

JUDGE BIAS IN LABOR COURTS AND FIRM PERFORMANCE

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Abstract

This paper documents the existence of judge-specific differences on granting compensation for wrongful dismissal and shows that their consequences are different for small low-performing firms than for other firms. Pro-worker judge bias reduces job creation for all firms, increases the destruction of permanent jobs in small and low-performing firms but reduces it in large high-performing firms. Pro-worker bias reduces employment and survival for small and low-performing firms but has no significant effects on these outcomes for the other firms. The probability that permanent incumbent workers keep their job in firms judged by a pro-worker judge increases in large and high-performing firms, while it decreases in small, poorly performing firms. (JEL: J33, J63, J65)

1. Introduction

The fear of a differentiated treatment and judges' alleged pro-worker biases are frequent worries of businesses heading to labor court. Many advanced economies have therefore enacted reforms that restrict judge latitude in awarding compensations, with the objective of guaranteeing the equal application of the law and guarding

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businesses against dramatic outcomes. Italian Prime Minister Matteo Renzi's flagship labor reform, the 2014–2015 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 in rigorous quantitative analysis, partly for lack of appropriate data. Therefore, there is no reliable empirical evidence that the subjectivity of judges in labor courts exists and 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% of the average compensation.

To analyze the consequences of the randomness of dismissal compensations, a feature which, to our knowledge, has been overlooked by the literature so far, we develop a theoretical model which distinguishes three channels through which dismissal costs shocks can operate: expectations of future dismissal costs, credit constraints, and the probability of bankruptcy. While positive dismissal costs shocks depress job *creation* in all firms, the impact on job *destruction* is ambiguous. This is because tighter credit constraints and a higher risk of bankruptcy increase job destruction, whereas the expectation of higher dismissal costs leads firms to avoid firing.

The impact on job destruction can vary depending on the financial situation and the size of firms. On the one hand, for large or high-performing firms who have grown out their credit constraints, the credit rationing channel is likely to be of second-order. As a result, shocks to dismissal costs operate primarily through the expectations channel.

1. In the U.S., employment protection is overseen by the National Labor Relation Board 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.

The model predicts fewer creation and destruction of permanent jobs, together with an increase in the share of temporary employment. The effect on total employment is therefore ambiguous. On the other hand, for small low-performing firms, both the credit channel and the expectations channel imply that positive shocks to layoff costs reduce survival and job creation, while increasing job destruction, which negatively impacts employment, and permanent jobs in particular.

To test these predictions, we draw on administrative firm-level records covering the whole universe of French firms. We focus on individual dismissals, when a single employee is dismissed at a time.⁴ We find that the judge pro-worker bias⁵ reduces job creation for all firms, increases the destruction of permanent jobs in small—less than 15 employees, which corresponds the median size in the sample—and low-performing—with returns on assets below the median—firms but reduces them in other firms. For small low-performing firms, an increase by one standard deviation in the pro-worker bias reduces employment growth by 6 percentage points and the survival rate by 2 percentage point three years after the judgment. There are no significant effects on employment and survival 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 shock on the dismissal cost by 1% of the payroll reduces the employment growth rate of small low-performing firms at a 3-year horizon by 5 percentage points. The overall employment impact of judge bias stems from permanent jobs, with no significant impact on temporary employment. The analysis of the trajectories of the employees present in the firm the year of the judgment shows that the decisions of pro-worker judges have different consequences for workers depending on the type of firm that employs them. In line with the prediction of the theoretical model, the probability of keeping their job in the judged firm increases for those working in large or high-performing firms while it decreases for those working in small, poorly performing firms. No significant effect on wages is detected, whatever the type of firm.

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. To identify the effects of judge-specific differences on compensations for wrongful dismissals, we leverage presidents’ rotations across courts. In particular, 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

4. Collective layoffs are a relatively rare event among the universe of layoffs handled by French labor courts, and even more so among those cases handled by Appeal courts.

5. By pro-worker bias, we mean that the judges are ranked in ascending order starting from the bias most favorable to firms and going toward the one which is most favorable to the worker.

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 that judges of different types are allocated sets of firms with similar characteristics. In particular, we compare total, permanent and temporary⁶ 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 our measure of judge bias is not associated with statistically different pre-judgment employment growth. In contrast, both total and permanent employment start diverging post-judgment, especially among small and low-performing firms. We also verify that the allocation of judges is unrelated to 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; Yang 2015; Dobbie, Goldin, and Yang 2018; Bhuller, Dahl, and Mogstad 2020), bankruptcies (Bernstein et al. 2018a; Bernstein, Colonnelli, and Iverson 2018b; Araujo et al. 2021), or decisions related to disability benefits (Maestas, Mullen, and Strand 2013; Dahl, Kostøl, and Mogstad 2014; French and Song 2014; Autor et al. 2015, 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, Polo, and Rettore (2003) and Jimeno, Martínez-Matute, and Mora-Sanguinetti (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.⁷

6. We count as permanent employment the employees hired on open-ended contracts or *Contrat a durée indéterminée* (CDI), as opposed to fixed-term contracts or *Contrat a durée déterminée*.

7. This is also consistent with Jimeno, Martínez-Matute, and Mora-Sanguinetti (2020)'s study of Spanish labor reforms of 2010 and 2012. Despite a broadening of the definition of fair economic dismissals, the

Second, our paper shows that the subjectivity of judges in terms of compensation for wrongful dismissal has significant but different consequences for small low-performing firms and other firms. A vast empirical literature analyzes the labor market impact of dismissal costs (see Cahuc, Carcillo, and Zylberberg 2014 for a survey) but causal evidence has mostly hinged on aggregate exogenous variations and the uncertainty of dismissal costs has been overlooked. In particular, studies of the effects of court decisions regarding unfair dismissals on firms' outcomes (Autor 2003; Autor, Donohue and Schwab 2006; Autor, Kerr, and Kugler 2007; Martins 2009; Fraisse, Kramarz, and Prost 2015; Bamieh 2016; Gianfreda and Vallanti 2017; Boeri and Garibaldi 2018) 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 these 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, Polo, and Rettore 2003; Berger and Neugart 2011; Martín-Roman, Moral, and Martínez-Matute 2015; Jimeno, Martínez-Matute, and Mora-Sanguinetti 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 and firm performance, an issue which has been overlooked by the literature so far although most countries have exemptions for small firms. We find statistically that pro-worker judges reduce job creation for all firms but has heterogeneous effects on job destruction: they increase the destruction of permanent jobs in small and low-performing firms but reduce it in other firms. From this perspective, our findings that cash-flow shocks induced by judge bias have heterogeneous impact on firms depending on their size and their performance adds to the results of the corporate finance literature that assesses the effects of cash flow and credit shocks on firms (Blanchard, Lopez-de Silanes, and Shleifer 1994; Rauh 2006; Chodorow-Reich 2013; Simintzi, Vig, and Volpin 2014; Giroud and Mueller 2017; Favilukis, Lin, and Zhao 2020). Eventually, our analysis of the trajectories of workers shows that the decisions of pro-worker judges have different consequences for workers depending on the type of firm that employs them.

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, and on the employment and wage trajectories of workers present in the firm the year of 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 the change in the legal definition of fair dismissals, suggesting that judges have significant margin for interpreting legal rules.

judgment. In Section 6, we conduct robustness and heterogeneity tests. Section 7 discusses the scope and limitation of our results.

2. Institutional Background

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

2.1. Legal Framework

Following the termination of an open-ended contract, employees with at least one year tenure are granted a minimum legal severance payment, as long as they did not commit any serious or gross misconduct (*faute grave* or *faute lourde*). The minimum severance payment amounts to one fifth of monthly salary per year of tenure, plus an additional two fifteenths per year 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 parity 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%.⁸ According to Desrieux and Espinosa (2019), among claims that reached the judicial stage at *Prud’hommes*

8. 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).

council from 1998 to 2012, 62% resulted in the acceptance of the employee's claims. Similarly Fraisse, Kramarz, and Prost (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.⁹ 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.¹⁰ 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 trials. She can nevertheless be replaced whenever needed, for instance during holidays. For each judgment, the chamber president is assisted by two councillor-judges.

