Firing Costs and Productivity: Evidence from a Natural Experiment*

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Abstract

This paper investigates the effect of firing costs on total factor productivity (TFP) and resource allocation. Exploiting heterogeneous changes in firing costs across worker types in Belgium, we find that increasing firing costs reduces firm-level TFP. Firms facing a net increase in firing costs reduce hiring and firing, increase hours worked per employee, adjust the composition of their workforce away from worker types whose firing costs have increased, and rely more on outsourced employees. Instead, we find no evidence of capital-intensive technology adoption. The decline in TFP is smaller for firms with better access to credit.

JEL classification: D24, D25, J63, J65

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1. Introduction

Firing costs are an important component of labour protection legislation in many economies but whether and how they impact firms' productivity remains unclear. On the one hand, firing costs have negative effects on productivity because they introduce allocation frictions in firms' production inputs. Since Bentolila and Bertola (1990) and Hopenhayn and Rogerson (1993), a large theoretical literature has shown that firing costs distort firms' optimal hiring and firing, leading to resource misallocation and a reduction in aggregate output.[1] Some authors have argued that allocation frictions also reduce productivity-enhancing investments (e.g., Da-Rocha et al., 2021). On the other hand, firing costs can have positive effects on productivity because firms may increase ex-ante screening and hire more productive workers as a result of higher firing costs, or decide to invest in efficient labour-saving technologies (Autor et al., 2007). Furthermore, as firing costs reduce dismissal risk, workers may be encouraged to invest more in firm-specific human capital (Acharya et al., 2014; Griffith and Macartney, 2014).

Given these opposing theoretical predictions, it is important to empirically investigate whether firing costs ultimately affect productivity positively or negatively; and what the most relevant channels are. While several existing studies have analysed the relationship between firing costs and economic outcomes, only a few settings have allowed to causally identify the effect of firing costs on firm-level productivity (e.g. Autor et al., 2007; Cingano et al., 2016).

Our paper contributes to this literature by (i) providing causal evidence from a novel quasi-natural experiment that significantly changed firing costs, by (ii) carefully identifying the most relevant channels through which the effect operates, and by (iii) highlighting the importance of accounting for different worker types in the production function estimation if these types face heterogeneous changes in firing costs.

For identification, we exploit a unique setting provided by the Belgian labour market. Since the early 1900s, Belgian law makes a distinction between blue-collar workers (those doing manual work) and white-collar workers (those doing non-manual work) and provided the latter with far better employment protection. However, the Belgian Constitutional Court ruled that this practice was discriminatory and unconstitutional, which led to a harmonization across worker types with the "Act of 26 December 2013". The Act unambiguously and significantly increased the firing costs for blue-collar workers. For example, the notice period

[1] Note that firing costs can further affect aggregate productivity by distorting firms' entry and exit decisions.

for an average blue-collar worker with 10 years of seniority increased from 42 days under the old law to 210 days under the new law. For white-collar workers, the impact of the Act is more ambiguous. For an average white-collar worker with 10 years of seniority, the notice period decreased from 303 days to 210 days. However, the Act also abolished the possibility for employment contracts to stipulate a trial period during which a worker could be fired at almost no cost. Trial periods were commonly used for all worker types, but ranged between 7 and 14 days for blue-collar workers and between 31 and 365 days for white-collar workers. The abolishment thus especially increased the firing costs of white-collar workers, implying that the total impact of the Act on their firing costs is ambiguous: it has become more costly to fire white-collar workers during their first year of tenure, but less costly thereafter. As we explain in more detail below, our main identifying assumption is that firms with a majority of blue-collar workers experienced an increase in firing costs, relative to firms with a majority of white-collar workers.

Our empirical analysis uses detailed firm-level panel data on workforce composition (e.g., worker type, remuneration, hours worked, outsourced labour, ...) as well as balance sheets and income statements of Belgian firms between 2009 and 2017 (four years before and after the legal change) to investigate the relationship between firing costs and productivity. It relies on a difference-in-differences (DiD) model that compares the productivity of firms with a relatively high ex-ante share of blue-collar workers (i.e., firms that faced an increase in firing costs, labelled *blue-collar firms*) to that of a matched sample of firms with a relatively low ex-ante share of blue-collar workers (i.e., firms whose firing costs remained unchanged or even slightly decreased, labelled *white-collar firms*) around the introduction of the Act. We saturate our regression models with firm and industry-year fixed effects to capture possible pre-existing trends pertinent to unobservable factors. The matching procedure, in turn, helps to rule out concerns that our results could be driven by differences in observables (Angrist and Krueger, 1999). However, our results also hold without matching or fixed effects. Finally, in line with a causal interpretation of our results, we find that differences in outcomes between blue- and white-collar firms appear after –but not before– the introduction of the new labour legislation.

As our main outcome variable, we estimate revenue total factor productivity (henceforth TFP) at the firm-year level. Importantly, our estimation accounts for the heterogeneous impact of the Act on worker types by treating blue- and white-collar workers as separate production factors. To highlight the importance of accounting for worker types in the production function estimation, we provide a stylised model with misallocation wedges. The model explains that a bias can arise in estimating the causal effect of exogenous changes in firing costs on productivity when the estimated production function pools heterogeneously affected worker types. The direction of the bias depends on several parameters, but in our context pooling worker types would lead to a significant *underestimation* of the effect of firing costs on TFP.

We find that blue-collar firms on average experienced a 5.6% decline in TFP relative to white-collar firms, after the legal change. The decline is very persistent over time and remains large and statistically significant over the entire 4 years after the legal change. This result is robust to (i) using alternative approaches to estimate firm-level productivity; (ii) considering both value-added or revenue-based estimation procedures as well as alternative production functions such as the translog; (iii) allowing time-varying input elasticities to account for possible changes in production technologies; and (iv) allowing for linear and non-linear variations of our main independent variable. Crucially, and consistent with our stylised model, the estimated effect on TFP becomes 70% smaller and statistically insignificant if we estimate the production function by pooling worker types in a single employment input, which emphasises the importance of separately identifying the heterogeneously affected worker types.

In terms of channels explaining this drop in TFP, our results are the following. We find that the Act caused a significant reduction in job flows among blue-collar firms, relative to white-collar firms, with an immediate drop in hiring of around 12% that persists for up to 3 years, and a gradual drop in firing that peaks at minus 16% after 3 years. This strongly supports the hypothesis that the legal change implied a large increase in firing costs for blue-collar firms relative to white-collar firms. Moreover, the worker-type composition changed significantly as a consequence of the Act. Compared to white-collar firms, blue-collar firms reduced the number of blue-collar workers, but increased the number of white-collar workers.

In line with Autor et al. (2007) and Cingano et al. (2016), we find evidence that higher firing costs lead to a (delayed) increase in tangible fixed assets, but in contrast to these authors, we find it hard to rationalize this as capital deepening or capital-labour substitution. This is because we find that the increase in tangible fixed assets is entirely driven by an increase in buildings and furniture, which we interpret as being correlated with the shift to more white-collar workers and hence more likely reflects capital-labour complementarity for such workers. Instead, we do not find significant increases in the types of capital that are usually associated with the adoption of labour-saving technologies such as machinery and equipment or intangible assets. Moreover, we estimate input elasticities for different industries with different intensities of blue-collar workers separately before and after the Act and find very small, insignificant changes. The hypothesis of technology adoption and capital-labour substitution would instead predict significant changes, especially for industries that use blue-collar workers more intensely.

Next to the insignificant effect on capital deepening, we also find no evidence that higher firing costs spur investment in training activities, which is different from Acharya et al. (2014) who predict that workers will invest more in human capital when they are more protected from dismissal.

Instead of investing in the skills of incumbent employees or transitioning to more capital-intensive technologies, blue-collar firms try to mitigate the impact of higher firing costs along alternative margins. Specifically, they change the composition and the usage of their labour force. Even though total employment at blue-collar firms does not decline relative to white-collar firms after the Act, blue-collar firms hire relatively fewer workers under a permanent contract, increase their reliance on outsourced workers through employment agencies, and increase the hours worked per worker.

Finally, using loan-level data from the Belgian credit register, we disentangle credit supply from credit demand. This allows us to show that, among highly leveraged firms, blue-collar firms experience a smaller decline in TFP relative to white-collar firms when they face relatively generous credit supply conditions. This suggests that access to credit may help firms to efficiently replace their workers when firing becomes more costly.

We consider several robustness checks of these results, including analyzing our research question using a different identification strategy. In this alternative setup, we compare Belgian firms in blue- and white-collar *industries* (affected by the legal change) to French and German firms in the same industries (not affected by the Belgian reform).

Taken together, our results strongly suggest that increases in firing costs reduce productivity mainly because they distort optimal firing and hiring policies, and neither spur investment in productivity-enhancing (labour-saving) technology nor investment in human capital. Moreover, it seems that the Belgian reform did not only reduce firm productivity, but also failed to achieve its intended objective of providing more protection to blue-collar workers, as affected firms partially offset the additional cost by using flexible employment arrangements such as outsourcing.

Our study contributes to the literature linking firing costs to productivity. Some papers document a negative association by exploiting cross-country variation in employment protection indexes (Micco and Pagés, 2006; Bassanini et al., 2009; Cingano et al., 2010). A natural concern with cross-country analyses is the comparability of labour laws across countries (Haltiwanger et al., 2014). A small set of studies conduct a within-country analysis. The two studies that are most closely related to ours are Autor et al. (2007) and Cingano et al. (2016).

Autor et al. (2007) study the staggered adoption of wrongful discharge laws (WDLs) by US states. These WDLs constrain the ability of employers to terminate workers at will, and hence increase firing costs. Consistent with the theoretical prediction that increasing firing costs should reduce hiring and firing, Autor et al. (2007) show that WDL adoption reduces total job flows, especially for manufacturing firms. They also find that WDL adoption leads to lower productivity and an increase in capital deepening. However, they downplay the robustness of their results on productivity and investment by noting that US states' adoption of WDLs seems to be preceded by investment downturns. As this could imply that WDL adoption is not exogenous with respect to the economic environment, they suggest: *"a cautious interpretation of the findings until further evidence accumulates"* (p. F212). Cingano et al. (2016) exploit a 1990 reform in Italy which eliminated differential labour protection in small and large firms by increasing the firing costs for firms with less than 15 employees. They also find that increasing firing costs reduces productivity and increases capital deepening. The Italian reform provides an interesting case of a large exogenous increase in firing costs. As the reform increased firing costs only for very small firms and given that larger firms might have different incentives and possibilities to respond to increasing firing costs, it is not straightforward that the results would hold for firms of all sizes.

We believe that our setting allows for complementary analyses and insights. The legal change we study (i) is plausibly exogeneous with respect to the economic environment, (ii) affects firing costs for the entire firm-size distribution, and (iii) our data allows us to carefully identify the effects of changes in firing costs on firm-level productivity in the short- and medium-run, as well as to provide new evidence on the relevant economic mechanisms.

The Belgian reform is credibly exogenous to the economic environment given that the driving force behind the reform was unrelated to the prevailing economic conditions (the goal was to eliminate historic discrimination) and the timing/deadline for the law was set to 2013 by the Constitutional Court. As such, the legal change induced plausibly exogenous and significant changes in firing costs across worker types.

Importantly, in the studies of Autor et al. (2007) and Cingano et al. (2016), the changes in firing costs are (assumed to be) uniform across worker types. However, even *de jure* homogeneous changes in firing costs can *de facto* heterogeneously affect worker types. For example, Autor et al. (2006) show that WDLs negatively affected employment and particularly that of less educated workers.[2] Our theoretical model shows that when firing costs (de facto) increase heterogeneously across worker types, then pooling worker types would lead to a bias of the effect of firing costs on TFP. We directly observe different worker types whose firing costs were heterogeneously changed. This allows us to estimate productivity by accounting for the differential effects of firing costs across employees and to rigorously examine the effect of firing costs on capital deepening and employment composition together.

Additionally, as we analyse an event with a single treatment timing, our results should not be subject to a "bad comparisons" problem that staggered DiD designs could potentially suffer from.[3] Finally, our analysis covers the entire firm-size distribution, rather than focusing only on firms around a specific size threshold, which provides a high external validity.

In order to identify the importance of the different channels highlighted in theoretical studies, we provide detailed evidence on a wide range of firms' response margins. We document that firing costs lead to changes in firms' workforce composition, which appears to imply a misallocation of labour inputs that drives the decline in TFP. Consistent with existing studies, we also show that firing costs lead to a reduction in firing and hiring (Kugler and Saint-Paul, 2004; Kugler and Pica, 2008; Marinescu, 2009; Alpysbayeva and Vanormelingen, 2022), which further corroborates the misallocation effects of firing costs.[4] Our evidence that firms seek higher flexibility, with an increase in employee outsourcing, is consistent with Autor (2003), but we add that flexibility is also increased by reducing the hiring of permanent workers and by increasing the average hours worked per employee.

Finally, our finding that increased firing costs did not lead to investments in capital-intensive technologies or human capital, also contributes to the related literature that focuses on the effect of firing costs on (in)tangible capital investment (Calcagnini et al., 2009, 2014; Bena and Simintzi, 2019; Bai et al., 2020) or human capital investment (Acharya et al., 2014; Griffith and Macartney, 2014).

[2] Autor et al. (2006) argue that, because low-wage workers have high employment flow rates (i.e., they more frequently enter and exit employment), they are the first group of workers affected by reductions in hiring.

[3] See, for example, Goodman-Bacon (2021), Callaway and Sant'Anna (2021), and Baker et al. (2022) for the recent discussions on difference-in-differences regressions with staggered treatment timing.

[4] Among this stream of the literature, Alpysbayeva and Vanormelingen (2022) also exploit the "Act of 26 December 2013" in Belgium. The authors quantify the labour gap (i.e., the wedge between the labour input's value marginal product and its marginal cost) induced by the legal change. We instead focus exclusively on the effect of the legal change on productivity and document mechanisms through which the effect operates.

2. The Harmonization of Notice Periods in Belgium

Belgian legislation defines blue-collar workers as workers who mostly do manual work and white-collar workers as workers who mostly do non-physical office jobs.[5] White-collar workers have traditionally enjoyed better working conditions, but the Belgian Constitutional Court ruled in 1993 that this practice was discriminatory and unconstitutional.[6] Following the ruling, employer and employee representatives (the "social partners") repeatedly attempted but failed to harmonize the treatment of worker types. In 2011, the Constitutional Court therefore set 8 July 2013 as a deadline for an agreement. The required agreement was reached by the social partners on 5 July 2013 and formalized in the "Act of 26 December 2013". The Act has been effective since 1 January 2014.

A cornerstone of the new legislation was the harmonization of notice periods. The notice period is defined as the time between the receipt of a dismissal letter (or the date of resignation) and the last day of employment. If an employee is fired before the end of the required notice period, the employer has to pay what the employee would have earned by working (payment in lieu of notice). Prior to 1 January 2014, notice periods for white-collar workers were substantially longer than for blue-collar workers. For blue-collar workers, they primarily depended on the industry and the seniority of the worker; for white-collar workers, seniority and remuneration were most important. Under the new law, notice periods for all workers depend exclusively on seniority.[7] That is, notice periods are equal for both blue- and white-collar workers with the same seniority. Overall, removing industry- and wage-related differences resulted in a significant increase in the notice periods for blue-collar workers and a reduction in the notice periods for white-collar workers.[8]

[**Table** 1]

To illustrate the effect of the new legislation, Table 1 compares notice periods under the

[5]The distinction between blue- and white-collar jobs is not always obvious. In ambiguous cases, employers are required to offer contracts based on the worker's primary task. Consistency between reported worker types and workers' primary tasks is closely monitored by Belgian authorities. Thus, it is very unlikely that employers, for example, offer blue-collar contracts for primarily white-collar jobs.

[6]Appendix B illustrates the main differences in working conditions between blue- and white-collar workers before the new legislation.

[7]See Claeys and Engels (2018) for a detailed summary of the Act of 26 December 2013.

[8]Initially, increased notice periods did not apply to blue-collar workers working at temporary and mobile workplaces in the construction and upholstery & woodworking industries. Following the so-called "summer agreement" (decided on 26 July 2017 and effective from 1 January 2018), however, these exceptions have also been abolished.

old and the new legislation, for blue- and white-collar workers with a seniority of exactly 10 years. Before 1 January 2014, the notice period for white-collar workers earning more than €32,254 was 303 days (or 10 months), compared to 42 days (or 6 weeks) for blue-collar workers in, for example, the agriculture, textile, or transportation industries. If the same workers accumulated the entire 10 years of seniority under the new regime, their notice periods -independent of their type- would be 210 days. For the white-collar workers, this would imply a reduction by one-third, while it would more than quadruple the notice period for the blue-collar workers. For workers hired before but laid off after 1 January 2014, the new law stipulates that notice periods should be calculated as the sum of two parts: a first part that uses the old regime and assumes the worker was dismissed on 31 December 2013, and a second part that uses the new regime and assumes employment started on 1 January 2014.

The new notice periods for blue-collar workers translated into real costs for employers. For example, an employer who laid off a white- and a blue-collar worker with a gross annual wage of €40,000 and a seniority of exactly 10 years in December 2013, had to make a payment in lieu of notice to the white-collar worker of around €43,000 and to the blue-collar worker of around €6,000.[9] An employer who lays off comparable workers in December 2023, will have to pay both worker types €30,000. Whereas this corresponds to a 30% decrease in the firing cost for the white-collar worker, it increases the firing cost for the blue-collar worker by 400%.[10] Given that the median share of blue-collar workers is 66% in our sample, the overall effect of the new legislation is thus an economically relevant increase in firing costs.

Note importantly that, for both blue- and white-collar workers, the new legislation has also abolished trial periods, improved outplacement rights, and increased protection against unfair dismissals. Taken together with changes in the notice periods, the new legislation has unambiguously increased firing costs for blue-collar workers, whereas the overall effect for white-collar workers is uncertain because their reduced notice periods are offset by increased protection through other factors. The most notable to mention here is that the Act abolished the possibility for employment contracts to stipulate a trial period during which a worker could be fired at almost no cost. Trial periods were very common and significantly longer for white-collar workers (up to 1 year) than for blue-collar workers (up to 2 weeks). Abolishing

[9]Note that employers need to pay a social security contribution tax on the gross wage, which is roughly equal to 30% in Belgium, and which also needs to be paid to the government on a payment in lieu of notice. Hence: $43{,}000 \approx 40{,}000 * 1.3 * 303/365$ and $6000 \approx 40{,}000 * 1.3 * 42/365$.

[10]For another example, the cost for an employer to lay off a white- and a blue-collar worker with a gross annual wage of €40,000 and a seniority of 4 years under the old regime, was around €17,000 for the white-collar worker and around €5,000 for the blue-collar worker. The cost for an employer who wants to lay off comparable workers under the new regime is approximately €15,000 for both worker types.

these trial periods thus increased firing costs, especially so for white-collar workers. We discuss the other factors in Appendix C in more detail. All in all, the new Belgian labour legislation has substantially increased the firing costs for blue-collar workers relative to white-collar workers.

3. Data, TFP construction, and empirical model

3.1. Data sources

We obtain firm-year level data on Belgian firms from the Bel-first database provided by Bureau van Dijk (BvD). Bel-first provides detailed financial accounts as well as granular workforce data from 1994 to the present. Workforce data includes information on the number of blue- and white-collar workers employed during the fiscal year, the number of employees with permanent or temporary contracts, the number of new hires, the number of employees that retire, resign, or get dismissed, and the number of employees that are outsourced through an employment agency. All firms that are incorporated in Belgium are obliged to report this type of information[11], but obligatory reporting is more detailed for large firms. Importantly, large firms have to report information on intermediate inputs, whereas small firms can provide this information on a voluntary basis.[12] Since intermediate input data is crucial for our estimation of the production function, our main analysis keeps only the roughly 12% of small firms that voluntarily provide it. As a consequence, large firms constitute around 86% of our main sample. We also estimate our production function with a less sophisticated approach that does not rely on intermediate inputs and show that our main results are robust to including all firms.

The sample period for our main empirical tests ranges from 2009 to 2017. This period corresponds to four years before the implementation of the new Belgian labour regulation (i.e., 2009-2012) and four years when the new legislation was in place (i.e., 2014-2017). We exclude observations in 2013, when the negotiations received much attention and the implementation of the regulation was announced. With this, we aim to mitigate contamination, e.g. from announcement effects. To the best of our knowledge, during our sample period, there was no other legal change in the Belgian employment legislation that differently affected blue- and white-collar workers.[13] The period prior to 2009 is excluded to ensure that our analysis

[11]Some exceptions are firms with sole proprietorship or unlimited liability (i.e., very small firms).

[12]In Belgium, a firm is "large" if it is listed on the stock exchange, or if it exceeds at least two of the following three thresholds: 50 employees, a turnover of €9 million, or total assets of €4.5 million.

[13]One major event that occurred during our sample period was the sovereign debt crisis in Europe. In Section 5.2, we show that our results are not affected by financial support programs implemented in response

is not distorted by the financial crisis of 2007-08. Nevertheless, we gather data for 2008 as we use one-year-lagged firm controls in our regression models. BvD updates yearly financial information from one vintage to the next, and firms that have not reported for a certain time are dropped in each update. Not incorporating leavers into the analysis would lead to biased estimates if, for example, surviving firms are more productive. To circumvent such survivorship bias, we use historical files and recover deleted firms for each year.

