associated with more censorship: the coefficient associated to *cohabitation* is indeed positive and significant in specifications 4 to 7 (1995-2013). It is also weakly significant in specifications 2 and 3 (1974-2013). This result is in alignment with the *opportunistic independence* theory.

Second, as far as the *senate* variable is concerned, the picture is more complex. It is indeed associated with a positive impact on the censorship likelihood in almost all specifications of table 3.6. This result would entail that, when the majority of the National Assembly and the majority of the Senate are aligned, the government faces a higher censorship probability. In other words, when both Chambers support the government, the CC is more likely to invalidate a law. This result would not support the *opportunistic independence* theory, but could be explained by the fact that the government passes less consensual -and therefore less constitutional- laws when it controls both Chambers.

However, the coefficient of correlation between *cohabitation* and *senate* in the 1995-2013 period is highly negative (-0.898) and statistically different from zero (p-value = 0.000), which raises serious collinearity concerns. Indeed, cohabitation occurred only once in the 1995-2013 period, and the senate was opposed to the National Assembly during this whole period. To test whether our results are driven by the collinearity of our variables, we proceed as follows: first, we run specification 4 by dropping *cohabitation* (specification 4c), and then by dropping *senate* (specification 4d). Results of table 3.7 show that only *cohabitation* remains significant, which suggests the censorship likelihood is higher under cohabitation, but not necessarily higher when the Senate and the National Assembly are opposed.

Control variables. We now turn to the discussion of the control variables. Table 3.8 in the appendix displays the results of the estimations of specifications 1 to 7.

Few observations can be made in the light of these elements. First, it appears that treaties have been less likely to be invalidated by the CC than the reference group, *i.e.* regular statutes. This result is however driven by the colinearity with *presRep*, whose coefficient is positive and highly significant. This reflects the fact that the President of the Republic asked the CC to review treaties only. Second, we also observe that resolutions, *i.e.* bills dealing with the regulation of the parliamentary activities, have been more likely to be invalidated in the recent years (1995-2013 subsample). Similarly, this second observation results from the high degree of correlation with *presAssem* and *presSenate*, whose associated coefficients are both negative and highly significant in specifications 4 to 7. Third, it appears that laws that have been challenged by both Senators and Deputies are

much more likely to be censored. This result is in line with the *opportunistic independence* theory, since a joint request results in a more isolated government, which may, *in fine*, gives more freedom to the CC to rule against the government. The positive and statistically significant coefficient associated to *joint* can also result from a selection effect: laws which are contested by both Senators and Deputies may also very well be less constitutional *per se*. Lastly, one can note that the area of the challenged law plays little role. Only budget bills face a higher censorship likelihood.[19]

3.5 Discussion

In the preceding analysis, we have found some evidence related to three theories of judicial decision-making: the *attitudinal model*, the *judicial self-restraint theory* and the *opportunistic independence theory*. We also found puzzling elements regarding the attitudinal and the opportunistic independence models.

To sum up our findings, we found that :

1. Justices are influenced by political or/and ideological concerns when ruling on the conformity of laws with the constitution;

2. Justices try to limit the number of censorship decisions;

3. Justices are in general more likely to rule against the government's interests if the Prime Minister and the President of the Republic are not from the same party (cohabitation)

These results are striking because they give a detailed and broad picture of the determinants of the CC's decisions. They show that elements of the three theories play an important role in determining Justices' choices.

As the econometric investigation has showed, political or ideological components affect Justices. As far as political/ideological voting is concerned, it has been shown in the literature that two effects may be at stake (Miles & Sunstein (2008)). First, this finding may be due to the fact that individual Justices are less likely to strike down laws passed by the party that appointed them. Second, it may also be that this first effect is amplified the more Justices appointed by the same party are attending the decision.[20] Such *panel* effects would reinforce the first effect: politicized Justices would be even more politicized when they rule with Justices who share their opinions.

[19]The high degree of censorship for budget bills is explained by the so-called *cavaliers budgétaires*, *i.e.* legislative provisions which are off-topic.

[20]This effect was described in Miles & Sunstein (2008). The authors say: "Democratic appointees show especially liberal voting patterns when sitting on all-Democratic panels; Republican appointees show especially conservative voting patterns when sitting on all-Republican panels."

The large level of political and ideological influence for CC's decisions represents a serious issue, especially since Justices do not seem to be aware of it (Schnapper (2010)).[21] Alternatively, it might be that Justices are perfectly aware of the influence of their political preferences on their decisions, but refuse to acknowledge it.[22] Since most of the appointees have had, at some point, links with political parties, it may be that their political preferences keep influencing their perception. In this respect, the current appointment mechanism may be accountable for the politicization of the CC. Thus, reforms that aim at increasing the level of judicial independence of the CC by changing the appointment process, or by giving more power to the judiciary, would definitively go in the right direction.

Similarly, our investigations suggest that cohabitation may severely increase the risks of invalidation. This result is in line with the judicial self-restraint theory, and directly questions the independence of the CC. If this finding is effectively driven by the decrease of political pressure exerted on the CC when the polity is divided, it suggests that the CC severely suffers from political influence when political power is concentrated. However, we cannot rule out the possibility that this result is driven by a change in the quality of the laws passed during cohabitations. Indeed, the French political system is, during cohabitation, much closer to a parliamentary system, where Deputies play a greater role in the law-making process.[23] The increased role of Deputies may affect the quality of laws, and may, *in fine*, lead to more censorship decisions. We believe however that this alternative theory is less likely to hold than the *opportunistic independence theory*, since majoritarian parties should seek to pass more extreme laws when they control both the executive and the legislative branches, which should lead to lower censorship rates during cohabitation.

One of the most innovative contributions of our work lies in the detection of judicial self-restraint. However, motivations for such a behavior are not clear, and it is very likely that Justices are not totally aware of this phenomenon. So far, we see two reasons that may explain Justices' behavior. First, their reluctance for invalidation may be due to their willingness to respect decisions of a legitimately elected Parliament (while they are only

[21]Schnapper argues in fact that Justices do not engage in political or ideological voting since it would damage their credibility inside the CC. More specifically, she writes: "Jean-Claude Colliard noticed that a Justice who would keep voting according to the interests of the party that appointed him/her would lose credibility with his/her colleagues". (In French: "Jean-Claude Colliard fait remarquer qu'un conseiller qui voterait systématiquement en faveur des positions de celui qui l'a nommé se déshonnorerait vis-à-vis de ses collègues.") (p.304) Schnapper (2010).

[22]Troper (2007) underlines that Justices are incentivized to declare that their have a very limited discretionary power, in order to increase the legitimacy of their decisions. He says : "[...] his legitimacy appears contestable if it was admitted -or if he admitted himself- that decisions were made on a discretionary basis. [...] One needs that the judge believes, pretend to believe, or make other believe that he strictly enforces pre-existing law. (In French: "[...] sa légitimité apparaîtrait contestable si l'on admettait -et surtout s'il admettait lui-même- qu'il n'y a de sens que celui qui résulte d'une décision arbitraire. [...] Il faut donc que le juge croit, feigne de croire ou fasse croire, qu'il se borne à appliquer un droit préexistant.")

[23]Under cohabitation, the government cannot rely on the support of the President of the Republic. In order to maximize its support, it generally gives a greater role to Deputies in the law-making process.

appointed Justices). As Troper (2007) mentions, constitutional judges may increase their legitimacy if they appear to limit the scope of their decisions to legal issues only, and not to intervene on political matters.[24] Second, as Schnapper (2010) claims, it could be due to their fear of a reform of the CC, and therefore to a lack of independence.[25] It is very likely that, in reality, both reasons influence Justices' choices. Here again, the lack of independence of the CC is very likely to influence Justices' decisions. Nevertheless, we cannot completely exclude the possibility that these findings are driven by a change in the quality of law. The negative influence of the previous decision may simply reflect the fact that the majority exerts more care for the future law when it has faced a recent censorship. Although we are not able to control for the strategic anticipation of the government, we believe that this may only play a limited role: because of the length of the legislative process, the government can hardly anticipate future decisions of the CC when it drafts the main legal provisions of a new bill. Moreover, regarding the second effect associated to the judicial self-restraint theory, *i.e.* the decrease of the censorship probability due to the number of previous invalidations in the parliamentary session, one could suspect it to be driven by a steady increase in the quality of laws over the parliamentary session. In the above section, we commented on an alternative specification we run to control for this potential issue by including a linear trend in the parliamentary session. We concluded that our results were not driven by a change of quality over the parliamentary session.

Limitations. Although these results raise serious questions about the French institutional framework, they must be interpreted with caution mainly because of the limited availability of the data.

First, as far as political/ideological voting is concerned, individual votes are not available, which refrains us from investigating individual voting patterns. Moreover, the collinearity concerns raised in the econometric analysis prevents us to distinguish what is due to regular Justices and what is due to the Chief Justice. So far, we are able to say that *something happens* in the black box of the CC, but we cannot say *what* happens. Second, the absence of individual votes prevents us from investigating related concerns, such as panel effects. For instance Cameron & Kornhauser (2010) have shown that collective judicial decision-making may have specific characteristics different from those commonly hold in legislative studies. Repeated interactions between Justices may for instance lead them to negotiate with each other. The authors emphasize indeed that standard models make a separability assumption, which "*insures that each judge's decision on a case is independent*

[24]Troper (2007) writes: "A judge [...] who bows to the Parliament shows that he will not intervene on political matters and that his role limits to the strict enforcement of the constitution." (In French: "Un juge [...] qui s'incline devant le Parlement montre qu'il ne s'aventure pas sur le terrain politique et que son rôle se limite à l'application de la constitution.")

[25]"Worries about the destiny of the institution -whose -legitimacy has never been fully recognized and about which politicians keep debating- force Justices to remain cautious." (In French: "L'inquiétude sur le destin d'une institution dont la légitimité n'est jamais totalement établie, et à propos de laquelle des reformes ont toujours été débattues, contribue à rendre les conseillers prudents.") Schnapper (2010).

of her decision on other cases". This assumption is however very likely to be violated in case of collective decision-making. Epstein et al. (2011) also show that dissent aversion may decrease Justices' willingness to depart from the group's dominant opinion.

Another possible challenge to the previous analysis lies in the possible strategic interactions from the government's side. In fact, if the government were aware of the factors that might influence the CC's decisions, it could try to strategically change its agenda, or to modify the content of its laws. In parallel to this work, we have tried to address this issue. To do so, we have collected data of public scrutinies for the Lower Chamber from 1997 to 2012, and we have run several regressions to figure out whether the level of political consensus of laws was affected by the decisions or by the composition of the CC. So far, all our attempts failed at detecting any strategic interaction from the government's side. Still, we believe that such an investigation should be part of a future research project. Let us finally note that, any strategic moves from the government's side would only strengthen our results. In fact, if the government were passing more consensual laws when facing a politically more opposed CC, it would reduce its censorship likelihood. Since we find some evidence for political/ideological voting, it would mean that the actual level of political/ideological voting would be even worse. To this respect, the results we reported above can be seen as a lower bound.

Aside from this two limitations, our empirical strategy face other limits. First, our analysis focused on one dimension of the constitutional review. In fact, we focused on how the CC reports its decision (censorship or conformity), but we neglected more detailed classifications (e.g. constitutional interpretation by the CC). Second, we attempted to capture as many components of the reviewed laws as possible. We controlled indeed for the type of law at stake (treaty, Parliament's rules, organic laws, regular statutes), the delay of the decision, and finance laws but it may be that these controls were not sufficient to capture core issues which may affect censorship. Finally, it might be that Justices select, at least partially, cases they attend. We proposed in the appendix a discussion to investigate this issue. However, the limited availability of the data makes it very difficult to ensure that Justices do not select cases.

3.6 Conclusion

This article aimed at investigating the relevance of three theories of judicial decision-making for the French Constitutional Council: the *attitudinal model*, the *judicial self-restraint* theory, and the *opportunistic independence* theory. To disentangle the role of these three theories, we analyzed rulings of the Constitutional Council from 1974 to 2013, and, more specially, from 1995 to 2013.

We found some evidence supporting the three theories, although we were not able to disentangle the specific roles of the Chief Justice and the regular Justices for the *attitudinal model*. All in all, we found (1) that Justices are influenced by political/ideological concerns when deciding on the conformity of laws with the constitution, (2) that they are reluctant

to invalidate laws voted by the Parliament, and (3) that the Constitutional Council is more independent when the Prime Minister and the President of the Republic are not from the same party.

In the last section, we discussed the implications of our findings. We concluded that our results support the three theories on judicial decision-making, which urges for an institutional reform of the Constitutional Council. We underlined indeed that political/ideological voting was likely to occur because of the appointment process. Moreover, we stressed that judicial self-restraint may also be driven by the fear of a reform of the Constitutional Council, and, therefore by the lack of independence. Finally, we noted that higher risks of censorship under cohabitation is very likely to reflect the pressure on the Constitutional Council that occurs when the President of the Republic and the Prime Minister are from different party.

The discussion also acknowledged some limits to our approach. First, data availability makes the study of the French Constitutional Council more difficult to replicate studies made in other countries. Our results should therefore be interpreted with caution, since some panel effects and related concerns might be at stake. Second, the analysis of Supreme Courts' decisions is always made at the political equilibrium. Other theories, such as the *strategic model*, may account for other strategic interactions that we did not consider here. Our results might be partially driven by unobserved changes in the inherent quality of laws. Third, part of our empirical strategy has relied on the variations of the number of Justices from one decision to another. Our results are therefore highly dependent on the assumption that changes in the attendance are purely random, and are not motivated by political, strategic or ideological concerns. Also case selection is discussed in the appendix, we cannot fully ensure that judges do not select cases they attend.

All in all, we believe that the linkages between the CC and the central political actors are too strong. In our view, an institutional reform should seek to increase the level of independence of the CC by changing the appointment process and by giving more power to the judiciary. In this respect, the 2008 constitutional reform, which gave the CC the possibility of concrete review, leads to the same policy recommendation: the increase in the complexity of the work of the CC following from concrete reviews also urges for an increase in the proportion of legal experts sitting at the CC, at the expense of former politicians.

Appendix 3.A: Time Components

Decisions of the CC can somehow be seen as a time series. In fact, we observe decisions of a unique actor over years, and usual challenges faced by time series may also apply here. A fundamental limitation, however, consists in the fact that decisions of the CC are not done on a regular basis (e.g. monthly, yearly), and that several decisions are sometimes published on the same day. To disentangle what is due to potential trend and cyclical components and what is due to the real effects of other explanatory variables, we propose to first have a first look at the evolution of the censorship rates over time.

Figure 3.1 displays the yearly average censorship rate since 1974. In order to distinguish between cyclical components and long-trend components, we use a Hodrick-Prescrott filter. The trend component is presented in figure 3.2. As we can see, the level of censorship has globally increased over years. Concerning the yearly cyclical component, figure 3.3 displays the average censorship rate per year of legislature. No real pattern can be observed from the data.

Moreover, we also propose to investigate whether there exists a monthly trend or/and monthly cycles. To do so, we present the average monthly censorship rate in figure 3.4. As we can see, no clear pattern appears from the data. One can note that the relative high censorship rate in December is due to the budget bills, which always face high risks of censorship.

Altogether, the only time component we are able to detect from the data is the global increase in censorship rate over years.

Appendix 3.B: Robustness Checks, Autocorrelation, and Regression Diagnosis

Robustness Checks

In order to control whether our results are driven by specific coding procedures or specifications, we have explored alternative solutions. First, as far as the econometric specification is concerned, we have also run probit and linear probability models, and results were qualitatively identical.

Second, we also proposed an alternative coding for Justice Bazy-Malaurie. In fact, while Justice Bazy-Malaurie was originally appointed by Bernard Accoyer (right-wing), she was reappointed by Claude Bartolone (left-wing) at the beginning of 2013. We have therefore computed two versions of our indicators: one in which Justice Bazy-Malaurie remains a right-wing appointed Justice and one in which she switches to left-wing appointed Justice after her reappointment. Results were nearly identical, since the correlation coefficient was close to 1.

Autocorrelation

A potential bias could emerge in our estimations if the error terms were correlated over time. Indeed: even if our dataset cannot be seen as a true time series (some days have more than one decision), the structure is nevertheless very close. This very special structure allowed us to create variables close to lag values of the dependent variable: *previous* is the average censorship rate of the last decision day.

Biases could emerge in our estimation if the error terms were correlated over time. To deal with this potential issue, we now consider the data as a time series, in which the decisions' publication order corresponds to the time component. We replace the variable *previous* by the lag value of the dependent variable (*L.censorship*). When estimating this new model, we estimate a coefficient associated to *L.censorship* equal to -0.4465. This coefficient is statistically different from zero at the 5% level, and statistically not different from what we found for *previous*. The coefficients associated to both variables have indeed same sign, the same significance level, and the same magnitude.

Second, to detect potential serial correlation between our error terms, we compute the residuals of this last model (without clustering). The correlation coefficient between the error term and the lag value of the error term is equal to -0.008, which suggests no serial correlation. Moreover, graph 3.6 plots the error terms with the lag values of the error terms. The graph detects no autocorrelation.[26]

Third, in order to run the *Breusch-Godfrey* test, we run the previous model in a linear form (OLS regression) without clusters.[27] The probability to rightfully reject the null hypothesis of the Breusch-Godfrey test is equal to 0.7129. The test fails therefore at rejecting the no-serial correlation hypothesis, which comforts results presented in the article.

Logit Diagnosis

Apart from collinearity, which was addressed in the above discussion, we now present several tests to check the validity of our estimations. As a baseline, we will focus on the specification of column 4 of table 3.6 (1995-2013 time period with a linear trend). To check the robustness of our specification, we proceed to several tests as mentioned by Peng et al. (2002).

First, we proceed to a link test, which aims at testing whether our model is well specified. It regresses the censorship decisions on the predicted values and the square root of the predicted values. If the predicted values turn to be significant, this entails that the model is not entirely misspecified. If the square root of the predicted values is highly significant, this implies that the model misses some important independent variables. Running the

[26] If serial correlation were to be positive, we should observe points in the upper-right and the lower-left parts of the graph. On the contrary, if serial correlation was negative, we should observe points in the lower-right and the upper-left parts of the graph.

[27] We cannot use the *Durbin-Watson* statistics because our set of independent variables includes a lag value of the dependent variable.

link test on the logistic regression of column 4 yields to a significative coefficient for the predicted values (at the 1% level), and to a non-significant coefficient for the square value of the predicted values at the 10% level. This result confirms the quality of our estimation.

Second, we propose to run the Hosmer and Lemeshow's goodness-of-fit test. This test aims at measuring the match between the predicted probabilities and the binary outcomes. Failing to reject the null hypothesis supports the empirical model. In our case, the probability to rightfully reject the null hypothesis is equal to 0.1829, which validates our estimation.

Moreover we propose to evaluate the prediction probabilities of our logit estimation, using the ROC curve analysis. The underlying idea of this instrument is to measure the number of false positive and of false negative that the estimation produces. The explanatory power of the regression to discriminate between censorship and validation decisions is represented by the area under the curve of the graph 3.7 in the appendix. As we can see, the area under the curve is close to 0.8, which indicates a good quality of the predicted values.

Last but not least, we look at the possibility that few outliers drive our results. To do so, we plot three statistics associated to each observation: the Pearson residuals (figure graph 3.8), the deviance residuals (figure 3.9), and the Pregibon leverage (figure 3.10).

As one can see from figure 3.10, three observations stand out (352, 491, 492). In order to verify these outliers do not drive our results, we run our baseline model without these observations. Results were not affected by the drop of these three observations, which supports the previous findings.

Appendix 3.C: Case Selection by Justices

The issue of case selection has been a major concern in the Law and Economics literature, since cases which effectively reach courts may not be a representative sample of all conflicts that emerged. Some works have also addressed the issue of case selection by *judges* themselves, when cases are not randomly assigned within a court (Shayo & Zussman (2011)).

In our case, the very question of case selection by Justices comes from the fact that Justices do not attend every single case. The variation in the attendance rate of the CC's Justices is useful to estimate the intensity of *political/ideological voting*. However, if Justices were to select cases they attend, and to miss cases they are willing to avoid, our estimations related to *shareRW* would be underestimated.

To instigate case selection by Justices, we present Justices' attendance rates at the

CC decisions in table 3.9.[28] The first column displays the average attendance rate for each judge. In the second and third columns, we show attendance rates respectively for conformity and censorship decisions. The last column presents the p-values of the two-group mean-comparison tests (for conformity and censorship decisions).

First of all, we can remark that, except for the two former Presidents of the Republic, attendance rates are above 90% for all Justices. Among the 30 regular Justices above 90%, 25 attended more than 95% of the cases. This suggests that, if case selection occurs, it is very limited in its frequency.

Looking at the fourth column, one can notice that no Justice is actually below the 5% threshold, which would suggest case selection. It seems that Justices do not attend more regularly censorship decisions than conformity decisions. A potential concern could emerge from Justice Veil, who is very close to the threshold. In fact, she attended all censorship decisions but only 95.8% of the conformity decisions.

To further investigate potential case selection, table 3.10 displays censorship rates when Justices attended the cases. The three first columns show censorship probabilities for decisions attended by Justices per category of law: (1) all laws, (2) laws voted by right-wing coalitions, and (3) laws voted by left-wing coalitions. The last column presents the p-values associated to the two-group mean-comparison tests for the level of censorship of cases attended for the two subsamples (laws voted by right-wing and by left-wing majorities).

As one can note, no regular Justice is below the 5% threshold in the last column. The former President of the Republic Giscard d'Estaing has a p-value below the threshold. This implies that Giscard d'Estaing attended more censorship decisions under right-wing legislatures than under left-wing legislatures.

Two regular Justices are very close to the 5% threshold, namely Justice Ameller and Justice Pelletier. As far as Justice Ameller is concerned, one can observe from table 3.9 that he attended all cases, which implies that this difference in censorship is not due to case selection. Second, Justice Pelletier attended all cases but one, which also shows that she did not select cases she attended.

Remark Regarding the data available to this date, it seems that appointed Justices do not select cases they attend.

[28] Although comparing the censorship likelihood of attended and unattended cases is not the best way to ensure that case selection does not occur, it is however the most appropriate way given the availability of the data.

Appendix 3.D: Figures and Tables

Legislature	Color	Cohabitation	Number of decisions	Censorship rate
1974-1978	Right-wing	No	35	.200
1978-1981	Right-wing	No	36	.278
1981-1986	Left-wing	No	77	.442
1986-1988	Right-wing	Yes	37	.432
1988-1993	Left-wing	No	75	.400
1993-1997	Right-wing	Partly	70	.429
1997-2002	Left-wing	Yes	68	.588
2002-2007	Right-wing	No	93	.495
2007-2012	Right-wing	No	98	.571
2012-2013	Left-wing	No	23	.391
		Total	612	.454

Table 3.3: Number of censorships and censorship rates by legislature.

Variable Name	Description
censorship	Dummy variable equal to one if the CC invalidated the law.
compRW	Proportion of Justices appointed by right-wing parties.
presRW	Dummy variable equal to one if the President of the CC was appointed by a right-wing party.
senate	Dummy variable equal to one if the majorities of the Senate and the National Assembly are aligned.
cohabitation	Dummy variable equal to one in case of cohabitation, i.e. when the Prime Minister and the President of the Republic are not from the same party.
previous	Dummy variable equal to one if the previous decision was a censorship. If several decisions were made on the same day, the variable is equal to the share of censorship decisions.
parliamentary	Number of previous censorship decisions made since the beginning of the parliamentary session.
sameDay	Number of (other) censorship decisions made the same day.
delay	Number of days between the request for judicial review and the decision.
deputies	Dummy variable equal to one when the judicial review was requested by 60 deputies and by 60 deputies only (reference group).
senators	Dummy variable equal to one if judicial review was requested by 60 senators and by 60 senators only.
joint	Dummy variable equal to one if judicial review was requested by both 60 deputies and 60 senators.
presRep	Dummy variable equal to one if judicial review was requested by the President of the Republic.
primeMin	Dummy variable equal to one if judicial review was requested by the Prime Minister.
presSenate	Dummy variable equal to one if judicial review was requested by the President of the Senate.
presAssem	Dummy variable equal to one if judicial review was requested by the President of the National Assembly.
reform	Dummy variable equal to one if the decision was made after the 2008 constitutional reform.
$\mathbb{1}_{Right}$	Dummy variable equal to one if the law was passed under a right-wing legislature.
economics	Dummy variable equal to one if the law implements economic reforms.
electoral	Dummy variable equal to one if the law contains electoral provisions.
finances	Dummy variable equal to one if the law is a budget bill.
institution	Dummy variable equal to one if the law modifies the institutions.
justice	Dummy variable equal to one if the law is about the judiciary system.
territorial	Dummy variable equal to one if the law targets local administrative entities.
regular	Dummy variable equal to one if the law is a regular bill (reference group).
organic	Dummy variable equal to one if the law is an organic bill.
resolution	Dummy variable equal to one if the law is a resolution.

Variable Name	Description
treaty	Dummy variable equal to one if the law is a treaty.
shareRW	Proportion of Justices appointed by a right-wing party among those who attend the decision.
giscard	Dummy variable equal to one if former President Giscard d'Estaing attended the decision.
chirac	Dummy variable equal to one if former President Chirac attended the decision.

Table 3.4: List of variables.

	1974-2013						1995-2013			
	All Laws		Right Laws		Left Laws		Right Laws		Left Laws	
Variable	Mean	Stand. Dev.	Mean	Stand. Dev.	Mean	Stand. Dev.	Mean	Stand. Dev.	Mean	Stand. Dev.
censorship	.454	.498	.447	.498	.465	.5	.514	.501	.538	.501
$\mathbb{1}_{Right}$	.603	.49	1	0	0	0	1	0	0	0
compRW	.719	.227	.77	.23	.642	.2	.814	.16	.675	.18
presRW	.562	.497	.71	.454	.337	.474	.884	.321	.604	.492
cohabitation	.245	.43	.222	.416	.28	.45	0	0	.747	.437
senate	.621	.486	.967	.178	.095	.293	.944	.23	.253	.437
sameDay	.301	.519	.301	.525	.3	.511	.282	.509	.242	.431
previous	.452	.453	.436	.454	.477	.451	.489	.457	.559	.459
parliamentary	3.768	2.961	3.913	3.302	3.547	2.339	4.912	3.486	3.857	2.593
delay	17.913	8.298	17.214	8.271	18.979	8.242	14.921	7.638	18.2	8.122
finances	.116	.321	.106	.308	.132	.339	.093	.291	.165	.373
organic	.18	.384	.209	.407	.136	.343	.199	.4	.187	.392
resolution	.087	.281	.081	.274	.095	.293	.083	.277	.055	.229
treaty	.02	.139	.014	.116	.029	.168	.019	.135	.044	.206
senators	.124	.33	.095	.293	.169	.375	.074	.262	.154	.363
joint	.276	.447	.263	.441	.296	.458	.329	.471	.341	.477
presRep	.016	.127	.011	.104	.025	.156	.014	.117	.044	.206
primeMin	.201	.401	.233	.423	.152	.36	.208	.407	.198	.401
presSenate	.044	.206	.043	.204	.045	.208	.042	.2	.011	.105
presAssem	.046	.209	.043	.204	.049	.217	.046	.211	.044	.206
year	1995.4	10.996	1997.1	11.863	1992.8	8.953	2005.8	4.574	2002.7	5.778
economics	.142	.349	.132	.339	.156	.364	.165	.372	.143	.352
electoral	.094	.293	.1	.3	.086	.282	.069	.254	.11	.314
finances	.124	.33	.108	.311	.148	.356	.101	.302	.165	.373
institution	.078	.269	.089	.285	.062	.241	.101	.302	.066	.25
justice	.055	.229	.067	.251	.037	.189	.046	.21	.033	.18
resolution	.086	.281	.081	.273	.095	.293	.083	.276	.055	.229
territorial	.062	.241	.051	.221	.078	.269	.05	.219	.055	.229
shareRW							.821	.175	.706	.155
giscard							.347	.477	.165	.373
chirac							.144	.351	.011	.105

Table 3.5: Summary Statistics.

Table 3.6: Logit Estimation of the Censorship Decision.

Specification	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Period	1974 -2013	1974 -2013	1974 -2013	1995 -2013	1995 -2013	1995 -2013	1995 -2013
$compRW$	0.0815	-1.680*	4.035				
	(1.155)	(0.926)	(4.618)				
$\mathbb{1}_{Right} \times compRW$	0.129	1.906	5.307				
	(0.958)	(1.629)	(9.283)				
$shareRW$				0.377	1.283	7.883**	-6.235*
				(0.972)	(1.615)	(3.582)	(3.496)
$\mathbb{1}_{Right} \times shareRW$				-9.543***	-12.87***	-17.40***	-5.996
				(2.267)	(3.180)	(3.519)	(4.694)
$presRW$	-0.00865	0.809***	-0.559	0.462**	0.893***	-13.02***	4.576***
	(0.535)	(0.310)	(1.510)	(0.201)	(0.215)	(1.133)	(0.567)
$\mathbb{1}_{Right} \times presRW$	0.197	0.532	-17.07***	1.452**	6.113***	-8.970***	-10.86***
	(0.765)	(0.437)	(4.086)	(0.624)	(1.146)	(0.738)	(1.193)
$cohab$	0.347	0.400*	1.922*	6.812***	2.867***	21.70***	18.61***
	(0.224)	(0.210)	(1.018)	(0.823)	(0.274)	(1.862)	(1.535)
$senate$	-0.665	0.943***	1.645***	1.697***	1.249***	1.811***	1.323**
	(0.700)	(0.238)	(0.626)	(0.304)	(0.227)	(0.362)	(0.522)
$sameDay$	-0.135	-0.132	-0.318*	-0.267	-0.301	-0.523	-0.512*
	(0.153)	(0.161)	(0.192)	(0.229)	(0.209)	(0.325)	(0.294)
$previous$	-0.199*	-0.276**	-0.711***	-0.263**	-0.263*	-0.632***	-0.832***
	(0.119)	(0.137)	(0.133)	(0.117)	(0.156)	(0.0915)	(0.132)
$parliamentary$	-0.0557	-0.0894*	-0.135**	-0.0985***	-0.0805***	-0.149***	-0.127***
	(0.0460)	(0.0484)	(0.0585)	(0.0209)	(0.0127)	(0.0316)	(0.0435)
Linear Trend	Yes	No	No	Yes	No	No	No
Year FE	No	Yes	Yes	No	Yes	Yes	Yes
Legislature FE	No	No	Yes	No	No	Yes	Yes
Court FE	No	No	No	No	No	No	Yes
Observations	611	611	611	306	306	306	297
Log-likelihood	-353.21	-348.64	-322.17	-166.40	-166.52	-152.55	-144.23
Pseudo-R^2	0.1611	0.1719	0.2348	0.2143	0.2137	0.2797	0.2977

Note: All specifications include information about the nature of the challenged law (*organic, treaty,* and *resolution*), about the content of the law (*economics*, *electoral*, *finances*, *institution*, *justice* and *territorial*), and about the time between the review was requested and the decision of the CC (*delay*). Specifications 1 and 4 also include a dummy variable for the 2008 reform (*reform*).
Clustered standard errors in parentheses.

Table 3.7: Logit Estimation of the Censorship Decision: **Robustness Checks**

Specification	(4a)	(4b)	(4c)	(4d)	(4e)
Period	1995 -2013	1995 -2013	1995 -2013	1995 -2013	1995 -2013
compRW					0.6233
					(1.563)
$\mathbb{1}_{Right} \times compRW$					-2.72
					(2.864)
shareRW	1.259		1.663	0.860	
	(0.972)		(1.473)	(0.861)	
$\mathbb{1}_{Right} \times shareRW$	-7.462***		-6.329***	-9.256***	
	(1.657)		(1.986)	(2.133)	
presRW		0.686**	0.441	0.504**	.4701
		(0.300)	(0.471)	(0.251)	(0.400)
$\mathbb{1}_{Right} \times presRW$		-1.482**	2.626**	1.802**	-.829
		(0.742)	(1.135)	(0.752)	(0.515)
cohab	7.173***	5.428***		4.070***	6.040***
	(0.749)	(0.979)		(0.661)	(1.349)
senate	1.817***	1.476***	-0.787		1.421***
	(0.225)	(0.389)	(0.745)		(0.290)
sameDay	-0.242	-0.182	-0.260	-0.270	-0.191
	(0.223)	(0.191)	(0.188)	(0.221)	(0.192)
previous	-0.222*	-0.232**	-0.125	-0.238*	-0.228**
	(0.114)	(0.0988)	(0.152)	(0.124)	(.1101)
parliamentary	-0.101***	-0.101***	-0.0238	-0.0618**	-0.100***
	(0.0198)	(0.0286)	(0.0234)	(0.0298)	(0.0273)
Linear Trend	Yes	Yes	Yes	Yes	Yes
Year FE	No	No	No	No	No
Legislature FE	No	No	No	No	No
Court FE	No	No	No	No	No
Observations	306	306	306	306	306
Log-likelihood	-167.12	-169.15	-172.93	-168.18	-169.04
Pseudo-R^2	0.2109	0.2013	0.1835	0.2059	0.2018

Note: All specifications include information about the nature of the challenged law (*organic, treaty,* and *resolution*), about the content of the law (*economics*, *electoral*, *finances*, *institution*, *justice* and *territorial*), and about the time between the review was requested and the decision of the CC (*delay*).
Clustered standard errors in parentheses.

Table 3.8: Control variables of Table 3.6.