2.2.1. Appointment of Judges. The independence of judges is a constitutional principle guaranteed by the Constitutional Council. The status of judges and their mobility is determined by the *Ordonnance Organique* of December 22, 1958. This regulation states that judges are irremovable (art. 64, al. 4, of the Constitution), that is to say that they cannot receive, without their consent, a new assignment, even for a promotion (art. 4, al. 2, *Ordonnance Organique* of December 22, 1958). Appeal courts judges are “placed judges”, that is, 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 an independent National Commission of Advancement composed of judges. 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 composed of judges. Besides, mobility requirements are enforced through several

9. All first and second instance judgments include a hearing of the parties and a written decision.

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

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 than ten years altogether, or geographical mobility requirements to achieve the first grade of the remuneration schedule (organic law 2001-539 of June 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).

2.2.2. Assignment of Cases to Judges. The assignment of cases to judges follows the principle of the “right to the natural judge”, which means that the allocation of cases to judges should not depend on the identity of the judge as this would lead to breaking the principle of equality between litigants (Jeuland 2008). In addition, according to the United Nations Charter, to which France adheres, the principles of judicial independence imply that “the distribution of cases to judges in the jurisdiction to which they belong is an internal matter which is a matter for the administration judicial.” In France, the application of the “right to the natural judge” principle in Appeal courts is implemented by the First President of the Appeal court under the control of the assembly of judges (articles R312-42 and R312-42-1 of the Judiciary Organization Code). In practice, these principles are applied as follows:

- Each Appeal court judges cases from a given geographical area.
- When there is a single social chamber, cases are dealt with in order of arrival. Judges cannot derogate from this principle. They must derogate only when they are in a situation of conflict of interest by referring to the President of the Appeal court, under the control of an assembly of judges.
- If there are several social chambers in the same Appeal court (i.e. Aix-en-Provence, Paris and Versailles, which have 4, 14, and 7 social chambers respectively), cases are distributed according to a geographical criterion and each social chamber deals with cases in order of arrival.

Insofar as the average processing time for a judgment is about two years, it is unlikely that a judge will ask to be transferred to an Appeal court to judge a particular case, especially since judges must report any potential conflict of interest.

All in all, the judicial system is organized to implement the principle of the “right to the natural judge” which implies that the allocation of cases to judges must be random i.e should not depend on the identity of judges. We will empirically document this property with randomization tests to verify whether the characteristics of firms and the evolution of our variables of interest before the judgment are indeed independent of whether they will be judged by a pro-worker or pro-employer judge.

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 B 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 B.1 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 seminal papers using this type of approach (Baker, Bloom, and Davis 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

11. See Appendix B for a more complete list of the dozens of possible additional compensations.

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.

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. We use this dataset 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

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.

(see Table 1).¹² 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.

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

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.

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.

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 favorable to workers than *Prud'hommes*.

It is apparent from the standard deviations reported in Table 2 that there is an important dispersion of the amount of compensation in both tribunals and that the dispersion of the compensations of Appeal courts is larger than that of *Prud'hommes*.¹⁴ Obviously, the variance of compensations originates from the diversity of situations specific to each case. Nevertheless, the subjective interpretation of judges might exert an important influence, to the extent that 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

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.

14. Online Appendix Figure J.1 displays the histogram of the compensation for wrongful dismissal in monthly wages in Appeal courts and Online Appendix Figure J.2 displays the scatter plot of the compensations for wrongful dismissals set by Appeal courts and by *prud'hommes*.

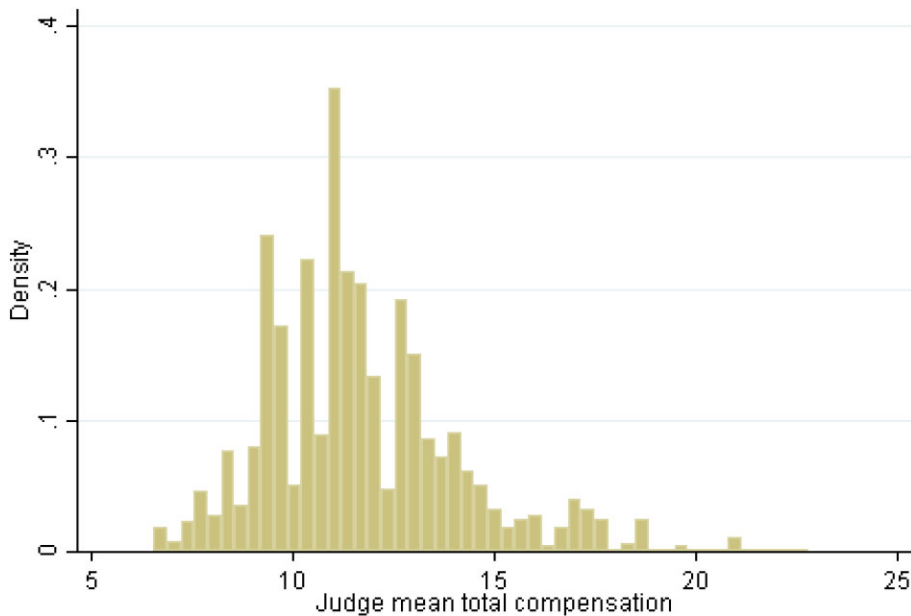


FIGURE 1. Histogram of mean compensation per judge. This figure exhibits the histogram of mean compensation in month of salary per judge. Case-level data are used, therefore the number of observations used is the number of different cases for which we are able to compute the pro-worker bias. Source: Appeal court rulings database.

covariates¹⁵ 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.

The 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 0.16 additional monthly wage granted for other motives.¹⁶ 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 1.¹⁷

15. That is, 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.

16. Regressing the amounts in monthly wage granted under other motives on the amount in monthly wage granted for unfair dismissal controlling for economic dismissal, wage, seniority, the prud'hommes compensation and the size of the firm yields a 95% confidence interval equal to [0.13, 0.20].

17. 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

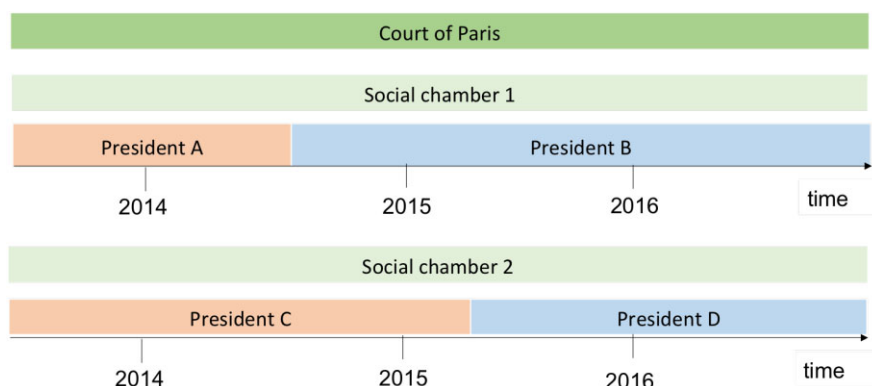


FIGURE 2. Allocation of cases exploited for identification. This figure displays the allocation of cases to judges used for identification. Within an Appeal court, there may be several social chambers. Within each social chamber, there is, at an instant t , one chamber president who judges the cases. 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.

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 2.2.2, this is supported by three key institutional features: (i) judges inherit a large backlog of cases that must be treated by order of arrival, (ii) judges are mobile, and (iii) conflicts of interests are controlled by the President of the Appeal court and an assembly of judges. Moreover, if the judge is absent on the day of the judgment, she can be replaced by another judge under the control of the President of the Appeal court, without notice to the plaintiff and the defendant.

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 2.

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 [Online Appendix D](#) the use of this variable, and display in [Online Appendix Figure D.1](#) the histogram of the frequency at which the judge grants a positive compensation.

We implement this strategy through using a residualized, leave-one-out judge bias measure that accounts for case selection following Dobbie, Goldin, and Yang (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 $\times$ year fixed effects to obtain the residualized component $\hat{\varepsilon}_{ij}$.¹⁸

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

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 $\times$ 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 $\hat{\varepsilon}_{i'j}$:

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

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.¹⁹

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 C). In our context, the network of judges is dense, thus indicating a high mobility of judges across social chambers.²⁰

18. Note that we account for all cases, including those with zero compensation.

19. The bias for case i judged by judge j can also be re-written as:

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

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

20. We document the extent of judge mobility in Online Appendix Figure J.3, 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. If judges were not mobile whatsoever, one would observe perfectly distinct judge clusters, each cluster representing one social chamber.