We further restrict the sample to unconsolidated accounts of profit-seeking firms with at least five employees in 2012. Of the five employees, our production function estimation further requires that they comprise at least one blue- and at least one white-collar worker. This is because we consider blue- and white-collar workers as distinct production inputs (see Sections 3.2 and 3.3). Finally, we exclude firms from the financial industry (K) as well as from non-market services (O,P,Q,T,U) as they are heavily regulated by the government (Burggraeve et al., 2015).[14] Our regression sample requires that firms are observed at least once in the pre-period (2009-2012) and at least once in the post-period (2014-2017). This allows us to compare two groups of firms with one group experiencing an increase in firing costs relative to the other group, and thus to isolate the effect of the new legislation on firm outcomes. Finally, we winsorize all ratios at their 1st and 99th percentiles. Applying these selection criteria implies a sample with 49,447 firm-year observations from 7,225 unique firms.

3.2. TFP Estimation

Our main dependent variable is firm-level TFP, which we recover as the residual from the estimation of a Cobb-Douglas production function, as shown in the following equation:[15]

$$\begin{aligned} TFP_{i,s,t} = {} & ln(Value\ added)_{i,s,t} - \hat{\alpha}_s\ ln(Blue\ emp.)_{i,s,t} \\ & - \hat{\beta}_s\ ln(White\ emp.)_{i,s,t} - \hat{\gamma}_s\ ln(Tangible\ fix.\ assets)_{i,s,t} \end{aligned} \tag{1}$$

where i, s, and t are indices for firm, 2-digit NACE industry, and year, respectively. In our

to the sovereign debt crisis.

[14]Note that the new law was initially not applicable to blue-collar workers performing at temporary and mobile workplaces in the construction and upholstery & woodworking industries. We do not exclude these industries from our main analysis because the exemptions did not apply to all types of blue-collar workers and therefore firms operating in these industries were still affected (albeit presumably less). In Section 4.4, we show that there is no significant difference in the effects between firms operating in the construction and upholstery & woodworking industries and firms operating in other industries.

[15]We show in our robustness tests that our results hold for a wide range of specifications, such as using a translog production function or allowing for time-varying elasticities.

benchmark estimation, our measure of output is value added. However, we show that our results are also robust to using revenues as output. We separately include the firm's number of blue- and white-collar employees as labour inputs to capture the possible compositional effects of the regulation. This is crucial in our setting. Specifically, the next section (3.3) illustrates how the bias in estimated TFP arises when we estimate the production function by pooling differently affected workers in a single employment input. Lastly, we use tangible fixed assets as a measure of capital stock.[16]

We recover the input elasticities by estimating the production function separately for each 2-digit NACE industry. A well-known concern for estimating the production function is a potential simultaneity bias. That is, the firm's output and choice of inputs might be determined by unobserved productivity shocks. To address this, the production function literature mainly employs semi-parametric approaches. Initially, Olley and Pakes (1996) used investment as a proxy for unobserved productivity. Later, Levinsohn and Petrin (2003) used material inputs, arguing that investment is "lumpy" and therefore may not fully capture productivity shocks. Ackerberg et al. (2015) then criticized the identification of the labour coefficient in the model of Levinsohn and Petrin (2003) and proposed an alternative semi-parametric estimation. We rely on the estimation strategy of Ackerberg et al. (2015) for our benchmark specification, and use intermediate inputs, deflated by 2-digit industry-level price indices, as a proxy. In our robustness tests, we also estimate the production function using a one-step generalized method of moments procedure proposed by Wooldridge (2009), which also addresses the critique of Ackerberg et al. (2015) and estimates all coefficients efficiently.

Another important concern is that the input elasticities in Equation (1) should be estimated using quantities of inputs and output, while we use balance sheet values that are quantities multiplied by prices. We control for changes in industry-level prices by deflating these variables using their respective 2-digit industry-level price indices from Eurostat. Because we do not observe firm-level prices, our measure of TFP might still capture a mix of true productivity and firms' ability to adjust prices due to market power (see Foster et al., 2008). However, we believe that this shortcoming is not likely to significantly affect our analysis. First, we study the implications of an exogenous shock, the increase in adjustment costs for blue-collar labour, which directly affects the efficiency of the firm, rather than its ability to set prices. Second, if firms react to a drop in production efficiency (caused by increased labour adjustment costs) by raising prices, we would pick this up as an increase

[16]In Autor et al. (2007) and Cingano et al. (2016), capital stock is calculated using the perpetual inventory method (see Gal (2013) for technical details). We nonetheless chose to use tangible fixed assets to ensure comparability with various existing papers studying Belgian data (e.g., De Loecker et al., 2018; Dewitte et al., 2020; Ferrando et al., 2020).

in measured TFP. This would imply that our estimation of the relative difference in TFP between primarily blue- and primarily white-collar firms after the legal change would be biased towards observing higher productivity among blue-collar firms. Since we find the opposite, our results should be interpreted as a lower bound if firms have market power. Third, we believe that market power is only a limited concern in our sample. Although most firms are large according to the Belgian classification, they are actually relatively small by international standards (the median number of employees is 43).

To illustrate the mechanisms through which firing costs affect productivity, we analyse the response of various dependent variables related to firms' employment. We examine the effect on the composition of the workforce using *ln(Blue emp.)* (the natural log of the number of blue-collar employees) and *ln(White emp.)* (the natural log of the number of white-collar employees). We further examine the effect on total employment using *ln(Emp.)* (the natural log of the total number of employees). In addition, Bel-first allows us to assess the effect on employee turnover using *ln(Entering emp.)* (the natural log of the number of new hires) and *ln(Exiting emp.)* (the natural log of the number of employees that left the firm). We can also explore the effect on labour costs and hours with *ln(Cost per emp.)* (the natural log of the average yearly cost per employee) and *ln(Hours per emp.)* (the natural log of the average yearly number of hours worked per employee). Lastly, not all people working at firms are 'on the books' of those firms. Firms can also rent workers externally through employment agencies.[17] We have information on this and can study how firing costs affect the firms' use of such workers using *ln(Outsourced emp.)* (the natural log of the number of outsourced employees through agencies).

Additionally, we examine the effect of the new legislation on firm capital. We first use *ln(Tangible fix.)* (the natural log of tangible fixed assets) as the dependent variable and then examine whether firms adopt labour-saving technologies in response to increased firing costs by breaking down capital into two parts. More specifically, we construct: *ln(Mac., Equip.)* (the natural log of the sum of machinery and equipment) and *ln(Land, Build., Furn., Other)* (the natural log of the sum of land, building, furniture, and other tangible fixed capital). Furthermore, we also consider the effect on intangible capital with *ln(Intangible fix.)* (the natural log of intangible fixed assets). Lastly, besides the effect on technology investment, we also investigate the effect on human capital investment using *Train. emp. share* (the share of employees that joined training activities), *ln(Train. cost per emp.)* (the natural log of the average yearly training cost per employee), and *ln(Train. hours per emp.)* (the natural log of the average yearly training hours per employee).

[17]Note that costs of outsourced workers are included in intermediate consumption.

3.3. Heterogenous workers, firing costs, and bias in estimated TFP: a stylised model

We present the following stylised model to illustrate the importance of accounting for different worker types in the production function estimation when these types face heterogeneous changes in firing costs. Specifically, the model shows how a bias can arise in estimating the effect of firing costs on TFP, when the estimated production function pools heterogeneously affected worker types.

Consider a firm that produces output employing capital K, white-collar workers W, and blue-collar workers B. The revenues Y generated from production are:

$$Y = TFP[K^{\alpha}\left(W^{\gamma}B^{1-\gamma}\right)^{1-\alpha}] \tag{2}$$

The firm chooses its inputs to maximise profits:

$$\max_{K,W,B} Y - RK - (1+\tau^{W})\omega^{W}W - (1+\tau^{B})\omega^{B}B \tag{3}$$

where ω^W and ω^B are wages paid to white- and blue-collar workers, respectively.[18] Following a popular approach in the misallocation literature (e.g., Restuccia and Rogerson, 2008 and Hsieh and Klenow, 2009), we denote with τ^W a wedge that raises the marginal cost of white-collar workers relative to the other inputs. It can be interpreted, given the objective of this paper, as a shortcut for expected firing costs. Intuitively, consider two firms, identical except that only one of them faces firing costs for white-collar workers. The firm facing the firing costs will hire fewer white-collar workers than the firm facing no such costs, to minimise expected firing costs. This cautionary behaviour will raise the marginal product of white-collar workers, and this difference will be reflected in a positive value of τ^W for the affected firm. A similar interpretation applies to τ^B, the wedge for blue-collar workers. To simplify notation, we define:

$$\widehat{\tau}^{W} \equiv (1+\tau^{W})\omega^{W}$$
$$\widehat{\tau}^{B} \equiv (1+\tau^{B})\omega^{B}$$

First order conditions then yield:

[18] All the results derived in this section would remain valid under the assumption that firms have monopoly power.

$$\frac{B}{W} = \frac{1-\gamma}{\gamma}\frac{\widehat{\tau}^W}{\widehat{\tau}^B} \tag{4}$$

The above expression clarifies that the optimal ratio between blue- and white-collar workers is affected by the relative wedges $\frac{\widehat{\tau}^W}{\widehat{\tau}^B}$, as well as by technological factors (the elasticity γ).

3.3.1. Bias in revenue TFP

In the following analysis, we assume input elasticities α and γ are observed accurately and focus on the bias induced by not observing all production inputs separately. Suppose a researcher follows the standard approach to measure revenue-based TFP as a residual from the production function, but only observes total employment $L = W + B$, and assumes the production function is

$$Y = \theta K^{\alpha} L^{1-\alpha}. \tag{5}$$

TFP will be measured as:

$$\log TFP^{biased} = \log Y - \alpha \log K - (1-\alpha)\log L \tag{6}$$

while the unbiased revenue TFP is obtained from Equation (2):

$$\log TFP^{true} = \log Y - \alpha \log K - \gamma(1-\alpha)\log W - (1-\gamma)(1-\alpha)\log B \tag{7}$$

The bias is then the difference, $Bias \equiv \log TFP^{biased} - \log TFP^{true}$, which can be expressed as:

$$Bias = -(1-\alpha)\log\left[\left(\frac{1-\gamma}{\gamma}\frac{\widehat{\tau}^W}{\widehat{\tau}^B}\right)^{\gamma-1} + \left(\frac{1-\gamma}{\gamma}\frac{\widehat{\tau}^W}{\widehat{\tau}^B}\right)^{\gamma}\right] \tag{8}$$

From Equation (8), it is easy to determine the three following results. First, the $Bias$ is always negative. In other words, using Equation (5) as the production function when the true production function is Equation (2), results in *underestimating* revenue TFP.

Second, Equation (8) implies that the absolute value of the bias is U-shaped in the relative wage-adjusted wedges $\frac{\hat{\tau}^W}{\hat{\tau}^B}$. It goes to infinity, if $\frac{\hat{\tau}^W}{\hat{\tau}^B}$ goes to zero or to infinity, and it reaches its minimum for some intermediate value. Furthermore, the relation between $\frac{\hat{\tau}^W}{\hat{\tau}^B}$ and the bias depends on the elasticity γ.

Third, suppose a researcher uses Equation (6) to estimate the causal effect of firing costs on productivity as the difference Δ log TFP^{biased}, estimated before and after a given exogenous change in firing costs. The above discussion clarifies that $\Delta \log TFP^{biased} \equiv \Delta \log TFP^{true} + \Delta Bias$. Therefore, it is only possible to get an unbiased estimated causal effect of firing costs on productivity from Equation (6) if $\Delta Bias = 0$ (i.e., if the relative wedge $\frac{\hat{\tau}^W}{\hat{\tau}^B}$ remains exactly the same before and after the change in firing costs). However, if firing costs change *differentially* across worker types, then $\Delta Bias \neq 0$ and the estimated causal effect of firing costs on productivity will be biased. Moreover, depending on whether $\Delta Bias$ is positive or negative (i.e. whether the underestimation of revenue TFP becomes smaller or larger after the change in firing costs), *the causal effect of firing costs on productivity will be underestimated or overestimated*. Furthermore, note that also $\Delta Bias$ might be different across firms depending on the relative importance of blue- and white-collar workers in production, as determined by the parameter γ.

In Section 4.2.2, we provide a simple quantification of the problem in our setting, to get an idea of the magnitude of the bias.

3.4. Empirical model, identification, and summary statistics

To study the effects of firing costs, the Belgian labour market provides a unique setting by originally discriminating in dismissal rights between blue- and white-collar workers and then by removing discrimination as of 2014. We exploit this change in the Belgian legislation by estimating a difference-in-differences model where we compare blue-collar firms (firms that have a high pre-period share of blue-collar workers) to white-collar firms (firms that have a low pre-period share of blue-collar workers).[19] Our analysis thus compares two groups of firms with one group experiencing an increase in firing costs relative to the other group. Our benchmark specification is the following DiD model:

[19]Note that, in Bel-first, besides blue- and white-collar workers, workers can also be classified as *management* and *other* workers. However, the number of workers in those categories is often zero (the sum of workers in those two groups is around 1% of employment for the average firm) and our results do not change if we exclude those categories from the firm's workforce. As such, *firms with a low share of blue-collar workers* are identical to *firms with a high share of white-collar workers*.

$$Y_{i,t} = \beta\ Blue - collar_i * Post_t + \Pi\ Firm\ controls + \mu_i + \theta_{st} + \varepsilon_{i,t} \tag{9}$$

where $Y_{i,t}$ denotes TFP of firm i in year t. In further tests, $Y_{i,t}$ also stands for variables related to employment and investment. $Blue - collar_i$ is a dummy variable equal to 1 if firm i's average share of blue-collar workers during the pre-period (2009-2012) was above the median, and to 0 otherwise. Pre-period averages are used to mitigate the concern that firms' time-specific labour demand confounds our treatment.[20] $Post_t$ is a dummy variable equal to 1 for observations in the period of 2014-2017, and to 0 for observations in the period of 2009-2012. We exclude observations in 2013 to eliminate contamination from announcement effects. *Firm controls* are the one-year lagged values of the firm's *size* (the natural log of total assets), *leverage* (the sum of long- and short-term debt, divided by total assets), *EBITDA-to-assets* (the ratio of EBITDA to total assets), *cash holdings* (the ratio of cash holdings to total assets), *capital-to-labour* (the natural log of the ratio of tangible fixed assets to total number of employees), and the contemporaneous value of the firm's *age* (the number of years since the incorporation date). The model also includes firm fixed effects (μ_i) and NACE 2-digit industry × year fixed effects (θ_{st}).[21]

We cluster standard errors at the firm level. One may also prefer to cluster standard errors at the industry level to allow for the correlation of shocks within an industry because we separately estimate the production function for each industry. However, we do not opt for this as a benchmark because our main sample only contains 35 industries. Nonetheless, in our robustness tests, we show that our results are robust to clustering standard errors at the industry-level.

The coefficient of interest is β, which quantifies the effect of the legal change on firms' productivity. Specifically, β reflects the difference in the evolution of TFP in blue-collar firms relative to white-collar firms (i.e., one group of firms that experienced an increase in firing costs relative to the other group) from before to after the legal change.

Endogeneity concerns. One natural concern for our identification strategy would be that the enactment of the new labour legislation is related to pre-period firm characteristics. However, this concern is likely not relevant because the introduction of the new notice periods

[20] In our robustness tests, we show that our results hold when we estimate Equation (9) by defining the blue-collar dummy based on the firm's share of blue-collar workers at the end of 2012 (instead of the pre-period average share) or with a treatment intensity variable (the firm's pre-period average share of blue-collar workers).

[21] Note that $Blue - collar_i$ and $Post_t$ are not separately included in Equation (9) because they are subsumed by fixed effects.

was based on the court's decision to remove discrepancies between blue- and white-collar workers, rather than based on firm performance.

Another concern would be that firms might have reacted to the new notice periods before the official announcement of the new legislation, especially given that in 2011 the Belgian Constitutional Court set 8 July 2013 as a deadline to harmonize the treatment of worker types. This is not likely to invalidate our results for two reasons. First, the agreement was reached just a few days before the deadline. This suggests that the exact terms of the agreement were not clear until the end of the deadline and firms might have waited to see the final agreement before reacting. Second, in Section 4.1.1, we run a dynamic version of Equation (9) and show that the effect of the new notice periods materialized after but not before the introduction of the new notice periods. This corroborates that the new labour legislation was an exogenous shock.

Additionally, our pre-period ends in 2012 when the European Central Bank started implementing financial support programs, such as the Outright Monetary Transactions program (Acharya et al., 2019), in response to the sovereign debt crisis. A potential concern would be that these programs might have differently affected blue- and white-collar firms and thus might confound our estimates. Our comparison of Belgian firms with French and German firms in the same industries in Section 5.2 not only helps to isolate the effects on white-collar firms from the effects on blue-collar firms, but –by comparing equally affected European firms in the same industries– also controls for any differential impact that the ECB's support programs might have. The corresponding results corroborate our main findings and lead us to conclude that crisis support measures are not likely to contaminate our results.

Lastly, another concern would be that some unobserved factors might bias our estimates. In our main model, firm fixed effects already control for time-invariant firm characteristics. Controlling for time-varying unobserved factors (e.g., changing business models), however, might also be crucial. To capture time-varying factors, we use NACE 2-digit industry × year fixed effects to net out any variation that is common to all firms in the same industry and year. In our robustness tests, we further restrict our main model by using 2-digit industry × province × year fixed effects. In addition, just as we control for several time-varying firm characteristics, we also perform a matching procedure to make blue- and white-collar firms in the regression sample as comparable as possible in their observed pre-period characteristics, which can also help reduce bias from unobserved factors (Angrist and Krueger, 1999).

Matching strategy and summary statistics. We carry out a nearest neighbor matching of propensity scores to select a sub-sample of white-collar firms that are similar to blue-collar firms in their observable characteristics. To do so, we first estimate a logit regression using the 2012 values of covariates, with a dependent variable that is a dummy equal to 1 for blue-collar firms, and to 0 for white-collar firms. As explanatory variables, we include the firm controls from our benchmark specification (*size, age, leverage, EBITDA-to-assets, cash holdings, capital-to-labour*), as well as the log growth rate of the number of blue-collar workers, the log growth rate of the number of white-collar workers, and the log growth rate of tangible fixed assets to ensure that our matched samples have similar pre-period trends in labour and capital growth. We then perform a nearest neighbor matching of propensity scores (i.e., the predicted probabilities of the logit model) with exact matches in NACE 2-digit industries. We retain one white-collar firm for each blue-collar firm, and allow that a white-collar firm can be used as a match for multiple blue-collar firms.[22] Although matching with replacement increases the variance of the estimator, it enhances the average quality of matches and ensures that the estimated effect is not sensitive to the order in which blue-collar firms are matched to white-collar firms (Rosenbaum, 1995; Smith and Todd, 2005; Roberts and Whited, 2013).

[**Table 2**]

[**Table 3**]

In total, we are able to successfully match 3,544 (around 98%) of the 3,612 blue-collar firms to white-collar firms. Table 2 compares the pre-period sample means of the control variables for blue- and white-collar firms before- and after the matching procedure.[23] We assess the quality of matching by performing t-tests as well as by looking at standardized differences in covariate means. An absolute value of standardized mean difference that is less than 0.1 is considered to imply a negligible imbalance between samples (Normand et al., 2001). According to the t-tests, the two groups of firms significantly differ in covariates (except cash holdings) before the matching procedure. The standardized mean differences indicate a non-negligible imbalance for *size*, *EBITDA-to-assets*, and *capital-to-labour* between the non-matched blue- and white-collar samples. After the matching procedure, according to both

[22] In Section 4.1.1, we show that our results hold when we retain two or three white-collar firms for each blue-collar firm.

[23] Table E1 compares the pre-period sample means of all the variables used in our regressions for the two groups before- and after the matching procedure.

the t-tests and the standardized mean differences, there are no meaningful differences in any covariate between blue-collar and white-collar firms on average. Thus, our matching procedure yields blue-collar and white-collar samples which are more similar in their characteristics, in comparison with the non-matched sample. Table 3 presents summary statistics for the entire sample period after the matching procedure.

4. Results

4.1. Firing costs and productivity: Main results

To assess the effect of firing costs on productivity, we estimate Equation (9) using the natural log of TFP as the dependent variable. The results are reported in Table 4.

[**Table** 4]

Column (1) presents the results from the estimation of Equation (9) without fixed effects and firm controls. The coefficient of interest on the interaction term is -0.05 and significant at the 10% confidence level. Column (2) adds NACE 2-digit industry×year fixed effects to control for factors that are common to all firms in the same industry and in the same year. After accounting for industry-year level heterogeneity, the estimated coefficient is -0.057, which is larger in magnitude. Furthermore, the coefficient is now significant at the 1% confidence level. Column (3) additionally adds firm fixed effects which account for any time-invariant firm heterogeneity. The inclusion of firm fixed effects besides industry-year fixed effects further increases the magnitude of the estimated coefficient to -0.06. Lastly, column (4) presents the results from the estimation of our preferred and most restricted specification which includes full sets of fixed effects and firm controls. The inclusion of firm controls decreases the estimated coefficient to -0.056. However, this coefficient still is significant at the 1% confidence level.

Summing up, we find that the increase in firing costs for blue-collar workers caused a large and statistically significant reduction in TFP. Based on the point estimate in column (4), firms with an above-median share of blue-collar workers during the pre-period (2009-2012) experienced a 5.6% decline in TFP ex-post, relative to firms with similar characteristics except having a below-median share of blue-collar workers.

4.1.1. Robustness checks

We perform various tests to ensure the robustness of our results in Table 4.