Specification	(1)	(2)	(3)	(4)	(5)	(6)	(7)
organic	-0.879	-1.023	-0.834	-0.139	-0.257	-0.721	-0.305
	(0.695)	(0.663)	(0.681)	(1.192)	(1.205)	(1.343)	(1.235)
resolution	-0.976	-0.756	-0.847	16.11***	16.18***	17.08***	19.67***
	(2.343)	(2.953)	(2.739)	(1.450)	(1.577)	(1.415)	(1.408)
treaty	-13.99***	-13.84***	-16.10***	-14.82***	-14.46***	-16.90***	-18.25***
	(0.880)	(0.848)	(1.000)	(1.227)	(1.213)	(1.333)	(1.504)
senators	-0.225	-0.226	-0.313	-0.818***	-0.698***	-0.750***	-0.832***
	(0.201)	(0.230)	(0.243)	(0.178)	(0.233)	(0.244)	(0.321)
joint	0.901***	0.902***	0.958***	0.836***	0.852***	0.962***	0.820***
	(0.193)	(0.198)	(0.276)	(0.233)	(0.227)	(0.300)	(0.288)
presRep	14.70***	14.65***	17.14***	16.88***	16.36***	19.62***	20.55***
	(1.305)	(1.244)	(1.458)	(1.645)	(1.772)	(1.707)	(1.855)
primeMin	-0.0663	0.0769	0.0354	-1.205	-0.978	-0.328	-0.793
	(0.747)	(0.728)	(0.725)	(1.110)	(1.122)	(1.227)	(1.097)
presSenate	0.153	-0.112	-0.126	-18.09***	-18.25***	-19.18***	-21.97***
	(2.638)	(3.269)	(3.091)	(0.955)	(1.154)	(1.013)	(0.792)
presAssem	-0.303	-0.525	-0.601	-16.29***	-16.38***	-17.66***	-20.28***
	(2.212)	(2.834)	(2.630)	(1.256)	(1.374)	(1.340)	(1.470)
economics	-0.332	-0.407**	-0.285	0.0160	-0.0548	0.0483	-0.0751
	(0.207)	(0.203)	(0.217)	(0.298)	(0.300)	(0.279)	(0.268)
electoral	-0.564*	-0.635*	-0.613	-0.196	-0.183	-0.0223	0.0470
	(0.327)	(0.335)	(0.391)	(0.572)	(0.565)	(0.618)	(0.540)
finances	1.452***	1.393***	1.631***	1.951***	1.859***	2.215***	2.105***
	(0.182)	(0.193)	(0.180)	(0.446)	(0.474)	(0.405)	(0.449)
institution	-0.00658	-0.136	-0.415	0.623	0.482	0.253	0.137
	(0.326)	(0.311)	(0.352)	(0.512)	(0.519)	(0.488)	(0.506)
justice	0.399	0.443	0.389	1.083	1.164	1.046	0.839
	(0.536)	(0.514)	(0.529)	(0.957)	(0.967)	(0.794)	(0.985)
territorial	0.384	0.295	0.387	1.256	1.191	1.607	1.518**
	(0.413)	(0.439)	(0.489)	(0.862)	(0.891)	(0.995)	(0.758)

Note: Standard errors in parentheses, clustered by legislature.
*** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$.

Table 3.9: Attendance rates per Justice (1995-2013).

	(1) All decisions	(2) Conformity	(3) Censorship	(4) P-value
Abadie	.962	.925	1	.087
Ameller	1	1	1	.
Barrot	.986	1	.971	.321
Bazy	.984	.967	1	.306
Belloubet	1	1	1	.
Cabannes	1	1	1	.
Canivet	.934	.929	.938	.829
Charasse	.957	1	.914	.083
Chirac	.508	.519	.5	.887
Colliard	.981	.972	.988	.474
Dailly	1	1	1	.
Denoix de Saint Marc	.992	.982	1	.283
Debre	.992	.982	1	.283
Dumas	.933	.92	.95	.697
Dutheillet	.987	.972	1	.121
Faure	.935	.95	.909	.67
Giscard d'Estaing	.508	.439	.568	.087
Guena	.99	.98	1	.292
Guillenchmidt	.947	.949	.946	.943
Haenel	.957	.943	.971	.562
Joxe	.904	.861	.941	.09
Lancelot	.97	.97	.971	.983
Lenoir	.987	.975	1	.333
Maestracci	1	1	1	.
Mazeaud	.981	.972	.988	.474
Pelletier	.985	.967	1	.277
Pezant	.956	.941	.968	.487
Robert	1	1	1	.
Rudloff	1	1	1	.
Schnapper	.956	.972	.942	.359
Steinmetz	.988	.987	.989	.894
Veil	.981	.958	1	.059

Note: To avoid low attendance rates due to illness leading to resignation, we assumed mandates began on the first day Justices sit and ended on the last day they sit.

Table 3.10: Censorship rates for decisions attended by Justices.

	(1) All laws	(2) Right Laws	(3) Left Laws	(4) P-value
Abadie	.507	.391	.558	.189
Ameller	.5	.407	.582	.05
Barrot	.493	.532	.409	.349
Bazy	.525	.59	.409	.181
Belloubet	.333	.	.333	.
Cabannes	.355	.36	.333	.906
Canivet	.54	.564	.421	.259
Charasse	.485	.533	.381	.255
Chirac	.563	.563	.	.
Colliard	.543	.5	.607	.2
Dailly	.385	.385	.	.
Debre	.542	.571	.409	.17
Denoix de Saint Marc	.542	.571	.409	.17
Dumas	.452	.375	.556	.255
Dutheillet	.551	.535	.714	.201
Faure	.345	.333	.4	.785
Giscard d'Estaing	.6	.645	.357	.044
Guena	.534	.444	.582	.185
Guillenchmidt	.543	.547	.5	.757
Haenel	.507	.556	.409	.267
Joxe	.563	.547	.714	.233
Lancelot	.508	.357	.549	.209
Lenoir	.494	.36	.558	.107
Maestracci	.333	.	.333	.
Mazeaud	.543	.5	.61	.187
Pelletier	.554	.441	.677	.057
Pezant	.56	.56	.	.
Robert	.355	.36	.333	.906
Rudloff	.25	.25	.	.
Schnapper	.536	.518	.714	.163
Steinmetz	.548	.555	.462	.519
Veil	.55	.505	.617	.181

Note: Missing values are due to the fact that some Justices served only under left-wing or right-wing legislatures.

Figure 3.1: Average Censorship Rate per year since 1974

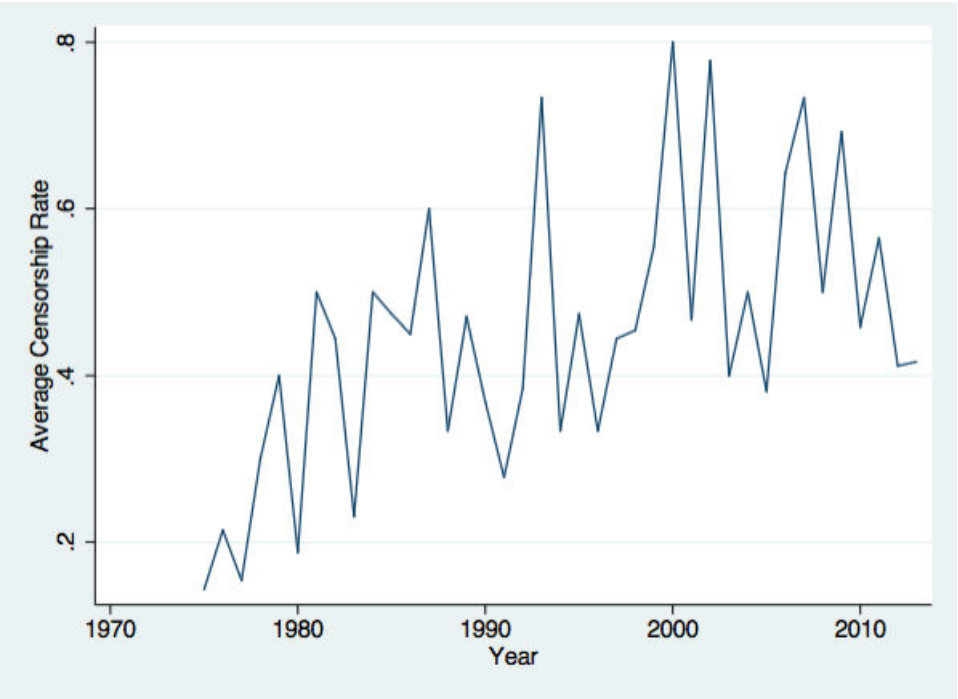


Figure 3.2: Trend Component of the Censorship Rate per year since 1974

Figure 3.3: Average Censorship Rate per Year of Legislature

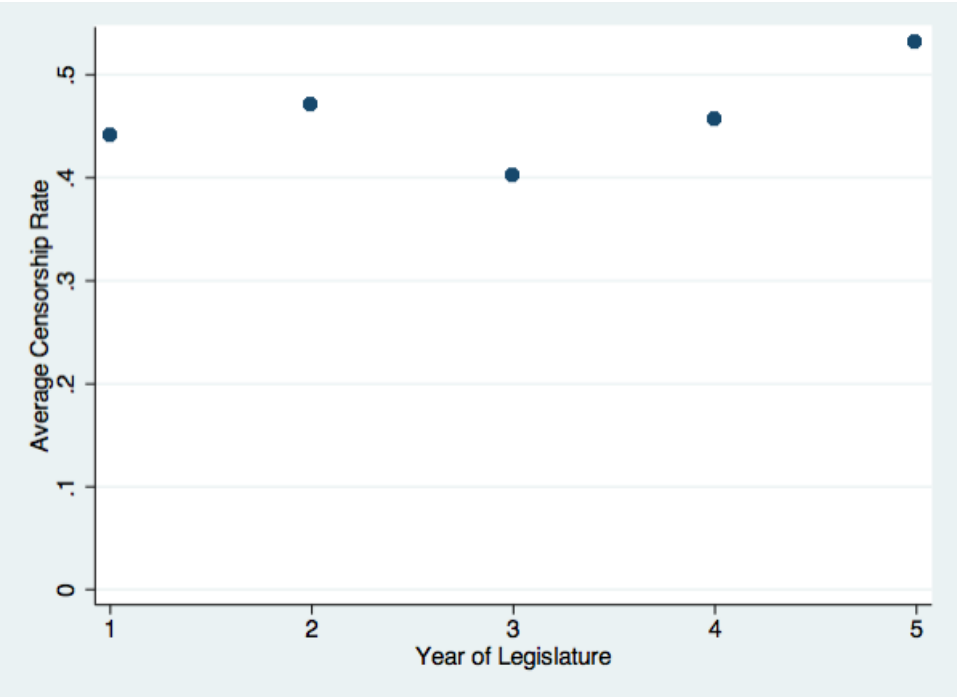


Figure 3.4: Average Censorship Rate per Month

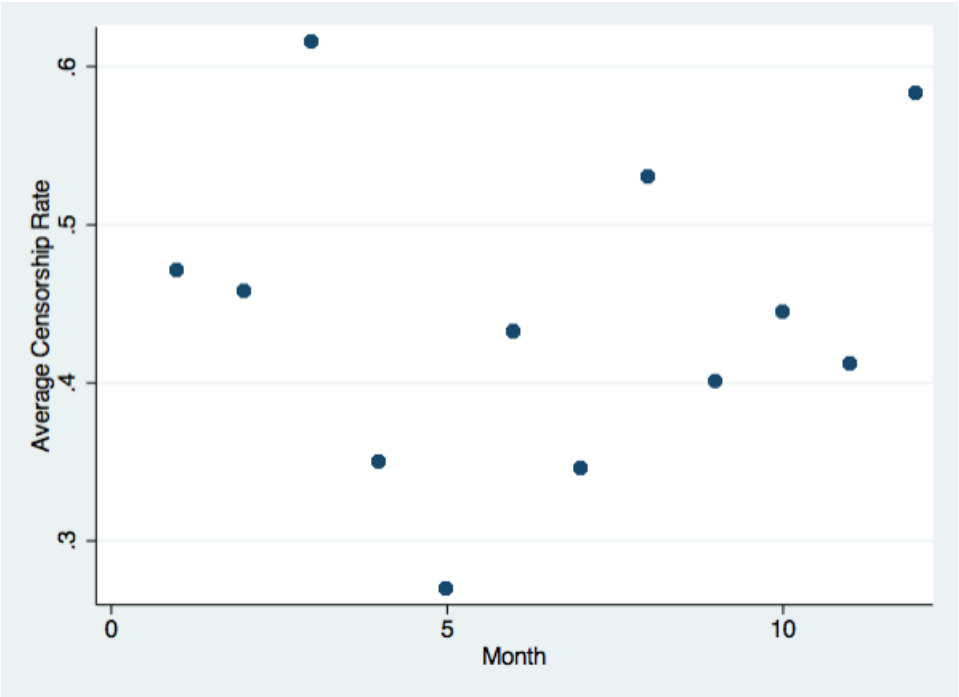


Figure 3.5: Composition of Challenged Laws.

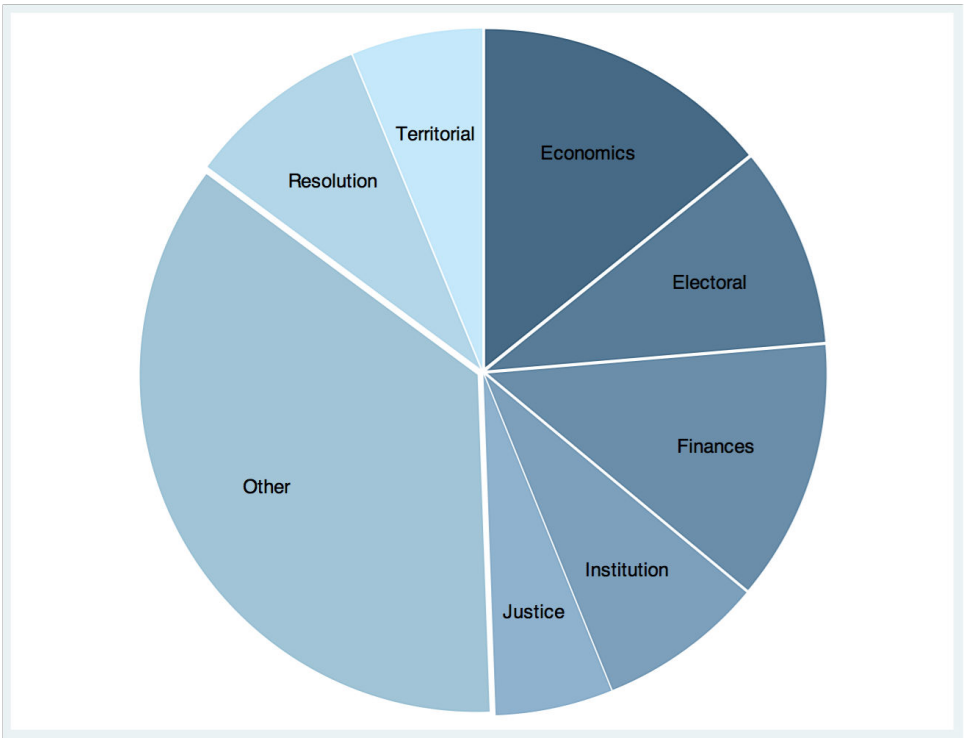


Figure 3.6: Plot of residuals and lag residuals.

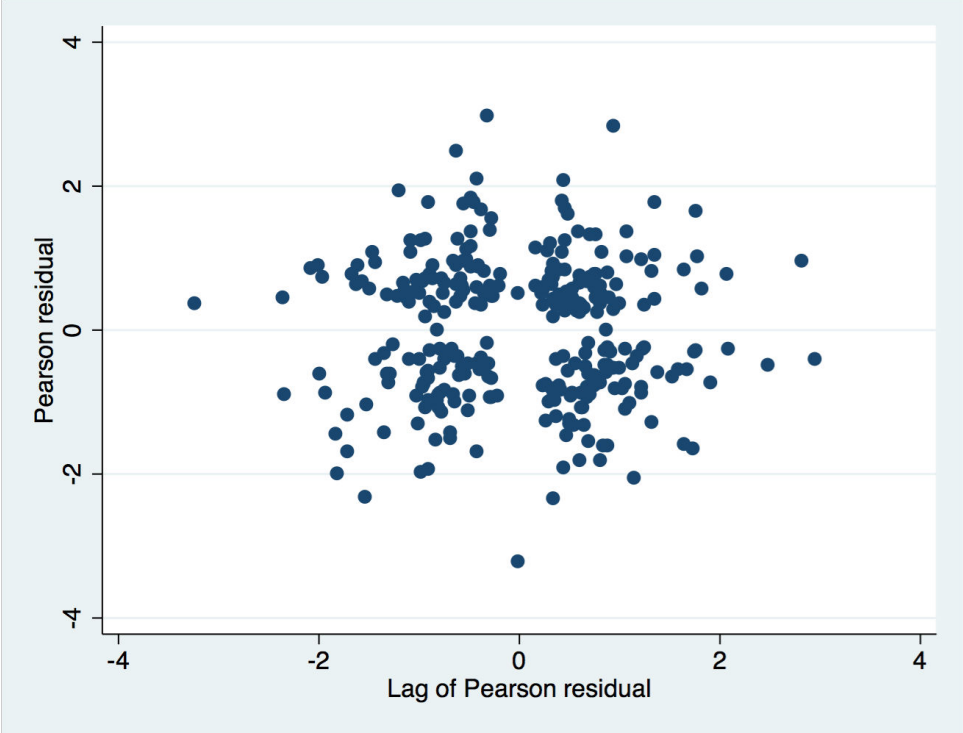


Figure 3.7: ROC curve analysis.

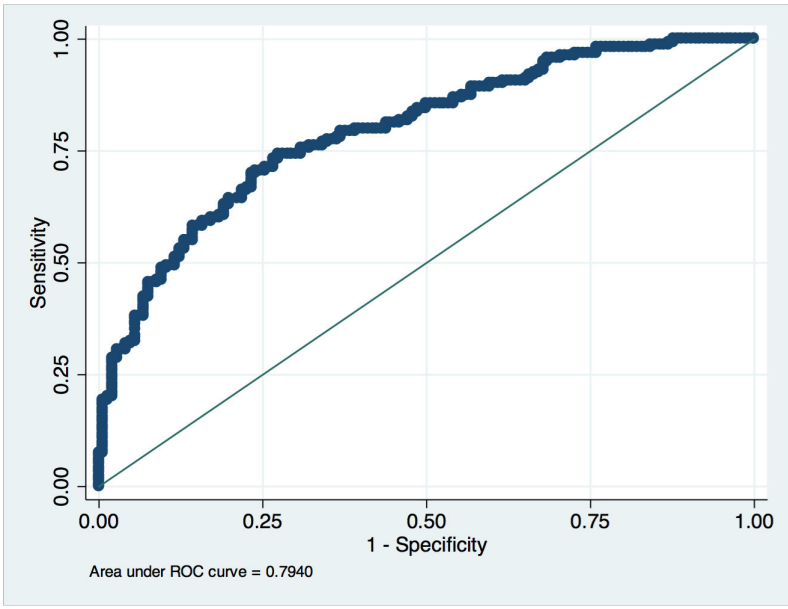


Figure 3.8: Standardized Pearson Residuals.

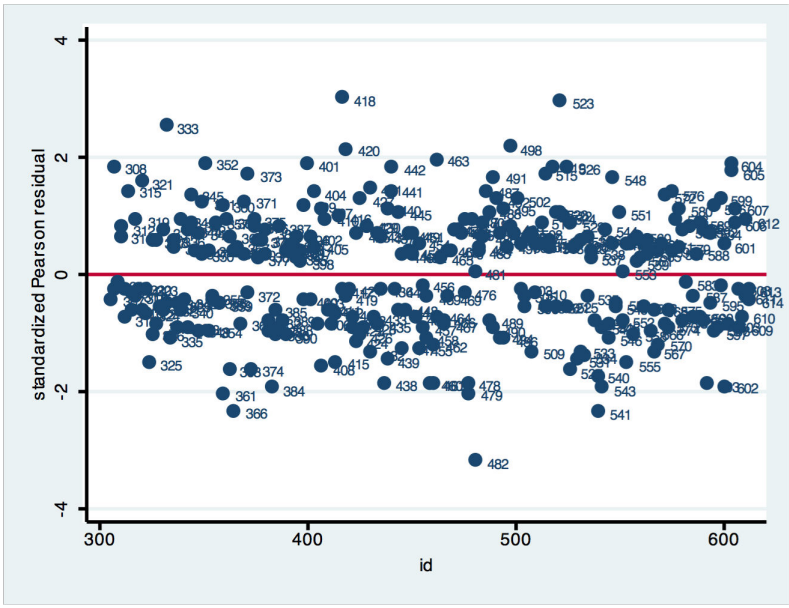
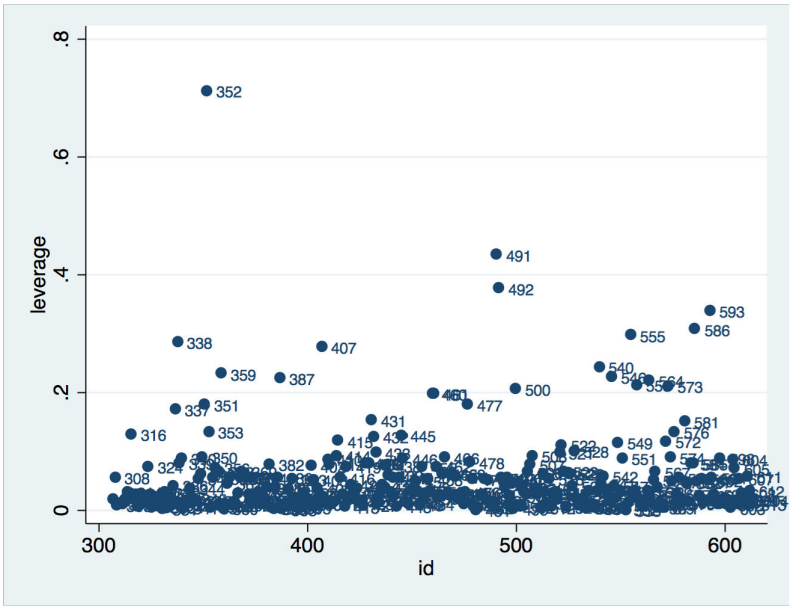


Figure 3.9: Deviance Residuals.

Figure 3.10: Leverage.



4 Fewer Courts, Less Justice? Evidence from the 2008 French Reform of Labor Courts

This paper has been accepted for publication the *European Journal of Law and Economics*. It has been co-written with Claudine Desrieux and Hengrui Wan.

Abstract

The need to provide a high-quality justice at reasonable cost represents a current challenge for many public authorities. Many reform projects propose to remove some courts in order to rationalize the judiciary. This paper explores the 2008 French reform of labor courts (removing 20% of the courts) to empirically investigate the determinants of the removal decision, and its consequences on caseload and case duration in the remaining courts. This represents -to our knowledge- the first attempt to evaluate the impacts of courts' removal. Using panel data, our empirical strategy is based on probit estimations, counterfactuals, as well as 3SLS estimations. Our results show that the reform targeted small and concentrated courts. At the aggregated national level, it appears that duration did not increase, but the demand for litigation decreased. Locally, we find that courts were affected in different ways according to the relative burden they took on.

JEL Codes: K40; K41; K31

Keywords: judiciary reform; courts' removal; judicial map; case duration; caseload

4.1 Introduction

With the growing constraint on public finance, the question of judiciary organization has become a major concern all over the world, and especially in Europe. In 2011, the ENCJ (*European Network of Councils for the Judiciary*) adopted the Vilnius Declaration which lists a set of recommendations to respond to the current challenges and opportunities European judiciaries face due to the new economic landscape. The rationalization and

re-organization of courts is motivated by the need to provide high-quality justice, and to make a more efficient use of the available resources. In the light of these concerns, France reformed in 2008 its judiciary by reorganizing and reducing the number of courts. The reform removed 20% of the labor courts. In this paper, we explore the determinants and the consequences of this reduction in the number of labor courts.
More precisely, we intend to investigate (*i*) the criteria that governed labor courts' removal, and (*ii*) the consequences of this reform on caseload and case duration. To do so, we build a cross-section and a panel databases. We collected information from the French Ministry of Justice on caseload and case duration in all French labor courts between 2004 and 2012, *i.e.* four years before and after the reform. We completed our databases by collecting information on the geographical distance between courts as well as on some macroeconomic indicators (such as unemployment rates or GDP levels) and the population in those areas. We also gathered information on the political representation of each area (*i.e.* the political affiliation of the Parliament's local representatives).

We run a probit estimation to figure out which factors drove the decisions of courts' removal. Results show that the reform targeted small and isolated courts in high-unemployment areas. Political considerations do not appear as significant. To understand the impact of the reform on caseload and case duration, we first propose a counterfactual analysis. This approach fails at detecting a contraction of the demand for litigation after the reform. Second, we run 3SLS estimations. At the national aggregated level, we detect a small contraction in demand for litigation. This contraction has been particularly strong in courts taking on a large burden from removed courts. Regarding the delays, our investigation does not find any significant increase at the national level. We show however that courts receiving a large burden might have suffered from increased delays, especially since 2011.

The originality of our work is to propose an evaluation of a recent reform reducing the number of courts. Our investigation is, to the best of our knowledge, the first attempt to assess the impacts of courts' removal on the judiciary. This may sound surprising, as many European countries (as Denmark, the Netherlands, Norway, Ireland, Portugal and Greece) have progressively implemented reforms including such removals. In some other countries, like Belgium or Italy, the revision of the judicial map is considered necessary, but no consensus on specific measures has been reached so far (ENCJ (2012)).

The paper is organized as follows: section 4.2 relates our paper to the previous economic literature. Section 4.3 presents the institutional context of the 2008 French reform of labor courts, and section 4.4 describes some stylized facts. Section 4.5 contains the empirical analysis of the determinants and consequences of courts' removal. Section 4.6 concludes and discusses our results.

4.2 Literature Review

The contribution of legal institutions to wealth creation and economic growth has drawn a lot of attention in the economic literature. From the early works of Smith (1762) or Montesquieu (1748) to more recent researches (Posner (1998), La Porta et al. (1998), Djankov et al. (2003); Botero et al. (2004) or Acemoglu & Johnson (2005)), the role of the legal framework in the economic activity has kept on being emphasized.

However, law can foster trade, investment and ultimately economic growth only if it is enforced in good conditions. In the light of these considerations, a growing attention has been devoted to the organization of the judiciary over the past decades. Without being exhaustive, topics such as the number of judges (Posner (2000); Beenstock & Haitovsky (2004); Dimitrova-Grajzl et al. (2012*b*); Mitsopoulos & Pelagidis (2007)), judges' monetary incentives (Boylan (2004); Yoon (2006); Choi et al. (2009*a*); Deyneli (2012); Garoupa et al. (2015)), training (Chemin (2009)) or appointment (Maskin & Tirole (2004); Choi et al. (2010); Lim (2013)) have been explored. Our work departs from these papers to the extent that it is primarily interested with courts' organization rather than with judicial behavior. More precisely, our paper deals with the determinants and the effects of a reduction in the number of courts. The question of the optimal number of courts has drawn some attention in the legal literature (Gomes (2007); Mak (2008); Van Djik & Horatius (2013)), and has raised many concerns in public debates (World Bank (2011); Sénat (2012); ENCJ (2012)). Yet, the economic literature has devoted little attention to this problem. Existing contributions to this topic are mainly theoretical. Chappe (2012) models how the demand for litigation evolves when courts are congested. She stresses that delay reduction efforts (such as greater court capacity) may be offset by a resulting increase in the demand for litigation, when litigants integrate congestion into their utility function. Chappe & Obidzinski (2014) also investigate the question of the optimal number of courts. They show that decreasing the number of courts has non-conclusive effects on the demand for litigation. Depending on the defendant's transport costs, diminishing the number of courts may induce more care (fewer accidents) and fewer suits, so that congestion may decrease. Nevertheless, this paper does not provide any empirical evidence supporting this proposition. More broadly, no paper - to our knowledge- has proposed an empirical investigation of a reform reducing the number of courts.

To evaluate the impacts of such a reform, we focus on the number of new cases brought to courts and on the average duration of claims. Judicial performance is multi-dimensional (Djankov et al. (2003); Cross & Lindquist (2009); Choi et al. (2009*b*, 2011, 2012)), but our focus is limited to these two indicators. Prior to its implementation, opponents to the reform argued that reducing the number of courts would make access to justice more difficult, and would increase court congestion. They emphasized that judicial delays are a strong concern in France, especially in labor courts (Marshall (2013); Lacabarats (2014)).[1]

[1] In 2013, France was convicted 66 times for judicial dysfunctions, including 51 cases relative to denial of justice in labor courts, caused by excessive delays (Lacabarats (2014)). However, concerns about excessive

As a consequence, our goal is not to propose a full evaluation of the reform, but to analyze whether the fears of the opponents regarding delays and demand for litigation were properly grounded.

Last, our paper is also related to the large literature on judicial reforms.[2] Many reports from the World Bank or other international organizations (Buscaglia et al. (1995); Buscaglia (2001); Dakolias (1996, 1999); Dakolias & Said (1999); World Bank (2012); European Commission (2014); Doing Business (2014); IMF (2014)) encourage judicial reforms to promote an independent, trustful and credible justice. Other organizations published reports to share experiences on practices in the judicial sector (Webber (2007); CEPEJ (2014*b*); OECD (2013)). The common idea shared by all these contributions is that a well-organized, credible and independent judicial system has positive consequences on economic outcomes. Following this idea, Botero et al. (2003) review evidence on judicial reforms and their impacts on economic performances. Chemin (2012) analyzes the impact of a reform aiming at reducing delays on firms' behavior. However, none of these contributions provides for empirical evidence of the determinants and consequences of the reduction in the number of courts, while many public decision-makers are discussing such reforms.

4.3 The Institutional Setting

4.3.1 French Labor Courts

Created by Napoléon in 1806, French labor courts (*"Conseils de Prud'hommes"*) are first-level tribunals[3], dealing with individual disputes affecting labor relationships in the private sector (validity of employment contracts, nullification of a dismissal, monetary compensations, level of severance payments, ...). Most of the cases are however brought by fired workers challenging their dismissals.[4] These courts only deal with individual disputes, since disputes affecting collective labor relationships are dealt with by ordinary civil courts (*"Tribunal de grande instance"*). There exist today 210 courts spread all over the territory. Each court is divided into 5 sections by activity (agriculture, commerce, industry, executives and diverse activities). Judges of labor courts are not professional judges but elected

delays are not specific to France. The European Commission for the Efficiency of Justice (CEPEJ), created by the Committee of Ministers of the Council of Europe in September 2002, was primarily set up to propose concrete solutions to current problems face by judiciaries, notably to reduce congestion in the European Court of Human Rights (CEPEJ (2014*a*)).

[2]Another strand of literature has explored reforms of legal services, such as legal education and the structure of law firms (Ribstein (1998); Stephen (2002, 2013); Garoupa (2014)). These works deal with important reforms relative to judicial staff, but not with the organization of the judiciary *per se*.

[3]Appeals are brought before the *"Cour d'Appel"* (*"Chambre sociale"*), and appeals against decisions of the *"cour d'appel"* are lodged in the *"Cour de cassation"* (*"Chambre sociale"*).

[4]According to the French Ministry of Justice, 8 out of 10 cases in labor courts come from fired workers challenging their dismissals. Other cases are mainly about unpaid wages or unpaid compensations (De Maillard Taillefer & Timbart (2009)).

representatives of employees and employers. These lay judges (also called "*councillors*") are elected on a parity basis: there is an absolute equality between lay judges representing employees and lay judges representing employers.

When a claim is brought to a labor court, it has to go first through the conciliation board (*"bureau de conciliation"*). If parties fail to settle at this stage, the plaintiff may either drop the case or go to the "*bureau de jugement*" (ruling panel), comprising two employer lay-judges and two employee lay-judges. In case of split votes within the ruling panel, a professional judge is asked to complete the jurisdiction in order to settle votes. Such an intervention is called "*départage*". In 2012, 146,192 new claims were brought to labor courts, and 158,391 claims were terminated. More precisely about termination, 67,567 were settled or dropped, 74,213 had a decision from "*bureau de jugement*" and a professional judge made the decision for 16,611 of them.

Labor courts in France represent a fair share of the total demand for litigation. As previously mentioned, 146,192 new cases were brought to the 210 labor courts in 2012. By comparison, in the same time, 196,630 new cases were opened in the 149 commercial courts and 674,760 new cases were brought to the 303 standard civil courts (*"tribunaux d'instance"*). Labor courts suffer however from great delays. Indeed, cases brought to labor courts need about 11.9 months to be terminated, while civil courts and commercial courts decide in half the time (respectively 5.4 and 5.8 months on average).[5]

4.3.2 Overview of the 2008 Reform

A reform project to reduce the number of courts in France was discussed in 2008. The reasons exposed by the government to support this reform were (*i*) the inadequacy between demographical evolution and the allocation of courts in the country, and (*ii*) the need to rationalize the management of courts.[6] The total cost of this reform is today evaluated to 413M €, and the savings on administrative expenditures are estimated to 9.1 M € per year (Cour des comptes (2015)).[7] A strong motivation for the reform was to remove courts with low activity levels, and to give more means to courts dealing with a larger number of claims. Before the reform, there were 1,206 courts in France, among which 271 were first-level labor courts. Strong inequalities of access could be observed: some

[5]Statistics come from both the Ministry of Justice (*www.justice.gouv.fr/statistiques.html*) and a report ordered by the Minister of Justice in 2014 (Lacabarats (2014)).

[6]The last general reform regarding the number of courts in France dated back to 1958. Another smaller reform targeting only labor courts was implemented in 1992: 11 labor courts were removed.

[7]These figures come from the institution in charge of evaluating the public organizations and public services in France (*Cour des Comptes*). They are relative to the whole reform. Let us recall that this reform concerned not only labor courts but also civil and commercial courts. A total of 341 courts were removed, among which 62 were labor courts.

départements[8] had 14 labor courts, while some others had only one (Sénat (2012)). The reform was enacted by decree n° 2008-514 of May 29^{th}, 2008, and removed 62 labor courts, *i.e.* more than 20% of the 271 former labor courts. One court was created, so that the total number of labor courts became 210 after the reform. The judicial map was redrawn: areas with removed courts were affected to other labor courts. This reform was effective on December 3^{rd}, 2008.[9] Two main criteria were announced as determining the choice of removed courts: first, public authorities wanted to maintain at least one labor court per "*département*"[10], and second, to remove low-activity courts (*i.e.* fewer than 500 new cases each year). Figures 3 and 4 in the appendix show the judicial map of French labor courts before and after the reform.

