Judge Bias in Labor Courts and Firm Performance

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Abstract

Does judge subjectivity in labor courts influence firm performance? We study the economic consequences of judge decisions by collecting information on Appeal court rulings, combined with administrative firm-level records covering the whole universe of French firms. The quasi-random assignment of judges to cases reveals that judge bias, defined as judge-specific differences on granting compensation for wrongful dismissal, has statistically significant effects on the survival and employment of small firms, especially among very small and low-performing ones. When compensation for wrongful dismissal is instrumented by judge bias, an increase in compensation of 1 percent of the payroll reduces employment growth by 5 percentage points after 3 years for those firms.

Key words: Dismissal compensation, judge bias, firm survival, employment.
JEL Codes: J33, J63, J65.

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1 Introduction

The fear of a differentiated treatment and judges' alleged pro-worker biases are frequent worries of businesses heading to labor court. Recently, many advanced economies have therefore enacted reforms that restrict judge latitude in awarding compensations, with the objective of guarding businesses against dramatic outcomes. Italian Prime Minister Matteo Renzi's flagship labor reform, the 2014-15 Jobs Act, aimed at reducing uncertainty due to excessive litigation and the unpredictability of judges' decisions (Boeri and Garibaldi (2018)). Similarly, in France, the 2017 Ordonnances reforming the labor code introduced a ceiling to the level of compensation granted by judges, based on firm size and worker seniority. In a majority of European countries, judges' discretion in compensating the individual damages following wrongful dismissals is actually capped (see Annex A).[1] However, none of these regulations has been grounded on rigorous quantitative analysis, partly for lack of appropriate data. Therefore there is no reliable empirical evidence that the subjectivity of judges in labor courts has an impact on the performance of firms.

This paper presents the first systematic evidence of the impact of labor court judge bias on firms' economic performance.[2] We use text analysis to extract rich information from the decisions made by French Appeals court over the period 2006-2016.[3] This allows us to identify judge bias – defined as the effects of judge-specific differences on compensations for wrongful dismissals – from the quasi-random allocation of cases to judges.

We find evidence that the subjective opinion of judges influences the amount of dismissal compensation. The difference between the compensation set by the most pro-worker and the most pro-employer judges is significant: moving from the bottom decile to the top decile of judge bias increases expected compensation payments by about two months of salary, or 20 percent of the average compensation.

We then explore the impact of judge bias on firm performance, drawing on administrative firm-level records covering the whole universe of French firms. We focus on individual dismissals, that is, dismissals of a single employee at a time, as collective layoffs are a relatively rare event among the universe of layoffs handled by French labor courts, and even more rarely induce decisions from Appeal courts. From reduced-form regressions, we

[1] In the U.S., employment protection is overseen by the National Labor Relation Board (NLRB) whose judges have been denounced by some critiques as being influenced by partisan ideology (Turner (2006), Semet (2016)).

[2] We use the expression "judge bias" rather than "judge leniency", often used in the literature about criminal justice, to avoid ambiguity, as a judge with a pro-worker bias is lenient with workers whereas a judge with a pro-employer bias is lenient with employers.

[3] We use all the judgments on dismissals of the *Dalloz* database which collects all the Appeal court rulings.

find that the judge bias has in fact a significant impact on firm survival and employment growth. The effect is mostly driven by firms that are very small – less than 15 employees, which corresponds the median size in the sample – and low-performing – with returns on assets below the median. For all firms, an increase by one standard deviation in the pro-worker bias[4] reduces employment growth by 3 percentage points and the survival rate by 1 percentage point three years after the judgment, while these figures amount to 6 and 2 percentage points respectively for very small low-performing firms. There are no significant effects for the other firms, whether with 15 employees or more, or with less than 15 employees and return on assets above the median. From instrumental variable regressions, in which the compensation for wrongful dismissal is instrumented by the pro-worker judge bias, we find that an increase in the amount of compensation by 1 percent of the payroll reduces the employment growth rate of all firms and firms with less than 15 employees at a 3-year horizon by respectively 4 and 5 percentage points if their return on assets is below the median, but has no employment effects for other firms.

We find that the overall employment impact of judge bias stems from permanent jobs, with no significant impact of the judge bias on temporary employment. This is consistent both with a cash flow shocks channel, as hiring employees on permanent contracts means making longer-term commitments, but also with an expectation channel, as employers might revise their expectations about dismissal costs according to the decisions of judges, which are all about permanent contract terminations.

We pay special attention to establishing the credibility of our identification strategy, which is supported by several key institutional features. Appeals cases for wrongful dismissal are decided by three-judge panels composed of a president and their two assessors in a section of the court called "social chamber". We focus on the presidents, who oversee all the rulings and accordingly play a key role in deciding the case, and leverage their rotations across courts. To identify the effects of judge-specific differences on compensations for wrongful dismissals, we compare the compensations decided by subsequent presidents of social chambers within the same social chamber of the same Appeal court within the same year. More precisely, we estimate the president bias for each judgment using a leave-one-out difference between the average compensations for all other cases that a president has handled and the average compensations handled by all presidents in the same social chamber within the same year.

In order to document the random allocation of cases to judges, we perform an event study to verify whether judges of different types judge firms with similar performance before

[4] By pro-worker bias we mean that the judges are ranked in ascending order starting from the bias most favorable to firms and going towards the one which is most favorable to the worker.

the judgment. In particular, we compare total, permanent and temporary[5] employment growth relative to the year preceding the judgment depending on the pro-worker bias of the judge faced by the firms. We find that, before the judgement, employment growth is not statistically different depending on the judge bias. However, total and permanent employment start diverging after the judgement, especially among small and low performing firms. We also verify that the allocation of judges is unrelated to the observable worker and firm characteristics of the cases they judge. We therefore interpret the differences between leave-one-out mean compensations set by subsequent judges in the same social chamber of the same Appeal court in a given year as reflecting the influence of judges' subjectivity.

Our paper makes two important contributions to the literature. First, we provide the first direct estimate of labor court judge bias on dismissal compensation, thanks to a novel dataset with detailed, case-by-case information about compensation for wrongful dismissal. Differentiated treatment by judges has been investigated in a rapidly growing and influential empirical literature, in particular regarding criminal sentencing (Scott, 2010; Dobbie et al., 2018; Yang, 2015; Bhuller et al., 2020), bankruptcies (Bernstein et al., 2018a,b), or decisions related to disability benefits (Autor et al., 2015; Dahl et al., 2014; French and Song, 2014; Kostol et al., 2017; Maestas et al., 2013; Autor et al., 2019). Relying on the quasi-random or random allocation of judges to cases, these contributions generally find that differentiated treatment by judges is significant, but, importantly, that it can be mitigated by sentencing guidelines (Scott, 2010; Yang, 2015; Cohen and Yang, 2019). Bamieh (2016) uses this approach to infer firing cost variations from the dispersion in trial duration in Italian labor courts driven by quasi-random judge appointments. Semet (2016) finds that the propensity to reach a decision favoring labor increases with each additional Democrat judge added to a panel of the US *National Labor Relation Board*. Our main addition to this literature is to establish the differentiated treatment by judges on the *amounts* of compensation themselves. Our measure of judge bias is in line with previous research studying the impact of extraneous factors on the qualification of dismissals as unfair by judges. Ichino et al. (2003) and Jimeno et al. (2020) show that the local unemployment and bankruptcy rates influence the probability that judges deem dismissals unfair. Consistent with these contributions, our findings show that judges retain some degree of autonomy in their interpretation of labor laws.[6]

[5] We count as permanent employment the employees hired on open-ended contracts or CDI (*Contrat a durée intederminée*), as opposed to CDD *(Contrat a durée determinée).*

[6] This is also consistent with Jimeno et al. (2020)'s study of Spanish labor reforms of 2010 and 2012. Despite a broadening of the definition of fair economic dismissals, the proportion of economic redundancies being ruled as fair by labor courts has not substantially increased. This discrepancy between the evolution of the legal rules and the "effective" rules is interpreted as arising from the opposition of judges to

Second, our paper shows that the subjectivity of judges in terms of compensation for wrongful dismissal has a significant impact on the performance of low-performing small firms. A vast empirical literature analyzes the labor market impact of dismissal costs (see Cahuc et al. (2014) for a survey) but causal evidence has mostly hinged on aggregate exogenous variations. In particular, studies of the effects of court decisions regarding unfair dismissals on firms' outcomes (Autor, 2003; Autor et al., 2006, 2007; Bamieh, 2016; Boeri and Garibaldi, 2018; Fraisse et al., 2015; Gianfreda and Vallanti, 2017; Martins, 2009) typically use the implementation of reforms of Employment Protection Legislation (EPL) to assess the effects of dismissal costs on employment or productivity. Our paper differs from previous studies in several crucial aspects. In the first place, our finding of a significant impact on low-performing small firms of the subjectivity of judges is a key point insofar as the uncertainty associated with dismissal compensations arising from the subjectivity of judges is considered to be a major weakness of employment protection legislation in European countries (Ichino et al., 2003; Berger and Neugart, 2011; Martín-Roman et al., 2015; Jimeno et al., 2020) and in the US (Posner, 2008). Hence, our paper contributes to the policy debates about regulations that curb judge discretion in matters of compensation for dismissals. In addition, our contribution finds a differentiated impact of EPL depending on firm size, an issue which has been overlooked by the literature so far although most countries have exemptions for small firms. We find statistically significant effects of shocks on compensation for wrongful dismissal on employment for small firms whose returns on assets is below the median, but not for other firms. From this perspective, our findings that cash-flow shocks have a significant impact on small and low-performing firms but not on other firms adds to the results of the corporate finance literature that assesses the effects of cash flow and credit shocks on firms (Blanchard et al., 1994; Chodorow-Reich, 2013; Giroud and Mueller, 2017; Rauh, 2006; Simintzi et al., 2014; Favilukis et al., 2020).

The paper is organized as follows. Section 2 describes the French institutional setting and Section 3 the data. Section 4 presents evidence about judge bias. Section 5 documents the impact of judge bias on firm survival and employment. In Section 6 we conduct robustness and heterogeneity tests. Section 7 discusses the scope and limitation of our results.

the change in the legal definition of fair dismissals, suggesting that judges have significant margin for interpreting legal rules.

2 Institutional background

This section starts by presenting the regulation of termination of open-ended employment contracts, which represent about 85 percent of ongoing contracts in France, before providing an overview of the organization of courts and describing the assignment of judges to cases.

2.1 Legal framework

Following the termination of an open-ended contract, employees with a tenure longer than one year and who did not commit any serious or gross misconduct (*faute grave* or *faute lourde*) are granted a minimum legal severance payment calculated as one fifth of monthly salary per year of tenure, plus an additional two fifteenths after a ten-year tenure. These amounts can be topped up if the professional branch to which the firm belongs has signed a collective agreement ensuring higher payouts.

Under French law, terminations of open-ended employment contracts are lawful if they are justified by a "real and serious cause", either economic or personal. Dismissals for economic reasons are lawful only to "safeguard" firms, but not to improve their profitability. Dismissals for personal reasons are lawful only in case of misconduct or lack of adaptation to the job. For both types of dismissal, the burden of the proof is on the side of employers. Furthermore, employers have to prove that there is no other position available in the firm (worldwide in the period we are studying) for dismissed employees when the dismissal is motivated by economic reasons or by lack of adaptation to the job.

When the employee deems her dismissal wrongful, she can file a complaint before the *Prud'hommes* councils, which are courts of first instance. While most European countries have specialized labor tribunals to deal with dismissal cases (OECD, 2013), in France judges in *Prud'hommes* councils are employee and employer representatives, with an exact equality between the numbers of councilors representing employers and those representing employees.

Serverin and Valentin (2009) calculate that for economic dismissals in 2006, the rate of employee recourse to *Prud'hommes* in case of dismissal is between 1% and 2% while for disciplinary dismissals it is between 17% and 25%.[7] According to Desrieux and Espinosa (2019), among claims that reached the judicial stage at *Prud'hommes* council from 1998 to 2012, 62% resulted in the acceptance of the employee's claims. Similarly Fraisse et al.

[7]Economic dismissals are therefore very rarely challenged, one reason being that their conditions are usually negotiated between social partners at the firm level. Another reason is that these layoffs only account for 2% of all exiters, since employers prefer to have recourse to personal motives given the complexity of their procedure (when more than one person is laid off) and the absence of a legal or conventional definition of a lawful separation for economic reason (at least until a 2016 law which clarified this notion).

(2015) estimate that in the 1996-2003 period, "60% of cases end up with a trial, among which 75% lead to a worker's victory".

The decisions of the *Prud'hommes* council are appealed in most of the cases: the appeal rates are, according to Guillonneau and Serverin (2015), between 60% and 67% in the 2004-2013 period. From 2006 to 2016, we find that only 44% of *Prud'hommes* councils decisions about compensations for dismissal were confirmed by Appeal courts.[8] Insofar as appeal rates are very high and the appeal suspends the application of the decisions of *Prud'hommes* councils which are frequently not fully confirmed, the compensation for wrongful dismissals decided at the Appeal court level is an important measure of the compensation to be paid by the firm.[9] Therefore, in what follows, we use the compensation for wrongful dismissals decided by Appeal courts.

2.2 Overview of Appeal court's organization

There are 36 Appeal courts and 210 *Prud'hommes* councils. Each French Appeal court has different chambers, among which at least one social chamber treats cases coming from the *Prud'hommes* council. Some Appeal courts have several social chambers, such as the Paris court which has fourteen of them. There is one president for each social chamber. This chamber president has administrative responsibilities within the court, and is in charge of presiding over all the chamber's trails. She can nevertheless be replaced whenever needed, for instance during holidays. For each judgment, the chamber president is assisted by two councillor-judges.

The status of judges and their mobility is determined by the *Ordonnance Organique* of 22 December 1958. This regulation states that judges in Appeal courts are "placed judges", *i.e.* assigned to a given Court or a given Chamber in a specific position according to decisions made every year by the First President of the Court of Cassation (the highest civil jurisdiction) and the First President of the Appeal court. Promotions are based on merit and decided every year by a National Commission of Advancement. The First President of the Appeal court herself is placed by a decree signed by the President of the Republic following the recommendation of the independent National Council of the Judiciary. Besides, mobility requirements are enforced through several regulations, such as promotions awarded only to judges in a given position for less than 5 years in a same jurisdiction (7 years from 2017), the prohibition to stay in the same specialized function in the same jurisdiction more that ten years altogether, or geographical mobility requirements to achieve the first grade of the remuneration schedule (organic law 2001-539 of June

[8] All first and second instance judgments include a hearing of the parties and a written decision.

[9] In any case, data about *Prud'hommes* councils decisions are not available.

25th, 2001). The turnover that follows is substantial: every year 20% of positions are re-assigned among judges (Conseil de la Magistrature, rapport d'activité 2016).

Importantly, the First President of the Appeal court sets objective criteria driving the distribution of the cases between the various chambers of the Appeal court, independently of the judges' identity, under the control of the assembly of judges (articles R312-42 and R312-42-1 of the Judiciary Organisation Code).

2.3 Assignment of judges to cases

To identify judge bias, the allocation of cases to judges must be independent of judges observable and non-observable characteristics. Therefore, our identification strategy relies on the quasi-randomness of the allocation of cases to judges. Two aspects of the organization of the judicial system imply that the allocation of judges to cases has important random components, *i.e.* does not depend on the identity of judges.

First, it takes a judge on average two years from the time of her appointment to rule on all the cases assigned to the social chamber *prior* to her arrival. The composition of the court cannot be changed by plaintiffs and judges cannot select their cases, except for conflict of interest. The presence of this backlog and the fact that cases cannot be re-allocated imply that it is almost impossible to assign a case to a specific judge, because the average spell of a judge in a social chamber is equal to about 2.5 years, meaning that the identity of the president that will judge a case assigned to a social chamber is generally unknown when the case is allocated to the social chamber. Moreover, when a president is absent, for vacation, sickness, vocational training or any other reason, she is replaced by the president of another chamber who judges the cases which are scheduled.

Second, the selection of cases settled before going to court can be influenced by the judge in charge of the case. However, employers, workers and lawyers do not know with certainty the identity of the president until the day of the judgment for several reasons: a new judge may be appointed, the judge may be absent and replaced by another one. In addition, in the case of larger Appeal courts, the existence of several social chambers in the same court implies that the social chamber that will judge the case is not known before the judgment.[10]

These institutional features imply that the assignment of judges to cases has important random components that we will leverage to identify the judge bias as explained in Section 4.2.

[10] Our main analysis relies on all Appeal courts, but we show that our results hold when the sample of cases is limited to large Appeal courts with several social chambers (see Section 6).

3 Data

3.1 Compensation data

The empirical analysis draws on a newly created dataset of French Appeal court rulings from 2006 to 2016 bringing together, for the first time, detailed information on compensation amounts decided in court along with a rich set of firm characteristics. From the court rulings, we extract a wide array of variables related to each case, as well as the firm's name and address. Then, using the firm's name and address, we are able to retrieve the firm's unique administrative identifier (*SIREN*), which allows us to link our compensation dataset to comprehensive, matched employer-employee data as well as to financial variables. This section highlights the key steps in the construction of this dataset and the main features of the data. Appendix E provides additional and technical details.

First, we gather 149, 542 Appeal court rulings published by the Ministry of Justice Each of these text documents contains a lot of information in a semi-structured format. Court rulings usually provide a description of the history of the contractual relationship between the employee and the employer. This presentation of facts also includes the claims of the parties and the decision of the *Prud'hommes* council. Court rulings then describe the reasons for the Appeal court decision and end with the compensation for dismissal if the dismissal is deemed wrongful. Figure 2 shows an extract of a typical ruling.

When her dismissal is ruled wrongful, an employee may receive additional compensations on top of the compensation for wrongful dismissal. Tracking and accounting for these different forms of compensation is important because even though the legal bases for granting them are distinct in principle, judges' full understanding of the case at hand might in practice create correlation patterns between these amounts. In other words, it is possible that a judge's appreciation of the case might color not only the amount granted for unfair dismissal but also the other forms of compensation. Possible additional compensations include: moral and financial damages, compensation for unpaid wages, etc.[11]

We extract all these variables automatically from the Appeal court rulings using a Python program based on keywords extraction and natural language processing techniques. In order to control the quality of the process, we assessed the accuracy of the results on a manually-filled dataset forming a subsample of about 2,500 observations, selected at random. We find that the correlation between the compensation amount of the manually-filled and the automatically-filled datasets is equal to 94%, which is in the upper range of

[11]See Appendix E for a more complete list of the dozens of possible additional compensations.

seminal papers using this type of approach (Baker et al. (2016)).

Finally, we also retrieve the unique administrative firm identifier known as *SIREN*, either directly from the text when it is displayed, or, using the firm's name and address, after an automatic search on online companies registries such as *societe.com* and *bodacc.fr*. The *SIREN* identifier, assigned by France's statistical agency to each company, then allows us to merge our rulings compensation dataset with French administrative social security and tax data. In some cases where the company is very small or when the cases were launched a long time ago, we were not able to retrieve the *SIREN*.

3.2 Social security and tax data

In order to analyze the impact of judge decisions on firm performance, we combine our novel rulings data with two comprehensive administrative datasets. Because both have been used in the literature we only briefly highlight their main characteristics

Matched employer-employee data. We merge the compensation data with social security data thanks to the firm identifier. We use the comprehensive matched employer-employee dataset called DADS Postes *Déclarations Administratives de Données Sociales* from 2002 to 2015, which reports detailed payroll information about each employee working for a French private firm. This dataset allows us to track the evolution over time of the wage bill and of the number of employees of the firms in our rulings dataset.

Tax data. We rely on tax data, FICUS-FARE, that contain the full company accounts, including for instance sales, net income, EBITDA. From these files we are able to construct a wide array of indicators for the firm's financial health such as the firm's leverage ratio, the return on assets, etc. These data are available from 2002 to 2016.