Timing of the effect. To address concerns about anticipation effects, we investigate when exactly the effect of the new labour act materialized. To do so, we estimate the dynamic version of Equation (9). Specifically, we interact $Blue-collar_i$ with year dummies, using 2012 as the omitted benchmark. Columns (1) and (2) of Table 5 present the results without and with firm controls, respectively. In both columns, the point estimates on the interactions of the blue-collar dummy with pre-treatment years are statistically insignificant. This suggests that there is no significant difference in the evolution of productivity between blue- and white-collar firms for the years 2009, 2010, and 2011, relative to 2012, which is consistent with the parallel trends assumption. Additionally, the estimates on the interactions of the blue-collar dummy with post-treatment years (2014, 2015, 2016, and 2017) are all significant at the 1% confidence level. The magnitude of the estimated effects seems to be constantly growing over the years in column (1), while in column (2) the magnitude of the effects seems to decrease somewhat in 2017 but remains statistically and economically significant. Overall, our dynamic estimates suggest that firing costs have persistent effects on firm productivity. The persistent effect may be explained by the fact that firing costs for blue-collar workers dynamically increased with tenure post-regulation more than they did pre-regulation, making it difficult for blue-collar firms to adjust their workforce even 4 years after the legal change.

[**Table 5**]

Different productivity measures. Panel A of Table 6 illustrates the results from the estimations of Equation (9) with different productivity measures. In column (1), TFP is recovered through the one-step estimation procedure suggested by Wooldridge (2009), where we use intermediate inputs as a proxy for unobserved productivity. In column (2), using our baseline estimation method, we estimate a translog production function which allows for more flexible elasticities (i.e., other than one in the case of Cobb-Douglas production function) and for both substitution and complementarity between inputs. In column (3), we recover TFP using our baseline approach (i.e., the approach of Ackerberg et al. (2015)), but instead of constant elasticities, we estimate elasticities separately for the pre- and post-period for each NACE 2-digit industry. We do so to account for the possibility that the increase in firing costs might incentivise firms to adopt new technologies that are less dependent on blue-collar workers, in which case imposing constant elasticities for the whole period would lead to biased

TFP estimates. To further address the concern of time-varying elasticities, in column (4), we estimate the production function for each NACE 2-digit industry×year, but use OLS because the production function cannot be estimated for each year with the estimators of Wooldridge (2009) or Ackerberg et al. (2015). For reference, in column (5), we estimate the production function for each NACE 2-digit industry with constant elasticities through OLS. Lastly, in column (6), we estimate the production function using revenues as output and materials as inputs to relax the assumption that the output elasticity of materials is equal to one.[24] Regardless of the TFP measure, the estimated effect is always negative and significant. Furthermore, the dynamic estimates in Panel B show that all different specifications support the ex-ante parallel trends assumption, and show similar dynamics ex-post, with an estimated drop in TFP (relative to 2012) that peaks around the year 2016. The magnitude of the peak depends on the specification, ranging between 2.1% and 6.6%, but most of the specifications show a significant drop of around 5%.

[Table 6]

Additional robustness tests. Appendix E includes the results from further robustness tests. Table E2 repeats our main estimation as well as its dynamic version on alternative matched samples and on the unmatched sample. In Table E3, we (i) cluster standard errors at the NACE 2-digit industry level, instead of at the firm level, (ii) further restrict our main model by including NACE 2-digit industry×province×year fixed effects, (iii) control for the interactions of the time-invariant pre-period control dummies and the post dummy (as in Costello (2020)) to address the possible concern that firm-specific factors might be correlated with the firm's workforce composition (i.e., selection into blue- and white-collar firm groups), (iv) define the blue-collar dummy based on the firm's share of blue-collar workers at the end of 2012, instead of the average pre-period share, (v) collapse time series data into two periods (a pre- and a post-period) around the event to address the autocorrelation issue that might arise from panel data models (Bertrand et al., 2004), and (vi) perform a placebo test where we pretend that the legal change occurred in 2009 instead of 2014. Finally, Table E4 additionally incorporates firms that operate with only blue- or only white-collar workers as well as firms that do not report intermediate inputs, by estimating the production function through alternative approaches. Overall, all these estimates confirm the validity of the result in Table 4.

[24]The sample size decreases by 14% for this analysis. This is because, revenue data, unlike value-added data, is only available for large firms.

4.2. Using total employment as the labour input in the production function

In this section, we investigate how our results change if we pool blue- and white-collar workers in a single employment input in our production function. We provide evidence from both a regression analysis and a quantitative exercise based on our stylised model shown in Section 3.3.

4.2.1. Regression results

In Table 7, we consider a measure of TFP obtained after estimating the production function with the natural log of the firm's total number of workers, instead of separately including the natural log of blue- and white-collar workers. Table E5 in the Appendix shows that the production elasticities of blue- and white-collar workers are substantially different. Furthermore, it is plausible to assume that blue- and white-collar workers are complementary rather than substitute inputs in the production process. It follows that a production function ignoring this difference is likely to be misspecified. Accordingly, in Table 7, we find that the effect of the legal change is measured to be 70% smaller than in our benchmark estimation in Table 4 and statistically insignificant. In the next section, we show that this finding is consistent with the bias implied by our stylised model (see Section 3.3). Overall, these results highlight the importance of separately identifying the inputs that are differentially affected by the legal change.

[Table 7]

An alternative explanation of why we do not find a drop in TFP in Table 7, could be that firms have a number of workers in a "grey" zone (i.e. that do both physical and non-physical work) that were previously given blue-collar contracts and are now given white-collar contracts, but actually still do the same work. According to this interpretation, pooling all workers together would then make no difference as there is no real change in the work done. Conversely, separating the workers in the production function might make a difference as previously these *grey* workers received the output-elasticity of blue-collar workers, while now they receive the output-elasticity of white-collar workers. However, it is unlikely that blue-collar contracts were provided for white-collar jobs after the legal change because Belgian authorities strictly monitor whether employers correctly classify workers into blue- and white-collar types based on workers' primary tasks. Moreover, if the presence of these *grey* workers, and their changing category between the pre- and post-periods, was important enough to drive the results, then it should also cause substantial changes in blue- and white-collar output elasticities between the periods. Yet, we do not find any evidence of such changes (see the discussion of Figure 1 in Section 4.3.2 below). In addition, under changing elasticities, we would also expect that our benchmark results change substantially when we consider time-varying elasticities. However, this is not the case (see columns (3) and (4) of Table 6).

4.2.2. Quantitative exercise

Following our regression analysis, we use Equation (8) from our stylised model in Section 3.3 to quantify the change in the bias ($\Delta Bias$) induced by the change in firing costs, using the following parameter values based on our data. We set α to 0.15, consistent with the values we estimate across industries (see Figure 1). From the Belgian administrative data, we estimate the wage ratio $\frac{\omega^W}{\omega^B}$ to be on average equal to 1.47. Furthermore, from Figure 1, we infer values of γ of 0.58 for the average firm in the sample, and values of 0.82 and 0.47 for firms with low and high intensity of blue-collar workers, respectively. We assume that firing costs are normalised to zero for blue-collar workers before the reform, and they increase to the level for white-collar workers after the reform. We consider a benchmark value of $\tau^W = 0.2$, indicating expected firing costs of around 20% of yearly wages. This number is relatively high and consistent with the high firing costs documented in Section 2 for white-collar workers. We also consider alternative values of $\tau^W = 0.3$ and $\tau^W = 0.1$.

[**Table 8**]

Table 8 shows that, for the average firm, estimating TFP without distinguishing between blue- and white-collar workers would *underestimate* the negative effect of firing costs on productivity substantially, between 2.85% and 0.87%. Furthermore, this underestimation is significantly larger for blue-collar firms, leading to a reduction in the difference in TFP between blue- and white-collar firms. This bias can at least in part explain why we estimate a smaller effect of the reform on productivity, when we pool worker types in the production function.

4.3. Firing costs and Productivity: Channels

We documented that the increase in firing costs caused a large, statistically significant, and persistent drop in TFP. In this section, we want to shed light on what are the likely determinants of this result.

First, the theoretical literature argues that firing costs distort optimal hiring and firing decisions and as such reduce TFP because these costs prevent firms from firing inefficient workers and hiring productive new workers. We will therefore verify whether the legal change caused significant changes in hiring and firing patterns.

Second, we will look for evidence of whether the affected firms transitioned to more capital-intensive (or white-collar intensive) technologies, which might also be related to the change in TFP.

Finally, we will investigate whether affected firms tried to mitigate the impact of the legal change along other relevant dimensions.

4.3.1. The effect on employment

To shed light on the mechanisms through which increased firing costs cause a drop in productivity, we first analyse how firms adjusted their workforce after the legal change. Panels A and B of Table 9 show the results from the estimation of Equation (9) and its dynamic version, respectively, using dependent variables related to firms' employment. We first analyse the effect on workforce composition. Columns (1) and (2) use the natural logs of the number of blue-collar workers and the number of white-collar workers as the dependent variables, respectively. The average effect on the number of blue-collar workers is negative. Although it is not significant, the dynamic estimates illustrate that blue-collar firms gradually reduced their blue-collar workers after the legal change, and significantly so from 2015. At the same time, blue-collar firms on average hired more white-collar workers. This effect is already significant in 2014. As for the reduction in blue-collar workers, the effect increased gradually over time. Overall, these results suggest that changes in firing costs lead to changes in firms' workforce composition. Taken together with the negative effect on TFP, the results suggest that the changing workforce composition suffered from a misallocation of labour inputs. This source of inefficiency is corroborated by the results obtained using quartiles of treatment intensity in Table E12, as those results show that the reduction in TFP and the change in workforce composition increase together in the upper quartiles. Overall, our study contributes to prior work (Autor et al., 2007; Cingano et al., 2016) by documenting a novel mechanism for which firing costs might lead to a decline in productivity.

[Table 9]

We further investigate the net effect on total employment and employee turnover. According to theory, the effect of firing costs on employment levels is ambiguous, as an increase in firing costs not only reduces the firm's willingness to fire, but also its willingness to hire (Lazear, 1990; Bentolila and Bertola, 1990). We separate the two effects in columns (3)-(5), where the dependent variables are the natural logs of the total number of employees, the number of employees hired during the year, and the number of employees that left the firm during the year, respectively.[25] While the effect on total employment level is not significant, the estimates show that an increase in firing costs overall induces firms to hire *and* fire less, in line with the theoretical prediction and consistent with the existing empirical evidence (Kugler and Saint-Paul, 2004; Kugler and Pica, 2008; Marinescu, 2009; Alpysbayeva and Vanormelingen, 2022). Specifically, hiring declined by about 12%, immediately and persistently up to 3 years, and firing peaked at minus 16% after 3 years. Overall, given the negative effect on TFP, these results suggest that firing costs hinder firms from optimally hiring and firing, signifying the misallocation effects of firing costs.

Our data also allows us to observe the firm's number of temporary and permanent employees at the end of the fiscal year, as well as -for large firms- how many of the newly hired and exiting workers (i.e., those in columns (4) and (5) of Table 9) are temporary and permanent employees. The results related to these variables are shown in Table E6 in the Appendix. We do not find a significant effect on the levels of temporary and permanent employees (measured at fiscal year-end). However, the results indicate that increased firing costs significantly reduce the turnover of employees with permanent contracts (both hiring and exiting), while the turnover of employees with temporary contracts remains largely unaffected by the new legislation.

Column (6) of Table 9 analyses how firing costs affect the use of outsourced labour. The dependent variable is the natural log of the number of workers outsourced through employment agencies. The results show that the legal change induced firms facing increased firing costs to resort more to outsourced labour. This result, which is in line with Autor (2003), indicates that firms tried to increase labour flexibility using external contracting, to compensate for the lower flexibility of blue-collar workers.

In addition to the effect on the number of employees, we also assess the impact on labour costs and hours. In columns (7) and (8), we use the natural logs of the average yearly cost per employee and the average yearly number of hours worked per employee, respectively. While the effect on the former is not significant, the effect on the latter is positive and significant,

[25]Unfortunately, firms do not need to report how many of entering and exiting employees are blue- or white-collar workers.

suggesting that blue-collar firms overall responded to increased firing costs by using their existing workforce more intensively.

Besides looking at the change in firms' employment levels from pre- to post-period, in Appendix E.7, we also investigate the effect of increased firing costs on firm exit and find a moderate increase in the number of exiting firms. Additionally, given the concern that firm exits confound our productivity measure, in Appendix E.8, we also show that our main result barely changes when we construct TFP through accounting for firms' survival probabilities (as in Olley and Pakes, 1996).

4.3.2. The effect on physical & human capital

Next, we investigate whether the legal change caused firms to adopt different and possibly more capital-intensive technologies. Theory suggests that the impact of firing costs on capital deepening is actually ambiguous (Cingano et al., 2016). On the one hand, in a competitive model with no labour and financial market frictions, increased labour costs may induce firms to substitute labour with capital (Autor et al., 2007; Cingano et al., 2016), towards more capital-intensive technologies in the long-term (Caballero and Hammour, 1998; Alesina et al., 2018). Additionally, the reduced dismissal threat (due to higher firing costs) may lead workers to invest in firm-specific capital, leading to an increase in productivity and investment rates (Nickell and Layard, 1999). On the other hand, in models with labour market frictions, increased firing costs may aggravate the "hold-up" problem which reduces firms' willingness to invest, resulting in a decrease in the stock of capital per employee (Bentolila and Dolado, 1994; Garibaldi and Violante, 2005). Besides, higher labour costs raise operating leverage and crowd out financial leverage, which could limit firms' ability to finance capital investments (Simintzi et al., 2015; Serfling, 2016; Bai et al., 2020).

[**Table 10**]

Table 10 presents the results from the estimation of Equation (9) and its dynamic version using dependent variables related to firm capital. In column (1), the dependent variable is the natural log of tangible fixed assets. The effect is positive and significant at the 10% confidence level, indicating a moderate increase in tangible fixed assets for blue-collar firms relative to white-collar firms. The dynamic version in column (2) shows that the effect becomes significant only towards the end of the period in 2016 and 2017, when blue-collar firms raise their capital stock by up to 10%, with respect to the pre-reform period, relative to white-collar firms. Although at first glance this seems to be in line with the arguments that an increase in firing costs may lead to capital-labour (blue-collar labour in our case) substitution, we break down tangible fixed assets into two categories to delve deeper into whether and how firms actually change their production techniques. Specifically, we use the natural log of the sum of machinery and equipment in columns (3)-(4) and the natural log of the sum of land, building, furniture, and other tangible fixed assets in columns (5)-(6).[26] We find that the increase in the firm's total capital stock stems from the increase in land, building, furniture, and other capital, which is perhaps related to the increase in white-collar workers rather than to a transition to capital-intensive technologies. Indeed the estimates show that firing costs on average did not lead firms to buy machines and equipment (see columns (3) and (4)). This suggests that the average firm did not opt to adopt new technology in response to increased labour costs. In addition, columns (7)-(8) show that the effect on intangible fixed assets is not significant, further corroborating the absence of technology adoption.

[**Figure 1**]

We further investigate the implication of the legal change for technology adoption in Figure 1, which shows input elasticities for industries with varying intensities of blue-collar workers, separately estimated pre- and post-reform. The changes in elasticities overall appear to be small and insignificant, consistent with the absence of changes in technology.

[**Table 11**]

Besides the effect on physical capital, we also investigate the effect of firing costs on human capital, using the information on firms' training activities from Bel-first. We examine the effect of increased firing costs on (i) the share of employees that joined training activities, (ii) the average yearly training cost per employee, and (iii) the average yearly training hours per employee. As shown in Table 11, we do not find a significant effect on training activities. This suggests that firms did not invest in human capital after the legal change and/or that the demand for training activities from employees remained unchanged. Contrary to Acharya et al. (2014), the latter would imply that workers did not invest more in firm-specific capital after becoming more entrenched.

[26] In unreported tests, as in Autor et al. (2007), we also estimate the production function by including these two capital items separately, instead of using the total capital stock. When we use this TFP measure as the dependent variable, the estimated effect is almost identical to the one in Table 4.

On the whole, our results show that the legal change implied a large and persistent decline in job flows (less hiring and firing), and a substantial and equally persistent drop in productivity. However, firms did not seem to react to this change by adopting more capital-intensive technologies or by investing in human capital. Rather they exploited other margins, such as outsourcing and increasing hours worked, to at least partially compensate for the lower flexibility of blue-collar workers.

4.4. Further Robustness Tests

Results across industries with varying change in the notice periods for blue-collar workers. As shown in Table 1, changes in the notice periods for blue-collar workers were not uniform across industries. If firing costs drive our results, then our results should be stronger for industries where increases in the notice periods for blue-collar workers were higher. To examine this, we run our main model (9) by splitting the sample into two groups: (i) industries where the notice period for a blue-collar worker with 10-year tenure increased by less than 168 days (i.e., less affected industries), and (ii) industries where the notice period for a similar blue-collar worker increased by 168 days or more (i.e., more affected industries). We use 168 days as the threshold because the notice period for a blue-collar worker with 10-year tenure increased from 42 to 210 days in the majority of the industries.[27] Table E9 illustrates that our main results are overall stronger for more affected industries.

Results across initially exempted industries. Our main sample includes the construction and upholstery & woodworking industries in which blue-collar workers performing at temporary and mobile workplaces were initially exempted from increased notice periods. We do not exclude these industries from our main sample because those exceptional rules did not cover all types of blue-collar workers and thus firms operating in these industries were still affected (albeit presumably less). We examine this by estimating Equation (9) with an additional interaction variable that is equal to 1 if the firm operates in the construction or upholstery & woodworking industries, and to 0 otherwise. In line with our intuition, Table E10 shows that there is no significant difference in the effects between firms in industries with some initial exceptions and firms in other industries.

Results with treatment intensity. We also estimate our main model (9) with a treatment intensity variable instead of our benchmark dummy. That is, we replace the blue-collar dummy with its continuous counterpart: the firm's pre-period average share of

[27]Note that our split is identical when we choose the threshold based on the increase in the notice period for a blue-collar worker with 1-year or 5-year tenure.

blue-collar workers. Panels A and B of Table E11 show the average estimated effects and dynamic estimates, respectively. All results are qualitatively similar to our main results.

Linear effects. In addition, we examine whether the effects of the new notice periods are linear to ensure that our empirical model is well-specified. To do so, we estimate Equation (9) using quartiles of the treatment intensity variable, rather than only the median (i.e., our blue-collar dummy). The first quartile is excluded as a reference category. As shown in Table E12, we find that the magnitude of the effect overall tends to increase in the upper quartiles.

Results without controls. As a benchmark, we control for some firm characteristics to make blue- and white-collar firms as comparable as possible. Nevertheless, Table E13 shows that our main results for firm employment and capital hold without control variables in both the matched (as shown in Panel A) and non-matched (as shown in Panel B) samples.[28]

5. Additional Analyses

5.1. The role of credit access

We have shown that, when hit by an increase in firing costs, firms reduce hiring and firing, change the composition of their workforce, and experience a decline in TFP. We further investigate how this effect varies with financial constraints. As financially unconstrained firms are better able to pay larger dismissal costs to replace their inefficient workers, they may experience a smaller decline in productivity when firing costs increase. We capture firms' financial constraints in our sample by estimating the availability of bank credit from loan-level data. Because the firms in our sample are private and relatively small, bank credit is their most important source of external finance. Our measure differs from the existing literature, which has mainly used *cash* as a proxy for financial constraints (e.g., Cingano et al., 2010, 2016). Despite its popularity, however, cash may not truly reflect firms' financial constraints, since it could also reflect the anticipation of future investment or precautionary motives (e.g., Berg, 2018). To address the resulting endogeneity concerns, we therefore use loan-level information from the Belgian credit register and estimate credit supply shocks that firms experience from their lenders by using the approach of Degryse et al. (2019), which is in the spirit of the methodology of Amiti and Weinstein (2018). Appendix D illustrates the construction of firm-level credit shocks in detail.

[28]The results for TFP without controls are reported in Tables 4 & 5 for the baseline matched sample and in Table E2 for the other matched and unmatched samples.

[**Table 12**]

Using TFP as the dependent variable, we then estimate Equation (9) by splitting the sample at the median of the firms' pre-period credit supply measure. Because credit supply conditions likely matter more for firms with less spare debt capacity, we also split the sample at the median of firms' long-term debt-to-fixed assets ratio, calculated in 2012 end year. Table 12 illustrates the results. Our sample is now smaller in size as we focus only on firms that borrowed before the legal change. Columns (1)-(3) exhibit the results for firms with lower long-term debt to fixed assets ratio (i.e., firms with more spare debt capacity). Columns (4)-(6) illustrate the results for firms with higher long-term debt to fixed assets ratio (i.e., firms with limited spare debt capacity). Within each group, we demonstrate the results for the entire group of firms, firms facing tight credit supply conditions (i.e., those experiencing low credit supply shocks), and firms enjoying lax credit conditions (i.e., those experiencing high credit supply shocks), respectively. The results show that credit supply conditions do not seem to matter for firms with higher spare debt capacity. In contrast, among firms with lower spare debt capacity, firms enjoying lax credit supply conditions on average experienced a smaller decline in TFP than firms facing tight credit supply conditions. Overall, these results suggest that relaxed credit supply conditions may help firms efficiently cope with increases in firing costs.

5.2. Belgian versus German and French firms

In Appendix E.14, we also compare Belgian firms to German and French firms (i.e., firms in the two largest neighbouring countries of Belgium) that are not affected by the Belgian labour legislation and that, to the best of our knowledge, did not experience a significant shock (legal or other) that would affect the composition of their workforce during our sample period. This analysis is interesting for two reasons. First, it helps us to separately assess the effect of the new Belgian labour legislation on blue- and white-collar firms. Second, it helps us to address potential concerns that financial support programs, such as the Outright Monetary Transactions program (Acharya et al., 2019), implemented in response to the sovereign debt crisis, might have differently affected blue- and white-collar firms.

We find that Belgian firms in blue-collar industries on average experienced a decline in value-added, relative to a matched sample of German and French firms in the same industries.[29] At the same time, we also find that Belgian firms in white-collar industries

[29]We do not have information on non-Belgian firms' blue- and white-collar worker composition. For this

experienced an increase in value-added (despite the abolition of trial periods, improved outplacement rights, and increased protection against unfair dismissals), relative to their matched non-Belgian counterparts in the same industries. These results suggest that the Belgian changes in the notice periods had symmetric effects and corroborate that notice periods are one of the most important elements of employment protection (OECD, 2013). Additionally, the fact that we now use unaffected European firms in the control group, further mitigates the concern that European programs to overcome the sovereign debt crisis drive our main results by differently affecting blue- and white-collar firms.

