

Heterogeneous Firing Costs, Worker Types, and Productivity: Evidence from a Natural Experiment*

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April 2026

Abstract

We investigate how firing costs affect input choices and total factor productivity (TFP) by exploiting a labor reform in Belgium that increased firing costs of blue-collar workers relative to white-collar workers. Using a difference-in-differences design, we show that hiring and separations at blue-collar-intensive firms declined by 8 percent after the reform relative to white-collar-intensive firms. Moreover, blue-collar-intensive firms shifted workforce composition toward white-collar workers, offered fewer permanent contracts, and relied more on outsourced labor. We find no evidence of capital-intensive technology adoption and only little evidence of human capital investment. Ultimately, blue-collar-intensive firms experienced a 4.8 percent decline in TFP.

JEL classification: D24, D25, J63, J65

Keywords: firing costs, employment protection, productivity, misallocation

*We are grateful to Árpád Ábrahám, Bart Cockx, Hans Degryse, Olivier De Jonghe, Alessandro Ferrari, Tomislav Ladika, Cristina Lafuente Martinez, Ramon Marimon, Bastian Schulz, Angelos Theodorakopoulos, Marijn Verschelde, and Michael Wittry; conference participants at the 2021 Annual Conference of the European Association of Labor Economists, 2021 Conference on International Economics of the Spanish Association of International Economics and Finance, 2021 International Conference of the French Finance Association, 2022 Conference in Honour of Ramon Marimon, 2023 Catalan Economic Society Conference, the 2023 Annual Meeting of the European Economic Association, and the 9th SDU Finance Workshop; and seminar participants at the Benelux Corporate Finance Network, Ghent University, KU Leuven, and Universitat Pompeu Fabra for valuable comments and suggestions. We thank Madalena Gaspar for excellent research assistance. We acknowledge the National Bank of Belgium for access to the Belgian Credit Register. The views expressed are solely those of the authors and do not necessarily represent the views of the National Bank of Belgium. Ozan Güler, Mike Mariathasan, and Klaas Mulier acknowledge the Research Foundation – Flanders for financial support (FWO Grant number G068218N). Andrea Caggese and Ozan Güler acknowledge financial support from the Ministerio de Educación y Ciencia grant n. PID2020-116268GB-I00 on “Firms and Households Heterogeneity, Aggregate Fluctuations, and Macroeconomic Policies.” Andrea Caggese acknowledges financing from the Severo Ochoa Programme for Centres of Excellence in R&D (Barcelona School of Economics CEX2019-000915-S). Ozan Güler also acknowledges financial support from the Spanish Ministry of Science, Innovation and Universities, Project PID2024-161782NB-I00. An earlier version of this paper was circulated under the title “Firing Costs and Productivity: Evidence from a Natural Experiment.”

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1. Introduction

Firms operate in environments where demand, technology, and competitive conditions can change unexpectedly. To respond efficiently, they must be able to swiftly adjust their use of inputs, including labor. When firing is costly, however, firms may be inefficiently reluctant to fire existing workers and hesitant to hire new ones. Such distortions in firing and hiring have been shown to reduce aggregate output by causing resource misallocation across firms, or by distorting firm entry and exit (e.g., [Bentolila and Bertola, 1990](#); [Hopenhayn and Rogerson, 1993](#); [Da-Rocha et al., 2019](#)).

Furthermore, firing costs can also affect firm-level productivity. They might lower productivity by inducing firms to retain less productive workers or by penalizing risky, productivity-enhancing investments that involve laying off workers in case of failure. At the same time, firing costs may also incentivize firms to adapt their practices in ways that could increase (measured) productivity. Firms might screen candidates more carefully and hence hire more productive workers, or invest in labor-saving technologies. In addition, firing costs reduce the risk of dismissal and may encourage workers and firms to invest in training and firm-specific human capital. Which of these channels dominates, and whether firing costs ultimately affect firm-level productivity positively or negatively, remain open questions that require empirical investigation.¹

Although several studies have analyzed the relationship between firing costs and broader economic outcomes, only very few have identified the effect on firm-level productivity in quasi-experimental settings. Among these, [Autor et al. \(2007\)](#) exploit the staggered adoption of wrongful discharge laws (WDLs) by US states, and [Cingano et al. \(2016\)](#) study an Italian reform introducing wrongful discharge protection for firms below a 15-employee threshold. Both papers find that higher firing costs reduce TFP. However, [Autor et al. \(2007\)](#) suggest “*a cautious interpretation of the findings until further evidence accumulates*” (p. F212) because US states’ WDL adoption was preceded by investment downturns, which raises concerns about reverse causality. In [Cingano et al. \(2016\)](#), instead, it remains unclear whether the identified effect applies across the entire firm-size distribution.

We contribute to the literature in two main ways. First, we study an increase in notice periods, which differs from the introduction of wrongful discharge protection studied in previous work, as it also affects good-faith layoffs and applies to firms of all sizes. Exploiting the exogenous nature of the increase in notice periods, we identify a negative, large, and persistent effect of firing costs on TFP. Second, we provide novel and detailed quasi-experimental evidence on the channels linking firing costs to TFP, including results on workforce composition and utilization, as well as on capital deepening, that differ from existing studies.

¹In Section 2, we provide a simple theoretical framework to describe the different channels through which firing costs affect measured firm-level productivity, and the related literature, in more detail.

For identification, we exploit a unique setting provided by the Belgian labor market. Since the early 1900s, Belgian law has made a distinction between blue-collar workers (those doing manual work) and white-collar workers (those doing non-manual work), and it used to provide 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 of employment protection legislation (EPL) across worker types with the “Act of 26 December 2013”. This Act significantly increased the firing costs for blue-collar workers. For example, the notice period 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 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 both worker types, but were capped at 14 days for blue-collar workers and at 365 days for white-collar workers. The total impact of the Act on firing costs of white-collar workers is hence ambiguous: it has become more costly to fire white-collar workers during their first year of tenure, but less costly thereafter. As we explain 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 characteristics (e.g., the number of workers and its disaggregation across worker types, remuneration, hours worked, and outsourced labor) as well as balance sheets and income statements of Belgian firms between 2009 and 2017 to investigate the relationship between firing costs and productivity. We estimate a difference-in-differences (DiD) model that compares input choices and TFP of *blue-collar firms* relative to *white-collar firms*, before and after the legal reform. We define blue-collar firms (white-collar firms) as those with an above-median (below-median) share of blue-collar workers before the announcement of the legal reform. As a result, we compare two groups of firms, with one group experiencing an increase in firing costs relative to the other group. We saturate our regression models with firm and industry $\times$ year fixed effects to capture potential pre-existing trends pertinent to unobservable factors. Consistent with a causal interpretation of our results, we find that differences between blue- and white-collar firms emerge only after, and not before, the announcement of the reform.

Using this setup, we first investigate how increasing firing costs affect employment decisions. We find that the reform led to an economically and statistically significant reduction in worker turnover: separations declined by up to 9 percent two years after the announcement, while new employment reached a peak reduction of 8 percent after one year. Firing costs thus affected both firing and hiring. In addition, we also observe that the reform induced firms to rely less on blue-collar workers and more on flexible labor inputs. Relative to white-collar

firms, blue-collar firms reduced blue-collar employment by up to 4 percent and gradually increased white-collar employment by up to 8 percent. The net effect, a relative reduction of total employment of around 2 percent, was primarily achieved by cutting permanent employment and partially offset by outsourcing workers through employment agencies.

Next, we examine the effect on capital. We find that higher firing costs led to a delayed increase in tangible fixed assets. However, the granularity of our data reveals that this increase is entirely driven by additional buildings and furniture, which we interpret as complementary investments to accommodate additional white-collar workers. Instead, we find no increases in the types of capital that are typically associated with the adoption of productivity-enhancing or labor-saving technologies, such as machinery and equipment, or intangible assets, and only limited evidence of increasing investments in human capital (training activities).

Finally, we assess the effect on output and TFP. We find that blue-collar firms on average experienced a significant 2 percent decline in value-added relative to white-collar firms after the legal reform. As this reduction may simply reflect changes in input use, such as reduced job flows, changes in workforce composition, or a lack of complementary investment in capital and training, we also look at TFP as this isolates the component of value-added that cannot be explained by observable inputs. This makes TFP a particularly relevant and informative outcome when firms adjust not only the quantity but also the quality and allocation of inputs. Efficiency-reducing frictions induced by firing costs may not be fully captured in employment or capital metrics alone, but are embedded in TFP. Thus, by examining firm-level TFP, we are able to assess more comprehensively the efficiency losses induced by increased firing costs.

We estimate firm-level TFP using the framework of [Akerberg et al. \(2015\)](#), and find that the reform led to an average drop in TFP of 4.8 percent among blue-collar firms, relative to white-collar firms.² The effect is already significant in the year of the reform announcement, remains significant over the subsequent four years, and grows gradually in magnitude over time. This result is robust to (i) using alternative methodological approaches to estimating firm-level productivity; (ii) considering value-added and revenue-based estimation procedures, as well as alternative production functions such as the translog; and (iii) allowing for time-varying input elasticities to account for possible changes in production technologies.

To help explain our empirical findings, we develop a simple theoretical framework and use it to discuss the different channels through which firing costs can affect firm-level TFP. We show how the relevant channels can be identified by linking observed changes in TFP to adjustments in input use and conclude that our findings are consistent with the following

²Our TFP estimation accounts for the heterogeneous impact of the reform on worker types by treating blue- and white-collar workers as separate production factors. We highlight the importance of separately accounting for worker types whose firing costs changed differently in production function estimation. We do so by showing how a bias can arise in estimating the causal effect of exogenous changes in firing costs on productivity when the estimated production function pools these worker types into a single employment input. See the discussion in Section 5.6.

interpretation: When firing costs increase, firms suffer substantial reductions in productivity because such costs prevent them from dismissing workers who are no longer productive in their jobs. The drop in measured TFP is large and persistent because, rather than offsetting it through improving worker selection, training, or productivity-enhancing investments, firms focus on adjusting their production processes to increase employment flexibility and minimize firing costs.

How do our results compare to [Autor et al. \(2007\)](#) and [Cingano et al. \(2016\)](#), who also study quasi-natural experiments on the firm-level effects of firing costs? First, we estimate a larger drop in firm-level TFP than these studies. Although it is difficult to quantitatively compare these findings given the different nature of the underlying policy reforms, we provide a simple calculation comparing implied costs. We conclude that our reform increased expected firing costs for the average treated firm by about 1.4 times more than the expected cost of introducing wrongful dismissal protection for small Italian firms in [Cingano et al. \(2016\)](#).³ Since the estimated impact of the Belgian reform on TFP is about 1.7 times larger than that of wrongful discharge protection in Italy, we conclude that the difference in the estimated effects is largely due to the previous papers analyzing less costly reforms and not due to differences in the elasticity of TFP with respect to different types of firing costs. Second, despite the significance of the Belgian reform, we do not find any evidence of capital deepening or capital-labor substitution. Although this is different from [Autor et al. \(2007\)](#) and [Cingano et al. \(2016\)](#), it aligns with cross-country evidence ([Janiak and Wasmer, 2014](#)) suggesting that one should not expect capital deepening in response to stricter EPL when EPL is already relatively high (as was the case in Belgium).

In the last part of the paper, we conduct an extensive set of robustness checks and additional analyses to corroborate our findings and their interpretation. Our results are robust to (i) alternative ways of identifying blue-collar firms (e.g., using the within-industry median share of blue-collar workers, or using the median share in 2008, i.e., before the start of our sample); (ii) alternative model specifications (e.g., using a matched sample or including controls and more granular fixed effects). We also perform a placebo test to further corroborate that our findings are indeed driven by the reform, rather than by (iii) mean reversion or (iv) omitted variables. Furthermore, we show that our results are stronger for (v) more affected industries and (vi) firms with worse credit access.

Finally, using a different, less granular dataset, we compare Belgian firms in *blue-collar industries* to French and German firms in the same industries, and repeat this for *white-collar industries*.⁴ Rather than comparing blue- to white-collar firms within Belgium, we compare

³We primarily focus on [Cingano et al. \(2016\)](#) in our discussion, since [Autor et al. \(2007\)](#) acknowledge potential limitations to their identification. However, their estimated impact of wrongful discharge protection on TFP in the US is very close to that in Italy.

⁴We do not have information on French and German firms' blue- and white-collar worker composition. Due to this data limitation, in this cross-country analysis, we define blue- and white-collar firms at the

Belgian blue-collar firms to non-Belgian blue-collar firms, and Belgian white-collar firms to non-Belgian white-collar firms. These comparisons allow us to separately assess the effects on blue- and white-collar workers and to ensure that unrelated events during our sample period (e.g., the sovereign debt crisis) did not confound our results by differentially affecting blue- and white-collar firms. We find that Belgian firms in blue-collar industries on average experienced a significant 2 percent decline in value-added relative to French and German firms in the same industries. For Belgian firms in white-collar industries, we do not observe a significant effect. These findings are consistent with the effects identified in the within-Belgium analysis and support the notion that the legal reform significantly increased firing costs for blue-collar workers while having an ambiguous effect on firing costs for white-collar workers.

Our results are important because they document unintended consequences of policies designed to improve job security for workers. We show that the gains of greater job security from increasing notice periods are unevenly distributed and uncertain, because firms offer fewer of the better-protected permanent contracts and increase employment flexibility through outsourcing. Furthermore, firms do not seem to shift towards technologies that increase workers' productivity, since there is no evidence of tangible or intangible capital deepening and little evidence of increased investment in human capital. In terms of policy implications, our analysis therefore emphasizes the importance of properly evaluating the trade-off between better-protected existing jobs and a more precarious situation for job seekers.

Related literature

Our study contributes to the literature linking firing costs to productivity. In this literature, some papers document a negative association by exploiting cross-country variation in employment protection indices (Micco and Pagés, 2006; Bassanini et al., 2009; Cingano et al., 2010). A natural concern with cross-country analyses is the comparability of labor laws across countries (Haltiwanger et al., 2014). Most closely related to our analysis, a few studies also conduct within-country analyses (Autor et al., 2007; Cingano et al., 2016).

A novelty of our paper is that we consider a reform that changed firing costs for all types of layoffs, including good-faith ones, and applied to all firms. 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). As such, the legal change induced plausibly exogenous and significant changes in firing costs across worker types. Additionally, as we analyze an event with a single treatment timing, our results should not be subject to a “bad comparisons” problem which

industry level and use value-added (instead of TFP) as the outcome, since we are not able to estimate elasticities separately for blue- and white-collar workers, which is crucial in our setting (see the discussion in Section 5.6).

might arise from staggered DiD designs.⁵ Finally, our analysis covers the entire firm-size distribution, rather than focusing only on firms around a specific size threshold, which strengthens the external validity of existing results. The Belgian reform is also exploited by [Alpysbayeva and Vanormelingen \(2022\)](#) to quantify the labor gap (i.e., the wedge between marginal product and marginal cost) induced by firing costs. Instead, we focus exclusively on the effect of the reform on firm-level productivity and document mechanisms through which the effect operates.

To identify the importance of the different channels highlighted in theoretical studies, we provide detailed evidence on a wide range of firms' adjustment margins. Consistent with existing studies, we document that firing costs lead to a reduction in both separations and hiring ([Kugler and Saint-Paul, 2004](#); [Kugler and Pica, 2008](#); [Marinescu, 2009](#); [Alpysbayeva and Vanormelingen, 2022](#)), supporting the view that such costs distort employment decisions. In addition, we show that affected firms shift toward production techniques that rely less on the types of workers whose dismissal has become more costly, leading to potential systematic changes in workforce composition. Furthermore, our evidence that firms seek greater flexibility, with an increase in employee outsourcing, is consistent with [Autor \(2003\)](#). We reinforce this evidence by adding that flexibility is also increased by reducing the hiring of permanent workers.

Finally, our finding that increased firing costs lead to no investments in capital-intensive technologies and only limited investment in human capital contributes to the literature on the effects of firing costs on (in)tangible capital investment ([Calcagnini et al., 2009, 2014](#); [Bena and Simintzi, 2019](#); [Bai et al., 2020](#); [Bena et al., 2022](#)) and human capital investment ([Acharya et al., 2014](#); [Griffith and Macartney, 2014](#)).

2. Theoretical framework

In this section, we discuss the dynamic effects of firing costs on firms' decisions and their impact on productivity. To relate this discussion to our empirical analysis, we consider a firm that produces output employing capital K , white-collar workers W , blue-collar workers B , and materials M . We define revenues as O and value-added as $VA = O - M$, and we assume that VA is characterized by a Cobb-Douglas function of capital and labor:

$$VA = z(\epsilon^z, F^W, F^B) K^\alpha \left[\theta^W (\epsilon^W, F^W) W^\gamma [\theta^B (\epsilon^B, F^B) B]^{1-\gamma} \right]^{1-\alpha}, \quad (1)$$

where z is Hicks-neutral productivity. The notation $z(\epsilon^z, F^W, F^B)$ indicates that z varies due to an exogenous productivity shock ϵ^z , but is also potentially affected by firing costs for white-

⁵See, for example, [Goodman-Bacon \(2021\)](#) and [Baker et al. \(2022\)](#) for discussions on difference-in-differences regressions with staggered treatment timing.

and blue-collar workers, F^W and F^B , respectively. Likewise, we allow labor-augmenting productivity, θ^W and θ^B , to depend on exogenous shocks and on the respective firing costs. We can rewrite the production function as follows:

$$VA = TFP \cdot K^\alpha [W^\gamma B^{1-\gamma}]^{1-\alpha}, \quad (2)$$

where $TFP \equiv z(\epsilon^z, F^W, F^B) \left[[\theta^W (\epsilon^W, F^W)]^\gamma [\theta^B (\epsilon^B, F^B)]^{1-\gamma} \right]^{1-\alpha}$

Firing costs dynamically affect both firing and hiring decisions and potentially alter the employment levels W and B , as well as indirectly the level of capital K , since the three inputs are complementary. We discuss the broader relationship between firing costs and input choices in Section 2.2. Furthermore, Equation (2) clarifies that changes in firing costs F^W and F^B might affect TFP by changing Hicks-neutral productivity z and labor-augmenting productivity θ^W and θ^B . In Section 2.1, we discuss the main channels proposed by the literature to rationalize this relation.

2.1. Firing costs and TFP

Firing costs mainly affect Hicks-neutral productivity z when they distort firms' productivity-improving investments, while they are more likely to affect labor-augmenting productivity θ by distorting separation, hiring, and training.