3.3 Sample restriction

From our initial sample of 149,542 rulings, we select those for which it is indicated that the firm was not in liquidation at the judgment date, because dismissal compensations of liquidated firms are paid by a public insurance agency (*Agence de Garantie des Salaires*). Since the parties involved in these cases are no longer the employer and the employee, but the employee and the public agency, these cases are not suitable to identify judge bias in situations where employers are directly involved. Then, we eliminate cases for which the relevant information about the presiding judge's name and surname, the total amount of compensation, and the monthly wage was either not retrieved or is not available. While the most important information is often retrievable – the identity of the Appeal court, compensation amounts for wrongful dismissal, worker's wage and seniority, location of the *Prud'hommes* council, whether the worker or the firm was the appellant, etc. – there are sizeable variations in the amount of available information from one ruling to the next. This heterogeneity reduces the size of the useable sample by about a half. Finally, we eliminate cases in which the employer belongs to the public sector and those judged by judges who have judged less than 50 cases. We end up eventually with 30,717 cases and 159 presidents[12] (See Table 1). The 159 presidents who judged more than 50 cases cover 93.3% of cases among the universe of cases that we analyze. Each of these presidents judged 193 cases on average in our sample.

4 Judge biases

This section is devoted to the analysis of judge bias. We start by reporting descriptive statistics about judgments before presenting the empirical strategy used to identify judge bias and showing the results.

4.1 Descriptive statistics

Table 2 presents descriptive statistics of judgments at the case level. Our sample comprises only cases that are judged in Appeal courts. The average amount of compensation for wrongful dismissal granted by Appeal courts is equivalent of 4.4 months of salary, while the total amount, including other possible indemnities for unpaid leave, unpaid (overtime) hours worked, unpaid notice, or (more rarely) compensation for damages in case of harassment or discrimination, represents 10.6 months of salary. The worker appeals in 60% of cases.

Figure 3 displays the histogram of the compensation for wrongful dismissal in monthly wages, conditional on being positive. There is a mass around six months of salary: this stems from French legislation that institutes a minimal threshold of six months of salary for workers with more than 24 months of seniority employed in firms with at least 11 workers, when the dismissal is deemed wrongful.

Table 2 also provides information about differences between decisions of Appeal courts and *Prud'hommes*. The amount given at Appeal court is the same as the amount decided at Prud'hommes in 44% of cases, while it is higher in 39% of cases and lower in 17% of cases. The average compensation for unfair dismissal set by Appeal courts is much higher (12,086€) than that of *Prud'hommes* (7,327 €).[13] All in all, Appeal courts are more

[12]Let us remind readers that the court is composed of a president and two councillor-judges. The president, who is in charge of supervising the writing of the judgments, plays the key role in the judgment.

[13]Note that we consider here only *Prud'hommes* judgments which are appealed and reach the Appeal court, as the information about other *Prud'hommes* judgments is not available

favorable to workers than *Prud'hommes*. Figure 4 shows the scatter plot of the amount of compensation in monthly wages depending on seniority set by Appeal courts (right panel) and by *Prud'hommes* (left panel). It is apparent that there is an important dispersion of the amount of compensation conditional on seniority in both tribunals. Table 2 shows that the variance of the compensations of Appeal courts is larger than that of *Prud'hommes*.

Obviously, the variance of compensations conditional on seniority originates from the diversity of situations specific to each case. Nevertheless, the subjective interpretation of judges might exert an important influence, as suggested by the difference between the judgments of *Prud'hommes* and Appeal courts, which is significant at all amounts of compensation (Figure 5). Only a small share of the variance of compensations is explained by observable case characteristics: for instance, only 13.6% of the variance is explained by salary and seniority. Adding many other covariates[14] makes this share jump to 32.9%. In other words, 67% of the variance of dismissal compensation is still left unexplained when controlling for a wide range of covariates.

Figure 6 shows that amounts granted for unfair dismissal are positively correlated with the amounts granted under other motives. On average one month of salary granted for unfair dismissal is associated with one fourth of additional monthly wage granted for other motives. In other words, judges' decisions not only bear on amounts granted for unfair dismissal, but also on other compensations related to contract breach, like unpaid hours of work, compensation for non-respect of the dismissal procedure and other reasons enumerated in Section 3.1. Therefore, the main variable of interest we use throughout our analysis is the total compensation for contract breach (for unfair or any other motive), the histogram of which is exhibited in Figure 7.[15]

In order to identify the judge bias, the allocation of judges to cases must be random. We devise in the following section our strategy to consistently identify judges biases.

4.2 Measuring bias

Our empirical strategy to measure judge bias rests on the assumption that the allocation of judges to cases is random. As argued in Section 2.3 this is supported by three key

[14] *i.e.* controlling for the amount granted at Prud'hommes, the amount claimed by the worker, the firm's number of workers, whether it was the worker who appealed, whether it is an economic dismissal and the time elapsed between the dismissal and the appeal judgment

[15] Our measure of Appeal courts judges bias does not rely on the difference between the outcome of the Appeal court and the outcome of *Prud'hommes* insofar as *Prud'hommes*' decisions are influenced by the potential bias of *Prud'hommes* counselors. An alternative variable one can use to evaluate a judge's bias is the frequency at which the judge grants a positive compensation to the worker (for unfair dismissal or any other motive). We describe in Appendix C the use of this variable, and display in Figure C.1 the histogram of the frequency at which the judge grants a positive compensation.

institutional features: i) judges inherit a large backlog, ii) judges are mobile and iii) defendants and plaintiffs have limited information about the identity of the judge which ensures that the personality of judges does not unduly generate case selection through pre-trial settlement. In this context, the random component of the allocation we use is the allocation of cases across different judges within court, social chamber and year. Hence, we rely on differences between decisions of presidents belonging to the *same* social chamber within the *same* year.

In a given year, the president of a social chamber may move to another job, either to another Appeal court or to another position within the same court, and is then replaced by a new president. The initial judge and the new judge may have different interpretations of labor laws influencing the amount of compensation in case of dismissal. For instance, in year 2014 and social chamber 1 of the Paris Appeal court, a case may be either allocated to president A in the first part of the year, or to president B in the second part of the year, as shown by Figure 8. Although unlikely, a non-random assignment of cases to judges is still possible. For instance, it is possible that judge A is specialized in sexual harassment cases and that all those cases allocated this year are systematically assigned to this judge. However, what makes such an allocation of cases highly implausible is the large backlog in each social chamber – the average waiting time before judgments is about two years (667 days), and only 10% of cases are judged in less than 300 days. In this context, insofar as the cases are allocated to the social chambers at the start of the appeal procedure, it is very unlikely that cases can be specifically allocated to presidents whose seniority in the chamber is less than one year. Thus, since we rely on differences between decisions of presidents belonging to the same social chamber within the same year to identify judge-specific differences, it is unlikely that this identification strategy is burdened by non-random allocation of cases to judges.

Moreover, if the judge is absent the day of the judgment, he can be replaced by another judge without notice to the plaintiff and the defendant. Regardless, the presence of several social chambers implies that the plaintiff and the defendant do not know which social chamber will judge their case before the judgment. This implies that it is very unlikely that the identity of the judge in charge of the case influences the settlements before the judgment.

We implement this strategy through using a residualized, leave-one-out judge bias measure that accounts for case selection following Dobbie et al. (2018). We first residualize, for each case i judged by judge j in social chamber $\times$ year pair (k_i, t_i), the compensation

y_{ij} by chamber × year fixed effects to obtain the residualized component $\hat{\varepsilon}_{ij}$:[16]

$$\hat{\varepsilon}_{ij} = y_{ij} - \left(\frac{1}{n_i} \sum_{i' \in (k_i, t_i)} y_{i'} \right) \tag{1}$$

where $i' \in (k_i, t_i)$ means that case i' is judged, as case i, in chamber k_i and year t_i; n_i is the number of cases judged in chamber k_i in year t_i. Taking out chamber × year fixed effects enables us to compare the compensations granted by a given judge to the average compensation in the same court and year, which is necessary insofar as cases are quasi-randomly assigned to judges conditionally on a given chamber and year (k, t).

The bias of judge j for case i is then measured by a leave-one-out average, meaning that it is judge-specific and case specific. Namely, the leave-one-out judge bias $\bar{\varepsilon}_{ij}$ for case i and judge j is computed as an average over all cases i' other than i judged by judge j of the residualized compensation $\hat{\varepsilon}_{i'j}$:

$$\bar{\varepsilon}_{ij} = \frac{1}{n_j - 1} \sum_{i' \in j, i' \neq i} \hat{\varepsilon}_{i'j} \tag{2}$$

where $i' \in j$ means that case i' is judged by judge j, n_j is the number of cases judged by judge j in all social chambers over the period of observation.[17]

It is clear that our measure of judge bias relies on judges' mobility across social chambers which is crucial for comparing all judges. This measure allows us to rank judges according to their bias. The higher the degree of judge mobility, the higher the probability to achieve a perfect ranking (see Appendix B). We document the extent of judge mobility in Figure 9, where each dot represents a judge, and where a line connects two dots if the two judges shared the same social chamber at least once. As is apparent, the network of judges is dense, thus indicating a high mobility of judges across social chambers.[18]

4.3 Results

In this section, we begin by documenting the relationship between judge bias and the full compensation package granted by the judge. In a second step, we proceed to the analysis

[16]Note that we account for all cases, including those with zero compensation.

[17]The bias for case i judged by judge j can also be re-written as:

$$\bar{\varepsilon}_{ij} = \left(\frac{1}{n_j - 1} \sum_{i' \in j, i' \neq i} y_{i'j} \right) - \left(\frac{1}{n_j - 1} \sum_{i' \in j, i' \neq i} \overline{y}_{k_{i'} t_{i'}} \right) \tag{3}$$

where $\overline{y}_{k_i, t_i}$ is the average compensation granted in chamber k_i and year t_i and n_i if the number of cases judges in chamber k_i and year t_i.

[18]If judges were not mobile whatsoever, one would observe perfectly distinct judge clusters, each cluster representing one social chamber.

of the randomness of the allocation of cases to judges.

Relation between the judge bias and the amount of compensation for wrongful dismissal

Our measure of judge bias is relevant only if it is significantly correlated with the amount of compensation in each specific case. To check whether our measure of judge bias is indeed related to the actual compensation, Figure 10 displays the local polynomial fit of the compensation explained by judge pro-worker bias. The judge bias computed from the amount of compensation is indeed highly correlated to the compensation granted by the judges.

Table 3 provides further evidence about the relation between the compensation granted by the judges and their bias computed with the amount of compensation. Table 3 displays the OLS estimators of the regression of the compensation for wrongful dismissal in monthly wages on the judge's pro-worker bias. Column (1) reports the result with Appeal court and sector $\times$ year fixed effects. Column (2) adds control variables comprising the worker's salary, seniority and whether the dismissal is economic or for personal reasons. An increase by one standard deviation in judge bias raises the compensation by about one month of salary.[19]

Judge subjectivity can influence both the compensation amount granted by the judge to the worker and the qualification of the dismissal – either wrongful or lawful. In Appendix C, we construct a judge-specific pro-worker bias with respect to the dismissal qualification and show that our two indices of pro-worker bias are positively correlated.

Analysis of the allocation of cases to judges

If judges are randomly assigned, the addition of control variables in the regression of the qualification of dismissal reported in Column (1) of Table 3 should not significantly change the estimates of the coefficient of the judge bias, as case characteristics should be uncorrelated with judge bias. The assumption that judges are randomly assigned is not rejected insofar as the coefficients are not significantly different (p-value = 0.65) across specifications reported in Columns (1) and (2) of Table 3.

To further check that the measure of judge bias is not the consequence of a non-random allocation of judges to cases, we examine whether judges biases are correlated to the

[19] The standard deviation of the bias of judges is equal to 0.97. Alternatively: being assigned to one of the 10% most pro-worker judges rather than one of the 10% least pro-worker judges increases the amount by about 2 months of salary – the judge bias of the 1st decile is equal to -1.19 and that of the 9th decile to 1.12.

observable characteristics of cases. Tables 4 and 5 display respectively the correlation between pro-worker biases and the characteristics of the case, and the correlations between pro-worker biases and the characteristics of the firm. The amount received at *Prud'hommes*, the seniority of the worker, and the worker's salary are all positively correlated to the compensation granted at Appeal court. The second column of Table 4 therefore offers a sharp contrast to its first column: when regressing the pro-worker bias on the same characteristics, one finds no significant relationship. The second column of Table 5 displays the regression of the judge's severity on the firm's characteristics the year before the judgment, i.e in *t-1*. No significant relationship is found.

Though we obviously cannot test the correlation between judge-specific differences and unobserved variables, such randomization tests are reassuring for our identification strategy. Moreover, we provide further evidence below, showing that the outcomes of firms before the judgment are not correlated with the fixed effect of the judge who handles their case.

All in all, our analysis of Appeal court rulings points to the existence of significant biases on the part of judges which influence the amount of compensation for wrongful dismissal. The next section analyzes the consequence of judge bias on firms' performance.

5 The effects of judge bias on firm performance and firm survival

This section is devoted to the analysis of the impact of judge bias on firms' performance. We start by presenting some descriptive statistics on firms, before presenting the empirical strategy and the results.

5.1 Descriptive statistics

We consider for-profit firms in the private sector, excluding the agricultural sector.[20] Among the sample of Appeal court rulings going from 2006 to 2016, we select firms going to court no later than 2012 in order to analyze outcome variables up to three years after the judgment.[21] As our empirical strategy consists in performing an event study to evaluate the impact of Appeal court judgments on the performance of firms over a period covering several years after the judgment, we drop firms going to court several

[20] The agricultural sector is excluded due to the frequency of seasonal employment in this sector.

[21] Matched employer-employee data are available from 2002 to 2015.

times during the period.[22] We also drop collective dismissals which are few in number and of too heterogeneous sizes to carry out a quantitative analysis. The description of sample restrictions is presented in Appendix D and in Table 6.

Table 7 provides descriptive statistics at the firm level, *i.e.* the level of analysis for our sample. The average number of workers is about 47.9 employees and the median is equal to 14. The firms are relatively young, as 27% are less than 10 years old. 52% of firms end up paying a positive compensation for wrongful dismissal. Conditional on paying a positive compensation, the average amount represents 10.7% of firms' annual payroll, while the median is equal to 2.15%. The survival probability of these firms one year after the judgment is equal to 95% and to 87% three years after.

For small firms at the median or below, i.e. with less than 15 employees (see Table 8), for which the judge bias will be shown to have more impact, the probability of wrongful dismissal is identical but the share of compensation for wrongful dismissal (conditional on being positive) in the annual payroll is much higher; it is equal to about 19.0% for small firms versus 10.7% for the others. Small firms are younger than larger firms as 38% have less than 10 years versus 27%, and their survival probability is significantly smaller: 82% three years after the judgment versus 87%.

5.2 Empirical strategy

Reduced-form regressions

We perform an event study – where the 'event' is the judgment – and analyze the impact of the judge bias on an array of firm performance indicators: firm survival, growth of total, temporary and permanent employment. This approach has two advantages. First, outcome variables can be observed before the judgment so that the potential selection of cases going to courts depending on the type of judge can be evaluated directly. Second, the research design allows for a transparent graphical assessment of the impact of judge bias over time.

For every year-to-event $k \in [-3, 3], k \neq -1$, we estimate from the following equation the average outcome difference between firms in the same social chamber that face a pro-worker judge and firms that face a pro-employer judge:

$$Y_{ik} = \alpha_{0k} + \alpha_{1k} bias_{ij} + \alpha_{2k} X_{ik} + \eta_{ik} \tag{4}$$

where Y_{ik} is the outcome of interest k years before/after the judgement for firm i assigned to judge j;[23] $bias_{ij} = (\bar{\varepsilon}_{ij} - \bar{\varepsilon})/\sigma_{\varepsilon}$, is the judge j's leave-one-out normalized bias (i.e., the

[22]Indeed, if a firm goes several years to Court, disentangling the effect of each Court ruling is impossible.
[23]Firms judged in the same year-to-event can be in different calendar years.

difference between the leave-one-out judge's bias defined in Section 4.2 and the average over all judges ($\bar{\varepsilon}$) scaled in standard deviation (σ_ε) units of the judge bias distribution). X_{ik} can include, depending on specifications, social chamber fixed effects interacted with calendar year fixed effects, an indicator variable for economic dismissals, the age of the firm, the return on assets, the leverage, and the capex of the firm in the year preceding the judgment. To control for potential differences across firms before the judgement, we use as outcome variables Y the growth rates of the variables of interest with respect to the year before the judgement - *i.e.* with respect to $k = -1$. Our dependent variables include indicator variables equal to one for firms which survive 1, 2 or 3 years after the judgment and symmetric growth rates for a set of variables, namely total, temporary and permanent employment.[24]

In the event-study graphs we plot the coefficients α_{1k}, which can be interpreted as the causal effect of the judge bias on the firm growth in year-to-event k. We estimate equation (4) separately for each year-to-event k with OLS. In our case, the randomization occurs primarily at the judge level. Therefore, all standard errors are clustered at the judge level, following Abadie et al. (2017) who state that the standard errors clustering must be decided according to the level at which either the sampling or the randomization is performed. In our case, the randomization occurs primarily at the judge level.[25]

Our approach is different from the two-way fixed effects model, frequently used in event studies, for two reasons. First, by definition, all firms survive until the judgment but can go bankrupt after. This implies that firms before the judgment cannot be a relevant control group to evaluate the counterfactual of firms after the judgment, as in the two-way fixed effects model.[26] From this perspective, estimates before the event date should be interpreted as placebo tests rather than tests of common trend which could be used to identify the effect of the bias of judges. Second, after the judgment, all firms are

[24]As standard in the firm dynamics literature, the symmetric growth rate between year-to-event -1 and k is computed as follows:

$$\Delta Y_{ik} = 2\frac{Y_{ik} - Y_{i-1}}{Y_{ik} + Y_{i-1}}$$

It is a second-order approximation of the log difference for growth rates around 0 and accommodates for entry and exit, by ensuring that growth rates remain between -2 and 2, thus preventing outliers from complicating the analysis. See Törnvist et al. (1985) and Davis et al. (1996). To account for exits in the evaluation of the employment effects, the employment of firms which do not survive is set to zero from the year of their death.

[25]An alternative, used for instance in Dobbie et al. (2018), is to use robust standard errors which are two-way clustered, at the individual and judge level. In our case it would correspond to clustering at the firm and judge level. Results are virtually unchanged.

[26]Hence, it is not possible to use firms that have not been judged yet to recover relevant causal estimates in settings with staggered treatment timing (Athey and Imbens (2021), Callaway and Sant'Anna (2021), de Chaisemartin and D'Haultfœuille (2020), Sun and Abraham (2021))

potentially impacted by the type of their judge. Thus we condition on the judge bias to compare firms' outcomes after the judgment date.

IV regressions

In order to quantify the impact of the compensation on the performance of firms, we regress the performance indicators on the share of the compensation for wrongful dismissal in the firm payroll in the year preceding the judgment, denoted by f_i. We estimate the following equation:

$$Y_{ik} = \beta_{0k} + \beta_{1k} f_i + \beta_{2k} X_{ik} + \epsilon_{ik} \tag{5}$$

where f_i is instrumented by the leave-one-out measure $bias_{ij}$ of the judge bias.

The IV estimation is useful to evaluate the impact of unexpected shocks on the amount of compensation induced by the subjectivity of judges on firms. From this perspective, the OLS estimates of equation (4) can be interpreted as the reduced-form of the IV model.

Identifying assumptions

The first identifying assumption is conditional independence. For both our reduced-form regressions and instrumental regressions to be valid, the judge bias should be uncorrelated with firm and worker characteristics that could affect the firm's future outcomes (controlling for fully interacted court and year dummies). Although this condition is fundamentally non-testable, the random nature of the assignment of judges to cases has been documented above in Sections 2.3 and 4 and is confirmed by the results of the event study in Section 5.3 below.

The second identifying assumption is exclusion. Conditional random assignment of cases to judges is sufficient for a causal interpretation of the reduced-form impact of being assigned to a pro-worker judge. Yet, to interpret the IV estimates as measuring the causal effect of paying a higher compensation to workers requires an exclusion restriction. Namely, the bias of the judge should affect the firm's outcomes only through the compensation to be paid by the firm to the worker, and not in any other way. A challenge is that judges' decisions are multidimensional: judges decide first to qualify the dismissal as fair or unfair, then decide of the compensation to be granted to the worker. We show in Section 6.5 that our estimates are robust to taking such multi-dimensionality into account.

The third identifying assumption is the relevance of the instrument. This has been shown in the previous sections, notably in Figure 10 and the first-stage estimates reported below will confirm that the judge bias is strongly correlated with the share of compensation for wrongful dismissal in the firm payroll.

