

# Institutions' and Firms' Adjustments: Measuring the Impact of Courts' Delays on Job Flows and Productivity

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## Abstract

Not only is labor-market rigidity the result of legislative provisions, but it is determined by the institutional framework at large. We argue that courts' delays in settling labor disputes affect the strictness of employment-protection legislation by increasing the expected firing costs. We exploit the variation in the length of labor trials across Italian judicial districts and the fact that the Italian legislation regarding firing prescribes different firing regimes for firms above the 15-employee threshold and provide evidence on the impact of courts' delays on job reallocation and firms' productivity. We show that in judicial districts with longer trials, the rate of job turnover is significantly lower. This occurs through lower rates of job destruction and, to a lesser extent, job creation. We also find a detrimental impact of courts' delays on the labor productivity of firms above the 15-employee threshold. The effect is stronger in sectors with higher flexibility requirements.

## 1. Introduction

Growing attention is being devoted to the role of institutions in affecting labor-market adjustments and firms' production decisions. Since the seminal contribution of Mortensen and Pissarides (1994), labor reallocation is considered crucial in determining labor-market outcomes through the capacity of the firm to quickly adjust to exogenous shocks. In a world where agents (firms and workers) are heterogeneous and the matching process between vacancies and workers is

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costly, when a shock hits the economy the desired allocation of jobs among firms and sectors changes, which leads to job destruction on the one hand and the creation of new vacancies on the other. As long as the reallocation of workers and jobs across industries and firms is important for productivity, policy and institutional factors that hinder the firm-worker match also affect firms' and aggregate economic performance.

Firms' capacity to reallocate labor depends on the strictness of employment-protection legislation (EPL). A higher degree of protection for workers has been shown to unambiguously reduce job creation and job destruction (Hopenhayn and Rogerson 1993; Mortensen and Pissarides 1994; Pissarides 2000). A more controversial issue is how this effect translates into changes in productivity at the firm level. In a standard search model of a labor market with Nash bargaining, the presence of firing costs on the one hand reduces the productivity threshold at which firms dismiss their workers, with a negative effect on productivity; on the other, it increases the productivity threshold at which firms hire workers, with an opposite effect on overall productivity. Moreover, firing restrictions may positively impact firms' productivity through human-capital-specific investments and learning by doing.

While the theoretical literature has made headway in exploring the role of employment protection as a determinant of labor-market flows, the empirical research has to grapple with the difficulty of determining correct measures of firing costs. Indeed, the degree of effective labor-market rigidity not only is the result of legislative rules but also depends on the institutional environment at large. Most empirical studies of the impact of dismissal costs on job reallocation and productivity are based on aggregate EPL indexes constructed by the Organisation for Economic Co-operation and Development (OECD)<sup>1</sup> that measure the strictness of the legislation regarding workers' dismissal for various countries (Venn 2009).<sup>2</sup> According to the OECD index, in 2008 Italy ranked broadly midfield (25th of 40 countries), with its EPL indicator being 1.89 versus an OECD average of 1.94. There is, however, a large consensus that the Italian labor market is one of the most regulated among European countries. This apparent disconnection between the OECD indicator and the perceived rigidity of the Italian labor market is attributed to the fact that *de jure* indicators, such as those constructed by the OECD, fail to capture the *de facto* impact of other institutional factors, which may nonetheless play a significant role in the extent of job protection.

<sup>1</sup> The Organisation for Economic Co-operation and Development (OECD) index for employment-protection legislation (EPL) is one of the most widely used in the empirical studies on the economic effects of labor-market regulation. Apart from the OECD index, other indicators of the stringency of labor regulation have been developed. Such indicators, which generally cover a larger set of countries than the OECD index or a longer period of time, have been constructed by the World Bank and by individual researchers (see, for example, Blanchard and Wolfers 2000; Belot, Boone, and van Ours 2004; Botero et al. 2004).

<sup>2</sup> Among others, Salvanes (1997), Messina and Vallanti (2007), and Cingano et al. (2010) use the OECD EPL index to assess the impact of firing costs on job reallocation and its components. Scarpetta et al. (2002), Bassanini, Nunziata, and Venn (2009), and Cingano et al. (2010) employ the OECD index to study the causal relationship between firing costs and productivity.

The judiciary plays a prominent role in how laws are enforced. According to OECD (2013), costly, complex, or time-consuming legal processes can add significant costs and burdens to firms, which can ultimately be a drag on economic activity; EPL does not escape this rule. Lengthy judicial proceedings about workers' dismissals directly translate into higher firing costs for firms by affecting legal expenses and any financial penalties that may be imposed by a judge; the extent to which firing costs depend on the length of labor trials varies according to country-specific institutions. Moreover, lengthy trials are particularly costly in countries such as Italy, where legislation gives judges significant discretion in determining the enforcement of the rules and the outcomes of trials. In this respect, the length of labor trials can add uncertainty for both the employee and the employer, which further increases firing costs. As a result of these forces, the perceived and actual costs of enforcing dismissal rules can be very different even across countries with similar EPL.

In this article, we investigate to what extent firing costs due to judicial efficiency matter for job-turnover rates and firms' productivity. In our analysis, we rely on an indicator of judicial efficiency that is constructed from the duration of trials concerning labor disputes in the private sector.<sup>3</sup> On empirical grounds, the uncertainty and costs associated with longer trials have been shown to reduce the efficiency of credit markets (Jappelli, Pagano, and Bianco 2005; Fabbri 2010), firms' size (Kumar, Rajan, and Zingales 1999; Giacomelli and Menon, forthcoming), trade flows (Nunn 2007), and economic development in general (Chemin 2009, 2012). However, the impact of the length of trials on labor reallocation and productivity is still largely an unexplored issue.<sup>4</sup> Our analysis contributes to the existing empirical literature on the effect of regulation on job reallocation and productivity along several dimensions.

First, by working with data from a single country, Italy, we are able to isolate the effects of restrictions on firing from those of other (time-varying) institutional features of the labor market such as wage compression (Bertola and Rogerson 1997).<sup>5</sup> Italy is a centralized country, which means that the legal procedures for labor litigation are homogenous across the national territory. Nevertheless, it displays a wide variation in judicial efficiency across court districts. Moreover, by

<sup>3</sup> To evaluate the efficiency of labor courts, we should also take into account other aspects of the judicial system, such as the number and complexity of cases faced by the court and the quantity and quality of financial and human resources (for example, the number of judges) devoted to justice. In our analysis the focus is mainly on trial length. The reason for this is twofold. From an economic point of view, trial length translates directly into higher firing costs for a firm when the firing decision is ruled by the judge to be unfair. Moreover, it is correlated with other aspects of performance such as, for example, individuals' confidence in and perceptions of fairness of the justice system (World Bank 2012; Palumbo et al. 2013). Trial length is used as a proxy for judicial efficiency in a number of papers assessing the effect of judicial performance on economic outcomes. See, among others, Fabbri (2010) and Nunn (2007).

<sup>4</sup> Fraise, Kramarz, and Prost (2015) examine various indicators characterizing the enforcement of labor regulation and find a causal effect of judicial case outcomes on job flows.

<sup>5</sup> Boeri and Jimeno (2005) stress the importance of using data referring to the same country and exploiting any time series available for regulations.

focusing on one country, we can construct comparable job flows and productivity indicators using a firm-level harmonized database.<sup>6</sup>

Second, we focus on a dimension of dismissal costs that is not (entirely) captured by the traditional EPL indicators, namely, the component of firing costs that is related to the length of labor trials. Typically, Italian labor courts take much longer to decide cases than courts in most other countries, and a large proportion of cases are appealed, which delays final decisions for firms and workers even longer.<sup>7</sup> More disaggregated data for Italy show that the length of trials is not homogenous. We exploit the heterogeneity in courts' efficiency across 26 Italian judicial districts and estimate the impact of the duration of labor trials on job flows (job creation and job destruction) and, ultimately, on firms' productivity.

There are a number of issues concerning the identification of the causal effect of the duration of labor trials on job turnover and productivity. First and foremost, the duration of labor trials and firms' production decisions may be driven by some unobserved factors such as the degree of local economic development and the quality of local institutions. Second, the fact that a higher rate of job reallocation may cause an increase in the number of dismissal suits brought to court creates the conditions for a reversed channel of causation between job reallocation and the length of trials.

Our identification strategy exploits the fact that in Italy EPL provisions are more stringent for firms above the 15-employee threshold and that the differential in firing costs between large and small firms increases with the length of the judicial procedure. A number of studies exploit the variation in EPL across firms of different sizes in Italy. Boeri and Jimeno (2005) study the effect of employment protection on the probability of layoff by comparing small and large firms. Borgarello, Garibaldi, and Pacelli (2004) and Schivardi and Torrini (2008) assess the effects of employment protection on the size distribution of Italian firms by looking at the probability of firm-size adjustments around the 15-employee threshold. Similarly Hijzen, Mondauto, and Scarpetta (2017) analyze the effect of different EPL provisions on the composition of workforces, workers' turnover, and productivity of firms above and below the 15-employee threshold. These papers identify the effect of employment protection by exploiting the fact that Italian firms with 15 or fewer employees are subject to lower dismissal costs than firms with more than 15 employees. Other studies exploit the discontinuity in EPL at the 15-employee threshold and the temporal variation in the legislation to assess the effect of reforms on job flows (Kugler and Pica 2008), wages (Leonardi and Pica 2013), and productivity and capital deepening (Cingano et al. 2016). Our

<sup>6</sup> A fundamental problem of the existing cross-country analyses of job flows is the lack of harmonized data at the firm level in terms of the source of the data (administrative versus survey), unit of observation (firms versus establishments), sector coverage, and period of observation (expansions versus recessions).

<sup>7</sup> OECD (2012) reports that, on average, the length of the process of dispute resolution in Italy is about 23 months in the first-level court (4 months in Germany), and the number of dismissed cases appealed is about 59 percent (3 percent in Germany). According to World Bank (2012), Italy ranks 158th in enforcing contracts, with 1,210 days from filing a case to the enforcement of judgment.

study builds on these works in one major respect. Unlike those papers, the aim of our analysis is to isolate the economic effects of the varying degree of efficiency of labor courts from the effect of labor legislation by exploiting the discontinuity of EPL at the 15-employee threshold and the cross-district and over-time variation in labor courts' efficiency (and then firing costs). As far as we know, the impact of labor courts' delays on the rigidity of the labor market is an unexplored issue in the economic literature.

In our empirical analysis, we also control for the potential endogeneity of our indicator of labor courts' efficiency by using a set of instruments that are shown to be disconnected from local business conditions and the functioning of other courts in the same judicial district (for example, civil courts). These include the number of judges' decisions concerning labor disputes of civil servants that are appealed before the court of appeal (the appeal rate)<sup>8</sup> and the number of vacant positions in local courts; the latter depends on the interplay between the personal characteristics of judges and the criteria on which the Consiglio Superiore della Magistratura (CSM), the judiciary self-governing body, approves the transfer (which gives rise to the vacancy).

As an additional check, following the well-established approach developed in Rajan and Zingales (1998), we construct an indicator for an employment-reallocation requirement at the industry level based on UK job flows. If the duration of labor trials is a relevant dimension of firing costs, we should find evidence that large firms requiring more reallocation perform relatively better when labor trials are faster.