While discussing the reform, policy-makers were mainly concerned by two aspects. First, some opponents to the reform argued that potential litigants would be prevented to go to court because of the increased distance to the new local court (Sénat (2012)). Second, some concerns were raised about the potential congestion of new courts, which would increase (already high) case duration, and would, in turn, prevent even more litigants to go to court.[11] Regarding the second aspect, the government answered that the reform would have little impact on case duration, arguing that judges from removed courts would be transferred to courts which would take on the geographical competence of the removed courts. In other words, the number of judges remains constant, and, therefore, case duration should not increase after the reform in *receiving* courts compared to the courts which were not affected by the reform. Last, some opponents to the reform criticized the choice of the removed courts. They claimed that this choice was made on political considerations rather than efficiency criteria.[12]

In the following section, we present the data that allow us to see whether all these fears were properly grounded or not.

[8] *Départements* are French administrative subdivisions of the territory. Metropolitan France is made up of 95 *Départements*.

[9] Judges of removed labor courts were reallocated to other courts. Some 114 civil servants were working in removed labor courts: most of them have been reallocated to other jurisdictions, and 26 positions have been removed between 2008 and 2010 (Sénat (2012)).

[10] The exact criterion was to keep one labor court per *"département"*, and one on the geographical area of each civil court. These two geographical areas are more or less the same.

[11] Illustrations of those fears were expressed in many newspapers, as in *Le Monde*, `http://www.lemonde.fr/societe/article/2012/07/13/la-reforme-de-la-carte-judiciaire-une-occasion-manquee-selon-la-commission-des-lois-du-senat_1733397_3224.html` (Last Access: January 2015).

[12] Source: `http://rue89.nouvelobs.com/2008/08/07/reforme-de-la-carte-judiciaire-dati-a-bien-dote-ses-amis` (Last Access: January 2015)

4.4 Stylized Facts

4.4.1 Presentation of the Databases

Our objectives are twofold: (i) finding out the determinants of courts' removal, and (ii) assessing the impact of the reform on the caseload and the average case duration in the remaining labor courts. Two distinct databases are used to achieve these objectives. First, we establish a cross-section dataset to figure out the characteristics of *removed* courts. Second, we build a panel database to evaluate the effects of the reform: we consider four years before and four years after the reform.

Since the government's decision on courts' removal was made in 2008, we first focus on 2007 data to build the cross-section database for the court removal analysis. Here we consider two dimensions: variables representing courts' activity, and those accounting for their environment. To collect information about labor courts' activity, we use data from the French Ministry of Justice.[13] As explained above, we restrict our attention to two variables: the average number of new cases received between 2004 and 2007, and the average case duration.
At the same time, we complete this database with some other data which account for the courts' environmental features, and which may influence decision-makers. First, we compute the distance between each court and its nearest court within the department. The rationale consists in exploring whether geographical concentration influenced courts' removal: courts may be more likely to be removed the closer the nearest court is, because litigants would not have to travel too far.[14] Second, we take into account macroeconomic indicators: the unemployment rate in 2007[15], the GDP per capita of 2005[16] and the population density in 2009.[17] These macroeconomic variables aim at controlling for the economic activity and the potential demand for litigation: higher unemployment rates, lower growth rate and higher population densities are likely to increase the demand for litigation, and, therefore, likely to affect choices to remove courts in such areas.
We also take into account the proportion of socialist deputies (Socialist Party, *PS*) in the city where the labor court is situated.[18] Since the reform was conducted by a right-wing majority (UMP and UDI), one can believe that right-wing deputies may have been more efficient than left-wing deputies to preserve their labor court.

[13] http://www.justice.gouv.fr/statistiques.html

[14] The distance is calculated with Google Maps, and corresponds to the number of kilometers litigants have to drive from their current court to the closest court.

[15] We take this rate at the *Zone d'emploi* (ZE) level. ZE are small subdivisions of the French territory used by the INSEE (the French Institute for Statistics). There are several ZE per *département*. We choose the unemployment rate at the ZE level because each labor court has a geographical competency in a given area that is close from the ZE.

[16] We take it at the *"Département"*'s level. This information was available for 2005 only.

[17] We take data from the 2009 census at the *"Département"*'s level.

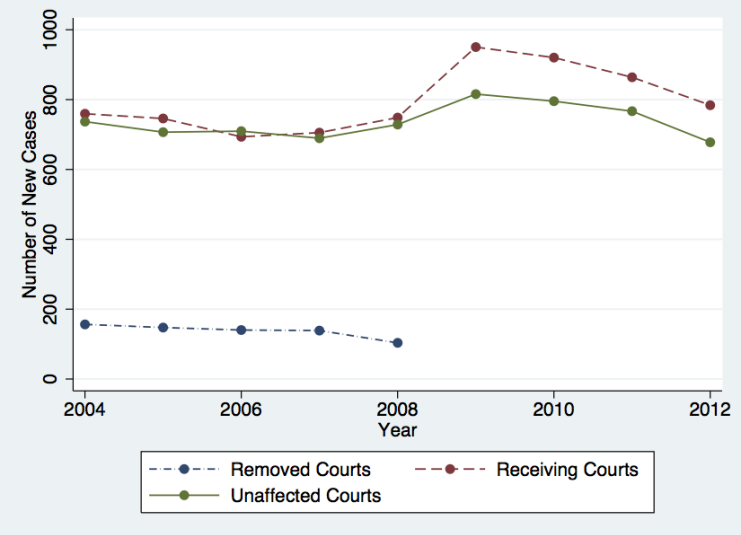
[18] Data from the French National Assembly.

In order to assess the reform's impact on caseload and case duration, we construct a second dataset.[19] This second dataset has a panel structure -it follows courts from 2004 to 2012-, and collects information similar to the cross-section dataset. We first consider two economic variables for each year between 2004 and 2012: the unemployment rate at the *zone d'emploi*'s level and the GDP per capita at the *région*'s level.[20] Second, regarding the courts' activity, we take into account the average duration of terminated cases in months and the average annual caseload. These two variables are likely to influence each other. Case duration may determine the caseload, since longer delays increase litigation costs. In addition, more cases may lead to longer claim durations, as judges might be overloaded.

4.4.2 Evolution of the Number of New Claims and of Case Duration Over Time

As the above sections pointed out, policy-makers were particularly concerned by two dimensions: (*i*) the geographical accessibility to labor courts, and (*ii*) case duration.

Figure 4.1: Number of new cases per type of labor court 2004-2012 (mean).

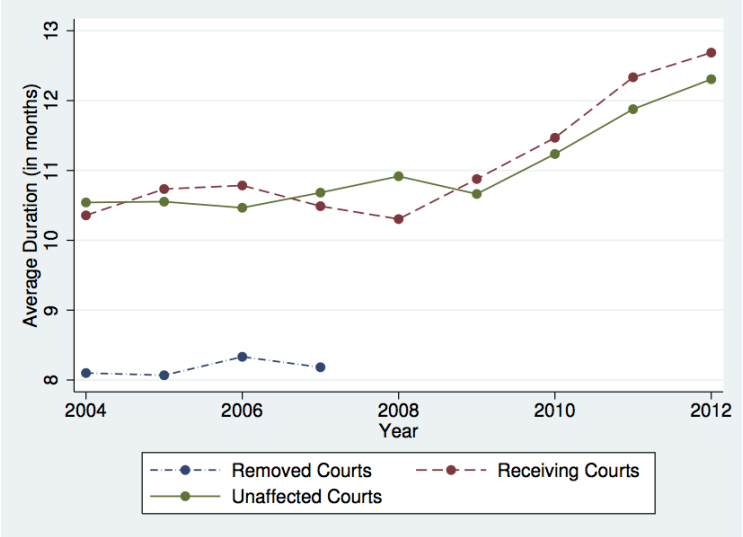


New claims Figure 4.1 shows the average number of new claims brought to courts over the years. We distinguish between 3 groups:

[19] Because of data availability, we exclude oversea regions from our analysis.

[20] *"Région"* is another French administrative subdivision. Metropolitan France is currently made up of 22 *"régions"*. GDP is not available at the "*département*" level so that we use the regional level.

Figure 4.2: Evolution of the case duration per type of labor court 2004-2012 (mean).



- Courts that were not affected by the reform (*unaffected courts*): this group gathers all courts that were not removed, and whose geographical competency was unchanged by the reform;
- Courts that were removed at the end of 2008 (*removed courts*);
- Courts that managed claims of removed courts after 2008 (*courts receiving cases*). The competency of these courts was extended after 2008 to cover the geographical areas of the removed courts. In the following, we refer to this third category as *receiving courts*.

The composition of these three groups was determined by the law (through decree n° 2008-514 of May 29^{th}, 2008) as the legislator listed removed courts, and courts whose geographical competency was extended. All on-going and new claims from each removed court were reassigned to one (and only one) receiving court.[21]

Several trends can be observed from figure 4.1. First, one can see that, before the reform, *removed* courts received on average fewer cases than *remaining* courts (i.e. *unaffected* and *receiving* courts). This indicates that removed courts were selected for their particular low level of activity. Our calculations show that removed courts represented on average 5.8% of the demand for litigation between 2004 and 2007. Second, one can also remark that *receiving* courts and *unaffected* courts displayed similar trends. Prior to the reform, *i.e.* between 2004 and 2008, both types of courts received on average the same amount of cases,

[21]In other words, litigants cannot choose the court to which they bring their cases. Litigants affected to a removed court are re-affected to a new court and are obliged to bring their claims to this specific designated court. This new reallocation was enacted by Decree n° 2008-514 published on May 29th 2008 in the Official Journal of the French Republic. The Decree states that all cases are transferred as they stand and do not have to be reopened from the beginning.

and the demand for litigation evolved in the same way.
Interestingly, the figure shows a gap between the two groups of *remaining* courts after the reform. Although this difference in 2009 is not significant ($p = 0.537$), this reflects the fact that *receiving* courts took on the geographical competences of the *removed* courts.[22]
Furthermore, we observe a peak in the number of cases in 2009. This increase is likely to result from the financial crisis.[23] Indeed, economic recessions lead to higher job outflows and lower inflows. Two effects might follow. First, a higher rate of dismissal may *de facto* lead to more claims because the set of potential claims increases. Second, the contraction of the job market may lead to a greater job scarcity: workers are more willing to contest their dismissal because of the difficulty to find a new job.

Case Duration Figure 4.2 displays the evolution of the average case duration for each of the three groups of labor courts.[24] We can first see that *removed* courts have had on average lower case duration than *remaining* courts. This is to link with Figure 1: removed courts had a low activity level, so that judges may have benefited from lower caseload and dealt more rapidly with the cases. *Receiving* and *unaffected* courts depict similar trends in case duration. One can also remark that all groups of courts have incurred an increase in duration since 2009. Because the number of claims increased in 2009 with the crisis, this probably created a congestion effect explaining the positive trend in claim duration.[25]

4.5 Empirical Studies

4.5.1 Investigation on Courts' Removal Decisions

As recalled in subsection 4.3.2, the government justified courts' removal in 2008 on efficiency criteria. Before turning to the econometric analysis, let us recall that the government did not remove labor courts in *"départements"* with only one labor court. The rationale was to insure a minimum access to justice all over the French territory.
Considering this very fact, the following analysis will focus solely on labor courts that were in *"départements"* with several courts (245 courts among 271). We shall refer to these courts as *removable* courts.

[22]The p-value is associated with the two-group mean comparison test for 2009.

[23]Source: *http* : *//www.lemonde.fr/la − crise − financiere/article/*2009/11/04/*la − crise − dope − les − recours − devant − les − prud − hommes*_1262613_1101386.*html*

[24]Note that data for the duration of cases terminated in 2008 are not available for the removed courts.

[25]Note that the we do not observe such trends in other jurisdictions. Concerning the commercial courts, delays in 2009, 2010, 2011, and 2012 were 7.9, 7.4, 7.8, and 8.1 months on average at the national level. As far as civil courts are concerned, these delays were respectively 5.6, 5.9, 5.9 and 4.9 months. (These statistics come from the Ministry of Justice: www.justice.gouv.fr/statistiques.html).

4.5.1.1 Empirical Strategies

In order to understand the determinants of the government's choice on courts' removal, we compare two sets of *removable* courts: those which have been effectively removed, and those which have remained. We propose to compare them on a list of observables we collected, and which are summarized in table 4.1. Conditional summary statistics for the two groups are displayed in table 4.2.

First, we collect legal characteristics, namely the duration of terminated cases (in months) for 2007 (*duration*), and the average number of cases in the past four years (*averageCases*). Second, we also consider economic and demographic variables: the unemployment rate at the *zone d'emploi* 'slevel (*unemployment*), the log GDP per capita at the *département*'s level (*ln(gdp)*), and the population of the *département* (*pop*). Third, we take into account the proportion of socialist deputies in the electoral districts of the city of the *removable* court (*propPS*). Fourth, we also control for the composition of the court (*propNR*).[26]
Table 4.2 displays the descriptive statistics for the two groups of *removable* courts. This table shows that the two groups of *removable* courts -the *removed* courts and the *maintained* courts - are similar in all observables but one. Indeed, a two-group mean comparison test suggests that the two categories of courts differ in the average number of cases they received in the past years preceding the reform. This confirms stylized facts presented in the previous section: *removed* courts dealt with fewer cases.

[26]Lay judges at the French labor courts are elected by both employees and employers. There is an equal number of judges elected by employers and by employees. Lists are proposed by both employers' federations and employees' unions. Most of the time, there is a common list from the employers' federations, while employees' unions compete. We control for the proportion of judges (at court level) elected by employees who are members of the two *non-reformist* unions, i.e. the two most leftist unions (CGT, FO) considered as the most demanding and "pro-employee" unions. The three other main unions are considered as "reformist", *i.e.* more willing to negotiate with employers' federations.

Table 4.1: Summary Statistics of the cross-section dataset

Variable	Meanings	Mean	Std. Dev.	Min.	Max.	N
unemployment	*Zone d'emploi*'s unemployment rate in 2007	8.273	2.086	4.175	14.675	245
ln(*gdp*)	Departmental GDP per capita in 2005 (log of €)	10.089	0.227	9.85	11.231	245
ln(*popDen*)	*Département*'s population density (log)	4.408	1.0387	3.136	9.962	245
duration	Average duration of cases in the court (months)	10.983	3.820	3.2	25.4	245
distance	Distance to the nearest court within the *département* (km)	42.501	23.269	2.8	137	245
propPS	Proportion of Socialist deputies in the court's city (%)	0.2204	0.3911	0	1	245
propNR	Proportion of *non-reformist* lay judges in the court (%)	0.5439	0.120	0.188	0.929	245
ln(*averageCases*)	Annual average number of new cases 2004-2007 (log)	5.793	0.909	3.323	8.466	245
remov	After reform status (1 if removed, 0 otherwise)	0.253	0.436	0	1	245

Sources: Ministry of Justice, INSEE, National Assembly

Table 4.2: Conditional mean of the cross-section dataset

Variable	**Mean**		**p-value**
	Maintained courts	Removed courts	
$unemployment$	8.195	8.314	0.692
$ln(gdp)$	10.079	10.137	0.084
$ln(popDen)$	4.368	4.551	0.239
$duration$	11.018	11.016	0.997
$distance$	43.659	39.084	0.181
$propPS$	0.246	0.1774	0.235
$propNR$	0.549	0.538	0.529
$ln(averageCases)$	6.120	4.858	0.000
$remov$	0	1	-

Table 4.3: Summary Statistics of the panel dataset

Variable	Meanings	Mean	Std. Dev.	Min.	Max.	N
duration	Average duration of terminated cases (months)	11.05	3.288	2.7	28.6	1817
$ln(newCases)$	Annual Average number of cases (log)	6.147	0.866	4.248	9.765	1817
unemployment	*Zone d'emploi*'s unemployment rate	9.052	2.226	4.03	16.9	1817
$ln(pop)$	Departmental population (log of hundred thousand inhabitants)	1.849	0.727	-0.277	3.252	1817
$ln(gdp)$	Regional GDP per capita (log of €)	10.190	0.2005	9.9144	10.844	1817
R	After reform status (1 if receiving, 0 otherwise)	0.1166	0.321	0	1	1817
$relativeBurden$	Relative size of the court(s) the receiving court takes on	9.890	22.407	0	135.6	1817
$yearref$	Number of years after the reform (0 before 2009)	1.112	1.449	0	4	1817
$distance$	Distance between receiving and removed courts (km)	10.742	20.922	0	124	1817
$R \times relativeBurden$	Interaction of R and $relativeBurden$	4.396	15.727	0	135.587	1817
$R \times yearref$	Interaction of R and $yearref$	0.292	0.889	0	4	1817
$R \times distance$	Interaction of R and $distance$	4.774	14.935	0	135.587	1817
$R \times relativeBurden \times distance$	Interaction of R, $relativeBurden$ and $distance$	178.876	707.874	0	8398.341	1817
$R \times relativeBurden \times yearref$	Interaction of R, $relativeBurden$ and $yearref$	10.989	43.348	0	542.35	1817
départage	Rate of *départage* (between 0 and 100)	15.021	11.017	0	77.3	1817
$winRate$	Proportion of cases won by plaintiffs (between 0 and 1)	0.3827	0.0912	0.0795	0.8629	1817

Source: French Ministry of Justice, INSEE.

In order to assess which factors played a decisive role in the government's choice, we now turn to an econometric investigation. The empirical goal consists in exploring whether, controlling for court's activity, other factors may have explained the government's decision. The binary variable *remov* accounts for these decisions: it is equal to one in case of effective removal and to zero otherwise (*remov* stands for *removal*). To do so, we consider the following probit model:

$$\begin{aligned} remov_i^* = \beta_0 + \beta_1 ln(gdp)_i + \beta_2 unemployment_i + \beta_3 ln(averageCases_i) + \beta_4 ln(popDen_i) \\ + \beta_5 distance_i + \beta_6 duration_i + \beta_7 propPS_i + \beta_8 propNR_i + \epsilon_i \end{aligned} \tag{4.1}$$

with

$$remov = \begin{cases} 1 & \text{if } remov^* > 0 \\ 0 & \text{otherwise} \end{cases} \tag{4.2}$$

where ϵ is an random term drawn from a standardized normal distribution.

We use this specification to investigate the *removal* decision on the 245 removable courts, among which 62 were effectively removed.

4.5.1.2 Main Results

Table 4 shows the results of the probit estimation. We report the p-value associated to the Pearson statistics, and the pseudo R^2. Both statistics indicate that our specification fits well the data. We also provide the correlation matrix of the independent variables in table 4.8 in the appendix.

First, regarding courts' activity, the average number of new cases between 2004 and 2007 plays a significant role in determining courts' removal. Courts which received more cases were indeed less likely to be removed. In fact, an increase in one percent of the number of new cases is associated to a decrease of 0.27 percentage point to be removed. The strong marginal effect indicates that the government intentionally targeted small labor courts. Regarding the second legal variable, it seems that case duration did not affect the removal decisions: the government did not seek to remove courts with higher delays.

Second, as far as economic and demographic variables are concerned, we observe a positive relationship between the unemployment level and the probability of removal: courts in *zones d'emploi* with higher levels of unemployment were more likely to be removed. On the other side, the level of GDP per capita did not seem to have played a significant role in the decision of the policy-makers.

Table 4.4: Probit Estimation of Courts' Removal Decision

Variable	**Coefficient**	**(Std. Err.)**	**Marg. effect**	**(Std.Err)**
unemployment	0.1335**	(0.0637)	0.01486*	(0.0071)
ln(gdp)	0.4048	(1.109)	0.0451	(0.126)
ln(popDen)	0.0531	(0.245)	0.0059	(0.0272)
duration	0.00813	(0.0328)	0.0009	(0.00364)
ln(averageCases)	-2.463***	(0.308)	-0.2741***	(0.0668)
distance	-0.0149***	(0.0054)	-0.00165**	(0.00075)
propPS	-0.3899	(0.3299)	-0.0434	(0.0357)
propNR	-1.919*	(1.042)	-0.2136*	(0.118)
Intercept	8.920	(10.928)	-	-

N	245
Log-likelihood	-63.782
P-value of the likelihood	0.000
Pseudo-R^2	0.5398

† Significance level: ***significant at 1% level **significant at 5% level *significant at 10% level. (Robust standard errors in parentheses.)

The positive effect of *unemployment* on the probability of removal must be interpreted in terms of deviation to the mean: courts in *zones d'emploi* with higher unemployment levels were more likely to be removed than their counterparts located in *zones d'emploi* with lower unemployment rates. This may suggest a will to concentrate efforts on the most dynamic areas of the country, as economic activity is likely to be more intense in areas with low levels of unemployment. This may lead to more labor relationships and a stronger need to have labor courts in these areas. In addition, areas with high unemployment levels are often rural areas whose industrial restructuring is difficult. The low perspective of business development may have been a factor to decide court's removal.

Third, our estimation suggests that the distance to the nearest court within the department was decisive in the decision-making: more isolated *removable* courts have been less likely to be removed than their counterparts. The marginal effect indicates that an increase of one kilometer between a removable court and the nearest court decreases by 0.17 percentage points the probability of being removed. This effect is consistent with the government's willingness to limit the decrease in courts' accessibility.

Fourth, we do not find any evidence suggesting that the proportion of socialist deputies influenced the government's decision. It seems that the government did not discriminate between labor courts in socialist and in conservative cities. Nor do we find strong evidence

suggesting that the proportion of non-reformist judges did affect policy makers' decision to remove a court. The coefficient associated to the proportion of non-reformist councillors is indeed only significant at the 90% confidence level. Areas where non-reformist judges are highly represented are generally zones under industrial restructuring. The result is then consistent with our previous interpretation: the reform might have aimed at concentrating efforts on the most dynamic areas of the country.

Result 1 In a nutshell, our findings indicate that the 2008 judicial reform mainly targeted smaller and less isolated courts in *"départements"* with high unemployment. These results suggest that the government was consistent with its goal to rationalize the French judicial system under the constraint of justice accessibility.

4.5.2 Reform's Impact on Caseload and Case Duration

In order to investigate the reform's impact on the number of new cases received by courts, we propose two strategies. First, we construct counterfactuals to evaluate whether the demand for litigation decreased as a consequence of the increased distance to labor courts. Second, we propose an econometric estimation of the reform's impact on both the number of new cases and case duration.

4.5.2.1 2009 Counterfactuals

The first and most natural approach to assess whether the decrease in the courts' accessibility reduced the demand for litigation consists in comparing the observed demand for litigation in 2009 (i.e. after the reform) to the demand for litigation that would have taken place without the reform. Such a comparison would require the construction of a counterfactual to establish whether the demand of litigation contracted.
Our counterfactual analysis relies on the three groups of courts described in section 4.3 (unaffected courts, removed courts and receiving courts), and proceeds as follows. First, we use courts which were not affected by the reform to estimate the natural growth rate in the demand for litigation between 2007 and 2009. Second, we use this growth rate to create counterfactuals of the *receiving* courts if the demand for litigation had not been affected by the reform. Third, we compare errors between the counterfactuals and the real data for both the control group (i.e. *unaffected* courts) and the *receiving* courts to determine whether the demand for litigation decreased in *receiving* courts.

In order to construct a counterfactual, we use the group of courts which were not affected by the reform as a control group. The underlying idea consists in using the control group to understand how the demand for litigation naturally evolved between 2007 and 2009, and to

construct a counterfactual of courts affected by the reform in the light of this evolution.[27] Note that we are able to do so because section 4.2 showed that *receiving* and *unaffected* courts displayed similar trends prior to the reform.

First, we compute the evolution rate of the demand for litigation for the control group between 2007 and 2009 (ER). It is defined by:

$$ER = \frac{1}{J} \sum_{i \in J} \frac{newCases_{2009,i} - newCases_{2007,i}}{newCases_{2007,i}} \tag{4.3}$$

where J denotes both the set and the number of labor courts not affected by the reform.

This evolution rate allows us to construct counterfactuals of the *receiving* courts in 2009 if the demand for litigation had not been affected by the reform (CF^R), *i.e.* if *receiving* courts had received their own demand for litigation together with the demand for litigation of the removed court as if nothing happened.

$$CF_i^R = ER \times newCases_{2007,i} + ER \times sumReceived_{2007,i} \tag{4.4}$$

where $sumReceived_{2007,i}$ stands for the sum of new cases in 2007 in removed labor courts that court i took on.

For the sake of comparison, we also create counterfactuals of the courts which were not affected by the reform (CF^C).

$$CF_i^C = ER \times newCases_{2007,i} \tag{4.5}$$

Note that the second term of the counterfactual disappeared because, by definition, courts which were not affected by the reform did not expand their geographical competences.

The computation of the evolution rate (ER) yields an increase of 21.4% in the demand for litigation between 2007 and 2009 in the courts which were not affected by the reform. After the construction of the counterfactuals, we compute the normalized difference between the observed number of new cases and the counterfactual (Δ_i).

$$\Delta_i = \frac{newCases_{2009,i} - CF_i}{newCases_{2009,i}} \tag{4.6}$$

[27] We choose 2007 rather than 2008 to make our counterfactual analysis, because the graph in the previous section showed a decrease in the number of new claims brought to the *removed* courts in 2008. It might be that litigants started anticipating the reform in 2008, and this impacted the decision to open new claims. We therefore consider cases in 2007 as the benchmark.

Moment statistics of these normalized differences displayed in table 4.5 suggest that the two distributions are very close: both the mean and the variance of these distributions are similar. Nevertheless, the mean of the *receiving* courts is lower than the mean of the *unaffected* courts. This result is driven by the fact that the real number of cases in 2009 is lower than what the counterfactual would have predicted for receiving courts. This finding would depict a contraction of the demand for litigation. In order to confirm this hypothesis, we run a two-group mean comparison test, but we fail at rejecting the null hypothesis of mean equality.

Table 4.5: Moment Statistics of the normalized differences

Group	Observations	Mean	St. Dev.
Receiving courts	53	-0.1698	0.0385
Unaffected courts	148	-0.09659	0.0276

4.5.2.2 Econometrics

Empirical Strategy

To deepen our previous analysis, we now investigate the impact of the 2008 reform on two variables that reflect court activities, *i.e.* the number of new cases per year and duration of terminated cases. To do so, we use the panel database presented in table 3, which provides information of each labor court from 2004 to 2012.

A major concern when determining a reform's impact lies in the potential selection of courts which were affected by the reform. As the above empirical analysis has shown, removed courts were not randomly selected. This also implies that courts which expanded their geographical competences were not randomly selected either. Nevertheless, the panel structure of our data allows us to capture the potential heterogeneity of courts by introducing fixed effects.

To empirically assess the reform's impact on the number of new cases per year (*newCases*), and on the duration of terminated cases (*duration*), we construct a variable r_{it} which accounts for the reform status (equal to 1 if court i has expanded its geographical competences at time t, and equal to 0 otherwise).

The general specification for the two dependent variables is defined by:

$$\begin{aligned} duration_{it} =& \gamma_0 + \gamma_1 ln(newCases_{it}) + \gamma_2 r_{it} + \gamma_3 r_{it} \times relativeBurden_i + \gamma_4 departage_{it} \\ &+ \gamma_5 ln(gdp)_{it} + \gamma_6 unemployment_{it} + \gamma_7 ln(pop)_{it} + \mu_i + \alpha_t + u_{it} \end{aligned} \tag{4.7}$$

$$ln(newCases_{it}) = \beta_0 + \beta_1 duration_{it} + \beta_2 r_{it} + \beta_3 r_{it} \times relativeBurden_i + \beta_4 winRate_{it} + \beta_5 ln(gdp)_{it} + \beta_6 unemployment_{it} + \beta_7 ln(pop)_{it} + \mu_i' + \alpha_t' + v_{it} \quad (4.8)$$

where u_{it} and v_{it} are two normally distributed random terms, μ_i and μ_i' are court fixed effects, α_t and α_t' are year fixed effects.

Few comments can be made in the light of this general specification. First, both specifications include the r_{it} variable, which accounts for the reform, since we aim at measuring the reform's impact on both the duration of terminated cases and on the inflow of new cases. Second, we create an interaction term between the reform and *relativeBurden*. The variable *relativeBurden* represents the relative size of the removed courts a receiving court has taken on. A *relativeBurden* equal to 50 means that the number of new claims dealt by removed courts between 2004 and 2007 represents 50% of the receiving court's activity level between 2004 and 2007. The interaction term depicts the effect of the reform conditionally on the burden a receiving court faces.[28] Third, both specifications contain macroeconomic controls (logarithm of the gdp per inhabitant, unemployment rate, and the logarithm of the population). Fourth, specification 7 includes the rate of *départage*, which is very likely to affect the duration of terminated cases, since this procedure expands the legal process.[29] *Départage* is however very unlikely to affect the number of new cases since it is unpredictable, and does not affect the very content of the decision.[30] Fifth, we consider the potential impact of win rates on the number of new cases brought to the court. We suspect indeed that parties take into account past information to form expectations about their own chances of winning a case. Recent works suggest indeed that increased chances of winning at the labor courts are associated with more settlement (Desrieux & Espinosa (2015)). *WinRate* corresponds to the number of cases which were favorably decided by lay judges over the total number of cases dealt by the court this year. Since judges' workload does not depend on the nature of their final decision, it is very unlikely for the win rate to have any impact on the duration of terminated cases.[31]

[28] Our specifications do not include *relativeBurden* separately, since it already contains court fixed effects and *relativeBurden* is time-invariant. Including *relativeBurden* would generate collinear explanatory variables.

[29] Let us recall that *départage* is a special procedure, in which lay judges ask a professional judge to intervene to help them to make a decision. Several reasons can justify *départage* (the need to clarify a legal argument, disagreement between lay judges about a decision or about the amount of damages...). Lay judges can ask such an intervention but do not need to motivate the precise reason. This procedure increases duration: while claims without *départage* are terminated on average in 15 months, the duration increases up to 25 months when *départage* is required (De Maillard Taillefer & Timbart (2009)).

[30] The win rate for plaintiffs is about 3 to 1 with or without *départage* (Desrieux & Espinosa (2015)).

[31] We also run our estimations with the lagged value of *WinRate*, and results were qualitatively similar. We display the results for the contemporaneous values of *WinRate* to maximize the number of used observations.

Estimation Method The estimation of the previous equations system raises some empirical challenges. Our general specification allows indeed both left-hand side variables to have an impact on each other. This specification is mainly motivated by the fact that litigants take into account delays before going to court, and the number of new claims is also likely to affect workload and judges' efficiency. The interdependence of the two equations constitutes an empirical issue for the standard OLS estimations. Indeed, if our general specification turned to be structurally correct, separate OLS estimations would be inconsistent (Greene (2003), Cameron & Trivedi (2005)). In order to investigate the relevance of this hypothesis, we run standard separate OLS estimations, and we display the results in table 4.9 in the appendix.
Considering that the coefficients associated to the duration and the number of new cases are both significant when they are used as regressors, we propose to rely on a 3SLS estimation strategy.[32] The 3SLS estimation combines IV and GLS estimation techniques. As in standard IV estimations, the number of instruments must be at least equal to the number of endogenous variables. The *order* condition in a 3SLS setting can also be stated in these terms: "The number of exogenous variables not appearing in [each] equation must be as great as the number of endogenous variables appearing on the RHS of this equation" (Wooldridge (2002)).
In the present case, the 3SLS estimation requires one instrument for each dependent variable. The general specification proposes two instruments. First, we propose to instrument the number of new claims by the win rate. As explained in the discussion of the general specification, we believe that the win rate is likely to influence the decision of litigants to open a claim (instrument relevance condition), but is not likely to affect duration (exclusion condition). Similarly, we believe that the rate of *départage* influences the duration of terminated cases but not the decision to open new claims (except through the channel of delays). Identification of our system of equations for the 3SLS estimation is therefore insured by these two variables. Results of the 3SLS estimations confirm moreover that our instruments are strongly correlated with the instrumented variables. The following analysis will thus focus on the 3SLS estimations. Note that the 3SLS estimation provided close results to the OLS estimates.

To go one step further, we wonder whether the impacts of the reform were different according to time and space considerations.[33] To address this issue, we explore two sub-

[32]Estimation of a system of related questions could also be obtained through 2SLS, or Full Information Maximum Likelihood techniques. Note however that the 3SLS technique is more efficient than the 2SLS estimation in presence of heteroscedasticity. The FIML estimation is as efficient as the 3SLS method if disturbances are normally distributed. We prefer to use the 3SLS estimation since the FIML makes an additional assumption.

[33]One could think that the reform has also a conditional impact according to the changes of GDP per capita. However, the local GDP could also be impacted by the reform that changes the conditions on the labour market (by making dismissal more difficult to challenge). In this regard, our estimation must be seen as capturing an overall effect of the reform: it estimates both the direct reform's effect (i.e. the

specifications. First, we allow the reform to have a conditional impact on the number of years the reform has been enforced. To do so, we introduce a variable $numYears_t$, which is equal to zero before the reform and equal to the number of years the reform has been in place between 2009 and 2012. A full description of the system of equations is given in the appendix. Second, we consider the possibility that the reform had an impact conditional on the distance between the *receiving* and the *removed* courts. We add a variable which accounts for the number of kilometers between the two kinds of courts (denoted $distance_i$).[34]

Results Table 4.9 in the appendix shows the results of the OLS estimation. Tables 4.10, 4.11, and 4.12 in the appendix display results of the general, time, and spatial specifications respectively. Because our specifications include several interaction terms, we also display graphically the marginal effects of the reform conditional on the relative burden *receiving* courts take on. Figures 4.6 and 4.7 in the appendix display the conditional marginal effect (CME) for both the duration of terminated cases and the log of the number of new cases for all specifications.
In order to visualize the reform's impact on both dependent variables according to each specification, we summarize results in table 4.6. The second and the third columns show whether the null hypothesis of the first row associated to each dependent variable is rejected in each specification. Rejecting the null hypothesis associated to *duration* means that the reform has, on average, increased the duration of terminated cases. Rejecting the second null hypothesis implies that the number of new cases has, on average, decreased because of the reform.[35]

General Comments First of all, one can remark that both dependent variables are significant in the OLS estimation when they are used as regressors, which suggests that the 3SLS estimation is more appropriate for our investigation. The coefficient associated to *duration* looses its significance in the 3SLS estimation, but still remains very close to the 10% threshold. The signs of the coefficients remain constant across estimations. In order to interpret the net effect of the reform for the OLS estimation, we display two graphs

decrease in the demand for litigation resulting from the increased costs in access to justice) and the indirect effects of the reform (i.e. changes in the number of new cases resulting from the new conditions in the labor market). However, as a robustness check, we have investigated whether the reform has had a conditional impact according to the level of GDP of the CPH's region in 2008. We found no evidence suggesting that richer regions have been affected in a different way by the reform.