The fourth and last identifying assumption is monotonicity which requires in our context a positive monotonous correlation between the leave-one-out judge bias and the compensation for wrongful dismissal. Figure 10 indicates that this correlation is indeed positive and monotonous. A second implication of the monotonicity assumption is that the first-stage estimates should be non-negative for all subsamples, a property that will be shown to be satisfied.

5.3 Reduced-form estimates

This section presents the results of the OLS estimation of equation (4). We start by looking at the outcomes in the years preceding the judgment to discuss the selection issue. Then, we analyze the impact of the bias of judges on total, permanent, temporary employment and on firm survival.

Years preceding the judgment

We first display the results of the baseline specification defined by equation (4) where the vector of control variables X_{ik} only includes social chamber fixed effects interacted with calendar year fixed effects and the dismissal type (*i.e.* whether the worker was dismissed for personal or for economic motives) besides the measure of judge bias. This allows us to check whether there is a correlation, before the judgment, between the bias of judge and the performance of firms judged the same year in the same social chamber by different judges without conditioning on any characteristic of firms.

Since it is probable that firms which do not perform well and small firms are more impacted by the compensations set by judges, we examine the impact of judge bias on small firms, with less than 15 employees and whose returns on assets is either below or above the median the year preceding the judgment.

It is clear from Figure 11, which reports the estimates of coefficients α_{1k} of equation (4), that there is no significant employment growth difference in the three years preceding the judgment date between firms that are judged by either by pro-worker or by pro-employer judges. The absence of correlation before the judgment between the bias of judge and employment growth is fulfilled for all groups of firms, depending on their size or on they return on assets the year preceding the judgment. This confirms the assumption that the type of judge does not influence the selection of firms which go to the judgment, even on observable characteristics, since equation (4) is estimated without other control variable than the social chamber fixed effects interacted with calendar year fixed effects and the type of dismissal. Figure 12 shows that controlling for the past performance of firms does not change the common trend.[27] The absence of statistical significant difference between the results obtained with and without control variables confirms once again the absence of selection of cases going to judgement according to the type of judge.

[27] We display the estimates in Table F.

Overall employment

After the judgment year, significant differences in employment growth arise between firms judged by pro-worker judges and firms judged by pro-employer judges – see Figure 12. For all firms, the year after the judgment, a difference in employment growth begins to emerge at the expense of firms facing pro-worker judges. The effects of judge bias become stronger two and three years after the judgment. Three years after the judgment, they are statistically significant for firms taken as a whole. An increase of one standard deviation in the judge bias reduces employment growth by 3 percentage points. At this horizon, the impact on firms is mostly driven by low-performing firms below 15 employees whose employment growth drops by 6 percentage points when the bias of judge raises by one standard deviation. The low-performing firms are more seriously affected by judge bias as time elapses whereas employment of high-performing firms, whose return on assets is above the median before the judgment, is not significantly impacted.

Permanent and temporary employment

Cash flow shocks due to judge bias are expected to have a stronger impact on permanent employment than on temporary employment for two main reasons. First, hiring employees on permanent contracts means making longer-term commitments, which are more difficult to implement when firms have just suffered negative shocks and financial markets are imperfect (Caggese et al., 2019). Second, employers might revise their expectations about dismissal costs according to the decisions of judges, which are all about permanent contract terminations.[28] This should induce firms judged by pro-workers judges to hire less permanent workers. The impact on the number of temporary jobs is ambiguous. It depends on the substitutability between permanent and temporary jobs and on the impact of cash-flow shocks on overall employment.

It is clear from Figure 13 that the overall employment impact of judge bias stems from permanent jobs: the growth rate of temporary employment (i.e. fixed term contracts) is not significantly affected by the bias of judges after the judgment date while that of permanent jobs is significantly impacted.

Firm survival

The judge bias has a significant impact on the survival rate of firms – see Figure 14. Once again, the effects are mostly driven by small low-performing firms whose point estimate is larger, corresponding to a drop of 2 percentage points in the survival rate

[28] Temporary contracts can be terminated at no cost at their termination date. Our sample only includes breaches of permanent jobs.

three years after the judgement when the pro-worker judge bias increases by one standard deviation. The survival probability of high performing firms is much less impacted by the type of judge: their point estimate is much smaller and not significantly different from zero at 95% confidence level.

Interestingly, the employment effects of the judge bias within a 3-year horizon are not solely driven by firm death. Figure 15 shows that judge pro-worker bias has a significant negative impact on the growth rate of employment of low-performing firms which survive 3 years after the judgment. Though the selection of this sub-sample is endogenous, it is still informative about the channels at play.

Overall, it is clear that the judge bias has a significant impact on employment and the survival of low-performing firms below 15 employees. The judge bias has no significant effects on employment of high-performing firms, even if they are small. The amplitude of the shock induced by the subjectivity of the judges seems too low to significantly affect the performance of firms with 15 employees or more. The following section, devoted to the analysis of the impact of the amount of severance pay, sheds additional light on this subject.

5.4 IV estimates

As explained in Section 5.2 our approach allows us to quantify the effect of the amount of compensation for wrongful dismissal induced by judge bias on the outcomes of firms by regressing the firm outcomes on the amount of compensation for wrongful dismissal, expressed in share of the payroll in the year preceding the judgment, and to instrument this variable by the leave-one-out measure of the judge bias. Table 9, which reports the results of the first-stage of IV estimations, confirms that the judge bias is strongly correlated with the share of compensation for wrongful dismissal in the firm payroll, although there is a lack of statistical power for small firms, below 15 employees, especially when the group is split into high-performing and low-performing firms.

Figure 16, which reports the results of the second stage of the IV estimations, shows that an increase in the amount of compensation of one percent of the payroll reduces employment growth by 3.2 percentage points at the 3-year horizon for all firms. The point estimate is larger for small low-performing firms, equal to 5.2 points of percentage. High performing firms are not impacted by the shock on their revenue induced by judge bias. For impacted firms, the effect arises from the growth of permanent employment, while temporary employment is not significantly impacted – see Figure 17.

The point estimates reported for all firms and for firms below 15 employees are very close. This suggests that a transitory shock on the revenue of firms equal to one percent of their payroll has a similar impact on all firms and on small firms. Hence, the stronger employment impact of pro-worker judges on small low-performing firms found in the reduced-form estimates is likely the consequence of the fact that dismissal compensations represent a higher share of the payroll for small firms, below 15 employees, than for all firms – as shown by Tables 7 and 8 – and that the financial capacity of firms depends on their return on assets.

In these circumstances, it can be argued that pro-worker bias on the part of judges has cleansing effects by destroying the structurally weakest firms, which are small and low-performing. It cannot be excluded that pro-worker judges improve overall efficiency, since the jobs they destroy in small low-performing firms might be reallocated at low cost to high performing firms. Addressing this question is left for future research.

6 Robustness checks

We conduct a range of checks both to test the robustness of the previous results and to investigate the mechanisms at play.

6.1 Firm fixed effects

So far, our analysis does not control for the time invariant, non-observable characteristics of firms before the judgment. In principle, this is not a first order concern to the extent that it has been shown that the outcome of firms before the judgment is not correlated with the bias of judges. Nevertheless, it is possible to control for such characteristics by regressing, for all years before $t-1$, our employment growth measure on firm fixed effects. From this we get the fixed effect for each firm. Then, we create a variable equal to employment growth minus this firm fixed effect and we proceed to the same OLS estimation of equation (4) as before using this variable. The comparison of Figures 12 and 18 shows that conditioning on firm fixed effects does not significantly change the results.

6.2 Non-linearity

It is plausible that judges with a strong pro-worker bias who set very high compensation for wrongful dismissal have a disproportionately strong impact, especially on small, low-performing firms. Therefore, we analyze whether judge bias has non-linear effects on firm outcomes by adding a quadratic term in equation (4) for the measure of bias. Table 10 shows that the quadratic term $bias_{ij}^2$ is not different from zero at a 5 percent significance level three years after the judgment for all types of firms. Moreover, from visual inspection of augmented component-plus-residual plots, we do not find any evidence of non-linearity for small firms below 10 employees whose return on assets is below the median, which are the firms for which judge bias has a stronger impact (see Figure 19).

6.3 Heterogeneous effects

The effects of judge bias we find are significant only for low-performing firms – defined as firms with a below-median return on assets. One may wonder whether this result would hold for different measures of the financial situation of firms. In order to investigate this issue, Figure 20 contrasts the effect of judge bias according to the level of return on equity. By definition, the return on equity of high-performing firms is above the median and that of low-performing firms is below the median. The bias of judges has a significant impact on small low-performing firms only, which confirms the results obtained when the performance of firms is measured with the return on assets.

6.4 Sub-sample of large Appeal courts

We examine the results for the sub-sample of cases which go to large Appeal courts that contain several social chambers, because, as explained above in Section 5.2, it is even more likely that the parties do not know until the day of the judgment the identity of the president who will be in charge of the case when there are several social chambers. These large Appeal courts, located at Aix-en-Provence, Paris and Versailles, have 4, 14 and 7 social chambers respectively. Although the number of observations is about half that of the whole sample, Figure 21 shows that we get similar results when the sample is restricted to large Appeal courts. This confirms that our results are not driven by non-random allocation of cases to judges.

6.5 Multidimensional aspects of judges decisions

Interpreting the IV estimates as measuring the causal effect of the compensation on firms requires an exclusion restriction: the amount of compensation determined by judges should affect firms only through the compensation channel and not directly in any other way. In our context, the challenge is that judges decisions include two dimensions: the compensation for wrongful contract breach and the qualification of dismissal which can impact the reputation of firms and therefore their performance independently of the amount of compensation.[29] To deal with this issue, we proceed in two steps.

First, we control, in the reduced-form equation (4), for the judge bias according to the qualification of the dismissal – presented in Appendix C . The results, reported in Table 11,

[29]See Bhuller et al. (2020) in the context of Criminal justice.

show that including this second measure of the judge bias does not change significantly the effect on firm employment growth of the judge bias for the amount of compensation, $bias_{ij}$. Moreover, the judge bias according to the qualification of the dismissal does not significantly affect firm employment growth.

Second, we add the indicator variable for wrongful dismissal in the vector of explanatory variables of the IV model (5). Since this variable is potentially endogenous, this variable and the amount of compensation are instrumented by the judge bias for the amount of compensation, $bias_{ij}$, and by the judge bias for the qualification of dismissal presented in Appendix C. The results, presented in Table 12, show that, when looking at the sample of all firms, having a dismissal deemed wrongful by judges does not significantly affect firm growth, while the total compensation in the share of firms' payroll does have a significant detrimental effect on firm growth.

7 Conclusion and discussion

Using new data on Appeal court rulings about dismissals merged with firm data, this paper provides the first systematic analysis of the impact of judge bias on dismissal compensation and on firm performance. It shows that the subjective opinion of judges influences the amount of dismissal compensation: some judges appear more likely to rule in favor of the employer and others in favor of dismissed workers. We find that the bias of judges has a significant impact on employment and survival of small firms, especially very small and low performing ones, hence partly confirming the intuition of policy makers who implemented reforms to limit the power of judges in the setting of dismissal compensation. While these results provide a useful contribution to the lively debate on the impact of labor court judges subjectivity on firm performance, further research is needed on this under-explored topic. In particular, two directions deserve special attention.

First, to what extent does judge bias contribute to the actual dispersion of compensations for wrongful dismissal? Although our article shows that the subjectivity of judges has a significant impact on the performance of small, low profitable firms, our measure of bias accounts for the variability of the judges' average judgments but does not account for the differences in the attitudes of judges to particular cases.[30] Assessing the impact of such a case-varying subjectivity would require controlling more comprehensively for the informational content extracted by the judge from each case, which could be obtained for instance in the context of an audit study (Clancy et al., 1981; Kahneman et al., 2021).

Second, through which channels do judges' decisions impact firms' performance? The

[30](Clancy et al., 1981; Kahneman et al., 2021) stress the importance of such "Patterned differences" between judges in the context of Criminal justice.

subjectivity of judges can have a direct impact on firms' cash flows but can also shape their expectations regarding termination costs. Disentangling these two channels would require overcoming current data limitations and develop research designs along the lines of Manski (2004) or D'Haultfoeuille et al. (2020) to characterize and test firms' subjective expectations.

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8 Tables

Table 1 – From the initial to the final number of observations used to estimate judge bias

	# of cases	# of judges
Initial severance pay data	149,542	-
(a) Cases for firms not already liquidated	123,304	-
(b) Cases with non-missing president name and surname	117,989	1,039
(c) Cases with non-missing total amount of compensation	84,151	878
(d) Cases with non-missing monthly wage	61,728	731
(e) Elimination of cases in the public sector	39,843	567
(f) Cases restricted to judges with at least 50 cases	30,717	159

Note: This table presents the selection process to obtain the sample of cases on which we estimate the judge fixed effects. Starting from the initial set of all Appeal court rulings from 2006 to 2016 published by the Ministry of Justice which covers all Appeal court rulings, we apply successive filters in order to retain (a) only those firms that we know were not liquidated at the judgment date, otherwise dismissal compensations of liquidated firms would be incurred by a public insurance agency (*Agence de Garantie des salaires*). Then, we eliminate cases for which we do not have the relevant information about either (b) the president's name and surname, (c) the total amount of compensation, or (d) the monthly wage. Finally, we eliminate cases (e) in which the employer belongs to the public sector, and (f) those decided by judges who covered less than 50 cases, our threshold for the calculation of judge fixed-effects. We eventually end up with 30,717 cases and 159 judges. Source: Authors' Appeal court rulings database.

Table 2 – Summary main variables of case-level data

	mean	min	max	sd	count
Total amount in euro	29,260.39	0.00	987,692.02	49,098.75	30,717
Total amount in months of salary	10.58	0.00	76.11	11.17	30,717
Positive total amount	0.90	0.00	1.00	0.30	30,717
Amount for unfair dismissal in euro	12,086.11	0.00	530,000.00	23,756.78	30,717
Amount for unfair dismissal in months of salary	4.40	0.00	66.54	6.21	30,717
Positive amount for unfair dismissal	0.58	0.00	1.00	0.49	30,717
Other amount in euro	17,174.28	0.00	963,154.56	37,273.04	30,717
Prud'hommes amount	7,326.66	0.00	277,200.00	17,642.78	22,519
Amount demanded by worker	42,434.59	1.00	985,536.00	61,265.51	15,349
Higher amount than prud'hommes	0.39	0.00	1.00	0.49	22,519
Lower amount than prud'hommes	0.17	0.00	1.00	0.38	22,519
Same amount as prud'hommes	0.44	0.00	1.00	0.50	22,519
Worker who appealed	0.60	0.00	1.00	0.49	27,925
Economic dismissal	0.15	0.00	1.00	0.36	30,717
Worker's seniority in months	82.16	0.00	538.00	88.45	22,142

Note: This table displays the mean, the minimum, the median, the maximum, the standard deviation and the number of observations for several important characteristics of the cases used to estimate judge bias. Source: Appeal court rulings database.

Table 3 – Correlation between judge bias and compensation for wrongful dismissal

	Compensation	Compensation
	(1)	(2)
Judge pro-worker bias	0.952***	0.936***
wrt compensation	(0.253)	(0.250)
Year FE	Yes	Yes
Court FE	Yes	Yes
Case controls	No	Yes
F test	14.20	13.97
# obs	8,354	8,354

Note: Each cell corresponds to one regression where the dependent variable is the total compensation for wrongful dismissal. Control variables included in column (2): indicator variable for economic dismissal,wage, seniority. The bottom and top fifth percentiles of judge bias are trimmed to account for the non-linearity of the relation between judge bias and the qualification of dismissal displayed on Figure 10. Court and year x sector fixed effects are used. Standard errors, clustered at the judge level, are in parenthesis.*, **, and *** denote statistical significance at 10, 5 and 1%. Source: Appeal court rulings database.

Table 4 – Randomization test for judge bias on total compensation for wrongful dismissal: case-level characteristics

	Compensation in monthly wages	Judge pro-worker bias in monthly wages
Amount at Prud'hommes (in months)	0.536***	-0.002
	(0.083)	(0.002)
Legislation threshold applied	0.116	0.013
	(0.386)	(0.027)
Seniority	0.019***	0.000
	(0.004)	(0.000)
Number of employees	-0.000	-0.000
	(0.000)	(0.000)
Worker's salary	-0.000***	0.000
	(0.000)	(0.000)
Economic dismissal	1.116	-0.025
	(0.683)	(0.030)
Time between dismissal and Appeal Court	0.001	-0.000
	(0.001)	(0.000)
Joint F-Test	0.0000	0.7458
Observations	4,948	4,948

Note: The dependent variable in the first column is the total compensation for wrongful dismissal. The dependent variable in the second column is the judge pro-worker bias computed as defined in section 4.2. Standard errors are displayed in parentheses. Covariates include Appeal court × year fixed effects. The number of observations is smaller than in the data set used to estimate the judge bias because the explanatory variables used in this table are not available in all rulings. Standard errors clustered at the judge level. Standard errors, clustered at the judge level, are in parenthesis.*, **, and *** denote statistical significance at 10, 5 and 1%. Source: Appeal court rulings database.

Table 5 – Randomization test for judge bias on compensation for wrongful dismissal: firm-level characteristics

	Compensation in monthly wages	Judge pro-worker bias in monthly wages
Number of workers in t-1	10.397*	0.933*
	(6.080)	(0.544)
Sales in t-1	0.044	-0.002
	(0.034)	(0.001)
Total wages in t-1	-0.538	-0.073
	(0.507)	(0.045)
Value added in t-1	0.321	0.025
	(0.315)	(0.024)
Value added in t-1	-0.807	-0.021
	(0.728)	(0.041)
Debt in t-1	0.125	0.011
	(0.156)	(0.010)
Joint F-Test	0.026	0.426
Observations	4,475	4,475

Note: The dependent variable in the first column is an indicator variable equal to one if the dismissal is deemed wrongful. The dependent variable in the second column is the judge pro-worker bias computed as defined in section 4.2. Standard errors are displayed in parentheses. Covariates include Appeal court × year fixed effects. All independent variables are transformed to increase clarity of the table: variables are divided by 1000. The number of observations is smaller than in the data set used to estimate the judge bias because the explanatory variables used in this table are not available from all rulings. Standard errors are clustered at the judge level.*, **, and *** denote statistical significance at 10, 5 and 1%. Source: DADS, FICUS-FARE, SIREN, Appeal court rulings database.

Table 6 – From the initial to the final number of observations used to estimate the effect of judge bias on firm performance

	# of cases	# of firms	# of judges
a. Initial sample used to compute judge fixed effects	30,717	-	159
b. Sample of cases judged by judges for which we have computed a fixed effect	101,010	-	159
c. Cases with non-missing firm identifier	65,623	39,966	159
d. Cases for which firm identifier is matched in DADS and FARE	43,882	25,833	159
e. Firms with only one Appeal Court case	18,046	18,046	159
f. Restriction to years of judgement < 2013	16,123	16,123	159
g. Surviving firms with non-missing required variables in DADS data	9,227	9,227	142
h. Trimming first and last centiles of judges' bias	9,035	9,035	135
i. Firms with non-missing required variables in FARE	7,329	7,329	133

Note: The final sample is restricted to private firms, with non-missing case-related, employment-related and financial information, which go to Appeal courts once for individual dismissals. Details on the sample selection are provided in Appendix D. Source: DADS, FICUS-FARE, SIREN, Appeal court rulings database.

Table 7 – Summary of main variables at firm-level - all firms

	mean	min	med	max	sd	count
Nb of workers	47.89	1.00	14.00	4645.00	142.57	7329.00
Nb of hires	10.95	0.00	4.00	725.00	32.25	7324.00
Nb of exiters	10.37	0.00	3.00	996.00	35.84	7329.00
Sales (in K euros)	6237.23	0.00	2008.00	64482.00	10360.51	6960.00
Value added (in K euros)	1819.74	0.00	778.00	17798.00	2717.28	6867.00
Share of firms in manufacturing	0.19	0.00	0.00	1.00	0.40	7329.00
Share of firms in construction	0.11	0.00	0.00	1.00	0.31	7329.00
Share of firms in services	0.33	0.00	0.00	1.00	0.47	7329.00
Share of firms < 10 years	0.27	0.00	0.00	1.00	0.44	7329.00
Survival at t+1	0.95	0.00	1.00	1.00	0.22	7329.00
Survival at t+2	0.90	0.00	1.00	1.00	0.30	7329.00
Survival at t+3	0.87	0.00	1.00	1.00	0.34	7329.00
Wrongful dismissal	0.52	0.00	1.00	1.00	0.50	5344.00
Amount in wage bill (when >0)	10.68	0.00	2.15	1336.30	40.08	5340.00
Judge pro-worker bias	-0.04	-2.05	-0.04	2.73	0.76	7329.00
Amount	11.81	0.00	8.40	442.43	15.68	3553.00

Note: The sample is defined in Appendix D. "Nb of workers" corresponds to headcounts on 31 December before the judgment year. "Amount" stands for the total amount of compensation (in euro). "Wrongful dismissal" is a dummy variable equal to one if the dismissal is deemed wrongful. Source: DADS, FICUS-FARE, SIREN, Appeal court rulings database.