The panel dimension of our data also allows us to control for unobserved heterogeneity among sectors and judicial districts via fixed effects. Therefore, our main results are not driven by cross-sectional differences among districts, such as cultural, economic, and social characteristics that may impact labor-market adjustments, firms' productivity, and the efficiency of courts. Finally, our results are also robust to the inclusion of district-specific and industry-specific time dummies, which capture any (time- and district-variant) omitted factors—such as local economic development and informal institutions<sup>9</sup>—that could influence productivity and judicial efficiency.

Our core results suggest that courts' efficiency significantly affects employment-adjustment costs by reducing the average rate of job reallocation in judicial districts. The magnitude of these effects is not irrelevant from an economic point of view. Between the 5th and the 95th percentile of the trail-length distribution (that is, from the judicial district of Salerno to Trento in our sample), the rate of job reallocation increases by almost 60 percent. This effect is almost entirely driven by the increase in the rate of job destruction, which almost doubles, while job cre-

<sup>8</sup> Since the 1998 reform, labor disputes involving public-sector workers—which were previously brought before the administrative courts—are now heard in the labor courts. The rate of appeal of public workers' suits is not directly related to labor-market adjustments, which occur in the private sector. Nevertheless, it contributes to the overall bulk of disputes that are settled by labor courts.

<sup>9</sup> Guiso, Sapienza, and Zingales (2004) show that informal institutions vary widely across Italian regions, which produces significant economic effects.

ation increases by around 25 percent. At the firm level, we find a negative effect of trial length on firms' productivity. Consistent with our expectations, trial length does not affect the productivity of firms below the 15-employee threshold (given that the judicial dimension of firing costs does not depend on trial length for this category of firms), but it is negative and significant for firms above the threshold. This lends further support to our identification strategy, which exploits the fact that trial length affects firms of different sizes differently. Finally, the duration of labor trials has a larger negative impact on firms operating in sectors with a higher reallocation requirement.

The rest of the article is organized as follows. Section 2 illustrates the causation channels from labor courts' delays to job reallocation and firms' labor productivity. Section 3 describes the institutional background and the identification strategy. Section 4 presents the main characteristics of the data, and Section 5 sets out the empirical methodology. The main results are presented in Section 6. In Section 7 we provide some robustness checks, and Section 8 concludes.

## 2. Judicial Efficiency, Job Flows, and Firms' Productivity

The efficiency of the judicial system influences firms' employment decisions and productivity through its impact on dismissal costs. For employers, delays in trials concerning labor disputes can add significantly to the costs of dismissing workers for at least two important reasons. First, longer trials directly imply higher monetary costs for firms since in many countries the employer is required to compensate the unfairly dismissed employee with the full forgone wages and social security contributions for the length of time between the dismissal and the judge's decision. In Italy, firms also have to pay a fine to the social security system for the delayed payment of welfare contributions, up to 200 percent of the original amount due. The dependence of dismissal costs on the duration of trials also implies that firing costs can vary considerably in the country as a result of differences in courts' delays. For example, focusing on ex post firing costs and using a formula suggested by Garibaldi and Violante (2005), we find that the computed ex post firing costs are 36 months of wages in Trento (with an average length of labor trials of 313 days) versus 160 months of wages in Salerno (with an average length of labor trials of 1,397 days). Hence, the costs of dismissing a worker for a firm located in the judicial district of Salerno are more than 300 percent higher than they are in Trento.<sup>10</sup>

<sup>10</sup> Garibaldi and Violante (2005) calculate the ex post firing costs of an Italian firm with more than 15 employees that fires a blue-collar worker with 8 years' tenure as  $FC = nw + (\tau^s + \tau^h + \phi)nw + sp + lc$ , where  $n$  is the number of months it takes to reach a court decision;  $w$  is the monthly gross wage;  $\tau^s$  and  $\tau^h$  are the social and health insurance contributions, respectively;  $\phi$  is the penalty rate on forgone contributions;  $sp$  is the mandatory severance payment; and  $lc$  is legal costs. The ex post firing costs in the example are estimated in the worst possible scenario, that is, once the case has been taken to court and the judge's verdict is favorable to the worker. If we consider the probability of a settlement agreement and the fact that not all layoffs are ruled by the judge to be unfair, the computed (ex ante) firing costs fall to 15 months of wages in Trento and 65 months of wages in Salerno. However, the difference in costs between Trento and Salerno remains unchanged in relative

Second, not all costs of courts' inefficiency have a monetary dimension. The duration of labor trials can also be a source of further uncertainty for both employees and employers. Independent of the judge's final decision, as long as a suit is not settled, the full extent of the costs related to the worker's dismissal (which may also encompass the reintegration of the dismissed worker) is not known to firms; such protracted uncertainty about the future can hinder, at least temporarily, the labor-adjustment process and thus can hamper job reallocation.<sup>11</sup>

Theoretical models offer clear predictions of the effects of firing costs on employment adjustments. In a standard search-and-matching model, the searching process is costly for both firms and workers. Firing costs protect existing jobs, thus reducing job destruction; however, they also undermine job creation as firms anticipate costly dismissals. By decreasing job creation and job destruction, higher firing costs unambiguously reduce job reallocation.

Nevertheless, from a theoretical point of view, the effect of higher firing costs on productivity is less clear-cut. On the one hand, in a standard search-and-matching model, the presence of dismissal costs reduces the productivity threshold at which workers and firms decide to terminate their relationship, and this causes a decrease in firms' average productivity. On the other, given that matching workers with vacancies implies the presence of quasi rents, which are typically allocated between workers and firms through a Nash bargaining mechanism, an increase in firing costs reduces firms' outside options.<sup>12</sup> This induces a rise in the reservation productivity (below which firms do not hire) and potentially increases firms' average productivity since less productive matches are not realized (Lagos 2006; Autor, Kerr, and Kugler 2007).

There are other channels through which the presence of dismissal costs can impact firms' productivity. When firing is costly, the firm has a lower incentive to undertake risky investments with high returns and high risk of failure in order to minimize the likelihood of paying firing costs. In this respect, Bartelsman and Hinlopen (2005) find that EPL has a significant negative effect on investments in information and communication technologies. Analogously, Saint-Paul (2002) argues that high firing costs may induce secondary innovation that improves existing products instead of introducing more innovative ones. Capital accumulation is another channel through which the extent of firing costs may affect productivity. Again, an increase in firing costs has an ambiguous effect on the capital-to-labor ratio. On the one hand, stricter dismissal rules may induce a substitution effect from labor to capital (Besley and Burgess 2004). On the other, EPL strengthens workers' bargaining power and exacerbates holdup problems related

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terms. This example clearly shows that, quantitatively, trial length may represent a large component of the total firing costs.

<sup>11</sup> Bloom (2009) shows how greater uncertainty causes firms to temporarily pause their investment and employment decisions.

<sup>12</sup> When a firm is negotiating a wage with a continuing employee, the threat point in the bargaining process is the value of an unfilled vacancy minus the firing costs that the firm must pay if the negotiation is not successful.

to the investment activity, which results in less investment and capital stock per worker (Bertola 1994; Garibaldi and Violante 2005).

Finally, dismissal costs influence productivity since they affect employees' behavior and incentives. Belot, Boone, and van Ours (2007) show that an increase in the stability of the employment relationship induces workers to invest in productivity-enhancing human capital, which would otherwise be suboptimal because of the holdup problem. Conversely, using a standard model of efficiency wages, Ichino and Riphahn (2005) claim that when firing becomes more costly for the firm, workers tend to exert less effort since there is less threat of layoff in response to shirking.

### 3. Institutional Background and Identification Strategy

#### 3.1. Labor Courts in Italy

In the Italian judicial system, labor disputes are heard before the labor court system, a division of the civil court system specializing in labor suits, and can be appealed before the court of appeal. Each civil court has a seat in the main town of each province<sup>13</sup> in an area called the *circondario* (167 in Italy), while each court of appeal has a seat in the district; there are 26 districts in Italy, each grouping several court areas (*circondari*). Court districts are located in a region's main town (administrative center), with the exception of the regions Lombardy (two districts), Puglia (two districts), Calabria (two districts), Campania (two districts), and Sicily (four districts).<sup>14</sup> In Italy, labor proceedings are assigned to courts on a geographical basis. In labor disputes, the court's jurisdiction is always determined by the residence of the firm, irrespective of who initiates the legal action.

Although the labor trial takes place within the civil trial system, there are important differences between the two procedures: the labor trial is faster, and the judge has more powers of inquiry compared with the civil judge. The first instance and the appeal take place in the same district for both civil and labor trials: a decision issued in the first instance by an ordinary court may be appealed to the court of appeal in the same district to which the originating court belongs. The last instance takes place before the Court of Cassation, which is based in Rome.

Until 1998, labor courts presided over disputes involving private-sector workers only, while labor trials involving workers in the public sector took place before the administrative courts, according to the old view of public administration supremacy. In the late 1990s, a series of reforms were passed aimed at bringing public-sector employment under private-law rules; only since 1993 has employment in the public sector been on a contractual basis (and no longer the result of an administrative deed of appointment). As a part of this legislation, a 1998 law established that labor suits involving civil servants had to be settled in ordinary labor courts.

<sup>13</sup> In Italy, a province (*provincia*) is an administrative territorial unit at an intermediate level between a municipality (*comune*) and a region (*regione*).

<sup>14</sup> In this article, we refer to districts and regions interchangeably.



However, there are still important differences between public and private employment that can impact labor trials. Although in principle the law concerning the termination of labor relations applies to both sectors, there are formal and de facto features that make dismissal in the public sector a much more complex and unlikely outcome. First, in cases of dismissal for economic causes, while private-sector workers terminate their labor relationships, civil servants usually enter into a procedure (*mobilità*) designed to place them in a new public office;<sup>15</sup> this procedure aims at both improving the labor organization and curbing costs. Second, dismissals from public employment are also hindered by legal provisions that impose a special responsibility on the public-sector manager, who, in the case of unfair dismissal of a worker, can be personally liable for the economic damage caused by the dismissal. Moreover, the public-sector manager has a different status than the private-sector manager. The latter can be fired if he or she has not been able to achieve the targets set by the firm or has incurred a loss of trust; on the contrary, the public-sector manager has the same juridical status as the employee and hence cannot be dismissed for poor performance or lack of trust. This provision adheres to the concept of stability of employment in the public sector, which is a major feature of Italian public administration.<sup>16</sup>

### 3.2. *The Identification Strategy*

#### 3.2.1. Firing Costs and the 15-Employee Threshold

According to Italy's Workers' Statute (L. May 20, 1970, n. 300), an (individual) instance of dismissal is legal only when it satisfies a just cause; that is, it can be justified by an objective reason (concerning production activity, for example) or subjective reasons, which are mainly related to misconduct on the part of the worker. The worker always has the right to appeal the firm's decision, and the final outcome ultimately depends on the court's ruling in the case. If the worker does not appeal the firing decision, or if the dismissal is ruled fair, the legislation does not impose any severance payment on the firm.<sup>17</sup> Conversely, when the dismissal is ruled unfair, the judge imposes specific compensation on the firm.

The maximum compensation to which an unlawfully fired worker is entitled varies with firms' size in two important dimensions. For firms with up to 15 employees, the unfairly dismissed worker must be compensated with a fixed severance payment that varies between 2.5 and 6 months of salary regardless of the length of the judicial procedure and with no obligation of reinstatement.

<sup>15</sup> Only after 2 years of *mobilità*, if the procedure has been unsuccessful or the worker has refused, can the employment relationship be terminated.

<sup>16</sup> The protection granted to civil servants—which is different for private-sector workers—is the subject of intense debate in Italy. In many cases, the idea of stability of employment in the public sector also has an impact on the productivity of civil servants; for example, from 2004 to 2008 the difference in the average annual rate of absenteeism between public-sector and private-sector workers was around 32 percent.