6. Conclusion

This paper examines the effect of firing costs on productivity and resource allocation by exploiting the 2013 Belgian labour reform that induced a plausibly exogenous and large increase in firings costs for blue-collar workers relative to white-collar workers. Using a DiD research design, we find that blue-collar firms (i.e., firms with an above-median initial share of blue-collar workers) on average experienced a 5.6% decline in TFP, relative to white-collar firms (i.e., firms with a below-median initial share of blue-collar workers), after the legal change. We highlight the importance of accounting for different worker types in the production function when these types face heterogeneous changes in firing costs. Specifically, both our stylised model and regressions show that pooling differently affected workers in a single employment input leads to a significant underestimation of the effect of firing costs on productivity.

Our results strongly suggest that an increase in firing costs reduces productivity because it distorts firms' optimal hiring and firing policies. Specifically, we find that the legal change in Belgium reduced job flows among blue-collar firms relative to white-collar firms, with an immediate drop in hiring of around 12% that persists for up to 3 years, and a gradual drop in firing that peaks at minus 16% after 3 years. Moreover, the worker-type composition changed significantly as a consequence of the legal change. Compared to white-collar firms, blue-collar firms reduced the number of blue-collar workers, but increased the number of white-collar workers.

Different from the related literature, we do not find evidence that higher firing costs

reason, we conduct this analysis at the industry level. That is, we compare Belgian firms in blue-collar (or white-collar) industries to German and French firms in the same industries. Furthermore, because of this data limitation, we do not use TFP as the dependent variable, since estimating elasticities separately for blue- and white-collar workers is crucial in our setting (see the discussions in Sections 3.3 and 4.2). Therefore, we instead look at the effect on value-added.

spur the adoption of labour-saving technologies (different from the findings of Autor et al. (2007) and Cingano et al. (2016)) or investment in human capital (different from the findings of Acharya et al. (2014)). Firms instead try to mitigate the impact of higher firing costs through alternative margins, such as hiring relatively fewer workers under a permanent contract, increasing their reliance on outsourced workers (through employment agencies), and increasing the hours worked per worker.

Additionally, by utilizing credit data from the Belgian credit register, we illustrate that the decline in productivity was smaller for firms with better access to credit, relative to firms facing tight credit supply conditions. This suggests that access to credit might help firms to efficiently replace workers when firing becomes more costly.

Our paper provides insights for policymakers regarding the effects of firing costs. While increased firing costs may provide benefits for incumbent workers, our results suggest that those benefits should be considered jointly with the potential unintended outcomes; the misallocation of resources, lower firm-level TFP, reduction in new hires with permanent contracts, more employee outsourcing, and increased working hours.

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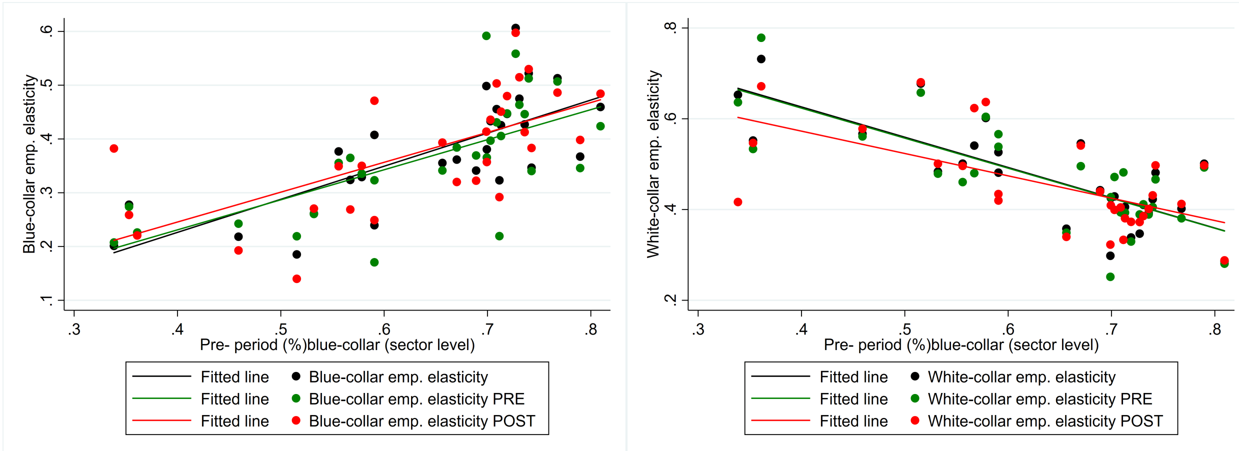
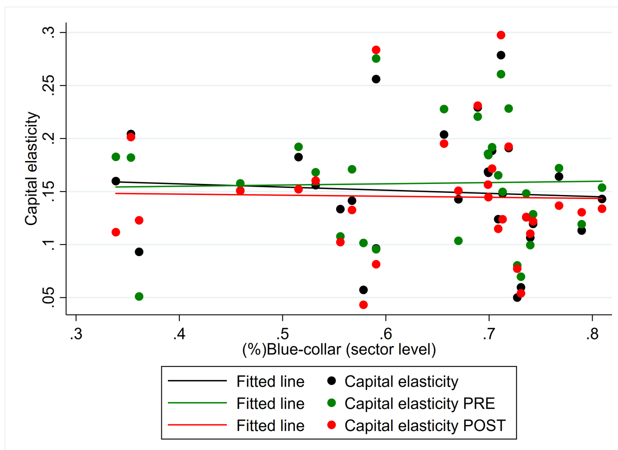


(a) Elasticities for blue-collar workers (b) Elasticities for white-collar workers



(b) Elasticities for capital

Figure 1: Comparing pre-period and post-period industry-level elasticities
This figure shows the pre-period (2009-2012) and post-period (2014-2017) industry-level elasticities for blue-collar workers (sub-figure a), white-collar workers (sub-figure b), and capital (sub-figure c). The x-axis shows the industry's average share of blue-collar workers over the pre-period. The y-axis illustrates the elasticity of substitution.

Table 1
Notice periods before and after the regulation

		Pre-regulation period 10-year seniority		Post-regulation period 10-year seniority	Difference
		Notice Periods for White-Collar Workers			
32,254 $\geq$ Gross annual wage		243 days	$\longrightarrow$	210 days	-33 days
32,254 $<$ Gross annual wage		303 days	$\longrightarrow$	210 days	-93 days
		Notice Periods for Blue-Collar Workers			
e.g. industries (Belgian joint committee no.)	*%blue-collar workers*				
Food (118)	64%	112 days	$\longrightarrow$	210 days	+98 days
Chemical (116)	60%	63 days	$\longrightarrow$	210 days	+147 days
Metal, machine, and electric construction (111)	64%	49 days	$\longrightarrow$	210 days	+161days
Hotel business (302)	63%	48 days	$\longrightarrow$	210 days	+162 days
Textile (120)	68%	42 days	$\longrightarrow$	210 days	+168 days
Cleaning services (121)	65%	42 days	$\longrightarrow$	210 days	+168 days
Transportation (140)	68%	42 days	$\longrightarrow$	210 days	+168 days
Fishing (143)	51%	42 days	$\longrightarrow$	210 days	+168 days
Agriculture (144)	67%	42 days	$\longrightarrow$	210 days	+168 days
Forestry (146)	65%	42 days	$\longrightarrow$	210 days	+168 days

This table provides a simple comparison of notice periods for blue- and white-collar workers laid off after 10 years of service under the old and the new legislation. Notice periods are calculated using the notice period calculator provided by the ABVV (General Belgian Trade Union). See https://www.abvv.be/bereken-je-opzeg.

Table 2
Comparing pre-period sample means for blue-collar and white-collar firms

	Non-matched sample means				Matched sample means			
	Blue-collar firms N=12,655	White-collar firms N=12,742	Difference	SMD	Blue-collar firms N=12,564	White-collar firms N=12,695	Difference	SMD
Size	15.67	16.037	-0.367***	-0.252	15.679	15.775	-0.096	-0.065
Age	27.615	29.043	-1.428***	-0.082	27.674	26.92	0.754	0.044
Leverage	0.596	0.612	-0.016***	-0.065	0.596	0.604	-0.008	-0.033
EBITDA-to-assets	0.119	0.105	0.014***	0.132	0.119	0.119	0.000	-0.005
Cash holdings	0.09	0.093	-0.003	-0.025	0.09	0.091	-0.001	-0.007
Capital-to-labour	10.112	10.339	-0.227***	-0.163	10.111	10.071	0.040	0.030

This table compares the sample means for blue-collar and white-collar firms over the pre-period (2009-2012). *Blue-collar firms (White-collar firms)* are those whose pre-period (2009-2012) average share of blue-collar workers was above (below) its sample median. The sample only contains firms that have at least one observation in the pre-period (2009-2012) and at least one observation in the post-period (2014-2017). *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively, for t-test of whether the treatment and control groups have equal means for a given variable. In our t-tests, we cluster standard errors at the firm level to account for that some white-collar firms are matched to blue-collar firms more than once. Standardized mean difference (SMD) for a variable is calculated as $(Mean_{blue-collar} - Mean_{white-collar})/\sqrt{(sd^2_{blue-collar} + sd^2_{white-collar})/2}$. Table A1 defines all variables.

Table 3
Summary statistics

	N	Mean	SD	p(25)	p(50)	p(75)
Dependent variables						
Productivity (Main)						
TFP (Ackerberg et al., Value-added, constant el.)	48,852	6.45	0.99	6.225	6.583	6.914
Productivity (Robustness)						
TFP (Wooldridge, Value-added, constant el.)	48,852	7.898	0.679	7.485	7.889	8.292
TFP (Ackerberg et al. (translog), Value-added, constant el.)	48,852	7.13	1.009	6.691	7.249	7.689
TFP (Ackerberg et al., Value-added, pre&post el.)	48,852	6.599	0.695	6.271	6.628	6.943
TFP (OLS, Value-added, yearly-varying el.)	48,852	6.584	0.574	6.265	6.617	6.931
TFP (OLS, Value-added, constant el.)	48,852	6.601	0.543	6.294	6.631	6.924
TFP (OLS, Revenue, constant el.)	41,863	2.393	0.669	1.842	2.337	2.774
Productivity (total emp. as the labour input)						
TFP (Ackerberg et al., Value-added, constant el.)	48,852	5.722	0.618	5.466	5.746	6.039
Employment						
ln(Blue emp.)	48,852	3.273	1.344	2.398	3.258	4.127
ln(White emp.)	48,852	2.557	1.414	1.609	2.565	3.434
ln(Emp.)	48,852	3.822	1.219	2.996	3.761	4.564
ln(Entering emp.)	44,917	2.257	1.403	1.386	2.197	3.045
ln(Exiting emp.)	45,772	2.236	1.385	1.386	2.197	2.996
ln(Outsourced emp.)	29,515	1.555	1.283	0.693	1.386	2.303
ln(Cost per emp.)	48,852	10.811	0.255	10.668	10.819	10.967
ln(Hours per emp.)	48,852	7.341	0.128	7.285	7.351	7.405
Capital						
ln(Tangible fix.)	48,852	13.903	1.767	12.854	14.034	15.05
ln(Mac., Equip.)	46,630	12.171	2.33	10.794	12.308	13.732
ln(Land, Build., Furn., Other)	48,620	13.431	1.872	12.372	13.619	14.684
ln(Intangible fix.)	21,794	10.584	2.48	9.017	10.528	12.187
Train. emp. share	48,852	0.398	0.409	0	0.234	0.856
ln(Train. cost per emp.)	24,003	6.25	1.046	5.499	6.341	7.008
ln(Train. hours per emp.)	24,003	2.882	1.275	2.166	2.933	3.589
Treatment intensity						
(%) Blue-collar	48,852	0.643	0.203	0.526	0.659	0.8
Control variables						
Size (t-1)	48,852	15.774	1.514	14.894	15.771	16.651
Age (t)	48,852	29.331	17.165	18	26	38
Leverage (t-1)	48,852	0.598	0.252	0.415	0.621	0.77
EBITDA-to-assets (t-1)	48,852	0.118	0.114	0.052	0.107	0.177
Cash holdings (t-1)	48,852	0.094	0.119	0.014	0.049	0.125
Capital-to-labour (t-1)	48,852	10.098	1.385	9.32	10.268	10.997

This table presents summary statistics for the variables used in the regressions. The main period of the analysis is from 2009 and 2017. The period for the lagged variables spans from 2008 to 2016. The observations during 2013 are excluded from the analysis. The sample only contains firms that have at least one observation in the pre-period (2009-2012) and at least one observation in the post-period (2014-2017). Table A1 defines all variables.

Table 4
Firing costs and productivity: Main results

	TFP	TFP	TFP	TFP
	(1)	(2)	(3)	(4)
Blue-collar x Post	-0.050*	-0.057***	-0.060***	-0.056***
	(0.028)	(0.017)	(0.014)	(0.013)
Blue-collar	0.088*	0.105***		
	(0.048)	(0.020)		
Post	0.020			
	(0.027)			
Size				0.037***
				(0.014)
Age				-0.001
				(0.003)
Leverage				0.144***
				(0.026)
EBITDA-to-assets				0.678***
				(0.052)
Cash holdings				0.035
				(0.036)
Capital-to-labour				-0.066***
				(0.007)
Observations	48,852	48,852	48,852	48,852
R-squared	0.001	0.780	0.941	0.944
Firm FE	No	No	Yes	Yes
2-digit industry x year FE	No	Yes	Yes	Yes

This table reports difference-in-differences estimates of total factor productivity over the period of 2009 to 2017. The observations during 2013 are excluded from the analysis. The dependent variable, *TFP*, is the residual from the estimation of a Cobb-Douglas production function through the methodology of Ackerberg et al. (2015), defined in natural log. *Blue-collar* is a dummy variable equal to 1 if the firm's pre-period (2009-2012) average share of blue-collar workers was above its sample median, and to 0 otherwise. *Post* is a dummy variable equal to 1 for observations in the period of 2014 to 2017, and to 0 for observations in the period of 2009 to 2012. Firm controls are $Size_{t-1}$, Age_t, $Leverage_{t-1}$, $EBITDA\text{-}to\text{-}assets_{t-1}$, $Cash\ holdings_{t-1}$, and $Capital\text{-}to\text{-}labour_{t-1}$. Table A1 defines all variables. The regressions only contain firms that have at least one observation in the pre-period (2009-2012) and at least one observation in the post-period (2014-2017). Standard errors (in parentheses) are clustered at the firm level. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Table 5
Firing costs and productivity: Dynamic estimates

	TFP	TFP
	(1)	(2)
Blue-collar x 2009 dummy	0.006	0.006
	(0.017)	(0.016)
Blue-collar x 2010 dummy	-0.002	-0.002
	(0.015)	(0.015)
Blue-collar x 2011 dummy	0.000	0.003
	(0.012)	(0.013)
Blue-collar x 2014 dummy	-0.052***	-0.045***
	(0.015)	(0.014)
Blue-collar x 2015 dummy	-0.051***	-0.054***
	(0.018)	(0.018)
Blue-collar x 2016 dummy	-0.068***	-0.068***
	(0.022)	(0.020)
Blue-collar x 2017 dummy	-0.070***	-0.056***
	(0.020)	(0.018)
Observations	48,852	48,852
R-squared	0.941	0.944
Firm controls	No	Yes
Firm FE	Yes	Yes
2-digit industry x year FE	Yes	Yes

This table reports difference-in-differences estimates of total factor productivity over the period of 2009 to 2017. The observations during 2013 are excluded from the analysis. The dependent variable, *TFP*, is the residual from the estimation of a Cobb-Douglas production function through the methodology of Ackerberg et al. (2015), defined in natural log. *Blue-collar* is a dummy variable equal to 1 if the firm's pre-period (2009-2012) average share of blue-collar workers was above its sample median, and to 0 otherwise. Year dummies are equal to 1 for observations in the given year, and to 0 otherwise. E.g., *2009 dummy* is equal to 1 for observations in the year of 2009, and to 0 for other years. Firm controls are $Size_{t-1}$, Age_t, $Leverage_{t-1}$, $EBITDA\text{-}to\text{-}assets_{t-1}$, $Cash\ holdings_{t-1}$, and $Capital\text{-}to\text{-}labour_{t-1}$. Table A1 defines all variables. The regressions only contain firms that have at least one observation in the pre-period (2009-2012) and at least one observation in the post-period (2014-2017). Standard errors (in parentheses) are clustered at the firm level. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Table 6
Firing costs and productivity: Other productivity measures
A. Main estimates

	TFP	TFP	TFP	TFP	TFP	TFP
	Wooldridge	Ackerberg et al. (translog)	Ackerberg et al.	OLS	OLS	OLS
	Value-added	Value-added	Value-added	Value-added	Value-added	Revenue
	constant el.	constant el.	pre&post el.	yearly-varying el.	constant el.	constant el.
	(1)	(2)	(3)	(4)	(5)	(6)
Blue-collar x Post	-0.042***	-0.028*	-0.044***	-0.039***	-0.056***	-0.015***
	(0.011)	(0.015)	(0.014)	(0.012)	(0.012)	(0.004)
Observations	48,852	48,852	48,852	48,852	48,852	41,863
R-squared	0.901	0.933	0.875	0.848	0.833	0.988
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
2-digit industry x year FE	Yes	Yes	Yes	Yes	Yes	Yes

Table 6
(Continued)
B. Dynamic estimates

	TFP	TFP	TFP	TFP	TFP	TFP
	Wooldridge	Ackerberg et al. (translog)	Ackerberg et al.	OLS	OLS	OLS
	Value-added	Value-added	Value-added	Value-added	Value-added	Revenue
	constant el.	constant el.	pre&post el.	yearly-varying el.	constant el.	constant el.
	(1)	(2)	(3)	(4)	(5)	(6)
Blue-collar x 2009 dummy	0.018	-0.038	0.016	0.006	0.012	-0.007
	(0.015)	(0.024)	(0.016)	(0.016)	(0.016)	(0.009)
Blue-collar x 2010 dummy	0.001	-0.029	0.001	0.009	0.002	-0.008
	(0.014)	(0.019)	(0.015)	(0.015)	(0.014)	(0.006)
Blue-collar x 2011 dummy	0.007	-0.004	0.003	0.010	0.004	-0.004
	(0.012)	(0.013)	(0.014)	(0.013)	(0.013)	(0.004)
Blue-collar x 2014 dummy	-0.033***	-0.018	-0.031**	-0.012	-0.040***	-0.016**
	(0.012)	(0.016)	(0.015)	(0.014)	(0.013)	(0.006)
Blue-collar x 2015 dummy	-0.040**	-0.055**	-0.036*	-0.041**	-0.050***	-0.024***
	(0.016)	(0.023)	(0.019)	(0.018)	(0.017)	(0.008)
Blue-collar x 2016 dummy	-0.044**	-0.063***	-0.051**	-0.044**	-0.066***	-0.021***
	(0.018)	(0.023)	(0.021)	(0.019)	(0.019)	(0.006)
Blue-collar x 2017 dummy	-0.028*	-0.046**	-0.043**	-0.042**	-0.057***	-0.019***
	(0.016)	(0.022)	(0.019)	(0.018)	(0.017)	(0.006)
Observations	48,852	48,852	48,852	48,852	48,852	41,863
R-squared	0.902	0.933	0.875	0.849	0.833	0.988
Firm controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
2-digit industry x year FE	Yes	Yes	Yes	Yes	Yes	Yes

This table reports difference-in-differences estimates of total factor productivity over the period of 2009 to 2017. Panels A and B show the main estimates and the dynamic estimates, respectively. The observations during 2013 are excluded from the analysis. The dependent variable, *TFP*, is the residual from the estimation of a production function, defined in natural log. A Cobb-Douglas production function is estimated through the methodology of Wooldridge (2009) (column (1)), through the methodology of Ackerberg et al. (2015) separately for the pre- and post-periods (column (3)), through OLS separately for each year (column (4)), through OLS (column (5)), through OLS and using revenues as the dependent variable (column (6)). In column (2), the dependent variable, *TFP*, is the residual from the estimation of a translog production function, defined in natural log. *Blue-collar* is a dummy variable equal to 1 if the firm's pre-period (2009-2012) average share of blue-collar workers was above its sample median, and to 0 otherwise. *Post* is a dummy variable equal to 1 for observations in the period of 2014 to 2017, and to 0 for observations in the period of 2009 to 2012. Year dummies are equal to 1 for observations in the given year, and to 0 otherwise. E.g., *2009 dummy* is equal to 1 for observations in the year of 2009, and to 0 for other years. Firm controls are $Size_{t-1}$, Age_{t}, $Leverage_{t-1}$, $EBITDA\text{-}to\text{-}assets_{t-1}$, $Cash\ holdings_{t-1}$, and $Capital\text{-}to\text{-}labour_{t-1}$. Table A1 defines all variables. The regressions only contain firms that have at least one observation in the pre-period (2009-2012) and at least one observation in the post-period (2014-2017). Standard errors (in parentheses) are clustered at the firm level. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Table 7
Firing costs and productivity: Using total employment level as the labour input

	Labour: Total employment level	
	TFP	TFP
	(1)	(2)
Blue-collar x Post	-0.015	
	(0.011)	
Blue-collar x 2009 dummy		-0.005
		(0.016)
Blue-collar x 2010 dummy		-0.012
		(0.014)
Blue-collar x 2011 dummy		-0.007
		(0.012)
Blue-collar x 2014 dummy		-0.016
		(0.013)
Blue-collar x 2015 dummy		-0.028*
		(0.016)
Blue-collar x 2016 dummy		-0.026
		(0.018)
Blue-collar x 2017 dummy		-0.012
		(0.016)
Observations	48,852	48,852
R-squared	0.871	0.871
Controls	Yes	Yes
Firm FE	Yes	Yes
2-digit industry x year FE	Yes	Yes

This table reports difference-in-differences estimates of total factor productivity over the period of 2009 to 2017. The observations during 2013 are excluded from the analysis. The dependent variable, *TFP*, is the residual from the estimation of a Cobb-Douglas production function through the methodology of Ackerberg et al. (2015), defined in natural log. The production function is estimated using the total employment level (the natural log of the number of total employees) as the labour input. *Blue-collar* is a dummy variable equal to 1 if the firm's pre-period (2009-2012) average share of blue-collar workers was above its sample median, and to 0 otherwise. *Post* is a dummy variable equal to 1 for observations in the period of 2014 to 2017, and to 0 for observations in the period of 2009 to 2012. Year dummies are equal to 1 for observations in the given year, and to 0 otherwise. E.g., *2009 dummy* is equal to 1 for observations in the year of 2009, and to 0 for other years. Firm controls are $Size_{t-1}$, Age_t, $Leverage_{t-1}$, $EBITDA\text{-}to\text{-}assets_{t-1}$, $Cash\ holdings_{t-1}$, and $Capital\text{-}to\text{-}labour_{t-1}$. Table A1 defines all variables. The regressions only contain firms that have at least one observation in the pre-period (2009-2012) and at least one observation in the post-period (2014-2017). Standard errors (in parentheses) are clustered at the firm level. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Table 8
Bias in estimating the negative effect of increasing firing costs (from 0 to X) on productivity

	X= 30%	X= 20%	X= 10%
Average firm (gamma=0.58)	2.85%	1.83%	0.87%
Blue-collar firms (gamma=0.47)	2.76%	1.78%	0.85%
White-collar firms (gamma=0.82)	1.98%	1.26%	0.59%
Difference in Blue- and White-collar firms	0.78%	0.52%	0.26%

This table quantifies the bias in estimating the effects of changing firing costs on productivity, when estimating the production function by pooling worker types facing heterogeneous changes in firing costs. The numbers therefore represent the value of the Bias in Equation (8) after the change in firing costs, minus its value before the change. X represents expected firing costs relative to yearly wages. *Blue-collar firms (White-collar firms)* are those whose pre-period (2009-2012) average share of blue-collar workers was above (below) its sample median.