[34]The variable $distance_i$ has two special features. First, it is equal to zero for *unaffected* courts. This coding is arbitrary, but does not affect our results since $distance_i$ is never used as a regressor, but is always used in an interaction term which would yield to zero scores to *unaffected courts*. For more details, see the appendix. Second, $distance_i$ is equal to the weighted average distance if the *receiving* courts take on the geographical competence of two *removed* courts. We take the average number of new cases between 2004 and 2007 for the weight of the *removed* courts.

[35]Here, 'on average' refers to the average relative burden *receiving* courts have received (weighted by their average number of new cases between 2004 and 2007).

Table 4.6: Impact of the reform on the duration of terminated cases and the number of new cases.

National Average Effect		Duration	New Cases
H_0		The reform did not increase the duration.	The reform did not decrease the demand for litigation.
General Specification		H_0 not rejected	H_0 rejected
Time Specification	2009	H_0 not rejected	H_0 rejected
	2010	H_0 not rejected	H_0 rejected
	2011	H_0 not rejected	H_0 rejected
	2012	H_0 not rejected	H_0 rejected
Spatial Specification		H_0 not rejected	H_0 rejected
Conclusion		Duration did not increase at the national average.	Number of claims did decrease at the national average.

Local Effects	Duration	New Cases
General Specification	Increase for courts receiving a great burden.	Contraction of the demand in courts receiving a great burden.
Time Specification	Increase for courts receiving a great burden in 2011.	Contraction occurred as soon as 2009.
Spatial Specification	Close *receiving* courts have increased duration.	-

in the appendix (Figure 4.5). These graphs are very similar to the net effect found with the subsequent 3SLS estimations. The 3SLS estimation takes into account both *direct* and *indirect* effects of the right-hand side variables.

Duration The reform's impact on the duration of terminated cases is derived from the 3SLS estimations, and is displayed in figure 4.6 in the appendix. The overall impact is composed of both the *direct* and *indirect* impacts. The direct impact corresponds to the coefficients associated to r_{it} and $r_{it} \times relativeBurden_i$ in column *Duration* of table 4.10. The indirect effect corresponds to the impact of the reform on the number of new cases, which is in turn passed on the duration. Because both the dependent variables are included in the set of explanatory variables for the other dependent variable, we face a feedback effect. The appendix shows how to derive the total impact of the reform on both dependent variables.

The first graph in figure 4.6 displays the marginal effect of the reform conditional on the relative burden a court receives (General Specification). As one can see, we cannot reject the null hypothesis that the reform did not increase the duration of terminated cases in the *receiving* courts, whatever the relative burden they received. Indeed, the horizontal zero line is below the lower curve, defined as the lowest bound of the 95% confidence interval, for some values of relative burden. This result suggests that courts which were receiving a large burden have suffered from an increase in delays. Nevertheless, the national average effect of the reform, displayed by the vertical line, is not statistically significant.[36]
The second graph of figure 4.6 (upper right) shows the reform's marginal effect in the spatial specification, *i.e.* conditionally on the distance between courts. Because the impact of the reform is conditioned on both the relative burden of the receiving courts and the distance with the removed courts, we displayed the marginal effect computed for each court. Indeed, since both *relativeBurden* and *distance* were uniquely defined by one single *receiving* court, we were able to display the marginal effect of each court on one single graph. The effect is obviously non-linear in the relative burden. One detects however that few courts might have suffered from an increase in duration, but we cannot distinguish a clear pattern. The third graph also displays the marginal effect of the spatial specification, but plots the marginal effects with regard to the distance between the *receiving* and the *removed* courts. A clearer pattern appears: closer courts were more likely to have an increase in delays, while more distant courts were more likely to have lower case duration. This effect might be due to the number of new cases: if the report of the demand for litigation was affected by the distance, it might be that close courts have been congested, while more distant courts have received more additional means by the state than the reported demand for litigation would have required. We then compute the national average effect of the reform, taking into consideration the role of the distance. Our computations, detailed in the appendix, yield a national average marginal effect of 0.3276, with an associated p-value equal to

[36] See the appendix for the computation of the national average effects.

0.132. The reform does not seem to have increase (on average) the duration of terminated cases at national level.
Third, the time specification yields the four remaining graphs in figure 4.6. Each graph is conditioned on a specific year, which induces straightforward interpretations. The marginal effects displayed for 2009 and 2010 are never statistically different from zero, which suggests that courts did not suffer from an increased duration in these two years. Graphs for 2011 and especially 2012 suggest however that *receiving* courts who took on a large burden (more than 50% of their capacity) incurred an increase in the duration of terminated cases. The apparition of the effect in 2011 and 2012 is not surprising, since the duration focuses on terminated cases only, and congestion effects may take time to be reflected in the data. Nevertheless, we do not find an overall increase at the national average, since the marginal effect is not statistically different from zero at the national average.

Result 2 (a) At the national aggregated level, the 2008 judicial reform did not have any impact on case duration in *receiving* courts.
(b) When *removed* courts were at close distance from their receiving court, duration might have increased in this *receiving* court. The effect is not large enough to appear at the national level.
(c) The higher the burden of a receiving court, the more duration might have increased, especially in 2011 and 2012. This effect is not large enough to appear at the national level.

New Cases Did the removal of courts decrease the demand for litigation when distance to go to court became higher, as feared by the opponents to the reform? In order to investigate this question, we ask whether *receiving* courts which took on claims from a *removed* court representing x% of their own size (*i.e.* their relative burden), also increased their number of new cases by x %. To interpret the estimation results, we plot the marginal effect of the reform conditioned on the relative burden together with a diagonal line which represents a full report of the demand for litigation. All graphs are displayed in figure 4.7 in the appendix.
The upper left graph, which displays the marginal effect given by the general specification yields a first set of results. One can observe that the average marginal effect is always below the 100% line, which suggests that no court had a full report of the demand for litigation. We are however not able to confirm this hypothesis for courts which received a small relative burden. The contraction of the demand for litigation seems to be increasing with the relative burden: the bigger the burden *receiving* courts had to deal with, the more the demand for litigation contracted. At the national average, *receiving* courts took on a burden equal to 22.34% of their size. However, their demand increased only by 14% after the reform. This suggests that the demand for litigation contracted by the difference (about 10 percentage points).
The spatial specification yields similar results. The demand for litigation seems to have contracted after the reform. Still, we are not able to confirm this hypothesis for courts receiving a small burden. It seems nevertheless that the decrease of the demand has been

more drastic for courts with higher relative burdens. The third graph, which plots the marginal effects on the distance, shows no clear pattern. At the national weighted average, we find that *receiving* courts received a burden of 22.34% of their previous capacity, but that the demand for litigation in those courts increased only by 13.52%.[37]
Third, as far as the time specification is concerned, we observe the same pattern as in the two previous specifications: the demand for litigation has contracted after the reform. Only tiny differences can be observed between the four graphs (2009, 2010, 2011, 2012). The effect becomes clearly significant from 2010. Moreover, the national average effect is not statistically different across years, which suggests that the reform's effect on the number of new cases was immediate.

Result 3 (a) At the national aggregated level, the demand for litigation decreased after the implementation of the reform.
(b) The contraction was more severe in *receiving* courts which took on a relatively high burden.
(c) The contraction occurred as soon as 2009, *i.e.* immediately after the reform was implemented.

Control Variables In order to interpret the remaining control variables, we compute the marginal effects taking into account the feedback effects. Table 4.7 displays their marginal impact on both dependent variables. All specifications yield similar results. First, the GDP is positively associated with the number of new cases, which suggests that courts are more active in richer areas. These results can be interpreted as follows: a higher GDP means that economic activity is flourishing and more people may be employed. This increase in employment relationships may lead to more labor conflicts between employers and employees, compared to areas with low economic activity. In addition, finding a job or finding a new employee may be easier in areas with a high GDP level, since they attract firms and workers. This may lead to more job turnover and potentially more conflicts when employment relationships end.[38] Another explanation could be that the GDP impacts the number of collective dismissals: the lower the GDP, the more collective dismissals we observe, and the fewer cases are brought to labor courts. Indeed, in France, collective dismissals are brought to civil courts (*tribunaux d'instance*) and not to labor courts that only deal with individual conflicts.[39] Second, the unemployment level is negatively corre-

[37]The demand for litigation at the national average is statistically different from the full report at the 99% confidence level. The associated test yields a z-value equal to -3.94.

[38]Some references support this interpretation. For instance, the French INSEE website documents that richer regions attract more people ($http://www.insee.fr/fr/ffc/ipweb/ip1501/ip1501.pdf$). In addition, Marinescu shows that the number of dismissals that are challenged in France is positively correlated with economic growth: any additional point of economic growth leads to 0,59 point increase in new labor cases (Marinescu (2005), p.121).

[39]Source: Official Report from the French Ministry of Economic and Financial Affairs entitled *Note du Trésor*, n°137, october 2014.

lated with the duration of terminated cases: Delays are shorter in localities with higher unemployment levels. One can think that areas with long-term unemployment are areas with high economic difficulties or industrial restructuring. There are fewer employment relationships and then fewer labor conflicts. A consequence is that the caseload, i.e. the "stock" of cases to be examined, is lower in these labor courts. The average duration for a new case to be heard can then be lower. A complementary explanation can be that a high level of unemployment may mean past collective dismissals. As previously mentioned, collective dismissals are brought to civil courts, since labor courts only deal with individual dismissals. This may explain why they face a lighter caseload. Last, we could also think that a high level of unemployment means that there were many labor conflicts in the past. Judges in labor courts may have faced a large diversity of situations and may have gained more experience to analyse a claim. Such experience effects may explain why duration (to make a decision about a case) decreases with unemployment.
Finally, the size of the population seems to be positively correlated with the duration of cases, but the coefficient is not statistically different from zero. Let us recall here that $ln(pop)$ does not measure population but rather population variations (because of the fixed-effects). Moreover, these variations are not relative to the working population but to the whole population. This may explain why $ln(pop)$ does not appear as significant.

Specification	General		Spatial		Time	
Marginal Effects	duration	ln(newCases)	duration	ln(newCases)	duration	ln(newCases)
ln(gdp)	-2.9261	0.4819**	-3.309	0.5115**	-3.006	0.480**
	(-1.23)	(1.97)	(-1.39)	(2.08)	(-1.26)	(1.96)
unemployement	-0.3221***	0.00542	-0.3325***	0.00459	-0.3196***	0.0054
	(-2.69)	(0.44)	(-2.79)	(0.37)	(-2.67)	(0.44)
ln(pop)	5.894	0.1776	5.426	0.1204	6.262	0.1790
	(1.47)	(0.43)	(1.35)	(0.29)	(1.56)	(0.43)

Table 4.7: Marginal Effects of local economic variables.

4.6 Concluding Remarks

The 2008 French judiciary reform removed about 20% of labor courts, *i.e.* about 5.8% of the demand for litigation in 2007. This reform came out after months of harsh discussions between the government and the Parliament. Advocates of the reform claimed that it would allow to reduce costs, and would not impact access to justice. On the contrary, opponents argued that it would decrease the demand for litigation, and increase court congestion. In this paper, we investigate these two arguments. We first look at the determinants of courts' removal. We find that the reform targeted low-level activity courts,

and that political representation (i.e. whether the local representative belongs to a left- or right-wing party) did not affect the decision to remove courts. Our second concern was about the consequences of the reform, both at the national and local levels. We find that the reform did not increase case duration at the aggregated national level. Yet, at the local level, case duration increased in courts taking on a large burden from closed courts. We also look at the demand for litigation (i.e. the number of new claims). At the national level, the reform decreased this demand. More precisely, we find that this decrease was observed as early as 2009 in areas where courts were removed, and where courts received a high level of new claims coming from suppressed courts. This suggests that the organization of the judiciary is not neutral on the demand for litigation. Unlike previous theoretical works, which concluded that court reduction would lead to mixed results on the demand for litigation, we found clear evidence documenting the decrease in the demand for litigation.
The originality of our work is to propose a first empirical evaluation of the reduction in the number of courts. Naturally, it calls for extensions to explore this question in other contexts, but it may serve as a benchmark for similar reform projects. As a first step, our study still proposes some elements to contribute to the debate about the organization of the judiciary. Fears are often expressed about the lack of proximity, and on the risk for some litigants to be prevented to sue because of the increased distance to courts. Our results suggest indeed that the demand for litigation has been affected by both the distance and the burden courts have taken on. In other words, potential litigants in areas where a court has been removed have been less likely to open a claim the further and the smaller the new court. Furthermore, the limited increase in delays we detect in courts receiving a great burden in 2012 indicates that congestion effects appear. The transfer of judges from the removed courts to the receiving courts may have decreased the average productivity of the court. This interpretation is in line with Dimitrova-Grajzl et al. (2012*a*): the relationship between the number of judicial staff and the courts' output might not necessarily be straightforward.

In spite of these contributions, our work has some limits. First, our results show that the reform mainly targeted small courts (i.e. courts with a low demand for litigation) in areas with high unemployment levels. However, the process of court selection does not necessarily entail that the reform was welfare-enhancing. These courts had indeed low levels of activity, but had also shorter delays compared to the other labor courts. Moreover, this decision may raise problems regarding equality of justice access and public service delivery in some economically affected areas. As a consequence, our results do not allow to conclude about the legitimacy and the relevance of the retained criteria. We only stress that the decision was made on economic criteria (such as the number of claims) and not on political considerations. Second, our findings need to be interpreted with caution. We cannot conclude too rapidly on the global (in)efficiency of the reform. For instance, our study does not discuss the global cost of the reform (direct costs such as the reallocation of

some judges to other courts, or indirect costs such as the impacts of courts' removal on the local economic activity in some cities). In the same way, the global benefits of the reform (financial gains through the removal of some courts, fewer civil servants to hire in future, ...) are difficult to evaluate precisely. Another difficulty regarding the welfare impact of the reform is that we are not able to distinguish between meritorious and frivolous claims. The decrease in the demand for litigation is then difficult to interpret as we do not know how this decrease affected the composition of claims brought to courts. A welfare analysis would also require to investigate the quality of courts' decisions. Discussions on appeal or reversal rates might provide some insight to understand whether the judiciary better succeeds in solving conflicts among litigants and to interpret the law in a proper manner. In addition, we can question to what extent delays are a good proxy for justice quality. Higher delays may result from more care devoted to each case. Referring to previous works, Mitsopoulos & Pelagidis (2007) state that "when justice is served with great delay, other qualitative measures of the judiciary's efficiency also worsen". Furthermore, the welfare implications of demand for litigation may be ambiguous: a higher demand may also result from more trust in institutions, and be a natural consequence of economic development and improved human well-being (Eisenberg, Kalantry & Robinson (2013)). In addition, data are only available until 2012 (*i.e.* four years after the reform): our effects are at best *medium-run* effects that may be different in the long run. Our study also focuses on labor courts, and effects may change in other courts such as civil courts. Moreover, we cannot collect information about the number of conflicts *per se* (*i.e.* the total number of conflicts that appeared and the eventual out-of-the-court settlement). One could imagine that the reform also impacts these two dimensions. Last, we focus on case duration and caseload to evaluate the consequences of the reform. Other quantitative or qualitative criteria could be explored in the future (such as judges' productivity, the use of alternative dispute resolution mechanisms, the global level of trust in justice, ...). Still more broadly, it would be worth investigating the impact of the reform on the labor market. Indeed, the enforcement of labor law is part of the total labor costs, and removing courts in areas with high unemployment rates might be detrimental in the long-run. In this respect, the potential increase in delays that appears in the data since 2012 might be an obstacle to a well functioning labor market. Worse, as we emphasized in section 4.2, litigants in areas where courts were removed have faced a double issue: they now have to go further, and they face longer delays. The degradation of justice accessibility in these areas might have affected the labor supply in a permanent way. These questions need to be deeply investigated by public authorities, and call for further research.

Appendix 4.A: Maps of Judicial System

Figure 4.3: French courts before the reform and removals during the reform

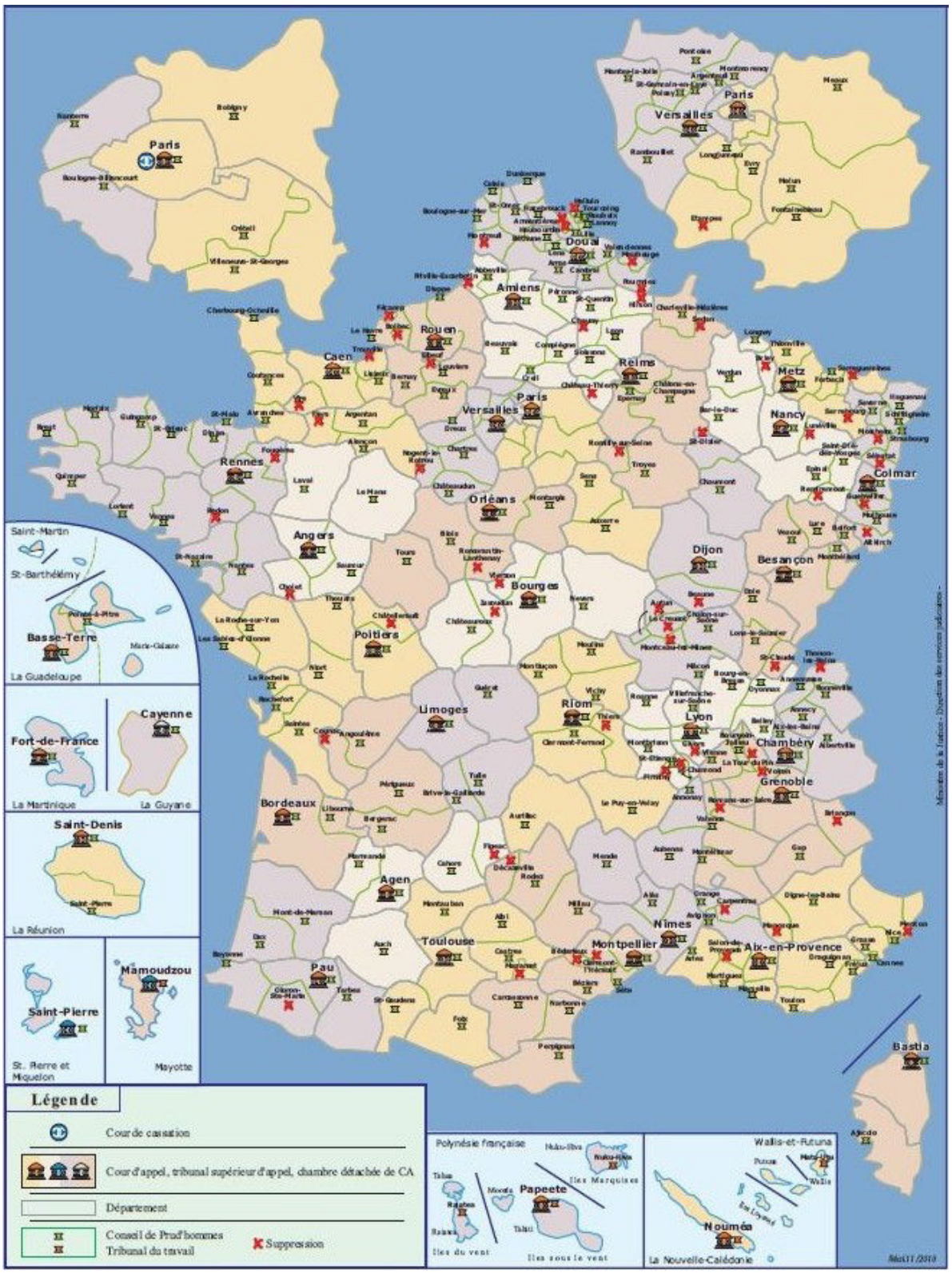
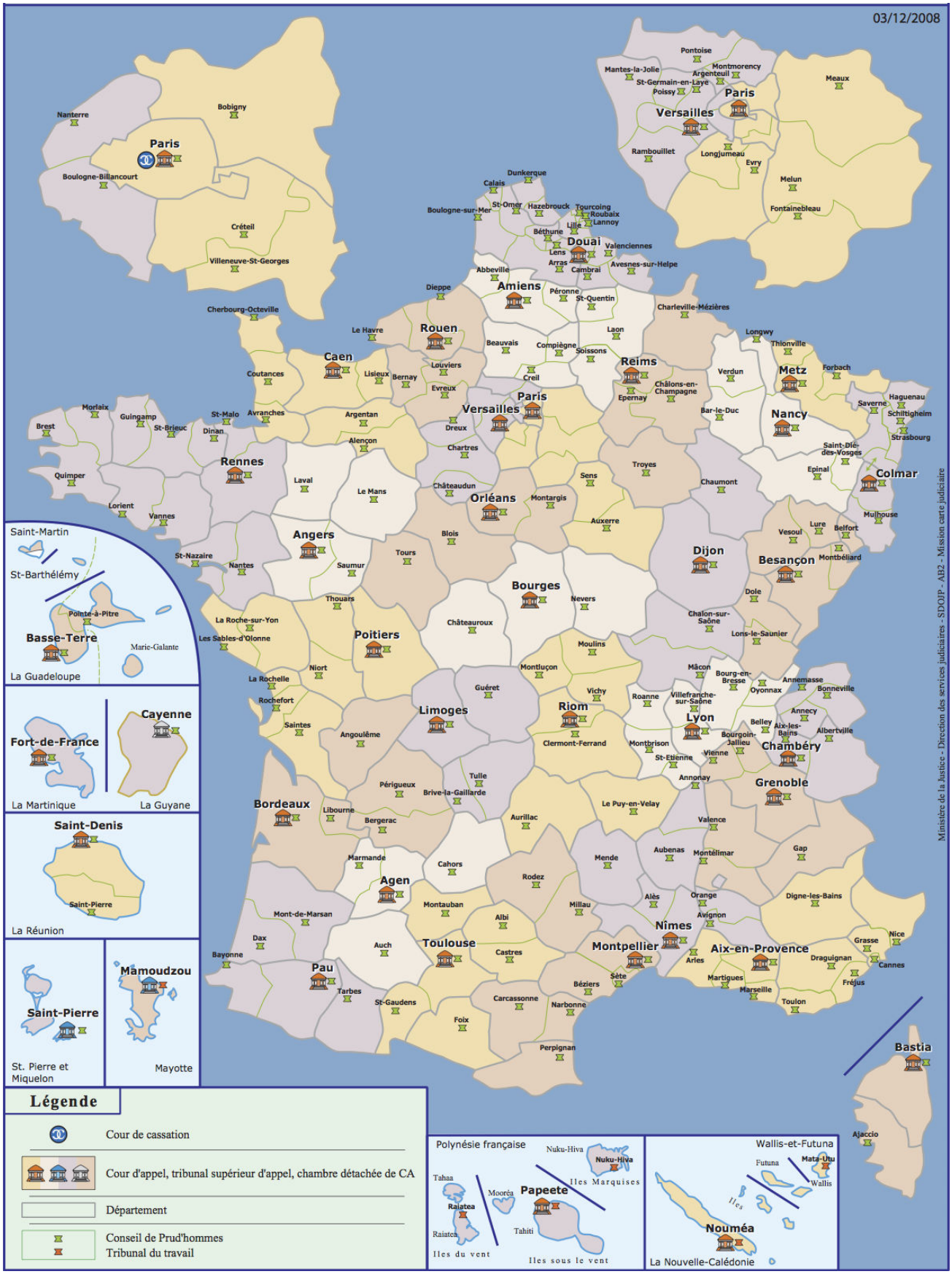


Figure 4.4: French courts after the reform



Appendix 4.B: Tables and Figures

	unemployment	ln(gdp)	ln(pop)	duration	distance	propPS	propNR	ln(averageCases)
ln(gdp)	-0.1591***	.						
	(0.0096)							
ln(pop)	-0.0983	0.8782***	.					
	(0.1112)	(0.000)						
duration	0.1025*	-0.0636	-0.0464	.				
	(0.0965)	(0.3030)	(0.4532)					
distance	-0.1007	-0.0487	-0.0755	-0.1126*	.			
	(0.1159)	(0.4481)	(0.2389)	(0.0785)				
propPS	0.0980	-0.1283**	-0.1264**	-0.0091	0.0453	.		
	(0.1123)	(0.0372)	(0.0402)	(0.8828)	(0.4804)			
propNR	0.2105***	0.0563	0.1000	0.1317**	-0.0202	0.1217**	.	
	(0.0006)	(0.3618)	(0.1050)	(0.0324)	(0.7530)	(0.0482)		
ln(averageCases)	0.0645	0.0114	0.0630	0.0861	-0.1543**	-0.0106	-0.0466	.
	(0.2964)	(0.8533)	(0.3080)	(0.1631)	(0.0156)	(0.8643)	(0.4510)	

Table 4.8: Correlation matrix of the independent variables of the probit specification.

Table 4.9: Results of the OLS estimation. (Robust clustered standard errors.)

	duration		*ln(newCases)*	
Variable	**Coefficient**	**(Z-stat)**	**Coefficient**	**(Z-stat)**
$ln(newCases)$	-1.1228***	(-4.36)		
départage	0.0521***	(6.36)		
duration			-0.00813***	(-2.97)
winRate			-0.3201***	(-3.74)
r	0.2183	(0.58)	0.04297	(1.15)
$r \times relativeBurden$	0.0127	(1.45)	0.00524***	(8.18)
$ln(gdp)$	-2.607	(-0.69)	0.7966***	(3.53)
unemployment	-0.3303**	(-2.04)	0.00728	(0.58)
$ln(pop)$	6.8556	(1.12)	0.5599***	(7.75)
Court FE	Yes		Yes	
Year FE	Yes		Yes	
N	1817		1817	
Within R^2	0.1745		0.2203	
P-value	0.000		0.000	

† Significance level: ***significant at 1% level **significant at 5% level *significant at 10% level.

Table 4.10: Results of the 3SLS estimation.

	duration		*ln(newCases)*	
Variable	**Coefficient**	**(Z-stat)**	**Coefficient**	**(Z-stat)**
$ln(newCases)$	-8.689***	(-3.79)		
départage	0.04706***	(6.74)		
$duration$			-0.01641	(-1.63)
$winRate$			-0.2940***	(-4.30)
r	0.2966	(0.80)	0.01872	(0.62)
$r \times relativeBurden$	2.0547***	(3.69)	0.00565***	(9.57)
$ln(gdp)$	1.261	(0.39)	0.4338*	(1.77)
$unemployment$	-0.275*	(-1.81)	0.000131	(0.01)
$ln(pop)$	7.437	(1.47)	0.2743	(0.66)
Court FE	Yes		Yes	
Year FE	Yes		Yes	
N	1817		1817	
Within R^2	0.5070		0.9535	
P-value	0.000		0.000	

† Significance level: ***significant at 1% level **significant at 5% level *significant at 10% level.

Table 4.11: Results of the 3SLS estimation - *time* subspecification.

	duration		*ln(newCases)*	
Variable	**Coefficient**	**(Z-stat)**	**Coefficient**	**(Z-stat)**
$ln(newCases)$	-8.654***	(-3.78)		
$départage$	0.0472***	(6.77)		
$duration$			-.01642	(-1.64)
$winRate$			-.2942***	(-4.31)
r	0.6958	(0.97)	.04574	(0.79)
$r \times relativeBurden$	0.03992**	(2.14)	.00515***	(4.52)
$r \times numYears$	-0.16004	(-0.65)	-.01081	(-0.54)
$r \times relativeBurden \times numYears$	0.00583	(1.21)	.000201	(0.51)
$ln(gdp)$	1.1520	(0.36)	.4311*	(1.75)
$unemployment$	-0.2726*	(-1.80)	.000179	(0.01)
$ln(pop)$	7.8103	(1.54)	.2818	(0.67)
Court FE	Yes		Yes	
Year FE	Yes		Yes	
N	1817		1817	
Within R^2	0.5091		0.9535	
P-value	0.000		0.000	

† Significance level: ***significant at 1% level **significant at 5% level *significant at 10% level.

Table 4.12: Results of the 3SLS estimation - *spatial* subspecification.

	duration		*ln(newCases)*	
Variable	**Coefficient**	**(Z-stat)**	**Coefficient**	**(Z-stat)**
$ln(newCases)$	-8.926***	(-3.81)		
départage	0.04653***	(6.59)		
$duration$			-.01687	(-1.67)
$winRate$			-.2881	(-4.21)
r	-0.6646	(-0.88)	-0.08743	(-1.51)
$r \times relativeBurden$	0.1039***	(4.24)	0.00813***	(5.94)
$r \times distance$	0.02280	(1.41)	0.00255**	(2.14)
$r \times relativeBurden \times distance$	-0.00116***	(-3.10)	-0.000060**	(-2.02)
$ln(gdp)$	1.256	(0.38)	0.4556*	(1.85)
$unemployment$	-0.2916*	(-1.90)	-.00102	(-0.08)
$ln(pop)$	6.501	(1.27)	0.2120	(0.51)
Court FE	Yes		Yes	
Year FE	Yes		Yes	
N	1817		1817	
Within R^2	0.4984		0.9536	
P-value	0.000		0.000	

† Significance level: ***significant at 1% level **significant at 5% level *significant at 10% level.

Appendix 4.C: Marginal effects

General specification The general specification is given by the following equation system:

$$\begin{aligned} duration_{it} =& \gamma_0 + \gamma_1 ln(newCases_{it}) + \gamma_2 r_{it} + \gamma_3 r_{it} \times relativeBurden_i + \gamma_4 departage_{it} \\ &+ \gamma_5 ln(gdp)_{it} + \gamma_6 unemployment_{it} + \gamma_7 ln(pop)_{it} + \mu_i + \alpha_t + u_{it} \end{aligned} \tag{4.9}$$

$$\begin{aligned} ln(newCases_{it}) =& \beta_0 + \beta_1 duration_{it} + \beta_2 r_{it} + \beta_3 r_{it} \times relativeBurden_i + \beta_4 winRate_{it} \\ &+ \beta_5 ln(gdp)_{it} + \beta_6 unemployment_{it} + \beta_7 ln(pop)_{it} + \mu_i' + \alpha_t' + v_{it} \end{aligned} \tag{4.10}$$

Note that these two equations do not include a control for $relativeBurden$ only, because this would be collinear with the court fixed effects.

The marginal effects of the reform are:

$$\frac{\partial duration_{it}}{\partial r_{it}} = \underbrace{\gamma_1 \frac{\partial ln(newCases_{it})}{\partial r_{it}}}_{\text{Indirect Effect}} + \underbrace{\gamma_2 + \gamma_3 relativeBurden_i}_{\text{Direct Effect}} \tag{4.11}$$

$$\frac{\partial ln(newCases_{it})}{\partial r_{it}} = \underbrace{\beta_1 \frac{\partial duration_{it}}{\partial r_{it}}}_{\text{Indirect Effect}} + \underbrace{\beta_2 + \beta_3 relativeBurden_i}_{\text{Direct Effect}} \tag{4.12}$$

Substituting each left-hand side in the right-hand side of the other equation:

$$(1 - \gamma_1\beta_1)\frac{\partial duration_{it}}{\partial r_{it}} = \gamma_1\beta_2 + \gamma_1\beta_3 relativeBurden_i + \gamma_2 + \gamma_3 relativeBurden_i \tag{4.13}$$

$$(1 - \beta_1\gamma_1)\frac{\partial ln(newCases_{it})}{\partial r_{it}} = \beta_1\gamma_2 + \beta_1\gamma_3 relativeBurden_i + \beta_2 + \beta_3 relativeBurden_i \tag{4.14}$$

Which yields:

$$\frac{\partial duration_{it}}{\partial r_{it}} = \frac{\gamma_2 + \gamma_1\beta_2 + (\gamma_3 + \gamma_1\beta_3) relativeBurden_i}{1 - \gamma_1\beta_1} \tag{4.15}$$

$$\frac{\partial ln(newCases_{it})}{\partial r_{it}} = \frac{\beta_2 + \beta_1\gamma_2 + (\beta_3 + \beta_1\gamma_3) relativeBurden_i}{1 - \beta_1\gamma_1} \tag{4.16}$$

Time Specification The time specification include more interaction variables with the reform dummy. The time specification is defined by:

$$\begin{aligned} duration_{it} =& \gamma_0 + \gamma_1 ln(newCases_{it}) + \gamma_2 r_{it} + \gamma_3 r_{it} \times relativeBurden_i + \gamma_4 departage_{it} \\ &+ \gamma_5 ln(gdp)_{it} + \gamma_6 unemployment_{it} + \gamma_7 ln(pop)_{it} + \gamma_8 r_{it} \times numYears_t \\ &+ \gamma_9 r_{it} \times numYears_t \times relativeBurden_i + \mu_i + \alpha_t + u_{it} \end{aligned} \tag{4.17}$$

$$\begin{aligned} ln(newCases_{it}) =& \beta_0 + \beta_1 duration_{it} + \beta_2 r_{it} + \beta_3 r_{it} \times relativeBurden_i + \beta_4 winRate_{it} \\ &+ \beta_5 ln(gdp)_{it} + \beta_6 unemployment_{it} + \beta_7 ln(pop)_{it} + \beta_8 r_{it} \times numYears_t \\ &+ \beta_9 r_{it} \times numYears_t \times relativeBurden_i + \mu_i' + \alpha_t' + v_{it} \end{aligned} \tag{4.18}$$

Note that these two equations do not include a control for $numYears$ only because this would be collinear with the year fixed effects. Moreover, it does not include the term $numYears_t \times relativeBurden_i$ because this would be collinear with $r_{it} \times numYears_t \times relativeBurden_i$. Indeed, the number of years is equal to zero for all courts before 2009 ($numYears$). Moreover, the relative burden courts receive is equal to zero for all non-receiving courts ($relativeBurden$). Multiplying the interaction term by r_{it} would not affect the values of the new variable, which would create perfect collinearity.