Table 8 – Summary of main variables at firm-level - small firms (< 15 employees)

	mean	min	med	max	sd	count
Nb of workers	6.56	1.00	6.00	14.00	3.67	3677.00
Nb of hires	2.42	0.00	2.00	251.00	4.94	3677.00
Nb of exiters	2.50	0.00	2.00	320.00	7.22	3677.00
Sales (in K euros)	1489.21	0.00	847.00	61353.00	2531.33	3607.00
Value added (in K euros)	466.57	0.00	338.00	14315.12	584.92	3534.00
Share of firms in manufacturing	0.14	0.00	0.00	1.00	0.35	3677.00
Share of firms in construction	0.11	0.00	0.00	1.00	0.32	3677.00
Share of firms in services	0.32	0.00	0.00	1.00	0.47	3677.00
Share of firms < 10 years	0.38	0.00	0.00	1.00	0.48	3677.00
Survival at t+1	0.93	0.00	1.00	1.00	0.26	3677.00
Survival at t+2	0.87	0.00	1.00	1.00	0.34	3677.00
Survival at t+3	0.82	0.00	1.00	1.00	0.38	3677.00
Wrongful dismissal	0.52	0.00	1.00	1.00	0.50	2641.00
Amount in wage bill (when >0)	18.98	0.00	6.39	1336.30	55.30	2641.00
Judge pro-worker bias	-0.02	-2.05	-0.04	2.73	0.76	3677.00
Amount	10.82	0.00	7.49	442.43	16.82	1809.00

Note: Firms whose size is equal or below the median size of the sample defined in Appendix D. “Nb of workers” corresponds to headcounts on 31 December before the judgment year. “Amount” stands for the total amount of compensation (in euro). “Wrongful dismissal” is a dummy variable equal to one if the dismissal is deemed wrongful. Source: DADS, FICUS-FARE, SIREN, Appeal court rulings database.

Table 9 – First-stage IV estimates

	(1) All All ROA	(2) < 15 All ROA	(3) All Low ROA	(4) < 15 Low ROA	(5) All High ROA	(6) < 15 High ROA
$bias_{ij}$	0.484**	0.512*	0.701**	1.069**	0.290	0.067
	(0.145)	(0.295)	(0.214)	(0.423)	(0.198)	(0.392)
N	5203	2502	2650	1279	2540	1200
R^2	0.087	0.128	0.127	0.174	0.117	0.173
F	49.962	5.490	22.129	5.285	23.916	2.094

Note: This table presents the first-stage estimates of the IV regression where the share of total compensation for wrongful dismissal in the payroll of the year preceding the judgment is instrumented by the continuous measure of the leave-one-out judge bias, $bias_{ij}$. Each cell corresponds to one regression where the dependent variable is the share of total compensation for wrongful dismissal in the payroll of the year preceding the judgment. Columns (1) and (2) display the results for all firms and firms with less than 15 employees the year preceding the judgment respectively; Columns (3) and (4) display similar results for all firms and firms with less than 15 employees with return on assets below the median the year preceding the judgment. Columns (5) and (6) display similar results for all firms and firms with less than 15 employees with return on assets above the median the year preceding the judgment. Covariates include social chamber fixed effects interacted with calendar year fixed effects, an indicator variable for economic dismissals, the age of the firm, the return on assets, the leverage and the capex the year preceding the judgment. The number of observations is smaller than for the reduced form estimations because there are missing observations for the payroll in the year preceding the judgment and the data has been trimmed to eliminate the observations with the top 5% share of total compensation for wrongful dismissal in the payroll of the year preceding the judgment. Standard errors, displayed in parentheses, are clustered at the judge level. *, **, and *** denote statistical significance at 10, 5 and 1%s, are clustered at the judge level. Source: DADS, FICUS-FARE, SIREN, Appeal court rulings database.

Table 10 – Judge bias and firm performance 3 years after the judgment, with quadratic terms

	Firm type			
	All firms	< 15 employees	All and low ROA	< 15 employees and low ROA
$bias_{ij}$	-0.031***	-0.043**	-0.039**	-0.059**
	(0.009)	(0.015)	(0.015)	(0.024)
$bias_{ij}^2$	0.012*	-0.000	-0.001	-0.030
	(0.007)	(0.011)	(0.011)	(0.018)
N	7329	3677	3673	1849
R^2	0.090	0.104	0.107	0.140

Note: This table displays the coefficients of equation (4) associated with the explanatory variables $bias_{ij}$ and $bias_{ij}^2$ three years after the judgment where the dependent variable is the symmetric employment growth rates relative to the year preceding the judgment, controlling for social chamber fixed effects interacted with calendar year fixed effects, an indicator variable for economic dismissals, the age of the firm, the return on assets, the leverage and the capex the year preceding the judgment. Standard errors, displayed in parentheses, are clustered at the judge level. *, **, and *** denote statistical significance at 10, 5 and 1%. Source: DADS, FICUS-FARE, SIREN, Appeal court rulings database.

Table 11 – Event study: employment growth rate depending on the judge bias (with control variables) and controlling for the judge bias as defined according to the dismissal qualification

	(1)	(2)	(3)	(4)	(5)	(6)
	t-3	t-2	t	t+1	t+2	t+3
All firms						
Judge bias wrt total amount	0.005	-0.004	0.002	-0.021**	-0.026**	-0.028**
	(0.006)	(0.004)	(0.005)	(0.007)	(0.010)	(0.011)
Judge bias wrt total amount	-0.005	-0.000	-0.006	-0.002	-0.001	-0.004
	(0.007)	(0.004)	(0.005)	(0.007)	(0.009)	(0.010)
N	7329	7329	7329	7329	7329	7329
< 15 employees						
Judge bias wrt total amount	0.001	-0.004	0.006	-0.022*	-0.038**	-0.044**
	(0.009)	(0.006)	(0.009)	(0.011)	(0.017)	(0.019)
Judge bias wrt dismissal qualification	-0.001	0.001	-0.008	-0.005	0.005	0.003
	(0.010)	(0.006)	(0.009)	(0.012)	(0.017)	(0.018)
N	3677	3677	3677	3677	3677	3677
All firms with low ROA						
Judge bias wrt total amount	0.004	-0.006	-0.005	-0.037**	-0.048**	-0.042**
	(0.010)	(0.005)	(0.006)	(0.013)	(0.018)	(0.017)
Judge bias wrt dismissal qualification	-0.001	0.002	-0.000	0.018	0.015	0.006
	(0.010)	(0.004)	(0.006)	(0.012)	(0.017)	(0.017)
N	3673	3673	3673	3673	3673	3673
< 15 employees & low ROA						
Judge bias wrt total amount	0.000	-0.007	0.001	-0.041*	-0.076**	-0.076**
	(0.014)	(0.007)	(0.011)	(0.021)	(0.034)	(0.033)
Judge bias wrt dismissal qualification	0.006	0.006	0.006	0.025	0.033	0.037
	(0.013)	(0.006)	(0.012)	(0.024)	(0.034)	(0.032)
N	1849	1849	1849	1849	1849	1849
All firms with high ROA						
Judge bias wrt total amount	0.006	-0.002	0.007	-0.005	-0.004	-0.010
	(0.009)	(0.006)	(0.006)	(0.009)	(0.012)	(0.014)
Judge bias wrt dismissal qualification	-0.011	-0.003	-0.011*	-0.021**	-0.015	-0.017

Table 11 – *Continued from previous page*

	(1)	(2)	(3)	(4)	(5)	(6)
	t-3	t-2	t	t+1	t+2	t+3
	(0.010)	(0.006)	(0.006)	(0.009)	(0.011)	(0.012)
N	3638	3638	3638	3638	3638	3638

< 15 employees & high ROA

Judge bias wrt total amount	0.002	0.005	0.013	0.003	0.001	-0.000
	(0.011)	(0.009)	(0.012)	(0.014)	(0.021)	(0.023)
Judge bias wrt dismissal qualification	-0.009	-0.010	-0.023**	-0.042**	-0.025	-0.046**
	(0.013)	(0.010)	(0.011)	(0.015)	(0.020)	(0.022)
N	1813	1813	1813	1813	1813	1813

Note: This table displays the coefficients α_{ik} of equation (4) in year k, $k \in [3, 3]$ relative to the judgment year t where the dependent variable is the symmetric employment growth rates relative to the year preceding the judgment year t and the explanatory variable is the leave-one-out judge bias, $bias_{ij}$, controlling for social chamber fixed effects interacted with calendar year fixed effects, an indicator variable for economic dismissals, the age of the firm, the return on assets, the leverage and the capex the year preceding the judgment. Contrary to equation (4), we also control for a second measure of judge bias, defined according to the dismissal qualification. Standard errors are clustered at the judge level. Source: DADS, FICUS-FARE, SIREN, Appeal court rulings database.

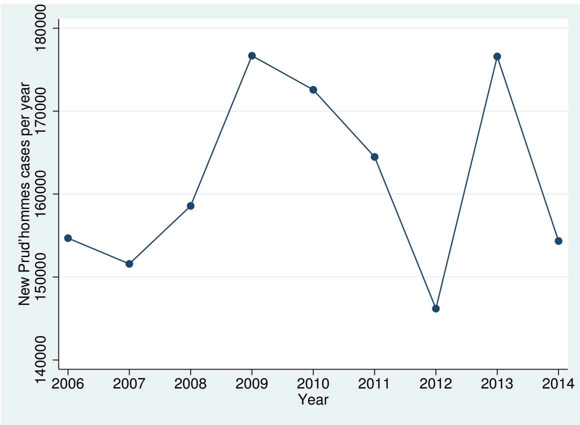
Table 12 – Event study: employment growth rate depending on the amount of compensation in the previous year payroll and the dismissal qualification, instrumented by two measures of judge bias

	(1)	(2)	(3)	(4)	(5)	(6)
	t-3	t-2	t	t+1	t+2	t+3
All firms						
Total compensation in % payroll	0.007	-0.008	-0.003	-0.036**	-0.046**	-0.034**
	(0.011)	(0.005)	(0.009)	(0.018)	(0.021)	(0.017)
Dismissal deemed unfair	-0.572	0.040	-0.165	0.353	0.349	-0.415
	(0.382)	(0.248)	(0.382)	(0.656)	(0.849)	(0.706)
N	5180	5180	5180	5180	5180	5180
< 15 employees						
Total compensation in % payroll	-0.030	-0.008	-0.006	-0.031	-0.035	-0.061
	(0.061)	(0.015)	(0.024)	(0.041)	(0.043)	(0.086)
Dismissal deemed unfair	-1.976	0.188	-0.643	-1.017	-0.248	-2.437
	(3.291)	(0.902)	(1.477)	(2.262)	(2.624)	(4.594)
N	2494	2494	2494	2494	2494	2494

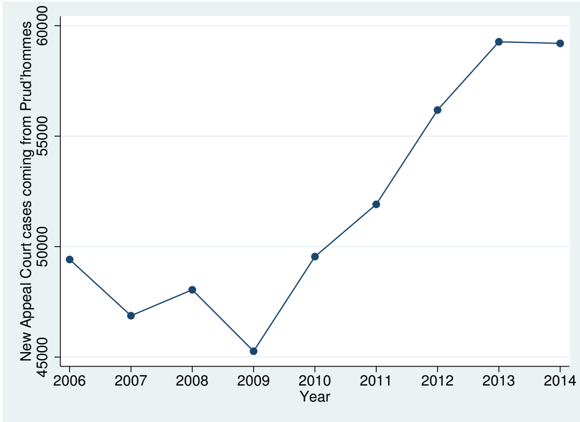
Note: This table displays the coefficient β_{1k} of equation (5) in year k,$k \in [-3, 3]$ relative to the judgment year t where the dependent variable is the symmetric employment growth rates relative to the year preceding the judgment year t and the explanatory variables are the total amount of compensation in the previous year payroll and the dismissal qualification. Those two explanatory variables are instrumented by the leave-one-out judge bias, $bias_{ij}$, and a measure of judge bias defined according to the dismissal qualification. We control for social chamber fixed effects interacted with calendar year fixed effects, an indicator variable for economic dismissals, the age of the firm, the return on assets, the leverage and the capex the year preceding the judgment. Standard errors are clustered at the judge level. Source: DADS, FICUS-FARE, SIREN, Appeal court rulings database.

9 Figures

Figure 1 – Number of new *Prud'hommes* cases per year and new Appeal court cases coming from Prud'hommes per year in France



(a) New *Prud'hommes* cases



(b) New Appeal court cases coming from *Prud'hommes*

Note: Figure (a) on the left displays the numbers of new cases opened per year for all French Employment Tribunals (including non-metropolitan France). Figure (b) on the right displays the numbers of new Appeal court cases coming from *Prud'hommes* opened per year. Figures were constructed using datasets on *Prud'hommes* and Appeal court activity available on the website of the French Ministry of Justice. Numbers displayed do not include requests for interim measures (*demande en référé*). Source: Appeal court rulings database.

Figure 2 – Example of end of Appeal court ruling

PAR CES MOTIFS

LA COUR,

Statuant par arrêt contradictoire,

INFIRME PARTIELLEMENT le jugement déféré et statuant à nouveau,

CONDAMNE la Société [redacted] à verser à Monsieur B. 30.000 € (TRENTE MILLE EUROS) à titre d'indemnité pour licenciement sans cause réelle et sérieuse ;

ORDONNE le remboursement par la Société [redacted] à l'organisme concerné des indemnités de chômage effectivement versées à Monsieur B. par suite de son licenciement et ce dans la limite de trois mois ;

DÉBOUTE Monsieur B. de sa demande au titre de dommages et intérêts pour manquement aux obligations conventionnelles ;

CONFIRME pour le surplus le jugement déféré ;

Y ajoutant,

CONDAMNE la Société [redacted] à verser à Monsieur B. la somme de 1.000 € (MILLE EUROS) au titre de l'article 700 du Code de Procédure Civile ;

DÉBOUTE la Société [redacted] de sa demande au titre de l'article 700 du Code de Procédure Civile ;

CONDAMNE la Société [redacted] aux entiers dépens.

Prononcé publiquement par mise à disposition de l'arrêt au greffe de la Cour, les parties en ayant été préalablement avisées dans les conditions prévues au deuxième alinéa de l'article 450 du Code de Procédure Civile,

Et signé par Madame [redacted] président, et par Madame [redacted], greffier, auquel la minute de la décision a été remise par le magistrat signataire.

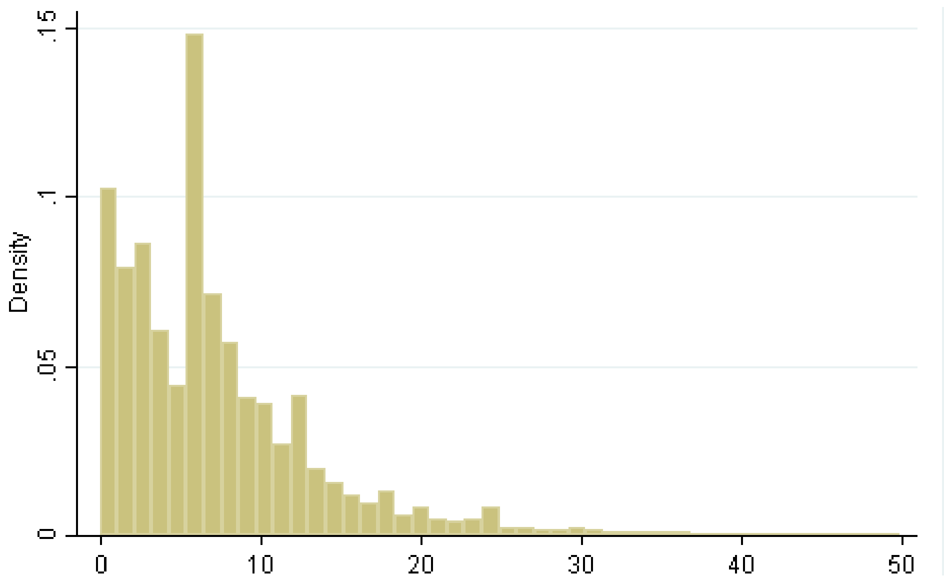
LE GREFFIER LE PRÉSIDENT

Minute en sept pages.

Composition de la juridiction : [redacted]
Décision attaquée : C. Prud. Longwy, Nancy 2011-02-25

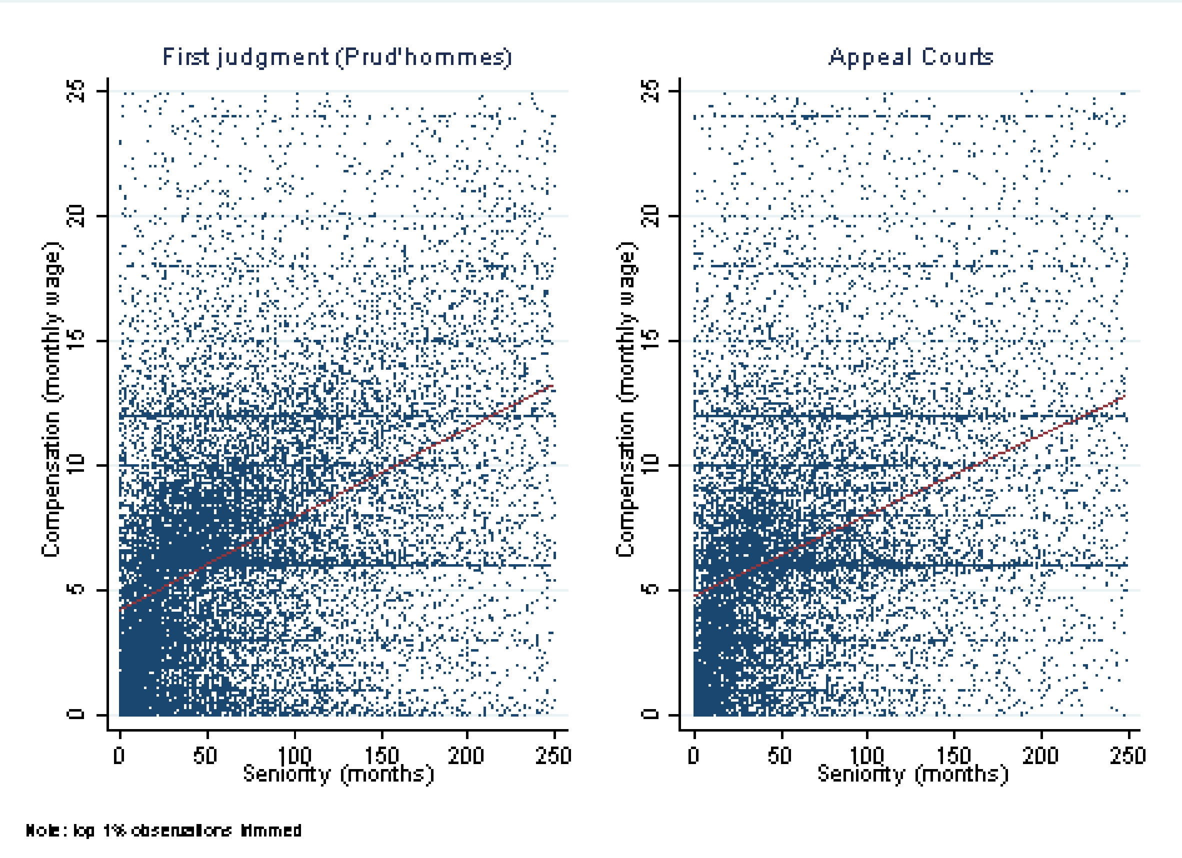
Source: Appeal court rulings database.

Figure 3 – Histogram of compensation amounts in monthly wage



Note: This graph is an histogram of compensation amounts in monthly wages, conditional on this amount being positive. Only amounts lower than 50 months of salary are displayed. Source: Appeal court rulings database.