Table 9
Firing costs and employment
A. Main estimates

	ln(Blue emp.)	ln(White emp.)	ln(Emp.)	ln(Entering emp.)	ln(Exiting emp.)	ln(Outsourced emp.)	ln(Cost per emp.)	ln(Hours per emp.)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Blue-collar x Post	-0.031	0.092***	-0.018	-0.088**	-0.071*	0.080*	0.001	0.008**
	(0.020)	(0.021)	(0.014)	(0.037)	(0.042)	(0.042)	(0.004)	(0.003)
Observations	48,852	48,852	48,852	44,917	45,772	29,515	48,852	48,852
R-squared	0.964	0.971	0.978	0.820	0.844	0.833	0.901	0.751
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
2-digit industry x year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 9
(Continued)
B. Dynamic estimates

	ln(Blue emp.)	ln(White emp.)	ln(Emp.)	ln(Entering emp.)	ln(Exiting emp.)	ln(Outsourced emp.)	ln(Cost per emp.)	ln(Hours per emp.)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Blue-collar x 2009 dummy	-0.032	0.032**	0.013	-0.033	-0.024	-0.045	0.003	-0.001
	(0.029)	(0.016)	(0.017)	(0.054)	(0.047)	(0.056)	(0.007)	(0.005)
Blue-collar x 2010 dummy	-0.024	0.000	0.004	-0.044	-0.088*	0.070	0.007	-0.005
	(0.020)	(0.013)	(0.011)	(0.045)	(0.048)	(0.048)	(0.006)	(0.004)
Blue-collar x 2011 dummy	-0.008	0.011	0.012*	-0.015	-0.020	0.004	0.006	0.005
	(0.010)	(0.012)	(0.007)	(0.042)	(0.053)	(0.039)	(0.004)	(0.003)
Blue-collar x 2014 dummy	-0.018	0.065***	-0.004	-0.122***	-0.049	0.055	0.006	0.008**
	(0.018)	(0.016)	(0.012)	(0.044)	(0.040)	(0.045)	(0.005)	(0.004)
Blue-collar x 2015 dummy	-0.054**	0.092***	-0.017	-0.119**	-0.102**	0.071	0.004	0.008*
	(0.023)	(0.027)	(0.015)	(0.046)	(0.051)	(0.064)	(0.006)	(0.005)
Blue-collar x 2016 dummy	-0.056**	0.123***	-0.011	-0.128***	-0.164***	0.112**	0.003	0.006
	(0.024)	(0.024)	(0.015)	(0.048)	(0.049)	(0.054)	(0.007)	(0.005)
Blue-collar x 2017 dummy	-0.063**	0.146***	-0.011	-0.059	-0.107**	0.129**	0.004	0.009*
	(0.027)	(0.026)	(0.017)	(0.052)	(0.053)	(0.057)	(0.007)	(0.005)
Observations	48,852	48,852	48,852	44,917	45,772	29,515	48,852	48,852
R-squared	0.964	0.971	0.978	0.821	0.844	0.833	0.901	0.751
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
2-digit industry x year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

This table reports difference-in-differences estimates of employment over the period of 2009 to 2017. Panels A and B show the main estimates and the dynamic estimates, respectively. The observations during 2013 are excluded from the analysis. The dependent variables: (1) *ln(Blue emp.)*, the natural log of the number of blue-collar employees, (2) *ln(White emp.)*, the natural log of the number of white-collar employees, (3) *ln(Emp.)*, the natural log of the total number of employees, (4) *ln(Entering emp.)*, the natural log of the number of new hires, (5) *ln(Exiting emp.)*, the natural log of the number of employees that left the firm, (6) *ln(Outsourced emp.)*, the natural log of the number of outsourced employees through agencies, (7) *ln(Cost per emp.)*, the natural log of the average yearly cost per employee, (8) *ln(Hours per emp.)*, the natural log of the average yearly number of hours worked per employee. *Blue-collar* is a dummy variable equal to 1 if the firm's pre-period (2009-2012) average share of blue-collar workers was above its sample median, and to 0 otherwise. *Post* is a dummy variable equal to 1 for observations in the period of 2014 to 2017, and to 0 for observations in the period of 2009 to 2012. Year dummies are equal to 1 for observations in the given year, and to 0 otherwise. E.g., *2009 dummy* is equal to 1 for observations in the year of 2009, and to 0 for other years. Firm controls are $Size_{t-1}$, Age_t, $Leverage_{t-1}$, $EBITDA\text{-}to\text{-}assets_{t-1}$, $Cash\ holdings_{t-1}$, and $Capital\text{-}to\text{-}labour_{t-1}$. Table A1 defines all variables. The regressions only contain firms that have at least one observation in the pre-period (2009-2012) and at least one observation in the post-period (2014-2017). Standard errors (in parentheses) are clustered at the firm level. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Table 10
Firing costs and capital

	Tangible capital						Intangible capital	
	ln(Tangible fix.)	ln(Tangible fix.)	ln(Mac., Equip.)	ln(Mac., Equip.)	ln(Land, Build., Furn., Other)	ln(Land, Build., Furn., Other)	ln(Intangible fix.)	ln(Intangible fix.)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Blue-collar x Post	0.042*		-0.098		0.074**		-0.020	
	(0.022)		(0.071)		(0.034)		(0.114)	
Blue-collar x 2009 dummy		-0.006		-0.058		0.008		0.142
		(0.036)		(0.059)		(0.050)		(0.128)
Blue-collar x 2010 dummy		-0.033		-0.036		-0.034		0.144
		(0.027)		(0.058)		(0.035)		(0.122)
Blue-collar x 2011 dummy		0.014		0.050		0.003		0.110
		(0.023)		(0.047)		(0.027)		(0.074)
Blue-collar x 2014 dummy		0.014		-0.075		0.022		0.081
		(0.032)		(0.064)		(0.042)		(0.095)
Blue-collar x 2015 dummy		-0.021		-0.132		-0.000		0.207
		(0.029)		(0.112)		(0.039)		(0.138)
Blue-collar x 2016 dummy		0.077**		-0.086		0.112***		0.021
		(0.031)		(0.104)		(0.042)		(0.148)
Blue-collar x 2017 dummy		0.105***		-0.138		0.182***		-0.088
		(0.040)		(0.108)		(0.052)		(0.154)
Observations	48,852	48,852	46,630	46,630	48,620	48,620	21,794	21,794
R-squared	0.955	0.955	0.913	0.913	0.924	0.924	0.790	0.790
Firm controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
2-digit industry x year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

This table reports difference-in-differences estimates of capital over the period of 2009 to 2017. The observations during 2013 are excluded from the analysis. In columns (1) and (2), the dependent variable, *ln(Tangible fix.)*, is the natural log of tangible fixed assets. In columns (3) and (4), the dependent variable, *ln(Mac., Equip.)*, is the natural log of the sum of machinery and equipment. In columns (5) and (6), the dependent variable, *ln(Land, Build., Furn., Other)*, is the natural log of the sum of land, building, furniture, and other tangible fixed capital. In columns (7) and (8), the dependent variable, *ln(Intangible fix.)*, is the natural log of intangible fixed assets. *Blue-collar* is a dummy variable equal to 1 if the firm's pre-period (2009-2012) average share of blue-collar workers was above its sample median, and to 0 otherwise. *Post* is a dummy variable equal to 1 for observations in the period of 2014 to 2017, and to 0 for observations in the period of 2009 to 2012. Year dummies are equal to 1 for observations in the given year, and to 0 otherwise. E.g., *2009 dummy* is equal to 1 for observations in the year of 2009, and to 0 for other years. Firm controls are $Size_{t-1}$, Age_t, $Leverage_{t-1}$, $EBITDA\text{-}to\text{-}assets_{t-1}$, $Cash\ holdings_{t-1}$, and $Capital\text{-}to\text{-}labour_{t-1}$. Table A1 defines all variables. The regressions only contain firms that have at least one observation in the pre-period (2009-2012) and at least one observation in the post-period (2014-2017). Standard errors (in parentheses) are clustered at the firm level. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Table 11
Firing costs and training activities

	Train. emp. share	ln(Train. cost per emp.)	ln(Train. hours per emp.)
	(1)	(2)	(3)
Blue-collar x Post	0.023	0.070	0.009
	(0.016)	(0.065)	(0.053)
Observations	48,852	24,003	24,003
R-squared	0.685	0.701	0.648
Controls	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
2-digit industry x year FE	Yes	Yes	Yes

This table reports difference-in-differences estimates of training activities over the period of 2009 to 2017. The observations during 2013 are excluded from the analysis. The dependent variables: (1) *Train. emp. share*, the share of employees that joined training activities, (2) *ln(Train. cost per emp.)*, the natural log of the average yearly training cost per employee, and (3) *ln(Train. hours per emp.)*, the natural log of the average yearly training hours per employee. *Blue-collar* is a dummy variable equal to 1 if the firm's pre-period (2009-2012) average share of blue-collar workers was above its sample median, and to 0 otherwise. *Post* is a dummy variable equal to 1 for observations in the period of 2014 to 2017, and to 0 for observations in the period of 2009 to 2012. Year dummies are equal to 1 for observations in the given year, and to 0 otherwise. E.g., *2009 dummy* is equal to 1 for observations in the year of 2009, and to 0 for other years. Firm controls are $Size_{t-1}$, Age_t, $Leverage_{t-1}$, $EBITDA\text{-}to\text{-}assets_{t-1}$, $Cash\ holdings_{t-1}$, and $Capital\text{-}to\text{-}labour_{t-1}$. Table A1 defines all variables. The regressions only contain firms that have at least one observation in the pre-period (2009-2012) and at least one observation in the post-period (2014-2017). Standard errors (in parentheses) are clustered at the firm level. *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively.

Table 12
Firing costs and productivity: The role of credit access

	Higher debt capacity			Lower debt capacity		
	Full sample	Low credit shock	High credit shock	Full sample	Low credit shock	High credit shock
	TFP	TFP	TFP	TFP	TFP	TFP
	(1)	(2)	(3)	(4)	(5)	(6)
Blue-collar x Post	-0.032*	-0.016	-0.034	-0.067***	-0.072***	-0.047*
	(0.017)	(0.023)	(0.023)	(0.017)	(0.021)	(0.027)
Observations	15,463	7,172	8,256	14,749	7,960	6,763
R-squared	0.935	0.937	0.938	0.943	0.941	0.951
Firm controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
2-digit industry x year FE	Yes	Yes	Yes	Yes	Yes	Yes

This table reports difference-in-differences estimates of total factor productivity across credit constraints over the period of 2009 to 2017. The sample only includes firms that borrowed during the pre-period. The observations during 2013 are excluded from the analysis. A firm has *higher debt capacity (lower debt capacity)* if its long-term financial debt-to-fixed assets ratio in 2012 end year was below (above) its sample median. *Low credit shock (High credit shock)* sample includes firms whose estimated credit supply shock over the pre-period was below (above) its sample median. The dependent variable, *TFP*, is the residual from the estimation of a Cobb-Douglas production function through the methodology of Ackerberg et al. (2015), defined in natural log. *Blue-collar* is a dummy variable equal to 1 if the firm's pre-period (2009-2012) average share of blue-collar workers was above its sample median, and to 0 otherwise. *Post* is a dummy variable equal to 1 for observations in the period of 2014 to 2017, and to 0 for observations in the period of 2009 to 2012. Firm controls are $Size_{t-1}$, Age_t, $Leverage_{t-1}$, $EBITDA\text{-}to\text{-}assets_{t-1}$, $Cash\ holdings_{t-1}$, and $Capital\text{-}to\text{-}labour_{t-1}$. Table A1 defines all variables. The regressions only contain firms that have at least one observation in the pre-period (2009-2012) and at least one observation in the post-period (2014-2017). Standard errors (in parentheses) are clustered at the firm level. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

- ONLINE APPENDIX -

Appendix A. Variable definitions

Table A1
Variable definitions

Variable	Definition
Firm level	
(%) Blue-collar	Pre-period (2009-2012) average share of blue-collar employees
Blue-collar	Dummy variable equal to 1 if the firm's pre-period (2009-2012) average share of blue-collar employees was above its sample median, and to 0 otherwise
Post	Dummy variable equal to 1 for observations in the period of 2014 to 2017, and to 0 for observations in the period of 2009 to 2012
TFP	Residual from the estimation of a Cobb-Douglas production function, defined in natural log
ln(Value-added)	Natural log of value added
ln(Blue emp.)	Natural log of the number of blue-collar employees
ln(White emp.)	Natural log of the number of white-collar employees
ln(Emp.)	Natural log of the total number of employees
ln(Entering emp.)	Natural log of the number of new hires
ln(Exiting emp.)	Natural log of the number of employees that left the firm
ln(Permanent emp.)	Natural log of the number of employees with permanent contracts
ln(Temporary emp.)	Natural log of the number of employees with fixed-term contracts
ln(Permanent entering emp.)	Natural log of the number of new hires with permanent contracts
ln(Temporary entering emp.)	Natural log of the number of new hires with fixed-term contracts
ln(Permanent exiting emp.)	Natural log of the number of employees with permanent contracts that left the firm
ln(Temporary exiting emp.)	Natural log of the number of employees with fixed-term contracts that left the firm
ln(Outsourced emp.)	Natural log of the number of outsourced employees through agencies
ln(Cost per emp.)	Natural log of the average yearly cost per employee
ln(Hours per emp.)	Natural log of the average yearly number of hours worked per employee
ln(Tangible fix.)	Natural log of tangible fixed assets
ln(Mac., Equip.)	Natural log of the sum of machinery and equipment
ln(Land, Build., Furn., Other)	Natural log of the sum of land, building, furniture, and other tangible fixed capital
ln(Intangible fix.)	Natural log of intangible fixed assets
Train. emp. share	Share of employees that joined training activities
ln(Train. cost per emp.)	Natural log of the average yearly training cost per employee
ln(Train. hours per emp.)	Natural log of the average yearly training hours per employee
Size	Natural log of total assets
Age	Number of years since the firm's incorporation date
Leverage	Sum of long- and short-term debt, divided by total assets
Cash holdings	Ratio of cash holdings to total assets
Capital-to-labour	Natural log of the ratio of tangible fixed assets to the total number of employees
industry level	
ln(1+#exit)	Natural log of one plus the number of total firm exits (defined through the firm's legal status) in the industry
Country level	
GDP growth	Annual growth rate of gross domestic product (in %)
GDP per capita	Gross domestic product, divided by midyear population (in thous. $)

Appendix B. Belgian employment pre-regulation

Table B1
Working conditions of blue- and white-collar workers before the regulation

	Blue-collar workers	White-collar workers
Notice Periods	-Fixed within each industry, and calculated based only on seniority.	-Irrespective of the worker's industry, calculated based mainly on seniority and wage.
Trial Period	-Maximum 2 weeks. The trial period can end prematurely for serious reasons between the 1st and the 7th days. After that, the trial period can finish without notice or compensation.	-Maximum 12 months. The trial period can finish only if the worker has used 7 days of sickness leave. If the trial period is terminated in the first month onwards, a notice period of 7 days is required.
Sickness Leave	-The first day of the sickness leave is not paid if the duration of sickness leave is less than 14 days. - The workers get paid at the regular wage rate for 7 days.	-The first day of the sickness leave is not paid if the duration of sickness leave is less than 14 days, only for those who are with temporary contracts or well-defined job contracts for less than three months, as well as for those who are on trial periods. - The workers get paid at the regular wage rate for 30 days.
Unfair dismissal	-The employer has to justify the reason for the dismissal under the work contract law (art. 63). In case of proof of unfair dismissal, the worker will be compensated with a fixed sum.	-The worker has to prove, under the common law, that the dismissal was unfair. In case of proof of unfair dismissal, the worker will be compensated in accordance with the level of the breach.
Temporary Unemployment	-The worker can be temporarily laid off.	-The worker can be temporarily laid off.
Wages	- Often paid based on an hourly rate twice a month.	- Often paid based on an hourly or flat rate once a month.
Holiday Bonus	-Paid by a government institution, namely the National Office of Annual Vacation.	-Paid by the employer. There is a chance of a double holiday bonus.
Contributions	-Employers' contribution is fixed within industries, usually at 38.38% of 108% of the worker's gross wage (the extra 8% adds up for holiday bonus). Social security contribution is 13.07% of 108% of the worker's gross wage.	-Employers' contribution is not fixed within industries, usually at 32.38% of the worker's gross wage. Social security contribution is 13.07% of the worker's gross wage.

This table demonstrates the main similarities and differences in the working conditions of blue- and white-collar workers prior to 1 January 2014, the effective date of the new Belgian labour regulation (the Act of 26 December 2013). Source: Ajzen and Vermandere (2014).

Appendix C. Act of 26 December 2013: Other changes than the new notice periods

Other elements of the new legislation suggest that the Act has primarily increased the labour adjustment costs for blue-collar workers. First, it is only possible for employers to specify a different notice period than the one stated under the new law if the specified period benefits the employee. While it is unlikely that employers increase the notice periods for blue-collar workers beyond the already significant increase imposed by the law, one can imagine that white-collar workers -especially those in higher pay grades- might be able to mitigate the effect of the new legislation by negotiating longer notice periods. Second, the Act of 26 December 2013 has also brought about changes that benefited both blue- *and* white-collar workers.

Importantly, the Act has also improved the protection against unfair dismissals. Previously, for blue-collar workers, the arbitrary "dismissal regime" applied; and for white-collar workers, there was no legal regulation but the "concept of abuse of rights" was used in practice. The new law, however, has created uniform protection against unfair dismissals. Particularly, the new law has allowed a dismissed employee to know the exact reasons of the dismissal and to obtain compensation from the employer in the case of a manifestly unfair dismissal. In addition, the new law has improved outplacement rights for all worker types. Specifically, the new law currently requires employers to provide outplacement support to all dismissed employees with at least 30 weeks of notice periods. Lastly, the new legislation has abolished trial periods and the rule of no pay for the first day of sick leave (the so-called "carenz" day). All these elements have increased labour costs for both worker types: for white-collar workers, employers need to weigh these costs against (somewhat) reduced notice periods, while they add to the cost of (significantly) increased notice periods for blue-collar workers.

Appendix D. Estimation of credit supply shocks

Data. To estimate credit supply shocks that firms experience from their lenders, we obtain monthly bank-firm level loan data from the Central Corporate Credit Register in Belgium. The availability of such granular data is crucial to disentangle firms' credit demand from banks' credit supply. All financial institutions established in Belgium and licensed by the National Bank of Belgium have to report to the credit register on all debtors whose total borrowing exceeds 25,000 euro. We focus only on banks and retain only those that have at least 30 firms in their lending portfolio. We also take into account bank mergers and acquisitions when measuring loan growth rates. In total, we utilize granted loans from 57 banks, and match them with borrowers' balance sheets.

Estimation of credit supply shocks. We construct firm-level credit shocks using the approach of Degryse et al. (2019), which is in the spirit of the methodology of Amiti and Weinstein (2018). We first run the following regression on a sample of borrowing firms at the bank-firm-month level separately for each pre-period year:

$$\Delta Lending_{b,f,t} = \alpha_{l,s,s,t} + \beta_{b,t} + error\ term \tag{10}$$

where $\Delta Lending_{bft}$ is the percentage growth rate of credit at the intensive margin from $t-1$ to t (i.e., annual growth rate at month t), α_{lsst} are location-industry-size-time (LSST) dummies that capture credit demand under the assumption that firms with similar size in the same industry and location in a given time have similar credit demand, and β_{bt} are bank-time dummies that capture time-varying credit supply shocks.[30] We estimate Equation 10 by omitting one firm dummy, and demean the estimated bank credit supply dummies (i.e., $\tilde{\beta}_{bt} = \hat{\beta}_{bt} - \bar{\beta}_t$) to leave out the omitted firm dummy effect. Then, we create yearly firm-level credit supply measures by averaging the demeaned estimated monthly bank credit supply dummies, weighted by each lender's share in the firm's borrowing portfolio.