Firing costs and productivity-enhancing investments

The literature has highlighted that firing costs can have positive and negative effects on firms' innovation and productivity. On the positive side, firing costs might increase job security for innovative employees. Firing costs reduce the chance that firms will fire workers and appropriate the innovation benefits, which, in turn, increases innovative employees' efforts. [Acharya et al. \(2014\)](#) outline this theoretical argument and provide empirical support using patenting activity of innovative US firms following staggered state-level WDL adoption. [Griffith and Macartney \(2014\)](#) provide additional empirical evidence consistent with this channel. To the extent that these innovations are directed to improving the quality of the firm's products or the efficiency of its production processes, they will result in higher measured Hicks-neutral productivity z .

On the negative side, firing costs might reduce productivity by penalizing riskier and more productive projects. This argument has been put forward by several authors, and its intuition is straightforward: if productivity-enhancing projects with greater potential to increase z are also more likely to fail, they are disproportionately penalized by higher firing costs. This is because, conditional on failure, the firm faces higher costs for laying off the workers employed in these projects (see, among others, [Saint-Paul, 2002](#) and [Bartelsman](#)

et al., 2016, as well as Da-Rocha et al., 2023 on the relationship between policy distortions and establishment-level productivity investments).

These two channels, which have opposite effects on productivity, are both related to workers employed in innovative activities, and therefore are especially relevant for white-collar workers. Since our experiment implies large changes in firing costs for blue-collar workers, we expect the impact of these channels to be relatively small. Nonetheless, one way to identify them in the data would be to focus on the differential effect of the reform in more innovative industries, for which these channels are presumably stronger.

Firing costs, worker selection, and training

Cross-country evidence shows that better employment protection reduces job flows in and out of employment, and several authors have developed search and matching models to provide a theoretical explanation for this evidence. Here, we focus on those theories that link these flows to worker selection and productivity. Blanchard and Portugal (2001) consider a model with worker-job matches, in which wages are negotiated when the match is formed, workers' productivity varies stochastically, and the firm has to pay a firing cost to replace a worker in a given match. In this framework, higher firing costs reduce firing and hiring, as well as workers' productivity, because the minimum productivity threshold for dismissal falls.

While the channel suggested by Blanchard and Portugal (2001) focuses on the effects of firing restrictions, Pries and Rogerson (2005) propose an additional channel related to hiring. They assume that firms and workers observe a signal of worker quality before hiring, but that the true quality is only revealed after the match is formed. Low firing costs thus allow firms to be less selective ex-ante: they hire more workers and fire them more frequently and more quickly once their quality is revealed. Higher firing costs instead force firms to be more selective ex-ante, which reduces hiring and firing rates but increases average match quality (and hence worker productivity) in equilibrium.

Blanchard and Portugal (2001) and Pries and Rogerson (2005) thus propose two complementary channels that have similar implications for job flows but suggest opposing predictions for worker productivity. Both papers derive these effects in the context of matching models with single worker-job pairs. In Online Appendix D, we build a stylized two-period model and show that these channels are also present in a multi-worker-type production context that is more closely related to our empirical exercise. On the one hand, if blue-collar workers that have become less productive in their jobs—for example, because their skills are no longer well aligned with their job requirements—cannot be fired and replaced with more productive new hires, the composition of blue-collar employment B worsens, resulting in lower labor-augmenting productivity θ^B (i.e., the weighted average of workers' productivities). On the other hand, higher firing costs create additional screening incentives, which imply

higher values of θ^B . Therefore, the two channels generate opposite predictions for the effect of firing costs on firm-level TFP, as defined in Equation (2).

Another factor that can explain a positive relationship between firing costs and workers' productivity is human capital investment, such as training. Wasmer (2006) shows that higher firing costs, by increasing job duration, encourage human capital investment in firm-specific skills, which tend to raise productivity more than general skills. When we include training in our stylized model in Online Appendix D, we confirm that firing costs increase training, because its benefits increase if workers are expected to stay longer in the firm, and that this effect can increase worker productivity and TFP.⁶ Since we observe various measures of training intensity, we can test for the presence of the training channel in our data.

2.2. Firing costs and input choice

While we have so far focused on the effects of firing costs on firm-level TFP, a large literature has analyzed their effects on input choices. Building on Lazear (1990), Bentolila and Bertola (1990), and Hopenhayn and Rogerson (1993), firing costs have been shown to distort optimal hiring and firing decisions. Relative to a frictionless benchmark, firms reduce hiring ex-ante and firing ex-post. On the one hand, firms that receive a negative shock might lay off too few workers relative to the benchmark without firing costs, because retaining workers avoids paying high firing costs. On the other hand, following a positive shock, firms might hire too few workers to reduce future expected layoff costs. Although the overall effect on firms' employment levels is uncertain, such distortions unambiguously increase the dispersion in marginal products of labor across firms and reduce aggregate TFP. In our Belgian context, these theories predict that higher firing costs for blue-collar workers should increase the dispersion in marginal products of labor, especially among firms that use such workers more intensely. Relatedly, Alpysbayeva and Vanormelingen (2022) show that the Belgian labor market reform indeed increased the dispersion in the marginal productivity of labor for blue-collar firms relative to white-collar firms.

With respect to input choices in general, firms are likely to adjust their production processes over time to reduce their reliance on input factors that have become more expensive (e.g., León-Ledesma and Satchi, 2019). Thus, increased firing costs for blue-collar workers can be expected to reduce the ratio of blue-collar workers to white-collar workers. It is also possible that such costs cause a shift toward more capital-intensive processes to save on labor

⁶In our model, the net effect of the training channel on TFP is in principle ambiguous. Intuitively, firing costs increase training and productivity for marginal workers, i.e., those at risk of dismissal in the absence of firing costs. Some of these workers can become sufficiently productive so that training prevents their dismissal (conditional on a negative shock to their own productivity) in the presence of firing costs. Nonetheless, they might still be less productive than the workers who would otherwise have replaced them. Training is therefore ex-ante optimal, because it saves firing costs, but might, under certain parameterizations, reduce efficient worker replacement and thereby average productivity ex-post.

costs. However, Janiak and Wasmer (2014) note that the relationship between firing costs and capital deepening is complex. If these costs lead to a less productive workforce, the capital-to-labor ratio might decrease because the demand for capital per unit of labor decreases. In contrast, if firing costs increase human capital investment and worker productivity, they might induce capital deepening because capital and skilled labor are highly complementary.⁷

In summary, firing costs might alter the optimal composition between labor and capital, or between blue- and white-collar workers. Because of such compositional changes, it is important to separately include worker types in the production function when estimating firm-level TFP. In Online Appendix F.21, we show that, if firing costs alter the optimal ratio between worker types differently for blue- and white-collar firms, pooling worker types in a production function with total employment likely biases the estimated effects of firing costs on TFP.

3. The Harmonization of Notice Periods in Belgium

Belgian legislation defines blue-collar workers as those who mostly perform manual work and white-collar workers as those who mostly perform non-manual office jobs.⁸ White-collar workers have traditionally enjoyed better working conditions, but the Belgian Constitutional Court ruled in 1993 that this practice was discriminatory and unconstitutional.⁹ Following the ruling, employer and employee representatives 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 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 those for blue-collar workers. For blue-collar workers,

⁷Other studies emphasize that firing costs can reduce capital investment because they may aggravate the “hold-up” problem, which reduces firms’ willingness to invest (Caballero and Hammour, 1998). In addition, higher firing costs have also been shown to increase 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).

⁸The 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.

⁹Online Appendix B illustrates the main differences in working conditions between blue- and white-collar workers before the new legislation.

notice periods primarily depended on the industry and the seniority of the worker; for white-collar workers, it depended primarily on seniority and remuneration. Under the new law, notice periods for all workers depend exclusively on seniority.¹⁰ 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.¹¹

To illustrate the effect of the new legislation, Table 1 compares notice periods under the old and 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 (10 months), compared to 42 days (6 weeks) for blue-collar workers, for example, in the agriculture, textile, or transportation industries. If the same blue- and white-collar workers accumulated the entire 10 years of seniority under the new regime, their notice periods would be 210 days—independent of their type. For white-collar workers, this implies a reduction by one-third, while it more than quadruples the notice period for 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 using the old regime and assuming the worker was dismissed on 31 December 2013, and a second part using the new regime and assuming 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 €6,000.¹² An employer who lays off comparable workers in December 2023 will have to pay both worker types around €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%.¹³ Given that the median share of blue-collar workers is 65% 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

¹⁰See [Claeys and Engels \(2018\)](#) for a detailed summary of the Act of 26 December 2013.

¹¹Initially, increased notice periods did not apply to blue-collar workers performing 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.

¹²Note 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 $6000 \approx 40,000 * 1.3 * 42/365$.

¹³For 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.

unfair dismissals. We discuss the potential impact of these last two factors in more detail in Online Appendix C, but the impact of the first is worth mentioning here. 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 these trial periods thus increased firing costs, but especially for white-collar workers.

Although notice periods were harmonized for blue- and white-collar workers, certain legislative and institutional differences between the two worker types remained after the reform. Some important differences, as detailed in Online Appendix B, include the law’s notion of manual versus non-manual labor, contracts stipulating an hourly wage versus a monthly wage, weekly versus monthly paydays, employers’ social security contributions, and employers’ possibility to place workers under temporary unemployment (reducing firms’ employment costs and allowing workers to apply for unemployment benefits). Hence, it remained equally important for firms to offer workers the correct contract type to ensure compliance with labor laws. Furthermore, Belgian authorities continued to strictly monitor whether employers correctly classify workers based on their primary tasks (manual or non-manual).

Taken together, the Act unambiguously increased firing costs for blue-collar workers, whereas the overall effect for white-collar workers is uncertain because their reduced notice periods are (to some extent) offset by increased protection through other factors. The net effect of the new Belgian labor legislation has thus substantially increased the firing costs of blue-collar workers relative to white-collar workers.

4. Data and empirical model

4.1. Data sources

We obtain firm-year level data on Belgian firms from the Bel-first database provided by BvD (Bureau van Dijk, 2017). Bel-first provides detailed financial accounts as well as granular workforce data from 1994 to the present. Workforce data include information on the number of blue- and white-collar workers, the number of separations and new hires during the year, the number of employees with permanent or temporary contracts, the cost and number of employees who are outsourced through an employment agency (and thus are not included in firms’ total reported employment), total employment costs, the total number of hours worked, and training activities (including training participation, costs, and hours).¹⁴ Regarding

¹⁴In our data, for a small set of observations, the number of outsourced employees (around 20%) or training participation/hours (around 10%) was missing, whereas the corresponding cost variable was observed. We impute the missing values by dividing the observed cost by the 2-digit industry median cost per unit, computed from observations with non-missing quantities. The results, however, remain unchanged even without imputation.

the blue- and white-collar workers, we only observe their number in full-time equivalents (FTEs), which include both permanent and temporary contracts. The other employment outcomes—such as separations, new hires, contract types, outsourced employment, costs, or hours—are not reported at a disaggregated level for blue- and white-collar workers.

All firms that are incorporated under limited liability in Belgium are obliged to report information on their financial accounts and workforce, but obligatory reporting is more detailed for large firms. For instance, large firms have to report information on intermediate inputs, whereas other firms can provide this information on a voluntary basis. Note, however, that in Belgium a firm is legally considered to be “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. Since intermediate input data is crucial for our estimation of the production function, our main analysis keeps only the firms that provide it. 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 announcement of the new Belgian labor legislation (i.e., 2009-2012), the announcement year (i.e., 2013), and four years thereafter (i.e., 2014-2017). To the best of our knowledge, during our sample period, there was no other legal change in Belgium that differentially affected blue- and white-collar workers.¹⁵ BvD updates yearly 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, that is, just before the reform announcement year of 2013. Our production function estimation further requires that firms have at least one blue- and at least one white-collar worker in every year of the sample period (2009–2017). This is because we consider blue- and white-collar workers as distinct production inputs (see the discussion in Sections 5.6 and F.21 of the Online Appendix). Notwithstanding this, we show that our results remain robust to including firms with only one worker type (see Online Appendix Section F.11). 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).¹⁶

¹⁵During our sample period, employers benefited from reductions in social security contributions for low-wage workers, and Europe experienced a sovereign debt crisis to which it responded with several crisis-related support programs. In Section 5.6, we show that neither materially affected our results.

¹⁶Note that the new law initially exempted blue-collar workers working 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 only applied to the subset of blue-collar workers in these industries

Our regression sample requires that firms are observed in 2012 (before the reform announcement) and in 2014 (after the reform took effect). This sample restriction ensures that we can track changes in outcomes for the same firms before and after the policy change, thereby allowing us to isolate the effect of the reform on firm outcomes. Finally, we winsorize all ratios at their 1st and 99th percentiles. Applying these selection criteria implies a sample with 54,452 firm-year observations from 6,903 unique firms. We provide summary statistics for the entire sample in Table 2. Section F.13 in the Online Appendix reports pre-period sample means of the covariates for blue- and white-collar firms.

4.2. Empirical model and identification strategy

We exploit the legal change discussed in Section 3 to study the effects of firing costs. In particular, we estimate a difference-in-differences model where we compare blue-collar firms (firms that have an above-median pre-period share of blue-collar workers) to white-collar firms (firms that have a below-median pre-period share of blue-collar workers).¹⁷ 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:

$$Y_{i,t} = \beta \text{Blue-collar}_i * \text{Post}_t (+ \Pi \text{Firm controls}) + \mu_i + \nu_{st} + \varepsilon_{i,t} \quad (3)$$

where $Y_{i,t}$ denotes several outcomes we look into (e.g., employment, investment, value-added, revenue TFP) of firm i in year t . 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 labor demand confounds our treatment.¹⁸ Post_t is a dummy variable equal to 1 for observations in the period of 2013-2017, and to 0 for observations in the period of 2009-2012. The model also includes firm fixed effects (μ_i) and NACE 2-digit industry $\times$ year fixed effects (ν_{st}).¹⁹

To avoid the problem of “bad controls” (Angrist and Pischke, 2009), our baseline regressions exclude firm controls. Nevertheless, we show that our results are robust to their

(namely those in temporary and mobile workplaces), and therefore, firms operating in these industries were still affected (albeit presumably less). In Section 5.6, we show that firms in these industries—despite the partial exemptions—were indeed also adversely affected by the increase in notice periods.

¹⁷Note 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, and their share is on average around 1% of employment. Our results remain unchanged if we exclude those categories from the calculation of the median share of blue-collar workers used to identify blue-collar firms.

¹⁸In our robustness tests, we show that our results hold when we estimate Equation (3) by defining the blue-collar dummy based on the firm's share of blue-collar workers at the end of 2012 (the year before the reform announcement) or 2008 (the year before the beginning of our sample period), or using a treatment intensity variable (the firm's pre-period average share of blue-collar workers).

¹⁹Note that Blue-collar_i and Post_t are not separately included in Equation (3) because they are subsumed by fixed effects.

inclusion. *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-labor* (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).

We cluster standard errors at the firm level. Alternatively, one may also wish to cluster standard errors at the industry level to allow for the correlation of shocks within an industry, as we construct TFP by separately estimating the production function for each industry (see Section 5.2.1 for details). We do not choose this as the benchmark because our main sample contains only 50 industries. Nevertheless, 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 reform on firms’ input choices and TFP. Specifically, β reflects the difference in the evolution of outcomes in blue-collar firms relative to white-collar firms, from before to after the reform.

Endogeneity concerns. One concern for our identification strategy could be that the introduction of the new labor legislation could be related to pre-period firm characteristics. However, this concern is likely not relevant because the introduction of the new notice periods was based on the court’s decision to remove discrepancies between blue- and white-collar workers, rather than on firm performance.

Another concern could 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. However, the announcement of the deadline is unlikely to invalidate our results for two reasons. First, the actual agreement was reached just a few days before the deadline, suggesting that the exact terms of the agreement were not clear beforehand. Second, in Figures 2, 3, and 4, we run a dynamic version of Equation (3) and show that the effects of the new notice periods materialized after but not before the announcement of the legal reform. This further corroborates the assumption that the terms of the reform were not anticipated.

During our sample period, employers benefited from reductions in social security contributions for low-wage workers. This could raise the concern that our results might be confounded by this policy, given that blue-collar workers typically earn lower wages than white-collar workers. In our robustness tests (Section 5.6), we compare firms with lower and higher average employee costs and find that the overall results are not different across these groups, suggesting that employer benefits targeting low-wage workers did not materially influence our findings.

Additionally, our pre-period ends in 2012, when the European Central Bank started

implementing financial support programs, such as the Outright Monetary Transactions program, 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 6.3 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 had. The corresponding results corroborate our main findings and lead us to conclude that crisis support measures are not likely to contaminate our results.

Our sample period also coincides with a phase of deindustrialization in the Belgian economy, characterized by a shift in employment from manufacturing to services (Bodart et al., 2018; International Monetary Fund, 2018). Figure E.1 in the Online Appendix indeed shows a declining trend in the *aggregate* share of blue-collar workers during our sample period. Yet, the decline is rather modest, as the fraction of blue-collar workers declines by only 2.8 percentage points from 2009 to 2017. In our benchmark analysis, we account for this trend by using NACE 2-digit industry $\times$ year fixed effects, which absorb any variation common to all firms within the same industry and year. In our robustness tests (Section 5.6), we also show that our results remain unchanged when we use 4-digit industry $\times$ year fixed effects to account for time-varying industrial trends at an even more granular level. Furthermore, we show that our results do not differ significantly across industries that experienced greater declines in their share of blue-collar workers during the pre-reform period.

A last concern might be that some unobserved factors bias our estimates. In our main model, firm fixed effects already control for time-invariant firm characteristics. However, controlling for time-varying unobserved factors (e.g., changes in business models) might also be crucial. Just as we control for time-varying observed and unobserved heterogeneity at the industry level using industry $\times$ year fixed effects in our baseline model, we also perform two additional robustness checks (Section 5.6): (i) we interact industry $\times$ year fixed effects with province dummies to control for time-varying factors within each industry-location-year, and (ii) we perform a matching procedure—alongside controlling for key time-varying firm characteristics—to ensure that blue- and white-collar firms in our sample are as similar as possible in their observed pre-period characteristics, which can also help reduce bias from unobserved factors (Angrist and Krueger, 1999).

5. Results

In this section, we present our results. We begin by investigating the effect of firing costs on inputs and output. Then, we examine the effect on productivity.

5.1. The effect on inputs and output

5.1.1. The effect on employment

We first investigate how firms adjusted their workforce in response to increased firing costs after the reform, starting from the effect on separations and hiring. As a first check, we plot the average separation and hiring rates for blue- and white-collar firms before and after the reform in Figure 1. Before the reform, separation and hiring rates for both groups moved in parallel. After the introduction of the reform, both rates declined, but the decline was stronger for blue-collar firms, such that the differences between the two groups started to widen. The divergence in hiring rates between blue-collar and white-collar firms emerged already in 2013, whereas the divergence in separation rates became visible from 2014 onward. By the end of 2017, both separation and hiring rates among blue-collar firms were still substantially lower than those of white-collar firms. Taken together, this unconditional evidence suggests that increased firing costs reduced both separations and hiring, in line with theoretical predictions.