<sup>17</sup> When the layoff is ruled fair, a common practice in Italy is for the labor union to pay the legal costs.

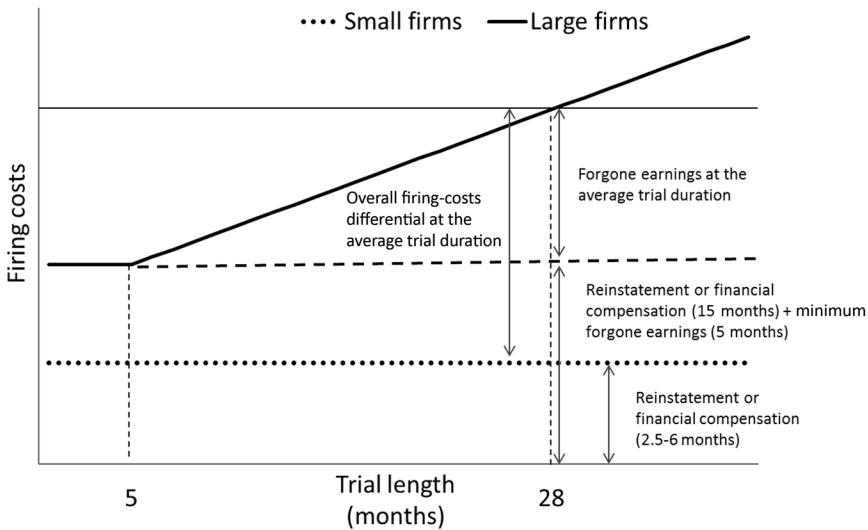


Figure 1. Expected firing costs for large versus small firms

Conversely, for firms with more than 15 employees, to which article 18 of the Workers' Statute applies, the worker is entitled to compensation equal to forgone wages and social security and health insurance contributions from the date of the dismissal to the judicial settlement of the case (with a minimum of 5 months and no upper limits). Moreover, he or she can choose either to be reinstated at the firm or to be paid an additional financial compensation of 15 months of salary.<sup>18</sup> This implies that firing costs for firms above the 15-employee threshold are always higher than those for smaller firms and that the costs of unfair dismissals increase with the duration of labor trials only for firms with more than 15 employees. Therefore, the monetary burden of lengthy judicial proceedings is substantially higher for firms with more than 15 employees.

Figure 1 illustrates the firing-cost structure for small firms (*dotted line*) and large firms (*solid line*) depending on the trial's duration.<sup>19</sup> Large firms pay higher firing costs than smaller firms for any length of trial as the minimum level of

<sup>18</sup> The recent reform of the Italian labor market, in force since July 18, 2012, has changed some of the rules relating to the termination of the employment relationship. In particular, for firms with more than 15 employees, reinstatement is restricted to certain specific cases of unfair or unjustified dismissal, and the compensation a firm has to pay in cases of unfair dismissal now has an upper limit of 24 months' salary. The change in legislation does not affect our estimates, since our data cover the period 2006–10.

<sup>19</sup> The line for firing costs for large firms is based on the formula from Garibaldi and Violante (2005) (see note 10). The fixed part (the intercept) represents the mandatory payment, that is, reinstatement or financial compensation and minimum forgone earnings ( $sp$  in Garibaldi and Violante [2005]) plus fixed legal costs ( $lc$ ). The slope of the line represents the sum of gross monthly wages ( $w$ ), the monthly social and health insurance contribution ( $\tau^s w$  and  $\tau^h w$ ), and the penalty on forgone wages ( $\phi w$ ). Firing costs for small firms are flat (that is, they are not affected by the duration of trial) and always below the fixed component of firing costs for large firms.

firing costs for large firms (*dashed line*) is always above that for smaller firms. If a trial lasts less than 5 months, the firing-costs differential between the two groups is constant. If, however, the unfavorable verdict for the firm comes after 5 months, the firing-costs gap widens since the forgone-earnings component of firing costs increases for large firms but not for smaller ones.

Previous studies that exploit the discontinuity in EPL at the 15-employee threshold are mainly focused on identifying the overall effect of EPL on firms' employment decisions and productivity, without distinguishing between the two dimensions of firing costs we discuss above. The identification strategy adopted in those works is based on the overall firing-costs differential between large and small firms at the average duration of labor trials in Italy (28 months in our sample), which is just one value on the kinked curve in Figure 1. Our empirical strategy, in contrast, allows us to identify the extent to which the inefficiency of the judicial system affects firms' firing costs in addition to other possible components of firing costs due to the presence of EPL, which, in our econometric specification, is absorbed by the presence of firm fixed effects. In particular, our identification scheme is based on the idea that different firms (below and above the threshold) are affected by trial length in different ways; that is, longer trials directly translate into higher monetary costs for firms with more than 15 employees but not for smaller firms. Lengthy trials do not imply any additional costs to small firms, since the maximum compensation to be paid in cases of unfair dismissal is fixed and known *ex ante* by the firm. This implies that small firms may take advantage of courts' inefficiency through discounting, since the duration of a labor trial would imply a delay in the payment of the fixed compensation due to the dismissed worker.<sup>20</sup> The discontinuity in legislation regarding firing at the 15-employee threshold should allow us to isolate the effect of interest (the impact of labor trials' length on productivity) from other (unobserved) factors such as the degree of local economic development and the quality of (local) institutions that affect all firms within the same judicial districts.

### 3.2.2. The Instrumental Variables

To corroborate our results and control for the potential endogeneity of the indicator of judicial inefficiency, we present two-stage least squares (2SLS) estimates using the following instruments for the duration of labor trials. These include the appeal rate of courts' decisions (that is, the ratio of the number of incoming

<sup>20</sup> Given the probability that a separation is ruled by the judge to be unfair, the expected firing costs of small firms vary from 0 (in cases with a favorable verdict) to a maximum of 6 months of forgone earnings. A longer trial horizon then reduces the expected discounted value of firing costs (and their variance) because of the discounting effect. Therefore, even when small firms are risk adverse, lengthy labor trials have a potential positive effect on firms' turnover and productivity. In contrast, for large firms, the increase in time horizon has two opposite effects on the expected value and variance of firing costs. On the one hand, it affects large firms' expected firing costs and variance through a positive direct effect on workers' compensation in cases with unfavorable verdicts. On the other, the negative discounting effect is still at work as in small firms. We control for both effects in our estimations.

suits in the appeal stage to the number of outgoing suits in the first-instance stage) regarding the labor disputes of civil servants and the number of vacant positions in courts at the district level.

The rate of appeal of courts' decisions concerning civil servants' labor disputes is correlated with trial length for private-sector workers, as it contributes to the overall bulk of disputes that are to be settled by (labor) courts: higher numbers of appeals imply more cases to be handled by courts and thus more congestion. Our instrument does not suffer from reverse causality (more job reallocation—in the private sector—may increase the number of labor suits and hence give rise to longer trials), nor is it a factor that matters for firms' adjustment decisions. However, a possible source of bias could still arise if the appeal rate of courts' decisions for civil servants' work disputes were driven by district-level (unobserved) variables that also affect our dependent variables. This could happen if, for example, the appeal rate, which can be taken as a proxy of the quality of courts' decisions, were correlated with the quality of (local) institutions, which may also affect firms' productivity. If this were the case, we should, however, also expect a significant degree of correlation between the rate of appeal for civil servants' suits and the rate of appeal for private-sector workers. This does not seem to be the case, as the districts with relatively high appeal rates for public-sector workers' suits tend to differ significantly from those with high appeal rates for private-sector workers' suits; moreover, the correlation rate between the two variables is negative and not significant, as shown in Figure 2.<sup>21</sup> This suggests that omitted variables that can also affect firm-level outcomes (territorial cultural or economic patterns) might not be a major concern.

Our second instrument is the number of vacant judicial positions at the district level, that is, positions in the organograms that are left vacant (for transfer of a judge) and not yet filled. This instrument is correlated with the length of trials (the fact that some positions in a district may be unfilled increases courts' congestion in that district) and satisfies the exclusion restriction, as the transfer of judges from one office to another is the result of a number of decisions made by agents at different levels of the judicial hierarchy who respond to different sets of incentives. The transfer generally follows a three-step procedure: the publication of vacant positions to be filled, the request of a judge who is willing to be transferred to the vacant position, and the approval by the self-governing body of the judiciary, the CSM.

Vacancies in judicial districts arise primarily because of transfers of judges to other districts or offices, for example, through career advancements, or retirement. Once a vacant position is created, a judge who is willing to be transferred has to apply to the CSM; as a general rule, judges cannot be transferred to dif-

<sup>21</sup> The lack of a significant correlation between the rate of appeal of courts' decisions in public- and private-sector labor trials suggests that the demand for justice by civil servants and private employees is not driven by the same factors at the court level. However, labor courts' congestion or courts' inefficiencies affect trial delays for both categories of workers, which implies a high correlation between the length of trials involving private employees and civil servants' disputes.

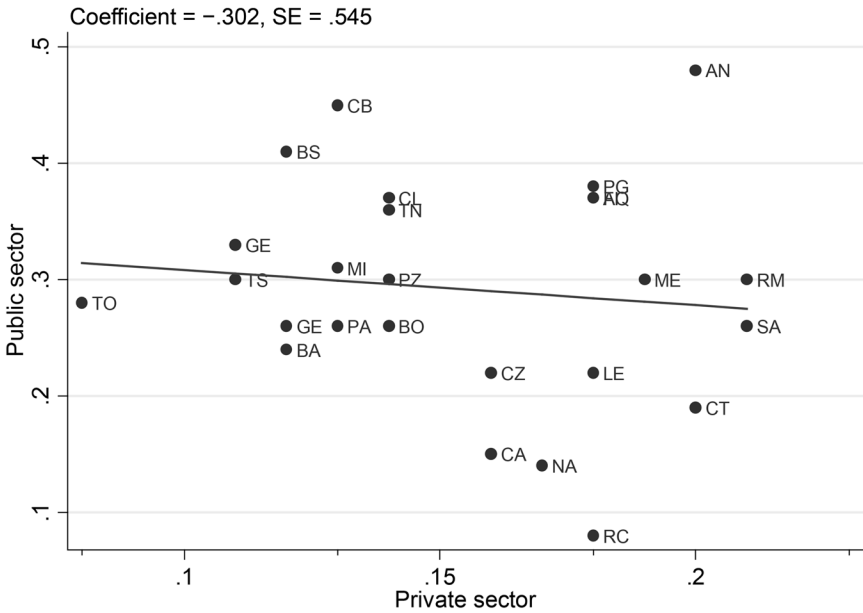


Figure 2. Appeal rates of public- and private-sector suits

ferent assignments or districts without their consent.<sup>22</sup> Once applications are received, the CSM decides on the basis of a competitive procedure among candidates. The criteria for the CSM's collegial decision are the following: competence, which is assessed on the basis of the functions so far carried out and the judge's capacities; the judge's health status and that of his or her family members (offspring, spouse, parents, brothers and/or sisters if living with the judge, and in some cases relatives and relatives-in-law); family ties; merit (which includes the judge having occupied vacancies in critical districts or vacancies for which no application was received); and seniority (see CSM circular 15098, November 30, 1993, and amendments).<sup>23</sup> Therefore, the complexity of the transfer procedure, to which the decisions taken by different agents contribute, is such that the number of vacant positions in each district ends up being independent from (local) factors that might also affect firm-level outcomes.