[30] Since Gan (2007) and Khwaja and Mian (2008), contemporary banking research has mainly relied on firm-time fixed effects to control for credit demand in samples where firms borrow from multiple banks. However, the Belgian economy is comprised vastly of single-bank firms to which the firm-time fixed effects approach is not applicable. Degryse et al. (2019) therefore suggest using LSST fixed effects instead of firm-time fixed effects to control for credit demand. They compare estimations with LSST fixed effects to those with firm-time fixed effects and show that LSST fixed effects are indeed capable of controlling for credit demand. See Güler et al. (2021) for a review of the corresponding literature.

Appendix E. Additional tables

E.1. Comparing pre-period sample means for blue- and white-collar firms

Table E1
Comparing pre-period sample means for blue-collar and white-collar firms

	Non-matched sample						Matched sample					
	Blue-collar firms		White-collar firms				Blue-collar firms		White-collar firms			
	N	Mean	N	Mean	Difference	SMD	N	Mean	N	Mean	Difference	SMD
TFP (Ackerberg et al., Value-added, constant el.)	12,655	6.497	12,742	6.556	-0.059***	-0.068	12,564	6.496	12,695	6.408	0.088*	0.092
TFP (Wooldridge, Value-added, constant el.)	12,655	7.884	12,742	8.026	-0.142***	-0.204	12,564	7.887	12,695	7.883	0.004	0.005
TFP (Ackerberg et al. (translog), Value-added, constant el.)	12,655	7.16	12,742	7.271	-0.111***	0.120	12,564	7.159	12,695	7.096	0.063	-0.064
TFP (Ackerberg et al., Value-added, pre&post el.)	12,655	6.608	12,742	6.618	-0.01	-0.013	12,564	6.608	12,695	6.518	0.09***	0.117
TFP (OLS, Value-added, yearly-varying el.)	12,655	6.628	12,742	6.644	-0.016	-0.027	12,564	6.628	12,695	6.532	0.096***	0.168
TFP (OLS, Value-added, constant el.)	12,655	6.65	12,742	6.648	0.002	0.003	12,564	6.65	12,695	6.538	0.112***	0.207
TFP (OLS, Revenue, constant el.)	10,545	2.399	11,235	2.048	0.351***	0.536	10,504	2.399	10,905	2.403	-0.004	-0.006
TFP (Ackerberg et al., Value-added, constant el., total emp. as the labour input)	12,655	5.646	12,742	5.958	-0.312***	0.485	12,564	5.647	12,695	5.768	-0.121***	0.199
ln(Blue emp.)	12,655	3.567	12,742	2.505	1.062***	0.831	12,564	3.574	12,695	2.964	0.61***	0.467
ln(White emp.)	12,655	1.982	12,742	2.947	-0.965***	-0.767	12,564	1.993	12,695	3.053	-1.06***	-0.821
ln(Emp.)	12,655	3.792	12,742	3.567	0.225***	0.192	12,564	3.8	12,695	3.812	-0.012	-0.010
ln(Entering emp.)	11,653	2.263	11,743	2.108	0.155***	0.110	11,567	2.267	11,926	2.224	0.043	0.031
ln(Exiting emp.)	11,897	2.264	11,826	2.081	0.183***	0.132	11,818	2.268	11,963	2.231	0.037	0.027
ln(Outsourced emp.)	6,684	1.545	7,200	1.276	0.269***	-0.218	6,660	1.548	8,016	1.49	0.058	-0.046
ln(Cost per emp.)	12,655	10.697	12,742	10.857	-0.16***	-0.638	12,564	10.697	12,695	10.839	-0.142***	-0.592
ln(Hours per emp.)	12,655	7.314	12,742	7.377	-0.063***	-0.523	12,564	7.314	12,695	7.367	-0.053***	-0.421
ln(Tangible fix.)	12,655	13.905	12,742	13.905	0	0.000	12,564	13.912	12,695	13.883	0.029	0.017
ln(Mac., Equip.)	12,141	12.207	12,213	11.9	0.307***	0.137	12,061	12.214	12,200	12.027	0.187	0.080
ln(Land, Build., Furn., Other)	12,591	13.413	12,691	13.503	-0.089**	-0.049	12,501	13.419	12,676	13.433	-0.014	-0.008
ln(Intangible fix.)	5,074	10.176	6,541	10.929	-0.753***	-0.307	5,051	10.178	5,943	10.736	-0.558***	-0.233
Train. emp. share	12,655	0.307	12,742	0.329	-0.022***	0.060	12,564	0.308	12,695	0.389	-0.081***	0.207
ln(Train. cost per emp.)	5,430	6.119	5,387	6.44	-0.321***	0.321	5,415	6.118	6,309	6.335	-0.217***	0.210
ln(Train. hours per emp.)	5,430	2.724	5,387	2.94	-0.216***	0.181	5,415	2.724	6,309	2.952	-0.228***	0.195
(%) Blue-collar	12,655	0.803	12,742	0.415	0.388***	2.810	12,564	0.803	12,695	0.483	0.32***	2.518
Size	12,655	15.67	12,742	16.037	-0.367***	-0.252	12,564	15.679	12,695	15.775	-0.096	-0.065
Age	12,655	27.615	12,742	29.043	-1.428***	-0.082	12,564	27.674	12,695	26.92	0.754	0.044
Leverage	12,655	0.596	12,742	0.612	-0.016***	-0.065	12,564	0.596	12,695	0.604	-0.008	-0.033
EBITDA-to-assets	12,655	0.119	12,742	0.105	0.014***	0.132	12,564	0.119	12,695	0.119	0	-0.005
Cash holdings	12,655	0.09	12,742	0.093	-0.003	-0.025	12,564	0.09	12,695	0.091	-0.001	-0.007
Capital-to-labour	12,655	10.112	12,742	10.339	-0.227***	-0.163	12,564	10.111	12,695	10.071	0.040	0.030

This table compares the sample means for blue-collar and white-collar firms over the pre- (2009-2012) period. The sample only contains firms that have at least one observation in the pre-period (2009-2012) and at least one observation in the post-period (2014-2017). *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively, for t-test of whether the treatment and control groups have equal means for a given variable. In our t-tests, we cluster standard errors at the firm level to account for that some white-collar firms are matched to blue-collar firms more than once. Standardized mean difference (SMD) for a variable is calculated as $(Mean_{blue-collar} - Mean_{white-collar})/\sqrt{(sd^2_{blue-collar} + sd^2_{white-collar})/2}$. Table A1 defines all variables.

E.2. Results on different matched samples and on the non-matched sample

Table E2
Firing costs and productivity: Different matched samples and the non-matched sample
A. Main Estimates

	Two-matches	Two-matches	Three-matches	Three-matches	Non-matched sample	Non-matched sample
	TFP	TFP	TFP	TFP	TFP	TFP
	(1)	(2)	(3)	(4)	(5)	(6)
Blue-collar x Post	-0.050***	-0.046***	-0.060***	-0.055***	-0.043***	-0.041***
	(0.014)	(0.012)	(0.016)	(0.015)	(0.009)	(0.009)
Observations	73,190	73,190	97,415	97,415	49,447	49,447
R-squared	0.939	0.942	0.936	0.938	0.914	0.919
Controls	No	Yes	No	Yes	No	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
2-digit industry x year FE	Yes	Yes	Yes	Yes	Yes	Yes

Table E2
(Continued)
B. Dynamic estimates

	Two-matches	Two-matches	Three-matches	Three-matches	Non-matched sample	Non-matched sample
	TFP	TFP	TFP	TFP	TFP	TFP
	(1)	(2)	(3)	(4)	(5)	(6)
Blue-collar x 2009 dummy	0.019	0.018	0.014	0.013	0.007	0.005
	(0.016)	(0.016)	(0.015)	(0.015)	(0.013)	(0.013)
Blue-collar x 2010 dummy	0.006	0.004	0.005	0.004	-0.012	-0.015
	(0.014)	(0.014)	(0.014)	(0.013)	(0.011)	(0.011)
Blue-collar x 2011 dummy	-0.001	0.001	0.001	0.003	-0.012	-0.011
	(0.011)	(0.012)	(0.012)	(0.014)	(0.009)	(0.009)
Blue-collar x 2014 dummy	-0.035**	-0.027*	-0.047***	-0.036**	-0.038***	-0.036***
	(0.014)	(0.014)	(0.016)	(0.017)	(0.010)	(0.010)
Blue-collar x 2015 dummy	-0.038**	-0.038**	-0.047**	-0.044**	-0.037***	-0.039***
	(0.018)	(0.018)	(0.020)	(0.020)	(0.012)	(0.012)
Blue-collar x 2016 dummy	-0.054***	-0.054***	-0.067***	-0.066***	-0.057***	-0.058***
	(0.019)	(0.018)	(0.023)	(0.023)	(0.013)	(0.013)
Blue-collar x 2017 dummy	-0.057***	-0.049***	-0.071***	-0.062***	-0.066***	-0.059***
	(0.018)	(0.017)	(0.018)	(0.017)	(0.014)	(0.013)
Observations	73,190	73,190	97,415	97,415	49,447	49,447
R-squared	0.939	0.942	0.936	0.938	0.914	0.919
Controls	No	Yes	No	Yes	No	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
2-digit industry x year FE	Yes	Yes	Yes	Yes	Yes	Yes

This table reports difference-in-differences estimates of total factor productivity over the period of 2009 to 2017. Panels A and B show the main estimates and the dynamic estimates, respectively. The observations during 2013 are excluded from the analysis. In columns (1) and (2), two white-collar firms are matched to each blue-collar firm. In columns (3) and (4), three white-collar firms are matched to each blue-collar firm. In columns (5) and (6), the non-matched sample is used. The dependent variable, *TFP*, is the residual from the estimation of a Cobb-Douglas production function through the methodology of Ackerberg et al. (2015), defined in natural log. *Blue-collar* is a dummy variable equal to 1 if the firm's pre-period (2009-2012) average share of blue-collar workers was above its sample median, and to 0 otherwise. *Post* is a dummy variable equal to 1 for observations in the period of 2014 to 2017, and to 0 for observations in the period of 2009 to 2012. Year dummies are equal to 1 for observations in the given year, and to 0 otherwise. E.g., *2009 dummy* is equal to 1 for observations in the year of 2009, and to 0 for other years. Firm controls are $Size_{t-1}$, Age_t, $Leverage_{t-1}$, $EBITDA\text{-}to\text{-}assets_{t-1}$, $Cash\ holdings_{t-1}$, and $Capital\text{-}to\text{-}labour_{t-1}$. Table A1 defines all variables. The regressions only contain firms that have at least one observation in the pre-period (2009-2012) and at least one observation in the post-period (2014-2017). Standard errors (in parentheses) are clustered at the firm level. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

E.3. Additional tests

Column (1) of Table E3 clusters standard errors at the NACE 2-digit industry level, instead of at the firm level, to allow for the correlation of shocks within an industry. The estimated effect remains significant at the 1% confidence level.

Column (2) further restricts our main model by including NACE 2-digit industry × province × year fixed effects that net out any variation that is common to all firms in the same industry and location during the same year.[31] Including these additional fixed effects neither alters the sign nor the significance of the main result.

Column (3) includes the interactions of the time-invariant pre-period control dummies and the post dummy, similar to Costello (2020).[32] This addresses the possible concern that firm-specific factors might be correlated with the firm's workforce composition (i.e., selection into blue- and white-collar firm groups). The inclusion of these controls yields an estimate that is very similar to the one in Table 4.

Column (4) defines the blue-collar dummy based on the firm's share of blue-collar workers at the end of 2012, instead of the average pre-period (2009-2012) share. The estimated effect is identical to that in Table 4.

Bertrand et al. (2004) show that standard errors from panel data models might be subject to autocorrelation. As a solution, they suggest collapsing time series data into two periods (a pre- and a post-period) around the event. Accordingly, we conduct the estimation on the collapsed data where we first-difference the dependent variable (and thus take the difference between the post- and pre-period). As such, the blue-collar dummy is the main difference-in-differences indicator here. NACE 2-digit industry fixed effects are included and standard errors are clustered at the NACE 2-digit industry level. Columns (5) and (6) report the results on non-matched and matched samples, respectively. The coefficients of interest in both columns are negative and significant at the 5% confidence level, and their magnitudes are comparable to that from the panel data estimation.

Column (7) reports the results from a placebo test where we pretend that the legal change occurred in 2009. Here, we take 2005-2008 as the pre-period and 2009-2012 as the

[31]There are 10 provinces in Belgium, namely Antwerp, East Flanders, Flemish Brabant, Limburg, West Flanders, Hainaut, Liège, Luxembourg, Namur, Walloon Brabant. On top of that, we separately treat the Brussels Capital Region as a province and take it out of Flemish Brabant.

[32]For example, for *size*, we define a dummy that is equal to 1 if the firm's pre-period average assets were above its sample median, and to 0 otherwise. We similarly create dummies also for *age, leverage, EBITDA, cash holdings, and capital-to-labour*. We then interact these dummies with the post dummy and include them in Equation (9) as explanatory variables, instead of our baseline controls.

post-period, and define $Blue - collar_i$ as a dummy that is equal to 1 if the firm's share of blue-collar workers over 2005-2008 was above the sample median, and to 0 otherwise. The placebo test does not yield a significant estimate.

Table E3
Firing costs and productivity: Additional robustness tests

	Clustering standard errors at the industry-level	Additional FE	pre- control dummies x post	Treatment based on 2012	Collapsed post - collapsed pre (Non-matched)	Collapsed post - collapsed pre (Matched)	Placebo
	TFP	TFP	TFP	TFP	TFP	TFP	TFP
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Blue-collar x Post	-0.056***	-0.043***	-0.060***	-0.056***			-0.013
	(0.016)	(0.010)	(0.014)	(0.013)			(0.013)
Blue-collar					-0.044**	-0.063**	
					(0.020)	(0.025)	
Observations	48,852	48,552	48,852	48,852	7,225	7,088	57,587
R-squared	0.944	0.953	0.942	0.944	0.059	0.073	0.826
Firm controls	Yes	Yes	No	Yes	Yes	Yes	Yes
Pre-period control dummies x post	No	No	Yes	No	No	No	No
Firm FE	Yes	Yes	Yes	Yes	No	No	Yes
2 digit industry FE	No	No	No	No	Yes	Yes	No
2-digit industry x year FE	Yes	No	Yes	Yes	No	No	Yes
2-digit industry x province x year FE	No	Yes	No	No	No	No	No

This table reports difference-in-differences estimates of total factor productivity. In columns (1)-(6), the period of analysis is from 2009 to 2017. The observations during 2013 are excluded from the analysis. In column (7), the period of analysis spans from 2005 to 2012. The dependent variable, *TFP*, is the residual from the estimation of a Cobb-Douglas production function through the methodology of Ackerberg et al. (2015), defined in natural log. In columns (5) and (6), the data is collapsed around the event (as opposed to using panel data), and the dependent variable is first differenced. In columns (1), (2), (3), (5), and (6), *Blue-collar* is a dummy variable equal to 1 if the firm's average share of blue-collar workers over 2009-2012 was above its sample median, and to 0 otherwise. In column (4), *Blue-collar* is a dummy variable equal to 1 if the firm's share of blue-collar workers in 2012 end year was above its sample median, to 0 otherwise. In column (7), *Blue-collar* is a dummy variable equal to 1 if the firm's average share of blue-collar workers over 2005-2008 was above its sample median, and to 0 otherwise. In columns (1)-(4), *Post* is a dummy variable equal to 1 for observations in the period of 2014 to 2017, and to 0 for observations in the period of 2009 to 2012. In column (7), *Post* is a dummy variable equal to 1 for observations in the period of 2009 to 2012, and to 0 for observations in the period of 2005 to 2008. In columns (1), (2), (4), and (7), firm controls are $Size_{t-1}$, Age_t, $Leverage_{t-1}$, $EBITDA\text{-}to\text{-}assets_{t-1}$, $Cash\ holdings_{t-1}$, and $Capital\text{-}to\text{-}labour_{t-1}$. In columns (5)-(6), we use the average values of firm controls over 2009-2012. Instead of firm controls, column (3) includes the interactions of the *Post* dummy and time-invariant pre-period control dummies (e.g., for *Size*, we define a dummy that is equal to 1 if the firm's average assets over the pre-period were above its sample median, and to 0 otherwise). Table A1 defines all variables. The regressions only contain firms that have at least one observation in the pre-period and at least one observation in the post-period. Standard errors (in parentheses) are clustered at the firm level. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

E.4. Adding firms that operate with only blue- or only white-collar workers and firms that do not report intermediate inputs

In our main analysis, we only include firms that have at least one blue-collar worker, at least one white-collar worker, and available information on intermediate inputs. Table E4 shows that our results are not sensitive to this selection. In columns (1) and (2), we recover TFP through estimating the production function using ln(1+blue-collar workers) and ln(1+white-collar workers) as labour inputs. By doing so, we also incorporate firms that have only blue- or only white-collar workers. In columns (3) and (4), we construct TFP by estimating our baseline production function through OLS, which allows us to include small firms that do not report intermediate inputs in Bel-first. Finally, to include firms with only blue- or only white-collar workers and firms with missing intermediate inputs simultaneously, columns (5) and (6) use TFP from an OLS estimation of the production function with ln(1+blue-collar workers) and ln(1+white-collar workers). In each case, the average effect is negative and significant at the 1% level, and the parallel trends assumption appears to hold.

Table E4
Firing costs and productivity: Incorporating firms with only one worker type as well as firms with missing intermediate inputs

	Incorporating firms with only one worker type		Incorporating firms with missing intermediate inputs		Incorporating firms with only one worker type and/or with missing intermediate inputs	
	TFP	TFP	TFP	TFP	TFP	TFP
	(1)	(2)	(3)	(4)	(5)	(6)
Blue-collar x Post	-0.051***		-0.033***		-0.036***	
	(0.016)		(0.009)		(0.010)	
Blue-collar x 2009 dummy		-0.002		0.002		-0.014
		(0.018)		(0.010)		(0.018)
Blue-collar x 2010 dummy		-0.027		0.006		-0.014
		(0.017)		(0.009)		(0.018)
Blue-collar x 2011 dummy		-0.005		0.006		0.007
		(0.018)		(0.007)		(0.008)
Blue-collar x 2014 dummy		-0.051***		-0.025**		-0.033***
		(0.016)		(0.010)		(0.012)
Blue-collar x 2015 dummy		-0.073***		-0.022**		-0.025**
		(0.020)		(0.010)		(0.011)
Blue-collar x 2016 dummy		-0.036		-0.031**		-0.043***
		(0.025)		(0.013)		(0.014)
Blue-collar x 2017 dummy		-0.079***		-0.042***		-0.065***
		(0.020)		(0.011)		(0.013)
Observations	67,788	67,788	146,228	146,228	167,923	167,923
R-squared	0.852	0.852	0.789	0.789	0.776	0.776
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
2-digit industry x year FE	Yes	Yes	Yes	Yes	Yes	Yes

This table reports difference-in-differences estimates of total factor productivity over the period of 2009 to 2017. The observations during 2013 are excluded from the analysis. The dependent variable, *TFP*, is the residual from the estimation of a Cobb-Douglas production function, defined in natural log. In columns (1) and (2), the production function is estimated through the methodology of Ackerberg et al. (2015) and using ln(1+blue-collar workers) and ln(1+white-collar workers) as labour inputs. In columns (3) and (4), the production function is estimated through OLS with the baseline production inputs. In columns (5) and (6), the production function is estimated through OLS and using ln(1+blue-collar workers) and ln(1+white-collar workers) as labour inputs. *Blue-collar* is a dummy variable equal to 1 if the firm's pre-period (2009-2012) average share of blue-collar workers was above its sample median, and to 0 otherwise. *Post* is a dummy variable equal to 1 for observations in the period of 2014 to 2017, and to 0 for observations in the period of 2009 to 2012. Year dummies are equal to 1 for observations in the given year, and to 0 otherwise. E.g., *2009 dummy* is equal to 1 for observations in the year of 2009, and to 0 for other years. Firm controls are $Size_{t-1}$, Age_t, $Leverage_{t-1}$, $EBITDA\text{-}to\text{-}assets_{t-1}$, $Cash\ holdings_{t-1}$, and $Capital\text{-}to\text{-}labour_{t-1}$. Table A1 defines all variables. The regressions only contain firms that have at least one observation in the pre-period (2009-2012) and at least one observation in the post-period (2014-2017). Standard errors (in parentheses) are clustered at the firm level. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

E.5. NACE 2-digit industry-level elasticities

Table E5
industry-level elasticities

NACE 2-digit industry	N	Blue-collar emp. el.		White-collar emp. el.		Capital el.	
1	330	0.32	(0.11)	0.39	(0.14)	0.28	(0.11)
8	305	0.44	(0.29)	0.43	(0.28)	0.18	(0.12)
10	3824	0.34	(0.00)	0.44	(0.00)	0.23	(0.00)
11	241	0.24	(0.08)	0.53	(0.19)	0.26	(0.09)
13	1299	0.88	(0.28)	1.17	(0.37)	0.26	(0.08)
16	792	0.43	(0.00)	0.39	(0.00)	0.13	(0.01)
17	729	0.43	(0.01)	0.43	(0.00)	0.19	(0.02)
18	841	0.50	(0.10)	0.30	(0.07)	0.17	(0.04)
20	665	0.19	(0.00)	0.68	(0.00)	0.18	(0.00)
22	1328	0.36	(0.00)	0.55	(0.00)	0.14	(0.00)
23	1822	0.45	(0.04)	0.34	(0.03)	0.19	(0.02)
24	980	0.46	(0.05)	0.39	(0.04)	0.12	(0.01)
25	3493	0.43	(0.00)	0.41	(0.00)	0.15	(0.00)
26	123	0.22	(0.00)	0.73	(0.00)	0.09	(0.00)
27	390	0.32	(0.07)	0.54	(0.10)	0.14	(0.08)
28	1214	0.33	(0.03)	0.60	(0.05)	0.06	(0.00)
29	572	0.47	(0.04)	0.41	(0.09)	0.06	(0.05)
31	861	0.38	(0.01)	0.43	(0.00)	0.17	(0.00)
32	181	0.20	(0.02)	0.59	(0.06)	0.11	(0.02)
33	376	0.44	(0.11)	0.79	(0.19)	0.12	(0.06)
38	709	0.36	(0.07)	0.36	(0.07)	0.20	(0.04)
41	3408	0.35	(0.04)	0.48	(0.06)	0.12	(0.02)
42	1832	0.37	(0.00)	0.50	(0.01)	0.11	(0.01)
43	5569	0.52	(0.00)	0.42	(0.00)	0.11	(0.00)
45	2408	0.38	(0.00)	0.50	(0.00)	0.13	(0.00)
46	4881	0.22	(0.00)	0.57	(0.00)	0.15	(0.00)
47	921	0.28	(0.00)	0.55	(0.00)	0.20	(0.00)
49	4731	0.46	(0.01)	0.29	(0.00)	0.14	(0.02)
52	1371	0.26	(0.00)	0.48	(0.00)	0.16	(0.00)
55	304	0.41	(0.10)	0.48	(0.11)	0.10	(0.03)
56	668	0.61	(0.14)	0.35	(0.08)	0.05	(0.03)
71	137	0.20	(0.03)	0.65	(0.09)	0.16	(0.02)
77	320	0.15	(0.06)	0.51	(0.03)	0.27	(0.06)
78	292	0.27	(0.12)	0.56	(0.11)	0.19	(0.09)
81	935	0.51	(0.02)	0.40	(0.02)	0.16	(0.03)

This table reports the estimates of industry-level elasticities. Standard errors (in parentheses) are estimated using the bootstrap with 200 replications.