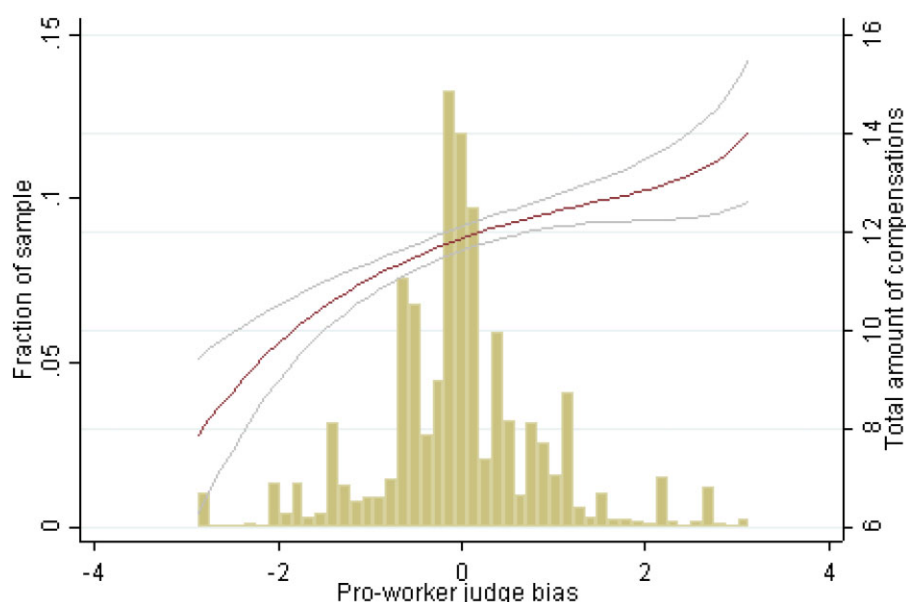


FIGURE 3. Judges pro-worker biases with respect to the compensation in months of salary. This figure displays the histogram of the pro-worker biases of judges with respect to the total amount of compensation for wrongful dismissal and a local polynomial fit of the total amount of compensation, represented by the black 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.

4.3. Results

In this section, we begin by documenting the relationship between judge bias and the full compensation package granted by the judge. We then document the relation between the bias of judges and their observable characteristics. In the second step, we proceed to the analysis of the randomness of the allocation of cases to judges.

4.3.1. 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 3 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)

TABLE 3. Correlation between judge bias and compensation for wrongful dismissal.

	Compensation (1)	Compensation (2)
Judge pro-worker bias wrt compensation	0.927*** (0.175)	0.832*** (0.167)
Year FE	Yes	Yes
Court FE	Yes	Yes
Case controls	No	Yes
adj – R ²	0.027	0.143
F test	28.05	147.8
# obs	8,353	8,353

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 total amount of compensation is trimmed at the top 0.1% of the distribution to eliminate outliers. Court and year $\times$ 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.

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.²¹

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 [Online Appendix D](#), 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.

4.3.2. Judge Bias and Judge Characteristics. The judge bias might be correlated with several judges characteristics, such as the judge’s experience, age or gender. We gathered information on judges’ professional trajectories such as their dates of first nomination as president of a social chamber, or their date of retirement, if applicable.²² Another variable interesting to correlate with the judge bias is the quality of judges, which can be proxied by the average length of the Appeal court texts of judges, which we measure in number of words, and the number of references to the Labor code in Appeal court texts.

Regressing the judge bias on these characteristics shows that there is no significant correlation between the judge bias with either the judge’s age, the judge’s gender, the

21. 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 .

22. The details are presented in [Online Appendix E](#).

average length of the judge's appeal court texts, or the average number of references to the Labor code. However, there is a positive correlation between the judge bias and the year of nomination of the judge as president of a social chamber. Being nominated one year later increases the judge bias by 2.77 percentage points. This effect could come from either from a cohort effect—judges nominated more recently are more pro-worker—or from an experience effect—judges who have been judge for a longer time are more pro-worker. To tease out the potential cohort effect from the year effect, we control for the average year of judgments of the judge. We still find a positive and significant correlation between the judge bias and the year of nomination of the judge as president, which points to cohort effects.

We complete this analysis by studying the evolution of the bias of a given judge over time. We do so by computing the judge bias at the judge-year level. Regressing this time varying measure on indicator variables for the number of years since the judge's first year of judgment in our data set, controlling for judge fixed effects and year-of-judgment fixed effects, we find that the judge bias when computed at the judge-year level does not vary across time for a given judge. This suggests that there is no or only small experience effects, which validates our use of a constant fixed effect per judge.

4.3.3. 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 observable characteristics of cases. 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 and the F -test does not reject the joint significance of these explanatory variables. However, regressing the pro-worker bias on the same characteristics, yields no significant relationship. Similarly, no significant relationship is found between pro-worker biases and the characteristics of the firm.²³

We provide further randomization tests showing that case and firm characteristics are not statistically different within pairs of judges, constituted of two successive judges in the same social chamber year, judge A being more pro-worker than judge B. This is consistent with the fact that cases are judged by their order of arrival, independently of judges' preferences.²⁴

23. Online Appendix Tables F.1 and F.2 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.

24. See Online Appendix Tables F.3 and F.4.

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 confirms the implementation of the allocation of cases independent of the identity of the judge, in accordance with the law, and 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 Firms and Workers

This section is devoted to the analysis of the impact of judge bias on firms' performance and workers' employment trajectories. In principle, judge biases can create unforeseen shocks to dismissal compensations for dismissals that have occurred in the past. This implies that dismissal compensations are random, which is a feature overlooked by the literature so far to our knowledge. Therefore, we begin by analyzing the consequences of this random dimension of dismissal compensations from a theoretical point of view. Then, we present some descriptive statistics on firms, before presenting the empirical strategy, the results and the main mechanisms at play.

5.1. Theoretical Framework

To analyze the consequences of random dismissal compensation, we consider a Mortensen and Pissarides (1994) type model of a multi-worker firm in partial equilibrium. The productivity of each worker is a random variable the new value of which is independently drawn at the beginning of each period. In this framework, presented in detail in [Online Appendix G](#), the firm hires workers and dismisses those whose productivity drops below a threshold that depends on the wage, the discount factor of the firm, its survival probability, the future productivity distribution of jobs, and the expected dismissal costs.

First, we show that a sufficiently large unanticipated shock to dismissal compensations can destroy the firm. These consequences are more likely for small low-performing firms. However, for surviving firms, unanticipated shocks to dismissal compensations have no impact on job creation and job destruction if they face no credit constraint and do not revise their expectations on dismissal costs.

Second, we examine the situation where unanticipated shocks on dismissal compensations, by reducing the cash-flow, can limit access to credit, which translates into an increase in the interest rate and therefore a drop in the present value of jobs and of the firm. In that case, more likely for small, low-performing firms, the unforeseen positive shocks can (i) decrease job creation, (ii) increase job destruction, and (iii) reduce the future survival probability of the firm.

Third, we account for the possibility that firms revise their expectations on dismissal compensations. In this context, positive shocks increasing the expected dismissal costs and reduce job creation. However, the impact on job destruction is ambiguous. On one hand, the expectation of higher dismissal costs, by making job destruction more costly, reduces job destruction. On the other hand, if higher dismissal costs are associated with a drop in the survival rate of the firm, the present value of jobs drops, which increases job destruction. It is likely that the first effect dominates for large, high-performing firms, while the second effect may dominate for small, low-performing firms.

Fourth, we introduce temporary jobs. In the absence of credit constraints and of revision of expectations, shocks on dismissal costs do not change the hiring of temporary and permanent workers for surviving firms. However, in presence of credit constraints or revision of expectations, positive shocks on dismissal costs increase the share of temporary jobs, because the increase in the interest rate or the higher expected layoff costs reduce the present value of permanent jobs relative to that of temporary jobs.

Overall, the model shows that the employment impact of layoff costs shocks can be very different depending on the situation of firms. For large or high-performing firms, the credit rationing channel is likely to be second-order, implying that the potential impact of positive shocks to dismissal costs is primarily through a revision of expectations, if any. This should result in fewer creation and destruction of permanent jobs and an increase in the share of temporary employment. The effect on total employment is therefore ambiguous. On the other hand, for small low-performing firms, the credit channel effect as well as the effect linked to expectations revisions imply that positive shocks on layoff costs reduce their survival, reduce job creation, increase job destruction, which ultimately results in a negative impact on employment, and especially on permanent jobs. Therefore, the model informs us on one way to disentangle the two mechanisms: we can study the effect of the judge bias on the destruction rate of permanent jobs depending on the size and performance of firms.