<sup>22</sup> An important feature of the Italian judicial system is the principle of *inamovibilità*, according to which a judge can be transferred to a different court or to a different assignment only on his or her consent. The principle of *inamovibilità* is a constitutional provision aiming at assuring the independence of the judiciary, which could be undermined should a judge be compelled to quit his or her activity for suspension or transfer. There are some exceptions, namely, the need to cover vacancies in cases established by law, those resulting from disciplinary actions, or vacancies for reasons of *incompatibilità ambientale* (that is, the judge is considered incompatible with the workplace). The judge can appeal to the Consiglio Superiore della Magistratura (CSM) for a decision in all cases.

<sup>23</sup> Health status and family ties are not taken into consideration for top positions, such as the Supreme Court.

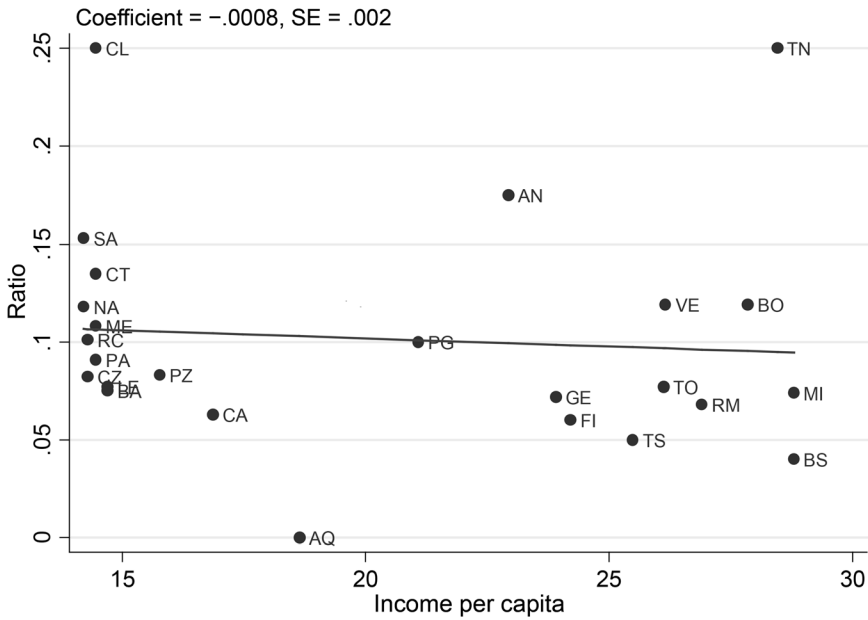


Figure 3. Ratio of vacancies to judges and income per capita by judicial district

This conclusion is also supported by the data in our sample (see Table 1), which show no clear territorial pattern in the number of vacant positions (expressed both in units and as a ratio of the total number of judges). Moreover, Figure 3 displays the sample correlation between the average number of vacant positions (normalized to the number of judges in office) and the level of per capita income (as a proxy for the degree of local economic development). The correlation is virtually 0. This supports our conclusion that the opening and filling of a vacant position in the courts is a complex process that is not related to local institutional and economic characteristics.

We use both instruments in the instrumental variables (IV) regressions together with a full set of district and district-year dummies (depending on the specification considered); the results are remarkably robust in every specification.<sup>24</sup> Moreover, we control for differences in the economic development at the district level by including among the regressors the district's per capita income (gross domestic product, or GDP) and for the overall quality of the judiciary by controlling for the length of civil trials. Finally, depending on the specification considered, we show that our results are also robust to the inclusion of a full set of district-year and sector-year dummies. In this way, we can rule out any possible source of bias arising from (time- and district-variant) omitted factors—not al-

<sup>24</sup> The results are also remarkably robust to using the two instruments separately. Results are available from the authors on request.

ready captured by the regional income per capita—that could influence productivity and judicial inefficiency, which thus yields further support to our identification strategy.

### 3.2.3. Sector-Reallocation Requirement

We finally provide additional evidence of causality by exploiting the industry dimension of our data and applying the approach in Rajan and Zingales (1998). The basic idea is that if courts' delays affect firms' productivity through the firing-costs channel, then the effect should be larger for firms operating in sectors with a higher reallocation requirement, which in turn depends on the technological characteristics or on the incidence of aggregate shocks (Bertola 1992).

A major issue is to define a measure of a sector's reallocation requirement that is not related to employment-protection provisions. In line with a number of labor studies adopting a similar approach (Micco and Pagés 2004; Bassanini, Nunziata, and Venn 2009; Cingano et al. 2010), we use the average job-reallocation rates computed at the industry level over 1992–2000 for a frictionless labor market (in our case, for the United Kingdom) as a proxy for the intrinsic reallocation requirement of a particular industry. The United Kingdom appears to be a natural benchmark because the UK labor market is much less regulated than other OECD countries. Our testable hypothesis is that firms in sectors with a higher degree of intrinsic reallocation requirement are more affected by the increase in firing costs induced by labor courts' delays.

## 4. Data Sources and Descriptive Statistics

### 4.1. Courts' Data

The Italian Ministry of Justice publishes annual data on labor trials for private- and public-sector workers at the district level; our data cover the period from 2007 to 2010.<sup>25</sup> Data are available on the flows of suits initiated during the year, the flows of suits that are closed every year, and the stock of pending suits every year in the first-instance (FI) and the appeal (A) stages for each of the 26 Italian judiciary districts. Following a standard formula used by the Ministry of Justice and the Italian National Institute of Statistics (ISTAT), the average number of days of trial can then be calculated as the ratio of the stock of cases (pending cases at the beginning plus pending cases at the end of the year) to the incoming plus outgoing flows (newly filed plus closed cases).<sup>26</sup>

<sup>25</sup> Jappelli, Pagano, and Bianco (2005) use similar data to estimate the effects of judicial inefficiency on credit markets.

<sup>26</sup> Since data on the duration of legal proceedings are not available, the index of trial duration is calculated as  $DLT_t = [(P_{t-1} + P_t)/(F_t + C_t)] \times 360$ , where  $P_{t-1}$  and  $P_t$  are the numbers of cases pending at the beginning and at the end of the year, respectively;  $F_t$  is the number of new cases filed during the year; and  $C_t$  is the number of cases that reached final judgment during the year (for methodological details from the Italian National Institute of Statistics, see ISTAT [2001]). This measure has been widely used in the economic literature in both cross-country and within-country studies. See, for example, Djankov et al. (2003) for a cross-country study and Jappelli, Pagano, and

This formula allows us to estimate the number of trial days in each stage of judgment. To take account of the overall number of days of trial in the first-instance and the appeal stages using the same criterion, one should sum the pending cases at the beginning and the end of the year in the two stages and divide by the sum of the inflows and outflows in the two stages. However, this procedure has a drawback, as it does not take into account the sequentiality of the two trials, that is, the fact that a suit closed before the court of first instance may or may not enter the appeal phase.

To account for the sequentiality of the two trials, we sum the average days of trial for the first instance and for the appeal (calculated using the Ministry of Justice's formula), where the appeal days are weighted by the number of incoming suits to the appeal stage relative to the number of outgoing suits in the first-instance stage:

$$\text{Length} = \text{DLT}_{rt}^{\text{FI}} + S_{rt} \times \text{DLT}_{rt}^{\text{A}}, \quad (1)$$

where DLT is the number of days of labor trials in the first instance (FI) and in the appeal (A) in district  $r$  at time  $t$  and  $S$  is the ratio of the newly filed suits before the appeals courts in district  $r$  at time  $t$  to the suits closed at first instance in district  $r$  at time  $t - 1$ . The term  $S$  ranges from 0 to 1 and takes into account the fact that not all the suits that are decided by the court of first instance reach the appeals courts. Therefore,  $S$  can be interpreted as a proxy for the probability that the first-instance judgment is appealed.<sup>27</sup>

From the Ministry of Justice database, we also draw annual data on the length of civil trials and on the rate of appeal in labor courts in suits involving civil servants, that is, the ratio of the incoming suits in the appeal stage to the outgoing suits in the first-instance stage. Finally, annual data on the number of judges appointed to courts at the district level and on the positions left vacant for transfer of a judge from 2007 to 2010 are taken from the CSM database.<sup>28</sup> Data on courts are matched with information about income and population at the district level provided by ISTAT.

Table 1 reports some descriptive statistics for the courts; in all cases the tables present average annual data from 2007 to 2010. Trial length is our indicator of judicial efficiency in relation to the private-sector labor trials for the 26 judicial districts. The data show great territorial heterogeneity in the duration of trials; for example, the length of trials in the least efficient district (Bari, 1,433 days)

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Bianco (2005), Giacomelli and Menon (forthcoming), and Coviello et al. (forthcoming) on the effect of Italian courts' efficiency on the performance of credit markets, firm size, and procurement performance.

<sup>27</sup> Our index does not consider the additional days of trial (which add to the total length) because of the possibility that suits are discussed before the Supreme Court. At this stage no heterogeneity could be observed at the district level; hence, days of trial calculated according to our index can understate the effective length of trial. In addition, our index does not take into account the period between the first suit and the appeal. However, the time that elapses between the two stages also depends on the decision of the party who appeals, which is not related to the courts' efficiency.

<sup>28</sup> The CSM database does not provide information about vacant positions for the judicial district of Campobasso.



Table 1  
Descriptive Statistics for the Sample

District	Trial Length (days)		Appeal Rate		Vacancies			
					N		Per Number of Judges	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Turin	224	18.32	.28	.05	1.75	.96	.08	.05
Trento	313	43.72	.36	.09	.75	.50	.25	.17
Milan	389	43.84	.31	.04	2.75	1.50	.07	.04
Genoa	492	80.41	.26	.07	1.25	1.50	.07	.09
Campobasso	499	91.41	.45	.20				
Brescia	554	54.01	.41	.03	.50	.58	.04	.05
Florence	646	56.61	.37	.06	1.25	.96	.06	.05
Trieste	735	87.08	.30	.05	.25	.50	.05	.10
Bologna	768	47.79	.26	.04	2.25	.96	.12	.05
Catanzaro	777	83.77	.22	.13	1.50	.58	.08	.03
Ancona	782	116.23	.48	.05	1.00	1.41	.17	.21
Rome	826	63.25	.30	.11	7.25	3.30	.07	.03
Venice	827	69.79	.33	.08	2.25	1.71	.12	.09
Palermo	875	150.99	.26	.10	1.25	1.26	.09	.10
Naples	889	57.56	.14	.07	12.00	2.94	.12	.04
L'Aquila	913	86.40	.37	.10	.00	.00	.00	.00
Caltanissetta	1,046	274.34	.37	.12	.50	1.00	.25	.50
Potenza	1,080	109.81	.30	.15	.25	.50	.08	.17
Cagliari	1,098	46.40	.15	.04	.50	1.00	.06	.13
Perugia	1,165	134.62	.38	.13	.50	.58	.10	.12
Reggio Calabria	1,177	96.37	.08	.01	1.00	.82	.10	.08
Messina	1,200	148.80	.30	.11	1.00	1.41	.11	.16
Catania	1,309	57.34	.19	.05	2.50	1.29	.14	.08
Lecce	1,325	282.44	.22	.07	2.00	.82	.08	.03
Salerno	1,397	71.53	.26	.07	2.25	1.26	.15	.10
Bari	1,433	288.06	.24	.25	2.50	.58	.08	.02
Average	852.84	102.34	.29	.09	1.96	1.12	.10	.10
Overall sample		354.32		.13		2.75		.13

Source. Ministry of Justice website and database and authors' calculations.