E.6. Turnover of permanent and temporary employees

Table E6
Firing costs and turnover of permanent & temporary employees
A. Main estimates

	ln(Permanent emp.)	ln(Temporary emp.)	ln(Permanent entering emp.)	ln(Temporary entering emp.)	ln(Permanent exiting emp.)	ln(Temporary exiting emp.)
	(1)	(2)	(3)	(4)	(5)	(6)
Blue-collar x Post	-0.014	0.023	-0.148***	-0.034	-0.100**	-0.003
	(0.015)	(0.058)	(0.045)	(0.063)	(0.045)	(0.067)
Observations	48,816	18,551	37,720	20,755	39,321	19,922
R-squared	0.977	0.820	0.741	0.813	0.792	0.816
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
2-digit industry x year FE	Yes	Yes	Yes	Yes	Yes	Yes

Table E6
(Continued)
B. Dynamic estimates

	ln(Permanent emp.)	ln(Temporary emp.)	ln(Permanent entering emp.)	ln(Temporary entering emp.)	ln(Permanent exiting emp.)	ln(Temporary exiting emp.)
	(1)	(2)	(3)	(4)	(5)	(6)
Blue-collar x 2009 dummy	0.004	0.047	0.017	-0.030	-0.019	0.028
	(0.017)	(0.068)	(0.059)	(0.076)	(0.049)	(0.076)
Blue-collar x 2010 dummy	-0.007	0.153**	-0.055	0.071	-0.075	0.010
	(0.012)	(0.075)	(0.053)	(0.070)	(0.052)	(0.065)
Blue-collar x 2011 dummy	0.008	0.060	-0.046	0.113	-0.029	0.038
	(0.008)	(0.073)	(0.045)	(0.073)	(0.055)	(0.077)
Blue-collar x 2014 dummy	-0.005	0.019	-0.126***	-0.074	-0.044	0.014
	(0.012)	(0.070)	(0.048)	(0.073)	(0.043)	(0.072)
Blue-collar x 2015 dummy	-0.024	0.068	-0.202***	-0.005	-0.128**	0.016
	(0.015)	(0.076)	(0.052)	(0.084)	(0.055)	(0.086)
Blue-collar x 2016 dummy	-0.014	0.072	-0.183***	0.012	-0.199***	-0.046
	(0.016)	(0.073)	(0.065)	(0.092)	(0.060)	(0.105)
Blue-collar x 2017 dummy	-0.010	0.220***	-0.174***	0.130	-0.176***	0.093
	(0.018)	(0.084)	(0.054)	(0.107)	(0.051)	(0.109)
Observations	48,816	18,551	37,720	20,755	39,321	19,922
R-squared	0.977	0.820	0.741	0.813	0.792	0.816
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
2-digit industry x year FE	Yes	Yes	Yes	Yes	Yes	Yes

This table reports difference-in-differences estimates of permanent and temporary employees over the period of 2009 to 2017. The observations during 2013 are excluded from the analysis. The dependent variables: (1) *ln(Permanent emp.)*, the natural log of the number of employees with permanent contracts, (2) *ln(Temporary emp.)*, the natural log of the number of employees with fixed-term contracts, (3) *ln(Permanent entering emp.)*, the natural log of the number of new hires with permanent contracts, (4) *ln(Temporary entering emp.)*, the natural log of the number of new hires with fixed-term contracts, (5) *ln(Permanent exiting emp.)*, the natural log of the number of employees with permanent contracts that left the firm, and (6) *ln(Temporary exiting emp.)*, the natural log of the number of employees with fixed-term contracts that left the firm. *Blue-collar* is a dummy variable equal to 1 if the firm's pre-period (2009-2012) average share of blue-collar workers was above its sample median, and to 0 otherwise. *Post* is a dummy variable equal to 1 for observations in the period of 2014 to 2017, and to 0 for observations in the period of 2009 to 2012. Year dummies are equal to 1 for observations in the given year, and to 0 otherwise. E.g., *2009 dummy* is equal to 1 for observations in the year of 2009, and to 0 for other years. Firm controls are $Size_{t-1}$, Age_t, $Leverage_{t-1}$, $EBITDA\text{-}to\text{-}assets_{t-1}$, $Cash\ holdings_{t-1}$, and $Capital\text{-}to\text{-}labour_{t-1}$. Table A1 defines all variables. The regressions only contain firms that have at least one observation in the pre-period (2009-2012) and at least one observation in the post-period (2014-2017). Standard errors (in parentheses) are clustered at the firm level. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

E.7. The effect on firm exit

In addition to looking at the change in firms' employment levels from pre- to post-period, we also investigate the effect of the legal change on firm exit. To do so, we run the following regression at the industry-level:

$$ln(1 + \#exit)_{s,t} = \alpha \; Blue - collar_s * Post_t + \Pi \; Industry \; Controls + \mu_s + \theta_t + \varepsilon_{s,t} \quad (11)$$

where the dependent variable, $ln(1 + \#exit)_{s,t}$ is the natural log of one plus the number of firm exits in the industry. $Blue - collar_s$ is a dummy variable equal to 1 if the industry's average share of blue-collar workers over the pre-period (2009-2012) was above the sample median, and to 0 otherwise. $Post$ is a dummy variable equal to 1 for observations in the period of 2014 to 2017, and to 0 for observations in the period of 2009 to 2012. We include $Industry \; Controls$, that is, we collapse the firm controls used in our main model (9) at the industry level. Additionally, the specification involves industry (μ_s) and time (θ_t) fixed effects. Standard errors are clustered at the industry level.

Columns (1) and (2) of Table E7 demonstrate the results from the estimation of Equation (11) at the 2-digit and 3-digit industry levels, respectively. Only in the latter, the effect on firm exit is significant. Overall, we find some evidence that increased firing costs lead to a rise in the number of exiting firms.

Table E7
Firing costs and firm exit

	2-digit industry	3-digit industry
	ln(1+#exit)	ln(1+#exit)
	(1)	(2)
Blue-collar (industry level) x Post	0.200	0.069**
	(0.126)	(0.032)
Observations	278	1,072
R-squared	0.490	0.310
Industry controls	Yes	Yes
Industry FE	Yes	Yes
Year FE	Yes	Yes

This table reports difference-in-differences estimates of firm exit over the period of 2009 to 2017. The observations during 2013 are excluded from the analysis. The dependent variable, *ln(1+#exit)*, is the natural log of one plus the number of total firm exits (defined through the firm's legal status) in the industry. In columns (1) and (2), the analysis is conducted at the 2-digit and 3-digit industry levels, respectively. *Blue-collar* is a dummy variable equal to 1 if the industry's average share of blue-collar workers over 2009-2012 was above the sample median, and to 0 otherwise. *Post* is a dummy variable equal to 1 for observations in the period of 2014 to 2017, and to 0 for observations in the period of 2009 to 2012. The regressions include the following firm controls collapsed at the industry level: $Size_{t-1}$, Age_t, $Leverage_{t-1}$, $EBITDA\text{-}to\text{-}assets_{t-1}$, $Cash\ holdings_{t-1}$, and $Capital\text{-}to\text{-}labour_{t-1}$. Table A1 defines all variables. Standard errors (in parentheses) are clustered at the industry level. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

E.8. The result with TFP constructed through accounting for selection bias

Table E8
Firing costs and productivity: Selection bias is addressed

	TFP	TFP
	(1)	(2)
Blue-collar x Post	-0.057***	
	(0.012)	
Blue-collar x 2009 dummy		0.017
		(0.016)
Blue-collar x 2010 dummy		0.006
		(0.015)
Blue-collar x 2011 dummy		0.004
		(0.013)
Blue-collar x 2014 dummy		-0.042***
		(0.014)
Blue-collar x 2015 dummy		-0.048***
		(0.017)
Blue-collar x 2016 dummy		-0.064***
		(0.019)
Blue-collar x 2017 dummy		-0.056***
		(0.018)
Observations	48,852	48,852
R-squared	0.858	0.858
Controls	Yes	Yes
Firm FE	Yes	Yes
2-digit industry x year FE	Yes	Yes

This table reports difference-in-differences estimates of total factor productivity over the period of 2009 to 2017. The observations during 2013 are excluded from the analysis. The dependent variable, *TFP*, is the residual from the estimation of a Cobb-Douglas production function through the methodology of Ackerberg et al. (2015), defined in natural log. The firm's survival probability is controlled for in the production function. *Blue-collar* is a dummy variable equal to 1 if the firm's pre-period (2009-2012) average share of blue-collar workers was above its sample median, and to 0 otherwise. *Post* is a dummy variable equal to 1 for observations in the period of 2014 to 2017, and to 0 for observations in the period of 2009 to 2012. Year dummies are equal to 1 for observations in the given year, and to 0 otherwise. E.g., *2009 dummy* is equal to 1 for observations in the year of 2009, and to 0 for other years. Firm controls are $Size_{t-1}$, Age_t, $Leverage_{t-1}$, $EBITDA\text{-}to\text{-}assets_{t-1}$, $Cash\ holdings_{t-1}$, and $Capital\text{-}to\text{-}labour_{t-1}$. Table A1 defines all variables. The regressions only contain firms that have at least one observation in the pre-period (2009-2012) and at least one observation in the post-period (2014-2017). Standard errors (in parentheses) are clustered at the firm level. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

E.9. Results across industries with varying change in the notice periods for blue-collar employees

Table E9
Firing costs and productivity: Results across industries with varying change in the notice periods for blue-collar workers

	less affected industries	more affected industries	less affected industries	more affected industries	less affected industries	more affected industries
	TFP	TFP	ln(Blue emp.)	ln(Blue emp.)	ln(White emp.)	ln(White emp.)
	(1)	(2)	(3)	(4)	(5)	(6)
Blue-collar x Post	-0.036**	-0.064***	0.011	-0.047*	0.047*	0.111***
	(0.018)	(0.016)	(0.036)	(0.024)	(0.027)	(0.027)
Observations	13,501	35,315	13,501	35,315	13,501	35,315
R-squared	0.899	0.952	0.974	0.960	0.976	0.970
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
2-digit industry x year FE	Yes	Yes	Yes	Yes	Yes	Yes

Table E9
(Continued)

	less affected industries	more affected industries	less affected industries	more affected industries	less affected industries	more affected industries
	ln(Tangible fix.)	ln(Tangible fix.)	ln(Mac. Equip.)	ln(Mac. Equip.)	ln(Land. Build. Furn. Other)	ln(Land. Build. Furn. Other)
	(7)	(8)	(9)	(10)	(11)	(12)
Blue-collar x Post	0.030	0.043	-0.086	-0.107	0.037	0.081**
	(0.032)	(0.028)	(0.056)	(0.097)	(0.051)	(0.040)
Observations	13,501	35,315	13,138	33,457	13,400	35,184
R-squared	0.966	0.949	0.930	0.901	0.940	0.917
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
2-digit industry x year FE	Yes	Yes	Yes	Yes	Yes	Yes

This table reports difference-in-differences estimates of total factor productivity, employment and capital over the period of 2009 to 2017. The observations during 2013 are excluded from the analysis. In columns (1) and (2), the dependent variable, *TFP*, is the residual from the estimation of a Cobb-Douglas production function through the methodology of Ackerberg et al. (2015), defined in natural log. In columns (3) and (4), the dependent variable, *ln(Blue emp.)*, is the natural log of the number of blue-collar employees. In columns (5) and (6), the dependent variable, *ln(White emp.)*, is the natural log of the number of white-collar employees. In columns (7) and (8), the dependent variable, *ln(Tangible fix.)*, is the natural log of total tangible fixed assets. In columns (9) and (10), the dependent variable, *ln(Mac., Equip.)*, is the natural log of the sum of machinery and equipment. In columns (11) and (12), the dependent variable, *ln(Land, Build., Furn., Other)*, is the natural log of the sum of land, building, furniture, and other tangible fixed capital. *Blue-collar* is a dummy variable equal to 1 if the firm's pre-period (2009-2012) average share of blue-collar workers was above its sample median, and to 0 otherwise. *Post* is a dummy variable equal to 1 for observations in the period of 2014 to 2017, and to 0 for observations in the period of 2009 to 2012. *Less affected industries* (*More affected industries*) are those where the notice period for a blue-collar worker with 10-year tenure increased by less than 175 days (by 175 days or more). Firm controls are $Size_{t-1}$, Age_t, $Leverage_{t-1}$, $EBITDA\text{-}to\text{-}assets_{t-1}$, $Cash\ holdings_{t-1}$, and $Capital\text{-}to\text{-}labour_{t-1}$. Table A1 defines all variables. The regressions only contain firms that have at least one observation in the pre-period (2009-2012) and at least one observation in the post-period (2014-2017). Standard errors (in parentheses) are clustered at the firm level. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

E.10. Results across initially exempted industries

Table E10
Firing costs and productivity: Results across industries where blue-collar workers were initially exempted

	TFP	ln(Blue emp.)	ln(White emp.)	ln(Tangible fix.)	ln(Mac., Equip.)	ln(Land, Build., Furn., Other)
	(1)	(2)	(3)	(4)	(5)	(6)
Blue-collar x Post	-0.058***	-0.023	0.094***	0.055**	-0.086	0.073**
	(0.015)	(0.022)	(0.026)	(0.023)	(0.082)	(0.035)
Blue-collar x Post x Exempted industry	0.007	-0.030	-0.008	-0.051	-0.049	0.003
	(0.024)	(0.048)	(0.036)	(0.058)	(0.147)	(0.085)
Observations	48,852	48,852	48,852	48,852	46,630	48,620
R-squared	0.944	0.964	0.971	0.955	0.913	0.924
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
2-digit industry x year FE	Yes	Yes	Yes	Yes	Yes	Yes

This table reports difference-in-differences estimates of total factor productivity, employment and capital over the period of 2009 to 2017. The observations during 2013 are excluded from the analysis. In column (1), the dependent variable, *TFP*, is the residual from the estimation of a Cobb-Douglas production function through the methodology of Ackerberg et al. (2015), defined in natural log. In column (2), the dependent variable, *ln(Blue emp.)*, is the natural log of the number of blue-collar employees. In column (3), the dependent variable, *ln(White-collar emp.)*, is the natural log of the number of white-collar employees. In column (4), the dependent variable, *ln(Tangible fix.)*, is the natural log of total tangible fixed assets. In column (5), the dependent variable, *ln(Mac., Equip.)*, is the natural log of the sum of machinery and equipment. In column (6), the dependent variable, *ln(Land, Build., Furn., Other)*, is the natural log of the sum of land, building, and furniture. *Blue-collar* is a dummy variable equal to 1 if the firm's pre-period (2009-2012) average share of blue-collar workers was above its sample median, and to 0 otherwise. *Post* is a dummy variable equal to 1 for observations in the period of 2014 to 2017, and to 0 for observations in the period of 2009 to 2012. *Exempted industry* is equal to 1 for construction, upholstery and woodworking industries, and to 0 otherwise. Firm controls are $Size_{t-1}$, Age_t, $Leverage_{t-1}$, $EBITDA\text{-}to\text{-}assets_{t-1}$, $Cash\ holdings_{t-1}$, and $Capital\text{-}to\text{-}labour_{t-1}$. Table A1 defines all variables. The regressions only contain firms that have at least one observation in the pre-period (2009-2012) and at least one observation in the post-period (2014-2017). Standard errors (in parentheses) are clustered at the firm level. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

E.11. Results with treatment intensity

Table E11
Firing costs and productivity: Results with treatment intensity
A. Main estimates

	TFP	ln(Blue emp.)	ln(White emp.)	ln(Tangible fix.)	ln(Mac., Equip.)	ln(Land, Build., Furn., Other)
	(1)	(2)	(3)	(4)	(5)	(6)
(%) Blue-collar x Post	-0.138***	-0.103*	0.245***	0.082	-0.083	0.150*
	(0.038)	(0.055)	(0.045)	(0.067)	(0.123)	(0.087)
Observations	48,852	48,852	48,852	48,852	46,630	48,620
R-squared	0.944	0.964	0.971	0.955	0.913	0.924
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
2-digit industry x year FE	Yes	Yes	Yes	Yes	Yes	Yes

Table E11
(Continued)
B. Dynamic estimates

	TFP	ln(Blue emp.)	ln(White emp.)	ln(Tangible fix.)	ln(Mac., Equip.)	ln(Land, Build., Furn., Other)
	(1)	(2)	(3)	(4)	(5)	(6)
(%) Blue-collar x 2009 dummy	0.068	-0.092	0.036	-0.010	0.105	-0.004
	(0.051)	(0.079)	(0.036)	(0.072)	(0.143)	(0.098)
(%) Blue-collar x 2010 dummy	0.002	-0.082	-0.055*	-0.050	0.123	-0.049
	(0.044)	(0.056)	(0.031)	(0.066)	(0.165)	(0.079)
(%) Blue-collar x 2011 dummy	0.030	-0.034	0.003	0.071	0.388**	0.061
	(0.038)	(0.030)	(0.031)	(0.063)	(0.177)	(0.073)
(%) Blue-collar x 2014 dummy	-0.077*	-0.082*	0.155***	0.043	0.025	0.052
	(0.040)	(0.050)	(0.033)	(0.093)	(0.112)	(0.106)
(%) Blue-collar x 2015 dummy	-0.157***	-0.162***	0.213***	-0.008	0.209	0.031
	(0.058)	(0.061)	(0.050)	(0.072)	(0.318)	(0.095)
(%) Blue-collar x 2016 dummy	-0.135**	-0.163**	0.303***	0.184**	0.060	0.265***
	(0.060)	(0.072)	(0.053)	(0.084)	(0.191)	(0.103)
(%) Blue-collar x 2017 dummy	-0.099*	-0.226***	0.352***	0.194*	-0.056	0.378***
	(0.057)	(0.085)	(0.059)	(0.106)	(0.207)	(0.123)
Observations	48,852	48,852	48,852	48,852	46,630	48,620
R-squared	0.944	0.964	0.971	0.955	0.913	0.924
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
2-digit industry x year FE	Yes	Yes	Yes	Yes	Yes	Yes

This table reports difference-in-differences estimates of total factor productivity, employment and capital over the period of 2009 to 2017. Panels A and B show the main estimates and the dynamic estimates, respectively. The observations during 2013 are excluded from the analysis. In column (1), the dependent variable, *TFP*, is the residual from the estimation of a Cobb-Douglas production function through the methodology of Ackerberg et al. (2015), defined in natural log. In column (2), the dependent variable, *ln(Blue emp.)*, is the natural log of the number of blue-collar employees. In column (3), the dependent variable, *ln(White emp.)*, is the natural log of the number of white-collar employees. In column (4), the dependent variable, *ln(Tangible fix.)*, is the natural log of total tangible fixed assets. In column (5), the dependent variable, *ln(Mac., Equip.)*, is the natural log of the sum of machinery and equipment. In column (6), the dependent variable, *ln(Land, Build., Furn., Other)*, is the natural log of the sum of land, building, furniture, and other tangible fixed capital. *(%) Blue-collar* is the firm's pre-period (2009-2012) average share of blue-collar workers. *Post* is a dummy variable equal to 1 for observations in the period of 2014 to 2017, and to 0 for observations in the period of 2009 to 2012. Year dummies are equal to 1 for observations in the given year, and to 0 otherwise. E.g., *2009 dummy* is equal to 1 for observations in the year of 2009, and to 0 for other years. Firm controls are $Size_{t-1}$, Age_t, $Leverage_{t-1}$, $EBITDA\text{-}to\text{-}assets_{t-1}$, $Cash\ holdings_{t-1}$, and $Capital\text{-}to\text{-}labour_{t-1}$. Table A1 defines all variables. The regressions only contain firms that have at least one observation in the pre-period (2009-2012) and at least one observation in the post-period (2014-2017). Standard errors (in parentheses) are clustered at the firm level. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