Figure 4 – Compensations for wrongful dismissals and seniority

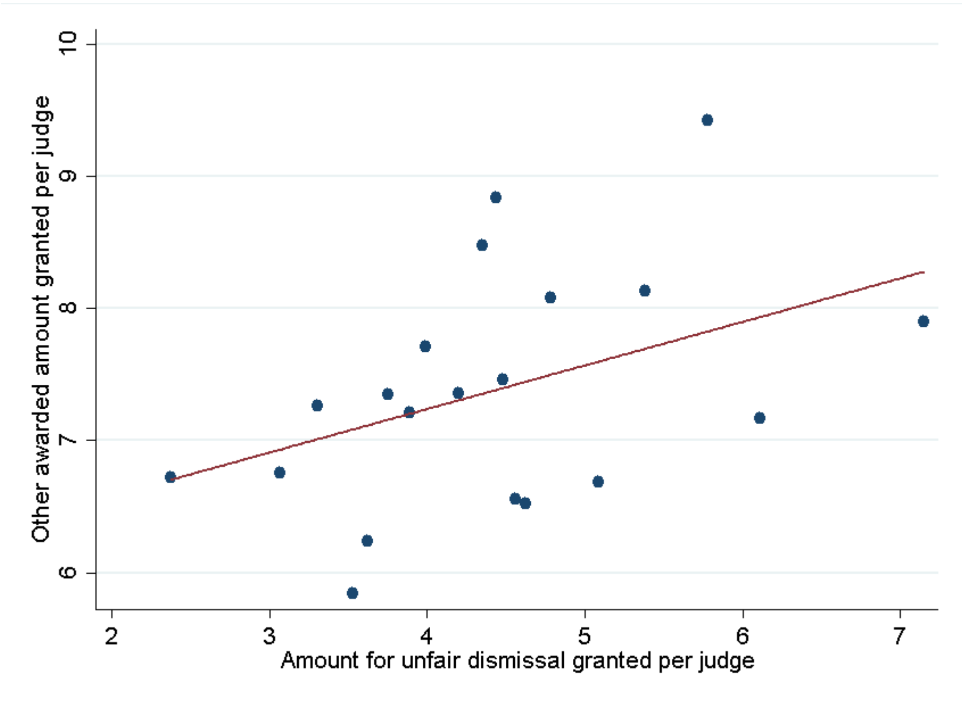


Note: These graphs are scatter plots of compensations for wrongful dismissals depending on seniority. Compensations are expressed in monthly wage. The left panel displays compensations set by *prud'hommes* and the right panel displays compensations set by Appeal courts. Source: Appeal court rulings database.

Figure 5 – Relation between compensations for wrongful dismissals set by Appeal courts and by *prud'hommes*

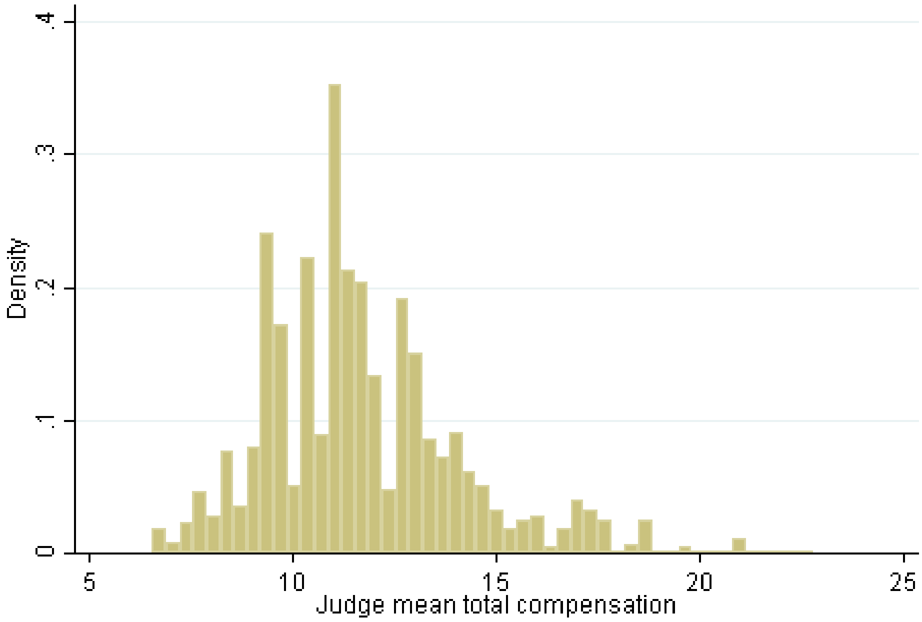
Note: This graph is a scatter plot of the compensations for wrongful dismissals set by Appeal courts and by *prud'hommes*. Compensations are expressed in monthly wage. Source: Appeal court rulings database.

Figure 6 – Relation between mean compensation per judge for unfair dismissal and mean compensation granted for other reasons



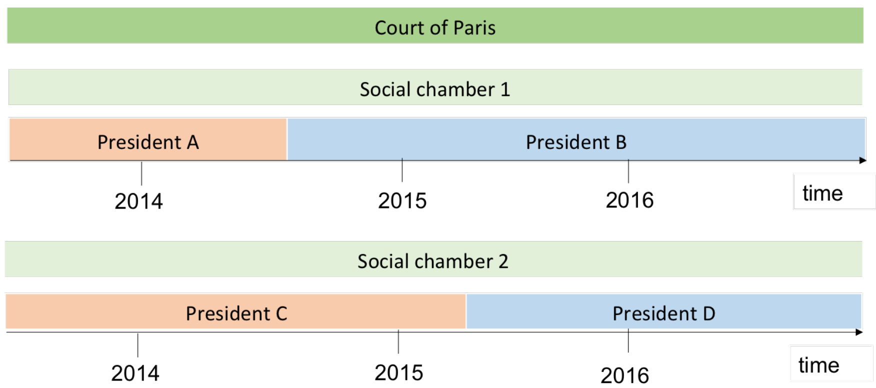
Note: This figure exhibits the scatter plot of mean compensation in month of salary for unfair dismissal per judge, grouped in 20 equal-sized bins, against the mean compensation for other reasons. Case-level data are used, therefore the number of observations used is the number of different cases for which we are able to compute the pro-worker bias. Source: Appeal court rulings database.

Figure 7 – Histogram of mean compensation per judge



Note: This figure exhibits the histogram of mean compensation in month of salary per judge. Case-level data are used, therefore the number of observations used is the number of different cases for which we are able to compute the pro-worker bias. Source: Appeal court rulings database.

Figure 8 – Allocation of cases exploited for identification

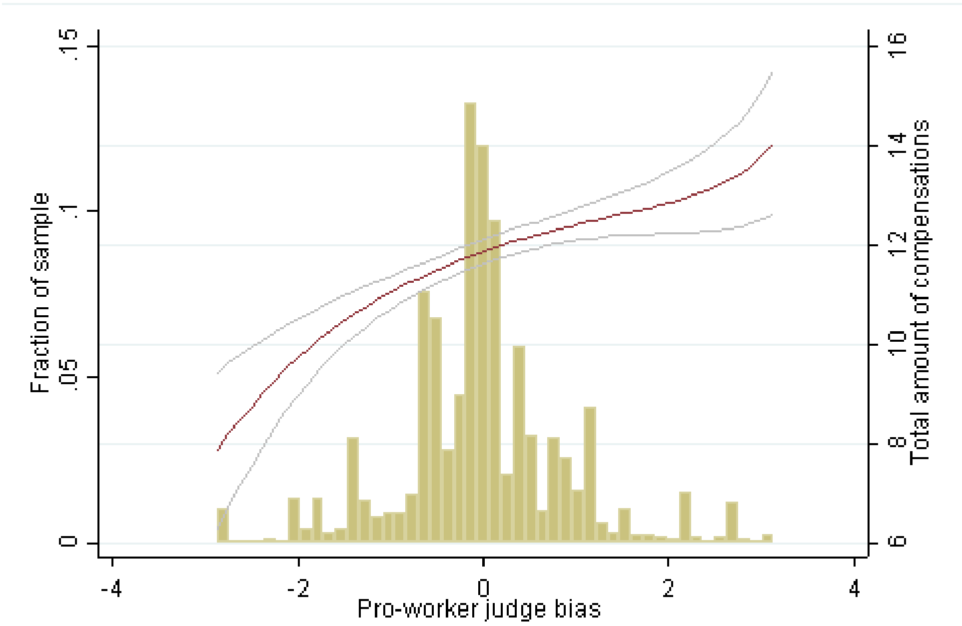


Note: This figure displays the allocation of cases to judges used for identification. Within an Appeal court, there may be several social chambers. Within each social chamber, there is, at an instant t, one chamber president who judges the cases. When judges change assignments in the course of a year, for instance in 2014, one can identify the allocation to president A or president B.

Figure 9 – Network of judges

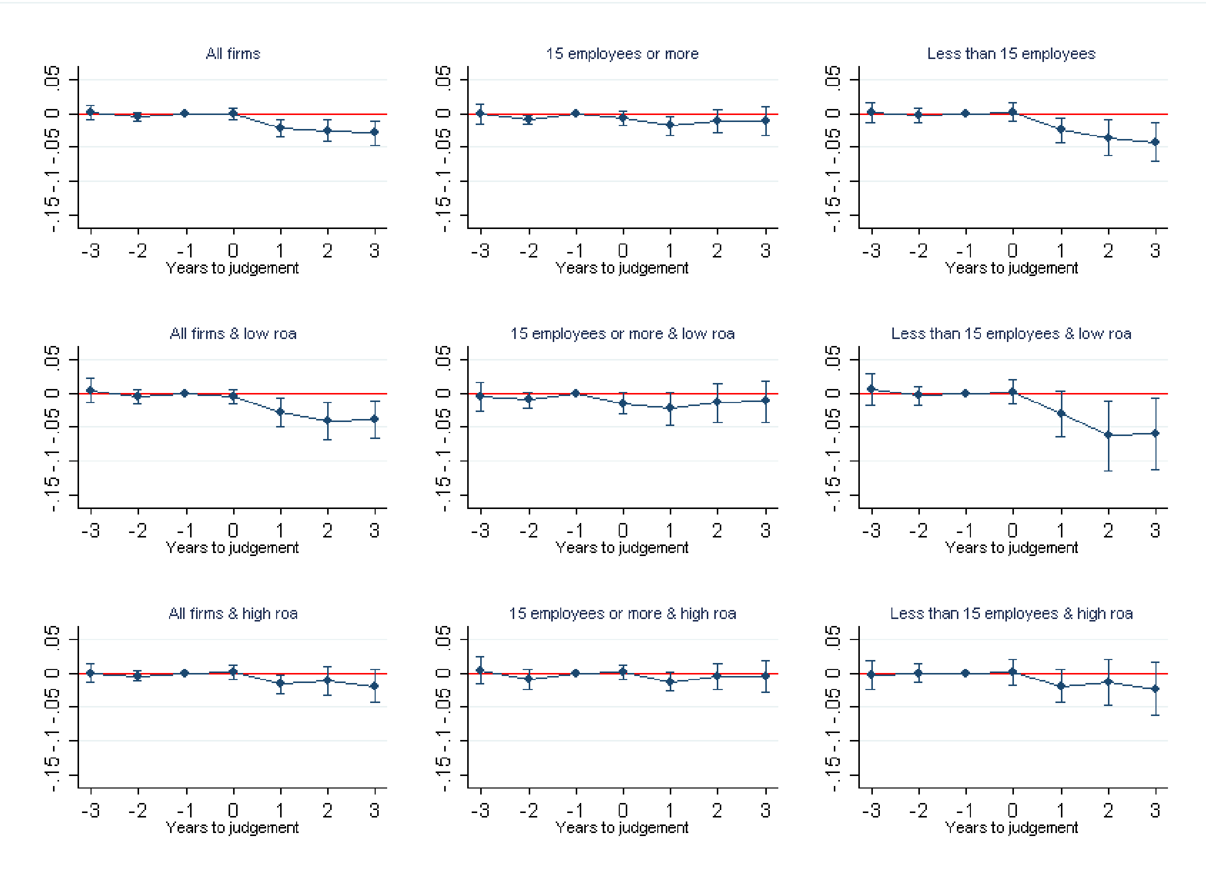
Note: Each dot represents a judge. Two dots are connected if the two judges shared the same social chamber at least once. The higher the network density, the higher the mobility of judges across social chambers. If judges were not mobile whatsoever, one would observe perfectly distinct judge clusters, each cluster representing one social chamber. Source: Appeal court rulings database.

Figure 10 – Judges pro-worker biases with respect to the compensation in months of salary



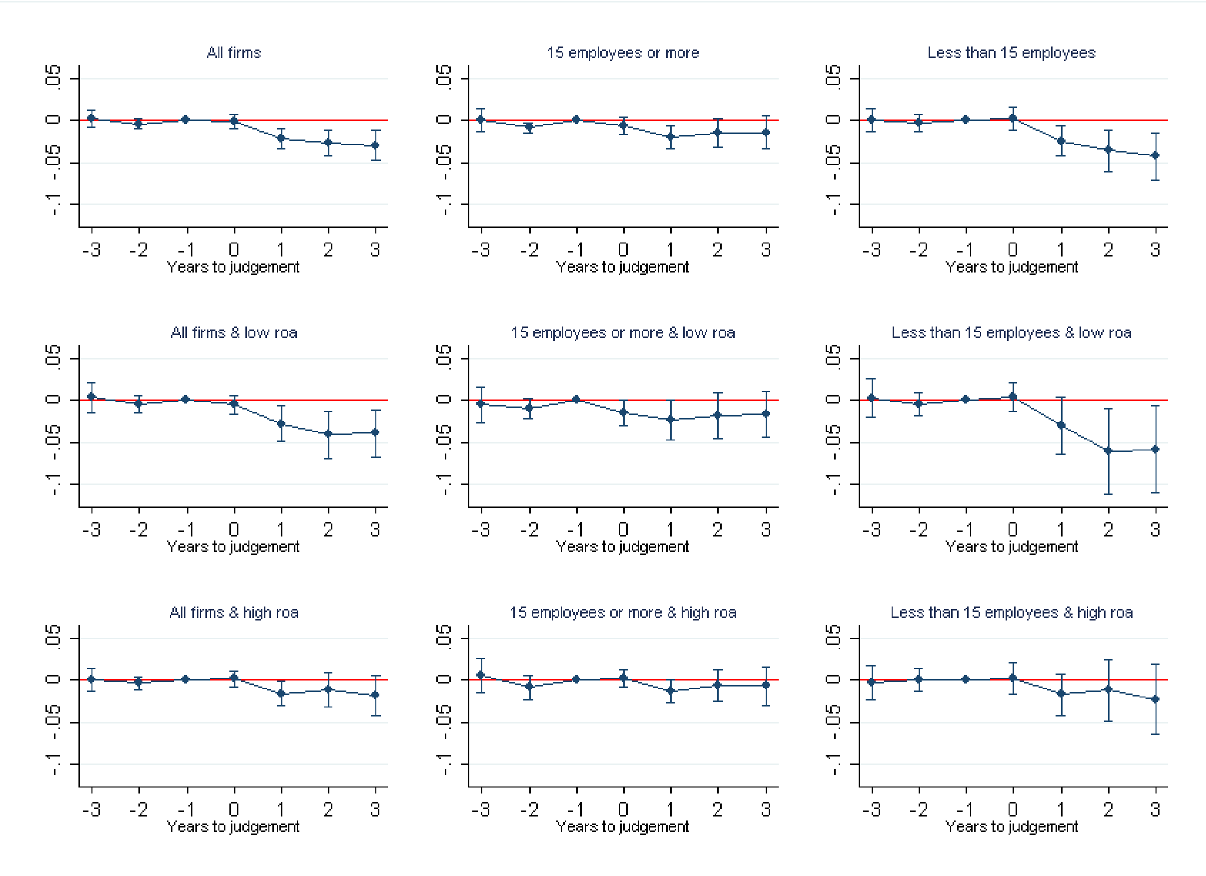
Note: This figure displays the histogram of the pro-worker biases of judges with respect to the total amount of compensation for wrongful dismissal and a local polynomial fit of the total amount of compensation, represented by the red line. The grey lines display the frontiers of the 95% confidence interval of the local polynomial fit. Case-level data are used, therefore the number of observations is the number of different cases for which we are able to compute the pro-worker bias reported in Table 2. The pro-worker bias is computed as defined in Section 4.2. Source: Appeal court rulings database.

Figure 11 – Event study: employment growth rate depending on the judge bias



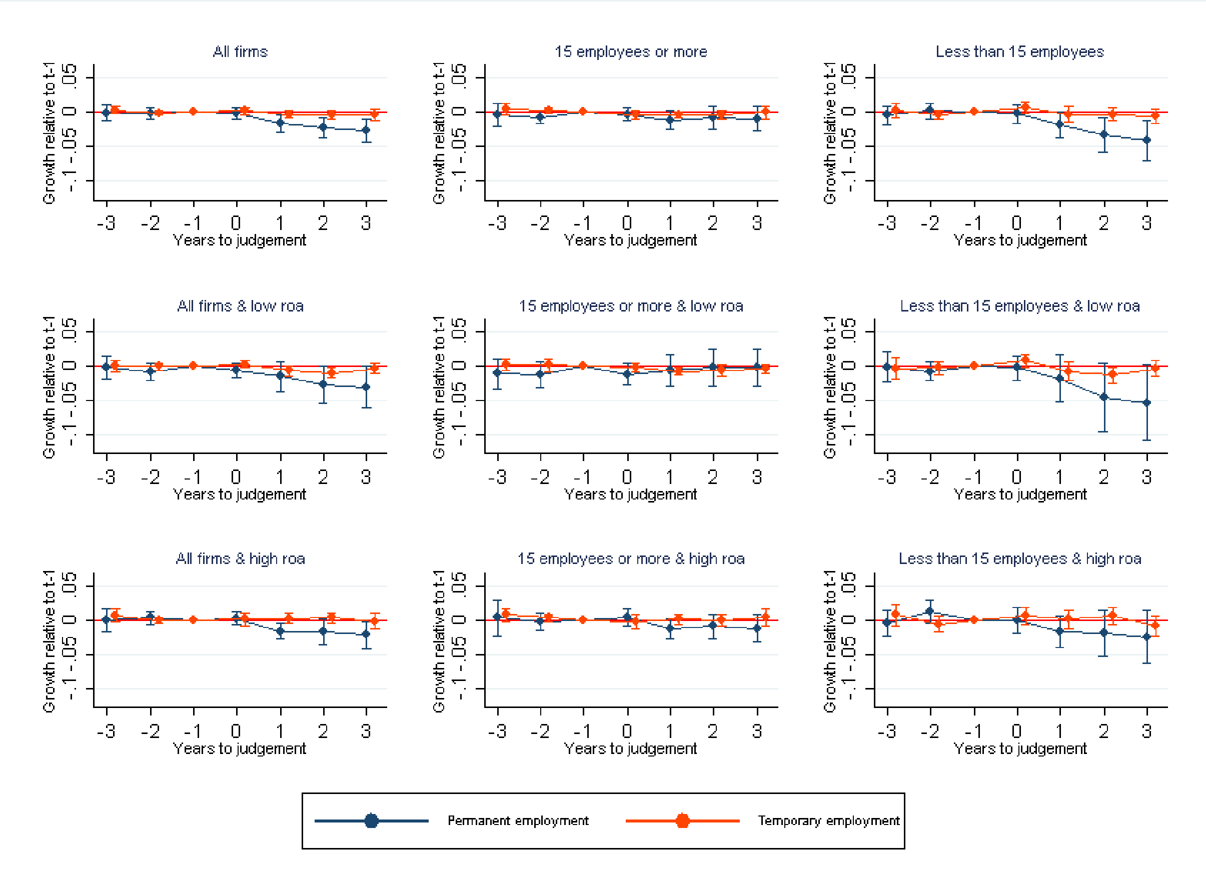
Note: This figure displays the coefficients α_{1k} of equation(4) in year k,$k \in [-3, 3]$ relative to the judgment year t where the dependent variable is the symmetric employment growth rate relative to the year preceding the judgment year t and the explanatory variable is the leave-one-out judge bias, $bias_{ij}$, controlling for social chamber fixed effects interacted with calendar year fixed effects and an indicator variable for economic dismissals. The left panel reports the results for all firms, the middle panel panel for firms with 15 employees or more and the right panel for firms under 15 employees the year preceding the judgment. The top panel is for all firms independently of their return on assets the year preceding the judgment, the middle panel for firms with return on assets below the median and the bottom panel for firms with return on assets above the median. Standard errors are clustered at the judge level. Vertical bars represent 95% confidence intervals. Source: DADS, FICUS-FARE, SIREN, Appeal court rulings database.