We now turn to our regression analysis. Table 3 shows, in columns (1) and (2), the results from the estimation of Equation (3), where the outcomes are the natural logs of the number of employees who left the firm during the year and those hired during the year, respectively.²⁰ The estimates indicate that an increase in firing costs induces firms to fire *and* hire less, consistent with our unconditional evidence and in line with existing empirical findings (Kugler and Saint-Paul, 2004; Kugler and Pica, 2008; Marinescu, 2009; Alpysbayeva and Vanormelingen, 2022). As shown by the dynamic estimates in panels (a) and (b) of Figure 2, both separations and hiring declined immediately following the reform announcement. Specifically, separations reached a peak decline of 9 percent two years after the announcement, and the decline remained significant in subsequent years, whereas hiring peaked at a reduction of 8 percent just one year after the announcement. Overall, these results suggest that higher firing costs may distort firms' optimal firing and hiring decisions.

We next analyze the effect on workforce composition. Columns (3) and (4) of Table 3 show that blue-collar firms experienced a decline in blue-collar employment of 1.7 percent and an increase in white-collar employment of 5.2 percent relative to white-collar firms, with these effects being statistically significant at the 10% and 1% confidence levels, respectively. The dynamic estimates shown in panels (c) and (d) of Figure 2 illustrate that these changes increased gradually over time, especially for white-collar employment. Thus, these findings suggest that blue-collar firms gradually shifted toward production arrangements that rely less on blue-collar workers and more on white-collar workers. Furthermore, as can be seen in

²⁰In Section F.2 of the Online Appendix, we show that our results are robust to alternative outcome definitions that retain firm-year observations with zero separations (7.2% of the main sample) or zero new hires (9.3% of the sample). Specifically, we consider $\ln(1 + \text{separations})$, $\ln(1 + \text{entering emp.})$, separations divided by 2012 employment, and new hires divided by 2012 employment.

Column (5) of Table 3, total employment at blue-collar firms declined by 2.2 percent relative to white-collar firms after the reform.

What do these results imply for the average blue- and white-collar firm in our sample? The average blue-collar firm reduced its reliance on blue-collar workers by 3.7 percent (from 60 to 57.8 FTEs), whereas the average white-collar firm reduced its reliance on blue-collar workers by only 2 percent (from 27 to 26.5 FTEs). At the same time, the average blue-collar firm increased its reliance on white-collar workers by 8.4 percent (from 15 to 16.3 FTEs), whereas the average white-collar firm increased its reliance on white-collar workers by only 3.2 percent (from 37 to 38.2 FTEs). As a result, total employment in blue-collar firms fell by 1.2 percent (from 75 to 74.1 FTEs), while it increased in white-collar firms by 1 percent (from 64 to 64.6 FTEs).

In addition to the effect on the number of employees, we also assess the impact on labor costs by using the natural log of the average yearly cost per employee in column (6) of Table 3. We do not find a significant difference in the effect on average employee costs between blue- and white-collar firms. This finding is consistent with closely related studies, which also report no significant increases in wage bills among firms experiencing higher firing costs, neither following the 1990 Italian reform (Leonardi and Pica, 2013) nor after the US wrongful discharge laws (Autor et al., 2006). There are two potential reasons why average employee costs did not increase even though blue-collar firms increased their reliance on white-collar workers (who on average across the entire worker-population earn higher salaries than blue-collar workers). First, white-collar workers represented only 20% of the average blue-collar firm’s total employment pre-reform. While a 5.2% relative increase may appear substantial, it may be too modest in absolute numbers to significantly drive average employment costs. Second, one possibility is that newly hired workers may have been relatively young. If this is the case, then salaries of white-collar workers are not necessarily higher than those of blue-collar workers. Indeed, until the age of 30, blue- and white-collar workers in Belgium earn roughly the same on average; and below 25, blue-collar workers even earn on average higher salaries than white-collar workers (Statbel, 2024).²¹ In column (7) of Table 3, we additionally examine the effect of the reform on the average yearly number of hours worked per employee, and the estimated effect is also not significant. On the whole, these results suggest that increased firing costs did not significantly affect wages or labor intensity.

Another question we address is whether increased firing costs induce firms to shift toward more flexible employment arrangements. Evidence on this is presented in Table 4. While the total employment level declined following the reform, column (1) illustrates that the number of workers outsourced through employment agencies (which are not included in firms’ measure

²¹We mention this channel as a plausible explanation. However, as noted in Section 4.1, we cannot provide direct evidence on this because our data do not allow us to observe the type or age composition of new hires.

of total employment) increased by 5.4 percent among blue- relative to white-collar firms. Panel (f) of Figure 2 shows that the number of outsourced employees increased gradually over time. This result, which is in line with Autor (2003), indicates that firms tried to increase labor flexibility using external contracting, to compensate for the reduced flexibility of blue-collar workers. Based on our estimates, conditional on using outsourced workers, the average blue-collar firm increased its number of outsourced employees by 14.9 percent (from 8.5 to 9.8 FTEs), whereas the average white-collar firm increased its number of outsourced employees by 9.5 percent (from 7.3 to 8 FTEs).

Firms can also increase employment flexibility by changing the types of contracts they offer. Our data allow us to observe the number of permanent and temporary employees at the end of each fiscal year, as well as—for large firms—how many of the separating and newly hired workers have permanent or temporary contracts. The results, as shown in columns (2) and (3) of Table 4, indicate that increased firing costs led to a decline in the number of permanent contracts and an increase in the number of temporary contracts. While the estimated impact on permanent contracts is significant and immediate following the reform announcement (panel (g) of Figure 2), we find no clear evidence of adjustments on the temporary contract margin: the dynamic estimates in panel (h) of Figure 2 are largely indistinguishable from a modest upward trend. Moreover, columns (4)-(7) of Table 4 demonstrate that increased firing costs significantly reduced the turnover of employees with permanent contracts (both separations and hiring), while the turnover of employees with temporary contracts remained largely unaffected. Taken together, the evidence suggests that firms adversely affected by firing costs avoided permanent contracts (i.e., rigid employment arrangements) and gradually resorted relatively more to temporary contracts (i.e., flexible employment arrangements).

5.1.2. The effect on physical & human capital

The effect on physical capital. Next, we investigate whether the legal reform caused blue-collar firms to adopt different and possibly more capital-intensive technologies. Table 5, in columns (1)-(4), presents the results from the estimation of Equation (3) using dependent variables related to firm capital. In column (1), the dependent variable is the natural log of tangible fixed assets. The estimated effect is positive but not statistically significant. However, the dynamic estimates shown in panel (a) of Figure 3 indicate that the effect becomes significant two years after the start of the reform. Specifically, blue-collar firms raised their capital stock significantly by approximately 5 percent in 2016 and 9 percent in 2017 relative to the pre-reform period and compared 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-labor (blue-collar labor, in our case) substitution, we break

down tangible fixed assets into two categories to delve deeper into whether and how firms changed their production technology. Specifically, we distinguish between the natural log of the sum of machinery and equipment in column (2), and the natural log of the sum of land, buildings, furniture, and other tangible fixed assets in column (3). While the estimated impact is larger for the latter category, the effects are not statistically significant in either column. Nevertheless, the dynamic estimates in panels (b) and (c) of Figure 3 show a significant positive effect only for land, buildings, furniture, and other capital in 2016 and 2017, mirroring the pattern observed for total tangible fixed assets. This indicates that the observed increase in firms' total capital stock stems from investment in non-machinery components, which is perhaps related to the increase in white-collar workers rather than to a transition toward capital-intensive, productivity-enhancing technologies. In addition, column (4)—with the dynamic estimates illustrated in panel (d) of Figure 3—shows that the effect on intangible fixed assets is not significant, further corroborating the absence of technology adoption.

The effect on human capital. Besides the effect on physical capital, we also investigate the effect of firing costs on human capital, using information on firms' training activities. Column (5) of Table 5 shows that the reform significantly increased the share of workers involved in training activities by 2.2 percent. Panel (e) of Figure 3 indicates that this effect is largest in 2013 and does not appear to increase over time. Moreover, columns (6) and (7) show no significant effects on the intensive margins of training, measured by the ratio of total annual training costs to total employment costs, and the ratio of total annual training hours to total employment hours, respectively. The corresponding dynamic estimates are reported in Online Appendix Table F.3, and are also not significant. Overall, these results suggest that the reform had positive but relatively small effects on human capital accumulation.

5.1.3. The effect on output

To assess the effect on output, we estimate Equation (3) using the natural log of value-added as the outcome. Column (8) of Table 5 illustrates that blue-collar firms on average experienced a 2 percent decline in value-added relative to white-collar firms. As shown in panel (f) of Figure 3, the effect was persistent and decreased further to 3 percent by 2017. This result, together with the earlier evidence—which did not show an overall decline in inputs caused by the reform (while blue-collar employment declined, other inputs such as white-collar employment and structures increased)—is consistent with the reform having broadly negative effects on firms' efficiency and emphasizes the importance of studying its effects on firm productivity, which is the objective of the next section.

5.2. The effect on productivity

In this section, we now examine the impact of firing costs on firm-level TFP. We first describe how we estimate TFP and then present our results.

5.2.1. TFP estimation

Taking logs of Equation (2), we recover firm-level revenue TFP as the residual from the estimation of a Cobb-Douglas production function:^{22,23}

$$\log TFP_{i,s,t} = \log VA_{i,s,t} - \hat{\alpha}_s \log K_{i,s,t} - \hat{\lambda}_s^W \log W_{i,s,t} - \hat{\lambda}_s^B \log B_{i,s,t} \quad (4)$$

where i , s , and t are indices for firm, 2-digit NACE industry, and year, respectively. In our benchmark estimation, we use value-added as the measure of output. However, we show that our results are robust to using revenues as output. We separately include the firm’s number of blue- and white-collar employees as labor inputs. This allows us to capture the possible compositional effects of the regulation and—as we discuss later in Section 5.6—to obtain an unbiased effect of firing costs on productivity. Lastly, we use tangible fixed assets as a measure of capital stock.²⁴

We recover the input elasticities by estimating the production function separately for each 2-digit NACE industry.²⁵ 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. [Akerberg et al. \(2015\)](#) then criticized the identification of the labor coefficient in the model of [Levinsohn and Petrin \(2003\)](#) and proposed an alternative semi-parametric estimation. We rely on the estimation strategy of [Akerberg et al. \(2015\)](#) for our benchmark specification and use intermediate inputs (including both intermediate goods

²²In Equation (4), λ^W and λ^B are functions of γ and α from Equation (2). Specifically, $\lambda^W = \gamma(1 - \alpha)$ and $\lambda^B = (1 - \gamma)(1 - \alpha)$.

²³We 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.

²⁴In [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 choose to use tangible fixed assets to ensure comparability with various existing papers studying Belgian data (e.g., [De Loecker et al., 2018](#)). Furthermore, in unreported tests, following [Autor et al. \(2007\)](#), we also estimate the production function by including capital items separately as machinery and non-machinery capital (as in columns (2) and (3) of Table 5), rather than using total capital stock. When we use this alternative TFP measure as the dependent variable, the estimated effect is nearly identical to that reported in Table 6.

²⁵After estimating elasticities, we drop negative ones. This reduces the number of industries in our sample from 58 to 50, corresponding to 1,445 firm-year observations. Nonetheless, our results remain unchanged if we retain these observations.

and services), deflated by 2-digit industry-level price indices, as a proxy. Our measure of intermediate inputs includes the cost of outsourced workers. In Section 5.6, we treat outsourced workers as part of the labor input in our production function and show that the results remain robust. In other robustness tests, we estimate the production function using a one-step generalized method of moments procedure proposed by Wooldridge (2009), which also addresses the critique of Akerberg et al. (2015) and estimates all coefficients efficiently.

While labor is measured in full-time equivalents in Equation (4), capital and output are measured using 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 the OECD Structural Analysis Database (OECD, 2017). 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, market power is likely only a limited concern in our sample: although most firms are large according to the Belgian classification, they are relatively small by international standards (the median number of employees is 36). Second, we study the implications of an exogenous shock, the increase in adjustment costs for blue-collar labor, which directly affects the efficiency of the firm, rather than its ability to set prices. Third, if firms react to a drop in production efficiency (caused by increased labor adjustment costs) by raising prices, we would pick this up as an increase 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 (see below), our results should be interpreted as a lower bound if firms have market power.

5.2.2. Main results

To assess the effect of firing costs on productivity, we estimate Equation (3) using the natural log of TFP as the dependent variable. Column (1) of Table 6 presents the results including neither control variables nor fixed effects. The coefficient of interest on the interaction term indicates a 1.9 percent decline in TFP and is significant at the 5% confidence level. In column (2), the inclusion of firm fixed effects slightly increases the magnitude of the effect to 2 percent and its significance to 1%, suggesting that our results do not depend on time-invariant firm heterogeneity. In column (3), when we use only NACE 2-digit industry $\times$ year fixed effects, the estimated effect substantially increases to a 4.5 percent decline. This indicates that our results are stronger when controlling for confounding time-varying industry-level factors. Column (4) presents the results from an estimation that includes both firm and NACE 2-digit industry $\times$ year fixed effects, and the coefficient of interest signals a 4.8 percent decline in TFP. Column (5) additionally controls for some key firm characteristics,

and the estimated coefficient remains in the same ballpark, showing a 4.5 percent decline and remaining significant at the 1% confidence level.

Based on the estimate in column (4), which we take as our baseline, firms with an above-median share of blue-collar workers during the pre-period (2009-2012) experienced a 4.8 percent decline in TFP after the reform, relative to firms with a below-median ex-ante share of blue-collar workers.

Figure 4 illustrates the estimates from the dynamic version of column (4). As can be seen from the insignificant coefficients before 2013, the TFP of blue-collar and white-collar firms evolved similarly before the labor market reform. However, after the announcement of the reform, the TFP of blue-collar firms declined significantly relative to that of white-collar firms. In the first years following the announcement, the relative decline in TFP was about 4 percent and then gradually increased to about 6 percent by 2017. These dynamic estimates help address concerns about pre-trends or anticipation effects. That is, they show that the effect of the reform materialized after, but not before, the announcement. We further perform a Lagrange Multiplier test which does not reject the null hypothesis of parallel trends between blue- and white-collar firms before the reform (with a p -value of 0.18) and also a Granger causality test which fails to reject the null hypothesis of no anticipatory behavior (with a p -value of 0.24), providing additional support for the exogeneity of the reform. Taken together, the results suggest that firing costs have persistent, negative effects on firm productivity.

5.2.3. Robustness to different productivity measures and treatment definition

In Online Appendix F.4, we explain and show that the results in Table 6 are robust to several alternative approaches for estimating TFP: (i) accounting for firms' survival probabilities;²⁶ (ii) applying the one-step estimation procedure proposed by Wooldridge (2009); (iii) employing a translog production function; (iv) estimating the production function separately for the pre- and post-reform periods; (v) estimating it separately for the pre- and post-reform periods as well as for blue- and white-collar firms; (vi) conducting year-by-year OLS estimations; (vii) using revenues as output and materials as inputs; and (viii) using (imputed) labor hours instead of the number of employees as the labor input. Across all specifications, the estimated effect on TFP remains consistently negative and statistically significant, and the dynamic estimates support the assumption of ex-ante parallel trends.

Furthermore, in our baseline model, we define treatment based on firms' blue-collar worker share over the pre-reform period (2009-2012). This could raise concerns about mean reversion when using firms' blue- and white-collar employment levels or TFP as outcome variables. Specifically, if blue-collar firms had unusually high levels of blue-collar employment

²⁶In Online Appendix F.22 (and as discussed in Section 6.1), we show that the Belgian reform led to an increase in firm exits.

pre-reform, they might revert to their long-term average share, and our estimated effects might reflect reversion to the mean rather than the impact of the reform. To address this, we perform two analyses.

First, we conduct a placebo test using data from 2005 to 2012 in which we pretend that the reform was announced in 2008. In this test, the pre-period is defined as 2005–2007 and the post-period as 2008–2012. We also redefine the blue-collar dummy: it is equal to 1 if a firm’s average share of blue-collar workers during the placebo pre-period (2005–2007) was above the sample median, and to 0 otherwise. If mean reversion drives the baseline results, then we should find similar effects in this placebo test. If the legal reform drives our baseline results, then this placebo test should yield insignificant results. As can be seen in Online Appendix Table F.5, the estimated effects on TFP and any of the employment outcomes in this placebo test are all close to zero and statistically insignificant. Second, we conduct a test using the baseline sample from 2009 to 2017, but we redefine the blue-collar dummy based on firms’ blue-collar share in 2008 (i.e., one year prior to the start of our sample period). If mean reversion drives our results, it should now already happen during the pre-period and hence weaken or even reverse the baseline findings. Nevertheless, Online Appendix Table F.6 shows that the estimated effects on TFP and employment outcomes remain robust when treatment is defined using 2008.

Our results are thus robust to alternative approaches for estimating TFP and are not driven by mean reversion, strengthening the credibility of a causal interpretation of the reform’s effect.

5.3. The effect on profits

Having shown that the reform reduced both productivity and output, a natural question is whether it also affected firm profitability. We examine this by estimating our baseline model (3) using $\ln(\text{EBITDA})$, EBITDA-to-assets, and EBITDA-to-sales as dependent variables. As shown in Table F.7 in the Online Appendix, the estimated DiD coefficients are small, negative, and statistically insignificant across all specifications, suggesting that the labor reform had no significant effect on profits. One possible explanation is that the decline in separations and hiring (columns (1) and (2) of Table 3) reduced hiring and firing costs, which may have helped blue-collar firms sustain profitability despite declining TFP. In addition, the evidence that total employment declined while the average cost per employee remained unaffected (columns (5) and (6) of Table 3) suggests a reduction in the total wage bill of blue-collar firms, which may also have contributed to preserving profitability.

5.4. More versus less affected industries

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, we should observe stronger effects in industries where notice periods for blue-collar workers increased more. We thus re-run our main model (3) for two separate groups: (i) industries where the notice period for a blue-collar worker with 10-year tenure increased by less than 168 days (“less affected industries”), and (ii) industries where the notice period for a similar blue-collar worker increased by 168 days or more (“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 industries.²⁷ Table F.8 confirms that our main results are generally stronger for more affected industries, which show a larger drop in TFP, a larger decline in separations, and a stronger increase in the white-collar to blue-collar ratio, relative to less affected industries.

5.5. Discussion of the results

5.5.1. Relation with the theory

The empirical analysis described above yields several insights on the relationship between firing costs, input choices, and productivity.