The marginal effects of the reform are given by:

$$\begin{aligned} \frac{\partial duration_{it}}{\partial r_{it}} =& \underbrace{\gamma_1 \frac{\partial ln(newCases_{it})}{\partial r_{it}}}_{\text{Indirect Effect}} \\ &+ \underbrace{\gamma_2 + \gamma_3 relativeBurden_i + \gamma_8 numYears_t + \gamma_9 numYears_t \times relativeBurden_i}_{\text{Direct Effect}} \end{aligned} \tag{4.19}$$

$$\begin{aligned} \frac{\partial ln(newCases_{it})}{\partial r_{it}} =& \underbrace{\beta_1 \frac{\partial duration_{it}}{\partial r_{it}}}_{\text{Indirect Effect}} \\ &+ \underbrace{\beta_2 + \beta_3 relativeBurden_i + \beta_8 numYears_t + \beta_9 numYears_t \times relativeBurden_i}_{\text{Direct Effect}} \end{aligned} \tag{4.20}$$

Which yields:

$$\frac{\partial duration_{it}}{\partial r_{it}} = \frac{\gamma_2 + \gamma_1\beta_2 + (\gamma_3 + \gamma_1\beta_3)relativeBurden_i + (\gamma_8 + \gamma_1\beta_8)numYears_t}{1 - \gamma_1\beta_1} + \frac{(\gamma_9 + \gamma_1\beta_9)numYears_t \times relativeBurden_i}{1 - \gamma_1\beta_1} \tag{4.21}$$

$$\frac{\partial ln(newCases_{it})}{\partial r_{it}} = \frac{\beta_2 + \beta_1\gamma_2 + (\beta_3 + \beta_1\gamma_3)relativeBurden_i + (\beta_8 + \beta_1\gamma_8)numYears_t}{1 - \beta_1\gamma_1} + \frac{(\beta_9 + \beta_1\gamma_9)numYears_t \times relativeBurden_i}{1 - \beta_1\gamma_1} \tag{4.22}$$

Spatial Specification The spatial specification is defined by the following system of equations:

$$\begin{aligned} duration_{it} =& \gamma_0 + \gamma_1 ln(newCases_{it}) + \gamma_2 r_{it} + \gamma_3 r_{it} \times relativeBurden_i + \gamma_4 departage_{it} \\ &+ \gamma_5 ln(gdp)_{it} + \gamma_6 unemployment_{it} + \gamma_7 ln(pop)_{it} + \gamma_8 r_{it} \times distance_i \\ &+ \gamma_9 r_{it} \times distance_i \times relativeBurden_i + \mu_i + \alpha_t + u_{it} \end{aligned} \tag{4.23}$$

$$\begin{aligned} ln(newCases_{it}) =& \beta_0 + \beta_1 duration_{it} + \beta_2 r_{it} + \beta_3 r_{it} \times relativeBurden_i + \beta_4 winRate_{it} \\ &+ \beta_5 ln(gdp)_{it} + \beta_6 unemployment_{it} + \beta_7 ln(pop)_{it} + \beta_8 r_{it} \times distance_i \\ &+ \beta_9 r_{it} \times distance_i \times relativeBurden_i + \mu_i' + \alpha_t' + v_{it} \end{aligned} \tag{4.24}$$

Note that these two equations do not include controls for *distance* nor *relativeBurden*× *distance* because they would be collinear with the court fixed effects.

The marginal effects of the reform are given by:

$$\frac{\partial duration_{it}}{\partial r_{it}} = \underbrace{\gamma_1 \frac{\partial ln(newCases_{it})}{\partial r_{it}}}_{\text{Indirect Effect}} + \underbrace{\gamma_2 + \gamma_3 relativeBurden_i + \gamma_8 distance_i + \gamma_9 distance_i \times relativeBurden_i}_{\text{Direct Effect}} \tag{4.25}$$

$$\frac{\partial ln(newCases_{it})}{\partial r_{it}} = \underbrace{\beta_1 \frac{\partial duration_{it}}{\partial r_{it}}}_{\text{Indirect Effect}} + \underbrace{\beta_2 + \beta_3 relativeBurden_i + \beta_8 distance_i + \beta_9 distance_i \times relativeBurden_i}_{\text{Direct Effect}} \tag{4.26}$$

Which yields:

$$\frac{\partial duration_{it}}{\partial r_{it}} = \frac{\gamma_2 + \gamma_1\beta_2 + (\gamma_3 + \gamma_1\beta_3)relativeBurden_i + (\gamma_8 + \gamma_1\beta_8)distance_i}{1 - \gamma_1\beta_1} + \frac{(\gamma_{10} + \gamma_1\beta_9)distance_i \times relativeBurden_i}{1 - \gamma_1\beta_1} \tag{4.27}$$

$$\frac{\partial ln(newCases_{it})}{\partial r_{it}} = \frac{\beta_2 + \beta_1\gamma_2 + (\beta_3 + \beta_1\gamma_3)relativeBurden_i + (\beta_8 + \beta_1\gamma_8)distance_i}{1 - \beta_1\gamma_1} + \frac{(\beta_{10} + \beta_1\gamma_9)distance_i \times relativeBurden_i}{1 - \beta_1\gamma_1} \tag{4.28}$$

Appendix 4.D: Marginal Effects at the National Level

In order to compute the national average effect of the reform, we attribute a weight w_i to each court. Let us denote z_j the average number of claims between 2004 and 2007 the *removed* court j was dealing with. For each receiving court i, we denote $J(i)$ the set of *removed* courts the *receiving* court i is taking on. Each weight is defined as:

$$w_i = \frac{\sum_{j \in J(i)} z_j}{\sum_k z_k} \tag{4.29}$$

It follows that $\sum_i w_i = 1$.

General Specification Let us denote $f(relativeBurden)$ the marginal impact of the reform conditionally on $relativeBurden$. In the general 3SLS specification, the average national effect of the reform on the dependent variable is linear in $relativeBurden$ (see previous section in the appendix). The national average marginal impact corresponds to $\sum_i w_i f(relativeBurden_i)$.

Moreover, because of the linearity of f(.), we have:

$$\begin{aligned}
\sum_i w_i f(relativeBurden_i) &= \sum_i w_i (a \times relativeBurden_i + b) \\
&= a \times (\sum_i w_i \times relativeBurden_i) + \sum_i w_i b \\
&= a \times (\sum_i w_i \times relativeBurden_i) + b \\
&= f(\sum_i w_i \times relativeBurden_i)
\end{aligned} \tag{4.30}$$

It follows that, because of the linearity of the marginal effect, the average marginal effect is equal to the marginal effect at the average. We can therefore compute the national average effect by considering the marginal effect at the national average.

Spatial Specification Let us denote $g(relativeBurden, distance)$ the marginal impact of the reform conditionally on both the $relativeBurden$ and the $distance$ of a court. The previous section has shown that the marginal effect is a function of the form: $g(x, y) = ax + by + cxy + d$. The national average marginal impact is defined by $\sum_i w_i g(relativeBurden_i, distance_i) = \sum_i g_i$. Unlike in the general setting, the marginal impact is not linear anymore. The national marginal impact is given by:

$$\begin{aligned}\sum_i w_i g_i &= \sum_i w_i[a \times relativeBurden_i + b \times distance_i + c \times relativeBurden_i \times distance_i + c] \\ &= a(\sum_i w_i \times relativeBurden_i) + b(\sum_i w_i \times distance_i) \\ &+ c(\sum_i w_i \times relativeBurden_i \times distance_i) + c \end{aligned} \tag{4.31}$$

The data yield the following results:

$$\begin{aligned}\sum_i w_i \times relativeBurden_i &= 22.33678 \\ \sum_i w_i \times distance_i &= 38.71412 \\ \sum_i w_i \times relativeBurden_i \times distance_i &= 857.2087\end{aligned} \tag{4.32}$$

Time Specification The time specification conditions the impact of the reform on both the relative burden of the *receiving* courts and the number of years the reform has been enforced. Since each graph of figures 4.6 and 4.7 is already conditioned on the year, the marginal effect is linear, and results are similar to the general specification. We can take the marginal effect at the national average to estimate the national average marginal effect.

Appendix 4.E: Graphs of marginal effects

Figure 4.5: Marginal effect of the reform with OLS estimations.

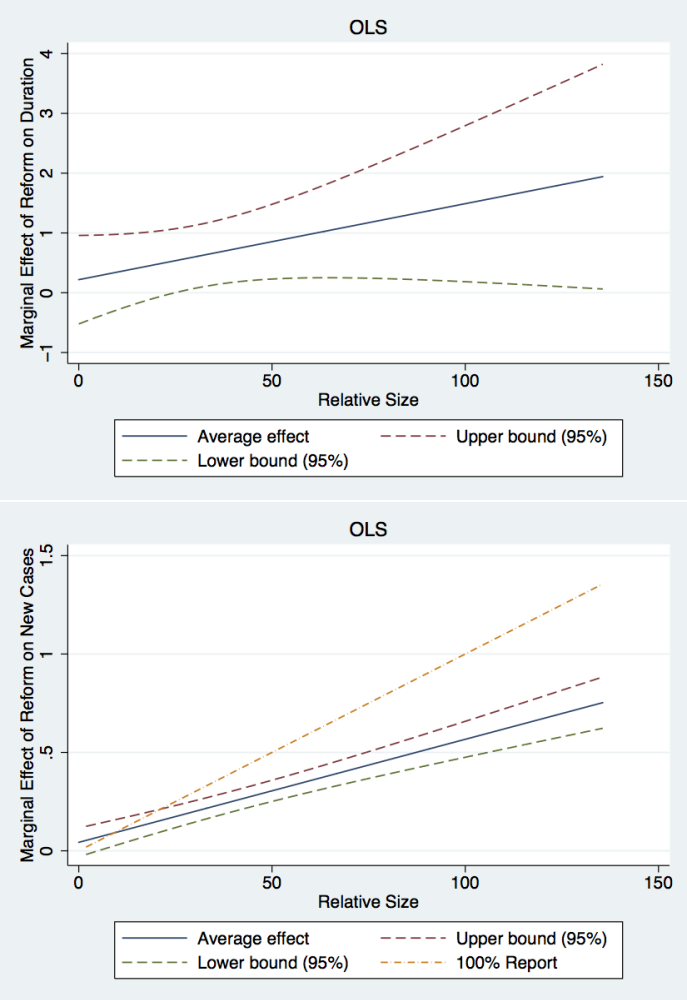


Figure 4.6: Marginal effect of the reform on the duration of terminated cases.

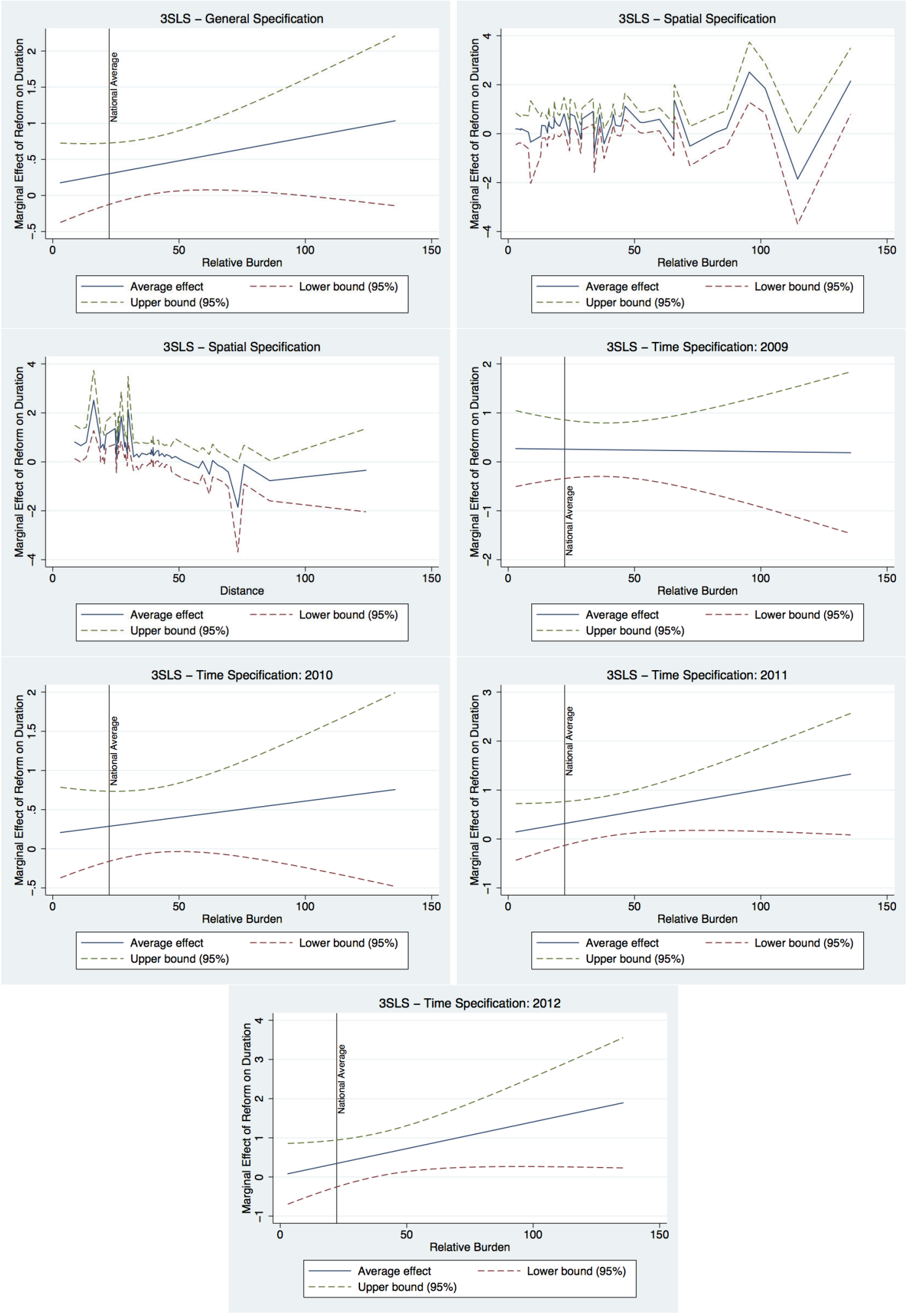
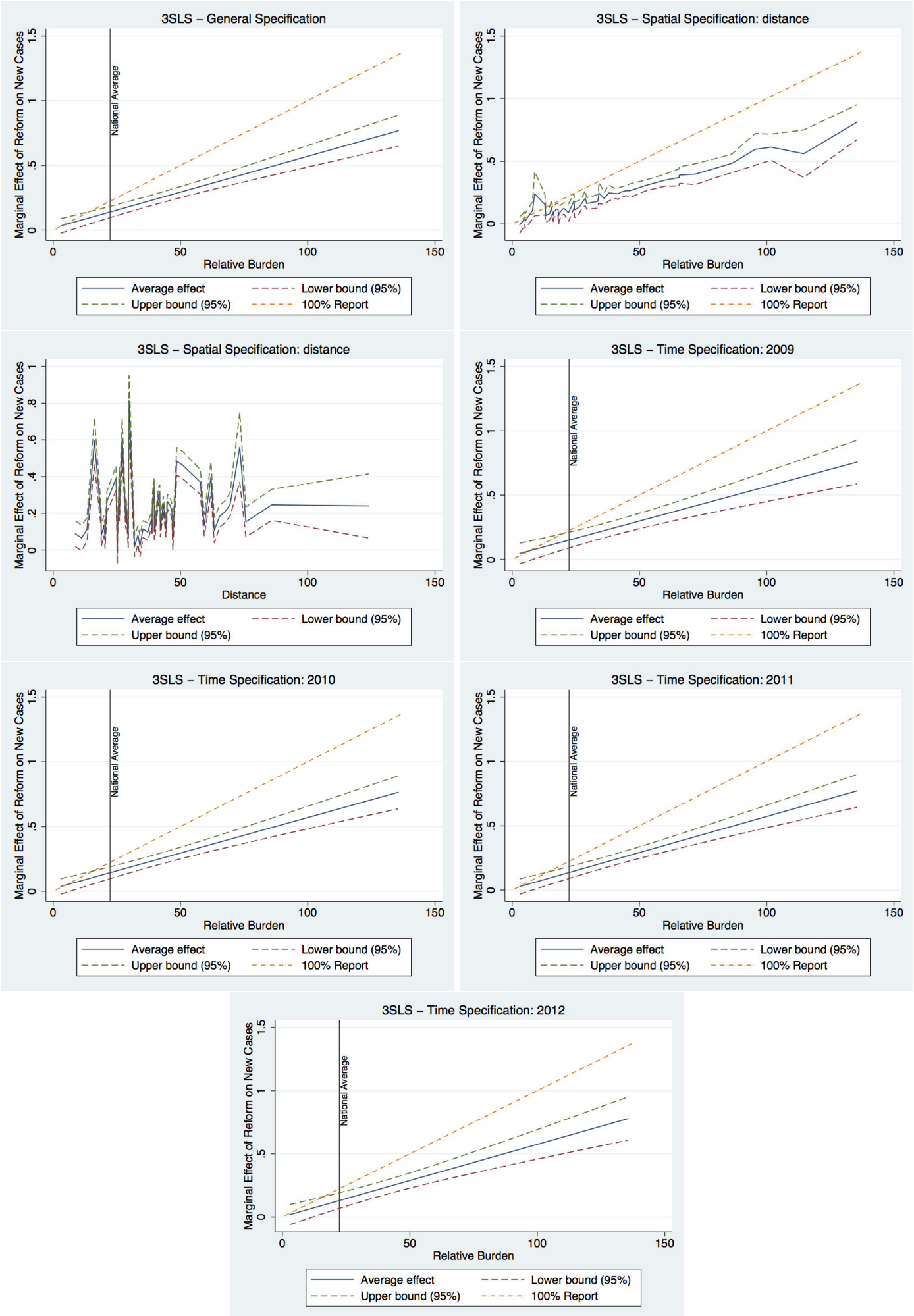


Figure 4.7: Marginal effect of the reform on the (log) number of new cases.



5 Litigants' strategies in Elected Courts: Evidence from French Labor Courts

This paper has been co-written with Claudine Desrieux.

Abstract

In this paper, we focus on conflict resolution in French labor Courts. These courts are not composed of professional judges but of equal elected members from both employees' unions and employers' federations. They are lay judges, also called "councilors". Using data from the French Justice Ministry between 1998 and 2012, we investigate whether union membership of the councilors influences judicial outcomes (settlement, decisions in court, intervention of a professional judge when no majority is found among the councilors ("départage")). Our methodology relies on probit and ordered probit estimations, with controls for endogeneity. We also run a double sample selection model to control for case selection. We find that courts with elected judges from the most confrontational unions lead to more settlement and more départage. However, there is no impact on the decision (rejection or acceptation of the claim), nor on the volume of litigation.

JEL: K31, K41

Keywords: Labor courts, unions, settlement, judicial bias.

5.1 Introduction

Are Unions appropriate labor law enforcers? Do litigants adapt their strategies to the judges they face? In this paper, we study labor law enforcement by judges elected from lists proposed by unions. We wonder whether the judge's affiliation to an union impacts the litigants' strategies.

It is now well established that the firing costs play a significant role in the decision to hire or fire workers. Part of these costs are made up of litigation costs appearing when dismissal is challenged at court. To avoid long delays and high litigation costs, many countries try to foster negotiation between parties. As an illustration, before litigating, mediation is now required in Australia or in the U.K., and conciliation is mandatory in France. When parties fail to reach an agreement, they end up at court to get a decision. Whether this decision should be made by professional judges or lay judges at this stage is an open question. Proponents of lay judges emphasize their legitimacy grounded on the proximity with the litigants' situations, and their experience of the working conditions. On the contrary, professional judges appear as more able to interpret labor law and to develop less empathy for one party or another. In practice, the situation is contrasted. While some countries only rely on professional judges in labor courts (as in Spain or Italy), some others have chosen mixed systems where professional judges and lay judges stand together to make a decision (as in Belgium or Germany). On this regard, the French experience is quite interesting as judges are elected in each local court on lists proposed by workers' and employers' unions, on a parity basis. All claims are litigated by a panel of four lay judges, and a professional judge steps in only when they fail at reaching an agreement. We then use French data to analyze labor law enforcement by elected judges. Our interest is to understand whether the litigants' decision to settle the case or to go to trial is affected by the union to which the judges are affiliated. While employer unions generally propose a common list, each worker union proposes its own list. The great ideological diversity among employee unions is a useful source of variation to identify the impacts of unions' affiliations. To reach our goal, we use data on all cases hold by employees in labor courts in France from 1998 to 2012 (1,339,496 observations). These data contain information at the case level. We match these observations to data on councilors' elections that took place in 1997, 2002 and 2008. We are then able to distinguish between "non-reformist" courts –where elected employees come from unions known to be the most confrontational at the national level– and reformist ones –where elected employees are considered as more prone to find agreements with employers' federations. Our empirical results show that parties settle more frequently when they face non-reformist courts. Claims with evidence in favor of the plaintiff are more likely to be abandoned, suggesting that the parties settle out of court. On the contrary, claims with little evidence for the plaintiff are rather conciliated at the mandatory settlement attempt. As for claims decided by the elected judges in non-reformist courts, they are more likely to need the intervention of a professional judge (*départage*), meaning that the elected employers and employees fail to reach an agreement on the case. National oppositions between non-reformist workers' unions and employers then seem to persist at the court level. There is however no evidence that the final decision (rejection or acceptance) is different in non-reformist and reformist courts. Similarly, we find that the volume of litigation is not impacted by the composition of the courts. Our paper contributes to the debate on labor law enforcement by showing that litigants adapt their strategies to the judges they face: when they anticipate confrontational debates

leading to *départage* (and then to higher delays), they have more incentives to settle their case. We also provide some evidence showing that the delegation of labor law enforcement to unions might replicate the tensions existing on the labor market itself.
Section 5.2 relates our paper to the previous economic literature. Section 5.3 describes the institutional framework of French labor courts. Section 5.4 presents our data. Preliminary analyzes are proposed in section 5.5. Complementary investigations are led in section 5.6, and interpreted in section 5.7. Discussion and concluding remarks follow in section 5.8.

5.2 Literature Review

Our paper is related to different fields of the economic literature. Many works have explored the effects of employment protection legislation (EPL) on labor markets (Lazear (1990); Portugal & Blanchard (2001); Addison & Teixeira (2003); Clark & Postel-Vinay (2009)). In this paper, we do not focus on employment protection or changes in legislation but on labor law enforcement itself. Some papers have investigated this question by exploring how judgments can be influenced by macro-economic conditions. Using detailed data from an Italian bank and aggregate macro data, Ichino, Polo & Rettore (2003) show that local labor market conditions influence labor courts' decisions: similar misconducts may be considered as a legitimate ground for firing in a tight labor market and as an illegitimate one otherwise. On the opposite, Marinescu (2005) works on French data, and shows that a higher unemployment rate is associated with a lower probability to win at court for employees. Marinescu (2011) investigates whether judges deciding on unfair dismissal cases are sensitive to economic conditions. Using the 1992 survey of Employment Tribunal Applications in Great Britain and controlling for case selection, she finds that both unemployment and bankruptcy rates significantly decrease the probability of judges deciding in favor of dismissed employees. These contributions explore how macroeconomic variables influence litigation outcomes. In our paper, we rather investigate how the judges' affiliation influences litigation outcomes.
Our work is also related to Fraisse, Kramarz & Prost (2014). The authors analyze how judicial outcomes impact employment flows in France (job creations and destructions). Their paper uses an instrumental approach, and exploits variations in the supply of lawyers. Their results show that a larger lawyer density encourages workers to file more cases, probably because of lower litigation costs. This, in turn, leads to lower probabilities to win at court (as lower quality claims are brought to courts), and to more limited job fluctuations, mainly due to fewer job destructions. Our paper differs from this approach as we focus on the determinants of litigation outcomes, not on their consequences on the labor market.
More broadly, there is also a growing literature on judicial selection systems and court decisions (Gregory & Gordon (2004, 2007); Tabarrok & Helland (1999); Lim (2013); Snyder, Stromberg & Lim (2015); Snyder & Lim (2015)). While dealing with elected judges, our

paper departs from this literature by focusing on the litigants' strategies facing judges elected through the same system but coming from different lists.
Our investigation also relies on two works exploring how judges' identity influences judicial decisions: Berger & Neugart (2011) have shown that (nomination) biases in case of labor law can influence trials' outcomes, and, therefore, parties' strategies.[1] In a older study, Spiller & Gely (1992) found that, in case of labor-relationships, a substitution of one Republican Justice by one Democrat Justice in the American Supreme Court increased the likelihood of pro-union decisions by 5 percentage points. Last, and more technically, our paper relies on methods used to determine judges' ideal points at the American Supreme Court found in Bafumi, Gelman, Park & Kaplan (2005), Martin & Quinn (2002) and Martin, Quinn & Epstein (2005).

5.3 The Institutional Setting

5.3.1 Organization of Labor Courts in France

French labor courts ("*Conseils de Prud'hommes*") deal with individual disputes affecting labor relations in the private sector (validity of employment contracts, nullification of a dismissal, compensations to be paid, level of severance payments,...).[2] There exist today 210 courts spread all over the territory. Each court is divided into 5 sections by activity (agriculture, commerce, industry, executives and diverse activities). Unlike other French jurisdictions, labor court judges (also called "councilors") are elected representatives of employees and employers. There is an absolute equality between the numbers of councilors representing employees and those representing employers. Geographical considerations entirely determine the court to which an employee has to bring his claim.[3] Figure 5.5 in the the appendix gives a graphical representation of this legal process.
Dispute resolution in French labor courts is composed of several stages. Once a claim has been opened, it has to go first through the conciliation board (*bureau de conciliation*). This first stage is mandatory, and is supervised by both one councilor representing employers and one councilor representing employees. This first stage aims at forcing parties to listen to each other's viewpoint, and, if possible, to reach an agreement to avoid litigation. If

[1] Judges are nominated by politically oriented representatives. Berger & Neugart (2011) show that changes in the identity of these representatives are associated with changes in the settlement rates, in the number of cases addressed to labor courts, in the number of appealed cases, and in the higher court settlement rate.

[2] These courts are first level tribunals. They only deal with individual disputes. Disputes affecting collective labor relationships are dealt with by ordinary civil courts (*Tribunal de grande instance*), only composed of professional judges.

[3] In other words, each court is competent for a given geographical area. If a labor conflict arises, the plaintiff cannot choose his court but has to go to the court on which his workplace depends. There are few exceptions to this general rule: for instance, workers doing work at home choose the court of the geographical area of their house.

parties fail to settle at this stage, the plaintiff may either drop the case or go to the "*bureau de jugement*" (ruling panel), comprising at least 2 councilors representing employers and 2 councilors representing employees (Blatman (2006)). When these four councilors agree on a decision, the claim ends. However, councilors may fail to reach an agreement, either on the decision, the amount of damages or on the litigation cost allocation (Lacabarats (2014)). In such a case, a professional judge intervenes to preside the panel and decides on the claim. It follows that the presence of a professional judge is not mandatory, and the vast majority of cases is decided without her intervention. It only occurs at the very last stage of the procedure in case both the conciliation and the "*bureau de jugement*" failed.[4] These features make French labor courts quite singular compared to other countries where professional judges or magistrates generally sit alongside lay judges or assessors from workers' and employers' organizations.[5]

5.3.2 The Role of Unions in Labor Courts

The French system of labor dispute resolution has given an important role to both unions and employers' federations in at least two respects. On the one side, the election of the 14,512 councilors occurs every 5 years within lists established by worker unions and employer federations.[6] The necessity to organize lists has given a dominant role to professional organizations in the selection of candidates. These general partisan elections, where all private sector workers can vote, are often considered by the unions as a test of their representativeness among the labor place. In addition to the important role played by unions in the organization of the elections, plaintiffs and defendants have also the possibility to choose to be represented by a union delegate rather than by a lawyer during the conciliation or the trial phases. Note that, since judicial representation is not mandatory, parties can also decide to defend their interests by themselves.
Councilors can therefore be elected either in the employer or in the worker pool. First, regarding the employers pool, the employers' federations (CGPME, MEDEF, FNSEA, UNAPL, UPA) generally propose a common list that gets the majority of the votes. The picture for workers' unions is substantially more complex.[7]

[4]Appeals are brought before the *Cour d'Appel* (*Chambre sociale*), composed only of professional judges, and appeals against *cours d'appel*'s decisions are lodged in the *Cour de cassation* (*Chambre sociale*).

[5]Source: The International Labour Organization, `http://www.ilo.org`

[6]Councilors are elected by universal suffrage by all employers and employees registered on the electoral roll (union membership is not required to vote). They are elected through proportional representation at the highest average, without splitting or preferential voting. Elections are organized by section and by college. The last election was held on December 2008, 3^{rd}. The mandate of the councilors was exceptionally extended to December 2017, 31^{st}, so as to think to a new nomination system. Elections for labor courts suffered from high levels of abstention: Only $25,48\%$ of workers participate to the last election in 2008.

[7]The unionization rate remains one of the lowest among industrialized countries (about 5% of the private workers' population in 2006). `http://www.insee.fr/fr/ffc/docs_ffc/donsoc06yt.pdf` (Last access: August 2014). However, and paradoxically, French unions have increased their presence in French companies, such that 44% of private workers had at least one union representative in their company in

In France, unions are national organizations. The State recognizes five unions (for employees) as representative at a national level. They are CGT, FO, CFDT CFTC, and CFE-CGC.[8] These unions are usually divided into two categories: the so-called *reformist* unions and, by opposition, the *non-reformist* unions, known to have tougher positions in debates.[9] French unions are usually characterized by their level of representativeness, which corresponded up to 2008 to their relative weight at national elections. Concerning the *reformist* unions, the CFDT, the CFE-CGC and the CFTC weight respectively 30%, 11% and 11%. As for non-reformist unions, CGT and FO represent respectively 31% and 18%. These average relative weights hide however some strong discrepancies of representation between and within labor courts. For instance, while the CFE-CGC dominates the *executives* section of labor courts, it receives a limited support in other sections. The relative weights of each union have slightly changed over time. Tables in the appendix show the national results of the three election waves (1997, 2002, 2008). These tables show a great increase over time in the number of seats devoted to CGT, a decrease of those devoted to FO and CFDT, and a small increase of those allocated to CFE-CGC and CFTC.

5.4 Data

5.4.1 Stylized Facts

We consider a dataset built by the French Ministry of Justice which includes information about all cases dealt by French labor courts between 1998 and 2012. The comprehensive dataset comprises about 2.4 millions cases that were addressed to French labor courts during this time period. Most of the cases resulted from dismissed workers who challenged their former employer's decision.[10]
In order to properly answer our research questions, we restrict our data in four ways. First,

2005. `http://travail-emploi.gouv.fr/IMG/pdf/2008.04-16.1.pdf` (Last access: August 2014).

[8]CGT is *Confédération générale du travail*/ General Confederation of Labour, CFDT is *Confédération Française démocratique du Travail* / French Democratic Confederation of Labour, FO is *Force Ouvrière* / Worker's Power, CFTC is *Confédération Française des Travailleurs Chrétiens* / French Christian Workers' Confederation, and CFE-CGC is *Confédération Française de l'Encadrement-Confédération générale des cadres* / French Confederation of Professional and Managerial Staff – General Confederation of Professional and Managerial Staff. Other unions are SUD (*Union Syndicale Solidaire* / Trade Union Solidarity) and UNSA (*Union nationale de Syndicats Autonomes*/ National Union of Autonomous Trade Unions), as well as some other independent unions.

[9]Reformist unions seek to reform within the framework of capitalism. They aim at the maintenance of competitive production-based industrial relations. They are opposed to "non-reformist" unions, considered as more revolutionary (Mouriaux (2013)).

[10]According to the French Ministry of Justice, 8 out of 10 cases in labor courts come from dismissed workers challenging their dismissals. Other cases are about unpaid wages or unpaid compensations (De Maillard Taillefer & Timbart (2009)). More recently, Serverin & Valentin (2009) show that 91 % of claims are about employees challenging personal dismissals.

we focus only on cases in which the employee sued his (former) employer, *i.e.* we get rid of cases where the plaintiff was an employer. Second, because of the limited quality of the database, we exclude observations (*i*) for which we are not able to determine how the case was terminated, (*ii*) cases that were joint[11], or (*iii*) for which essential characteristics are not reported. Third, because of data availability of the control variables, our sample restricts to courts located in metropolitan France. Fourth, to limit the unobserved heterogeneity of our data, we do not take into account cases that did not go through the standard legal process.[12]

Our final sample consists in 1,339,496 cases that were opened in 1998 or after, and that were terminated before the end of 2012. Figure 5.1 shows that the number of new cases opened each year in our dataset lies between 105,000 to 128,000. Note that only cases between 1998 and 2008 are displayed, because most cases that were opened after 2008 were not terminated in 2012. The number of new claims reached a peak in 1998 and 2002.[13] The slow decrease until 2007 can be explained by good economic conditions over the period, leading to few dismissals (De Maillard Taillefer & Timbart (2009)). The number of cases filed is indeed connected to economic conditions: about 1 in 4 dismissed workers challenges his dismissal in labor courts (Fraisse, Kramarz & Prost (2014)).

For each observation, we match three geographical variables: the logarithm of the regional level of GDP per habitant, the departmental level of unemployment[14], and the composition of the court which heard the case (we define "the composition of the court" as the union membership of the councilors in this court). All variables were collected on the INSEE (French National Institute for Statistics) website, except the composition of the courts (Ministry of Labor).[15]

As recalled in subsection 5.3.1 (and in Figure 5.5 in the appendix), judicial claims may have different outcomes: they can be either conciliated, or dropped, or decided by councilors with or without the intervention of a professional judge. In the remaining of the article, we shall refer to *conciliation* as the mandatory (official) settlement stage. Nevertheless,

[11]Several cases can be grouped (*jonction*) into a single case in very special circumstances. Joint cases must be nearly identical regarding both facts and legal considerations (same employer, same claims of the plaintiffs, simultaneity of suits, etc...)