Figure 12 – Event study: employment growth rate depending on the judge bias (with control variables)



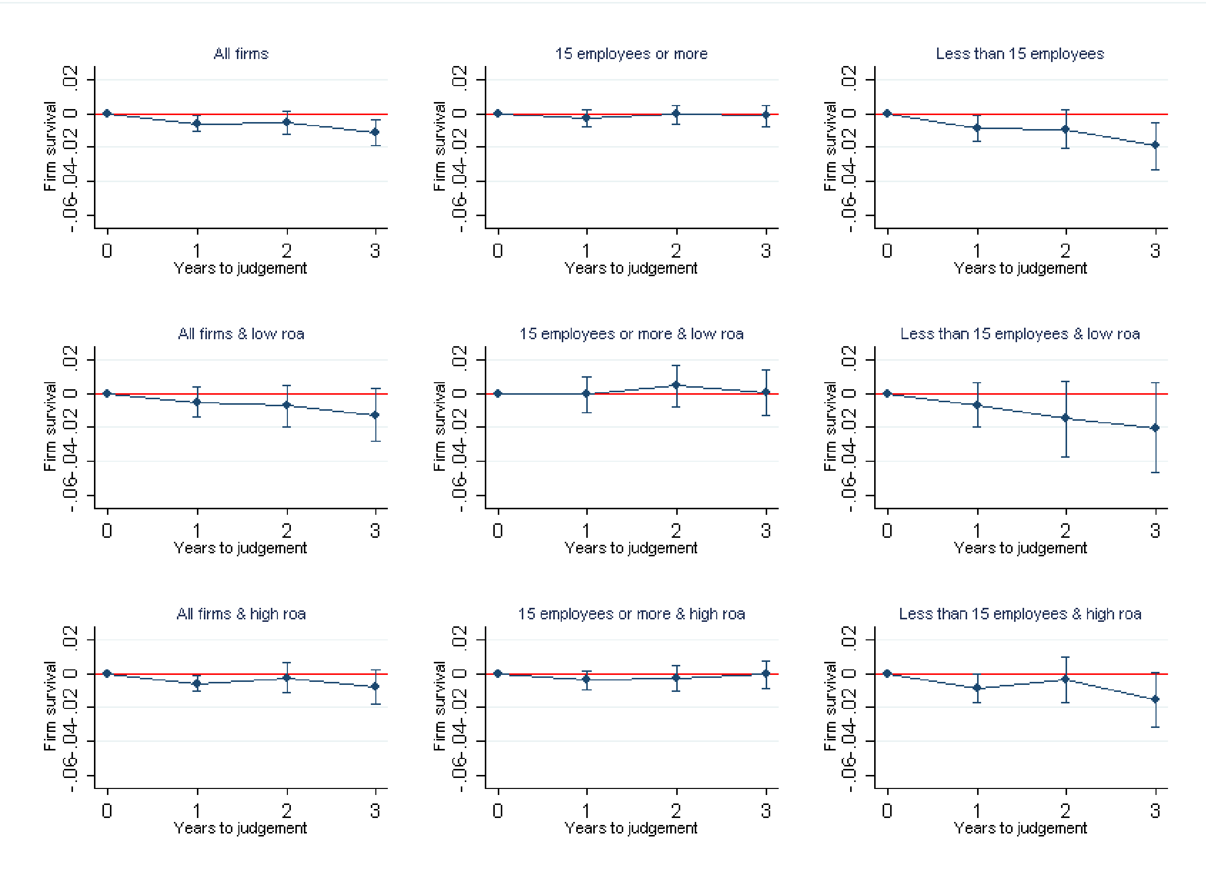
Note: This figure displays the coefficients α_{1k} of equation (4) in year k,$k \in [-3, 3]$ relative to the judgment year t where the dependent variable is the symmetric employment growth rates relative to the year preceding the judgment year t and the explanatory variable is the leave-one-out judge bias, $bias_{ij}$, controlling for social chamber fixed effects interacted with calendar year fixed effects, an indicator variable for economic dismissals, the age of the firm, the return on assets, the leverage and the capex the year preceding the judgment. The left panel reports the results for all firms, the middle panel panel for firms with 15 employees or more and the right panel for firms under 15 employees the year preceding the judgment. The top panel is for all firms independently of their return on assets the year preceding the judgment, the middle panel for firms with return on assets below the median and the bottom panel for firms with return on assets above the median. Standard errors are clustered at the judge level. Vertical bars represent 95% confidence intervals. Source: DADS, FICUS-FARE, SIREN, Appeal court rulings database.

Figure 13 – Event study: permanent and temporary employment growth rates depending on the judge bias



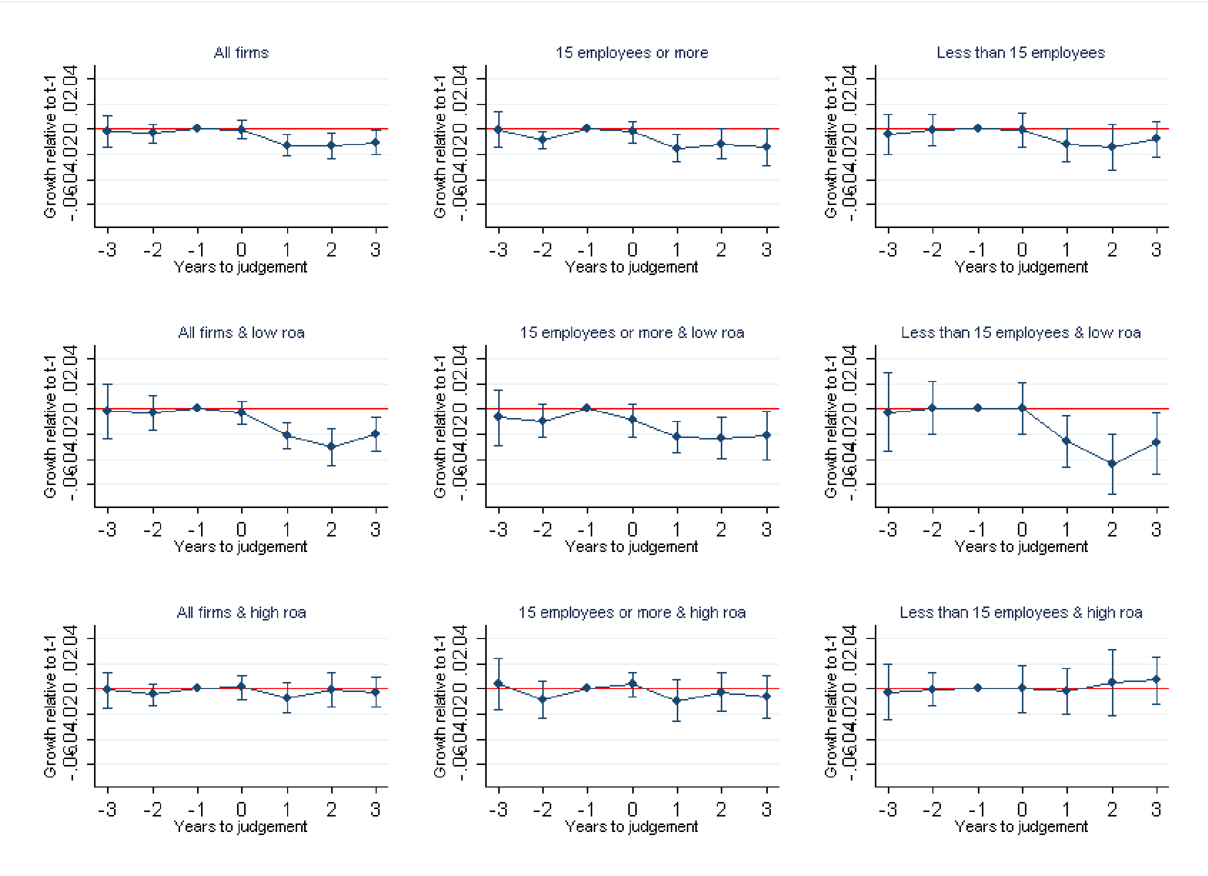
Note: This figure displays the coefficients α_{1k} of equation(4) in year k,$k \in [-3, 3]$ relative to the judgment year t where the dependent variables are the symmetric growth rates of permanent and temporary employment relative to the year preceding the judgment year t and the explanatory variable is the leave-one-out judge bias, $bias_{ij}$, controlling for social chamber fixed effects interacted with calendar year fixed effects, an indicator variable for economic dismissals, the age of the firm, the return on assets, the leverage and the capex the year preceding the judgment. The left panel reports the results for all firms, the middle panel panel for firms with 15 employees or more and the right panel for firms under 15 employees the year preceding the judgment. The top panel is for all firms independently of their return on assets the year preceding the judgment, the middle panel for firms with return on assets below the median and the bottom panel for firms with return on assets above the median. Standard errors are clustered at the judge level. Vertical bars represent 95% confidence intervals. Source: DADS, FICUS-FARE, SIREN, Appeal court rulings database.

Figure 14 – Firm survival rate depending on the judge bias



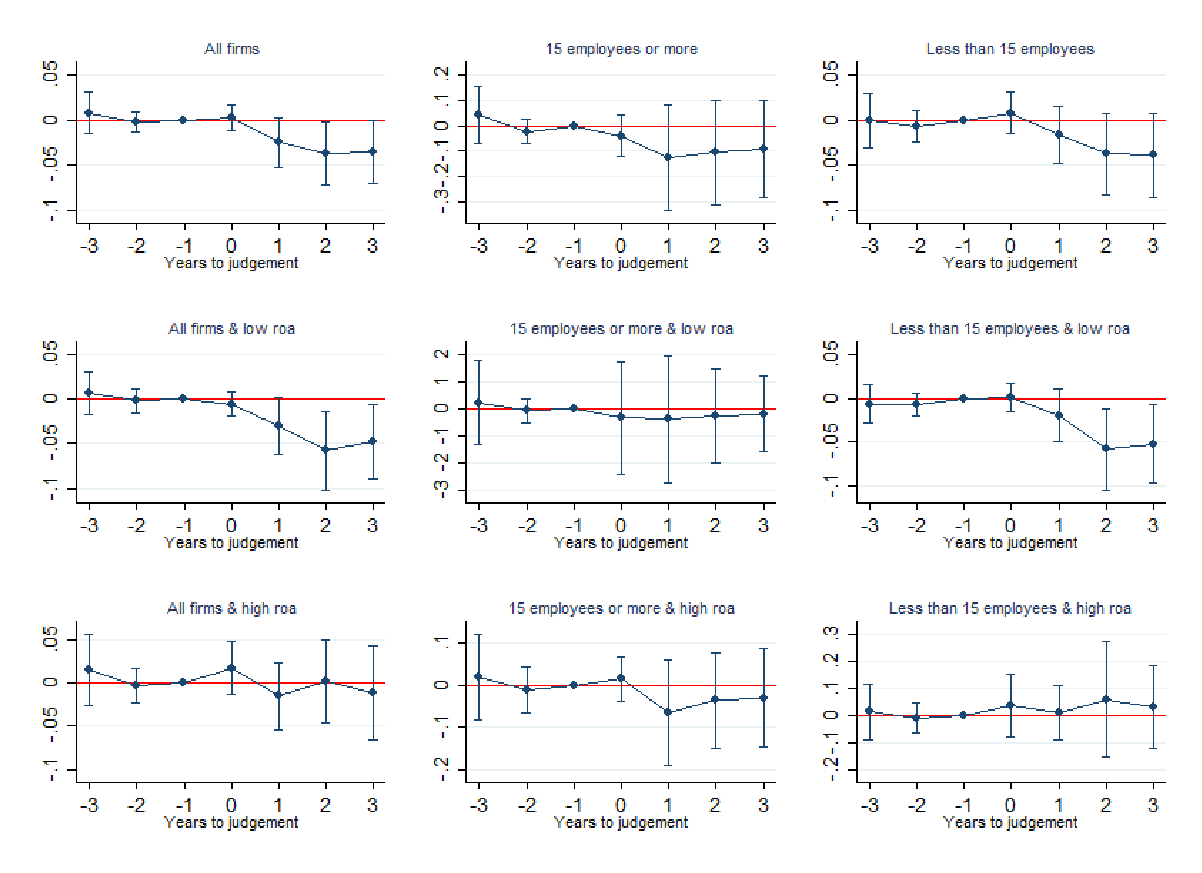
Note: This figure displays coefficients α_{1k} of equation (4) in year k,$k \in [-3, 3]$ relative to the judgment year t where the dependent variable is an indicator variable for firm survival and the explanatory variable is the leave-one-out judge bias, $bias_{ij}$, controlling for social chamber fixed effects interacted with calendar year fixed effects, an indicator variable for economic dismissals, the age of the firm, the return on assets, the leverage and the capex the year preceding the judgment. The left panel reports the results for all firms, the middle panel panel for firms with 15 employees or more and the right panel for firms under 15 employees the year preceding the judgment. The top panel is for all firms independently of their return on assets the year preceding the judgment, the middle panel for firms with return on assets below the median and the bottom panel for firms with return on assets above the median. Standard errors are clustered at the judge level. Vertical bars represent 95% confidence intervals. Source: DADS, FICUS-FARE, SIREN, Appeal court rulings database.

Figure 15 – Event study: employment growth rate depending on the judge bias conditional on firm survival



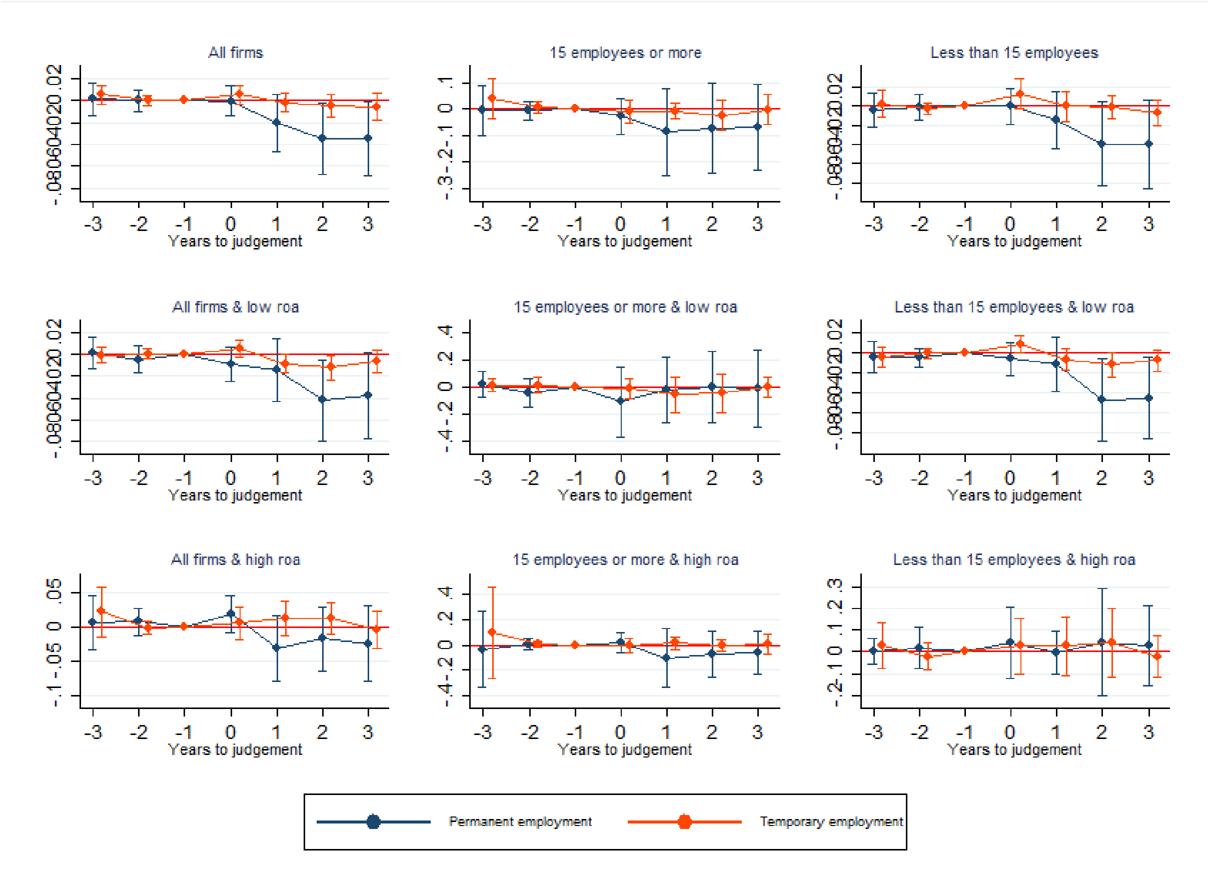
Note: This figure displays the coefficients α_{1k} of equation (4) in year k,$k \in [-3, 3]$ relative to the judgment year t where the dependent variable is the symmetric employment growth rates relative to the year preceding the judgment year t for firms which survive at least until three years after the judgment, and the explanatory variable is the leave-one-out judge bias, $bias_{ij}$, controlling for social chamber fixed effects interacted with calendar year fixed effects, an indicator variable for economic dismissals, the age of the firm, the return on assets, the leverage and the capex the year preceding the judgment. The left panel reports the results for all firms, the middle panel panel for firms with 15 employees or more and the right panel for firms under 15 employees the year preceding the judgment. The top panel is for all firms independently of their return on assets the year preceding the judgment, the middle panel for firms with return on assets below the median and the bottom panel for firms with return on assets above the median. Standard errors are clustered at the judge level. Vertical bars represent 95% confidence intervals. Source: DADS, FICUS-FARE, SIREN, Appeal court rulings database.

Figure 16 – Event study: employment growth rate depending on the amount of compensation in the previous year payroll instrumented by judge bias



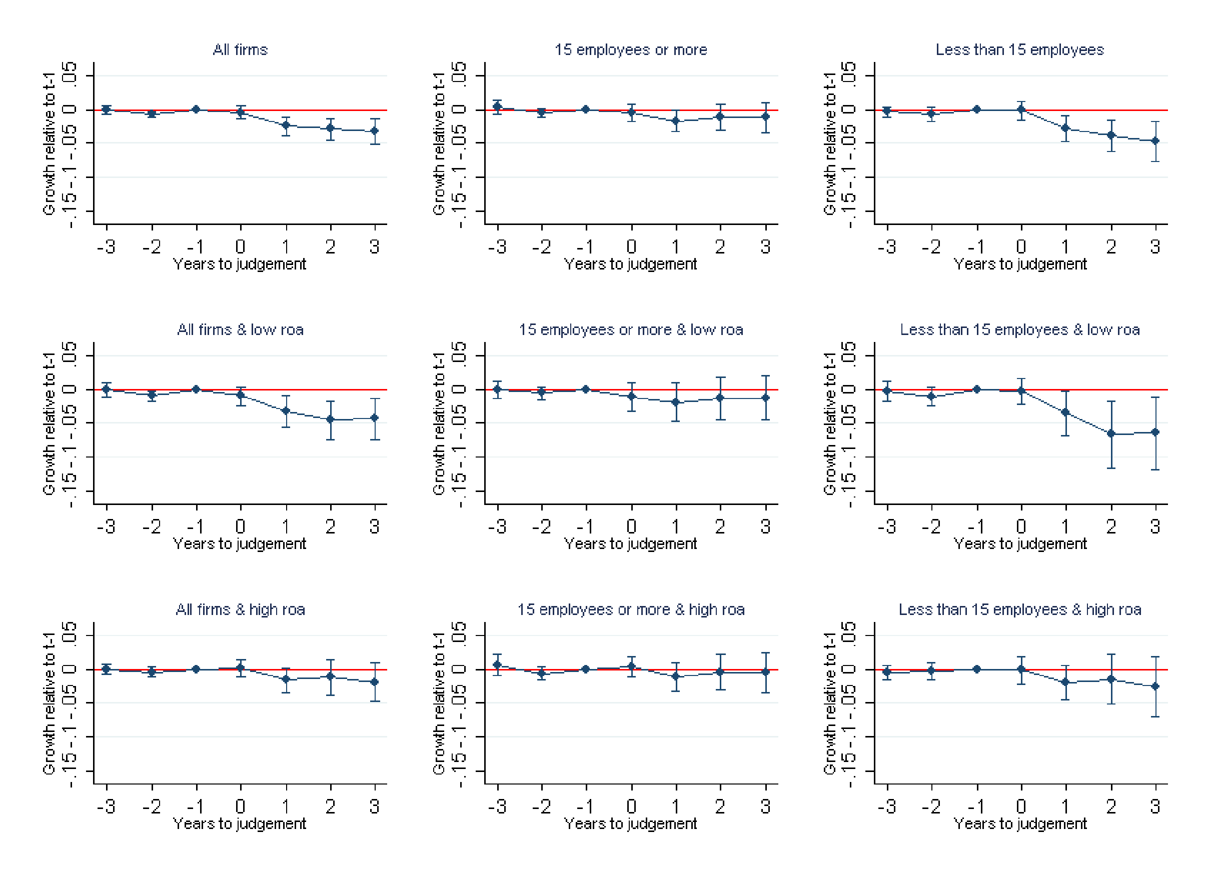
Note: This figure displays the coefficients β_{1k} of equation (5) in year k, $k \in [-3, 3]$ relative to the judgment year t where the dependent variable is the symmetric employment growth rates relative to the year preceding the judgment year t and the explanatory variable is the total amount of compensation in the previous year payroll instrumented by leave-one-out judge bias, $bias_{ij}$, controlling for social chamber fixed effects interacted with calendar year fixed effects, an indicator variable for economic dismissals, the age of the firm, the return on assets, the leverage and the capex the year preceding the judgment. The left panel reports the results for all firms, the middle panel panel for firms with 15 employees or more and the right panel for firms under 15 employees the year preceding the judgment. The top panel is for all firms independently of their return on assets the year preceding the judgment, the middle panel for firms with return on assets below the median and the bottom panel for firms with return on assets above the median. The number of observations is smaller than for the reduced form estimations because there are missing observations for the payroll in the year preceding the judgment and the data has been trimmed to eliminate the observations with the top 5% share of total compensation for wrongful dismissal in the payroll of the year preceding the judgment. Standard errors are clustered at the judge level. Vertical bars represent 95% confidence intervals. Source: DADS, FICUS-FARE, SIREN, Appeal court rulings database.