5.2. Descriptive Statistics

We consider for-profit firms in the private sector, excluding the agricultural sector.²⁵ 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.²⁶ 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

25. The agricultural sector is excluded due to the large share of seasonal employment in this sector.

26. Matched employer–employee data are available from 2002 to 2015.

TABLE 4. Summary of main variables at firm-level—all firms.

	Mean	Min	Med	Max	sd	Count
Nb of workers	47.89	1.00	14.00	4,645.00	142.57	7,329.00
Nb of hires	10.95	0.00	4.00	725.00	32.25	7,324.00
Nb of exiters	10.37	0.00	3.00	996.00	35.84	7,329.00
Sales (in K euros)	6,237.23	0.00	2,008.00	64,482.00	10,360.51	6,960.00
Value added (in K euros)	1,819.74	0.00	778.00	17,798.00	2,717.28	6,867.00
Share of firms in manufacturing	0.19	0.00	0.00	1.00	0.40	7,329.00
Share of firms in construction	0.11	0.00	0.00	1.00	0.31	7,329.00
Share of firms in services	0.33	0.00	0.00	1.00	0.47	7,329.00
Share of firms < 10 years	0.27	0.00	0.00	1.00	0.44	7,329.00
Survival at $t+1$	0.95	0.00	1.00	1.00	0.22	7,329.00
Survival at $t+2$	0.90	0.00	1.00	1.00	0.30	7,329.00
Survival at $t+3$	0.87	0.00	1.00	1.00	0.34	7,329.00
Wrongful dismissal	0.52	0.00	1.00	1.00	0.50	5,344.00
Amount in wage bill (when >0)	10.68	0.00	2.15	1,336.30	40.08	5,340.00
Judge pro-worker bias	-0.04	-2.05	-0.04	2.73	0.76	7,329.00
Amount	11.81	0.00	8.40	442.43	15.68	3,553.00

Note: The sample is defined in [Online Appendix H](#). “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.

times during the period.²⁷ 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, presented in [Online Appendix H](#), shows that the composition of the sample is stable at the various stages of restrictions, although firms in our final sample tend to have a higher return on assets (ROA) because we only keep surviving firms at the date of the judgment. Besides, firms going to court in our final sample also appear broadly comparable with other firms recording hiring and separations over the same period but not going to court, including in terms of economic performance, layoff rates and turnover.

Table 4 provides descriptive statistics at the firm level, that is, 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. Out of the total, 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, that is, with less than 15 employees (see Table 5), for which the judge bias will be shown to have more impact, the probability

27. Indeed, if a firm goes several years to Court, disentangling the effect of each Court ruling is impossible.

TABLE 5. 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	3,677.00
Nb of hires	2.42	0.00	2.00	251.00	4.94	3,677.00
Nb of exiters	2.50	0.00	2.00	320.00	7.22	3,677.00
Sales (in K euros)	1,489.21	0.00	847.00	61,353.00	2,531.33	3,607.00
Value added (in K euros)	466.57	0.00	338.00	14,315.12	584.92	3,534.00
Share of firms in manufacturing	0.14	0.00	0.00	1.00	0.35	3,677.00
Share of firms in construction	0.11	0.00	0.00	1.00	0.32	3,677.00
Share of firms in services	0.32	0.00	0.00	1.00	0.47	3,677.00
Share of firms <10 years	0.38	0.00	0.00	1.00	0.48	3,677.00
Survival at $t+1$	0.93	0.00	1.00	1.00	0.26	3,677.00
Survival at $t+2$	0.87	0.00	1.00	1.00	0.34	3,677.00
Survival at $t+3$	0.82	0.00	1.00	1.00	0.38	3,677.00
Wrongful dismissal	0.52	0.00	1.00	1.00	0.50	2,641.00
Amount in wage bill (when >0)	18.98	0.00	6.39	1,336.30	55.30	2,641.00
Judge pro-worker bias	-0.02	-2.05	-0.04	2.73	0.76	3,677.00
Amount	10.82	0.00	7.49	442.43	16.82	1,809.00

Note: Firms whose size is equal or below the median size of the sample defined in [Online Appendix H](#). “Nb of workers” corresponds to headcounts on December 31 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.

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%.

5.3. 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, and 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:

$$Y_{ik} = \alpha_{0k} + \alpha_{1k} \text{bias}_{ij} + \alpha_{2k} X_{ik} + \eta_{ik}, \quad (4)$$

where Y_{ik} is the outcome of interest k years before/after the judgment for firm i assigned to judge j ,²⁸ $bias_{ij} = (\bar{e}_{ij} - \bar{e})/\sigma_e$, is the judge j 's leave-one-out normalized bias (i.e. the difference between the leave-one-out judge's bias defined in Section 4.2 and the average over all judges ($\bar{e}$) scaled in standard deviation (σ_e) 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 judgment, we use as outcome variables Y the growth rates of the variables of interest with respect to the year before the judgment—that is, 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.²⁹ To analyze the impact of judge bias on the trajectories of workers employed in the firm the year of the judgment, dependent variables include indicator variables equal to one for workers who keep their job, for workers employed either in the same firm or in another firm, 1, 2, or 3 years after the judgment. We also document the evolution of the wages of these workers over this time span.

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. (2022) who state that the standard errors clustering must be decided according to the level at which either the sampling or the randomization is performed.³⁰

Our approach is different from the two-way fixed effects model, frequently used in event studies. The two-way fixed effects estimator is the weighted average of the treatment effect with weights that sum to one but which can be negative. Negative weights can arise because the estimator is a weighted sum of several difference-in-differences (DiD), which compare the evolution of the outcome between consecutive

28. Firms judged in the same year-to-event can be in different calendar years.

29. 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 Törnqvist, Vartia, and Vartia (1985) and Davis, Haltwanger, and Schuh (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.

30. An alternative, used for instance in Dobbie, Goldin, and Yang (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.

time periods across pairs of firms.³¹ Alternatively, two-way fixed effects estimators use the difference between treated units and units not already treated or never treated to correct for this issue. In our set-up, firms before the judgment are not a valid control group because they necessarily survive until the judgment, while they can be destroyed after the judgment. Comparing the impact of judgments between firms after the judgments (which can be destroyed) with firms before the judgment (which necessarily survive until the judgment) with two-way fixed effects would amount to estimating the mean of DiD estimators (with possible negative weights) between treated and untreated firms which are not comparable by construction. Accordingly, the estimators constructed by the two-way fixed effects literature, which all assume the presence of a valid control group, cannot be used in our framework. Hence, our specification does not have firm fixed effects, but the outcome is differenced with respect to the period before the judgment, so it can be thought of as a long difference version of a two-way fixed effects event study regression.³² Such regressions yield unbiased estimates of the average dynamic treatment effects under (i) a parallel trends assumption, and (ii) if firms treatment intensity is uncorrelated with their treatment effects. In our framework, the trend before the judgment, equal, for each year before the judgment, to the OLS estimator of the judge's bias on the firm's outcome, only shows that the judge's bias is not correlated with the firm's performance, which is consistent with the absence of selection of firms before the judgment based on the bias of the judge. From this perspective, estimates before the event date can be interpreted as placebo tests. Our small and insignificant placebo coefficients displayed in Section 5.4.1 indicate that the first assumption is plausible. Our balancing checks in Section 4.3.3 indicate that the second assumption is also plausible. Moreover, as shown in Section 6.1, adding firm fixed effects estimated before the event date in the set of covariates does not significantly change the results.

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:

$$Y_{ik} = \beta_{0k} + \beta_{1k} f_i + \beta_{2k} X_{ik} + \varepsilon_{ik}, \quad (5)$$

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.

31. See Athey and Imbens (2021), Callaway and Sant'Anna (2021), de Chaisemartin and D'Haultfœuille (2020), Goodman-Bacon (2021), Sun and Abraham (2021), and Borusyak, Jaravel, and Spiess (2022).

32. We thank Clément de Chaisemartin for suggesting this interpretation.

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 2.2.2 and 4 and is confirmed by the results of the event study in Section 5.4.1 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 6.4 that our estimates are robust to taking such multidimensionality into account.

The third identifying assumption is the relevance of the instrument. This has been shown in the previous sections, notably in Figure 3 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 3 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.