Note. The length of labor trials excludes the appeal stage argued before the Supreme Court. Districts are ordered from most efficient to least efficient. Values for averages are the sample mean and the within-district (average) standard deviation.

is more than six times longer than the most efficient district (Turin, 224 days). The standard deviation across courts is quite large, being more than one-third of the average. The within-district time-series variation of labor trials' length is also substantial: the standard deviation normalized on the mean ranges from .04 (in Cagliari) to .26 (in Caltanissetta), with a sample average of around .12. This descriptive evidence suggests considerable heterogeneity in labor courts' efficiency (and law enforcement) both cross-sectionally and over time.

Values for the rate of appeal of labor suits involving public-sector workers show that extreme values are recorded in Reggio Calabria (.08) and Ancona (.48), while the rate of appeal in Turin is close to the average, and Bari falls a few posi-

tions behind it. Finally, vacant positions in courts at the district level are shown in absolute values and normalized to the number of judges. Note that the numbers of vacant positions in Naples (12) and in Rome (7.25) are much higher than the average; this is explained by the difference in the sizes of the courts, which are much larger in Naples and Rome. When the number of judges is taken into account, the number of vacancies in Naples lies slightly above the average (.12 versus .10), while Rome's is below average (.07).<sup>29</sup>

#### 4.2. *Firms' Data and Job-Flow Statistics*

Firm-level data are drawn from the *Analisi Informatizzata delle Aziende Italiane* (AIDA) produced by Bureau van Dijk. Bureau van Dijk collects annual balance-sheet data from the national chambers of commerce. The version of AIDA used in our analysis includes Italian firms that reported their financial statements to the national chambers of commerce in the period 2007–10, for a total of more than 800,000 firms operating in all sectors of production. Apart from balance-sheet data, AIDA provides a wide range of financial and descriptive information (industry and activity codes, firms' age, and so on) and the number of employees. Moreover, AIDA contains information about the location of firms at the municipality level, which allows us to match firms' data with the courts' database.

The AIDA database has a drawback, as it does not allow us to distinguish between newly created firms and firms that enter the sample at a given period  $t$  but were operating in the previous period; similarly, it is not possible to distinguish firms that close from firms that exit the sample for other reasons. Therefore, we restrict the analysis to continuing firms, namely, those that are in the sample for at least 2 consecutive periods. Given this limitation and after the data are cleaned of outliers and missing information, our final sample consists of around 160,000 private firms operating in 20 manufacturing and nonmanufacturing sectors.<sup>30</sup> Labor productivity is calculated as value added per worker.

Since data on job creation (hirings) and job destruction (separations) at the firm level are not available from AIDA, we follow the literature (Salvanes 1997; Gómez-Salvador, Messina, and Vallanti 2004) and calculate yearly job creation (JC), job destruction (JD), and job reallocation (JR) rates at the district-industry level using the number of employees at the end of the budget year. For each sector, district, and year, JC is the weighted sum of employment gains over growing firms, JD is the weighted sum of employment losses (in absolute value) over con-

<sup>29</sup> In our estimations, we account for the differences in the size of the courts by using district and firm fixed effects, depending on specification.

<sup>30</sup> The 20 sectors are agriculture, forestry, and fishing; mining and quarrying; food, beverages, and tobacco; textiles; wood products; paper products, publishing, and printing; refined petroleum, nuclear fuel, and chemical products; rubber and plastic products; other nonmetallic products; basic metals and fabricated metal products; machinery and equipment; electrical and optical equipment; transport equipment; other manufacturing sectors; electricity, gas, and water supply; construction; wholesale and retail trade, repairs; hotels and restaurants; transport and communications; and other services. We exclude financial and public sectors from the analysis.

Table 2  
Descriptive Statistics for Firms and Job Flows

	Mean	SD	10th Percentile	50th Percentile	90th Percentile
Value added per worker (€1,000s)	63.94	298.99	27.84	50.51	114.00
Firm size	33.94	505.08	2	9	52
>15 Employees (dummy)	.29	.45			
Firm age	10.65	6.14	5	17	26
JR	.074	.038	.035	.065	.128
JC	.034	.024	.010	.030	.068
JD	.040	.032	.011	.032	.080
Reallocation requirement	.099	.020	.069	.088	.135

Source. Analisi Informatizzata delle Aziende Italiane database and authors' calculations. The reallocation requirement at the industry level is constructed using data from Messina and Vallanti (2007).

tracting firms, and JR equals the sum of JC and JD.<sup>31</sup> Job-flow statistics are then defined for narrow cells, obtained as the crossing of 20 productive sectors, 20 districts, and 4 years (2007–10).

Finally, data on gross job reallocation for the United Kingdom (the frictionless economy) are from the Messina and Vallanti (2007) job-flow database, which provides cross-country comparable job-flow statistics for 24 sectors in 13 EU countries for 1992–2001. This indicator is industry specific and time invariant and is constructed as the average job-turnover rate in the United Kingdom for each sector over the period 1992–2000.

Table 2 reports summary statistics for firms' variables, JC, JD, JR, and the reallocation requirement. The average firm is 11 years old, has a value added per worker of around €64,000, and employs 34 workers. Firm-size distribution is significantly skewed, as shown by the low value of the median, which is nine employees. A major feature of the Italian productive structure is that the size of firms is quite small. In our sample, 71 percent of firms have 15 or fewer workers, and microfirms (with fewer than 6 workers) account for around 44 percent of the sample. These figures show that firms below the 15-employee threshold are well represented.<sup>32</sup>

Finally, the average rates of JC and JD are around 3 percent, with an overall job turnover equal to 7.4 percent. As expected, JR in the frictionless economy (the United Kingdom), which is our proxy for industry reallocation requirement, is almost 3 percentage points higher than it is in Italy. From Table 3, which reports the reallocation requirement by production sector, the research and development

<sup>31</sup> We use the standard Davis, Haltiwanger, and Schuh (1996) methodology to compute job-flow statistics. Technical details can be found in the online appendix.

<sup>32</sup> Our sample's characteristics and coverage are in line with those employed in Hijzen, Mondauto, and Scarpetta (2017), which uses data from administrative sources (In Practice Systems and the ISTAT Archivio Statistico delle Imprese Attive database). Although our database and that used in Hijzen, Mondauto, and Scarpetta (2017) are from two different sources, they are very similar in terms of the average number of employees, industries, and geographical coverage.

Table 3  
 Reallocation Requirement by Industry

	Flex
Electricity, gas, and water supply	.066
Paper products, publishing, and printing	.069
Refined petroleum, nuclear fuel, and chemical products	.078
Other nonmetallic products	.082
Food, beverages, and tobacco	.084
Transport and communications	.085
Wood products	.086
Basic metals and fabricated metal products	.086
Textiles	.087
Rubber and plastic products	.088
Transport equipment	.089
Machinery and equipment	.089
Other manufacturing sectors	.094
Electrical and optical equipment	.098
Wholesale and retail trade and repairs	.101
Mining and quarries	.105
Hotels and restaurants	.112
Agriculture, forestry, and fishing	.119
Construction	.135
Real estate and renting: computer research and development	.145
Average	.099

Source. Messina and Vallanti (2007).

of computer activities for real estate and renting emerges as the industry with the highest flexibility index, with construction following close behind it. On the opposite side of the flexibility ranking, we find electricity, gas, and water supply and the paper products, publishing, and printing sector.

## 5. Empirical Methodology

### 5.1. Job Flows

As discussed above, labor-trial delays increase the cost of dismissing a worker and the uncertainty about the outcome of a judge's decision; in this way they increase firing costs and hinder labor reallocation. To isolate the effect of trial length on firms' adjustments, we estimate the effect of our variable of interest on job reallocation and then on job creation and job destruction separately.

Apart from firing costs, labor adjustments can be influenced by other institutional and economic factors that could in principle also affect judicial inefficiency. Although district fixed effects allow us to control for unobserved heterogeneity at the district level, in the regressions we also control for (time-variant) differences in real per capita income among districts to account for potential endogeneity resulting from the influence of underlying economic conditions on the variables included in the model. Differences in the pace of development may indeed have an impact on the quality of institutions (judicial efficiency) and their outcomes.

Finally, we also control for the judicial inefficiency in ordinary civil trials<sup>33</sup> at the district level, which may impact firms' decisions related to input adjustments.<sup>34</sup>

Since job-flow statistics are measured at the industry-district level, the model specification is

$$JF_{srt} = \beta_1 \text{Length}_{rt} + \beta_2 \text{Civil}_{rt} + \beta_3 \text{Income}_{rt} + \beta_4 \mathbf{X}_{srt} + \mathbf{D}\eta + u_{srt}, \quad (2)$$

where  $JF_{srt}$  is the set of all job-flow rates (that is, JR, JD, and JC);  $\text{Length}_{rt}$  is the log length of labor trials;  $\text{Civil}_{rt}$  is the log length of civil trials;  $\text{Income}_{rt}$  is the log of real per capita income;  $\mathbf{X}_{srt}$  is a set of industry-district time-variant firm (average) characteristics; the indices  $s$ ,  $r$ , and  $t$  refer to the industry, the district, and the time period, respectively;  $\mathbf{D}$  is the matrix of dummies that includes district-by-year and industry-by-district dummies; and  $u_{srt}$  is the error term. Industry-by-year dummies control for differential trends in job flows by type of economic activity; for example, throughout all districts, some industries may experience faster job reallocation than others. Industry-by-district dummies capture cross-district differences in the structure of each industry.

Since the intensity of job reallocation depends on various firm-specific characteristics, with job creation being negatively associated with firms' age and size, the set of controls  $\mathbf{X}_{srt}$  includes the log of the average age of the firm (Age) in each industry-district-year cell and dummies for the three size groups: 16–50 employees (Size16–50), 51–250 employees (Size51–250), and more than 251 employees (Size251–). Cells with an average firm size of 15 or fewer employees represent the base group. Size dummies are defined on the basis of the average firm size in each industry-district-year cell. As discussed in Section 3.2.2, Length is instrumented with the number of vacant judicial positions and the rate of appeal of the public-sector labor trials.

## 5.2. Labor Productivity

In a second set of regressions, we estimate the effect of labor courts' inefficiency on firms' labor productivity using firm-level data. At the firm level, the size of firms plays a significant role in moderating the impact of courts' inefficiency on productivity. We exploit the fact that, according to article 18 of the Workers' Statute (L. May 20, 1970, n. 300), firms with more than 15 employees have to reinstate workers and pay their forgone wages for the entire period of the judicial procedure in cases of unfair dismissal. In contrast, firms with 15 or fewer employees have to pay a fixed severance payment without any reinstatement. The legal costs of unfair dismissals for firms below the threshold of 15 employees are then unrelated to the length of trials, while the expected firing costs increase with trial length for firms above the 15-employee threshold. We exploit the disconti-

<sup>33</sup> Ordinary civil trials include private disputes between individuals or organizations with the exclusion of labor disputes.

<sup>34</sup> Per capita income at the district level and the duration of ordinary civil trials may themselves be endogenous. However, our results are not affected by the exclusion of these two controls from our baseline specifications. Regressions are available from the authors on request.

nity of legislation at the 15-employee threshold to identify the causal effect of trial length on productivity.

As in the job-flow regressions, to control for the fact that labor courts' inefficiency can be related to local economic development and the inefficiency of courts at large, we also include district per capita income and the length of civil trials among the controls. Moreover, estimating the impact of judicial inefficiency at the firm level allows us to enrich our controls on productivity. Given that (time-variant) differences in the regional underlying economic conditions (as GDP per capita) may not necessarily capture all the factors affecting firms' productivity (such as those related to the institutional environment at large), depending on the specification considered we include a full set of district-by-year and sector-by-year dummies. In this way we can rule out any possible source of endogeneity arising from (time- and district-variant) omitted factors—not already captured by income per capita—that could influence productivity and judicial inefficiency.