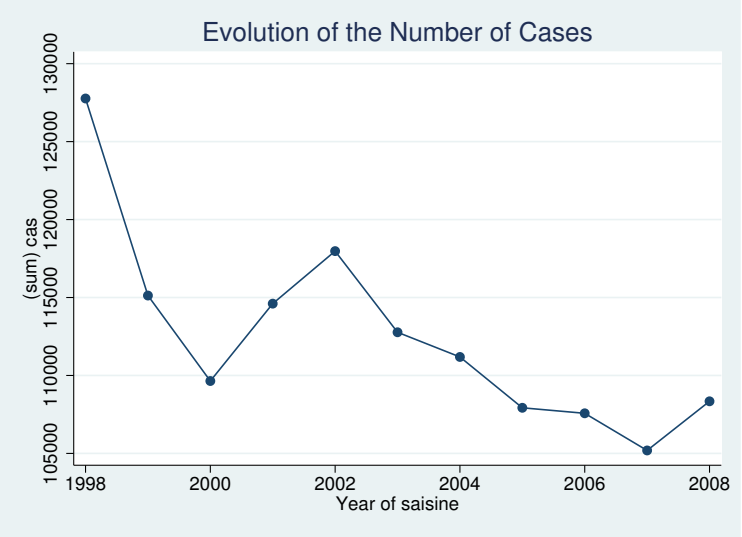
[12]For some specific legal areas, there is no mandatory *conciliation* phase. This includes: reclassification of a temporary contract of employment, disagreement on the employer's refusal of days off, dismissal of an elected employee, suits about physical or mental injuries, suits in case of violation of individual rights.

[13]According to the Justice Ministry, the 2002 peak may be caused by the regulations on working time in France that were passed in 2000 (De Maillard Taillefer & Timbart (2009)).

[14] *Département* is an administrative subdivision of the French territory. Metropolitan France is made up of 95 *Départements*. We then collect the unemployment rate in the *Département* of each court. *Région* is another (and larger) administrative subdivision. Metropolitan France is currently made up of 22 regions. GDP is only available at this regional level.

[15]Note that, for each claim, these variables are collected both at the conciliation (in-court settlement) period and when the claim goes to trial (with the councilors).

Figure 5.1: Evolution of the number of new cases opened per year between 1998 and 2008



settlement can also occur in the shadow of the court: some claims which appeared to have been dropped in our data can have been settled out-of-court. As the investigation shall demonstrate, this is indeed the case.

Table 5.1 displays the number and the proportion of case outcomes for the whole dataset. Several remarks can be made in the light of these descriptive statistics. First, the proportion of cases settled during the *conciliation* phase is limited but not negligible (13.47%). This suggests that in-court settlement is a well established phenomenon in the French labor courts. Second, a great proportion of cases is not decided by councilors nor by professional judges. Indeed, almost 24.75% of the cases disappear between the end of the *conciliation* period and the councilors' decision. These cases represent either plaintiffs who decide to abandon their claims, or plaintiffs who reach an out-of-court agreement with the defendants. Third, only a minority of cases is dealt by *professional judges* rather than by councilors (9.28% vs 52.5%). However, considering that not all cases reach the councilors' decision (38.22% are either conciliated or abandoned), the proportion of *départage* (i.e. cases where a professional judge steps in) represents 15.02% of the litigated cases.

Figure 5.2 displays the evolution of the structure of case outcomes over the past years. Several comments can be made in the light of this graph. First, one can note that plaintiffs have always been more likely to win than to loose (for both councilors' decisions and judges' decisions). Second, the proportion of cases which are settled in court (conciliation) is relatively stable over time (between 9% and 13%). Third, and most surprisingly, this graph shows a very strong substitutability between cases which are won by plaintiffs after the councilors' decision and dropped cases. This finding suggests that cases that are

Table 5.1: Cases by outcome.

Case outcome	Number of cases	Proportion of cases
Conciliation	180,436	13,47 %
Drop	331562	24,75 %
Acceptation of employee's claims by the councilors	514,447	38.41 %
Rejection of employee's claims by the councilors	188,762	14.09 %
Acceptation of employee's claims by the professional judge	86,888	6.49 %
Rejection of employee's claims by the professional judge	37,401	2.79 %

dropped before reaching the *bureau de jugement* are cases that would have been won by the plaintiff. A possible interpretation is that a relatively important share of the dropped cases is due to out-of-court settlement, and not to a unilateral abandon by the plaintiff.

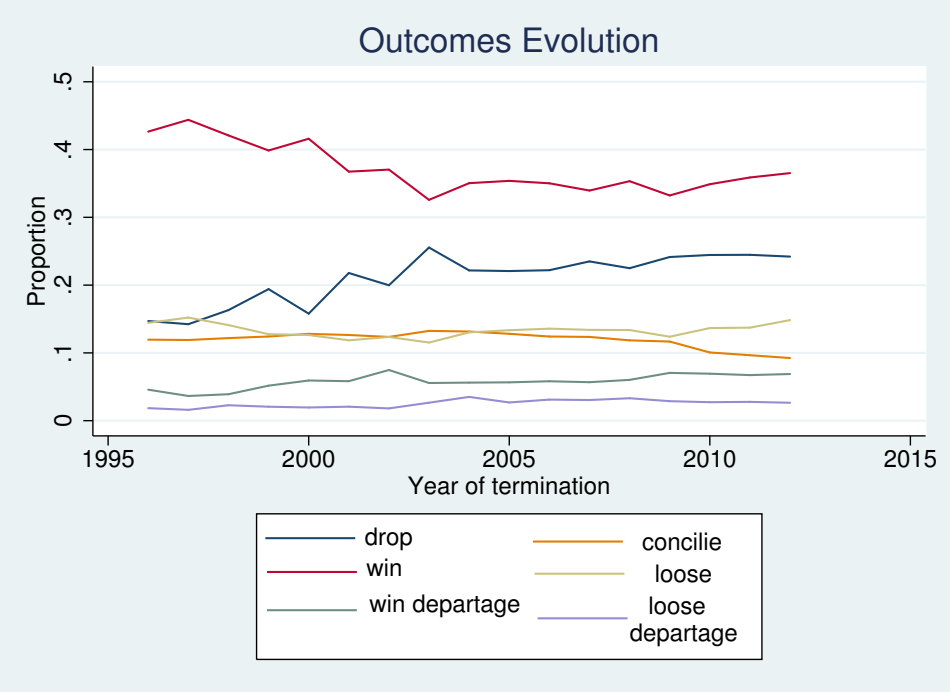
Last, table 5.7 in the appendix displays some statistics about the characteristics of the claims at each possible stage. They indicate that women represent about two fifth of the plaintiffs. Regarding legal representation, one can observe that plaintiffs tend to be more represented than defendants (26.38% vs. 20.9 % of cases). Finally, one can also note that plaintiffs are more likely to be defended by a lawyer (19.2% of cases) than by an unionist (7.18% of cases).

5.4.2 Courts' Preferences

Between 1998 and 2012, two elections occurred to replace labor court councilors (in 2002 and 2008). We dispose of election data at the section level of each court (hereafter "CPH" for *Conseil des Prud'hommes*) for 1997, 2002 and 2008 elections. Summary statistics for each election are presented in the appendix (tables 5.10, 5.11, 5.12).

To evaluate the potential ideological heterogeneity among unions, and therefore to estimate courts' potential confrontational preferences, we rely on three strategies. These strategies

Figure 5.2: Evolution of cases outcomes over time.



assume that councilors of the same union share the union's confrontational preferences. Given the institutional context, this assumption is very likely to hold: the unions play indeed a major role in the election process since they propose the lists of candidates allowed to compete. Therefore, they usually choose candidates who share their beliefs, and elected councilors need to follow their instructions to get reelected.

The first and most natural strategy to test for the presence of confrontational preferences consists in creating two groups of unions, and to investigate whether the proportion of councilors of one of the two groups influences cases' outcomes. To implement this solution, we take on the classical distinction of French unions described in subsection 3.2., which usually categorizes them as either *reformist* or *non-reformist*. According to this view, reformist unions are more prompt to discuss with firm owners, and to negotiate with them at both the local and the national levels (Mouriaux (2013)). On the contrary, this view holds that non-reformist unions are more likely to refuse to negotiate with firm owners, and are more prompt to organize strikes. We consider that this reformist vs. non-reformist dichotomy is similar to confrontational preferences. Following this approach, we compute the proportion of *non-reformist* unions at each court. To do so, we figure out the percentage of seats obtained by both the CGT and FO (known to be non-reformist unions) together for each court.[16] The resulting variable $propNR$ for a court j is defined by:

[16]Note that we focus on the five biggest national unions: CFDT, CFTC, FO, CFE-CGC, CGT. The

$$\text{propNR}_j = \frac{\text{seats}_{j,CGT} + \text{seats}_{j,FO}}{\sum_i \text{seats}_{j,i}} \tag{5.1}$$

This first solution to capture the councilors' confrontational preferences makes the implicit assumption that both groups of unions are homogeneous, that is to say that councilors of the same category (i.e. reformist vs. non-reformist) share the same *level* of confrontationalism. This assumption is however very restricting, and can be doubtful since unions need to differentiate when competing for elections.

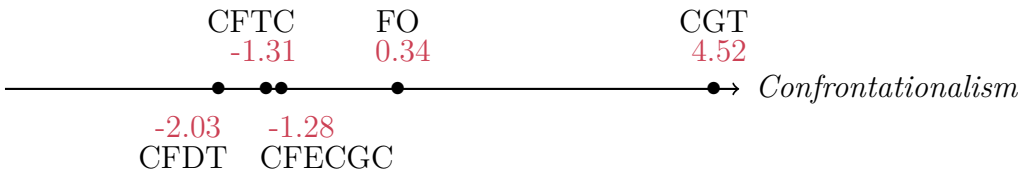
To release this assumption, we propose an alternative approach to capture unions' heterogeneity. This second approach aims at taking into account the *distance* between unions in terms of confrontational preferences. The objective consists in better estimating unions' contribution to the courts' overall confrontationalism. To do so, we rely on a Bayesian estimation method which uses past inter-professional national agreements that unions had the possibility to ratify. Inter-professional national agreements (*Accords Nationaux Interprofessionnels*, ANI) are country-wide agreements between worker unions and firm owners' representative organizations.[17] Unions have the possibility to accept or to reject each agreement. The enforcement of the agreements depends on weighted sum of unions which ratified them. We focus on agreements that could be signed between 1996 and 2012.
The Bayesian techniques we propose to use are commonly referred as *ideal points* estimation techniques. We rely on a well developed literature in political science that evaluates judges' or politicians' bliss points. These techniques have mainly been used to determine judges' ideal points at the American Supreme Court (Bafumi, Gelman, Park & Kaplan (2005), Martin & Quinn (2002), Martin, Quinn & Epstein (2005)). Following Bafumi, Gelman, Park & Kaplan (2005) (p.171), we define "an individual's "ideal point" as a point referring to his or her preferences or capacities within a spatial framework (...) characterized by a single dimension. Within a[n American] political context, this dimension is often conceived of as an ideological continuum, a line whose left end is understood to reflect an extremely liberal position and whose right end corresponds to extreme conservatism. In this one-dimensional spatial model, an person's ideological preference can be depicted by a point on this line". We propose therefore to locate unions on a single dimension. Here, the left end represents lower degrees of confrontationalism, while the right end depicts more confrontationalist preferences.
The appendix gives a full description of the methodology supporting Bayesian methods at stake.

limitation to these unions, which represent more than 95% of all councilors, is motivated by the fact that only these unions are recognized at the national level, *i.e.* able to sign national agreements with firm owners representatives.

[17] http://uimm.fr/textes-conventionnels/accords-nationaux-interprofessionnels (Last visit: May, 2014).

Applied to our topic, this estimation computes an average ideal point for each union for the whole time period. Results of this estimation are displayed in figure 5.3.

Figure 5.3: Results of the Bayesian estimation of unions' ideal points.



These results are in line with the classical distinction between reformist and non reformist unions presented above. This new estimation gives however more precise results since it takes into account the distance between each union.
It shows a strong heterogeneity among *non-reformist* unions, which reflects the limits of the first -and most intuitive- approach. It also shows that the two main non-reformist unions (FO and CGT) have yet different intensities of opposition to reforms. Taking into account these results, we are now able to compute ideological scores for each CPH. We call this variable *confront* since it represents confrontational preferences.

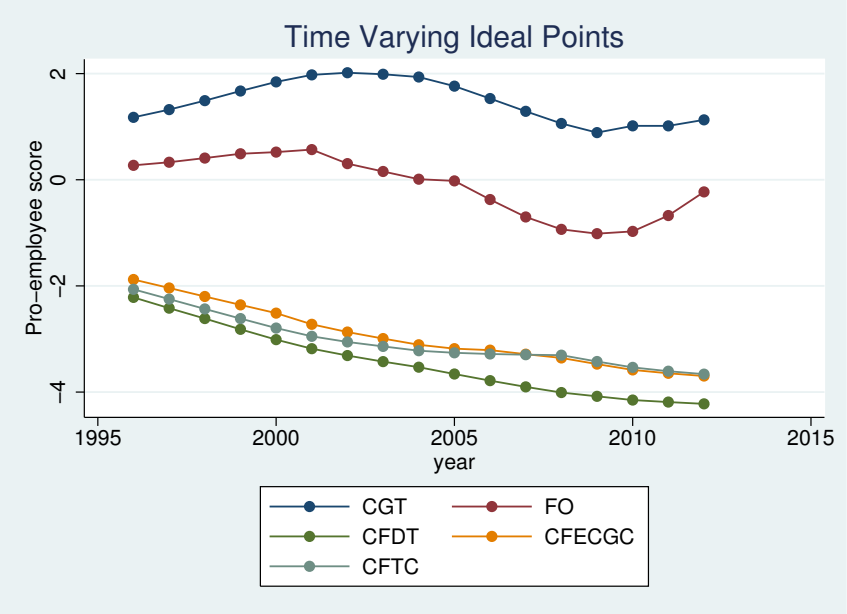
$$\text{confront}_{j,t} = \sum_i \beta_i \frac{s_{i,j,t}}{\sum_k s_{k,j,t}} \tag{5.2}$$

where $s_{i,j,t}$ is the share of seats of union i in court j at period t, β_i represents union i's score on the confrontational axis.[18]
This second solution indirectly assumes however that unions display time-invariant confrontational preferences. Although the time period considered is relatively limited, *i.e.* 14 years, it is very likely that unions have changed their preferences over time. Several factors may have contributed to this change of ideology (change of government's majority, differentiation at elections, the financial crisis, etc...).

To relax this assumption, we propose a third estimation that generates time-varying unions' preferences. This third estimation yields results presented in figure 5.4. The results give a similar picture to the second estimation in so far as it distinguishes three groups of unions: the CFTC, the CFE-CGC, and the CFDT, which can be labeled as 'reformist' (less confrontational), the CGT, which depicts the most confrontational union, and the FO which stands in-between. The time variations that some unions have experienced since 1996, especially FO, suggests that this third estimation better captures the ideological gap between unions.

[18]We divide by the total share of seats of the five unions to insure normalization since some independent unions obtain few seats in some CPH.

Figure 5.4: Results of the Bayesian estimation of *time varying* unions' ideal points.



Using this last set of results, we compute a third indicator for courts' level of confrontational behavior. We call this last indicator *tv_confront*, where *tv* stands for *time varying*.

$$\text{tv\_confront}_{j,t} = \sum_i \beta_{i,t} \frac{s_{i,j,t}}{\sum_k s_{k,j,t}} \tag{5.3}$$

Table 5.2 displays the correlation coefficients between the probability that councilors accept the employee's claims and our three proxies for courts' ideology. We can observe that all correlation coefficients are negative, and all of them are statistically significant. Surprisingly, these naive results suggest that employees are less likely to win when confrontational unions are stronger. The following sections propose an econometric investigation to better understand the link between the composition of the court and judicial outcomes.

Table 5.2: Correlation coefficients between the probability that a plaintiff's claims is accepted (vs. rejected) and variables that account for the degree of pro-employee bias. (P-values in parenthesis)

Correlation	Acceptation
Share of seats hold by CGT and FO	-0.0225***
	(0.000)
confront	-0.0251***
	(0.000)
tv_confront	-0.0075***
	(0.000)

5.5 Preliminary Analysis

Probit and Ordered Probit Estimations

We first conduct a simple analysis to understand how political ideology of the councilors correlates with judicial outcome. We represent the four possible judicial outcomes with the following variables: *Conciliation* (1 if parties settle at court, 0 if parties fail at settling at court), *Drop* (1 if the case is dropped, 0 if the case goes to full hearing, missing if parties settled at the conciliation stage), *Decision2* (0 if the councilors reject the employee's claim, 1 if the councilors accept the claim, missing if the councilors can't reach an agreement or if the parties settled at the conciliation stage or if the case is dropped), *Departage* (0 if the councilors decide on the case, 1 if the councilors can't reach an agreement on the case, missing if the case did not reach full hearing), and *Judgment* (0 if the professional judge decides to reject the employee's claim, 1 if the professional judge decides to accept the claim, missing if the councilors succeed in reaching an agreement or if the parties decided to settle). According to the perception of *départage* (i.e. when a professional judge needs to step in), *Decision2* can be recoded into a ternary variable *Decision3* (0 if the councilors decide to reject the claim, 2 if the councilors decide to accept the plaintiff's claims, and 1 if the councilors fail at reaching a decision).[19]

To understand how court's composition is correlated with cases' outcomes, we run a series of probit and ordered probit estimations. The general specification of the latent variables writes:

[19]The main difference between *Decision2* and *Decision3* lies in the way one considers *départage* issues. If one believes that *départage* mainly results from a tie between pro-employee and pro-employer votes, *Decision3* is the most suitable coding. On the other hand, if one assumes that *départage* results from legal consideration which are orthogonal to the employee vs. employer debate or is caused by the need of legal clarification, then *Decision2* is a more accurate model.

$$
\begin{aligned}
y^*_{ijst} = \quad & \beta_0 + \beta_1 comp_{ist} + \beta_2 unemployment_{it} + \beta_3 ln(gdp)_{it} + \beta_4 woman_{ist} \\
& + \beta_5 plaintiff\_lawyer_{ist} + \beta_6 plaintiff\_UnionWorker_{ist} + \beta_7 def\_lawyer_{ist} \\
& + \beta_8 def\_lawyer_{ist} + \beta_9 def\_lawyer_{ist} + \beta_{10} def\_lawyer_{ist} + \mu_{is} + \alpha_t + \epsilon_{ist}
\end{aligned} \tag{5.4}
$$

The dependent variable y^*_{ijst} represents the latent variable determining either *Conciliation*, *Drop*, *Decision3*, *Decision2*, *Départage*, or *Judgement*. For instance, considering *Conciliation*, it describes the likelihood that a claim i is successfully conciliated in court j, section s in year t. The most important variable is $comp_{jst}$ that measures the level of non-reformism of the section s of court j at this year t. This variable takes in turn the values of variables $propNR$, $confront$ and $tv\_confront$ as defined above.
The remaining variables are control variables. First, we take into account variations of macroeconomic factors, since they have been shown to influence judicial decision-making (Ichino, Polo & Rettore (2003), Marinescu (2011)). To do so, we include the unemployment rate (at the department's level) and the GDP level (log of the GDP per capita at the regional level) in year t. Second, we also take into account variables that account for case-specific characteristics: the plaintiff's gender, the plaintiff's legal representation (by a lawyer or by an unionist), and the defendant's legal representation (by a lawyer, by a worker unionist, or by an employer unionist). Third, we propose a series of fixed effects to account for unobserved heterogeneity. To do so, we include year fixed effects to control for macroeconomic factors which would affect the whole country in the same way (α_t), and CPH $\times$ section fixed effects (μ_{is}) to control the unobserved fixed heterogeneity that would be associated to each section in each court. Fourth, we include a variable $duration_{js,t-1}$ that represents the average age (number of days) of cases terminated by a decision (either from the councilors or from the professional judges) the previous year in the same court (and same section). This variable is included for *Conciliation* and *Drop*, because we assume that, when settling or dropping a case, litigants anticipate their outside option, i.e. litigation, and its associated costs, such as the expected duration.
Finally, let us note that the values of the independent variables that are not determined by the case itself, i.e. the composition of the court and macroeconomic variables, are set at the date of the audience for conciliation for the estimation of *Conciliation* and to the date of the decision for the remaining variables.

Table 5.8 in the appendix summarizes the results of these estimations, which we refer to as the *baseline* model. These results lead to four main observations. First, the degree of non-reformism of the court is positively associated with conciliation: more cases are conciliated in courts that are dominated by non-reformist unions. Second, more cases are dropped in such courts. The coefficients associated to the court's composition is positive and statistically significant for all probit estimations of *Abandon*. Third, we observe no relation between the court's composition and the probability for an employee to win a

case: this result holds for both cases actually decided by the councilors (*Decision2*) and for cases decided by a professional judge (*Judgement*). Fourth, we observe a significant positive relationship between the level of non-reformism and the probability of *départage*: cases are more likely to go to *départage* in non-reformist courts. The two last results explain the lack of (or the very weak) significance level associated to the court's composition in *Décision3*.

Result 1 In non-reformist courts, controlling for court's section and time fixed-effects, workers (i) are more likely to settle at the conciliation step, (ii) more likely to drop their cases, and (iii) more likely to have their cases decided by a professional judge.

5.6 Further Investigations

In the following section, we go deeper in the analysis to better understand how the composition of the courts influences the litigants' behavior and strategies. To reach this goal, we first explore endogeneity concerns in order to figure out whether the above results capture a causal effect. Second, we wonder whether the volume of the demand for litigation depends on the composition of the courts. Third, we run a triprobit estimation to identify the hidden characteristics of the conciliated and abandoned cases.

5.6.1 Endogeneity Issues

The above findings have shown that the courts' composition is correlated with cases' outcomes. One possible explanation to these results is that our estimations do not correctly capture the causal impact of the courts' composition. Indeed, since councilors are elected by workers, it is likely that a common factor, that we refer to as the *population's preferences*, affects both the councilors' election and the litigants' strategies. Changes in the population's preferences may be correlated with changes in strategies such that coefficients associated with the composition of the courts capture both the causal impact of judges and the latent phenomenon that determines this composition.

More technically, if population's preferences do affect both choices (strategies at court determining the final outcome of a claim and elections of councilors), the above results would suffer from an omitted variable bias. We propose to deal with this source of endogeneity by approximating preferences by results to national presidential elections. This strategy consists in solving the problem of endogeneity by including a proxy for the "omitted" variable. We hope to capture the level of confrontational preferences of the population by controlling for their votes at the national presidential elections. Subsequent estimations would then measure the correlation of non-reformist councilors and cases' outcomes for groups of workers with similar preferences.

Because we are not able to use votes as a direct proxy for the population's preferences[20], local results to national presidential elections appear as an interesting proxy for the preferences for confrontation of the population.

The non-reformist unions are known to be politically more left-oriented than the reformist unions. We use therefore the share of votes of the first round of the presidential elections devoted to left parties to approximate the overall preferences of the population. Because presidential elections do not occur every year, we make a linear intrapolation to obtain the shares of votes for years between election dates.[21] We denote this variable $shareLeft_{jt}$. It is defined at the court's level for each year. In our dataset, it is significantly positively correlated with the share of votes of non-reformist unions for both the date of the conciliation attempt and the decision (p-values=0.000).

Table 5.9 presents the results of the estimations of the baseline models including variable $shareLeft_{jt}$. First, regarding the newly included variable, it is significantly correlated with outcomes (Conciliation, Drop, Decision, Départage, Judgement). In more leftist areas, workers are more likely to conciliate, less likely to abandon, more likely to have their case decided by a professional judges and more likely to win the case. Regarding the probability to win, we detect a positive effect both for the decision of the councilors and the decision of the professional judge.
The estimated coefficients associated with the court's composition are similar to those of our previous estimations. In other words, we still observe a positive relationship between the proportion of confrontational unions and the conciliation, the probability to abandon and *départage*. Quite interestingly, the composition of a court has no significant effect on the decision made by councilors at the *bureau de jugement*, nor on the decision made by the professional judge in case of *départage*.

Result 2 Controlling for the political preferences of the population, court's section and year fixed-effects, a higher proportion of non-reformist judges is associated with (i) more conciliation, (ii) more abandon, and (iii) more départage. There is however no significant

[20]The most natural candidate for the omitted variable would be to include the proportion of votes devoted to non-reformist unions. Let us recall that seats are assigned through a proportional election at the highest average, so that there can be a slight difference between the percentage of votes and the percentage of seats each union gets. In our dataset, the correlation between the proportion of seats and the share of votes devoted to non-reformist unions is equal to 91.7% for the conciliation stage and to 91.4% for the judgment stage. The percentage of votes would capture the preferences while the proportion of seats would capture the real impact of councilors. However, the inclusion of the percentage of votes to the councilors' elections is impossible because the election process is too proportional, and therefore generates too much collinearity.

[21]The coding procedure is as follows. First, we took election data at the city level for all French municipalities. Second, we summed up all votes devoted to left-wing parties for cities belonging to the same court. Third, we computed the share of votes received by left-wing parties over the total number of valid votes. Finally, we computed weighted averages to make linear intrapolations for years with missing values.

impact of non-reformist courts on the decision made on the claims (with or without the intervention of a professional judge).

5.6.2 Volume of Litigation

In order to understand the causal impact of courts' composition, we now investigate whether the volume of litigation changes across courts. More precisely, we propose to explore whether the number of claims that reaches labor courts is correlated with the composition of the courts. We propose to discriminate between three situations: (i) more non-reformist courts might attract more cases (*case inflation*), (ii) fewer cases (*case deflation*), or (iii) might not change the amount of opened claims (*case stagnation*).
To distinguish between these scenarios, we proceed in two steps. First, we collapse the data to obtain a panel dataset that contains the number of cases opened at each section of each labor court per year. We use these data to measure the extent to which the demand for litigation is correlated with the composition of the court (subsection 5.6.2.1). However, because such an investigation might also suffer from the same omitted variable bias as the above baseline models[22], we also explore the impact of the changes of the courts' composition on a limited time-span where we believe preferences are constant (subsection 5.6.2.2).

5.6.2.1 Correlation Between Demand for Litigation and Non-Reformism

To determine whether the volume of litigation is correlated with the composition of a court, we collapse the dataset presented above to obtain for each section j of each labor court i the number of new cases at year t (yc_{ijt}, where yc stands for *yearly cases*). In a similar way, we also compute the number of new cases per councilor (pc_yc_{ijt} where pc stands for *per councilor*). We will use these two variables in turn.

We now estimate by OLS the relationship between the proportion of non-reformist councilors and the volume of the demand for litigation. Our regressions include time and spatial (at the court's section level) fixed effects and some control variables (GDP, unemployment). First, we run the estimation from 1998 to 2012 for the variable yc_{ijt}, but we exclude some courts after 2008.[23] Second, we run the estimation for pc_yc_{ijt} for all years and all labor courts.[24] The estimated coefficients associated to the proportion of

[22]Changes in the demand for litigation might indeed result from changes in the workers' preferences, which may also determine the number of non-reformist councilors.

[23]In 2008, the judiciary map was reformed (Decree n.2008-514 of May 29th, 2008): some labor courts were removed, while others took over their competency. The courts that have expanded their geographical competency have received a great amount of new claims after this reform depending on the size of the removed courts (Espinosa & Desrieux (2015)). We therefore exclude data after 2008 for the courts that expanded their competency.

[24]All councilors from the removed courts were reaffected in the courts that took over removed courts' geographical competency. The number of councilors has not been affected by the reform. Let us precise

Table 5.3: OLS estimates of the level of non-reformism on the volume of the demand for litigation. Robust z-statistics in parentheses.

	Number of claims		Number of claims per councilor	
	Pooled OLS	Within	Pooled OLS	Within
propNR	20.863	20.863	9.188	9.188*
	(1.24)	(1.29)	(1.6)	(1.67)
confront	4.279	4.279*	1.178	1.178
	(1.63)	(1.70)	(1.29)	(1.34)
tv_confront	5.632*	5.632*	1.621	1.621
	(1.71)	(1.78)	(1.44)	(1.50)

non-reformist councilors are displayed in table 5.3.

Table 5.3 does not yield decisive evidence with regard to the correlation between the composition of the court and the volume of litigation. Although all specifications give a positive coefficient, few of them yield a coefficient statistically different from 0 at 10% and none of them at 5%. There is then no convincing evidence of the impact of labor courts' composition on the demand for litigation. Let us now deepen our analysis to deal with potential endogeneity issues.

5.6.2.2 Stable Preferences, Non-Reformism and the Demand for Litigation

Our main challenge here is to capture the causal impact of the proportion of non-reformist judges on the demand for litigation, while controlling for the *preferences* that could affect both the composition of the court and the volume of litigation. To reach this goal, we consider changes in the demand for litigation within a one-year span (6 months before the elections and 6 months after the elections). We assume that, within this period, preferences are relatively stable.

First, we consider the change of councilors in 2003, namely when the councilors who were elected on December, 11th 2002 took office and replaced those elected in 1996. We compute the amount of cases opened at each section i of court j from June to November 2002 ($cases_{i,j,2002}$) and those from January to June 2003 ($cases_{i,j,2003}$). We apply a similar strategy for the 2008 election: We compute the sum of all cases opened between June and November 2008 ($cases_{i,j,2008}$) and those opened between January 2009 and June 2009 ($cases_{i,j,2009}$).

that the reform did not modify the composition of receiving courts. The transfer of cases and councilors' positions occur at the same time as the councilors elected in 2008 took their duties.

Table 5.4: Correlation between the growth rate of the number of claims and changes in the composition of the courts. (P-values in parentheses)

	January 2003			January 2009		
	$\Delta propNR$	$\Delta confront$	$\Delta tv\_confront$	$\Delta propNR$	$\Delta confront$	$\Delta tv\_confront$
Agriculture	0.088	0.125	0.119	0.025	0.022	0.019
	(0.29)	(0.135)	(0.154)	(0.815)	(0.835)	(0.86)
Commerce	-0.062	-0.097	-0.076	-0.045	-0.056	- 0.048
	(0.32)	(0.117)	(0.222)	(0.587)	(0.498)	(0.564)
Diverse Act.	0.007	0.022	0.024	-0.154*	-0.2**	- 0.17*
	(0.936)	(0.785)	(0.769)	(0.091)	(0.027)	(0.06)
Executives	0.132**	0.12*	0.126**	0.004	0.104	0.066
	(0.037)	(0.057)	(0.046)	(0.966)	(0.208)	(0.424)
Industry	-0.001	-0.03	-0.019	0.047	-0.02	0.007
	(0.986)	(0.626)	(0.758)	(0.571)	(0.812)	(0.934)

We then compute the growth rate of cases before/after election. We compute the change in the proportion of seats devoted to non-reformist unions ($\Delta propNR$, $\Delta confront$, $\Delta tv\_confront$), the change of unemployment, and the growth rate of log of the GDP per inhabitant.

Table 5.4 displays the correlation coefficients between the growth rates of the number of claims and the changes in the proportion of non-reformist councilors. Correlation coefficients are computed per section.[25] In order to control for possible changes in the employment market, we also control for unemployment and GDP changes. OLS coefficients of this first-difference estimation are displayed in table 5.5.
Tables 5.4 and 5.5 show a common pattern: on overall, we do not detect any significant increase nor decrease in the volume of the demand for litigation following an increase in the court's non-reformism level. Table 5.4 detects a positive increase for the executives' section, but this increase holds for 2003 only and is not significant when controlling for the situation of the employment market. Both tables detect a decrease in the demand for litigation for the section of diverse activity. Controlling for the employment market increases the significance of the correlation. This result does however not hold for the 2003 replacement.

On the whole, this evidence tends to support the *case stagnation* hypothesis mentioned above: the size of the demand for litigation is independent on the level of non-reformism of a court.

[25]Note that, because of the 2008 reform described in footnote 26, the analysis of the 2009 replacement limits to courts that have not been affected by the reform.

	January 2003			January 2009		
	$\Delta propNR$	$\Delta confront$	$\Delta tv\_confront$	$\Delta propNR$	$\Delta confront$	$\Delta tv\_confront$
Agriculture	1.406	0.341	0.386	0.057	0.05	0.056
	(0.352)	(0.266)	(0.271)	(0.967)	(0.801)	(0.845)
Commerce	-0.381	-0.1*	-0.096	-0.85	-0.206	- 0.221
	(0.201)	(0.057)	(0.124)	(0.171)	(0.343)	(0.355)
Diverse Act.	0.041	0.018	0.024	-0.527**	-0.117***	- 0.126**
	(0.907)	(0.708)	(0.717)	(0.03)	(0.008)	(0.02)
Executives	0.746	0.118	0.148	-0.082	0.105	0.078
	(0.119)	(0.146)	(0.137)	(0.842)	(0.164)	(0.322)
Industry	-0.04	-0.046	-0.035	0.312	-0.03	0.01
	(0.949)	(0.705)	(0.794)	(0.448)	(0.822)	(0.941)

Table 5.5: OLS estimates of the changes in the composition of the courts on the volume of the demand for litigation. Robust standard errors. (P-values in parentheses)

Result 3. The volume of litigation is independent of the composition of a court.

5.6.3 Judicial Outcomes and Quality of Claims

After having investigated the volume of litigation, we now focus on the link between the quality of the claims and the judicial outcomes. Here, we define "quality" by the characteristics of a claim increasing the probability of the plaintiff's victory, such as the number of testimonies, legal rules or clear pieces of evidence in favor of the plaintiff. Thus, good quality cases must be understood as those that are more likely to be won by employees *ceteris paribus*, because the available information on the claim and the rules regulating this type of claim are in favor of the plaintiff. Because our data lack information about such evidence, determinants of the quality of the claims are unobservable variables. We even believe that the quality of claims is the main part of the unobservables determining the winning probability of a plaintiff. The effects of these unobservables are hidden in the error terms of our previous estimations. There is then a need to control for their impacts, as the quality of a claim may also determine the strategies of the litigants in courts: the decisions to conciliate or to abandon a claim could be influenced by its quality. This would influence the composition of the claims reaching the *bureau de jugement*, i.e. the claims on which the councilors have to make a decision. To address this issue, we propose a double sample selection model. As illustrated in figure 5.5, there are two selection stages before the *bureau de jugement*: the decision to conciliate, and the decision to abandon the claim. We then estimate a triprobit model, in which the first step is conciliation, the second step is abandon, and the third step is the decision of the councilors (*Decision2*).[26] The objective is to estimate the correlation coefficients between the error terms of these

[26]The estimation method of the triprobit is detailed in the appendix.

Table 5.6: Results of the triprobit estimation. (Z-statistics in parentheses.)