5.4. The Impact on Firms

5.4.1. 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.

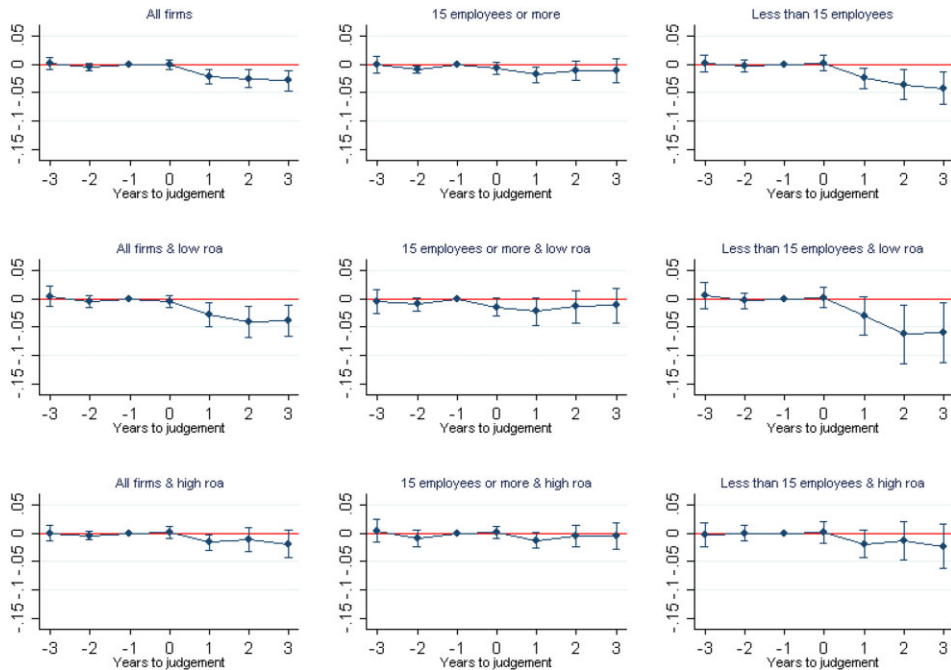


FIGURE 4. Event study: employment growth rate depending on the judge bias (without control variables). This figure displays the coefficients α_{1k} of equation (4) in year k , $k \in [-3, 3]$ relative to the judgment year t where the dependent variable is the symmetric employment growth rate relative to the year preceding the judgment year t and the explanatory variable is the leave-one-out judge bias, $bias_{ijt}$, 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 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.

We distinguish firms according to whether their size is below or above the median (equal to 15 employees) and according to whether their returns on assets are above or below the median the year preceding the judgment. We thus obtain four groups of firms. The theoretical model predicts that small, low-performing firms should be affected more negatively by pro-worker judges.

It is clear from Figure 4, 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 judge bias 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

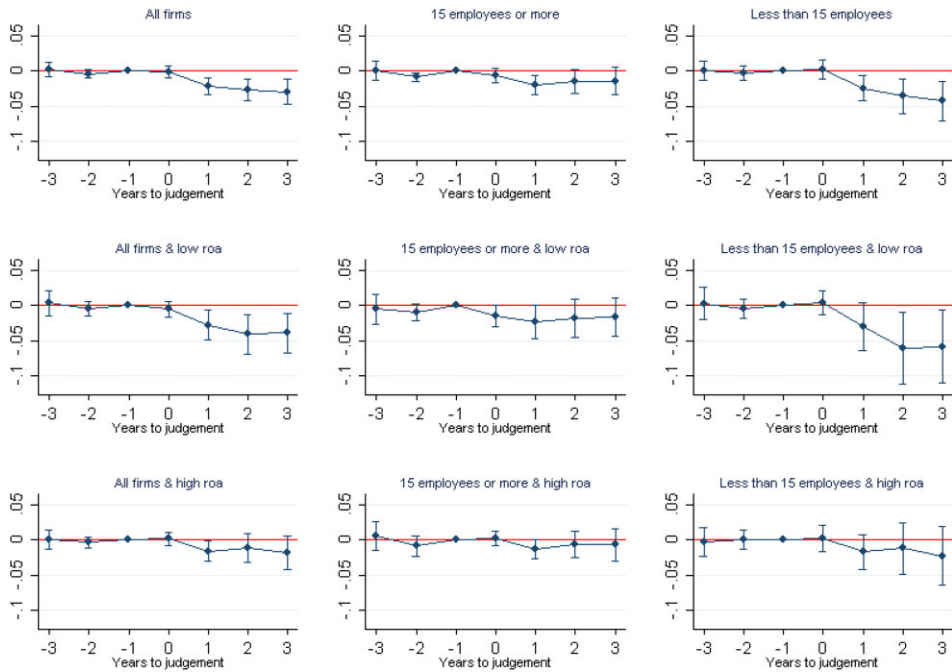


FIGURE 5. Event study: employment growth rate depending on the judge bias (with control variables). This figure displays the coefficients α_{1k} of equation (4) in year k , $k \in [-3, 3]$ relative to the judgment year t where the dependent variable is the symmetric employment growth rates relative to 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 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.

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 5 shows that controlling for the past performance of firms does not change the common trend.³³ 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 judgment according to the type of judge.

33. We display the estimates in [Online Appendix Table J.1](#).

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 5. 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 According to our theoretical model, 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.³⁴ Second, employers might revise their expectations about dismissal costs according to the decisions of judges, which are all about permanent contract terminations.³⁵ 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 6 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 7. 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 three years after the judgment 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.

34. See also Caggese, Cuñat, and Metzger (2019).

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

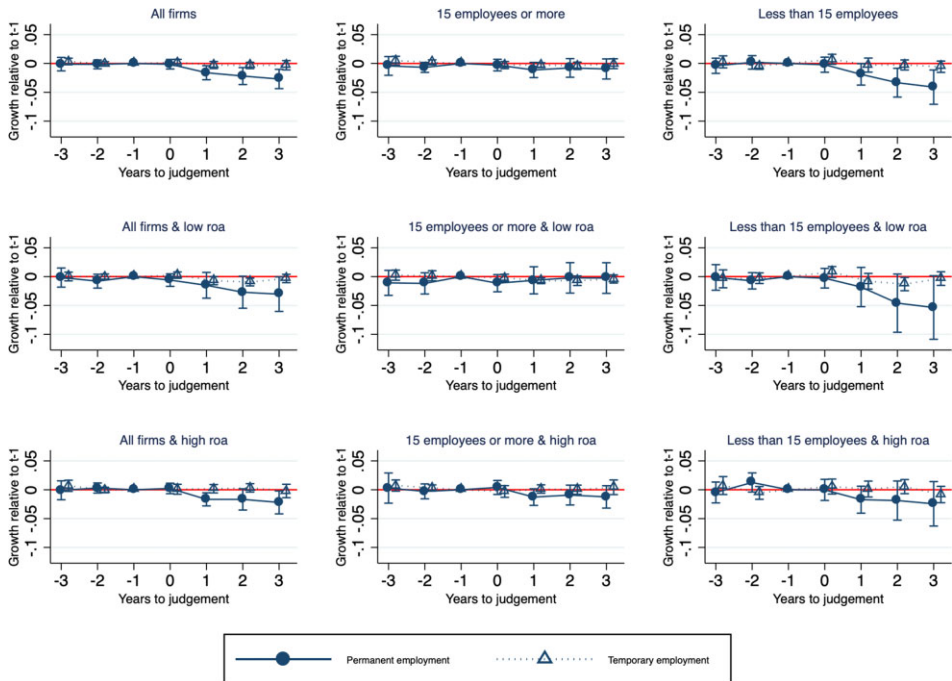


FIGURE 6. Event study: permanent and temporary employment growth rates depending on the judge bias. This figure displays the coefficients α_{1k} of equation (4) in year k , $k \in [-3, 3]$ relative to the judgment year t where the dependent variables are the symmetric growth rates of permanent and temporary 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, and the leverage and the capex the year preceding the judgment. The left panel reports the results for all firms, the middle 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.

Interestingly, still in line with the theoretical model, the employment effects of the judge bias within a 3-year horizon are not solely driven by firm death. The judge pro-worker bias also has a significant negative impact on the growth rate of employment of low-performing firms which survive 3 years after the judgment.³⁶

All in all, relative to an hypothetical situation where all judges would be as pro-worker as the average of our sample—that is, a bias equal to zero—the employment growth of all firms would drop by 1 percentage point and the survival rate by 0.3 percentage point 3 years after the judgment if all judges were as pro-worker as

36. See [Online Appendix Figure J.4](#). Although the selection of the sub-sample of surviving firms is endogenous, the results reported in this figure are still informative about the channels at play.