E.12. Linear effects of firing costs

Table E12
Firing costs and productivity: Linear effects

	TFP	ln(Blue emp.)	ln(White emp.)	ln(Tangible fix.)	ln(Mac., Equip.)	ln(Land, Build., Furn., Other)
	(1)	(2)	(3)	(4)	(5)	(6)
Q2 (%) Blue-collar x Post	-0.010	-0.009	0.051	0.041	0.236*	0.009
	(0.024)	(0.038)	(0.034)	(0.042)	(0.129)	(0.063)
Q3 (%) Blue-collar x Post	-0.046**	-0.025	0.085***	0.057**	0.047	0.077**
	(0.020)	(0.026)	(0.027)	(0.028)	(0.060)	(0.038)
Q4 (%) Blue-collar x Post	-0.077***	-0.046*	0.153***	0.068**	-0.002	0.079*
	(0.025)	(0.028)	(0.036)	(0.032)	(0.064)	(0.041)
Observations	48,852	48,852	48,852	48,852	46,630	48,620
R-squared	0.944	0.964	0.971	0.955	0.913	0.924
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
2-digit industry x year FE	Yes	Yes	Yes	Yes	Yes	Yes

This table reports difference-in-differences estimates of total factor productivity, employment and capital over the period of 2009 to 2017. The observations during 2013 are excluded from the analysis. In column (1), the dependent variable, *TFP*, is the residual from the estimation of a Cobb-Douglas production function through the methodology of Ackerberg et al. (2015), defined in natural log. In column (2), the dependent variable, *ln(Blue emp.)*, is the natural log of the number of blue-collar employees. In column (3), the dependent variable, *ln(White-collar emp.)*, is the natural log of the number of white-collar employees. In column (4), the dependent variable, *ln(Tangible fix.)*, is the natural log of total tangible fixed assets. In column (5), the dependent variable, *ln(Mac., Equip.)*, is the natural log of the sum of machinery and equipment. In column (6), the dependent variable, *ln(Land, Build., Furn., Other)*, is the natural log of the sum of land, building, and furniture. *(%) Blue-collar* is the firm's pre-period average share of blue-collar workers. Qi (%) Blue-collar is a dummy equal to 1 if the firm belongs to the ith quartile of the treatment intensity variable (i.e., the firm's average share of blue-collar workers over 2009-2012) .*Post* is a dummy variable equal to 1 for observations in the period of 2014 to 2017, and to 0 for observations in the period of 2009 to 2012. Firm controls are $Size_{t-1}$, Age_t, $Leverage_{t-1}$, $EBITDA\text{-}to\text{-}assets_{t-1}$, $Cash\ holdings_{t-1}$, and $Capital\text{-}to\text{-}labour_{t-1}$. Table A1 defines all variables. The regressions only contain firms that have at least one observation in the pre-period (2009-2012) and at least one observation in the post-period (2014-2017). Standard errors (in parentheses) are clustered at the firm level. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

E.13. Results without controls

Table E13
Results without controls
A. Matched sample

	ln(Blue emp.)	ln(Blue emp.)	ln(White emp.)	ln(White emp.)	ln(Tangible fix.)	ln(Tangible fix.)	ln(Mac., Equip.)	ln(Mac., Equip.)	ln(Land, Build., Furn., Other)	ln(Land, Build., Furn., Other)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Blue-collar x Post	-0.027		0.096***		0.100***		-0.046		0.129***	
	(0.021)		(0.022)		(0.037)		(0.078)		(0.046)	
Blue-collar x 2009 dummy		-0.041		0.022		-0.031		-0.090		-0.016
		(0.032)		(0.018)		(0.061)		(0.064)		(0.075)
Blue-collar x 2010 dummy		-0.026		-0.003		-0.038		-0.047		-0.037
		(0.023)		(0.014)		(0.038)		(0.058)		(0.043)
Blue-collar x 2011 dummy		-0.015		0.003		-0.009		0.018		-0.023
		(0.010)		(0.012)		(0.018)		(0.043)		(0.025)
Blue-collar x 2014 dummy		-0.021		0.062***		0.045		-0.050		0.056
		(0.017)		(0.016)		(0.037)		(0.064)		(0.050)
Blue-collar x 2015 dummy		-0.047**		0.098***		0.025		-0.091		0.037
		(0.023)		(0.028)		(0.045)		(0.117)		(0.053)
Blue-collar x 2016 dummy		-0.058**		0.120***		0.108**		-0.065		0.142***
		(0.024)		(0.024)		(0.044)		(0.106)		(0.054)
Blue-collar x 2017 dummy		-0.067**		0.141***		0.184***		-0.082		0.258***
		(0.027)		(0.027)		(0.055)		(0.113)		(0.067)
Observations	48,852	48,852	48,852	48,852	48,852	48,852	46,630	46,630	48,620	48,620
R-squared	0.961	0.961	0.968	0.968	0.925	0.925	0.901	0.901	0.896	0.896
Firm controls	No	No	No	No	No	No	No	No	No	No
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
2-digit industry x year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table E13
(Continued)
B. Non-matched sample

	ln(Blue emp.)	ln(Blue emp.)	ln(White emp.)	ln(White emp.)	ln(Tangible fix.)	ln(Tangible fix.)	ln(Mac., Equip.)	ln(Mac., Equip.)	ln(Land, Build., Furn., Other)	ln(Land, Build., Furn., Other)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Blue-collar x Post	-0.021*		0.062***		0.030		-0.010		0.033	
	(0.012)		(0.011)		(0.022)		(0.030)		(0.025)	
Blue-collar x 2009 dummy		-0.022*		0.008		0.029		-0.009		0.039
		(0.013)		(0.012)		(0.023)		(0.035)		(0.029)
Blue-collar x 2010 dummy		-0.015		-0.003		0.024		-0.019		0.035
		(0.011)		(0.010)		(0.019)		(0.030)		(0.024)
Blue-collar x 2011 dummy		-0.009		-0.001		0.024*		-0.022		0.026
		(0.008)		(0.007)		(0.013)		(0.021)		(0.017)
Blue-collar x 2014 dummy		-0.027***		0.039***		0.029		-0.023		0.034
		(0.010)		(0.009)		(0.018)		(0.027)		(0.022)
Blue-collar x 2015 dummy		-0.020*		0.062***		0.028		-0.012		0.019
		(0.012)		(0.011)		(0.022)		(0.034)		(0.026)
Blue-collar x 2016 dummy		-0.037**		0.072***		0.060**		-0.046		0.084***
		(0.015)		(0.013)		(0.025)		(0.038)		(0.030)
Blue-collar x 2017 dummy		-0.050***		0.088***		0.094***		-0.008		0.112***
		(0.017)		(0.015)		(0.028)		(0.041)		(0.035)
Observations	49,447	49,447	49,447	49,447	49,447	49,447	47,156	47,156	49,188	49,188
R-squared	0.963	0.963	0.967	0.967	0.925	0.925	0.903	0.903	0.898	0.898
Firm controls	No	No	No	No	No	No	No	No	No	No
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
2-digit industry x year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

This table reports difference-in-differences estimates of employment and capital over the period of 2009 to 2017. Panels A and B show the results using the matched sample and the non-matched sample, respectively. The observations during 2013 are excluded from the analysis. In columns (1) and (2), the dependent variable, *ln(Blue emp.)*, is the natural log of the number of blue-collar employees. In columns (3) and (4), the dependent variable, *ln(White emp.)*, is the natural log of the number of white-collar employees. In columns (5) and (6), the dependent variable, *ln(Tangible fix.)*, is the natural log of total tangible fixed assets. In columns (7) and (8), the dependent variable, *ln(Mac., Equip.)*, is the natural log of the sum of machinery and equipment. In columns (9) and (10), the dependent variable, *ln(Land, Build., Furn., Other)*, is the natural log of the sum of land, building, furniture, and other tangible fixed capital. *Blue-collar* is a dummy variable equal to 1 if the firm's pre-period (2009-2012) average share of blue-collar workers was above its sample median, and to 0 otherwise. *Post* is a dummy variable equal to 1 for observations in the period of 2014 to 2017, and to 0 for observations in the period of 2009 to 2012. Year dummies are equal to 1 for observations in the given year, and to 0 otherwise. E.g., *2009 dummy* is equal to 1 for observations in the year of 2009, and to 0 for other years. The regressions only contain firms that have at least one observation in the pre-period (2009-2012) and at least one observation in the post-period (2014-2017). Standard errors (in parentheses) are clustered at the firm level. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

E.14. Cross-country analysis: Belgian versus German and French firms

In this section, we compare Belgian firms to German and French firms (i.e., firms in the two largest neighbouring countries of Belgium) that are not affected by the Belgian labour legislation and that, to the best of our knowledge, did not experience a significant shock (legal or other) that would affect the composition of their workforce during our sample period. This analysis is interesting for two reasons. First, it helps us to separately assess the effect of the new Belgian labour legislation on blue- and white-collar firms. Second, it helps us to address potential concerns that financial support programs, such as the Outright Monetary Transactions program (Acharya et al., 2019), implemented in response to the sovereign debt crises, might have differently affected blue- and white-collar firms.

Data. We collect data on German and French firms' financial accounts and employment from the ORBIS database provided by BvD. Just as in our main sample, we use historical files and incorporate both surviving firms and failed firms into our analysis. Compared to the Bel-first database, however, ORBIS does not provide information on German and French firms' workforce composition. For this reason, in this analysis, we compare Belgian firms in blue-collar industries to a matched sample of German and French firms in the same industries, assuming that the workforce composition in Belgium, Germany, and France is similar in the same industries. We also do the same for firms in white-collar industries.

Methodology. We estimate the following difference-in-differences model separately for blue-collar industries (i.e., NACE 4-digit industries whose pre-period average share of blue-collar workers was above its sample median) and white-collar industries (i.e., NACE 4-digit industries whose pre-period average share of blue-collar workers was below its sample median):

$$\mathit{Ln(Value\text{-}added)}_{i,t} = \beta Treated_c * Post_t + \Pi Controls + \theta_{st} + \mu_{cs} + \varepsilon_{i,t} \tag{12}$$

where the dependent variable is the natural log of value-added. $Treated_c$ is a dummy variable equal to 1 if firm i is located in Belgium, and to 0 if firm i is located in Germany or in France. $Post_t$ is a dummy variable equal to 1 for observations in the period of 2014 to 2017, and to 0 for observations in the period of 2009 to 2012. As before, we exclude observations in 2013 to eliminate the contaminating effects of the announcement. Just as *Controls* include firm characteristics (Size_{t-1}, Age_t, $\mathit{Leverage}_{t-1}$, $\mathit{EBITDA\text{-}to\text{-}assets}_{t-1}$, $\mathit{Cash\ holdings}_{t-1}$, and $\mathit{Capital\text{-}to\text{-}labour}_{t-1}$) as in Equation (9), this time we additionally include *GDP growth* and

GDP per capita to account for time-varying macroeconomic conditions across countries.[33] This is because we cannot include country-time fixed effects in our model as they would subsume the interaction term (i.e., our main variable of interest). We saturate our model with 4-digit industry × year (θ_{st}) and country × 4-digit industry (μ_{cs}) fixed effects. We also use firm fixed effects instead of country × 4-digit industry fixed effects. Lastly, standard errors are clustered at the country × 4-digit industry level.[34]

Note that we do not use TFP as the dependent variable because we are not able to estimate elasticities separately for blue- and white-collar workers due to data limitations for German and French firms, which is crucial in our setting. See the discussions in Sections 3.3 and 4.2.

Given that treated and control firms are drawn from different countries, as before, we conduct a propensity score matching procedure to make these two groups as comparable as possible. To obtain propensity scores, we consider 2012 as a baseline year and estimate a logit regression using *Treated* as the dependent variable. As explanatory variables, we include firm controls, as well as a set of additional variables to ensure similar pre-period trends for the treatment and control groups. Specifically, we add the log growths of value-added, tangible fixed assets, and the total number of employees, as well as both the lagged and contemporaneous values of the natural log of the total number of employees. Furthermore, we include 4-digit industry dummies. Then, we carry out a nearest neighbour matching of propensity scores with exact matches in NACE 4-digit industries. We match each treated firm with a control firm, and allow that a control firm can be used as a match for multiple treated firms.[35] Panel A of Table E14.1 compares the pre-treatment sample means of treated and controls firms in the unmatched and matched samples. Our matching procedure yields treatment and control samples which are more similar in their firm characteristics, relative to the unmatched sample. Panel B of Table E14.1 demonstrates the summary statistics for the matched sample.

Results. Panel A of Table E14.2 illustrates the results from the estimation of Equation (12). Columns (1) and (2) show the results on blue-collar and white-collar industries, respectively. Columns (3) and (4) repeat the same estimations, except that country × 4-digit industry fixed effects are replaced with firm fixed effects. The negative point estimates for blue-collar industries indicate that Belgian firms in blue-collar industries (i.e., those that mainly experienced an increase in firing costs) on average experienced a decline in

[33] We obtain data on *GDP growth* and *GDP per capita* from the World Bank.

[34] We obtain nearly similar results when we cluster standard errors at the firm level.

[35] Our results are also robust when two control firms are retained for each treated firm.

value-added, relative to their matched German and French counterparts. At the same time, the positive point estimates for white-collar industries suggest that the decrease in the notice periods for white-collar workers (despite the abolition of trial periods, improved outplacement rights, and increased protection against unfair dismissals) helped firms raise value-added. This is consistent with that notice periods constitute one of the most important elements of employment protection (OECD, 2013) and have been shown to affect firms' employment decisions (Lazear, 1990).

Using the full sample of firms, we also conduct a triple difference-in-difference approach by interacting the treated and post dummy variables in Equation (12) with an indicator variable that is equal to 1 if the firm operates in a blue-collar industry, and to 0 if the firm operates in a white-collar industry. For this analysis, we are able to use country $\times$ year fixed effects as the coefficient of interest now varies at the country-industry-year level. These results are shown in Panel B of Table E14.2. Columns (1) and (2) demonstrate the estimates with and without triple interactions, respectively. Although the average estimated effect is not significant in the former, the point estimate on the triple interaction term is negative and significant at the 1% confidence level in the latter. This indicates that, after the implementation of new notice periods, the value-added of Belgian firms relative to non-Belgian firms in blue-collar industries was significantly lower than the value-added of Belgian firms relative to non-Belgian firms in white-collar industries. Column (3) exhibits the dynamic version of the estimation in column (2). These estimates show that the triple interaction terms are significant after but not before the introduction of the new notice periods. Finally, columns (4)-(6) repeat the first three estimations by additionally using firm fixed effects. The inclusion of firm fixed effects barely changes the results in columns (1)-(3).

Overall, we find that Belgian blue-collar (white-collar) firms on average experienced a decrease (an increase) in value-added, relative to their matched non-Belgian counterparts, suggesting that changes in the notice periods had symmetric effects. Additionally, these results further mitigate the concern that financial programs to overcome the sovereign debt crisis confound our main estimates through differently affecting blue- and white-collar firms.

Table E14.1
Firing costs and productivity: Cross-country analysis

A. Comparing pre-period sample means for treatment and control firms

	Non-matched sample			Matched sample		
	Treated N=19,746	Control N=99,147	Difference	Treated N=19,168	Control N=18,043	Difference
Firm level						
Ln(Value added)	15.644	14.789	0.855***	15.656	15.869	-0.213
Size	16.106	14.807	1.299***	16.119	16.178	-0.059
Age	29.101	23.551	5.55***	29.233	30.36	-1.127
Leverage	0.622	0.638	-0.016***	0.62	0.629	-0.009
EBITDA-to-assets	0.109	0.114	-0.005***	0.109	0.11	-0.001
Cash holdings	0.116	0.168	-0.052***	0.116	0.118	-0.002
Capital-to-labour	10.246	9.125	1.121***	10.244	10.21	0.034
Country level						
GDP growth (%)	0.886	0.647	0.239***	0.894	0.795	0.099***
GDP per capita (in thous. $)	40.373	37.555	2.818***	40.341	38.287	2.054***

B. Full sample

	N	Mean	SD	p(25)	p(50)	p(75)
Firm level						
Ln(Value added)	68,539	15.829	1.381	14.922	15.714	16.675
Size	68,539	16.205	1.498	15.319	16.113	17.095
Age	68,539	31.863	25.529	18	26	40
Leverage	68,539	0.622	0.243	0.452	0.642	0.794
EBITDA-to-assets	68539	0.108	0.103	0.047	0.095	0.158
Cash holdings	68,539	0.117	0.146	0.014	0.058	0.164
Capital-to-labour	68,539	10.225	1.391	9.441	10.339	11.123
Country level						
GDP growth (%)	68,539	1.01	1.863	0.459	1.478	2.033
GDP per capita (in thous. $)	68,539	41.111	3.969	37.786	40.144	44.93

This table presents summary statistics for the main variables used in the cross-country analysis. Panel A compares the sample means of treated and control firms in the unmatched and matched samples over the pre- (2009-2012) period. Panel B shows the summary statistics for the matched sample. The main period of the analysis is from 2009 and 2017. The period for the lagged variables spans from 2008 to 2016. The observations during 2013 are excluded from the analysis. *Treated* is a dummy variable equal to 1 if the firm is located in Belgium, and to 0 if the firm is located in Germany or in France (i.e., Control). Table A1 defines all variables. The sample only contains firms that have at least one observation in the pre-period (2009-2012) and at least one observation in the post-period (2014-2017). *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively, for t-test of whether the treatment and control groups have equal means for a given variable. In our t-tests, we cluster standard errors at the firm level to account for that some control firms are matched to affected firms more than once.

Table E14.2
Firing costs and productivity: Belgian firms versus German and French Firms
A. Split sample analysis

	Blue-collar industries	White-collar industries	Blue-collar industries	White-collar industries
	ln(Value-added)	ln(Value-added)	ln(Value-added)	ln(Value-added)
	(1)	(2)	(3)	(4)
Treated x Post	-0.028***	0.015**	-0.020**	0.017**
	(0.010)	(0.008)	(0.008)	(0.007)
Observations	32,910	35,629	32,910	35,629
R-squared	0.886	0.880	0.984	0.985
Firm controls	Yes	Yes	Yes	Yes
Macro controls	Yes	Yes	Yes	Yes
Firm FE	No	No	Yes	Yes
4-digit industry x year FE	Yes	Yes	Yes	Yes
Country x 4-digit industry FE	Yes	Yes	No	No

Table E14.2
(Continued)
B. Triple-difference

	ln(Value-added)	ln(Value-added)	ln(Value-added)	ln(Value-added)	ln(Value-added)	ln(Value-added)
	(1)	(2)	(3)	(4)	(5)	(6)
Treated x Post	-0.006			-0.001		
	(0.007)			(0.006)		
Blue-collar industries x Treated x Post		-0.047***			-0.037***	
		(0.014)			(0.011)	
Blue-collar industries x Treated x 2009 dummy			0.008			0.011
			(0.020)			(0.014)
Blue-collar industries x Treated x 2010 dummy			0.006			0.002
			(0.014)			(0.009)
Blue-collar industries x Treated x 2011 dummy			0.006			0.002
			(0.012)			(0.009)
Blue-collar industries x Treated x 2014 dummy			-0.034**			-0.026***
			(0.014)			(0.010)
Blue-collar industries x Treated x 2015 dummy			-0.043**			-0.035***
			(0.018)			(0.013)
Blue-collar industries x Treated x 2016 dummy			-0.038*			-0.042***
			(0.020)			(0.014)
Blue-collar industries x Treated x 2017 dummy			-0.065**			-0.034*
			(0.028)			(0.020)
Observations	68,539	68,539	68,539	68,539	68,539	68,539
R-squared	0.882	0.882	0.882	0.984	0.984	0.984
Firm controls	Yes	Yes	Yes	Yes	Yes	Yes
Macro controls	Yes	No	No	Yes	No	No
Firm FE	No	No	No	Yes	Yes	Yes
4-digit industry x year FE	Yes	Yes	Yes	Yes	Yes	Yes
Country x year FE	No	Yes	Yes	No	Yes	Yes
Country x 4-digit industry FE	Yes	Yes	Yes	No	No	No

This table reports difference-in-differences (Panel A) and triple-difference (Panel B) estimates of value-added over the period of 2009 to 2017. The observations during 2013 are excluded from the analysis. *Blue-collar industries (White-collar industries)* are 4-digit industries whose pre-period average share of blue-collar workers was above (below) its sample median. The dependent variable, *ln(Value added)*, is the natural log of value added. *Treated* is a dummy variable equal to 1 if the firm is located in Belgium, and to 0 if the firm is located in Germany or France. *Post* is a dummy variable equal to 1 for observations in the period of 2014 to 2017, and to 0 for observations in the period of 2009 to 2012. Year dummies are equal to 1 for observations in the given year, and to 0 otherwise. E.g., *2009 dummy* is equal to 1 for observations in the year of 2009, and to 0 for other years. Firm controls are $Size_{t-1}$, Age_t, $Leverage_{t-1}$, $EBITDA\text{-}to\text{-}assets_{t-1}$, $Cash\ holdings_{t-1}$, and $Capital\text{-}to\text{-}labour_{t-1}$. Macro controls are $GDP\ growth_{t-1}$ and $GDP\ per\ capita_{t-1}$. Table A1 defines all variables. The regressions only contain firms that have at least one observation in the pre-period (2009-2012) and at least one observation in the post-period (2014-2017). Standard errors (in parentheses) are clustered at the country × 4-digit industry level. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.