Step	Conciliation	Abandon	Decision2
propNR	0.03492*	0.34565***	-0.000103
	(1.72)	(19.00)	(-0.00)
ρ_{cd}		-0.685***	
		(-39.14)	
ρ_{ca}		-0.284***	
		(-7.69)	
ρ_{ad}		0.168***	
		(3.65)	
Observations		905,125	
Log-Likelihood		-1,123,310.9	

three steps to figure out whether the unobservables (i.e. the quality of the claims) impact these decisions. This allows us to understand whether conciliated or abandoned claims are of good or bad quality. With such a control of case selection, we will observe whether the composition of the court still impacts the probability to accept the employee's claims.
Let us note ρ_{cd} the correlation between the error terms of the conciliation step and the decision of the councilors. A positive and significant coefficient would indicate that the unobservables that lead to more conciliation also lead to a higher winning probability (by a decision of the councilors). In other words, a positive coefficient would depict a *good quality* case, i.e. a case that would have been favorable to employees if it had reached the *bureau de jugement*. Similarly, we note ρ_{ad} (resp. ρ_{ca}) the correlation between the error terms of the abandon step and the decision (in favor of the plaintiff) of the councilors (resp. the conciliation step).

Table 5.6 displays the results of the triprobit for the variables of interest $propNR$ and the coefficient ρ_{cd} and ρ_{ad}.[27] First, we find a negative correlation coefficient between the error terms of the conciliation phase and the councilors' decisions (ρ_{cd}=-0.685^{***}<0). This implies that claims that are conciliated are claims with a low winning probability for the plaintiff at the *bureau de jugement*, i.e. claims of poor quality. Second, we observe a positive coefficient of correlation between the error terms of the abandon and the councilors'

[27]Note that the triprobit estimation was made with $propNR$. Full estimates are available in the appendix. The estimation was made on Stata and took 3 weeks. The maximum likelihood program was performed using Stata's MNVP package (Cappellari & Jenkins (2003, 2006)). The maximization program is explained in details in the appendix.

decision ($\rho_{ad} = 0.168^{***}$). Abandoned claims would have won at *the bureau de jugement*: they can then be considered as good-quality claims. This result suggests that the abandon is very close to an out-of-court settlement phase, where high-quality cases are settled outside the courtroom. There would be indeed little interest for a plaintiff to abandon a claim with a high winning probability, except if the employer makes a proposal leading to an out of court agreement. Then, parties do not show up at the court hearing, leading to *abandon* outcomes.
It is interesting to put these two results in parallel: settlement occurs both in-court and out-of-court, but concern different types of cases (respectively low- and high-quality cases). This interpretation is confirmed by the negative and statistically significant correlation coefficient between conciliation and abandon ρ_{ca}: this negative coefficient shows that conciliated and abandoned claims have opposite characteristics.
Third, this estimation shows that, controlling for these two selection steps, the coefficient associated with $propNR$ regarding $Decision2$ is still not statistically different from zero. There is no clear evidence that the composition of the court impacts the decision made at the *bureau de jugement*. However, $propNR$ is significantly correlated with *Abandon*, and to a lesser extent, with *Conciliation*. Then, controlling for case selection, non-reformist courts still induce more conciliation and abandon.

Result 4 (i) Conciliated cases are more likely to be decided against the plaintiff; (ii) abandoned cases are more likely to be decided in favor of the employee; and (iii) controlling for the two selection steps that take place after the opening of a claim, the composition of a court does not significantly impact the decision made by the councilors.

5.7 Interpretation of the Results

Let us first summarize our previous results: non-reformist courts are associated with (i) more conciliation, (ii) more abandon, (iii) more départage; (iv) they do not significantly increase or decrease the worker's probability to win (with or without the intervention of a professional judge) and (v) have no impact on the volume of litigation. We also showed that (vi) conciliated cases are low-quality claims (i.e. likely to be lost by the plaintiff at the *bureau de jugement*) while (vii) abandoned claims are high-quality claims (i.e. likely to be won by the plaintiff at the *bureau de jugement*). Controlling for these two selection steps, our previous results still hold: (viii) non-reformist courts are not associated with more favorable decisions for the employees, but lead to more conciliation and abandon.

Let us now comment these results. First, non-reformist courts are not associated with more decisions in favor of the plaintiff (the employee). While non-reformist unions are known to be tougher in debates with the employers at the national level, one could expect that the decisions made in non-reformist courts are on average more in favor of the plaintiff. We do not find such evidence. This seems however quite logical given the institutional context

described in section 5.3. Remember that the decision at *bureau de jugement* is made by a panel of four councilors, two from the employers' college and two from the employees' college. Even if non-reformist councilors claim to be more active in defending the employees' interests, they cannot make the decision on their own. If they would decide in favor of the employee whatever the quality of the claim, the outcome would yield at best more *départage* but not a higher likelihood of the plaintiff's victory, since the councilors from the employers' college need to be also convinced of the decision.

Our results also show that the decision about claims made by the professional judge is not impacted by the composition of the court. This suggests that there is no change in the average quality of the claims reaching *départage* in non-reformist courts (compared to reformist ones). If non-reformist councilors always decide in favor of the employee at the *bureau de jugement* (i.e. even for low-quality claims), this would mean that more low-quality claims would go to *départage* in non-reformist courts. The professional judge would face more low-quality claims and we should observe a negative and significant coefficient associated with $propNR$ (or with $confront$ and $tv\_confront$) on the decision he makes. Indeed, his/her decision should correct the inappropriate judgment of the non-reformist councilors regarding low-quality claims at the previous stage. We do not observe such a result in tables 5.8 and 5.9. The coefficient is never significantly different from zero, suggesting that there is no change in the average quality of claims reaching *départage* in non-reformist courts. Whatever the ideological differences between reformist and non-reformist unions, confrontational councilors do not succeed in increasing the probability of employees to win at the *bureau de jugement*.

Still regarding *départage*, we find that non-reformist courts are associated with a higher frequency of *départage*. As commented above, this does not come from a higher propensity of non-reformist councilors to decide in favor of the plaintiff (even for low-quality claims). We then believe that there is a polarization of the behavior of both employers' councilors and employees' councilors. By polarization, we mean that councilors are less likely to reach an agreement for claims with mixed evidence. Because non-reformist unions are known to be tough and often opposed to the employers' propositions at the national level, the employers' councilors may anticipate disagreements (at the court's level) and themselves behave less cooperatively when evidence is mixed. In other words, tough behavior of non-reformist councilors would lead to tough behavior of the employers' councilors. This situation is all the more likely to appear for medium-quality claims as they are those depending the most on the councilors' interpretation of legal rules, and, thus, the most disputable. Then, more medium-quality claims are reaching *départage* in non-reformist courts: the probability of *départage* is impacted by the composition of the court, but not the decision made in this procedure, as it mainly concerns medium-quality claims (that can be either won or lost by the plaintiff).[28]

[28]On this regard, our results are in line with Priest & Klein (1984)'s argument whereby the percentage of litigated cases won by the plaintiffs is around 50% whatever the legal standard, because of selection effects.

Our results show that non-reformist courts are also associated with more conciliation and abandon. Since claims are more likely to end with *départage* in non-reformist courts, this means that the average duration of a claim in non-reformist courts is likely to be longer.[29] Anticipating these long delays, both the plaintiff and the defendant have an interest to settle rather than to litigate. This then explains why both conciliation and abandon are more frequently observed in non-reformist courts: these two judicial outcomes correspond to settlement, as described in subsection 5.6.3. In parallel, the results of our triprobit estimations show that low-quality claims are rather conciliated, while high-quality claims are rather abandoned. Because of a higher probability to go to *départage* in non-reformist courts, the parties are more likely to conciliate for low-quality claims and to abandon for high-quality claims (i.e. to find an out-of-the court agreement). There are then fewer low-quality claims in non-reformist courts, but also fewer high-quality claims. Cases that remain are claims with mixed evidence, that can be either won or lost by the plaintiff. This explains why non-reformist courts are not associated with more (or fewer) decisions in favor of the plaintiff (employee) that would be caused by an "in-court" selection effect.[30] Let us now focus on pre-court selection effects: Do non-reformist courts attract different claims than reformist ones? First, regarding the volume of litigation, there is no evidence that the demand for litigation would be higher or lower in non-reformist courts (see subsection 5.6.2). Another question is whether the quality of this demand is different. Our results cannot provide a full answer to this point. It may be that the quality of claims is constant across kinds of courts. This would be consistent with the absence of correlation between the courts' decisions and their composition. However, since we also observe more conciliation in non-reformists courts, it may be that these courts attract on average more low-quality claims (assuming that high-quality claims would be settled before opening a claim). In this situation, the increase in conciliation would be sufficiently large in non-reformist court to offset both the decrease in quality and the increase in abandon and to maintain the ratio of low-quality vs. high-quality cases that reach the *bureau de jugement* constant across kinds of courts.[31]

5.8 Conclusion

This paper studies labor law enforcement by elected judges representing workers' and employers' unions. We focus on the heterogeneity of the judges elected by the employees to explore how the composition of the courts influences the strategies of the litigants and the decisions about the claims. Our results show that judges in non-reformist courts fail more

[29]The average duration of a claim is about 12 months without *départage* and litigants have to wait again 15 months in some courts to get the decision resulting from the *départage* (Lacabarats (2014))

[30]By in-court selection effect, we mean the selection of the claims resulting from the first two steps at court: conciliation and abandon.

[31]This would be the only way to account for the absence of significant impact of the court's composition on $Decision2$.

often at finding an agreement, and thus need more frequently to call a professional judge to make the decision. Anticipating longer delays because of this procedure, litigants are more likely to settle in-court and out-of-court. We show that in-court settlement mainly concerns cases that are unfavorable to the employees, while out-of-court settlement targets cases that are favorable to employees. We also found that the volume of the demand for litigation is independent from the court's composition.
Our findings have some implications for labor law enforcement. While judges are elected on unions' lists, we find no impact on the decision made in courts. We believe that the procedural rules governing the labor courts explain this result: judges elected by the workers do not make the decision on their own, but with the representatives of the employers, on a parity basis. This result illustrates how organizational and institutional features may prevent ideologically based decisions, when justice is made by politically oriented people.[32]
Our results also show that the composition of a court is not neutral on the strategies of the litigants. However, welfare implications are difficult to assess. One could think that more settlement is welfare-enhancing both for the parties and the society. But, without any information on the contents of agreements, such a conclusion can be discussed.

Future research could go in several directions. First, while non-reformist and reformist courts do not differ in their probability to accept or reject a claim, one could imagine that the amount of compensation a plaintiff gets varies in both types of courts. More detailed data could allow to test whether the composition of the court has some impact on the amount of compensation a party is entitled to. Second, the motivation of the claims could also be investigated. In this dataset, we only know that 80% of claims are about individual dismissals, but we cannot distinguish between the different types of claims brought to court. Should this be possible, we could wonder whether non-reformist courts attract claims with different motivations than the other courts. A third investigation could be to explore what happens before the opening of a claim. We could think that the composition of the courts impacts the number of conflicts *per se* and the informal agreements that may occur between the disputants before the decision to go to court. This would probably require data at the firm level, allowing to follow the conflicts from their very beginning. Fourth, a study could also be elaborated on the appeal process. A recent report for the Ministry of Justice (Lacabarats (2014)) mentions that 60% of decisions are appealed. However, the determinants of appeals are not really identified, so that research on this topic could be

[32]This result contributes to the debates about the perceived politicization of the judiciary system. This forced the European Court for Human Rights (ECHR) to intervene. In a series of decisions, the ECHR ruled that the presence of lay judges does not violate Article 6 of the European Convention on Human Rights, and thus does not fail to meet the impartiality requirement. It considered that in the absence of lay assessors' interests contrary to those of the applicant, the Labour Court did not fail to meet the requirement of impartiality (Van Hiel (2010)). However, the debate is far from over. In December 2013, the Group of States against Corruption (GRECO) from the Council of Europe recommended to France to reform of "the labor and commercial courts, which are composed of lay judges who are subject to few effective safeguards".

useful to better understand labor law enforcement. Last, complementary analyses could be done to understand whether the composition of the courts have consequences on economic outcomes, such as job flows on the labor market or firms' creations or exits. This would allow us to have a broader picture of the consequences of labor law enforcement by unions' representatives.

Appendix 5.A: Probit and Ordered Probit Estimations

Table 5.7: Descriptive statistics per regression sample. Means. Standard deviations in parentheses.

Variable	Conciliation	Abandon	Decision2	Départage	Judgement
concil	.135				
	(.3417)				
abandon		.278			
		(.448)			
decision2			.7315		
			(.4432)		
departage				.162	
				(.3685)	
judgement					.6988
					(.4588)
propNR	.5205	.5203	.5192	.5189	.5128
	(.2167)	(.2133)	(.2161)	(.2141)	(.207)
confront	.7878				
	(1.0708)				
tv_confront	-1.0634				
	(1.0151)				
shareLeft	.4289	.4285	.4279	.4288	.4331
	(.0636)	(.0648)	(.0643)	(.0642)	(.0636)
durationLY	396.183	412.0879	403.1429	410.3286	442.7418
	(127.9133)	(134.6883)	(130.755)	(134.7873)	(148.263)
unemployment	8.2339	8.1177	8.1434	8.1262	8.057
	(2.0714)	(1.8837)	(1.869)	(1.87)	(1.885)
lngdp	10.2258	10.2571	10.2387	10.2506	10.3084
	(.2867)	(.2946)	(.2893)	(.2918)	(.2984)
woman	.3842	.3788	.376	.3811	.405
	(.4864)	(.4851)	(.4844)	(.4857)	(.4909)
def_lawyer	.2074	.2139	.2391	.2279	.1743
	(.4055)	(.41)	(.4265)	(.4195)	(.3793)
def_unionWorker	.0004	.0004	.0005	.0004	.0003
	(.0206)	(.0203)	(.0214)	(.0206)	(.0172)
def_unionEmployer	.0017	.0014	.0016	.0015	.0011
	(.0415)	(.0373)	(.0405)	(.0392)	(.0324)
plaintiff_lawyer	.1917	.1946	.2097	.2043	.1799
	(.3937)	(.3959)	(.4071)	(.4032)	(.3841)
plaintiff_unionWorker	.0718	.0677	.0758	.0718	.0541
	(.2582)	(.2512)	(.2647)	(.2581)	(.2262)

Table 5.8: Results of probit and ordered probit estimation of the set of dependent variables. **BASELINE** model. T-values in parentheses. Robust standard errors.

Model	Variable Technique	Conciliation Probit	Abandon Probit	Decision3 Ordered Pr.	Decision2 Probit	Départage Probit	Judgement Probit
(1)	propNR	0.0379**	0.371***	-0.0360**	-0.0254	0.0710***	-0.00835
		(2.172)	(23.59)	(-2.078)	(-1.234)	(3.188)	(-0.153)
(2)	confront	0.00342	0.0618***	0.00228	0.00529	0.00660	-0.00166
		(1.053)	(21.01)	(0.707)	(1.386)	(1.585)	(-0.162)
(3)	tv_confront	0.00802**	0.0810***	-0.00445	-0.000375	0.0166***	-0.00218
		(2.055)	(23.15)	(-1.153)	(-0.0821)	(3.331)	(-0.179)
	(CPH × Section) FE	Yes	Yes	Yes	Yes	Yes	Yes
	Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Table 5.9: Results of probit and ordered probit estimation of the set of dependent variables. **ENDOG** Model. T-values in parentheses. Robust standard errors.

Model	Variable Technique	Conciliation Probit	Abandon Probit	Decision3 Ordered Pr.	Decision2 Probit	Départage Probit	Judgement Probit
(1)	propNR	0.0502***	0.364***	-0.0358**	-0.0216	0.0898***	0.0102
		(2.861)	(23.00)	(-2.056)	(-1.042)	(4.020)	(0.186)
	shareLeft	1.150***	-0.452***	0.0176	0.274*	1.599***	1.806***
		(8.018)	(-3.826)	(0.140)	(1.820)	(9.951)	(4.567)
(2)	confront	0.00565*	0.0604***	0.00242	0.00611	0.0104**	0.00239
		(1.731)	(20.39)	(0.745)	(1.592)	(2.492)	(0.233)
	shareLeft	1.136***	-0.478***	0.0518	0.315**	1.582***	1.808***
		(7.918)	(-4.041)	(0.411)	(2.091)	(9.836)	(4.570)
(3)	tv_confront	0.0112***	0.0795***	-0.00437	0.000695	0.0221***	0.00331
		(2.867)	(22.50)	(-1.125)	(0.151)	(4.407)	(0.269)
	shareLeft	1.156***	-0.408***	0.0257	0.293*	1.623***	1.811***
		(8.048)	(-3.441)	(0.203)	(1.941)	(10.07)	(4.571)
	(CPH × Section) FE	Yes	Yes	Yes	Yes	Yes	Yes
	Year FE	Yes	Yes	Yes	Yes	Yes	Yes
	N	1,343,494	1,160,938	838,895	703,455	840,020	125,613

Appendix 5.B: Elections in Labour Courts

Summary statistics for each election between 1998 and 2012 are presented in the appendix (tables 5.10, 5.11, 5.12).

Table 5.10: Summary Statistics of the share of seats obtained by each union at the 1997 elections.

Union	Share of seats			
	Mean	St. Dev.	Min	Max
CGT	0.346	0.190	0	0.833
CFDT	0.322	0.157	0	1
FO	0.220	0.133	0	0.75
CGC	0.065	0.137	0	0.75
CFTC	0.031	0.076	0	0.5
UNSA	0.005	0.039	0	0.5
CSL	0.003	0.018	0	0.25
GDIX	0.001	0.0123	0	0.25
DIV	0.007	0.045	0	0.6

Table 5.11: Summary Statistics of the share of seats obtained by each union at the 2002 elections.

Union	Share of seats			
	Mean	St. Dev.	Min	Max
CGT	0.350	0.188	0	0.8
CFDT	0.319	0.151	0	1
FO	0.183	0.131	0	0.667
CFE-CGC	0.071	0.149	0	0.75
CFTC	0.049	0.093	0	0.5
UNSA	0.016	0.058	0	0.5
GSEA	0.000	0.002	0	0.07
GDIX	0.003	0.028	0	0.5
DIV	0.007	0.052	0	0.75

Table 5.12: Summary Statistics of the share of seats obtained by each union at the 2008 elections.

Union	Share of seats			
	Mean	St. Dev.	Min	Max
CGT	0.397	0.189	0	1
CFDT	0.269	0.143	0	1
FO	0.160	0.113	0	0.667
CFE-CGC	0.089	0.172	0	0.75
CFTC	0.036	0.0814	0	1
UNSA	0.026	0.067	0	0.5
Solidaires	0.004	0.020	0	0.2
DIV	0.019	0.105	0	1

Appendix 5.C: Bayesian Estimation of Ideal Points

The Bayesian estimation of ideal points is usually referred as the *one dimensional item response theory*. Such models originally aimed at measuring students' performance to a test, and to locate them on a unique dimension. The objective consisted in estimating three sets of parameters: (*i*) an ability parameter for each student, (*ii*) a difficulty parameter for each question of the test, and (*iii*) a discrimination parameter for each question. Bayesian methods were developed to discriminate students according to their ability, by taking into account questions' difficulty level, and by estimating their 'relevance' to correctly discriminate students.[33]
These models have then be used in the political science literature, especially in the case of Supreme Court voting, where researchers were willing to locate Justices on a liberal-conservative dimension. Our approach follows this literature: we aim at locating unions on a *confrontationalism* axis by investigating their ability to successfully negotiate with firms owners.

More formally, our goal consists in estimating unions' positions (β_i) on a *confrontationalism* axis. To do so, we also need to estimate ANI-specific parameters, i.e. their location on the *confrontationalism* axis (α_j) and their discrimination parameter (γ_j), i.e. their capacity to discriminate unions on the *confrontationalism* dimension. The model is defined by a logistic utility model, where the latent utility depends both on unions and ANI parameters:

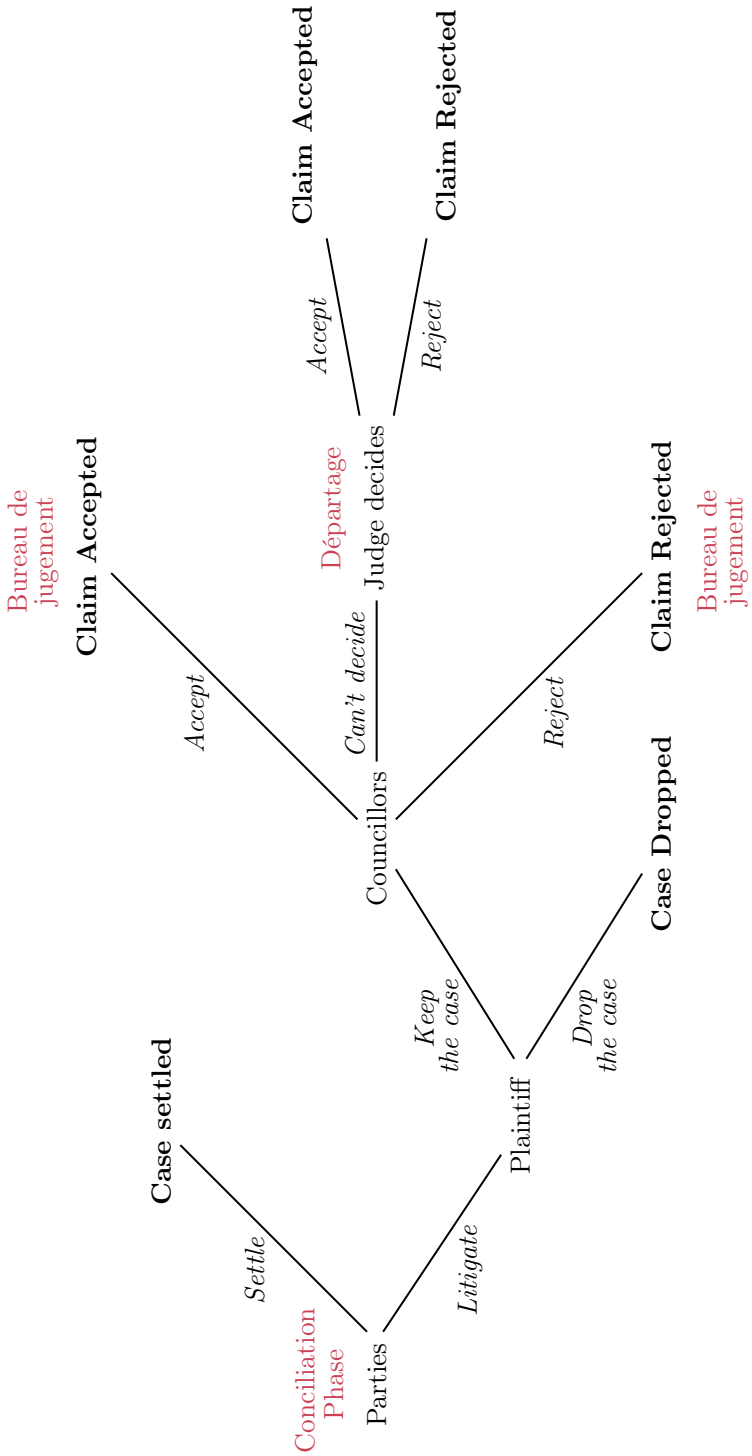
$$u_{i,j} = -\alpha_j + \gamma_j \beta_i + \epsilon_{i,j} \tag{5.5}$$

where $u_{i,j}$ is the utility of union i to ratify ANI j, and $\epsilon_{i,j}$ is a random component.
Ideal points are assumed to be normally distributed with mean μ_β and variance σ^2_β, and the ANI-specific parameters are assumed to be jointly distributed : $(\alpha_j, \gamma_j) \sim N_2(M, T^{-1})$. In order to avoid additive and multiplicative aliasing, as well as reflection invariance, we set parameters' priors to default values of the *MCMCpackage* in R ($\mu_\beta = 0$, $\sigma_\beta = 1$, $M = 0$, and $T = 0.25$). Moreover, identification requires an additional constraint on the ideal points. Since our goal is to create a confrontational scale, we assume that the CGT, which is usually seen as the least likely to negotiate with firm owners, is more confrontational than the CFDT, which is seen as the most reformist union. In other words, we constraint the model such that the CGT will get a positive score on the confrontational dimension, while the CFDT will get a negative score on the confrontational dimension. Of course, such a constraint does not claim that the CFDT is not confrontational at all, it only assumes that the CGT is more confrontational than the CFDT.

[33] Researchers anticipated the possibility that some questions could be correctly answered by low-skilled students and wrongly answered by high-skilled students

Appendix 5.D: The Legal Process in French Labour Courts

Figure 5.5: Sequence of the legal process (strategies in italics, final observed outcome in bold).



Appendix 5.E: BASELINE and ENDOG Tables

We display the full set of estimates of the main regressions of the paper. Tables 5.13, 5.14 and 5.15 summarize the estimations of the BASELINE model, where the court's composition is proxied by *propNR*, *confront* and *tv_confront* respectively. Tables 5.16, 5.17 and 5.18 show the estimates for the ENDOG model for *propNR*, *confront* and *tv_confront* respectively.

Table 5.13: Results of probit and ordered probit estimation of the set of dependent variables. Court's composition: **propNR**. **BASELINE** Model. Robust z-statistics in parentheses .

Variable Technique	Conciliation Probit	Abandon Probit	Decision3 Ordered Pr.	Decision2 Probit	Départage Probit	Judgement Probit
propNR	0.0379**	0.371***	-0.0360**	-0.0254	0.0710***	-0.00835
	(2.172)	(23.59)	(-2.078)	(-1.234)	(3.188)	(-0.153)
avDuraffLY	5.84e-05***	-0.000309***				
	(2.932)	(-18.17)				
unemployment	0.00114	-0.00675***	-0.00607***	-0.00373***	0.0141***	0.0188***
	(1.430)	(-7.425)	(-6.650)	(-3.375)	(11.93)	(6.879)
lngdppc	0.693***	-0.239***	-0.364***	-0.409***	0.358***	-1.310***
	(10.91)	(-4.407)	(-6.328)	(-5.855)	(4.825)	(-7.571)
gender	0.0634***	-0.0295***	0.0412***	0.0644***	0.0448***	0.0470***
	(21.44)	(-10.92)	(14.42)	(18.31)	(12.21)	(5.729)
def_lawyer	-0.256***	-0.154***	0.0722***	0.0622***	-0.115***	-0.152***
	(-51.38)	(-34.76)	(15.52)	(11.66)	(-19.88)	(-9.356)
def_workerUnion	-0.0257	-0.195***	-0.0239	-0.0397	-0.0624	-0.652***
	(-0.390)	(-3.156)	(-0.338)	(-0.511)	(-0.650)	(-3.088)
def_employerUnion	0.431***	-0.254***	-0.170***	-0.228***	-0.233***	-0.113
	(15.05)	(-6.902)	(-4.732)	(-5.828)	(-4.586)	(-0.879)
plaint_lawyer	0.108***	-0.142***	0.0133*	0.0343***	0.0839***	0.0958***
	(18.09)	(-23.21)	(1.783)	(4.229)	(10.00)	(3.108)
plaint_union	0.279***	-0.191***	-0.0440***	-0.0235**	0.116***	-0.0858**
	(38.46)	(-25.04)	(-4.939)	(-2.399)	(10.73)	(-2.482)
(CPH × Section) FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,344,121	1,161,476	839,27	703,812	840,458	125,617
LL	-503525	-667209	-765516	-397024	-338838	-71266
Pseudo-R2	0.0535	0.0282	0.0190	0.0302	0.0898	0.0727

Table 5.14: Results of probit and ordered probit estimation of the set of dependent variables. Court's composition: **confront**. **BASELINE** Model. Robust z-statistics in parentheses.

Variable	Conciliation	Abandon	Decision3	Decision2	Départage	Judgement
Technique	Probit	Probit	Ordered Pr.	Probit	Probit	Probit
confront	0.00342	0.0618***	0.00228	0.00529	0.00660	-0.00166
	(1.053)	(21.01)	(0.707)	(1.386)	(1.585)	(-0.162)
avDuraffLY	5.80e-05***	-0.000308***				
	(2.911)	(-18.11)				
unemployment	0.00113	-0.00692***	-0.00608***	-0.00377***	0.0141***	0.0188***
	(1.424)	(-7.622)	(-6.662)	(-3.404)	(11.94)	(6.878)
lngdppc	0.684***	-0.274***	-0.343***	-0.386***	0.340***	-1.310***
	(10.79)	(-5.065)	(-5.971)	(-5.538)	(4.589)	(-7.568)
gender	0.0635***	-0.0295***	0.0412***	0.0644***	0.0448***	0.0470***
	(21.44)	(-10.92)	(14.43)	(18.31)	(12.20)	(5.730)
def_lawyer	-0.257***	-0.154***	0.0720***	0.0621***	-0.115***	-0.152***
	(-51.40)	(-34.74)	(15.48)	(11.64)	(-19.85)	(-9.353)
def_workerUnion	-0.0254	-0.198***	-0.0251	-0.0409	-0.0619	-0.652***
	(-0.386)	(-3.202)	(-0.355)	(-0.526)	(-0.645)	(-3.087)
def_employerUnion	0.431***	-0.254***	-0.170***	-0.228***	-0.233***	-0.113
	(15.04)	(-6.915)	(-4.721)	(-5.818)	(-4.593)	(-0.879)
plaint_lawyer	0.108***	-0.143***	0.0133*	0.0343***	0.0839***	0.0958***
	(18.08)	(-23.44)	(1.782)	(4.224)	(9.999)	(3.108)
plaint_union	0.279***	-0.192***	-0.0439***	-0.0234**	0.116***	-0.0857**
	(38.44)	(-25.27)	(-4.931)	(-2.392)	(10.72)	(-2.480)
(CPH × Section) FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,344,121	1,161,476	838,586	703,183	840,458	125,533
LL	-503527	-667265	-764945	-396677	-338913	-71234
Pseudo-R2	0.0535	0.0281	0.0189	0.0302	0.0896	0.0726

Table 5.15: Results of probit and ordered probit estimation of the set of dependent variables. Court's composition: **tv_confront**. **BASELINE** Model. Robust z-statistics in parentheses.

Variable	Conciliation	Abandon	Decision3	Decision2	Départage	Judgement
Technique	Probit	Probit	Ordered Pr.	Probit	Probit	Probit
tv_confront	0.00802**	0.0810***	-0.00445	-0.000375	0.0166***	-0.00218
	(2.055)	(23.15)	(-1.153)	(-0.0821)	(3.331)	(-0.179)
avDuraffLY	5.81e-05***	-0.000310***				
	(2.916)	(-18.19)				
unemployment	0.00113	-0.00681***	-0.00608***	-0.00375***	0.0141***	0.0188***
	(1.427)	(-7.498)	(-6.656)	(-3.389)	(11.93)	(6.878)
lngdppc	0.691***	-0.247***	-0.357***	-0.398***	0.360***	-1.311***
	(10.89)	(-4.562)	(-6.203)	(-5.708)	(4.854)	(-7.562)
gender	0.0635***	-0.0295***	0.0412***	0.0644***	0.0448***	0.0470***
	(21.44)	(-10.92)	(14.43)	(18.31)	(12.21)	(5.730)
def_lawyer	-0.257***	-0.154***	0.0721***	0.0621***	-0.115***	-0.152***
	(-51.39)	(-34.76)	(15.51)	(11.65)	(-19.88)	(-9.356)
def_workerUnion	-0.0260	-0.197***	-0.0242	-0.0402	-0.0630	-0.652***
	(-0.394)	(-3.188)	(-0.342)	(-0.517)	(-0.657)	(-3.087)
def_employerUnion	0.431***	-0.255***	-0.170***	-0.228***	-0.233***	-0.113
	(15.04)	(-6.929)	(-4.728)	(-5.824)	(-4.586)	(-0.879)
plaint_lawyer	0.108***	-0.142***	0.0133*	0.0343***	0.0839***	0.0958***
	(18.09)	(-23.28)	(1.785)	(4.229)	(9.994)	(3.108)
plaint_union	0.279***	-0.192***	-0.0439***	-0.0235**	0.116***	-0.0857**
	(38.45)	(-25.18)	(-4.931)	(-2.394)	(10.72)	(-2.481)
(CPH × Section) FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,344,121	1,161,476	839,27	703,812	840,458	125,617
LL	-503525	-667218	-765517	-397024	-338838	-71266
Pseudo-R2	0.0535	0.0282	0.0190	0.0302	0.0898	0.0727

Table 5.16: Results of probit and ordered probit estimation of the set of dependent variables. Court's composition: **propNR**. **ENGOD** Model. Robust z-statistics in parentheses.

Variable	Conciliation	Abandon	Decision3	Decision2	Départage	Judgement
Technique	Probit	Probit	Ordered Pr.	Probit	Probit	Probit
propNR	0.0502***	0.364***	-0.0358**	-0.0216	0.0898***	0.0102
	(2.861)	(23.00)	(-2.056)	(-1.042)	(4.020)	(0.186)
shareLeft	1.150***	-0.452***	0.0176	0.274*	1.599***	1.806***
	(8.018)	(-3.826)	(0.140)	(1.820)	(9.951)	(4.567)
avDuraffLY	4.04e-05**	-0.000299***				
	(2.015)	(-17.36)				
unemployment	0.00110	-0.00673***	-0.00605***	-0.00367***	0.0141***	0.0186***
	(1.378)	(-7.402)	(-6.627)	(-3.320)	(11.95)	(6.820)
lngdppc	0.542***	-0.157***	-0.368***	-0.463***	0.00679	-1.721***
	(8.182)	(-2.687)	(-5.796)	(-6.121)	(0.0824)	(-8.768)
gender	0.0635***	-0.0296***	0.0413***	0.0646***	0.0449***	0.0470***
	(21.46)	(-10.94)	(14.45)	(18.35)	(12.22)	(5.727)
def_lawyer	-0.256***	-0.155***	0.0717***	0.0618***	-0.113***	-0.150***
	(-51.36)	(-34.79)	(15.40)	(11.58)	(-19.54)	(-9.231)
def_workerUnion	-0.0287	-0.194***	-0.0243	-0.0404	-0.0620	-0.648***
	(-0.435)	(-3.140)	(-0.344)	(-0.520)	(-0.646)	(-3.073)
def_employerUnion	0.432***	-0.255***	-0.171***	-0.228***	-0.229***	-0.109
	(15.07)	(-6.927)	(-4.740)	(-5.826)	(-4.520)	(-0.847)
plaint_lawyer	0.108***	-0.142***	0.0135*	0.0346***	0.0839***	0.0963***
	(18.00)	(-23.22)	(1.815)	(4.263)	(9.999)	(3.124)
plaint_union	0.278***	-0.191***	-0.0438***	-0.0234**	0.116***	-0.0857**
	(38.32)	(-25.05)	(-4.917)	(-2.387)	(10.71)	(-2.478)
(CPH × Section) FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,343,494	1,160,938	838,895	703,455	840,02	125,613
LL	-503238	-666878	-765264	-396841	-338718	-71253
Pseudo-R2	0.0536	0.0282	0.0189	0.0302	0.0898	0.0728

Table 5.17: Results of probit and ordered probit estimation of the set of dependent variables. Court's composition: **confront**. **ENGOD** Model. Robust z-statistics in parentheses.