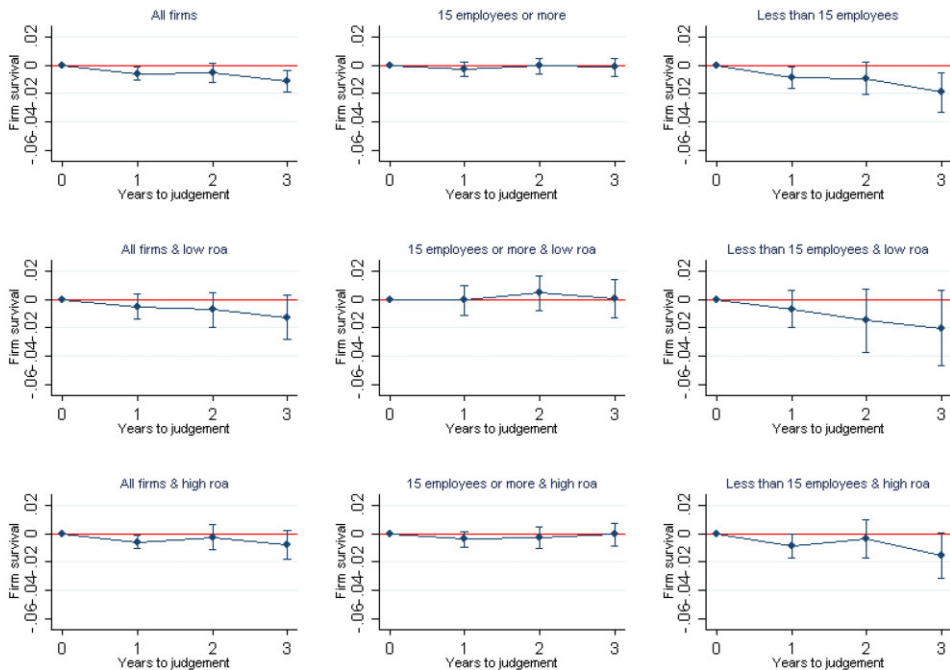


FIGURE 7. Firm survival rate depending on the judge bias This figure displays coefficients α_{1k} of equation (4) in year k , $k \in [-3, 3]$ relative to the judgment year t where the dependent variable is an indicator variable for firm survival and the explanatory variable is the leave-one-out judge 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 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.

those belonging to the 75th percentile of the distribution of bias.³⁷ The drop for small low-performing firms would be equal to 1.9 percentage points for employment growth and to 0.7 percentage point for firm's survival.

While these results show that the subjectivity of judges has a significant impact on survival and employment among low-performing small firms, additional mechanisms need to be examined to quantify this impact on overall employment. In particular, the fact that the subjectivity of judges affects hiring and dismissal decisions post-judgment could be anticipated by employers and affect firms' pre-judgment behavior.

37. These figures correspond to the estimated value of coefficient α_{13} of equation (4) times the value of the judge bias normalized by its standard error at the 75th percentile of the distribution of judges bias in our sample, equal to 0.34.

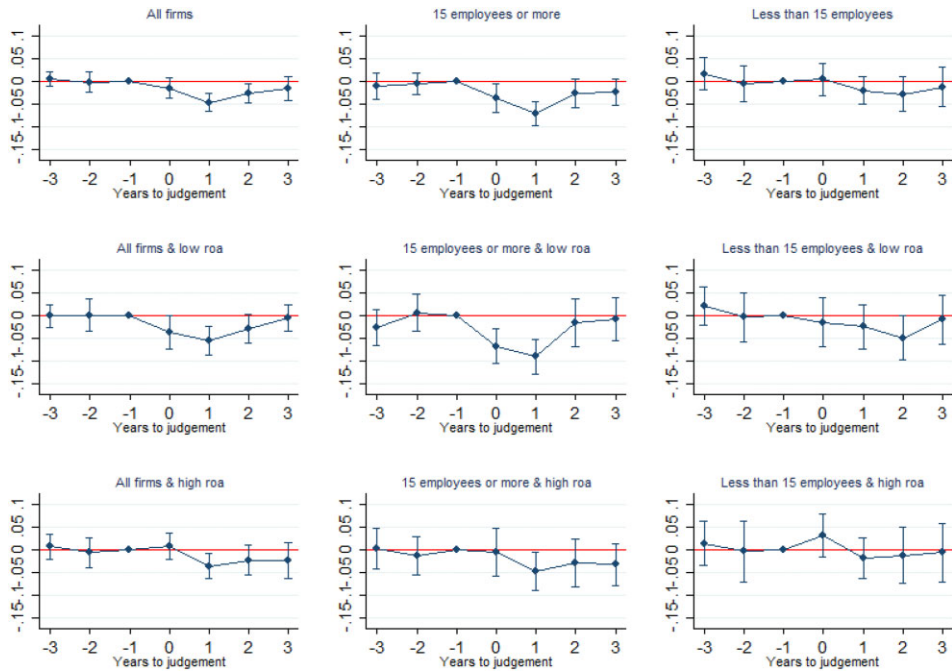


FIGURE 8. Event study: growth in the number of entrants in the firm depending on the judge bias. This figure displays the coefficients α_{1k} of equation (4) in year k , $k \in [-3, 3]$ relative to the judgment year t where the dependent variable is the growth rate in the number of entrants in the firm 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 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.

We examine this mechanism formally in a model presented in the appendix and leave this quantification for future research.

5.4.2. Mechanisms. The judge bias can affect firm performance through the impact on cash flow and firms' expectations of dismissal costs. According to the theoretical model, both effects reduce job creation following a judgment by a pro-worker judge. However, the revision of expectations following a judgment by a pro-worker judge should reduce the destruction of permanent jobs, while the impact on cash flow should increase it. We expect the first effect to dominate for large and/or high-performing firms and the second effect to dominate for small low-performing firms.

Regarding job creation, Figure 8 shows that, indeed, the number of entrants decreases after facing a pro-worker judge, a finding which holds for large firms—both

high and low-performing firms, although the point estimate is larger in absolute value for low-performing firms. This result confirms the predictions of the model regarding job creation.

Regarding job destruction, the relative strength of each channel—expectations and cash-flows—varies by firm sizes and workers' characteristics, which can be seen by tracking the trajectories of the employees present the year before the judgment.³⁸ Permanent workers present the year before the judgment in firms that faced a pro-worker judge are more likely to remain in the same firm in the years after the judgment—see Figure 9—, meaning that the expectation channel is overall dominating. This effect, however, is entirely driven by large, high-performing firms, while for small and low-performing firms the effect is negative. This means that the cash-flow effect dominates for small and low-performing firms. The difference in impact of a pro-worker judge on employees of small low-performing firms and of large high-performing firms is significant: when the judge's pro-worker bias increases by one standard deviation, the probability of keeping their job for employees of small low-performing firms decreases by two percentage points, while it increases by two percentage points for employees of large high-performing firms three years after the judgment. The effects on small low-performing firms are partly driven by firms which do not survive: conditional on the firm surviving, the point estimate for remaining employed three years after the judgment in firms that faced a pro-worker judge is still negative, but of small magnitude and not statistically significant—see [Online Appendix Figure J.5](#).

5.4.3. IV Estimates. As explained in Section 5.3, our approach allows us to quantify not only the impact of the judge bias, but also 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.³⁹ Table 6, 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 10, 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

38. In the DADS data, workers have identifiers which are valid only two years in a row. In order to reconstruct an employee's trajectory over multiple years, we need to re-construct individual identifiers that are unchanged for the period. We do so by using the codes of Babet, Godechot, and Palladino (2022) to build an exhaustive matched employer–employee database for our period of interest.

39. The unit of measurement is expressed in month of salary to compute the judge bias, because this is how judges usually define dismissal compensations. To analyze the impact on firms of shocks on dismissal compensations induced by the fixed effects of judges, the unit of measurement is the amount of the compensation divided by the past payroll of the firm in order to take into account that the impact of the income shock on the performance of the firm must be proportional to the size of the firm.