The model specification is

$$Y_{fst}^r = \beta_1 \text{Length}_t^r + \beta_2 (\text{Length}_t^r \times \text{Size}_f^r) + \beta_3 \text{Civil}_t^r + \beta_4 \text{Income}_t^r + \eta_f + \mathbf{X}_{sft}^r \gamma + \mathbf{D}\eta + u_{fst}^r, \quad (3)$$

where  $Y_{fst}^r$  is the log of labor productivity;  $\text{Size}_f^r$  is a dummy that takes a value of one for firms with more than 15 employees and zero otherwise;<sup>35</sup>  $\mathbf{X}_{sft}^r$  is a set of other controls;  $\eta_f$  is firm fixed effects that absorb any time-invariant unobservable attributes at the firm level and in particular the effect of the different (time-invariant) EPL regime that applies to firms above and below the 15-employee threshold; the indices  $s$ ,  $r$ , and  $t$  indicate industries, districts, and time, respectively; and  $\mathbf{D}$  is the matrix of dummies that includes, depending on the specification considered, district-by-year<sup>36</sup> and industry-by-year dummies. The coefficient on  $\text{Length}_t^r$  gives the common effect of trial length on small and large firms due to discounting, while the term  $\text{Length}_t^r \times \text{Size}_f^r$  captures the differential effect of firing costs induced by the length of trials on large firms' productivity. Here again Length is instrumented with the number of vacant judicial positions and the ap-

<sup>35</sup> Although the threshold of 15 employees is clearly set by Italian labor law, the employment cut-off may be imprecisely estimated at the firm level because of the complex calculation of the workforce (for example, part-time and atypical workers) and measurement errors (for a discussion, see Hijzen, Mondauto, and Scarpetta 2017). We define firms as small if they have fewer than 15 employees and large if they have more than 15 employees in all years of the sample period. See Kugler and Pica (2008) for analogous definitions of control and treatment groups. We check the robustness of our results by using an alternative cutoff threshold that defines the control group as firms with fewer than 10 employees and the treatment group as firms with more than 20 employees. We also show that our results are robust to changes in the sample by restricting the analysis to firms closer to the threshold. See the online appendix for these results.

<sup>36</sup> The inclusion of district-by-year dummies allows us to control for all district-specific time-varying characteristics (for example, the quality of local infrastructure), which have the same effects across firms. Notice that this set of dummies absorbs the main effect of trial length, as it varies only by district and time.

peal rate of trials in public-sector labor suits, while  $\text{Length}_t^r \times \text{Size}_f^r$  is instrumented by interacting each instrument with the dummy  $\text{Size}_{fs}^r$ .

Our identification strategy relies on the assumption that firms do not endogenously sort in or out of treatment. In specification (3), firm fixed effects capture all time-invariant unobserved factors that may affect the propensity of firms to self-select above or below the threshold. However, firm fixed effects cannot account for the selection induced by time-varying factors such as courts' delays. In our analysis, the possible selection bias due to sorting around the threshold is mitigated by the fact that we assign firms to the control and treated groups if they stay below or above the threshold, respectively, in all years of the sample period. Moreover, the firm-growth regressions reported in Section 7.3 suggest that courts' delays do not affect firms' propensity to grow around the threshold and therefore lend further support to our identification strategy.<sup>37</sup>

We finally check the robustness of our results by applying the approach from Rajan and Zingales (1998). Our testable hypothesis is that the differential effect of firing costs related to court delays for firms above and below the 15-employee threshold is larger in industries with a higher flexibility requirement.

The model specification is

$$Y_{fst}^r = \beta_1(\text{Length}_t^r \times \text{Flex}_s) + \beta_2(\text{Length}_t^r \times \text{Size}_f^r) + \beta_3(\text{Length}_t^r \times \text{Size}_f^r \times \text{Flex}_s) + \mathbf{X}_{fst}^r \gamma + \eta_f + \mathbf{D}\eta + u_{fst}^r, \quad (4)$$

where  $\text{Flex}_s$  is the extent of intrinsic job reallocation in industry  $s$ .<sup>38</sup> The coefficient  $\beta_3$  of the third-level interaction term captures the differential effect of lengthy labor trials on productivity for firms above and below the 15-employee threshold in sectors with a different reallocation requirement. If our assumption is correct, the differential effect is negative and increases (in absolute terms) with the sector-flexibility requirement ( $\text{Flex}_s$ ); that is, the coefficient  $\beta_3$  is negative.

## 6. Results

### 6.1. Courts' Delays and Job Reallocation

We first estimate the impact of labor-trial length on JR, JD, and JC as in equation (2). Table 4 reports results of regressions using the instruments discussed in the previous sections—namely, vacant judicial positions and the rate of appeal in suits involving only public-sector workers.<sup>39</sup>

The IV results in Table 4 show that the coefficient on labor-trial length is nega-

<sup>37</sup> The result that EPL provisions do not affect firms' propensity to grow is not new in the empirical literature. See, among other studies, Cingano et al. (2016), Leonardi and Pica (2013), and Schivardi and Torrini (2008), which examine the effect of EPL on firms' size distribution below and above the 15-employee threshold in Italy.

<sup>38</sup> The regressors  $\text{Size}_f^r$ ,  $\text{Flex}_s$ , and their interaction are time invariant; therefore, they are absorbed by firm fixed effects.

<sup>39</sup> The Stata module `xivreg2` (Schaffer 2015) is used for all instrumental variables regressions in this article.

Table 4  
The Effect of Labor-Trial Length on Job Flows

	Ordinary Least Squares			Instrumental Variables		
	JR (1)	JD (2)	JC (3)	JR (4)	JD (5)	JC (6)
Length	-.012** (.004)	-.007 (.004)	-.008* (.003)	-.070** (.021)	-.059** (.023)	-.011 (.008)
Civil length	.021 (.015)	.021+ (.015)	-.001 (.011)	.069* (.025)	.079* (.028)	-.010 (.019)
Income	-.010 (.024)	.015 (.023)	-.025 (.018)	.009 (.015)	-.016 (.026)	.025 (.022)
Size16-50	-.003 (.003)	-.002 (.003)	-.001 (.002)	-.001 (.003)	.001 (.003)	-.002 (.002)
Size51-250	-.004 (.004)	-.006 (.004)	.002 (.003)	-.003 (.003)	-.004 (.004)	-.002 (.003)
Size251-	-.007 (.005)	-.010+ (.005)	.002 (.004)	-.006 (.005)	-.008+ (.004)	-.002 (.004)
Age	-.000 (.000)	-.000 (.000)	-.00 (.000)	.000 (.000)	.000 (.000)	.000 (.000)
R <sup>2</sup>	.686	.550	.553			
First-stage statistics:						
Kleibergen-Paap <i>F</i> -test				33.75	33.75	33.75
Sargan test				.0451	.010	.639
<i>p</i> -Value				.502	.931	.424

Note. Robust standard errors are in parentheses. All regressions include sector-year and sector-district fixed effects.  $N = 1,974$ .

+ Significant at 10%.

\* Significant at 5%.

\*\* Significant at 1%.

tive and significant, which indicates that courts' delays have a negative and significant impact on job reallocation. In accordance with previous empirical results, the effect of firing costs on job creation and job destruction is not symmetric, and the overall impact on job turnover is driven mainly by a reduction in job destruction and, to a lesser extent, job creation (Gómez-Salvador, Messina, and Vallanti 2004).

In economic terms, the estimated coefficients are sizeable:  $-.070$  for JR,  $-.059$  for JD, and  $-.011$  for JC. Our estimates also allow us to infer the effect of labor trials' length on net employment changes, traditionally defined as the difference between JC and JD rates (Davis, Haltiwanger, and Schuh 1996). According to Table 4, trial length has an overall positive effect on net employment equal to  $.048$ , which implies that, at least in the short run, higher firing costs induced by courts' inefficiency are positively related to employment growth. This result is in line with that in Autor, Kerr, and Kuger (2007) and, more recently, Fraise, Kramarz, and Prost (2015) and OECD (2016), which show that an increase in dismissal costs induced by a change in legislation (Autor, Kerr, and Kugler 2007;



OECD 2016) or by judicial activity (Fraisie, Kramarz, and Prost 2015) may have a positive short-run impact on net changes in employment.<sup>40</sup>

We can quantify the effects on job flows by computing the estimated increase in job flows that would result from moving from a district at the 95th percentile (less efficient) to the 5th percentile (more efficient) of the inefficiency distribution (that is, from the district of Salerno to the district of Trento in our sample). Reducing the length of labor trials by almost 76 percent in Salerno would lead to a 5.4-percentage-point increase in JR, a 4.5-percentage-point increase in JD, and a .9-percentage-point increase in JC.

Interestingly, the length of civil proceedings has an opposite effect on job flows, through an increase in job destruction, with an unambiguous negative effect on net change in employment. This result reinforces our findings; that is, that labor and civil courts' inefficiency affects labor dynamics through different channels, the former by increasing firing costs and then dampening job destruction and job creation, the latter by negatively affecting contract enforcement and therefore reducing employment growth.<sup>41</sup>

The relevant statistics to test the validity (relevance and orthogonality) of the instruments and the associated  $p$ -values are also given in Table 4. The  $F$ -statistic for the relevance of instruments is above the rule-of-thumb threshold of 10 in all specifications, and the overidentification test does not reject the null hypothesis that the instruments are uncorrelated with the error term.

Finally, comparing the results of the OLS and IV regressions highlights the reduced significance and magnitude of the effects of trial delays in the OLS regressions. The OLS coefficient on labor-trial length in the JD equation is not significant, and the point estimate is lower in absolute value than in the IV estimation. This seems to suggest that the OLS coefficients are downward biased for the presence of reverse causality, which stems from the fact that a higher rate of job destruction in a given court may lead to a higher number of dismissal cases brought to court and in this way to an increase in courts' congestion and trial length. Conversely, reverse causality is less of a problem for the JC equation, as it is confirmed by the OLS results: indeed, the effect of trial length on job creation remains marginally significant and quantitatively similar to that estimated in the IV regression.

Table 5 reports the coefficients of the instruments of the first-stage and reduced-form regressions. The first-stage results show that, as expected, both instruments are positively and significantly correlated with the duration of labor trials. Moreover, in the reduced-form regressions, both vacant judicial positions and the rate of appeal of civil-servants' legal disputes have a negative and signifi-

<sup>40</sup> The positive relationship between firing costs and employment growth is also consistent with the theoretical findings in Garibaldi and Violante (2005), which shows that, when wages are constrained for insiders and flexible for outsiders (as is typically the case in a dual labor market), an increase in job-security provisions reduces job destruction with no effect on job creation, which implies an overall short-term positive effect on employment.

<sup>41</sup> Similar results are reported in Giacomelli and Menon (forthcoming), which finds that the length of civil proceedings has a negative effect on firms' growth and employment.

Table 5  
The Effect of Labor-Trial Length on Job Flows: First-Stage  
and Reduced-Form Regressions

	First Stage: Length	Reduced Form		
		JR	JD	JC
Vacancies	.007** (.003)	-.001* (.000)	-.001* (.000)	.000 (.000)
Rate of appeal	.026** (.007)	-.001 (.001)	-.002* (.001)	.000 (.001)

Note. All regressions include civil trial length, income, age, and size dummies. Robust standard errors are in parentheses.