First, we find strong evidence that higher firing costs for blue-collar firms reduced both separations and hiring. Our stylized model supports a causal link from this reduction in separations and hiring to a decline in TFP because firing costs prevent the efficient separation of unproductive workers (Blanchard and Portugal, 2001). One interesting observation is that the decline in TFP materializes rapidly after the announcement of the labor reform. Several explanations could contribute to this. First, the share of workers who become unproductive each period may not be small, such that, when firing costs increase, they prevent the firm from firing a non-negligible share of workers. The fact that separation and hiring rates are high in our sample (i.e., around 25%; see Figure 1) indicates that this is plausible. Second, if hiring new workers declines because unproductive workers are not being fired and replaced by more productive ones, then workers’ productivity might decline across the whole distribution and not only around the firing threshold (i.e., the productivity threshold below which workers are dismissed). Therefore, the sharp drop in hiring by blue-collar firms at the beginning of the reform might have exacerbated the initial drop in TFP. However, we cannot rule out that other factors also contribute to explaining why the decline in TFP might be large immediately after the reform and become only modestly larger thereafter. One additional explanation is that firms are found to gradually increase other margins of worker flexibility, such as outsourcing, which might have mitigated the negative impact of firing costs on productivity

²⁷The sample split is identical when we choose the threshold based on notice period increases for blue-collar workers with 1- or 5-year tenure.

over time.

Second, we find strong evidence that firms gradually shift their production inputs, using less of the production factor that became more expensive (blue-collar workers) because of higher future expected firing costs. This effect is reflected in a gradual and substantial decline in the blue- to white-collar ratio among blue-collar firms relative to white-collar firms following the reform. Interestingly, these modifications are also reflected in a clear shift toward more flexible arrangements, with blue-collar firms hiring fewer workers under permanent contracts and increasing their reliance on outsourced labor. Dynamically, these trends do not seem to reverse the negative effect of the reform on TFP, since the productivity decline remained large until the end of our sample period (see Figure 4). This reinforces the conclusion that the observed changes in inputs are adjustments to minimize production costs, rather than more radical changes to shift toward innovative and more productive technologies (e.g., [Acharya et al., 2014](#)).

Third, supporting this conclusion, we find no evidence that higher firing costs led firms to increase capital intensity. Firms did not raise their investment in assets associated with technology adoption, such as machinery, equipment, or intangible assets. One possible explanation is that this productivity-enhancing channel operates through workers employed in innovative activities ([Acharya et al., 2014](#)) and is therefore especially relevant for white-collar workers, who did not experience an increase in firing costs. As an additional test, in Table F.9 in the Online Appendix, we show that the results are not significantly different between less and more R&D-intensive industries, while we would expect the drop in TFP to be smaller for the latter industries if this channel was important in our data.

Fourth, our results do not strongly support the prediction that higher firing costs increase human capital investment ([Wasmer, 2006](#)). We observe a small but temporary increase in the share of workers trained, but no increase in the intensity of training.

Overall, our findings paint a negative picture of firing costs, as they are broadly consistent with the following narrative: when firing costs increase, firms suffer substantial reductions in productivity because they are less able to fire workers who are no longer productive in their jobs. Rather than offsetting this drop in productivity through improving worker selection, training, and productivity-enhancing investments, firms focus on adjusting their production processes to minimize firing costs. They rely less on blue-collar workers and more on flexible production arrangements based on fewer permanent workers and more outsourcing. However, these adjustments, especially when not coupled with more human capital investments, are likely to broadly undermine the job stability of workers employed in blue-collar jobs, and therefore partially offset the intended objectives of higher firing costs.

5.5.2. Relation with other empirical studies

We have documented that higher firing costs, induced by longer notice periods, cause a large reduction in firm-level TFP. To our knowledge, there are no comparable estimates in the literature that exploit increases in notice periods. However, we can compare our findings to the effects of other forms of EPL on TFP. Closely related to our study, [Cingano et al. \(2016\)](#) find a 2.9 percent reduction in TFP among small firms—those affected by the introduction of severance payments for unfair dismissal in Italy—relative to large firms. Focusing on similar specifications with industry-year fixed effects, we find that TFP declines by 4.8 percent in blue-collar firms relative to white-collar firms, suggesting a 1.7 times larger coefficient.

The larger impact could be due to differences in the costs of the reforms or due to differences in the elasticities of TFP with respect to different firing costs. Since, prior to the reform, protection against unfair dismissal was essentially non-existent for workers in small Italian firms, and because we do not observe workers' seniority, we cannot calculate and compare elasticities in a meaningful way. Instead, we can compare expected costs, expressed in months of pay, by focusing on workers with 10-year tenure. For such workers, the Italian reform raised severance pay from 0 to up to 10 months ([Del Punta, 2022](#)).²⁸ In Belgium, notice periods (and thus payments in lieu of notice) increased by 168 days (8.4 months) for most blue-collar workers with 10-year tenure (Table 1). The cost of wrongful dismissal (in Italy) is therefore larger than the cost of extended notice periods (in Belgium). However, firms should not expect having to pay these costs with the same probability. Wrongful discharge protection only applies to layoffs that can be challenged successfully in court. In contrast, extended notice periods also apply to other job separations, including good-faith layoffs (e.g., for economic reasons). During collective layoffs, for example, wrongful discharge protection only plays a minor role, whereas extended notice periods benefit most blue-collar workers, e.g., via union negotiations with the employer.

Back-of-the-envelope calculations for Italy suggest that the cost of wrongful discharge protection arises with an annual probability of 1.3%, while evidence from Belgium indicates that the annual probability of an involuntary contract termination is 5.6%.²⁹ The average

²⁸The literature, including [Cingano et al. \(2016\)](#), often mentions that severance pay increased by 2.5-6 months for most workers. However, our comparison focuses on workers with 10 years of tenure, who can receive higher severance pay ([Del Punta, 2022](#)). In addition, [Garibaldi and Violante \(2005\)](#) suggest that, for blue-collar workers in larger Italian firms, the *de facto* dismissal cost (including social security contributions, legal costs, etc.) is 1.3 times larger than the granted severance pay. Even if judges do not always grant 10 months, the *de facto* cost should thus be close to it.

²⁹In Italy, the separation rate in small firms was 26.4% ([Kugler and Pica, 2008](#)), and 9.4% of separations tend to be dismissals ([Jin et al., 2016](#)). In addition, if workers bring cases to court, they have a 51% chance of winning ([Bertola et al., 2000](#)). We use the product of these probabilities as our estimate of the likelihood that wrongful discharge costs apply. In practice, not all dismissals are wrongful, and not all dismissed workers challenge their dismissal in court. At the same time, success rates in court are likely higher conditional on a dismissal being wrongful, and costs also accrue in out-of-court settlements. We are not aware of reliable evidence disentangling these different contingencies and thus work with the simple proxy. The data for

expected cost of the Italian reform is thus $1.3\% \times 10$ months, while the expected cost of the Belgian reform is $5.6\% \times 8.4$ months. Since firms in our treatment group have a roughly 40% higher blue-collar share than firms in our control group (Online Appendix Table F.13.1), the increase in the expected cost for the average treated firm would be $40\% \times 5.6\% \times 8.4$, which is approximately 1.4 times larger than the cost for small Italian firms. Our simple cost comparison is thus broadly consistent with the 1.7 times stronger estimated impact of EPL on TFP, suggesting that the smaller effect in Cingano et al. (2016) is primarily the result of a more marginal policy change, not of different elasticities.

Although the magnitude of our estimated effect on TFP is thus in line with the literature, Cingano et al. (2016) and Autor et al. (2007) differ from our analysis with respect to their conclusions on capital deepening. Both papers find that wrongful discharge protection leads to capital deepening, i.e., to more capital-intensive production.³⁰ In line with their results, we observe a (weak) increase in tangible fixed assets at the end of our sample period, but find that it is driven by land, buildings, and furniture, rather than machinery and equipment. Because we primarily associate the latter with a channel in which firing costs induce productivity-enhancing capital investment, we interpret our results as not showing evidence of capital deepening. As mentioned in Section 5.5.1, this finding is consistent with the broad picture implied by our results on firing costs not stimulating productivity-enhancing investment.

Furthermore, the absence of capital deepening following the Belgian labor market reform is also consistent with the broader literature, which suggests that the relationship between EPL and capital intensity is characterized by an inverse U-shape. For low initial levels of employment protection (like in the US, or for small Italian firms prior to 1990), additional protection is associated with capital deepening, while additional protection when original EPL is already high (as in many European countries) appears to lead to reduced capital intensity (Calcagnini et al., 2009; Cingano et al., 2010). Janiak and Wasmer (2014) document the inverse U-shaped relationship between EPL and capital intensity across countries and show that Belgium is located near the flat top of the curve, where one would not expect an additional increase in EPL to induce capital deepening. That we do not find such evidence is therefore consistent with the existing cross-country evidence.

5.6. Further robustness tests

We perform various tests to ensure the robustness of our main findings. We describe these tests here, while the full results are presented in the Online Appendix.

Treating outsourced employees as input in the construction of TFP. In our

Belgium is from a Human Resources and Payment Service provider: <https://press.securex.be/le-nombre-de-licenciements-atteint-son-plus-haut-niveau-depuis-2020>; last accessed: 19.06.2025

³⁰Capital comprises machinery and structures in Autor et al. (2007), and fixed assets in Cingano et al. (2016).

dataset, outsourced workers are not included in firms' recorded total employment. However, when estimating our production function, our measure of value-added is net of the cost of outsourced workers, and the cost of outsourced workers is included in our measure of intermediate inputs, which we use as a proxy following the framework of [Akerberg et al. \(2015\)](#). In Table [F.10](#), we treat outsourced workers as part of the labor input in our production function.³¹ Specifically, we add the cost of outsourced workers to value-added (i.e., the measure of output), subtract the cost of outsourced workers from intermediate inputs, and add the outsourced workers as a production input. Since we do not observe outsourced employment by worker type, we integrate outsourced workers into the employment input using three approaches: (i) allocating them proportionally based on the firm's blue- and white-collar employment share in a given year; (ii) assuming all outsourced workers are blue-collar, as manual tasks are more likely to be outsourced ([Eurofound, 2014](#)); and (iii) treating outsourced labor as a separate worker category alongside blue- and white-collar workers. Under each approach, the estimated impact is almost identical to our main result. When all outsourced workers are pooled into the blue-collar category (i.e., the second approach), the estimated effect is slightly smaller than our main estimate (-3.8% versus -4.8% in our benchmark results) but remains significant at the 1% confidence level.

Relaxing the sample selection criteria. Our production function estimation requires restricting the main sample to firms that employ at least one blue-collar worker, at least one white-collar worker, and for which data on intermediate inputs are available in every year of the sample. In Section [F.11](#), we show that our main results hold if we relax these selection criteria in three ways: (i) incorporating firms with only one worker type, (ii) incorporating firms with missing intermediate inputs while still employing both worker types, and (iii) incorporating both firms with only one worker type and those with missing intermediate inputs. Overall, our main results remain consistent across all three alternative samples.

Results with controls. Table [F.12](#) shows that our results for firm employment and capital remain robust when we add several key time-varying firm characteristics as controls to our baseline specification, which includes firm and industry×year fixed effects. The corresponding results for TFP with controls are reported in column (5) of Table [6](#).

Matched sample analysis. In addition to employing an extensive set of fixed effects and firm controls, we implement a matching procedure to ensure that blue- and white-collar firms are as comparable as possible in their observed characteristics during the pre-reform period, which can also help reduce potential bias from unobserved factors ([Angrist and Krueger, 1999](#)). In Section [F.13](#), we describe two different matching strategies and demonstrate that the results using matched samples are qualitatively similar to our main estimates.

³¹After estimating each production function, as in our benchmark analysis, we drop the resulting negative elasticities, leading to small variations in sample size across columns. However, our results remain unchanged if we retain them.

Treatment intensity. We also estimate our baseline Equation (3) with a treatment intensity variable instead of our baseline dummy. That is, we replace the blue-collar dummy with its continuous counterpart: the firm’s average pre-period share of blue-collar workers. As shown in Table F.14, the estimates based on treatment intensity remain qualitatively consistent with our main results.

Linear effects. Next, we test the linearity of the effects of the reform to ensure that our empirical model is well-specified. To do so, we estimate Equation (3) using quartiles of the treatment intensity variable rather than only the median (our blue-collar dummy). Excluding the first quartile as a reference category, we find that the estimated effects are overall larger in the upper quartiles (see Table F.15), suggesting that the effects of the reform are linear.

Results across industries experiencing larger declines in blue-collar employment share. Figure E.1 shows that the aggregate share of blue-collar employment in the economy was already declining prior to the introduction of the reform. In our baseline model (3), we address this by including industry $\times$ year fixed effects. After absorbing time-varying industry-specific trends (together with all time-invariant firm heterogeneity via firm fixed effects), our estimates show insignificant differences in the evolution of outcomes between blue- and white-collar firms during the pre-reform period, and significant differences emerging after the reform announcement. Although this supports the parallel trends assumption and lends some credibility to our results, we further investigate whether our main findings are driven by the underlying secular trend in blue-collar employment.

We do so by examining whether our results differ across industries with larger declines in their blue-collar employment share. Specifically, we compute the average change in the aggregate share of blue-collar workers for each industry from 2009 to 2012, i.e., over the pre-reform period. Then, we create a dummy variable equal to 1 if the industry’s change in blue-collar share is below the median (capturing the industries with the largest decline in blue-collar share), and to 0 otherwise. We interact this dummy with the variables in Equation (3) and present the results in Table F.16. We observe no statistically significant differences in outcomes between industries with larger declines in blue-collar share and the rest.

Controlling for industry heterogeneity at a more granular level. We also account for industrial trends at a more granular level. In Panel A of Table F.17, we estimate our main regressions using NACE 4-digit industry $\times$ year fixed effects (instead of 2-digit industry $\times$ year fixed effects). Moreover, in Panel B, we redefine our treatment variable within industries. For this analysis, the blue-collar dummy is equal to 1 if the firm’s average pre-period (2009–2012) share of blue-collar workers is above the industry median (instead of the sample median). The latter adjustment divides firms within each industry into equal numbers of *more* affected firms and *less* affected firms and ensures that always equal numbers of blue- and white-collar firms are compared within the same industry, thereby minimizing the confounding elements due to industry-level differences. In both panels, the estimated effects on TFP and employment

outcomes are very similar to our baseline results. The estimated impact on capital appears larger, but the increase is again driven by non-machinery capital, which is consistent with our baseline findings.

Additional robustness tests. In Table F.18, we show that our main effect is robust across several alternative specifications and procedures: (i) further restricting our baseline model by including NACE 2-digit industry $\times$ province $\times$ year fixed effects and even NACE 4-digit industry $\times$ province $\times$ year fixed effects; (ii) collapsing time series data into two periods (pre- and post-reform) around the event to address the potential autocorrelation issue that might arise from panel data models (Bertrand et al., 2004); (iii) clustering standard errors at the NACE 2-digit industry level instead of at the firm level; (iv) bootstrapping our entire procedure—both estimating TFP and running the DiD regression—given that TFP is an estimated outcome; (v) controlling for the interactions between time-invariant pre-period control dummies and the post dummy 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), and (vi) defining the blue-collar dummy based on the firm’s blue-collar share at the end of 2012, rather than using the average pre-period share.

Absence of other changes that differentially affected blue- and white-collar workers. During our sample period, employers benefited from a reduction in social security contributions for low-wage workers. Since blue-collar workers on average earn lower wages than white-collar workers (Statbel, 2024), this policy may have created a wedge in the demand for the two groups, potentially confounding our estimates. In Section F.19, we provide the regulatory details on the employer’s social security contributions and address this concern by analyzing how our results vary across low-wage firms (i.e., firms with annual average employee costs below the median as of the end of 2012). We find some evidence that low-wage firms experienced a smaller decline in new hires following the reform. However, we do not find statistically significant evidence that these firms experienced a smaller decline in TFP, compared to higher-wage firms. Overall, our findings suggest that the benefits for employers related to low-wage workers did not materially drive our results.

Initially exempt industries. Our main sample includes the construction and upholstery & woodworking industries (NACE codes 41–43, 16, and 31), in which blue-collar workers employed at temporary and mobile workplaces were initially exempted from the increased notice periods. We do not exclude these industries from our sample because the exemptions applied only to a subset of blue-collar workers. Therefore, firms operating in these “exempt industries” were still affected by the reform. To examine the robustness of our results within this subset of industries, we restrict the sample to these industries and re-run our analysis. Table F.20 shows that blue-collar firms in these industries were also adversely affected by increased notice periods, despite the partial exemptions.

Using pooled labor input in the production function. As the reform affected

the firing costs of blue- and white-collar workers differently, it is important to include these worker types separately in the production function. In Section F.21 of the Online Appendix, we show analytically how pooling differentially affected worker types in a production function with total employment biases the TFP estimate. The reason, in short, is that pooling treats blue- and white-collar workers as substitutes rather than complements. In the appendix, we derive the bias and show that it varies with the ratio of white- to blue-collar workers. Since we find that this ratio changes differentially for blue- and white-collar firms after the reform, it follows that pooling workers in the production function would distort the measured effect of the reform on productivity. Consistent with this consideration, Table F.21 in the Online Appendix shows that the measured effect of the reform is statistically insignificant when we estimate TFP with a production function that treats worker types as a single employment input.

Classification of blue- and white-collar workers. Our identification strategy assumes that firms correctly classify workers as blue- or white-collar workers. If there would be a “gray” zone for workers who do both manual and non-manual work, and firms could shift these workers from blue- to white-collar contracts after the reform, our results could be biased. In practice, however, such a shift is highly unlikely because Belgian authorities strictly monitor whether employers correctly classify workers based on their primary tasks because, as we discuss in detail in Section 3, these worker types still differ along crucial dimensions. These include, among others, the condition under which they can be put under temporary unemployment and the way social contributions are calculated. Moreover, if the presence of a *gray* zone and changes in worker type classifications between the pre- and post-periods were important enough to affect our results, we should also observe substantial changes in blue- and white-collar output elasticities, especially in industries with more blue-collar workers. Yet, we find no evidence of such changes (see Online Appendix Figure E.2).

6. Additional Analyses

6.1. The effect on firm exit & entry

In Online Appendix F.22, we examine the impact of increased firing costs on firm exit and entry, complementing our analysis of changes in firms’ employment levels before and after the reform (see Section 5.1.1). While we do not observe a significant effect on firm entry, we find some evidence of a significant increase in firm exit, with exit rates increasing around 0.2-0.3 percentage points following the rise in firing costs.³²

³²Given the concern that firm exits might confound our productivity measure, in column (1) of Online Appendix Table F.4, we show that our main result remains unchanged when we construct TFP while accounting for firms’ survival probabilities (as in Olley and Pakes, 1996).