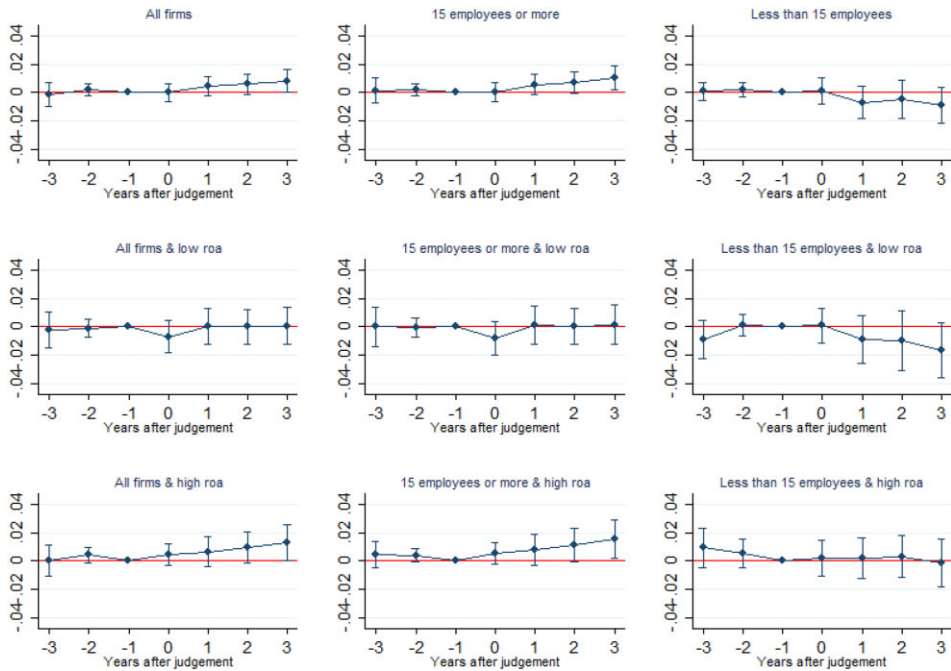


FIGURE 9. Event study: probability for a permanent worker to work for the same firm in the years after the judgment depending on the judge bias. This figure displays the coefficients α_{1k} of equation (4) in year k , $k \in [-3, 3]$ relative to the judgment year t where the dependent variable is an indicator variable equal to one for the survival of permanent workers employed in the firm the year before the judgment and 1, 2, and 3 years after the judgment. 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 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.

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 [Online Appendix Figure J.6](#).

The point estimates reported for all firms and for firms below 15 employees are very close. This suggests, in line with the theoretical model, 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

TABLE 6. 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** (0.145)	0.512* (0.295)	0.701** (0.214)	1.069** (0.423)	0.290 (0.198)	0.067 (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%. Source: DADS, FICUS-FARE, SIREN, Appeal court rulings database.

judges on small 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 4 and 5—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 a cleansing effect, 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.

5.5. The Impact on Workers

We have just seen that the decisions of the judges, by affecting the performance of the firms, also have an impact on the future of the workers employed by the firms in the year of the judgments. To analyze these consequences in more detail, we examine the wage trajectories and employment trajectories of these workers.

5.5.1. Wage Trajectories. Working for a firm judged by a pro-worker judge does not have a significant effect on the wage trajectory of the worker overall, in the three

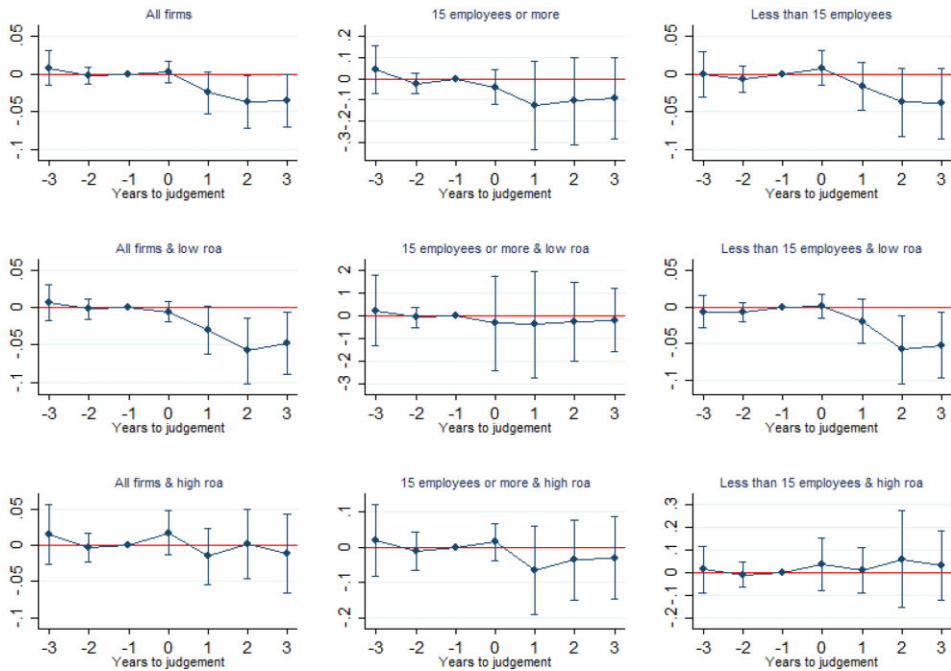


FIGURE 10. Event study: employment growth rate depending on the amount of compensation in the previous year payroll instrumented by judge bias. This figure displays the coefficients β_{1k} of equation (5) in year k , $k \in [-3, 3]$ relative to the judgment year t where the dependent variable is the 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, $bias_{ijt}$, 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 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.

years after the judgment, and conditional on remaining employed—see Figure 11—or conditional on staying in the same firm.⁴⁰ Overall, the judge bias has no impact on wages, which is consistent with the characteristics of the French labor market, in which

40. See Online Appendix Figure J.7. Note that this analysis may suffer from a selection bias, insofar as the probability of workers to remain in the same firm may be correlated with the judge bias and may differ across worker types. For instance, if good workers tend to leave the firms facing a severe judge, then we would observe a negative effect of the judge bias on the worker's hourly wage in the years after the

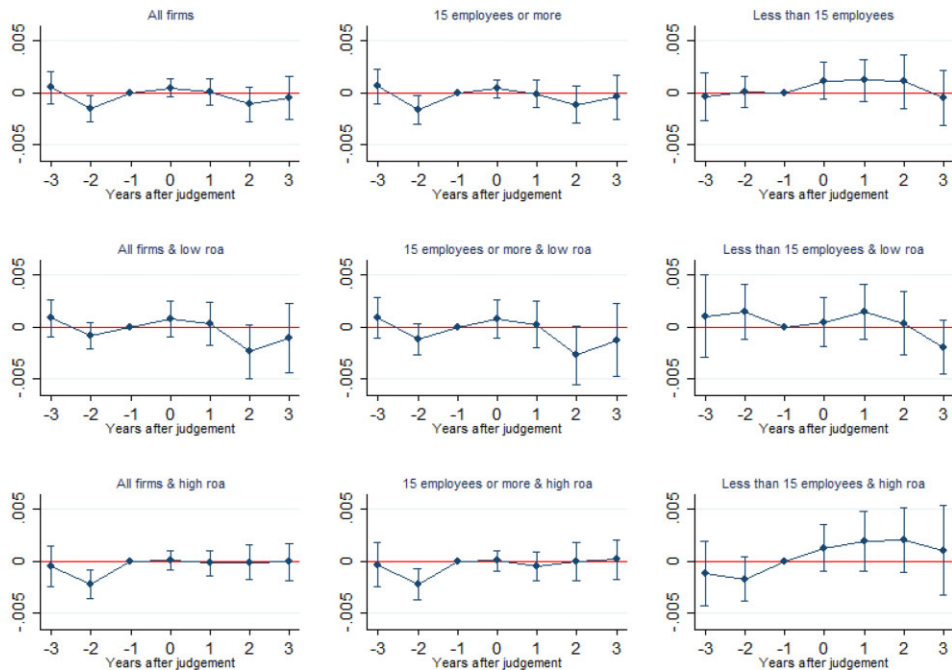


FIGURE 11. Event study: growth of the hourly wage, conditional on remaining employed, in the years after the judgment depending on the judge bias. This figure displays the coefficients α_{1k} of equation (4) in year k , $k \in [-3, 3]$ relative to the judgment year t where the wage growth, relative to the year before the judgment, of workers employed in the firm the year before the judgment and employed in any firm 1, 2, and 3 years after the judgment. 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 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.

the minimum wage and the collectively agreed minima at sector level are binding for a wide range of wages. The impact could be different in a more competitive labor market.