Variable	Conciliation	Abandon	Decision3	Decision2	Départage	Judgement
Technique	Probit	Probit	Ordered Pr.	Probit	Probit	Probit
confront	0.00565*	0.0604***	0.00242	0.00611	0.0104**	0.00239
	(1.731)	(20.39)	(0.745)	(1.592)	(2.492)	(0.233)
shareLeft	1.136***	-0.478***	0.0518	0.315**	1.582***	1.808***
	(7.918)	(-4.041)	(0.411)	(2.091)	(9.836)	(4.570)
avDuraffLY	4.01e-05**	-0.000298***				
	(2.000)	(-17.27)				
unemployment	0.00109	-0.00690***	-0.00606***	-0.00370***	0.0141***	0.0186***
	(1.372)	(-7.596)	(-6.638)	(-3.347)	(11.97)	(6.821)
lngdppc	0.535***	-0.187***	-0.354***	-0.448***	-0.00684	-1.720***
	(8.078)	(-3.211)	(-5.582)	(-5.932)	(-0.0830)	(-8.765)
gender	0.0635***	-0.0296***	0.0413***	0.0646***	0.0448***	0.0470***
	(21.46)	(-10.94)	(14.46)	(18.35)	(12.22)	(5.725)
def_lawyer	-0.257***	-0.154***	0.0715***	0.0617***	-0.113***	-0.150***
	(-51.38)	(-34.78)	(15.37)	(11.57)	(-19.51)	(-9.232)
def_workerUnion	-0.0286	-0.197***	-0.0255	-0.0417	-0.0616	-0.648***
	(-0.434)	(-3.182)	(-0.361)	(-0.536)	(-0.642)	(-3.074)
def_employerUnion	0.432***	-0.255***	-0.170***	-0.228***	-0.229***	-0.109
	(15.06)	(-6.941)	(-4.728)	(-5.814)	(-4.526)	(-0.846)
plaint_lawyer	0.108***	-0.143***	0.0135*	0.0346***	0.0839***	0.0963***
	(17.99)	(-23.44)	(1.814)	(4.258)	(9.993)	(3.123)
plaint_union	0.278***	-0.193***	-0.0437***	-0.0233**	0.116***	-0.0857**
	(38.30)	(-25.27)	(-4.909)	(-2.382)	(10.70)	(-2.479)
(CPH × Section) FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,343,494	1,160,938	838,895	703,455	840,02	125,613
LL	-503241	-666933	-765266	-396840	-338723	-71253
Pseudo-R2	0.0536	0.0281	0.0189	0.0302	0.0898	0.0728

Table 5.18: Results of probit and ordered probit estimation of the set of dependent variables. Court's composition: **tv_confront**. **ENGOD** Model. Robust z-statistics in parentheses.

Variable Technique	Conciliation Probit	Abandon Probit	Decision3 Ordered Pr.	Decision2 Probit	Départage Probit	Judgement Probit
tv_confront	0.00565*	0.0604***	0.00242	0.00611	0.0104**	0.00239
	(1.731)	(20.39)	(0.745)	(1.592)	(2.492)	(0.233)
shareLeft	1.136***	-0.478***	0.0518	0.315**	1.582***	1.808***
	(7.918)	(-4.041)	(0.411)	(2.091)	(9.836)	(4.570)
avDuraffLY	4.01e-05**	-0.000298***				
	(2.000)	(-17.27)				
unemployment	0.00109	-0.00690***	-0.00606***	-0.00370***	0.0141***	0.0186***
	(1.372)	(-7.596)	(-6.638)	(-3.347)	(11.97)	(6.821)
lngdppc	0.535***	-0.187***	-0.354***	-0.448***	-0.00684	-1.720***
	(8.078)	(-3.211)	(-5.582)	(-5.932)	(-0.0830)	(-8.765)
gender	0.0635***	-0.0296***	0.0413***	0.0646***	0.0448***	0.0470***
	(21.46)	(-10.94)	(14.46)	(18.35)	(12.22)	(5.725)
def_lawyer	-0.257***	-0.154***	0.0715***	0.0617***	-0.113***	-0.150***
	(-51.38)	(-34.78)	(15.37)	(11.57)	(-19.51)	(-9.232)
def_workerUnion	-0.0286	-0.197***	-0.0255	-0.0417	-0.0616	-0.648***
	(-0.434)	(-3.182)	(-0.361)	(-0.536)	(-0.642)	(-3.074)
def_employerUnion	0.432***	-0.255***	-0.170***	-0.228***	-0.229***	-0.109
	(15.06)	(-6.941)	(-4.728)	(-5.814)	(-4.526)	(-0.846)
plaint_lawyer	0.108***	-0.143***	0.0135*	0.0346***	0.0839***	0.0963***
	(17.99)	(-23.44)	(1.814)	(4.258)	(9.993)	(3.123)
plaint_union	0.278***	-0.193***	-0.0437***	-0.0233**	0.116***	-0.0857**
	(38.30)	(-25.27)	(-4.909)	(-2.382)	(10.70)	(-2.479)
(CPH × Section) FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,343,494	1,160,938	838,895	703,455	840,02	125,613
LL	-503241	-666933	-765266	-396840	-338723	-71253
Pseudo-R2	0.0536	0.0281	0.0189	0.0302	0.0898	0.0728

Appendix 5.F: Triprobit

The latent utilities of *Conciliation*, *Abandon* and *Decision2* are defined as follows:

$$\begin{aligned} y^*_{ci} &= X_{ci}\beta_c + u_{ci} \\ y^*_{ai} &= X_{ai}\beta_a + u_{ai} \\ y^*_{di} &= X_{di}\beta_d + u_{di} \end{aligned} \tag{5.6}$$

The outcome of the Abandon decision is not observed if the case is conciliated. The decision of the councilors is not available if the case is conciliated or dropped. The system of observed outcomes is:

$$\begin{aligned} y_{ci} &= \begin{cases} 0 & \text{if } y^*_{ci} \leq 0 \\ 1 & \text{if } y^*_{ci} > 0 \end{cases} \\ y_{ai} &= \begin{cases} . & \text{if } y^*_{ci} \leq 0 \\ 0 & \text{if } y^*_{ai} \leq 0 \text{ and } y^*_{ci} > 0 \\ 1 & \text{if } y^*_{ai} > 0 \text{ and } y^*_{ci} > 0 \end{cases} \\ y_{di} &= \begin{cases} . & \text{if } y^*_{ai} \leq 0 \text{ or } y^*_{ci} \leq 0 \\ 0 & \text{if } y^*_{di} \leq 0 \text{ and } y^*_{ai} > 0 \text{ and } y^*_{ci} > 0 \\ 1 & \text{if } y^*_{di} > 0 \text{ and } y^*_{ai} > 0 \text{ and } y^*_{ci} > 0 \end{cases} \end{aligned} \tag{5.7}$$

Assuming that the error terms are normally distributed, we compute the contributions to the likelihood. We note ρ_{ca}, ρ_{cd} and ρ_{ad} the correlations between u_c and u_a, u_c and u_d, u_a and u_d. We note $V = \begin{bmatrix} 1 & & \\ \rho_{ca} & 1 & \\ -\rho_{cd} & -\rho_{ad} & 1 \end{bmatrix}$ and $V' = \begin{bmatrix} 1 & & \\ \rho_{ca} & 1 & \\ \rho_{cd} & \rho_{ad} & 1 \end{bmatrix}$.

The contribution for a conciliated case is:

$$\Phi_1(X_{ci}\beta_c) \tag{5.8}$$

The contribution for an abandoned case is:

$$\Phi_2(-X_{ci}\beta_c, X_{ai}\beta_a; -\rho_{ac}) \tag{5.9}$$

The contribution for a case decided by the councilors in favor of the employee:

$$\Phi_3(-X_{ci}\beta_c, -X_{ai}\beta_a, X_{di}\beta_d; V) \tag{5.10}$$

The contribution for a case decided by the councilors against the employee:

$$\Phi_3(-X_{ci}\beta_c, -X_{ai}\beta_a, -X_{di}\beta_d; V') \tag{5.11}$$

The associated Stata maximization program is:

```
program triprob
version 11.0
args lnf x1 x2 x3 r12 r13 r23
    tempname p1 p01 p000 p001 positif negatif V2 V3 rf12 rf22 rf13 rf23 rf33
    gen ‘positif’=1
    gen ‘negatif’=-1

   //Generate cholesky matrices
   sum ‘r12’, meanonly
   scalar ‘rf12’=r(mean)
   scalar ‘rf22’=sqrt(1-‘rf12’^2)
   sum ‘r13’, meanonly
   scalar ‘rf13’=r(mean)
   sum ‘r23’, meanonly
   scalar ‘rf23’=r(mean)
   scalar ‘rf33’=sqrt(1-‘rf13’^2-‘rf23’^2)

   mat ‘V2’=(1,0 \ ‘rf12’,‘rf22’)
   mat ‘V3’=(1,0,0 \ ‘rf12’,‘rf22’,0\ ‘rf13’,‘rf23’,‘rf33’)

   //Likelihood contribution
   quietly{
    gen ‘p1’=normal(‘x1’) if $ML_y1==1
    egen ‘p01’=mvnp(‘x1’ ‘x2’) if $ML_y2==1, chol(‘V2’) dr($dr) prefix(z)/*
         */signs(‘negatif’ ‘positif’)  adoonly
    egen ‘p000’=mvnp(‘x1’ ‘x2’ ‘x3’) if $ML_y3==0, chol(‘V3’) dr($dr) prefix(z)/*
         */signs(‘negatif’ ‘negatif’ ‘negatif’)  adoonly
    egen ‘p001’=mvnp(‘x1’ ‘x2’ ‘x3’) if $ML_y3==1, chol(‘V3’) dr($dr) prefix(z)/*
         */signs(‘negatif’ ‘negatif’ ‘positif’)  adoonly

    replace ‘p1’=0.0001 if ‘p1’<=0
```

```
 replace ‘p01’=0.0001 if ‘p01’<=0
 replace ‘p000’=0.0001 if ‘p000’<=0
 replace ‘p001’=0.0001 if ‘p001’<=0
}
quietly replace ‘lnf’ =  ln(‘p1’) if $ML_y1==1
quietly replace ‘lnf’ =  ln(‘p01’)  if $ML_y2==1
quietly replace ‘lnf’ =  ln(‘p000’)  if $ML_y3==0
quietly replace ‘lnf’ =  ln(‘p001’)  if $ML_y3==1

end
```

The maximization of the likelihood is computationally very demanding. In order to estimate the model, we randomly selected 75% of the cases and maximized the above program. Identification was insured by the fact that (i) values of the variables of the conciliation step are set at the date of the conciliation attempt, while they are set at the date of the first audition in front of the councilors for the abandon and the decision stages, and (ii) the abandon stage includes the average duration of cases terminated the previous year while the decision does not. The convergence of the program took three weeks. The estimation was performed using *propNR*. The results of the estimation are displayed in table 5.19.

Table 5.19: Results of the triprobit estimation. (Z-statistics in parentheses.)

Step	Conciliation	Abandon	Decision2
propNR	.0349*	.3456***	-.0001
	(1.72)	(19.00)	(-0.00)
shareLeft	.2428	.3140***	.0162
	(1.46)	(2.68)	(0.11)
avDuraffLY	1.21e-05	-.00016***	
	(0.52)	(-8.31)	
unemployment	-.00188**	-.0081***	-.00256*
	(-2.07)	(-7.89)	(-1.91)
lngdppc	.5288***	-.4783***	-.4392***
	(6.95)	(-11.17)	(-7.82)
gender	.0701***	-.0364***	.0539***
	(19.88)	(-11.61)	(12.08)
def_lawyer	-.2737***	-.1226***	.0871***
	(-50.15)	(-24.15)	(12.02)
def_workerUnion	-.0765	-.1175	-.0561
	(-1.00)	(-1.55)	(-0.66)
def_employerUnion	.4008***	-.4122***	-.3000***
	(11.94)	(-10.06)	(-6.57)
plaint_lawyer	.0966***	-.1438***	.0106
	(15.79)	(-23.40)	(1.10)
plaint_union	.2637***	-.2167***	-.0731
	(33.71)	(-26.87)	(-5.77)
ρ_{cd}		-0.685***	
		(-39.14)	
ρ_{ca}		-0.284***	
		(-7.69)	
ρ_{ad}		0.16844***	
		(3.65)	
Observations		905,125	
Log-Likelihood		-1,123,310.9	

Conclusion

Les travaux présentés dans cette thèse ont eu pour objectif de présenter un aperçu complet des différentes étapes qui composent la vie d'une norme juridique. Le premier travail s'est intéressé à la genèse du cadre constitutionnel encadrant la création de normes. La seconde étude s'est focalisée sur le processus politique de production des normes juridiques. La troisième analyse a étudié le contrôle de conformité de la norme de droit avec les dispositions substantielles contenues dans la constitution. Le quatrième papier a traité de la structure du système juridique permettant la mise en œuvre de la norme. Enfin, le dernier travail a été dévoué à l'étude de la mise en œuvre de la norme de droit par le système prud'homal.

Les principales implications et limitations de chacune de ces approches ont été présentées dans les conclusions respectives. Plusieurs éléments méritent cependant d'être soulignés.

L'émergence des systèmes constitutionnels. Le premier article a mis en évidence plusieurs résultats sur la relation entre droits constitutionnels et dépenses publiques. Ce travail montre d'une part que les pays ayant les dépenses publiques les plus élevées sont ceux ayant inscrit le moins de droits dans leur constitution depuis 1960. Il conclut également que le nombre de droits constitutionnels n'a pas d'impact sur les dépenses gouvernementales.

Les conclusions de cet article questionnent la théorie constitutionnelle et soulèvent plusieurs questions eu égard à l'utilité des droits constitutionnels. L'absence de relation causale suggère en effet que les Etats ne réagissent pas à l'introduction de nouveaux droits. Quatre facteurs limitatifs de notre analyse sont évoqués dans l'article. Premièrement, il est possible que les droits constitutionnels servent de signaux, à savoir que des pays inscrivent dans leurs constitutions des promesses qu'ils savent qu'ils ne tiendront pas. Les motivations pour l'absence de mise en œuvre des normes constitutionnelles peuvent être intentionnelles, à l'instar des dictatures qui se couvrent d'un vernis démocratique, ou matérielles, à savoir par manque de moyens. Certains droits sociaux comme le droit au logement demandent d'importantes ressources qui sont en effet au delà des capacités financières des Etats. Deuxièmement, il est possible que les droits constitutionnels n'aient aucune valeur contraignante en droit interne, les rendant *de facto* purement déclaratifs. L'absence de mécanismes institutionnels permettant aux cours de forcer le gouvernement ou les tierces parties à respecter et/ou de mettre en place les droits constitutionnels peut en effet rendre inopérantes les provisions constitutionnelles. Troisièmement, il est possible que la mise en œuvre des provisions constitutionnelles soit effectuées par des parties privées, de telle sorte que les coûts de leur mise en œuvre ne soient pas supportés par l'Etat. Dans un tel cas, les droits constitutionnels induiraient effectivement un accroissement des dépenses publiques mais ce dernier ne serait pas capturé par nos variables dépendantes. Quatrièmement, notre étude ne prend pas en compte le niveau de fédéralisme des Etats : dans les Etats décentralisés, les coûts de mise en œuvre des garanties constitutionnelles peuvent être pris en charge par les collectivités locales. Ici aussi, l'utilisation des dépenses de l'Etat central comme mesure

des dépenses publiques serait inappropriée. D'autres explications demeurent possibles. Il n'est pas exclu que les Etats inscrivent dans leur constitution des droits qu'ils fournissent déjà. Cette ratification *ex-post* expliquerait ainsi l'absence de corrélation mise en évidence par l'article.

L'hypothèse la plus crédible demeure néanmoins celle de l'application limitée, voire la non-application, des normes constitutionnelles substantielles. En effet, la provision des biens constitutionnels par des tierces parties ou par des instances fédérées n'est pas systématique et devrait seulement conduire à estimer un effet moyen inférieur à l'impact réel, mais ne devrait pas conduire à un effet nul. De manière similaire, l'hétérogénéité des pouvoirs attribués aux Cours Suprêmes devrait seulement réduire l'estimation à un effet moyen mais positif. Eu égard aux discussions introductives sur l'importance du Contrat Social comme source de légitimité des Etats, ces résultats concluent sur une note pessimiste : de manière générale, les provisions constitutionnelles ne sont, au mieux, que partiellement mises en place, ou, au pire, de vaines promesses. Des résultats similaires, quoique plus optimistes, ont été mis en lumière dans la littérature analysant l'impact des traités internationaux sur les droits de l'homme. Ces derniers ont majoritairement conclu à une tautologie : seuls les pays démocratiques mettent en place les droits de l'homme. Dans le cas des constitutions, il est fort probable que seuls les pays désireux de respecter les droits de l'homme appliquent les droits contenus dans leur constitution.

La Production de Normes Juridiques. Le second travail de cette thèse s'est concentré sur la relation entre le choix de normes redistributives et le biais cognitif d'auto-complaisance. Le projet expérimental a cherché à induire un biais psychologique parmi les participants afin d'étudier son impact sur l'offre et la demande de redistribution. L'article conclut que le biais d'auto-complaisance, parce qu'il conduit les participants ayant relativement mieux réussi à surestimer l'impact de leur effort sur leur résultat, amène ces derniers à moins redistribuer en tant que tierce partie (offre) et à demander moins de redistribution pour eux-mêmes (demande).

Ces résultats posent de nombreuses questions quant à la littérature sur les inégalités, et davantage concernant les travaux en économie politique. Ils montrent que le processus de création de normes peut se détacher des préférences de la population à cause de biais psychologiques. L'article souligne en effet que les élus peuvent en effet être considérés comme ayant connu plusieurs succès (conséquence du processus électoral). Le biais d'auto-complaisance leur attribuerait ainsi une plus forte propension à percevoir la réalité comme le fruit d'un effort, ce qui limiterait l'offre de redistribution. Ces conclusions ne forment pas l'alpha et l'omega de la question de la redistribution, mais l'interrogent en analysant comment le processus de création de normes redistributives peut être affecté par l'identité et l'histoire personnelle des élus politiques.

Notre travail questionne également la demande politique, à savoir les électeurs : en sus des intérêts égoïstes, les personnes ayant des revenus élevés (resp. faibles) sont susceptibles de demander moins (resp. plus) de redistribution, car elles sont davantage susceptibles de

penser que leur fortune résulte de leurs efforts (resp. de leur manque de chance). Ce phénomène peut possiblement conduire à une polarisation de l'électorat, où les individus ayant une demande supérieure (resp. inférieure) à la redistribution en place demanderont davantage (resp. moins) de redistribution. Le principal problème que soulève cette polarisation est la grande difficulté, voire l'impossibilité, de la résoudre par des mécanismes institutionnels. S'il est en effet possible de mitiger, à défaut d'annuler, les intérêts égoïstes au travers de mécanismes institutionnels tels ceux proposés par Madison, Rawls ou Buchanan, les biais cognitifs restent attachés à l'identité des individus.

Nos résultats s'inscrivent également dans une lignée de travaux en économie expérimentale qui pointent les défaillances des marchés économique et politique quant au manque d'information. Dans son article fondateur, Hayek (1945) a souligné l'importance du problème de rétention d'information, et en a ainsi déduit une supériorité du modèle décentralisé de planification par rapport au système centralisé. Selon Hayek, l'information qu'a un individu sur les opportunités auxquelles il fait face est le problème principal auquel se confronte la science économique. Dans nos travaux, nous montrons en effet que l'asymétrie d'information subie par les agents économiques conduit à une polarisation du marché politique. Celle-ci peut conduire à un échec d'entente sur une solution de redistribution. La question que nos conclusions soulèvent est de savoir comment un transfert d'information (crédible) sur les situations individuelles peut être mise en place et dans quelle mesure il peut conduire à modérer les conséquences du biais d'auto-complaisance.

Le Contrôle de Conformité à la Constitution. Le troisième travail de cette thèse confirme et développe des éléments déjà mis en exergue dans la littérature. Les propos introductifs ont montré la nécessité d'avoir un large consensus autour de la Constitution, et nos travaux questionnent l'interprétation partisane de la Constitution quant à ses effets sur la légitimité du système politique. Nos travaux sont avant tout limités par les données disponibles. En effet, les juges du Conseil Constitutionnel n'expriment pas leur vote individuel lors de la décision finale, contrairement aux modèles anglo-saxons. Cette limitation n'est pas sans importance, dans la mesure où plusieurs travaux se sont intéressés aux questions de prise de décision des courts collégiales (e.g. Cameron & Kornhauser (2010)). Kornhauser & Sager (1993) livre une discussion très détaillée des décisions de courts collégiales et propose une analyse des phénomènes de paradoxes doctrinaux (*Doctrinal Paradox*) pour la Cour Suprême Américaine. Kornhauser et Sager montrent en effet que l'agrégation des votes individuels peut conduire à rejeter une décision finale qui aurait pourtant été approuvée à la majorité, si l'agrégation s'était faite sur le résultat du cas traité et non pas sur les questions légales contenues dans le cas.[34] Nos travaux ne permettent pas de capturer ces effets d'interactions, propres aux entreprises *collégiales* telles que définies par Kornhauser et Sager. Notre étude capte cependant un effet global au niveau de la cour.

La littérature dans laquelle s'inscrit ce travail constate empiriquement que les juges – ou les cours–, interprètent les textes fondateurs à la lumière de leurs propres préférences,

[34]Deux exemples de la Cour Suprême sont cités à titre d'exemple : *Union Gas* et *Fulminante*.

mais reste timide quant à ces conséquences sur le système politique. En effet, il est difficile de juger de l'efficacité globale d'un tel système : l'interprétation constitutionnelle doit également se faire à l'aune des préférences de la société afin d'assurer au système institutionnel un consentement populaire solide. Le débat autour de l'interprétation constitutionnelle se polarise ainsi entre les partisans d'une interprétation stricte, fidèle au sens originel du texte, et une interprétation actualisée, alignées avec les préférences de la population.

Aux Etats-Unis, cette ligne de fracture se retrouve dans l'opposition entre Justice Scalia et Justice Ginsburg, le premier étant partisan d'une interprétation historique et le second d'une interprétation actualisée. La situation constitutionnelle des Etats-Unis est caractéristique de cette division et est, par ce fait, victime de nombreux blocages. La longévité de la Constitution américaine montre que la survie du système repose sur l'actualisation de l'interprétation des normes constitutionnelles : les changements de positions de la Cour Suprême sur les discriminations envers les minorités ethniques montrent que le 14ème amendement a connu une interprétation variable au cours du temps. Néanmoins, les tragédies endémiques autour des armes à feu soulignent l'impossibilité d'un changement d'interprétation allant contre des normes constitutionnelles explicites, et ce, quand bien même les préférences de la population aient changé à cet égard.

L'adaptation de l'interprétation des normes constitutionnelles au cours du temps selon les préférences de la population semble ainsi essentielle pour assurer la stabilité du système mais contredit également le but originel de la constitution, à savoir de fixer des règles immuables qui encadrent le jeu politique. Dans quels cas peut-on considérer qu'une interprétation partisane de la constitution qui prend en compte les préférences de la population est légitime ? Une proportion plus élevée de juges de gauche siégeant au Conseil Constitutionnel est le résultat d'élections politiques ayant favorisé les partis de gauche, fruit de préférences populaires plus prononcées pour les thèses socialistes. Ainsi, n'est-il pas légitime de voir le Conseil Constitutionnel infléchir son interprétation en faveur de ces courants de pensée ? Mais, dans quelle mesure les minorités conservatrices peuvent-elles se prévaloir de leur droit à une Constitution exempte du jeu partisan qu'elles se sont elles-mêmes engagées à respecter en cas de victoire ?

L'exercice délicat de l'interprétation constitutionnelle consiste à satisfaire ces deux visions légitimes. Ainsi que le note Michel Troper, la légitimité du système tient de la conviction des juges d'appliquer une norme de manière non-partisane.[35] Le biais idéologique des juges, dont ils ne seraient pas conscients, permettraient ainsi d'adapter la norme aux préférences de la société, tandis que leur conviction d'impartialité assurerait le consentement des minorités politiques au système constitutionnel.

Définition et Tracé du Système Juridique. La quatrième étude a montré les conséquences d'une réforme visant à changer la carte judiciaire sur la demande de résolution

[35]Michel Troper note que les juges du Conseil Constitutionnel pensent que le texte constitutionnel contient une norme objective, qu'ils sont chargés de dégager et d'appliquer. Leur pouvoir d'interprétation est, à leurs yeux, très limité.

de litige et l'activité des tribunaux. Nos résultats ont montré une diminution significative du nombre de cas portés aux Conseils de Prud'hommes pour les zones connaissant les plus grandes difficultés d'accès à la justice. Ils ont aussi montré que les durées de procédure ont augmenté dans les cours ayant considérablement augmenté leur charge de travail suite à la réforme.

Notre analyse porte un regard mitigé sur la réforme de 2008. D'une part, les résultats montrent que la réforme a pu diminuer le bien-être d'une large franche de la population : des demandes légitimes n'ont pas pu être formulées, et les justiciables font potentiellement face à des durées de procédure plus élevées. D'autre part, l'absence de données sur les bénéfices de cette réforme encourage à la plus grande prudence avant de juger de l'efficacité de la réforme. En effet, l'article n'est pas capable d'apprécier l'efficacité de la réforme, étant donné les dimensions encore non-explorées, telle que la qualité des décisions rendues, ou encore le manque d'information quant aux bénéfices de la réforme.

Notre étude est la première à apporter des éléments empiriques sur de telles réformes, et montre toute l'importance de la définition et du tracé du système judiciaire dans la mise en œuvre des normes de droit. La diminution du nombre d'affaires portées pose d'autres questions aux responsables politiques. Cette diminution peut refléter la contraction du nombre de plaintes *frivoles*, ce qui serait bénéfique à la société, mais pourrait également résulter d'une diminution du nombre de plaintes *méritantes*, réduisant ainsi le bien-être social.[36] Dans ce dernier cas, le législateur ferait face à davantage de situations où des justiciables ne seraient pas en mesure de réclamer la mise en œuvre de la norme juridique telle que le conçoit le législateur. La non-mise en œuvre du droit serait alors un risque pour la légitimité du système politique : Quels seraient les intérêts des citoyens à soutenir un système institutionnel qui ne leur donne pas les moyens de faire valoir leurs droits ?

Mise en Œuvre de la Norme Juridique. Le dernier chapitre de notre étude s'est intéressé à la mise en œuvre des normes du droit du travail et à ses conséquences sur les stratégies des requérants. L'analyse a eu pour objectif d'étudier les variations dans la composition des cours en opposant les syndicats réformistes (CFDT, CFE-CGC, CFTC) aux syndicats non-réformistes (CGT, FO). Nos résultats ont mis en évidence des taux de conciliation, d'abandon et de départage plus élevés dans les cours non-réformistes. Notre étude a également montré que les cas conciliés sont davantage défavorables aux salariés, tandis que les cas 'abandonnés' sont plus souvent favorables aux employés. Ce dernier résultat suggère que les cas reportés comme abandonnés sont en réalité sûrement négociés en dehors des cours.[37]

L'article propose une interprétation des résultats fondée sur la rationalité des justiciables. Les cours non-réformistes font face à des taux de départage plus élevés. Anticipant

[36]Nous reprenons ici la distinction bien établie en économie du droit entre plaintes *frivoles* et plaintes *méritantes*. Les premières font références aux cas dont l'instruction par un tribunal induit un coût social supérieur au bénéfice social. Au contraire, les plaintes méritantes sont celles où le bénéfice social compense les coûts pour la société.

[37]Les salariés n'auraient pas intérêt à abandonner les cas qu'ils sont certains de gagner.

des délais de procédure plus longs dans les cours dominées par les syndicats non-réformistes, les justiciables négocient davantage en amont, à la fois lors des épisodes de tentative de conciliation obligatoires, et en dehors des cours. Les cas ainsi négociés correspondent aux cas les plus tranchés, à savoir ceux clairement favorables et ceux clairement défavorables à l'employé. Il en résulte que seuls les cas les plus litigieux remontent au bureau de jugement, augmentant ainsi le taux de départage.

Ce travail est la première étude à s'intéresser à la relation entre la composition syndicale des Conseils de Prud'hommes et l'activité de ces cours. La mise en évidence de l'accroissement des taux de départage sous les cours non-réformistes pose de nombreuses questions sur l'organisation du pouvoir judiciaire. L'association des organisations patronales et syndicales à la mise en œuvre des règles de droit avait originellement pour objectif d'augmenter la légitimité des décisions des cours. Il est possible cependant que leur implication ait transposé les tensions présentes sur le marché du travail au système judiciaire : les conseillers prud'homaux employeurs et employés auraient davantage de mal à s'entendre lorsque les représentants employés appartiennent à des organisations non-réformistes. Si un tel scenario s'avérait exact, il signifierait l'échec de la justice prud'homale dans son objectif de pacification des relations du travail.

Les résultats de notre étude doivent néanmoins s'interpréter avec la précaution nécessaire à ce type de travaux. Les données fournies par le ministère de la Justice contiennent certes beaucoup d'observations mais sont néanmoins limitées en information : de nombreuses variables sont mal renseignées et de nombreuses dimensions essentielles à la compréhension des décisions prud'homales, tels que les montants des indemnités versées, ne sont pas reportées. Notre analyse se limite en effet à une étude *extensive* des décisions (acceptation ou rejet des requêtes du salarié) alors qu'il est possible que les arbitrages s'effectuent sur une dimension *intensive* (montant des indemnités effectivement accordées). De plus, bien que notre étude ait cherché à corriger pour les potentielles sources d'endogénéité, il est possible que nous n'ayons pas réussi à circonscrire entièrement le problème, rendant ainsi nos estimations biaisées. Enfin, ainsi que l'a discuté notre article, nos données se limitent aux cas effectivement ouverts : il est possible que les types de cas atteignant la cour soient différents entre les cours réformistes et non-réformistes.

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Résumé. Analyse Économique de la Norme Juridique : des Origines Constitutionnelles à la Mise en Œuvre par le Juge

Les questions de légitimité et de stabilité des systèmes politiques ont longtemps été étudiées séparément des problèmes de mise en œuvre du droit en sciences économiques. L'objectif de cette thèse est concilier ces différentes approches afin de replacer la mise en œuvre de la norme juridique au centre du débat institutionnel. Ce travail se décompose en cinq investigations empiriques ou expérimentales portant chacune sur une des étapes du processus politique et judiciaire. Le premier article s'intéresse à l'impact des droits constitutionnels sur les dépenses publiques. La seconde étude explore l'influence des biais d'auto-complaisance sur la demande et l'offre de redistribution. Le troisième travail analyse les décisions rendues par le Conseil Constitutionnel. La quatrième partie examine la réforme de la carte judiciaire des Conseils de Prud'hommes de 2008. Le dernier chapitre étudie la relation entre la composition syndicale des Conseils de Prud'hommes et les issues des litiges qui y sont portés. Nos analyses reposent sur les outils économétriques et expérimentaux. Elles font usage de méthodes d'estimations classiques (*OLS, GLS, Probit, Logit, Within OLS*), de modèles à sélection (*Heckman, Triprobit*), des outils destinés aux problèmes d'endogénéité (*2SLS*) et des techniques d'estimation de systèmes d'équation (*3SLS*). L'approche expérimentale contient également des tests statistiques communément appliqués (*tests de permutation*, *tests de comparaison de moyenne*, *tests de proportion*) ainsi que de récentes méthodes pour traiter l'hétérogénéité (*wild clustering*).

Descripteurs : Économie du droit, Économie politique, Économie constitutionnelle, Économétrie appliquée, Économie expérimentale, Norme juridique, Constitution, Conseil Constitutionnel, Biais d'auto-complaisance, Conseils de Prud'hommes, Justice, Délais, Résolution de litige.

Abstract. Economic Analysis of Legal Norms: from the Constitutional Origins to the Enforcement by the Judge

The legitimacy and the stability of political systems have very often been studied in economics separate from considerations about legal norms' enforcement. My objective is to combine these different approaches, and to place the question of the legal enforcement at the heart of the debate about institutions. This work is made of cinq empirical and experimental investigations that deal with each of the stages of the political and legal process. This first paper analyzes the impact of constitutional rights on public expenditures. The second article explores the influence of self-serving biases on the demand and the supply of redistribution. The third analysis focuses on the decisions of the French Constitutional Council. The fourth work deals with the recent reform of the judiciary map of French labor courts. The last study investigates the relationship between the composition of the elected jurors in French labor courts and the way cases are terminated. Our investigations rely on econometric and experimental techniques. They use standard estimation methods (*OLS, GLS, Probit, Logit, Within OLS*), selection models (*Heckman, Triprotibt*), techniques for endogeneity correction (*2SLS*), and methods to estimate systems of equations (*3SLS*). The experimental analysis makes use of standard statistical tests (*permutation tests, proportion tests, two-group mean-comparison tests*), and more recent methods to solve heterogeneity (*wild clustering*).

Keywords: Law and Economics, Political Economy, Constitutional Economy, Applied econometrics, Legal norm, Constitution, Constitutional Courts, Self-serving bias, Labor Courts, Justice, Delays, Litigation.