6.2. The role of credit access

In Online Appendix F.23, we investigate how the negative effect of increased firing costs on TFP varies with firms' financial constraints. Financially unconstrained firms are better able to pay higher dismissal costs and thus to replace inefficient workers. Hence, they might experience smaller declines in productivity when firing costs increase. We proxy financial constraints by estimating bank credit availability using loan-level data from the Belgian credit register. Since the firms in our sample are private and relatively small, bank credit is their most important source of external finance.

We find that, among highly leveraged firms, blue-collar firms experience a smaller decline in TFP relative to white-collar firms when they enjoy generous credit supply conditions. Our results therefore suggest that access to credit can help firms cope with increases in firing costs more efficiently.

6.3. Belgian versus German and French firms

In Online Appendix F.24, we use a different dataset and identification strategy to study the Belgian labor market reform. Specifically, we compare Belgian firms to German and French firms (i.e., Belgium's two largest neighboring countries) before and after the legal reform. German and French firms were not affected by the Belgian labor legislation and did not, to the best of our knowledge, experience any shocks or reforms that would affect the composition of their workforce during our sample period. This analysis is interesting for two reasons. First, it allows us to assess the effect of the Belgian labor reform on blue- and white-collar firms separately. Instead of comparing blue- to white-collar firms within Belgium, we now compare blue-collar firms in Belgium to blue-collar firms in Germany and France, and do the same for white-collar firms. In this way, we can elucidate whether the observed effects are primarily driven by the increased firing costs for blue-collar workers, or whether the reform caused stronger changes in firing costs for white-collar workers than we anticipated. Second, this analysis helps us address potential concerns that financial support programs—such as the Outright Monetary Transactions program, implemented in response to the sovereign debt crisis—might have confounded our results if they had affected blue- and white-collar firms differently.

One shortcoming of this approach is that we do not have information on the blue- and white-collar worker composition of German and French firms. For this reason, we define blue- and white-collar firms 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 cannot use TFP as the dependent variable, since estimating elasticities separately for blue- and white-collar workers is crucial in our setting (see the discussion in Sections 5.6 and F.21 of the Online Appendix). Therefore, we

instead look at the effect on value-added. As we argue in Section 5.1.3, the different dynamics of value added for blue- versus white-collar firms are informative of the frictions imposed by higher firing costs.

We find that Belgian firms in blue-collar industries on average experienced a significant 2.1 percent decline in value-added relative to German and French firms in the same industries (see Appendix Table F.24.2). This estimated impact is consistent with the effect identified in the within-Belgium analysis (see column (8) of Table 5). At the same time, we do not observe a significant effect on the value-added of firms in white-collar industries. This is in line with our presumption that the legal reform had an ambiguous effect on the firing costs of white-collar workers and that the reduction in notice periods may indeed have been offset by increased protection through other measures, including the abolition of trial periods, improved outplacement rights, and enhanced safeguards against unfair dismissal. Overall, these results corroborate the negative consequences of higher firing costs for blue-collar workers. Additionally, the fact that we now use unaffected European firms as the control group, further mitigates the concern that European programs to overcome the sovereign debt crisis confound our main results.

7. Conclusion

This paper studies the effect of firing costs on input choices and productivity by exploiting a Belgian labor market reform that induced a plausibly exogenous and substantial increase in firing costs for blue-collar workers relative to white-collar workers. Using a difference-in-differences design, we compare firms with an ex-ante share of blue-collar workers above the median (*“blue-collar firms”*) to firms with a below-median share (*“white-collar firms”*).

We find that separations and hiring among blue-collar firms declined relative to white-collar firms. The effect is significant and occurred immediately after the reform announcement: separations dropped by up to 9 percent after two years; hiring reached a peak reduction of 8 percent after just one year. Firms further responded to higher firing costs for blue-collar workers by relying less on blue-collar workers and more on white-collar workers, which changed the workforce composition. They also shifted toward flexible employment by hiring fewer workers on permanent contracts and outsourcing more workers through employment agencies. Instead, we find no evidence that blue-collar firms adopted capital-intensive technologies and only limited evidence that they increased investment in human capital (training activities). Importantly, we show that blue-collar firms experienced a significant, large, and persistent decline (4.8 percent) in TFP following the reform.

Interpreted through the lens of our theoretical framework, our results support the view that firing costs have negative effects for firms and workers. When such costs increase, firms find it more difficult to adjust employment efficiently, which substantially reduces productivity

through several channels. For workers, the intended gains of more job stability are unevenly distributed and uncertain because firms offer fewer of the better-protected permanent contracts and increase flexibility through outsourcing. In terms of policy implications, our analysis therefore identifies unintended consequences of higher firing costs and corroborates the existence of a trade-off between better-protected existing jobs and a more precarious situation for job seekers.

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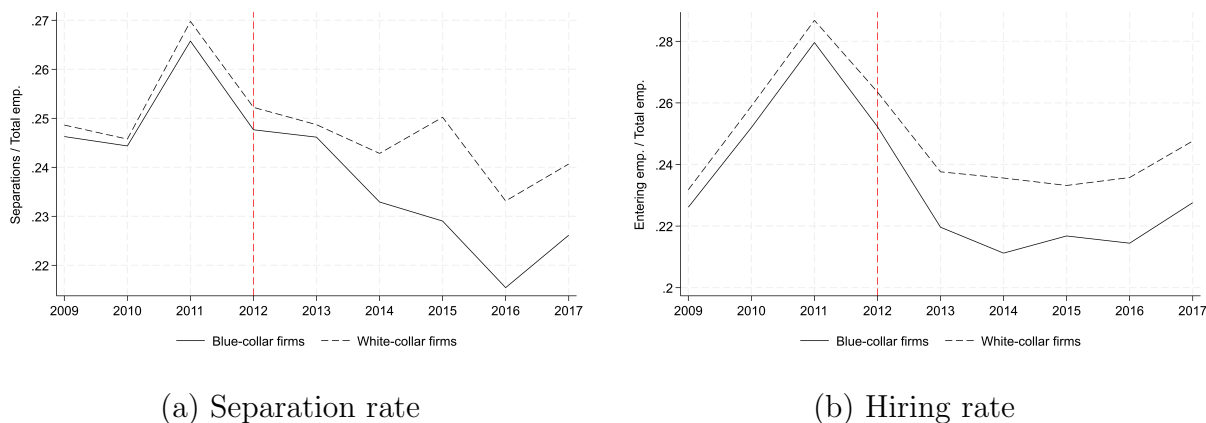
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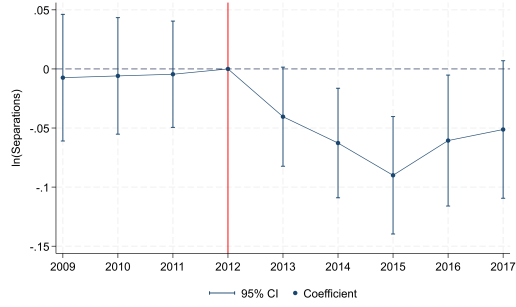
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Figure 1. Separation and hiring rates

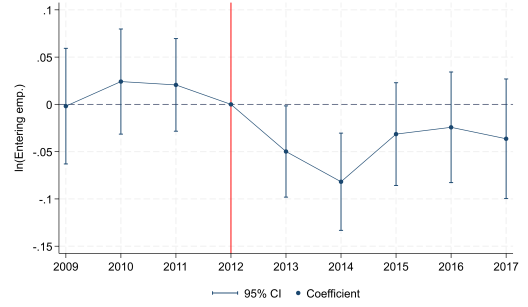


This figure presents the average separation rates (the ratio of the number of exiting employees to the total number of employees) and hiring rates (the ratio of the number of entering employees to the total number of employees) across blue-collar and white-collar firms over 2009–2017. Blue-collar firms (white-collar firms) are defined as those whose average share of blue-collar workers over 2009–2012 was above (below) the sample median.

Figure 2. Dynamic estimates of employment outcomes



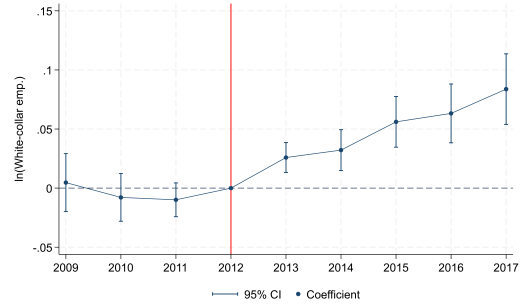
(a) $\ln(\text{Separations})$



(b) $\ln(\text{Entering emp.})$



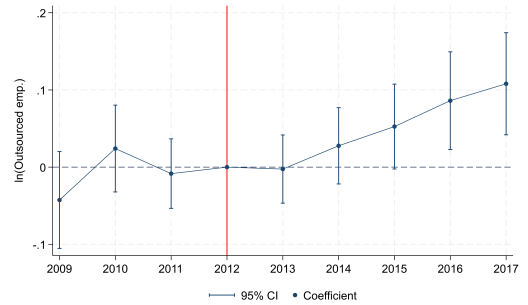
(c) $\ln(\text{Blue-collar emp.})$



(d) $\ln(\text{White-collar emp.})$



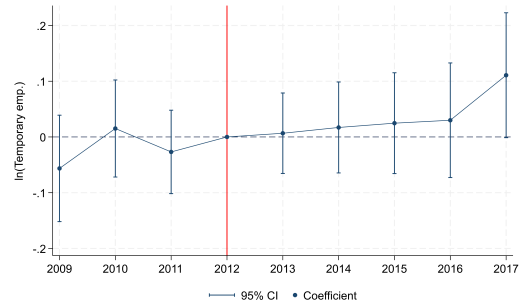
(e) $\ln(\text{Total emp.})$



(f) $\ln(\text{Outsourced emp.})$



(g) $\ln(\text{Permanent emp.})$



(h) $\ln(\text{Temporary emp.})$

This figure presents dynamic estimates of key employment outcomes. Specifically, we estimate Equation (3) by replacing the post dummy with year-specific dummies. The year 2012 is omitted as the reference category. Dynamic estimates for the remaining employment outcomes are reported in Online Appendix Table F.3.

Figure 3. Dynamic estimates of capital & output



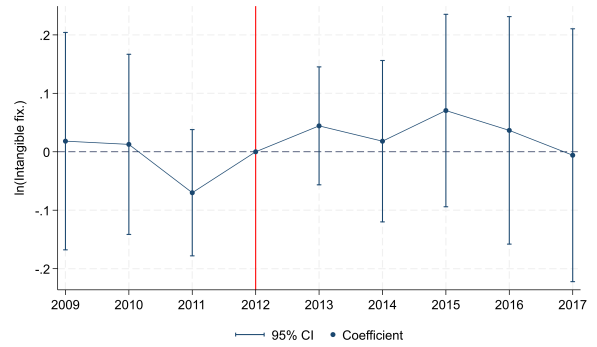
(a) $\ln(\text{Tangible fix.})$



(b) $\ln(\text{Mac., Equip.})$



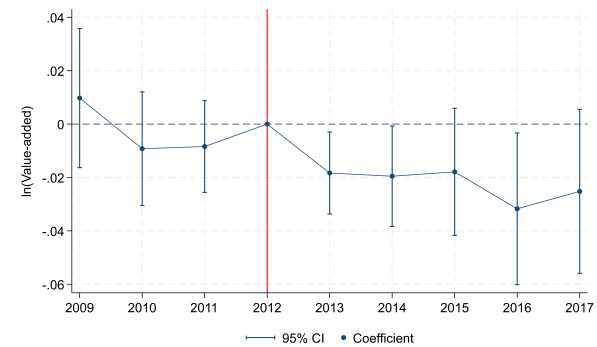
(c) $\ln(\text{Land, Build., Furn., Other})$



(d) $\ln(\text{Intangible fix.})$



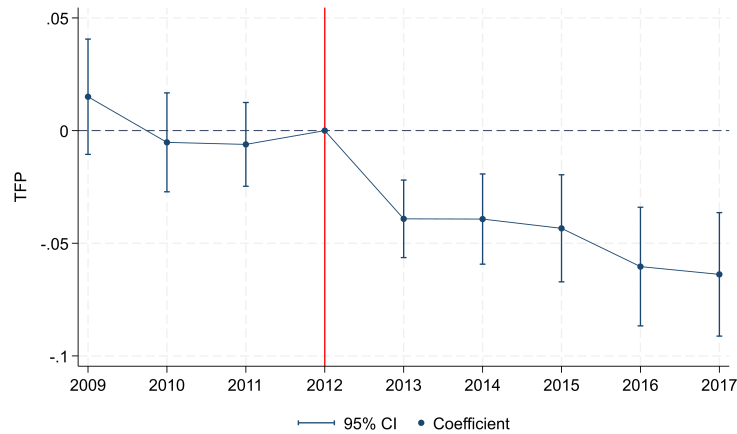
(e) Train. emp.



(f) $\ln(\text{Value added})$

This figure presents dynamic estimates of capital outcomes and output. Specifically, we estimate Equation (3) by replacing the post dummy with year-specific dummies. The year 2012 is omitted as the reference category. Dynamic estimates for the remaining capital outcomes are reported in Online Appendix Table F.3.

Figure 4. Dynamic estimates of TFP



This figure presents dynamic estimates of productivity. Specifically, we estimate Equation (3) by replacing the post dummy with year-specific dummies. The year 2012 is omitted as the reference category.

Table 1: Notice periods before and after the labor market reform

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

	N	Mean	SD	p(25)	p(50)	p(75)
Dependent variables						
TFP	54,452	6.54	0.59	6.22	6.55	6.86
ln(Value-added)	54,452	14.87	1.21	14.11	14.83	15.59
Separations	54,452	12.63	18.29	3	7	15
ln(Separations)	50,528	1.99	1.12	1.10	1.95	2.77
Entering emp.	54,452	12.85	18.23	2	6	15
ln(Entering emp.)	49,371	2.01	1.15	1.10	1.95	2.83
Blue-collar emp.	54,452	43.40	76.35	8	19	47
ln(Blue-collar emp.)	54,452	2.94	1.33	2.08	2.94	3.85
White-collar emp.	54,452	27.30	58.81	5	12	26
ln(White-collar emp.)	54,452	2.45	1.27	1.61	2.48	3.26
Total emp.	54,452	71.66	120.43	17	36	75
ln(Total emp.)	54,452	3.62	1.09	2.83	3.58	4.32
ln(Cost per emp.)	54,452	10.83	0.29	10.67	10.82	10.99
ln(Hours per emp.)	54,452	7.34	0.16	7.28	7.35	7.42
ln(Outsourced emp.)	37,106	1.18	1.20	0	1.10	1.95
ln(Permanent emp.)	54,418	3.57	1.11	2.83	3.56	4.28
ln(Temporary emp.)	19,244	1.10	1.08	0	0.69	1.79
ln(Permanent separations)	43,650	1.78	1.04	1.10	1.79	2.48
ln(Temporary separations)	21,804	1.44	1.16	0.69	1.39	2.20
ln(Permanent entering emp.)	41,780	1.75	1.07	1.10	1.79	2.48
ln(Temporary entering emp.)	22,879	1.55	1.19	0.69	1.39	2.40
ln(Tangible fix.)	54,452	13.88	1.73	12.84	13.99	15.02
ln(Mac., Equip.)	51,939	12.03	2.24	10.59	12.06	13.57
ln(Land, Build., Furn., Other)	54,150	13.43	1.84	12.39	13.64	14.68
ln(Intangible fix.)	24,997	10.60	2.44	8.99	10.60	12.19
Train. emp.	54,452	0.43	0.56	0	0.21	0.75
Train. costs	54,452	0.01	0.02	0	0	0.01
Train. hours	54,452	0.01	0.11	0	0	0.01
Blue-collar	54,452	0.50	0.50	0	0	1
Treatment						
(%) Blue-collar	54,452	0.60	0.24	0.44	0.65	0.80
Size	54,452	15.85	1.42	15.06	15.82	16.64
Controls						
Age	54,452	30.41	17.65	18	27	39
Leverage	54,452	0.60	0.25	0.42	0.62	0.78
EBITDA-to-assets	54,452	0.11	0.10	0.05	0.10	0.16
Cash holdings	54,452	0.09	0.12	0.01	0.05	0.12
Capital-to-labour	54,452	10.27	1.38	9.48	10.37	11.16

This table presents summary statistics for the variables used in the regressions. The main period of the analysis is from 2009 to 2017. The period for the lagged variables spans from 2008 to 2016. 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 (2013-2017). Appendix A defines all variables.