\* Significant at 10%.

\* Significant at 5%.

\*\* Significant at 1%

cant effect on job destruction, which results in a reduction of job turnover, while the job creation rate is not significantly affected.

### 6.2. Courts' Delays and Productivity

In Table 6 we report the effect of trial length on labor productivity at the firm level estimated as in equation (3). As courts' delays vary across districts and time, we are able to control for any time-invariant unobserved characteristics of firms by the use of firm fixed effects, thus fully exploiting the firm-level dimension of the data set.

We find that the length of labor trials is associated with a lower level of labor productivity for firms exceeding 15 workers. The coefficient of the interaction term is negative and significant (column 2), and the overall elasticity estimated for large firms is .049. Conversely, the effect due to discounting is positive though not statistically significant for firms below the 15-employee threshold, while the estimated differential elasticity for firms above the 15-employee threshold is -.051. This implies that if the labor court in Salerno were as efficient as the one in Trento, labor productivity would increase by almost 4 percent in large firms relative to small firms.

The fact that courts' activity has an immediate causal effect on firms' decisions might seem surprising at first sight. Nevertheless, it is important to consider that the duration of trials in a given year is not independent of the bulk of pending suits (and their characteristics) that were initiated and not concluded in the preceding year(s). Firms can therefore easily infer the expected duration of trials on the basis of the number and characteristics of outstanding disputes (inherited from the previous years) at the local (district) level and on other publicly available information, such as, for example, courts' scheduling of comparable suits.<sup>42</sup>

<sup>42</sup> We also address this issue empirically by using lagged indicators of trial length. We find that the lagged trial-length variable keeps the same sign and magnitude as in the baseline specifications. Results are available from the authors on request.

Table 6  
The Effect of Labor-Trial Length on Firms' Productivity: Instrumental Variables Regressions

	(1)	(2)	(3)	(4)	(5)
Length	-.019 (.054)	.003 (.053)			
Length × Size		-.051** (.002)	-.050** (.001)	-.050** (.001)	-.018** (.004)
Length × Flex				-.181 (.155)	-.172 (.154)
Length × Size × Flex					-.321** (.052)
Civil length	-.055 (.050)	-.053 (.050)			
Regional Income	.324** (.115)	.348** (.109)			
District-year dummies	No	No	Yes	Yes	Yes
First-stage statistics:					
Kleibergen-Paap <i>F</i> -test	2,541.8	790.3	137.3	790.3	137.3
Sargan statistic	.011	2.606	2.558	4.360	5.943
<i>p</i> -Value	.991	.271	.109	.114	.111

Note. Robust standard errors, in parentheses, are clustered at the district level in columns 1 and 2 and at the firm level in columns 3–5. All specifications include employment as an indicator of firm size, firm fixed effects, and sector-year fixed effects.  $N = 458,145$  observations and 154,658 firms.

\*\* Significant at 1%.

The differential impact of firing costs related to court delays on productivity for firms above and below the 15-employee threshold is remarkably robust to the inclusion of district-year dummies. In this specification the main effect (Length) is not included because it is absorbed by the district-year dummies, which also capture all time-variant and district-specific factors that may simultaneously affect the efficiency of labor courts and firms' productivity.<sup>43</sup> The stability of the coefficient on Length × Size when adding district-year dummies provides further reassurance that the estimated differential effect is not driven by any possible source of bias arising from district-time-variant omitted factors (column 3).

Columns 4 and 5 show the effect of courts' delays on firms' productivity, taking into account the sector-intrinsic need for labor flexibility, following the Rajan and Zingales (1998) approach. The empirical strategy, outlined in equation (4), consists of evaluating if the differential effect of trial length on small and large firms we estimated in the first set of regressions depends on a sector's reallocation requirement. Column 4 shows that the length of trials decreases labor productivity for firms operating in sectors where the need for labor flexibility is higher, as shown by the negative—although not statistically significant—coefficient for the interaction with flexibility. More interesting, the impact of courts' delays on labor productivity is stronger for larger firms operating in sectors with a greater need for labor turnover, as shown by the negative and highly significant coefficient for

<sup>43</sup> In this set of regressions, the identification of the effect comes entirely from the differential in the EPL provisions for firms above and below the threshold.

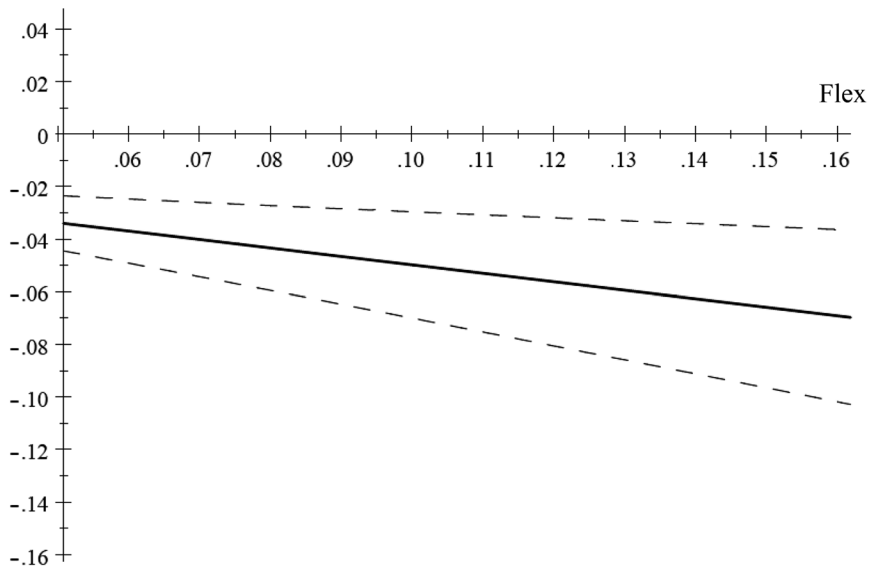


Figure 4. Trial length and flexibility for firms with more than 15 employees

the triple interaction term (column 5). Here again the instruments satisfy the orthogonality conditions in all specifications, and the Anderson canonical correlation statistic rejects the null hypothesis of no correlation between the endogenous regressors and the instruments, which suggests that the instruments we consider are adequate to identify our equations.

Quantitatively, the effect of increasing courts' efficiency in Salerno to the level in Trento would be an increase in the labor productivity of large firms relative to small firms ranging from 3.1 percent for firms in a low-job-reallocation sector—that is, the sector at the 5th percentile of the reallocation distribution (paper products, publishing, and printing, with a Flex index equal to .069)—to 4.8 percent for firms in a high job-reallocation sector—that is, the sector at the 95th percentile (construction, with a Flex index equal to .135).<sup>44</sup> These results are in line with those in Cingano et al. (2010), which, using firm-level data and exploiting cross-country and cross-sector variation in the OECD EPL index, show a sizable and negative impact of firing costs on labor productivity in high-reallocation industries.

Figure 4 plots the predicted differential effect of trial length on the productivity of large firms relative to small firms in industries with different reallocation requirements. The solid line indicates the predicted differential effect of courts' delays on labor productivity as a function of the reallocation requirement; that

<sup>44</sup> In this set of estimates, we include district-year dummies that absorb, among other effects, the main effect of trial duration (Length), which is district specific (and time varying). The estimated coefficients on Length  $\times$  Size and Length  $\times$  Size  $\times$  Flex are very similar when we include the main effect and drop the district-year dummies.

Table 7  
The Effect of Labor-Trial Length on Firms' Productivity: First-Stage and Reduced-Form Regressions

	First Stage			Reduced Form	
	Length	Length	Length × Size	Productivity	Productivity
Vacancies	.011** (.000)	.010** (.000)	-.048** (.001)	.000 (.000)	.003+ (.001)
Vacancies × Size		.003** (.000)	.221** (.001)		-.012** (.002)
Rate of appeal	.228** (.002)	.234** (.002)	-3.402** (.015)	-.004 (.015)	.167** (.021)
Rate of appeal × Size		-.026** (.003)	12.878** (.019)		-.642** (.041)

Note. All regressions include civil trial length, regional income, and employment dummies. Robust standard errors are in parentheses.

+ Significant at 10%.

\*\* Significant at 1%.

is,  $\partial^2 Y_{fst}^r / (\partial \text{Length}_t^r \partial \text{Size}_{fs}^r) = \hat{\beta}_2 + \hat{\beta}_3 \text{Flex}_s$ . The dotted lines indicate 95 percent confidence intervals. According to Figure 4, the predicted difference in the response of productivity to trial length is negative and, in general, statistically significant for all values of Flex. Moreover, the differential effect on productivity between firms above and below the threshold increases, in absolute terms, with the sector-reallocation requirement, which confirms that the greater the need for flexibility, the stronger the impact of the length of trial on large firms relative to small ones.

The results of first-stage and reduced-form regressions are shown in Table 7. In the first column, the coefficients of the first-stage regressions show that both instruments are positively correlated with our endogenous regressor. Moreover, the reduced-form regressions confirm that both judicial vacancies and the rate of appeal decrease the productivity of firms above the 15-employee threshold, while the effects on smaller firms is less clear-cut.

Finally, in Table 8 we present the OLS estimates of equations (3) and (4). The OLS coefficient on the main effect (Length) is positive and is statistically significant when the interaction term is added to the regression. The point estimates tend to be larger than the IV estimates, which suggests that the potential endogeneity of the regressor leads to a downward bias in the estimate of the overall negative effect of court delays on productivity. Conversely, the OLS coefficients on Length × Size are almost the same as those obtained in the IV estimates, even when district-year dummies are not included among the controls. These results lend support to our identification strategy and suggest that omitted variables might not be a major concern in estimating the differential effect of trial length for firms above the 15-employee threshold.

Table 8  
The Effect of Labor-Trial Length on Firms' Productivity: Ordinary Least Squares Regressions

	(1)	(2)	(3)	(4)	(5)
Length	.014 (.013)	.030 (.013)*			
Length × Size		-.053 (.002)**	-.053 (.001)**	-.053 (.001)**	-.020 (.005)**
Length × Flex				-.272 (.346)	-.174 (.346)
Length × Size × Flex					-.324 (.045)**
Civil length	-.077 (.034)*	-.075 (.035)*			
Income	.350 (.102)**	.374 (.098)**			
R <sup>2</sup>	.036	.099	.101	.101	.101
District-year dummies	No	No	Yes	Yes	Yes

Note. Robust standard errors, in parentheses, are clustered at the district level in columns 1 and 2 and at the firm level in columns 3–5. All specifications include employment as an indicator of firm size, firm fixed effects, and sector-year fixed effects.  $N = 458,145$ .

\* Significant at 5%.

\*\* Significant at 1%.