5.5.2. Employment Trajectories. We find that the empirical trajectories of workers are consistent with the mechanism explained in Section 5.4.2, through which the decisions of pro-worker judges have different consequences for workers depending on the type of firm that employs them. The probability of keeping their job in the judged

judgment. We control for this selection bias by controlling for the initial hourly wage of the worker, and by including occupation fixed effects to control for the hierarchy of the worker's position.

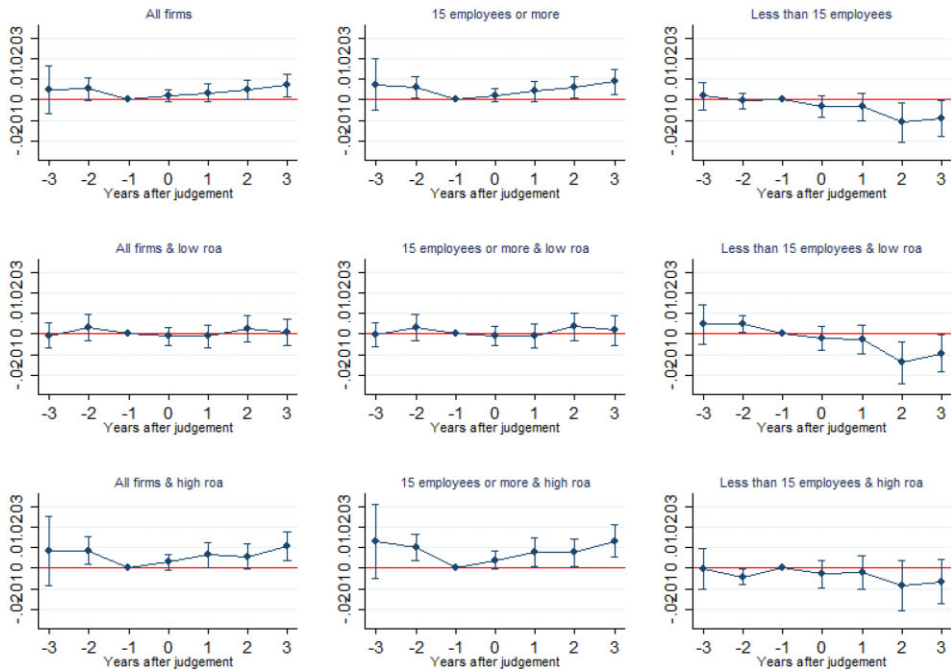


FIGURE 12. Event study: probability for a permanent worker to be employed in the years after the judgment depending on the judge bias. This figure displays the coefficients α_{1k} of equation (4) in year k , $k \in [-3, 3]$ relative to the judgment year t where the dependent variable is an indicator variable equal to one if the permanent worker employed in the firm the year before the judgment is employed 1, 2, and 3 years after the judgment. 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 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.

firm increases for those working in large or high-performing firms while it decreases for those working in small, poorly performing firms.

In particular, Figure 12 shows that employees of small low-performing firms have a lower probability of being employed of 1 percentage point three years after the judgment when the judge's pro-worker bias increases by one standard deviation. The comparison with the previous results, according to which the probability of keeping their job for employees of small low-performing firms decreases by two percentage points when the judge's pro-worker bias increases by one standard deviation, indicates that about half of those who lost their job are not employed three years after the judgment.

6. Robustness Checks

We conduct a range of additional 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, as shown Section 4.3.3, 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 5 and Online Appendix Figure J.8 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. Online Appendix Table J.2 shows that the quadratic term $bias_{ij}^2$ is not different from zero at a 5% 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 Online Appendix Figure J.9).

6.3. Heterogeneous Effects

6.3.1. Return on Equity. The effects of judge bias we uncover 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, Online Appendix Figure J.10 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.

6.3.2. Credit Constraints. According to our theoretical model, shocks on redundancy indemnities should have a greater impact on firms that have access to limited credit. In order to evaluate this prediction more precisely, we use the credit rating of the *Banque de France* (the French Central Bank). The *Banque de France* gives a credit score to firms which have sales above a given threshold (750,000 of annual sales). However, firms may also apply to be listed. There are therefore firms whose turnover is less than 750,000 euros. The threshold and the voluntary registration introduce selection effects, and missing information, especially for small firms. To obtain a proxy of the credit score, we regress, for firms which have a non-missing credit score, the credit score on the firm's leverage, lag leverage, total sales, lag total sales, returns on assets, and lag returns on asset. We explain 28.6% of the variance of the firm's credit score. We then predict the credit score for each firm of our sample, based on this regression model. In line with the model's prediction, [Online Appendix Figure J.11](#) shows that firms with a bad credit score are more affected by the judge bias.

6.4. 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.⁴¹ 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 [Online Appendix D](#). The results, reported in [Online Appendix Table J.3](#), 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 [Online Appendix D](#). The results, presented in [Online Appendix Table J.4](#), show that 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.

41. See Bhuller, Dahl, and Mogstad (2020) in the context of Criminal justice.

6.5. *Comparing the Effect of the President to other Judges*

So far, our analysis has only focused on the presidents of social chambers. However, these presidents are assisted by two judges for each judgment. In order to study the impact of assessor judges, we extract the name of each of those assessors for each firm of our final sample, and compute the leave-one-out fixed effect for each assessor—see [Online Appendix I](#). We then study the impact of those biases on the growth of employment three years after the judgment. We find that the average bias of the two assessor judges has a negative effect, significant at the 10% level, on the employment growth 3 years after the judgment—see Table I.1. However, when both the bias of the president and the average bias of the two assessor judges are included as explanatory variables, the average bias of the two assessor judges is not significant any more while that of the president is significant at 95% confidence level, and not statistically different from that obtained when the average bias of the assessor judges are not included in the explanatory variables. Overall, our results point toward the fact that the president of the social chamber might not be the only judge that matters for firms' outcomes, but is the one with the highest impact by large.

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 significant but heterogeneous effects on firms, depending on their size and their performance. The subjectivity of judges can have a direct impact on firms' cash flows but can also shape their expectations regarding termination costs. We show that the revision of expectations likely dominates for large firms, while the cash flows mechanism dominates for small firms.

While these results provide a useful contribution to the lively debate on the existence and the impact of labor court judges subjectivity on firm performance, further research is needed on this under-explored topic. In particular, three directions deserve special attention.

First, are judges on average more pro-workers or pro-employers than other categories of the population? Our measure of judges' subjectivity only compares judges to each other. It assesses whether certain judges are more pro-worker or pro-employer than colleagues who worked in the same social chambers the same year as them. But our measurement does not make it possible to know whether they differ from the general population. This is an interesting question worth exploring.

Second, 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.⁴² 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, Sibony, and Sunstein 2021).

Third, what is the effect of judges' subjectivity on the employment trajectories of dismissed workers? As decisions are anonymized, we cannot identify in our dataset the worker who was dismissed, and who therefore receives the dismissal compensation. Further work could use the random assignment of cases to judges to inspect the effect of receiving a dismissal compensation on job search behavior.

Appendix 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; and 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.

42. Clancy et al. (1981), Kahneman, Sibony, and Sunstein (2021) stress the importance of such "Patterned differences" between judges in the context of Criminal justice.

- 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, and 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 than 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).

Appendix B. Sample 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. All Appeal judgments have a ruling and all Appeal court rulings are published. An example of an Appeal court ruling is displayed on Figure B.1. We use natural language techniques 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.

PAR CES MOTIFS

LACOUR,

Statuant par arrêt contradictoire,

INFIRME PARTIELLEMENT le jugement déféré et statuant à nouveau,

CONDAMNE la Société [REDACTED] à verser à Monsieur B. 30.000 € (TRENTÉ 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

FIGURE B.1. Example of end of Appeal court ruling. Source: Appeal court rulings database.

B.1. 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 winning party; compensation for unpaid annual leave (*indemnité compensatrice de congés payés*); and 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 to 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 be recovered from the mentions of when the employee was hired. We therefore use multiple approaches to recover 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.

B.2. 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, Bloom, and Davis 2016).

Appendix C. 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.

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Supplementary Data

Supplementary data are available at [JEEA](https://www.jeeaonline.org) online.