Table 3: The effect of firing costs on employment turnover, composition, costs, and hours

	ln(Separations)	ln(Entering emp.)	ln(Blue-collar emp.)	ln(White-collar emp.)	ln(Total emp.)	ln(Cost per emp.)	ln(Hours per emp.)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Blue-collar x Post	-0.057*** (0.017)	-0.058*** (0.018)	-0.017* (0.010)	0.052*** (0.010)	-0.022*** (0.008)	0.002 (0.003)	0.002 (0.003)
Observations	50,528	49,371	54,452	54,452	54,452	54,452	54,452
R-squared	0.769	0.736	0.965	0.966	0.972	0.837	0.539
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
2-digit industry x year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

This table reports difference-in-differences estimates of employment over the period of 2009 to 2017. The dependent variables: (1) $\ln(\text{Separations})$, the natural log of the number of employees that left the firm; (2) $\ln(\text{Entering emp.})$, the natural log of the number of new hires; (3) $\ln(\text{Blue-collar emp.})$, the natural log of the number of blue-collar employees; (4) $\ln(\text{White-collar emp.})$, the natural log of the number of white-collar employees; (5) $\ln(\text{Total emp.})$, the natural log of the total number of employees; (6) $\ln(\text{Cost per emp.})$, the natural log of the average yearly cost per employee; (7) $\ln(\text{Hours per emp.})$, 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 2013 to 2017, and to 0 for observations in the period of 2009 to 2012. Appendix A 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 (2013-2017). Standard errors (in parentheses) are clustered at the firm level. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Table 4: The effect of firing costs on flexible employment arrangements

	ln(Outsourced emp.)	ln(Permanent emp.)	ln(Temporary emp.)	ln(Permanent separations)	ln(Temporary separations)	ln(Permanent entering emp.)	ln(Temporary entering emp.)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Blue-collar x Post	0.054** (0.022)	-0.023*** (0.008)	0.046 (0.034)	-0.062*** (0.017)	-0.032 (0.035)	-0.080*** (0.020)	-0.012 (0.035)
Observations	37,106	54,418	19,244	43,650	21,804	41,780	22,879
R-squared	0.830	0.971	0.755	0.729	0.691	0.679	0.692
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
2-digit industry x year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

This table reports difference-in-differences estimates of flexible employment arrangements over the period of 2009 to 2017. The dependent variables: (1) $\ln(\text{Outsourced emp.})$, the natural log of the number of outsourced employees through agencies; (2) $\ln(\text{Permanent emp.})$, the natural log of the number of employees with permanent contracts; (3) $\ln(\text{Temporary emp.})$, the natural log of the number of employees with fixed-term contracts; (4) $\ln(\text{Permanent separations})$, the natural log of the number of employees with permanent contracts that left the firm; (5) $\ln(\text{Temporary separations})$, the natural log of the number of employees with fixed-term contracts that left the firm; (6) $\ln(\text{Permanent entering emp.})$, the natural log of the number of new hires with permanent contracts; (7) $\ln(\text{Temporary entering emp.})$, the natural log of the number of new hires with fixed-term contracts. *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 2013 to 2017, and to 0 for observations in the period of 2009 to 2012. Appendix A 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 (2013-2017). Standard errors (in parentheses) are clustered at the firm level. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Table 5: The effect of firing costs on capital & output

	Tangible capital			Intangible capital		Human capital		Output
	ln(Tangible fix.)	ln(Mac., Equip.)	ln(Land, Build., Furn., Other)	ln(Intangible fix.)	Train. emp.	Train. costs	Train. hours	ln(Value added)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Blue-collar x Post	0.020 (0.020)	0.000 (0.028)	0.023 (0.023)	0.046 (0.069)	0.022** (0.010)	-0.000 (0.000)	-0.002 (0.001)	-0.020** (0.010)
Observations	54,452	51,939	54,150	24,997	54,452	54,452	54,452	54,452
R-squared	0.926	0.903	0.901	0.789	0.619	0.559	0.151	0.954
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 & output over the period of 2009 to 2017. The dependent variables: (1) $\ln(\text{Tangible fix.})$, the natural log of tangible fixed assets; (2) $\ln(\text{Mac., Equip.})$, the natural log of the sum of machinery and equipment; (3) $\ln(\text{Land, Build., Furn., Other})$, the natural log of the sum of land, building, furniture, and other tangible fixed capital; (4) $\ln(\text{Intangible fix.})$, the natural log of intangible fixed assets; (5) Train. Emp. , the number of employees who participated in training activities, scaled by the total number of employees; (6) Train. costs , total training costs scaled by the total employee costs; (7) Train. hours , total training hours scaled by the total number of employment hours; (8) $\ln(\text{Value added})$, the natural log of value-added. *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 2013 to 2017, and to 0 for observations in the period of 2009 to 2012. Appendix A 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 (2013-2017). Standard errors (in parentheses) are clustered at the firm level. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Table 6: The effect of firing costs on productivity

	TFP	TFP	TFP	TFP	TFP
	(1)	(2)	(3)	(4)	(5)
Blue-collar x Post	-0.019** (0.008)	-0.020*** (0.007)	-0.045*** (0.009)	-0.048*** (0.008)	-0.045*** (0.008)
Blue-collar	0.038*** (0.013)		0.054*** (0.013)		
Post	0.001 (0.006)	0.003 (0.005)			
Size					0.043*** (0.009)
Age					0.001 (0.001)
Leverage					0.177*** (0.017)
EBITDA-to-assets					0.718*** (0.030)
Cash holdings					0.037 (0.023)
Capital-to-labour					-0.058*** (0.004)
Observations	54,452	54,452	54,452	54,452	54,452
R-squared	0.001	0.781	0.377	0.826	0.834
Firm FE	No	Yes	No	Yes	Yes
2-digit industry x year FE	No	No	Yes	Yes	Yes

This table reports difference-in-differences estimates of total factor productivity over the period of 2009 to 2017. The dependent variable, *TFP*, is the residual from the estimation of a Cobb-Douglas production function through the methodology of [Akerberg et al. \(2015\)](#), using blue- and white-collar worker categories as separate inputs, defined in the natural log. *Blue-collar* is a 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* is a dummy variable equal to 1 for observations in the period of 2013 to 2017, and to 0 for observations in the period of 2009 to 2012. Appendix A 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 (2013-2017). Standard errors (in parentheses) are clustered at the firm level. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

- ONLINE APPENDIX -

This Online Appendix accompanies the paper **“Heterogeneous Firing Costs, Worker Types, and Productivity: Evidence from a Natural Experiment”**, by Andrea Caggese, Ozan Güler, Mike Mariathasan, and Klaas Mulier.

This Online Appendix provides additional details on various aspects:

- Appendix [A](#) provides variable definitions.
- Appendix [B](#) compares the pre-reform working conditions of blue- and white-collar workers.
- Appendix [C](#) explains additional changes introduced by the reform.
- Appendix [D](#) presents a stylized model on firing costs, selection, training, and TFP.
- Appendix [E](#) illustrates additional figures.
 - Evolution of the aggregate share of blue-collar employment ([E.1](#))
 - Comparing pre-period and post-period elasticities ([E.2](#))
- Appendix [F](#) demonstrates additional tables.
 - NACE 2-digit industry-level elasticities ([F.1](#))
 - Alternative dependent variables for separations and entering emp. ([F.2](#))
 - Dynamic estimates (not shown in the main text) of the outcomes in Tables 3, 4, and 5 ([F.3](#))
 - Using different productivity measures ([F.4](#))
 - Placebo test: assuming the reform was announced in 2008 ([F.5](#))
 - Defining treatment based on 2008 ([F.6](#))
 - The effect on profits ([F.7](#))
 - Less versus more affected industries ([F.8](#))
 - Results across less versus more R&D intensive industries ([F.9](#))

- Treating outsourced labor as a production input (F.10)
- Adding firms that operate with only blue- or only white-collar workers and firms that do not report intermediate inputs (F.11)
- Results with controls (F.12)
- Results with the matched samples (F.13)
- Results with treatment intensity (F.14)
- Linear effects of firing costs (F.15)
- Results across industries with larger declines in blue-collar employment share (F.16)
- Accounting for industry heterogeneity at a more granular level (F.17)
- Additional tests (F.18)
- Results across firms with low average cost per employee (F.19)
- Results within initially exempt industries (F.20)
- Firing costs, input choices, and bias in estimating TFP when using a pooled-workers production function (F.21)
- The effect on firm exit & entry (F.22)
- The role of credit access (F.23)
- Cross-country analysis: Belgian versus German and French firms (F.24)

Appendix A. Variable definitions

Variable	Definition
<i>Firm level</i>	
(%) 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 the sample median, and to 0 otherwise
Post	Dummy variable equal to 1 for observations in the period of 2013 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-collar emp.)	Natural log of the number of blue-collar employees
ln(White-collar emp.)	Natural log of the number of white-collar employees
ln(Separations)	Natural log of the number of employees that left the firm
ln(1+Separations)	Natural log of one plus the number of employees that left the firm
Separations/2012 emp.	Number of employees that left the firm, divided by the firm's total number of employees in 2012
ln(Entering emp.)	Natural log of the number of new hires
ln(1+Entering emp.)	Natural log of one plus the number of new hires
Entering emp./2012 emp.	Number of new hires, divided by the firm's total number of employees in 2012
ln(Total emp.)	Natural log of the total number of employees
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(Outsourced emp.)	Natural log of the number of outsourced employees through agencies
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 separations)	Natural log of the number of employees with permanent contracts that left the firm
ln(Temporary separations)	Natural log of the number of employees with fixed-term contracts that left the firm
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(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.	Number of employees who participated in training activities, scaled by the total number of employees
Train. costs	Total training costs scaled by the total employee costs
Train. hours	Total training hours scaled by the total number of employment hours
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
ln(EBITDA)	Natural log of EBITDA
EBITDA-to-assets	Ratio of EBITDA to total assets
EBITDA-to-sales	Ratio of EBITDA to total sales
Cash holdings	Ratio of cash holdings to total assets
Capital-to-labor	Natural log of the ratio of tangible fixed assets to the total number of employees
<i>Industry level</i>	
Exit share	Ratio of the total number of firm exits (defined through firms' legal status) to the total number of firms in the industry in a given year.
Entry share	Ratio of the total number of firm entries (defined through the firm's year of incorporation) to the total number of firms in the industry in a given year.
<i>Country level</i>	
GDP growth	Annual growth rate of gross domestic product (in %)
GDP per capita	Gross domestic product, divided by midyear population (in thous. \$)

Appendix B. Pre-reform working conditions

Table B1: Pre-reform working conditions of blue- and white-collar workers

	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 from the first month onward, 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 placed on temporary unemployment (relatively easily) in a number of circumstances: e.g., economic reasons, bad weather conditions, technical reasons,...	-The worker can be placed on temporary unemployment (under strict conditions) in a few circumstances: e.g., economic reasons (only if the firm is officially recognized as an enterprise in distress),...
Wages	-Contract stipulates an hourly wage. -Weekly payday (or twice per month).	-Contract stipulates a monthly wage. -Monthly payday.
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 labor legislation (the Act of 26 December 2013). Source: [Ajzen and Vermandere \(2014\)](#).

Appendix C. Act of 26 December 2013: Further changes

Other elements of the reform suggest that the Act has primarily increased labor 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 would extend notice periods for blue-collar workers beyond the already substantial increase mandated by the reform, one can imagine that white-collar workers—especially those in higher pay grades—might be able to mitigate the effect of the legal reform by negotiating longer notice periods.

Second, the Act has also introduced changes that benefited both blue- and white-collar workers. Importantly, it has 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 for 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, it now requires employers to provide outplacement support to all dismissed employees with at least 30 weeks of notice. Lastly, the new legislation has abolished the rule of no pay for the first day of sick leave (the so-called “carenz” day). All these elements have increased labor 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. Firing costs, selection, training, and TFP: A stylized model

In this section, we provide a stylized model to explain how firing costs might affect TFP in Equation (2). We consider many firms that operate for two periods and assume a real interest rate of zero. The firms are identical, except for their productivity z and the cost of training workers m . For simplicity but without loss of generality, we further assume that all workers are blue-collar workers ($\gamma = 0$) and that capital is irrelevant for production ($\alpha = 0$). It follows that value added VA is linear in blue-collar employment B (henceforth “workers”):

$$VA = TFP \cdot B \quad (1)$$

The total number of workers B comprises L_0 workers already employed in period 0 and L_1 workers hired in period 1. Workers’ productivity is initially normalized to 1 but can change through training and depreciation. In period 1, firms can fire existing workers by paying a per-worker cost $F > 0$ or hire new workers with a normalized productivity of 1. In period 0, firms decide on worker training and skills randomly depreciate.

Timing

At the beginning of period 0, firms decide whether to train existing workers at cost m to increase their skills to $\lambda > 1$. Since productivity is linear in labor, firms either train all workers, or none. After the training decision, a skill depreciation shock occurs. With probability $\mu \in (0, 1)$, workers’ skills depreciate by a factor $\delta \in (0, 1)$; with probability $1 - \mu$, no depreciation occurs. At the end of the period the original workers’ skills h_0 can thus have four values: δ (no training and depreciation), $\delta\lambda$ (training and depreciation), 1 (no training and no depreciation), or λ (training and no depreciation). We assume $\delta\lambda < 1$, so that depreciation causes even trained workers to be less productive than new workers:¹

$$\delta < \delta\lambda < 1 < \lambda$$

In period 1, firms can hire an exogenously fixed number L_1 of new workers, each with skills equal to 1.² TFP in Equation (1) thus becomes:

¹As we explain below, we focus on firms with $z > w$, which always find it optimal to hire in period 1. For these firms, $\delta\lambda \geq 1$ implies that trained workers are never fired and that firing costs do not affect firms’ training decisions. With $\delta\lambda < 1$, instead, firing costs interact with the training decision. For firms with $z < w$, on the other hand, firing costs might also increase training if $\delta\lambda \geq 1$.

²We assume L_1 to be fixed because our framework is a partial equilibrium model and because technology is linear in labor. Firms would thus optimally hire infinitely many workers in period 1. Below, we show that our results also hold in a model with concave technology, in which the optimal value of L_1 is derived endogenously.

$$TFP = z\theta^B,$$

where z is neutral productivity and θ^B is the weighted average labor productivity of the remaining original workers $L_0^k \in \{0, L_0\}$ and new workers L_1 :

$$\theta^B = \frac{h_0 L_0^k + L_1}{L_0^k + L_1},$$

Period 1 decisions

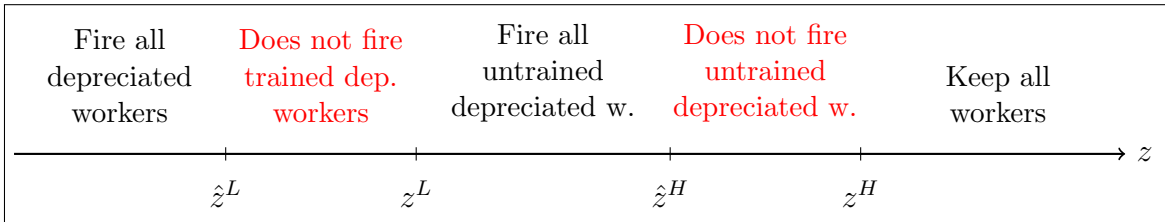
For simplicity, we assume that all workers are paid the same exogenous wage w and focus on firms with $z > w$, i.e., on firms that find it optimal to hire new workers in period 1. The fact that older workers are unprofitable if $w > zh_0$ and get fired if $w - zh_0 > F$ then implies:

Proposition 1. *Higher firing costs reduce measured TFP by inducing firms to retain more unproductive workers.*

Proof. Consider $F = 0$ and define two productivity thresholds $z^H \equiv \frac{w}{\delta}$ and $z^L \equiv \frac{w}{\lambda\delta}$. Because $\delta < \lambda\delta$ and $\lambda\delta < 1$, it follows that $w < z^L < z^H$. For firms with $z \geq z^H$, old workers, even untrained and with depreciated skills, are always profitable and firing costs are irrelevant. For firms with $z^L \leq z < z^H$, untrained depreciated workers are unprofitable, while trained workers remain profitable if their skills depreciate. For $z < z^L$, even trained workers become unprofitable if their skills depreciate. Without firing costs, firms fire all unprofitable workers. Next, consider $F > 0$. With firing costs, firms with $\hat{z}^L < z < z^L$ (where $\hat{z}^L \equiv \frac{w-F}{\lambda\delta}$) no longer fire trained depreciated workers and firms with $\hat{z}^H < z < z^H$ (where $\hat{z}^H \equiv \frac{w-F}{\delta}$) no longer fire untrained depreciated workers. Since these—now remaining—workers are less productive than new workers, θ^B and measured TFP drop. For higher F , the intervals $(\hat{z}^L, z^L)$ and $(\hat{z}^H, z^H)$ widen, firing costs affect more firms, and TFP drops further. $\square$

To illustrate the effect of firing costs, we summarize firms' firing decisions as a function of productivity z in the graph below and highlight the inefficient worker retentions in red.

Firing decisions as a function of firms' productivity z .



Period 0 decisions

In period 0, firms decide whether or not to invest in training their original workforce. We show that firing costs can increase training, but that the effect on *TFP* is ambiguous.

Proposition 2. *Firing costs induce some firms to train their existing workforce, but the productivity effect of such training is ambiguous.*

Proof. Assume that firms are uniformly and independently distributed over productivity z and training costs m . From Proposition 1, we know that firing costs change firms' firing behavior only in the intervals $z \in (\hat{z}^L, z^L)$ and $z \in (\hat{z}^H, z^H)$. This changing behavior implies that the returns of training are higher in the presence of firing costs in both these regions.

Consider first the region $z \in (\hat{z}^L, z^L)$. In the absence of firing costs, firms fire trained depreciated workers, and the returns from training are the difference between the profits generated by trained $(\lambda z - w)$ and untrained $(z - w)$ workers, multiplied by the probability of no depreciation: $(1 - \mu) \cdot z(\lambda - 1)$. In the presence of firing costs, firms in this region keep trained depreciated workers. In other words, with probability μ they keep workers that generate (negative) profits equal to $\lambda \delta z - w$, rather than pay the firing cost $-F$. Hence their additional profits are $\lambda \delta z - w - (-F) = F + \lambda \delta z - w > 0$.

Now consider the region $z \in (\hat{z}^H, z^H)$. In this region, in the absence of firing costs, the firm fires untrained workers with depreciated skills. Thus without training, profits are $(1 - \mu) \cdot (z - w)$, while with training, they are: $(1 - \mu) \cdot (\lambda z - w) + \mu \cdot (\lambda \delta z - w)$. Taking the difference between these two values yields:

$$T^0 = (1 - \mu) \cdot z(\lambda - 1) + \mu \cdot (\lambda \delta z - w)$$

Instead, in the presence of firing costs, the firm does not fire untrained depreciated workers. Returns without training are: $(1 - \mu) \cdot (z - w) + \mu \cdot (\delta z - w)$; with training they are $(1 - \mu) \cdot (\lambda z - w) + \mu \cdot (\lambda \delta z - w)$. Taking the difference yields:

$$T^F = (1 - \mu) \cdot z(\lambda - 1) + \mu \cdot \delta z(\lambda - 1)$$

Thus, firing costs increase the returns from training by $T^F - T^0 = \mu \cdot (w - \delta z) > 0$. This difference is positive because we are in the region in which untrained depreciated workers are loss-making, so $\delta z < w$.

Finally, firing costs also increase the returns from training for $z \in [z^L, \hat{z}^H]$. Firms in this region always fire untrained workers with depreciated skills, and the return to training, absent firing costs, is T^0 . Firing costs add the return of not having to pay $-F$ with probability μ .

To sum up, define Φ as the increase in returns from training induced by firing costs:

$$\Phi = \mu(F + \lambda\delta z - w) > 0 \text{ if } z \in (\hat{z}^L, z^L) \quad (2)$$

$$\Phi = \mu F > 0 \text{ if } z \in [z^L, \hat{z}^H] \quad (3)$$

$$\Phi = \mu(w - \delta z) > 0 \text{ if } z \in (\hat{z}^H, z^H) \quad (4)$$

Recall that firms are uniformly distributed over training costs m . Assume some training happens in equilibrium, so that for each productivity level z , some firms have values of m just above the threshold that would make training profitable. To prove that firing costs increase training, it is then sufficient to show that they increase the value of Ω for some firms. This follows immediately from Equations (2) to (4). They imply $\Omega = 0$ for all firms if $F = 0$, while the range of firms with $\Omega > 0$ expands as F increases.

Although training increases workers' productivity, the overall effect of firing costs on TFP is in principle ambiguous. On the one hand, higher firing costs induce more firms to invest in training, which positively contributes to TFP if skills do not depreciate. On the other hand, firing costs and training also prevent the firing of inefficient workers, which negatively contributes to TFP. This potentially happens in the region $z \in (\hat{z}^L, z^L)$. In this region, trained depreciated workers are fired without firing costs, because they generate losses. Firing costs instead imply that the firm keeps these workers. Even though training unambiguously increases productivity in all the other regions, which effect dominates is ultimately a question of how firing costs and productivity are distributed across the firm population, and thus ex-ante ambiguous.