Figure 17 – Event study: permanent and temporary employment growth rates depending on the amount of compensation in the previous year payroll instrumented by judge bias



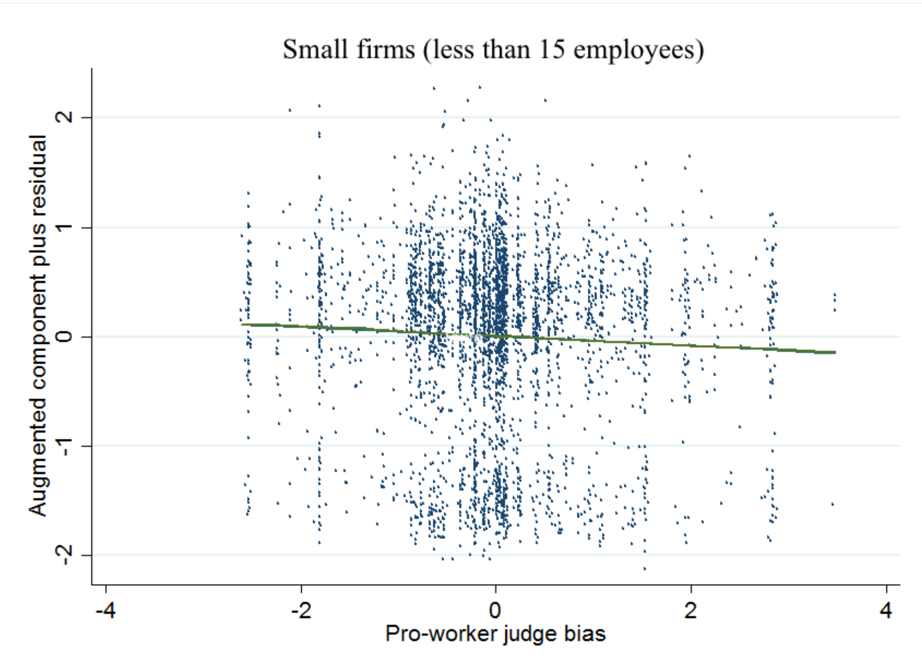
Note: This figure displays the coefficients β_{1k} of equation (5) in year k,$k \in [-3, 3]$ relative to the judgment year t where the dependent variables are the symmetric growth rates of permanent and temporary employment relative to the year preceding the judgment year t and the explanatory variable is the total amount of compensation in the previous year payroll instrumented by leave-one-out judge bias, $bias_{ij}$, controlling for social chamber fixed effects interacted with calendar year fixed effects, an indicator variable for economic dismissals, the age of the firm, the return on assets, the leverage and the capex the year preceding the judgment. The left panel reports the results for all firms and the right panel for firms under 15 employees the year preceding the judgment. The top panel is for all firms independently of their return on assets the year preceding the judgment, the middle panel for firms with return on assets below the median and the bottom panel for firms with return on assets above the median. Standard errors are clustered at the judge level. Vertical bars represent 95% confidence intervals. Source: DADS, FICUS-FARE, SIREN, Appeal court rulings database.

Figure 18 – Event study: employment growth rate depending on the judge bias conditional on firm fixed-effects



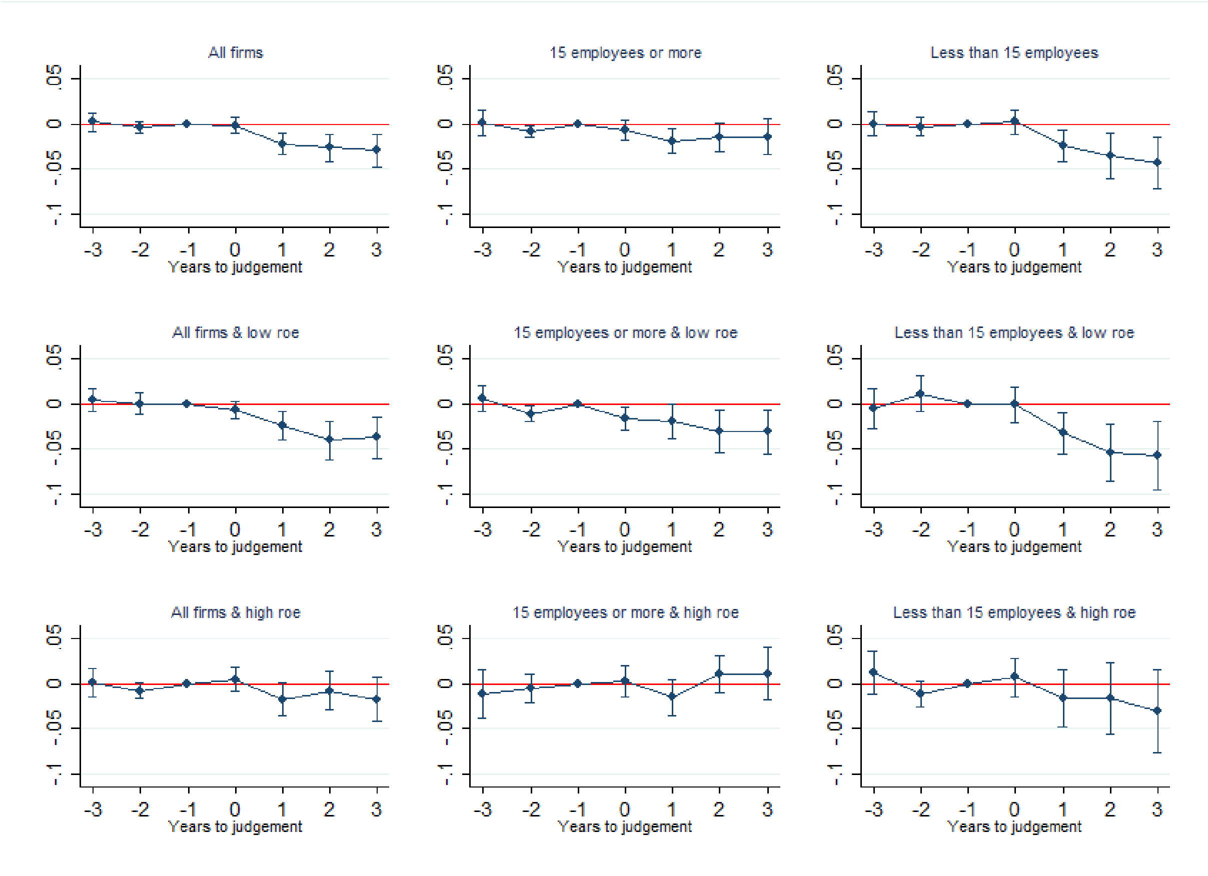
Note: This figure displays the coefficients α_{1k} of equation (4) in year k,$k \in [-3, 3]$ relative to the judgment year t where the dependent variable is the symmetric employment growth rates relative to the year preceding the judgment year t minus the firm fixed effect estimated before year $t-1$ as explained in Section 6.1. The explanatory variable is the leave-one-out judge bias, $bias_{ij}$, controlling for social chamber fixed effects interacted with calendar year fixed effects, firm fixed-effects and an indicator variable for economic dismissals. The left panel reports the results for all firms, the middle panel panel for firms with 15 employees or more and the right panel for firms under 15 employees the year preceding the judgment. The top panel is for all firms independently of their return on assets the year preceding the judgment, the middle panel for firms with return on assets below the median and the bottom panel for firms with return on assets above the median. Standard errors are clustered at the judge level. Vertical bars represent 95% confidence intervals. Source: DADS, FICUS-FARE, SIREN, Appeal court rulings database.

Figure 19 – Augmented component-plus-residual plot



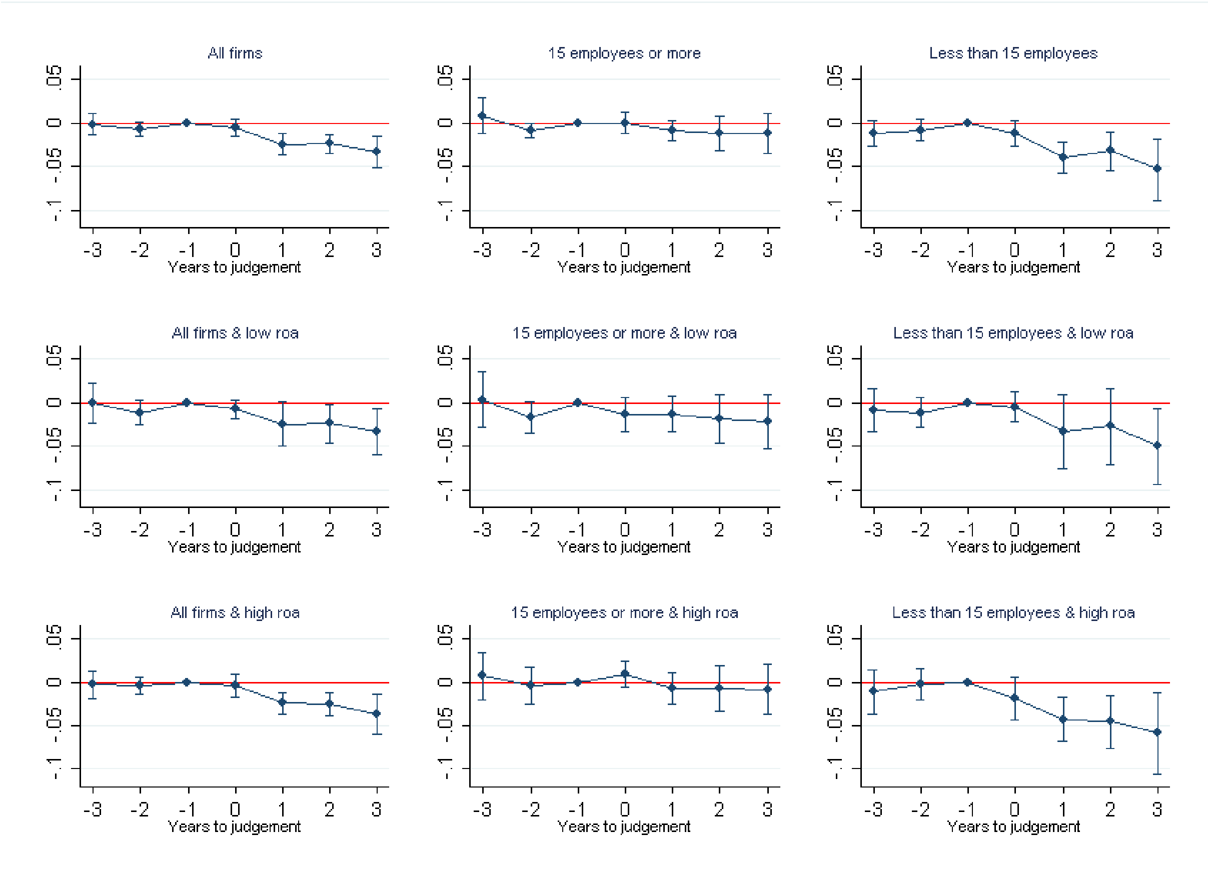
Note: This figure is an augmented component-plus-residual plot of the reduced-form estimation of the correlation between the leave-one-out judge bias and the employment growth of firms with fewer than 15 employees and whose return on assets is below the median at 3-year horizon. The non-linear line is a lowess smooth of the plotted points. Source: DADS, FICUS-FARE, SIREN, Appeal court rulings database.

Figure 20 – Event study: employment growth rate depending on the judge bias, firms split by return on equity



Note: This figure displays coefficients α_{1k} of equation (4) in year k,$k \in [-3,3]$ relative to the judgment year t where the dependent variable is the symmetric employment growth rates relative to the year preceding the judgment year t and the explanatory variable is the leave-one-out judge bias, $bias_{ij}$, controlling for social chamber fixed effects interacted with calendar year fixed effects, an indicator variable for economic dismissals, the age of the firm, the return on assets, the leverage and the capex the year preceding the judgment. The left panel reports the results for all firms, the middle panel panel for firms with 15 employees or more and the right panel for firms under 15 employees the year preceding the judgment. The top panel is for all firms independently of their return on assets the year preceding the judgment, the middle panel for firms with return on assets below the median and the bottom panel for firms with return on assets above the median. Standard errors are clustered at the judge level. Vertical bars represent 95% confidence intervals. Source: DADS, FICUS-FARE, SIREN, Appeal court rulings database.

Figure 21 – Event study: employment growth rate depending on the judge bias, sample restricted to large Appeal courts



Note: This figure displays, for the sample of firms judged in large Appeal courts comprising several social chambers, the coefficients α_{1k} of equation (4) in year k,$k \in [-3, 3]$ relative to the judgment year t where the dependent variable is the symmetric employment growth rates relative to the year preceding the judgment year t and the explanatory variable is the leave-one-out judge bias, $bias_{ij}$, controlling for social chamber fixed effects interacted with calendar year fixed effects, an indicator variable for economic dismissals, the age of the firm, the return on assets, the leverage and the capex the year preceding the judgment. The left panel reports the results for all firms, the middle panel panel for firms with 15 employees or more and the right panel for firms under 15 employees the year preceding the judgment. The top panel is for all firms independently of their return on assets the year preceding the judgment, the middle panel for firms with return on assets below the median and the bottom panel for firms with return on assets above the median. Standard errors are clustered at the judge level. Vertical bars represent 95% confidence intervals. Source: DADS, FICUS-FARE, SIREN, Appeal court rulings database.

A Caps on dismissal compensation in European countries

A majority of European countries have set rules that limit the amounts granted by judges in case of unfair dismissal (excluding cases of discrimination or harassment):

- In **Italy**, a fixed amount compensating an unfair dismissal was introduced in 2014 by the so-called (***Jobs Act***) for the new indefinite-duration contract with progressive employment protection, which depends on seniority: from 4 months for less than 2 years of seniority to 24 months for 12 years of seniority. From these amounts one must deduce the compensation received at the time of dismissal. In 2018 the Italian Constitutional Court overruled this regulation, stating that the amount of compensation to the worker cannot be based only on her seniority.

- In **Germany** the schedule depends on seniority and reaches 12 months of salary (and even 15 months if the worker is more than 50 years old with more than 15 years of seniority, and 18 months if more than 55 years old with more than 20 years of service).

- In **Austria**, the schedule depends on seniority: for those with less than 2 years the amount is 6 weeks of salary; between 2 and 5 years it is 2 months; between 5 and 15 years, 3 months; between 15 and 25 months, 4 months; beyond that: 5 months of salary.

- In **Belgium**, the minimum compensation is 3 weeks and the maximum 17 weeks of salary.

- In **Denmark**, worker compensation is capped at 1 year of salary for blue-collar; for white-collar workers, compensation goes up to half of the wages received during the notice period, capped at 3 months for those under 30, at 4 months if more than 10 years of service and 6 months if they have more than 15 years of service.

- In **Spain**, the indemnity is set at 33 days per year of seniority with a maximum of 24 months of salary, for contracts signed since the 2012 labor market reform.

- In **Finland**, the allowance is between 3 (minimum) and 24 (maximum) months of salary, depending on several factors including seniority, the age of the employee, the length of unemployment period, or the loss of income.

- In the **Netherlands**, the schedule depends above all on the age of the employee (1/2 month of salary per year of seniority up to 35 years old, 1 month per year of seniority between 35 and 45 years old, 1.5 month per year of seniority between 45 and 55 years old, 2 months per year of seniority beyond 55), to which a correction factor can be added depending on the exact situation. From these amounts one must deduce the compensation received at the time of dismissal.

- In **Portugal**, the court may grant between 15 (minimum) and 45 (maximum) days of salary per year of seniority with a minimum of 3 months.

- In the **United Kingdom**, for employees with more than two years of seniority the allowance consists of two components (i) a basic allowance which depends on seniority and capped at £ 14,250 and (ii) a compensatory allowance capped at one year of salary and limited to £ 78,335.

- In **Sweden**, the allowance is 16 months of salary for employees with less than 5 years of seniority, 24 months between 5 and 10 years, and 32 months for more than 10 years.

- In **France** since 2017 (***Ordonnances***), compensation for unfair dismissal is capped by an amount that depends on seniority varying from 1 month to 20 months for employees with 30 year or more of tenure, and cannot be less that 3 months of salary for employees with at least 2 years of seniority (at least 11 years for those working in firms with fewer than 11 employees).

B Judge mobility and judge ranking

To illustrate the relation between the mobility of judges and their ranking according to their bias, suppose a simple situation with one period only and four judges, A, B, C, D, ranked from the least to the most (unknown) pro-worker bias. Suppose that A and D belong to the same social chamber and that C and B belong to another social chamber during the whole period. Our measure of the bias relies on the difference in the share of dismissals deemed wrongful by different judges belonging to the same social chamber with respect to the average share of dismissals deemed wrongful in that social chamber. It allows us to conclude that D is more pro-worker than A and that C is more pro-worker than B. But it yields information neither about the comparison of B and A nor about the comparison of D and C because the average share of dismissals deemed wrongful in the social chamber is different, and depends, among other factors, on the true bias of judges allocated to the social chamber. Depending on the selection of judges in social chambers according to their bias, we may conclude that the ranking is (by increasing order of pro-worker bias) B, A, C, D, or B, C, A, D or A, D, B, C instead of the true ranking A, B, C, D. In our approach, this problem is mitigated insofar as judges are mobile across social chambers. In the previous example, A might, during the period of interest, share the same social chamber as both D and B, which may enable us to rank A,B and A,D. Such judge mobility thus may help us to exclude the erroneous rankings B,A,C,D and B,C,A,D. Hence, the higher the degree of judge mobility, the higher the probability to achieve a perfect ranking.

C Judge bias with respect to qualification of dismissals

We construct here an alternative measure of judge bias. Namely, we build a judge-specific pro-worker bias with respect to the dismissal qualification. First, Figure C.1 displays the histogram of the frequency at which the judge grants a positive compensation. Figure C.2 presents the histogram of the judges' pro-worker bias among the population of cases defined by equation (2) where y_{ij} is an indicator equal to one when the dismissal of case i judged by judge j is deemed wrongful.