## 7. Robustness and Other Results

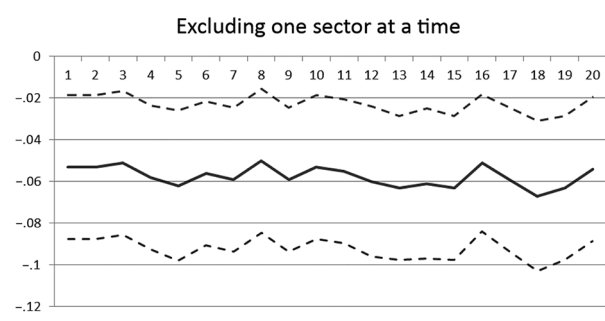
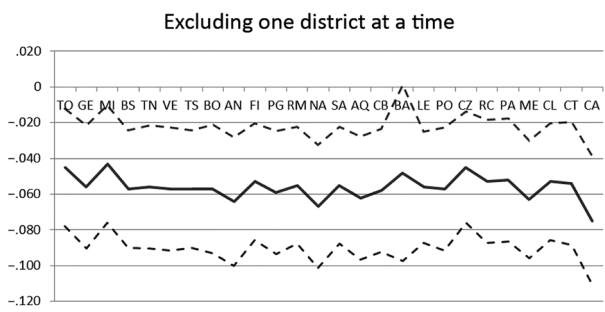
### 7.1. Robustness to the Exclusion of Sectors and Districts

We check the sensitivity of our main results to the exclusion of specific sectors and districts in the regressions. Figures 5 and 6 show the impact of dropping one district at a time and one sector at a time on the average impact of courts' delays on job turnover and productivity, respectively. In this exercise, we focus on our baseline specifications as reported in the IV regressions in Table 4 and column 2 of Table 6.

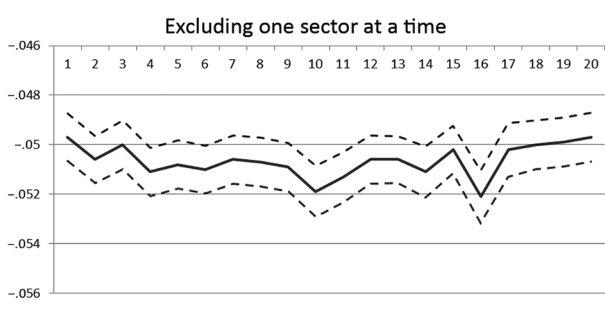
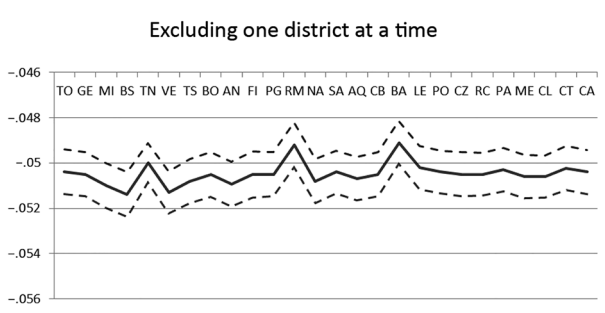
With regard to job turnover, Figure 5 shows that dropping one sector at a time never turns the sign of our variable of interest, which remains negative in all of the regressions. Moreover, the coefficients are always statistically significant at the 10 percent level.<sup>45</sup>

Figure 6 reports the coefficients of trial length on firms' productivity for the estimated large-small firm differential. Again, the estimated effects are remarkably stable when specific districts and sectors are excluded from the sample, and the

<sup>45</sup> We repeated the same exercise for job creation and job destruction. The estimated coefficients on job destruction are always negative and robust to changes in the sample's composition along both the district and industry dimensions. The sensitivity analysis also confirms that trial length tends to exert a negative impact on job creation, although the results are, in general, not statistically significant. Results are available in the online appendix.



**Figure 5.** Job-reallocation sensitivity analysis



**Figure 6.** Firms' productivity sensitivity analysis

differential effect retains its negative sign and is always highly statistically significant at standard levels of testing.<sup>46</sup>

### 7.2. Firms' Entry and Exit

One additional channel through which courts' delays may affect firms' productivity is through a change in the sample's composition induced by firms' entry and exit. The theoretical argument is the following: dismissal costs raise the productivity threshold above which firms enter the market and, when firing costs are charged on continuing firms only, the threshold below which firms exit the market (Bertola 1994; Koeniger and Prat 2007; Poschke 2009). As a consequence, an increase in firing costs may intensify selection and therefore increase the average productivity of firms above the 15-employee threshold.

Since our sample is unbalanced and potentially includes entering and exiting firms, the coefficient we estimate may be the result of both effects: the effect of labor courts' delays on the productivity of ongoing firms and the effect on the average productivity due to firms' entry and exit. Unfortunately, our data do not allow us to estimate the two effects separately, since in the AIDA database firms enter and exit the sample for a number of reasons (change of name, mergers, or misreported or missing information in the balance sheets) not necessarily related to firms' closures or foundings. In our baseline specifications, we can control for specific industry and location time-varying factors that can affect firms' churning rates by including industry-year and district-year dummies. However, industry and district time-varying fixed effects do not allow us to control for the fact that courts' inefficiency differently affects firms' churning rates (and therefore the average productivity) of the treatment group (firms above the 15-employee threshold) vis-à-vis the control group.<sup>47</sup>

To assess the possible effect induced by firms' entry and exit, we therefore follow Autor, Kerr, and Kugler (2007) and Cingano et al. (2010) and reestimate our baseline specifications using a balanced sample that includes only firms that are continuously observable every year from 2007 to 2010. In this way, we can exclude any effect of courts' delays on productivity occurring through entry and exit. In this new set of regressions, we have 56,721 firms, about one-third of the original sample.

The coefficients for the balanced sample in Table 9 are remarkably similar to those obtained for the unbalanced sample in Table 6. In line with previous empir-

<sup>46</sup> The online appendix presents the same exercise for the coefficients of small and large firms separately. Results show that the estimated impact of trial length is never statistically significant for small firms and is negative and statistically significant for large firms.

<sup>47</sup> Notice that, to the extent that courts' delays affect exit and entry rates, labor-market churning due to firms' entry and exit is expected to increase the average productivity of the treatment group (firms above the 15-employee threshold) vis-à-vis the control group. This implies that not controlling for differential entry and exit would bias our coefficient downward, and therefore, if anything, our estimates would understate the true effect of courts' delays on the productivity of incumbent firms.



Table 9  
The Effect of Labor-Trial Length on Firms' Productivity: Balanced Sample

	(1)	(2)	(3)
Length × Size	−.049** (.001)	−.049** (.001)	−.024* (.007)
Length × Flex		−2.840* (1.689)	−2.844* (1.690)
Length × Size × Flex			−.257** (.069)

Note. Robust standard errors, in parentheses, are clustered at the firm level. The dependent variable is the log of labor productivity. All specifications include firm size, firm fixed effects, sector-year fixed effects, and district-year fixed effects. The interactions of Length are instrumented with the interactions of the appeal rate of civil servants' trials and the interactions of judges' vacant positions. N = 226,884 observations and 56,721 firms.

+ Significant at 10%.

\* Significant at 5%.

\*\* Significant at 1%.

ical studies,<sup>48</sup> this evidence suggests that, at least in the short run, courts' delays have no effect on firms' churning rates.

### 7.3. Firm-Growth Regressions

We check for the potential sorting induced by courts' delays. To this aim, we restrict our analysis to a sample of firms in a narrow interval around the 15-employee cutoff (firms between 5 and 25 employees) and estimate the following linear probability model for the probability of the growth of firms around the threshold (see Schivardi and Torrini 2008):

$$g_{ft} = S_{ft}\gamma + (S_{ft} \times \text{Length}_t^r)\delta + \mathbf{X}_{ft}\beta + \mathbf{D}\varphi + \eta_f + u_{ft}^r, \quad (5)$$

where  $g_{ft}$  equals one if firm  $f$  in year  $t$  has a larger size than in  $t - 1$ . The term  $S_{ft}$  denotes a set of size dummies for firms with 13, 14, and 15 employees, and  $\text{Length}_t^r$  is trial length at the district level. The matrix  $\mathbf{X}_{ft}$  includes a set of controls at the firm level such as a polynomial in firm size. Finally, we also include a full set of district-year and industry-year dummies to control for district and sector unobserved time-varying factors and firm fixed effects to account for firm-specific time-invariant factors that may affect firms' propensity to grow. We instrument the interactions  $(S_{ft} \times \text{Length}_t^r)$  with the two instruments (interacted with the size dummies) used in our baseline specifications (that is, the rate of

<sup>48</sup>For example, Autor, Kerr, and Kugler (2007) for the United States and Kugler and Pica (2008) for Italy find that reforms that increase EPL affect firms' entry and exit rates only marginally, with the effects being small and not always significant. Using more aggregated data, Aghion, Fally, and Scarpetta (2007) and Bottasso, Conti, and Sulis (2016) find that EPL is associated with lower entry and exit rates, particularly in industries that are characterized by stronger labor reallocation.

Table 10  
 Probability of Growth around the Threshold and Courts'  
 Delays: Firms with 5–25 Employees

	Full Sample (1)	Productivity	
		Low (2)	High (3)
Size13	.423 (.467)	.425 (.640)	.369 (.712)
Size14	.606 (.508)	1.129 (.704)	.142 (.777)
Size15	–.551 (.556)	.115 (.781)	–.995 (.834)
Length × Size13	–.07 (.073)	–.07 (.099)	–.062 (.113)
Length × Size14	–.098 (.079)	–.177 (.119)	–.026 (.123)
Length × Size15	.089 (.087)	–.014 (.121)	.16 (.132)
Observations	199,711	99,254	100,457
Firms	87,109	44,377	42,732

Note. Robust standard errors, in parentheses, are clustered at the firm level. The dependent variable is a dummy that takes the value one if employment at time  $t$  is greater than employment at time  $t - 1$  and zero otherwise. Interactions of Length are instrumented with the interactions of the appeal rate of civil servants' trials and judges' vacant positions. All regressions include firm, sector-year, and district-year fixed effects.

appeal of civil servants' trials and judges' vacancy rate). The results are reported in Table 10.

Table 10 shows that the probability of expansion of firms with just below 15 employees is not significantly different from that of other firms.<sup>49</sup> Moreover, the interaction terms in column 1 are not significant, which implies that courts' delays do not affect such a probability. The results for the samples of firms with high levels of productivity (above the median) and low levels of productivity indicate once again that the probability of growth for firms just below the threshold is not significantly affected by courts' delays. These results confirm that courts' inefficiency does not significantly affect the propensity to grow around the 15-employee threshold and therefore the self-selection of firms into treatment and control groups.

## 8. Conclusions

We assess the impact of courts' delays on labor-market adjustments as a factor influencing the strictness of firing costs, thus highlighting a cause of within-country variation in the costs and enforcement of EPL. We argue that labor

<sup>49</sup> This result is in line with that in other empirical studies for Italy on the effect of EPL discontinuity at the 15-employee threshold (see, for example, Schivardi and Torrini 2008).

courts' inefficiency is an important dimension of de facto EPL to the extent that delays in legal trials of labor disputes can add significantly to the costs of dismissing workers. So far, this aspect has been largely neglected by the existing research. We provide evidence that courts' inefficiency—measured by the average length of trials—implies per se a high economic cost for labor markets in terms of misallocation of resources and productivity.

Exploiting the variability of the length of labor trials across Italian judicial districts and the discontinuity of the legislation regarding firing at the 15-employee threshold, we show that the length of trials significantly reduces job flows, and this effect translates into a reduction in labor productivity at the firm level. The latter effect is related to firms' flexibility requirements, being stronger for firms in high-reallocation sectors. Our results are remarkably robust to using different sets of instruments and to a number of other robustness checks, which capture any possible source of endogeneity arising from district-specific and time-variant omitted factors.

Overall, the evidence points to the fact that the duration of judicial proceedings should not be overlooked as a component of firing costs in future studies. Our findings also have important policy implications. Any reforms aimed at reducing the strictness of EPL should also consider the role played by courts' efficiency (and, more generally, the wider institutional framework) in enforcing it and how these reforms interact with the complexity and length of legal procedures, the latter being an important dimension of EPL.

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