□

Extension: ex-ante selection

One important way in which firing costs can affect worker quality is ex-ante selection (Pries and Rogerson, 2005). To view this effect in our simple model, consider a modification in which there is no training, but in which the firm has to hire the L_0 workers in period 0. Before hiring them, the firm receives a signal ξ_i about worker quality. The signal is equal to the probability that the worker will be highly productive ($h_0 = 1$), while with probability $1 - \xi_i$ the worker has lower productivity ($h_0 = \delta < 1$).

In other words, while in the previous model workers exogenously depreciated with the same probability $1 - \gamma$, in this version workers are ex-ante heterogeneous with respect to that probability. After observing ξ_i , the firm then decides whether to pay $\kappa > 0$ in hiring costs and hire the worker. After hiring, all workers reveal their quality (that is, they depreciate with their individual probability $1 - \xi_i$).

Firing costs matter for ex-ante selection when low-quality workers are loss-making, so we focus on $\delta z < w$, i.e., on the case in which depreciated workers are always fired in the absence of firing costs. If ξ_i is independently and identically distributed across workers between 0 and 1, and there are no firing costs, firms hire all workers with a signal $\xi_i \geq \underline{\xi}$, where $\underline{\xi}(z - w) = \kappa$. Intuitively, since the worker is fired in period 1 if her productivity turns out to be low, the expected value of the marginal worker is $\underline{\xi}(z - w)$, which implies $\underline{\xi} = \frac{\kappa}{z - w}$. Naturally, we focus on the case in which $\frac{\kappa}{z - w} < 1$ so that some workers are always hired in period 0.

If firing costs rise above zero, but it remains optimal to fire low-productivity workers, the overall cost of a worker increases by the expected firing costs $(1 - \xi_i)F$. The marginal worker quality with firing costs $\underline{\xi}$ is thus given by $\underline{\xi}(z - w) - (1 - \underline{\xi})F = \kappa$, which implies $\underline{\xi} = \frac{\kappa + F}{z - w + F}$. If firing costs are even higher and low-productivity workers are not fired, the hiring threshold becomes: $\underline{\xi} = \frac{\kappa + (w - \delta)}{z - w + (w - \delta)}$. We can thus summarize the effect of firing costs on the hiring threshold $\underline{\xi}$ as follows:

No firing costs ($F = 0$)	$\underline{\xi} = \frac{\kappa}{z - w}$
Low firing costs ($w - \delta z > F > 0$)	$\underline{\xi} = \frac{\kappa + F}{z - w + F}$
High firing costs ($F > w - \delta z > 0$)	$\underline{\xi} = \frac{\kappa + (w - \delta)}{z - w + (w - \delta)}$

In other words, for firms with $\delta z < w$, firing costs unambiguously increase the minimum threshold $\underline{\xi}$. By being more selective, the firm obtains the additional benefit of reducing expected firing costs (or losses if it is too costly to fire the worker). For more productive firms with $\delta z > w$, firing costs are irrelevant (period 0 workers are never fired) and do not affect ex-ante selection. It follows that higher firing costs increase the average threshold $\underline{\xi}$ across firms and thus average labor productivity θ^B .

Other Extensions

Our results assume that wages cannot be contingent on training or skill depreciation. For skill depreciation, this assumption is supported by ample evidence that wages are downward rigid, especially for heavily unionized employees such as Belgian blue-collar workers.

With respect to the training results, if wages are upward flexible, the training surplus could in part accrue to the worker. While this would generally reduce training incentives for the firm, higher firing costs would continue to make ex-ante training more likely.

Likewise, adding other production inputs would quantitatively change the optimality conditions but not the qualitative results: rising firing costs would still imply that some firms find it optimal to keep some unproductive workers, which, in turn, would cause a drop in measured TFP.

Finally, our results also hold for production functions with decreasing marginal returns. We only show this explicitly for the firing decisions in period 1, but similar results can be obtained for the training and selection decisions in period 0. The production function is:

$$Y = z[h_0 L_0^k + L_1]^\alpha,$$

with $0 < \alpha < 1$. A generic firm chooses new hiring L_1 and the retained number of old workers L_0^k , to maximize profits:

$$\pi = \max_{L_1, L_0^k} Y - w(L_0^k + L_1) - F(L_0 - L_0^k),$$

subject to $L_1 \geq 0$ and $L_0^k \leq L_0$, with $L_0 - L_0^k$ being the number of old workers that are fired. Taking the first-order conditions with respect to L_1 and L_0^k , we can show that the optimal number of new hires, given L_0^k , is:

$$L_1^* = \left(\frac{z\alpha}{w} \right)^{\frac{1}{1-\alpha}} - h_0 L_0^k.$$

If $L_1^* > 0$, older workers are fired if $F < w(1 - h_0)$ and kept if $F \geq w(1 - h_0)$. Intuitively, the loss of keeping old workers is their lower return $w(1 - h_0)$ relative to new workers, which is a positive quantity if they depreciated and hence $h_0 \in \{\delta, \delta\lambda\} < 1$. Old workers are fired if this loss is larger than the firing cost F . Thus, an increase in firing costs would prevent the firing of these workers and reduce TFP, as in the benchmark model illustrated above.

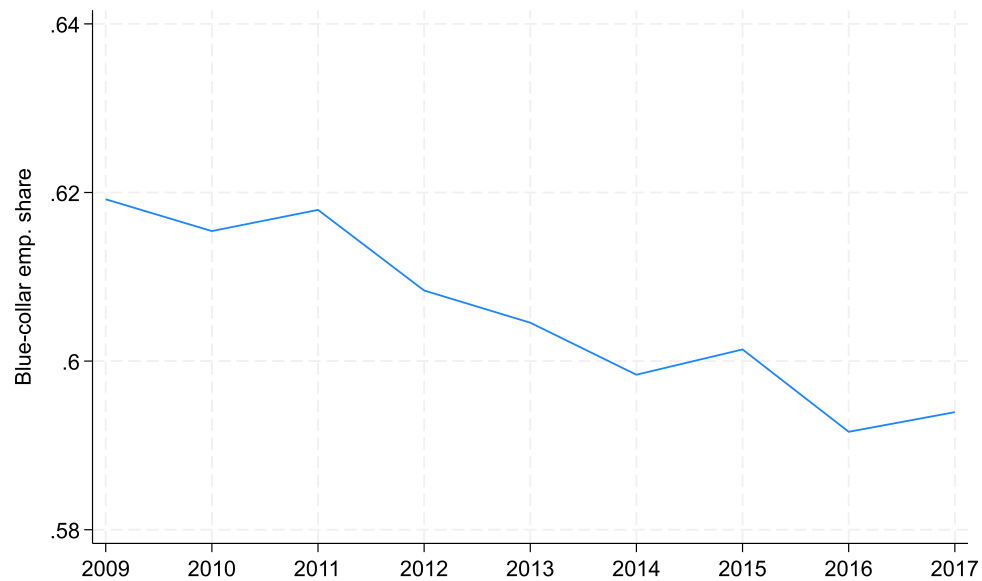
If $L_1^* \leq 0$, the constraint $L_1 \geq 0$ holds with equality. Substituting $L_1 = 0$ and taking the first-order condition for L_0^k yields:

$$L_0^k = \left(\frac{\alpha z h_0^\alpha}{w - F} \right)^{\frac{1}{1-\alpha}}$$

If $L_0^k > L_0$, the constraint $L_0^k \leq L_0$ holds with equality and the firm sets $L_0^k = L_0$. The firm neither hires nor fires and remains with the current old workers. If $L_0^k \leq L_0$, the first-order condition above determines L_0^k .

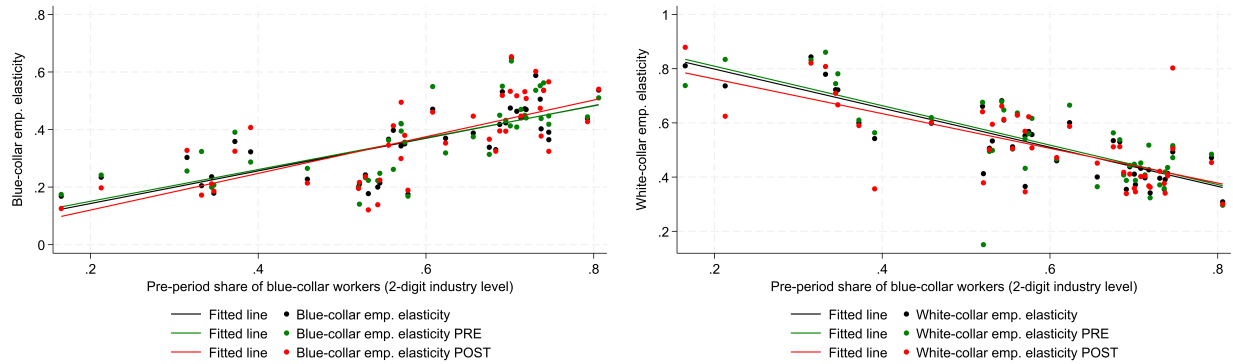
Appendix E. Additional figures

Figure E.1. Evolution of the aggregate share of blue-collar employment



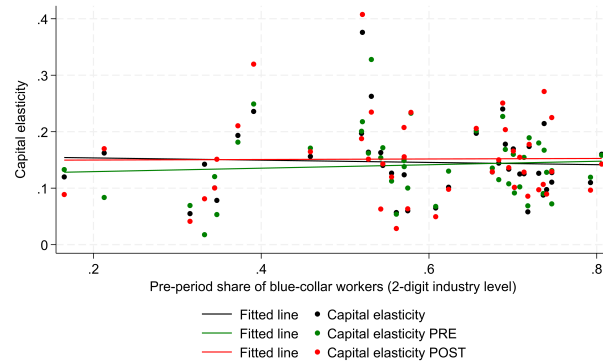
This figure presents the aggregate share of blue-collar workers in our regression sample. The share is calculated as the total number of blue-collar workers in a given year divided by the total number of workers in that year.

Figure E.2: Comparing pre-period and post-period elasticities



(a) Elasticities for blue-collar workers

(b) Elasticities for white-collar workers



(c) Elasticities for capital

This figure shows the pre-period (2009-2012) and post-period (2013-2017) industry-level elasticities for blue-collar workers (Panel (a)), white-collar workers (Panel (b)), and capital (Panel (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.

Appendix F. Additional tables

F.1. NACE 2-digit industry-level elasticities

Table F.1: NACE 2-digit industry-level elasticities

NACE 2-digit industry	N	Blue-collar emp. el.		White-collar emp. el.		Capital el.	
1	196	0.33	(0.16)	0.53	(0.26)	0.14	(0.09)
8	261	0.40	(0.19)	0.39	(0.18)	0.21	(0.10)
10	3247	0.42	(0.00)	0.41	(0.00)	0.24	(0.00)
11	345	0.17	(0.04)	0.56	(0.12)	0.23	(0.05)
13	940	0.39	(0.17)	0.49	(0.22)	0.11	(0.05)
14	210	0.34	(0.30)	0.55	(0.46)	0.15	(0.15)
16	622	0.51	(0.00)	0.36	(0.00)	0.09	(0.00)
17	572	0.47	(0.08)	0.41	(0.07)	0.17	(0.04)
18	708	0.44	(0.03)	0.35	(0.02)	0.18	(0.00)
20	1480	0.20	(0.00)	0.66	(0.00)	0.20	(0.00)
21	208	0.16	(0.10)	0.75	(0.44)	0.12	(0.07)
22	1211	0.34	(0.00)	0.53	(0.00)	0.13	(0.00)
23	1438	0.47	(0.00)	0.34	(0.00)	0.17	(0.00)
24	758	0.46	(0.02)	0.43	(0.02)	0.13	(0.01)
25	2890	0.44	(0.00)	0.40	(0.00)	0.13	(0.00)
26	406	0.18	(0.01)	0.72	(0.06)	0.08	(0.02)
27	536	0.35	(0.07)	0.61	(0.10)	0.12	(0.09)
28	1532	0.35	(0.00)	0.57	(0.00)	0.06	(0.00)
29	406	0.47	(0.05)	0.43	(0.08)	0.06	(0.05)
30	130	0.40	(0.99)	0.63	(1.56)	0.06	(0.14)
31	647	0.42	(0.00)	0.44	(0.00)	0.13	(0.00)
32	278	0.21	(0.03)	0.61	(0.07)	0.14	(0.02)
33	422	0.37	(0.06)	0.60	(0.08)	0.10	(0.03)
37	65	0.68	(0.24)	0.34	(0.17)	0.16	(0.10)
38	688	0.39	(0.02)	0.40	(0.01)	0.20	(0.02)
41	2399	0.36	(0.03)	0.51	(0.04)	0.13	(0.01)
42	1292	0.44	(0.04)	0.47	(0.04)	0.11	(0.01)
43	3988	0.54	(0.00)	0.41	(0.00)	0.10	(0.00)
45	5680	0.37	(0.00)	0.51	(0.00)	0.13	(0.00)
46	10598	0.23	(0.00)	0.60	(0.00)	0.16	(0.00)
47	2443	0.36	(0.00)	0.60	(0.00)	0.19	(0.00)
49	2983	0.54	(0.02)	0.31	(0.01)	0.16	(0.03)
52	1550	0.24	(0.01)	0.51	(0.02)	0.16	(0.03)
55	289	0.42	(0.13)	0.37	(0.11)	0.12	(0.04)
56	332	0.65	(0.01)	0.37	(0.01)	0.10	(0.01)
58	105	0.17	(0.07)	0.81	(0.25)	0.12	(0.05)
59	103	0.20	(0.10)	0.68	(0.25)	0.16	(0.10)
62	114	0.23	(0.20)	0.74	(0.29)	0.16	(0.13)
68	235	0.32	(0.07)	0.54	(0.06)	0.24	(0.08)
69	34	0.20	(0.16)	0.78	(0.47)	0.14	(0.13)
70	229	0.29	(0.09)	0.67	(0.19)	0.13	(0.06)
71	301	0.24	(0.04)	0.72	(0.09)	0.10	(0.02)
73	161	0.30	(0.11)	0.84	(0.29)	0.05	(0.03)
77	464	0.18	(0.06)	0.53	(0.02)	0.26	(0.08)
78	46	0.33	(0.29)	0.54	(0.47)	0.02	(0.05)
80	102	0.45	(0.09)	0.46	(0.06)	0.08	(0.06)
81	492	0.59	(0.03)	0.40	(0.02)	0.13	(0.02)
92	32	0.21	(0.12)	0.41	(0.21)	0.38	(0.20)
95	61	0.47	(0.28)	0.46	(0.28)	0.06	(0.08)
96	223	0.53	(0.08)	0.35	(0.11)	0.18	(0.04)

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

F.2. Alternative dependent variables for separations and entering emp.

Table F.2: Analysis of employment turnover with alternative dependent variables

A. Main estimates

	ln(Separations)	ln(Entering emp.)	ln(1+Separations)	ln(1+Entering emp.)	Separations/2012 emp.	Entering emp./2012 emp.
	(benchmark)	(benchmark)				
	(1)	(2)	(3)	(4)	(5)	(6)
Blue-collar x Post	-0.057*** (0.017)	-0.058*** (0.018)	-0.047*** (0.015)	-0.062*** (0.017)	-0.010** (0.005)	-0.015*** (0.005)
Observations	50,528	49,371	54,452	54,452	54,452	54,452
R-squared	0.769	0.736	0.781	0.750	0.640	0.639
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
2-digit industry x year FE	Yes	Yes	Yes	Yes	Yes	Yes

(Continued)

B. Dynamic estimates

	$\ln(\text{Separations})$	$\ln(\text{Entering emp.})$	$\ln(1+\text{Separations})$	$\ln(1+\text{Entering emp.})$	$\text{Separations}/2012 \text{ emp.}$	$\text{Entering emp.}/2012 \text{ emp.}$
	(benchmark)	(benchmark)				
	(1)	(2)	(3)	(4)	(5)	(6)
Blue-collar x 2009 dummy	-0.007 (0.027)	-0.002 (0.031)	0.011 (0.024)	0.010 (0.028)	0.004 (0.008)	0.010 (0.008)
Blue-collar x 2010 dummy	-0.006 (0.025)	0.024 (0.028)	0.009 (0.022)	0.014 (0.025)	0.005 (0.008)	0.011 (0.008)
Blue-collar x 2011 dummy	-0.004 (0.023)	0.021 (0.025)	0.002 (0.020)	0.011 (0.022)	-0.000 (0.007)	0.006 (0.007)
Blue-collar x 2013 dummy	-0.040* (0.021)	-0.050** (0.025)	-0.019 (0.019)	-0.041* (0.021)	-0.007 (0.006)	-0.013** (0.007)
Blue-collar x 2014 dummy	-0.063*** (0.024)	-0.082*** (0.026)	-0.059*** (0.021)	-0.098*** (0.023)	-0.014** (0.007)	-0.017** (0.007)
Blue-collar x 2015 dummy	-0.090*** (0.025)	-0.031 (0.028)	-0.053** (0.022)	-0.034 (0.025)	-0.011 (0.007)	-0.001 (0.008)
Blue-collar x 2016 dummy	-0.061** (0.028)	-0.024 (0.030)	-0.041* (0.025)	-0.043 (0.027)	-0.005 (0.008)	-0.003 (0.009)
Blue-collar x 2017 dummy	-0.051* (0.030)	-0.036 (0.032)	-0.036 (0.026)	-0.042 (0.029)	-0.001 (0.009)	-0.003 (0.010)
Observations	50,528	49,371	54,452	54,452	54,452	54,452
R-squared	0.769	0.736	0.781	0.751	0.640	0.639
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 employment turnover over the period of 2009 to 2017. The dependent variables: (1) $\ln(\text{Separations})$, the natural log of the number of employees that left the firm; (2) $\ln(\text{Entering emp.})$, the natural log of the number of new hires; (3) $\ln(1+\text{Separations})$, the natural log of one plus the number of employees that left the firm; (4) $\ln(1+\text{Entering emp.})$, the natural log of one plus the number of new hires; (5) $\text{Separations} / 2012 \text{ emp.}$, the number of employees that left the firm, divided by the firm's total number of employees in 2012; (6) $\text{Entering emp.} / 2012 \text{ emp.}$, the number of new hires, divided by the firm's total number of employees in 2012. *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 2013 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. Appendix A 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 (2013-2017). Standard errors (in parentheses) are clustered at the firm level. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

F.3. Dynamic estimates (not shown in the main text) of the outcomes in Tables 3, 4, and 5