Relation between judge bias and the qualification of dismissals

Our measure of judge bias is relevant only if it is significantly correlated with the qualification of dismissal in each specific case. To check whether our measure of judge bias is indeed related to the actual qualification of dismissals, Figure C.2 displays the local polynomial fit of the probability that dismissals are deemed wrongful explained by the judge pro-worker bias. The judge pro-worker bias is indeed positively related to the probability that dismissals are deemed wrongful. Being assigned to one of the 10% most pro-worker judges as compared to one of the 10% least pro-worker judges increases the probability that the dismissal is deemed wrongful by about 4 percentage points, which corresponds to an increase of 7% in the probability that the dismissal is deemed wrongful.

Table C.1 further documents the relation between the qualification of dismissals and judge pro-worker bias. This table displays the OLS estimator of the regression of the indicator variable equal to one if the dismissal is deemed wrongful on the judge's pro-worker bias. All standard errors are clustered at the judge level. Column (1) includes Appeal court and year fixed effects. Column (2) adds control variables comprising the worker's salary, seniority and whether the dismissal is economic or for personal reasons. The coefficients, which are significant at 5% level of confidence, are consistent with those obtained from the polynomial fit without any control, displayed on Figure C.2. Indeed, according to Table C.1, being assigned to one of the 10% most pro-worker judges as compared to one of the 10% least pro-worker judges increases the probability that the dismissal is deemed wrongful by 4.1 percentage points[31] which is very close to the prediction of the polynomial fit.

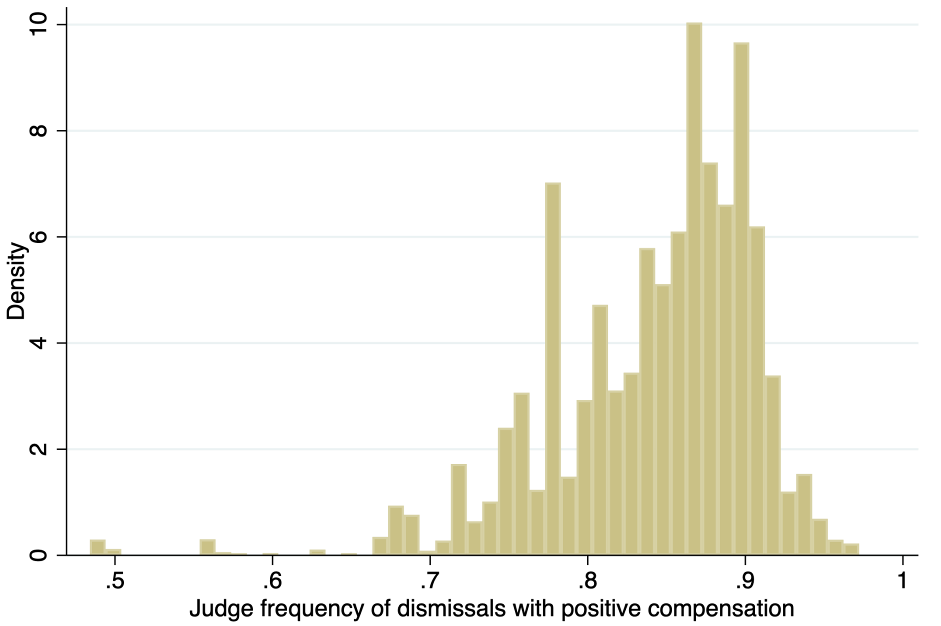
Relation between judge bias with respect to dismissal qualification and with respect to compensation for wrongful dismissal

Judges who often qualify the dismissal as wrongful are also those who, conditional on granting a positive compensation, grant the highest compensations. In other words, our two indices of pro-worker bias are highly and positively correlated. We display this correlation in Figure C.3, which presents the scatter plot of the pro-worker bias with respect to the compensation granted, conditional on being positive,[32] and the pro-worker bias with respect to the dismissal qualification.

[31] The computation is performed as follows: we multiply the point estimate given in column (3) of Table C.1 by the difference of pro-worker bias when going from the 1st to the 9th decile of the pro-worker bias, respectively equal to -0.46 and 0.36.

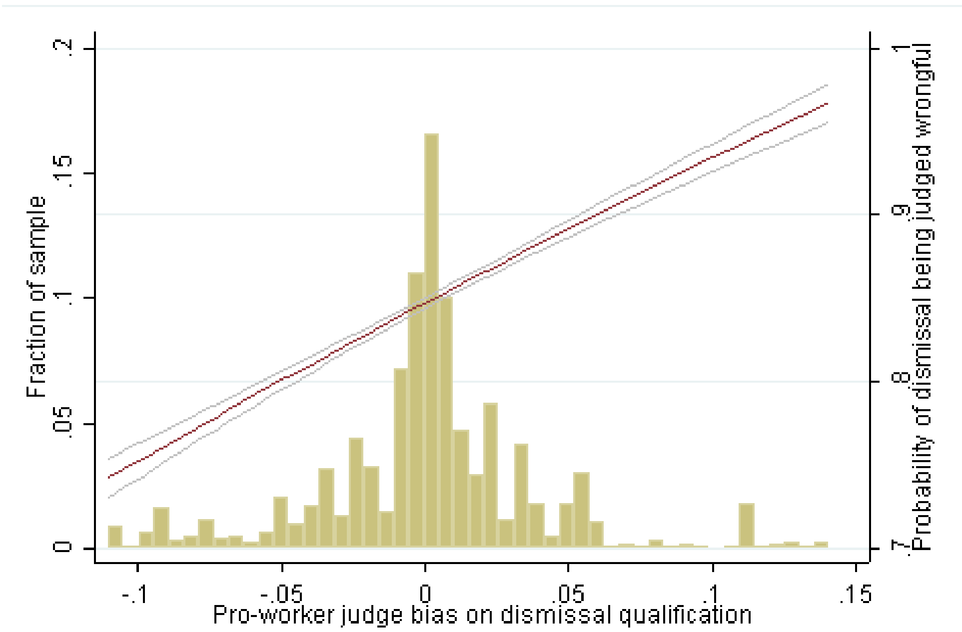
[32] Note that Figure 10 reports judges biases for the average compensation unconditional on being positive.

Figure C.1 – Histogram of frequency of dismissals deemed unfair per judge



Note: This Figure exhibits the histogram of frequency of dismissals deemed unfair per judge. Case-level data are used, therefore the number of observations used is the number of different cases for which we are able to compute the pro-worker bias. Source: Appeal court rulings database.

Figure C.2 – Judge pro-worker bias with respect to the dismissal qualification



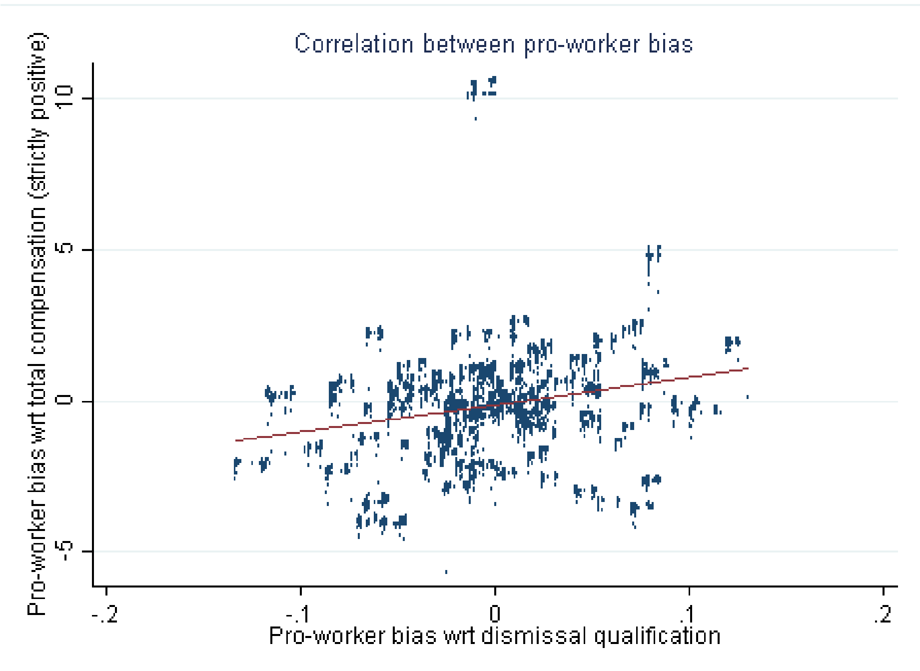
Note: This figure displays the histogram of pro-worker biases of judges with respect to the qualification of dismissals in background and a local polynomial fit of the indicator variable equal to one if the dismissal is deemed wrongful, represented by the red line. The grey lines display the frontiers of the 95% confidence interval of the local polynomial fit. Case-level data are used, therefore the number of observations is the number of different cases for which we are able to compute the pro-worker bias reported in Table 2. The pro-worker bias is computed as defined in Section 4.2. Source: Appeal court rulings database.

Table C.1 – Correlation between judge bias and dismissal qualification

	Dismissal qualification	Dismissal qualification
	(1)	(2)
Judge pro-worker bias	0.4022 **	0.408**
wrt dismissal qualification	(0.152)	(0.142)
Year FE	Yes	Yes
Court FE	Yes	Yes
Case controls	No	Yes
F test	7.66	8.26
# obs	8,354	8,354

Note: Each column corresponds to one regression. The dependent variable is an indicator variable equal to one if the dismissal is deemed wrongful. Court and year fixed effects are included. Control variables included in column (2): indicator variable for economic dismissal, worker's wage, worker's seniority. Standard errors, displayed in parentheses, are clustered at the judge level. *, **, and *** denote statistical significance at 10, 5 and 1%. Source: Appeal court rulings database.

Figure C.3 – Correlation between the two indices of pro-worker biases



Note: This figure is a scatter plot of the pro-worker bias measure computed from the dismissal qualification and the pro-worker bias computed from the compensation amount, conditional on being positive. Pro-worker biases are computed as defined in Section 4.2. Source: Appeal court rulings database.

D Construction of sample of firms

To get the sample of firms, we start from the sample of cases used to compute the judge fixed effects described in Tables 1 and 2. There are 159 judges who judged cases for which information about the variables required to compute judge fixed effects is non-missing. As reported by the first seven rows of Table 2, those 159 judges judged 30,717 cases for which variables required to compute the judge fixed effects are available. This figure is reported in the first Row of Table 6 which presents the sample of firms. The 159 judges judged 101,010 cases (Row b of Table 6) in total. For some cases, variables about the case are missing, which impedes to compute the leave-one-out judge fixed effect. We attribute to such cases the average of their judge fixed effect. Among the 101,010 cases present in Row b of Table 6, there are 65,623 cases for which an identifier of a for-profit private firm of the non-agricultural sector has been found (Row c of Table 6). 43,882 are matched in the DADS and FARE registers involving 25,833 firms (Row d). There are 18,046 firms with only one case judged by Appeal courts over the period covered by our study (Row e). Keeping cases judged before 2013 (Row f) with non-missing information for the use of the event study (such as the age of the firm, employment and wages, row g), and after trimming the top and bottom centiles of judges' bias distribution, yields 9,035 cases and firms (row h). Finally, restriction to firms with non-missing financial variables, such as return on asset and leverage, yields 7,329 cases (or firms) and 133 judges (Row i) used in the event study with controls.

E Extraction of compensation amounts and other variables of Appeal court rulings

This section provides additional details on the construction of our novel database of anonymized Appeal court rulings. We use the universe of Appeal court ruling over ten years. The latter are available and digitized on a systematic basis, contrary, to first instance rulings, which are collected locally at the court level and are not compiled in a common legal database. We use Natural Language Techniques (NLP) to extract the information from the Appeal court rulings. Each of these rulings is a few pages long, with some spreading over a dozen pages. Extracting information accurately from textual documents that contain many digressions and qualitative arguments is not a straightforward exercise. In order to reduce the complexity of the problem, we exploit the structure of these legal documents, which follow a well-established template.

Structure and recognizable information within rulings

Each ruling can naturally be divided into roughly five blocks as follows i) a brief header with the case number, the date of the audience, identities of the parties, etc.; ii) a description of the history of the contractual relationship between the employee and the employer with the parties' claims iii) a restatement of the decision appealed; iv) the main arguments behind the rulings containing the reassessment by the Appeal Court of factual elements and the legal groundings of the first-instance decision; and v) the conclusion ruling whether the dismissal is deemed wrongful, and assigning monetary awards, if any. We split these main blocks by tagging the text with specific legal keywords used to mark the boundaries of the different sections. For instance, the conclusion is generally introduced by the expression "Par ces motifs" (For these reasons) or variants thereof.

We then extract the information from each block and generate up to several hundred variables for any given text. This is because there is a wide array of potential damages that can be sought by the parties and/or awarded. Besides compensation for wrongful dismissal (*indemnité pour licenciement sans cause réelle et sérieuse*), the following compensations may also be awarded by Appeal court judges: compensation for non-respect of the dismissal procedure; compensation for unpaid wages (*indemnité pour rappel de salaire*); compensation for moral and financial damages (*indemnité pour préjudice moral et financier*); compensation in lieu of notice period (*indemnité compensatrice de préavis*) when the notice period was not respected; compensation under article 700 of the French Code of Civil Procedure, which covers the legal costs of the wining party; compensation for unpaid annual leave (*indemnité compensatrice de congés payés*) ; allowance for overtime hours (*heures supplémentaires*). An employee may receive these different compensations concurrently.

It is important to track compensations along all these dimensions because the amounts granted by judges under these various motives are not fully independent, even though in principle the legal bases for granting them are distinct. In other words, it is possible that in a judge's assessment of the case the amounts become correlated. To detect substitution between the different types of monetary awards, we keep track of all of them using initially more than twenty categories before aggregating them. Because of the length of legal proceedings, some amounts, still expressed in French francs before the adoption of the Euro in 2001, also need be appropriately converted.

It turns out that judges often award these other types of compensation. They are awarded alongside compensations for wrongful dismissal to workers, but not only that, as rightful dismissal can also be marred by procedural irregularities. In total, out of 145,000 cases in our original sample of court decisions, a positive amount is awarded to workers in 60% of the cases, whatever the motive. Out of these cases receiving a positive amount, the dismissal is deemed unfair 61% of the time. But workers also receive compensation for other reasons, such as paid leave (47% of cases), advance notice (40%), salaries (13%) or overtime hours (7%) when these amounts were due but had not been fully paid by the employer prior to the dismissal. More rarely do judges award compensation for moral damage (2%), harassment (2%) or discrimination (0.3%). One or several of these other types of compensation are awarded in 93% of the cases with a positive amount paid to the worker at the end of the trial.

The data include a wide array of variables related to the case (compensations for wrongful dismissals, worker seniority, wage, Appeal court, city of the *Prud'hommes* council, whether it was the worker who appealed, etc.), as well as the firm's name and address. Using the firm's name and address we are able to retrieve the firm identifier (*SIREN*), and then link the compensation dataset to matched employer-employee data as well as financial variables. The stages for the construction of this dataset are the following.

Extracting wages and tenure requires paying close attention to the wording of rulings as there is substantial heterogeneity in how they are reported. For instance tenure information is sometimes not explicitly stated as a duration but can to be recovered from the mentions of when the employee was hired. We therefore use multiple approaches to revover the information. Recovering wages is crucial in order to express the compensation in terms of months of salary. Again, we target a large number of keywords to detect mentions of annual, monthly, weekly, or even hourly wages. Despite our best efforts, for some court rulings the information could not be fully extracted, thus creating missing observations.

Variable selection and sample attrition

Heterogeneity in the writing of the rulings across jurisdictions and over time means that an automatic extraction can generate mistakes and approximations. Therefore we conducted a series of manual checks on a subsample of 2,560 observations, selected at random. The manual dataset creation was undertaken as part of a project of Pierre Cahuc and Stéphane Carcillo, and funded by the *Chaire sécurisation des parcours professionnels*. To examine the Appeal court rulings published by the Minister of Justice, ten research assistants were hired, each of them being in charge of a given year. These assistants carried out the research with the following key words: '*licenciement sans cause réelle et sérieuse*' (unfair dismissal) and '*indemnités*' (compensation). Even though the research assistants were asked to select randomly Appeal court rulings within the year, some of them selected only rulings from particular months: the assistants in charge of studying the 2009, 2010 and 2012 years mostly selected court rulings of September and October, and marginally court rulings from November and December. We find that the correlation between the compensation amount of the manually-filled and the automatically-filled datasets is equal to 94%, which is in the upper range of seminal papers using this type of approach (Baker et al. (2016)).

F Estimates of event-studies

Table F.1 – Event study: employment growth rate depending on the judge bias (with controls)

	(1)	(2)	(3)	(4)	(5)	(6)
	t-3	t-2	t	t+1	t+2	t+3
All firms						
$bias_{ij}$	0.002	-0.004	-0.001	-0.022***	-0.026***	-0.030**
	(0.005)	(0.003)	(0.004)	(0.006)	(0.008)	(0.009)
N	7329	7329	7329	7329	7329	7329
R^2	0.042	0.042	0.038	0.063	0.079	0.089
15 and more employees						
$bias_{ij}$	0.001	-0.008**	-0.007	-0.019**	-0.015*	-0.014
	(0.007)	(0.003)	(0.005)	(0.007)	(0.008)	(0.010)
N	3638	3638	3638	3638	3638	3638
R^2	0.076	0.066	0.062	0.087	0.115	0.116
1-14 employees						
$bias_{ij}$	0.000	-0.003	0.002	-0.024**	-0.036**	-0.043**
	(0.007)	(0.005)	(0.007)	(0.009)	(0.013)	(0.015)
N	3677	3677	3677	3677	3677	3677
R^2	0.072	0.065	0.072	0.089	0.094	0.104
All firms with low ROA						
$bias_{ij}$	0.004	-0.005	-0.005	-0.028**	-0.041**	-0.039**
	(0.009)	(0.005)	(0.005)	(0.011)	(0.014)	(0.014)
N	3673	3673	3673	3673	3673	3673
R^2	0.068	0.068	0.063	0.082	0.095	0.107
15 employees and more & low ROA						
$bias_{ij}$	-0.005	-0.009	-0.015*	-0.023*	-0.018	-0.017
	(0.011)	(0.006)	(0.008)	(0.012)	(0.014)	(0.014)
N	1815	1815	1815	1815	1815	1815
R^2	0.126	0.107	0.124	0.138	0.170	0.161
1-14 employees & low ROA						
$bias_{ij}$	0.003	-0.004	0.003	-0.029*	-0.061**	-0.059**
	(0.012)	(0.007)	(0.009)	(0.018)	(0.026)	(0.027)
N	1849	1849	1849	1849	1849	1849
R^2	0.112	0.129	0.107	0.123	0.122	0.139
All firms with high ROA						
$bias_{ij}$	0.001	-0.004	0.002	-0.016**	-0.011	-0.018
	(0.007)	(0.004)	(0.005)	(0.007)	(0.011)	(0.013)
N	3638	3638	3638	3638	3638	3638
R^2	0.070	0.065	0.058	0.061	0.070	0.075
15 employees and more & high ROA						

Table F.1 – *Continued from previous page*

	(1)	(2)	(3)	(4)	(5)	(6)
	t-3	t-2	t	t+1	t+2	t+3
$bias_{ij}$	0.005	-0.008	0.002	-0.013*	-0.006	-0.007
	(0.010)	(0.007)	(0.005)	(0.007)	(0.009)	(0.012)
N	1808	1808	1808	1808	1808	1808
R^2	0.118	0.122	0.128	0.092	0.103	0.120
1-14 employees & high ROA						
$bias_{ij}$	-0.003	0.001	0.002	-0.017	-0.012	-0.023
	(0.011)	(0.007)	(0.009)	(0.012)	(0.019)	(0.021)
N	1813	1813	1813	1813	1813	1813
R^2	0.124	0.101	0.102	0.122	0.116	0.122

Note: This table reports the estimates displayed in Figure 12. This table displays the coefficients α_{1k} of equation (4) in year k, $k \in [-3, 3]$ relative to the judgment year t where the dependent variable is the symmetric employment growth rates relative to the year preceding the judgment year t and the explanatory variable is the leave-one-out judge bias, $bias_{ij}$, controlling for social chamber fixed effects interacted with calendar year fixed effects, an indicator variable for economic dismissals, the age of the firm, the return on assets, the leverage and the capex the year preceding the judgment. Standard errors are clustered at the judge level. Source: DADS, FICUS-FARE, SIREN, Appeal court rulings database.