Table F.3: The effect of firing costs: Supplementary dynamic estimates

	ln(Cost emp.)	per ln(Hours emp.)	per ln(Permanent separations)	ln(Temporary separations)
	(1)	(2)	(3)	(4)
Blue-collar x 2009 dummy	-0.000 (0.006)	-0.013** (0.005)	0.002 (0.030)	-0.011 (0.057)
Blue-collar x 2010 dummy	0.001 (0.005)	-0.005 (0.005)	-0.009 (0.028)	-0.020 (0.052)
Blue-collar x 2011 dummy	-0.004 (0.005)	-0.004 (0.005)	-0.022 (0.026)	0.022 (0.046)
Blue-collar x 2013 dummy	0.003 (0.005)	-0.004 (0.004)	-0.029 (0.025)	-0.023 (0.043)
Blue-collar x 2014 dummy	0.007 (0.005)	0.001 (0.005)	-0.055** (0.026)	-0.035 (0.048)
Blue-collar x 2015 dummy	-0.006 (0.006)	-0.010** (0.005)	-0.101*** (0.027)	-0.026 (0.051)
Blue-collar x 2016 dummy	0.003 (0.006)	0.001 (0.005)	-0.079*** (0.030)	-0.074 (0.057)
Blue-collar x 2017 dummy	-0.000 (0.006)	-0.001 (0.005)	-0.096*** (0.031)	-0.008 (0.060)
Observations	54,452	54,452	43,650	21,804
R-squared	0.837	0.539	0.729	0.691
Firm FE	Yes	Yes	Yes	Yes
2-digit industry x year FE	Yes	Yes	Yes	Yes

(Continued)

	ln(Permanent entering emp.)	ln(Temporary entering emp.)	Train. costs	Train. hours
	(5)	(6)	(7)	(8)
Blue-collar x 2009 dummy	0.024 (0.034)	-0.049 (0.058)	0.001 (0.000)	0.003* (0.001)
Blue-collar x 2010 dummy	0.007 (0.032)	0.021 (0.052)	0.000 (0.000)	0.001 (0.001)
Blue-collar x 2011 dummy	0.014 (0.028)	0.002 (0.045)	-0.000 (0.000)	-0.001 (0.001)
Blue-collar x 2013 dummy	-0.048* (0.029)	-0.034 (0.043)	-0.000 (0.000)	0.001 (0.001)
Blue-collar x 2014 dummy	-0.069** (0.030)	-0.075 (0.047)	-0.000 (0.000)	-0.006 (0.005)
Blue-collar x 2015 dummy	-0.072** (0.031)	0.005 (0.050)	-0.000 (0.000)	0.000 (0.001)
Blue-collar x 2016 dummy	-0.067** (0.034)	-0.009 (0.056)	-0.001 (0.000)	-0.001 (0.002)
Blue-collar x 2017 dummy	-0.096*** (0.036)	0.055 (0.058)	-0.001 (0.001)	-0.002 (0.002)
Observations	41,780	22,879	54,452	54,452
R-squared	0.679	0.692	0.559	0.151
Firm FE	Yes	Yes	Yes	Yes
2-digit industry x year FE	Yes	Yes	Yes	Yes

This table reports dynamic estimates of main outcomes (those that are not shown in the main text) over the period of 2009 to 2017. The dependent variables: (1) $\ln(\text{Cost per emp.})$, the natural log of the average yearly cost per employee; (2) $\ln(\text{Hours per emp.})$, the natural log of the average yearly number of hours worked per employee; (3) $\ln(\text{Permanent separations})$, the natural log of the number of employees with permanent contracts that left the firm; (4) $\ln(\text{Temporary separations})$, the natural log of the number of employees with fixed-term contracts that left the firm; (5) $\ln(\text{Permanent entering emp.})$, the natural log of the number of new hires with permanent contracts; (6) $\ln(\text{Temporary entering emp.})$, the natural log of the number of new hires with fixed-term contracts; (7) *Train. costs*, total training costs scaled by total employee costs; (8) *Train. hours*, total training hours scaled by the total number of employment hours. *Blue-collar* is a 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. 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. Appendix A 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 (2013-2017). Standard errors (in parentheses) are clustered at the firm level. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

F.4. Using different productivity measures

Table F.4 illustrates the results from estimating Equation (3) using alternative productivity measures.³ In column (1), we construct TFP by accounting for firms' survival probabilities, motivated by the finding that increased firing costs are associated with higher firm exit rates (as shown in Table F.22). In column (2), TFP is recovered through the one-step estimation procedure suggested by Wooldridge (2009), where we use intermediate inputs as a proxy for unobserved productivity.

We also consider specifications that are more flexible and consistent with possible differences across the production functions of blue- and white-collar firms, as well as between the pre- and post-reform periods. In column (3), we estimate a translog production function which is a more general functional form that includes the Cobb-Douglas as a special case, and also allows that the elasticity of substitution between inputs can vary with the levels of inputs. Therefore, it is potentially consistent with different types of firms employing different shares of blue- and white-collar workers. In column (4), we recover TFP using our baseline approach (i.e., the approach of Akerberg 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 could incentivize firms to adopt new technologies that are less dependent on blue-collar workers, even though we do not find evidence of such behavior. In column (5), we additionally estimate a Cobb-Douglas production function separately for blue- and white-collar firms.

In column (6), to further address the concern about time-varying elasticities, we estimate the production function separately for each NACE 2-digit industry $\times$ year, using OLS because the production function cannot be estimated for each year with the estimators of Wooldridge (2009) or Akerberg et al. (2015). For reference, in column (7), we estimate the production function for each NACE 2-digit industry with constant elasticities through OLS. In column (8), 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.⁴

Lastly, in column (9), we construct TFP using our baseline approach but use labor hours (instead of the number of employees) as the measure of labor input to account for potential substitutions between job positions and hours. Note that hours are not disaggregated by worker type in our dataset. To approximate hours by worker category, we assume that firms are likely to report the contractually binding 38 hours per week for their white-collar workers, but the number of hours worked per week by a blue-collar worker can easily vary.

³In Table F.4, after estimating each production function, as in our benchmark analysis, we drop observations with resulting negative elasticities, which leads to some variation in sample size across columns. Nevertheless, our results remain unchanged if we retain them.

⁴The sample size decreases by 15% for this analysis. This is because revenue data, unlike value-added data, is only available for large firms.

This is because blue-collar workers (i) are paid on a weekly basis for the actual number of hours worked, based on an hourly wage, and (ii) can more readily be placed on temporary unemployment due to, for example, economic reasons such as declining demand or technical reasons such as adverse weather conditions. In contrast, white-collar workers receive a fixed monthly salary, so variations in their hours worked are likely to average out over the year. Then, we assume that each full-time white-collar worker works 42 weeks because, in Belgium, we first deduct paid leave (24 days or 4.8 weeks) and public holidays (10 days or 2 weeks) from the 52 weeks. Additionally, we deduct another 3.2 weeks to account for other potential unworked time, such as sick leave, personal leave, or seniority-based vacation. Taken together, we assume that each full-time white-collar worker works 1,596 hours annually ($= 38 \text{ hours} \times 42 \text{ weeks}$), and allocate the remaining firm-level hours to blue-collar workers. Nevertheless, our results remain robust when we assign different numbers of working weeks to white-collar workers, ranging from 40 weeks (further extending time not worked) to 46 weeks (assuming fewer holidays/more days worked).

Regardless of the TFP measure, as illustrated in Panel A, the estimated effect is always negative and significant.⁵ Furthermore, the dynamic estimates in Panel B show that all different specifications support the ex-ante parallel trends assumption, and exhibit similar dynamics ex-post, with an estimated drop in TFP (relative to 2012) that often peaks around the year 2017. The magnitude of the peak depends on the specification, ranging between 1.4% and 6.3%, but most of the specifications show a significant drop of around 5%.

⁵The estimated coefficient is particularly large in column (5). However, this specification is very demanding for the data, since we estimate four different sets of elasticities for each 2-digit sector, resulting in around 200 different production function estimations in total. Therefore, these results are likely to be less reliable than those in the other specifications.

Table F.4: Firing costs and productivity: Different productivity measures

A. Main Estimates

	TFP	TFP	TFP	TFP	TFP	TFP	TFP	TFP	TFP
	Akerberg et al. (with firm sur- vival)	Wooldridge	Akerberg et al. (translog)	Akerberg et al.	Akerberg et al.	OLS	OLS	OLS	Akerberg et al.
	Value-added	Value-added	Value-added	Value-added	Value-added	Value-added	Value-added	Revenue	Value-added
	constant el.	constant el.	constant el.	pre & post el.	blue- & whitecollar firm, pre & post el.	yearly- varying el.	constant el.	constant el.	constant el. with imputed labor hours
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Blue-collar x Post	-0.047*** (0.008)	-0.040*** (0.008)	-0.018* (0.010)	-0.042*** (0.009)	-0.243*** (0.067)	-0.035*** (0.008)	-0.047*** (0.008)	-0.011*** (0.003)	-0.039*** (0.008)
Observations	54,246	51,440	54,452	53,556	52,122	54,221	54,452	46,297	52,597
R-squared	0.839	0.888	0.903	0.864	0.837	0.838	0.817	0.987	0.906
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
2-digit industry x year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

(Continued)

B. Dynamic Estimates

	TFP	TFP	TFP	TFP	TFP	TFP	TFP	TFP	TFP
	Akerberg et al. (with firm survival)	Wooldridge	Akerberg et al. (translog)	Akerberg et al.	Akerberg et al.	OLS	OLS	OLS	Akerberg et al.
	Value-added	Value-added	Value-added	Value-added	Value-added	Value-added	Value-added	Revenue	Value-added
	constant el.	constant el.	constant el.	pre & post el.	blue- & white-collar firm, pre & post el.	yearly-varying el.	constant el.	constant el.	constant el. with imputed labor hours
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Blue-collar x 2009 dummy	0.015 (0.013)	0.016 (0.012)	-0.014 (0.016)	0.012 (0.013)	-0.060 (0.042)	0.004 (0.013)	0.015 (0.013)	0.009** (0.005)	0.023* (0.013)
Blue-collar x 2010 dummy	-0.006 (0.011)	-0.006 (0.011)	-0.023* (0.014)	-0.007 (0.011)	-0.053 (0.037)	-0.009 (0.011)	-0.006 (0.011)	0.002 (0.004)	-0.007 (0.011)
Blue-collar x 2011 dummy	-0.007 (0.010)	-0.007 (0.009)	-0.009 (0.009)	-0.008 (0.009)	-0.031 (0.024)	-0.011 (0.009)	-0.006 (0.009)	-0.001 (0.003)	-0.011 (0.009)
Blue-collar x 2013 dummy	-0.039*** (0.009)	-0.037*** (0.009)	-0.024** (0.011)	-0.035*** (0.010)	-0.205** (0.080)	-0.016* (0.009)	-0.039*** (0.009)	-0.004 (0.003)	-0.031*** (0.009)
Blue-collar x 2014 dummy	-0.039*** (0.010)	-0.038*** (0.010)	-0.016 (0.012)	-0.035*** (0.011)	-0.192** (0.079)	-0.026** (0.010)	-0.039*** (0.010)	-0.007* (0.004)	-0.034*** (0.010)
Blue-collar x 2015 dummy	-0.044*** (0.012)	-0.033*** (0.012)	-0.035** (0.014)	-0.039*** (0.013)	-0.393*** (0.081)	-0.050*** (0.012)	-0.043*** (0.012)	-0.009** (0.004)	-0.023* (0.012)
Blue-collar x 2016 dummy	-0.061*** (0.013)	-0.050*** (0.013)	-0.039*** (0.015)	-0.056*** (0.014)	-0.312*** (0.094)	-0.056*** (0.014)	-0.060*** (0.013)	-0.011*** (0.004)	-0.054*** (0.014)
Blue-collar x 2017 dummy	-0.063*** (0.014)	-0.042*** (0.013)	-0.039*** (0.015)	-0.058*** (0.015)	-0.314*** (0.097)	-0.059*** (0.014)	-0.063*** (0.014)	-0.014*** (0.005)	-0.063*** (0.014)
Observations	54,246	51,440	54,452	53,556	52,122	54,221	54,452	46,297	52,597
R-squared	0.839	0.888	0.903	0.864	0.837	0.838	0.817	0.987	0.906
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
2-digit industry x year FE	Yes	Yes	Yes	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 and dynamic estimates, respectively. The dependent variable, *TFP*, is the residual from the estimation of a production function, defined in the natural log. A Cobb-Douglas production function is estimated, using blue- and white-collar workers as separate inputs, through the methodology of [Akerberg et al. \(2015\)](#) and controlling for the firm's survival probability ((column (1)), through the methodology of [Wooldridge \(2009\)](#) (column (2)), through the methodology of [Akerberg et al. \(2015\)](#) separately for the pre- and post-periods (column (4)), through the methodology of [Akerberg et al. \(2015\)](#) separately for the pre- and post-periods across blue- and white-collar firms (column (5)), through OLS separately for each year (column (6)), through OLS (column (7)), through OLS and using revenues as output (column (8)), through the methodology of [Akerberg et al. \(2015\)](#) and using imputed labor hours (instead of the number of employees) as inputs (column (9)). In column (3), *TFP* is instead constructed from the estimation of a translog production function, defined in the natural log. *Blue-collar* is a 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* is a dummy variable equal to 1 for observations in the period of 2013 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. Appendix A 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 (2013-2017). Standard errors (in parentheses) are clustered at the firm level. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

F.5. Placebo test: assuming the reform was announced in 2008

Table F.5: The effect of firing costs: A placebo test

	TFP	ln(Separations)	ln(Entering emp.)	ln(Blue- collar emp.)	ln(White- collar emp.)
	(1)	(2)	(3)	(4)	(5)
Blue-collar ('05-'07) x Post ('08-'12)	0.003 (0.007)	-0.008 (0.016)	-0.023 (0.018)	-0.009 (0.009)	0.010 (0.008)
Observations	52,065	48,192	47,278	52,065	52,065
R-squared	0.834	0.783	0.748	0.972	0.973
Firm FE	Yes	Yes	Yes	Yes	Yes
2-digit industry x year FE	Yes	Yes	Yes	Yes	Yes

This table reports difference-in-differences estimates of total factor productivity, employment, and capital over the period of 2005 to 2012. The dependent variables: (1) *TFP*, the residual from the estimation of a Cobb-Douglas production function through the methodology of [Akerberg et al. \(2015\)](#), using blue- and white-collar worker categories as separate inputs, defined in the natural log; (2) *ln(Separations)*, the natural log of the number of employees that left the firm; (3) *ln(Entering emp.)*, the natural log of the number of new hires; (4) *ln(Blue-collar emp.)*, the natural log of the number of blue-collar employees; (5) *ln(White-collar emp.)*, the natural log of the number of white-collar employees. *Blue-collar* is a dummy variable equal to 1 if the firm's average share of blue-collar employees over 2005-2007 was above its sample median, and to 0 otherwise. *Post* is a dummy variable equal to 1 for observations in the period of 2008 to 2012, and to 0 for observations in the period of 2005 to 2007. Appendix A 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.

F.6. Defining treatment based on 2008

Table F.6: The effect of firing costs: Defining treatment based on 2008

	TFP	ln(Separations)	ln(Entering emp.)	ln(Blue- collar emp.)	ln(White- collar emp.)
	(1)	(2)	(3)	(4)	(5)
Blue-collar ('08) x Post	-0.032*** (0.009)	-0.067*** (0.019)	-0.054** (0.021)	-0.020* (0.011)	0.047*** (0.011)
Observations	41,555	38,837	37,786	41,555	41,555
R-squared	0.844	0.763	0.728	0.964	0.967
Firm FE	Yes	Yes	Yes	Yes	Yes
2-digit industry x year FE	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 dependent variables: (1) *TFP*, the residual from the estimation of a Cobb-Douglas production function through the methodology of [Akerberg et al. \(2015\)](#), using blue- and white-collar worker categories as separate inputs, defined in the natural log; (2) *ln(Separations)*, the natural log of the number of employees that left the firm; (3) *ln(Entering emp.)*, the natural log of the number of new hires; (4) *ln(Blue-collar emp.)*, the natural log of the number of blue-collar employees; (5) *ln(White-collar emp.)*, the natural log of the number of white-collar employees. *Blue-collar* is a dummy variable equal to 1 if the firm's share of blue-collar employees in 2008 was above its sample median, and to 0 otherwise. *Post* is a dummy variable equal to 1 for observations in the period of 2013 to 2017, and to 0 for observations in the period of 2009 to 2012. Appendix A 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.

F.7. The effect on profits

Table F.7: The effect of firing costs on profits

	ln(EBITDA)		EBITDA-to-assets		EBITDA-to-sales	
	(1)	(2)	(3)	(4)	(5)	(6)
Blue-collar x Post	-0.021 (0.018)		-0.001 (0.002)		-0.001 (0.001)	
Blue-collar x 2009 dummy		0.029 (0.029)		0.006* (0.003)		0.004** (0.002)
Blue-collar x 2010 dummy		-0.009 (0.026)		0.001 (0.003)		0.002 (0.002)
Blue-collar x 2011 dummy		-0.018 (0.023)		-0.000 (0.002)		0.000 (0.001)
Blue-collar x 2013 dummy		-0.008 (0.023)		-0.001 (0.002)		-0.000 (0.001)
Blue-collar x 2014 dummy		-0.015 (0.025)		0.003 (0.002)		0.001 (0.002)
Blue-collar x 2015 dummy		-0.006 (0.027)		0.004 (0.003)		0.002 (0.002)
Blue-collar x 2016 dummy		-0.071** (0.031)		-0.003 (0.003)		-0.001 (0.002)
Blue-collar x 2017 dummy		-0.023 (0.033)		-0.004 (0.003)		-0.002 (0.002)
Observations	49,923	49,923	54,452	54,452	54,452	54,452
R-squared	0.860	0.860	0.649	0.649	0.784	0.784
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 profits over the period of 2009 to 2017. The dependent variables: (1)-(2) $\ln(EBITDA)$, the natural log of EBITDA; (3)-(4) $EBITDA\text{-}to\text{-}assets$, the ratio of EBITDA to total assets; (5)-(6) $EBITDA\text{-}to\text{-}sales$, the ratio of EBITDA to sales. *Blue-collar* is a dummy variable equal to 1 if the firm's share of blue-collar employees in 2008 was above its sample median, and to 0 otherwise. *Post* is a dummy variable equal to 1 for observations in the period of 2013 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. Appendix A 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.

F.8. Less versus more affected industries

Table F.8: The effect of firing costs: Less versus more affected industries

	less affec.	more affec.	less affec.	more affec.	less affec.	more affec.	less affec.	more affec.
	TFP		ln(Separations)		ln(Entering emp.)		ln(Blue-collar emp.)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Blue-collar x Post	-0.034** (0.015)	-0.048*** (0.010)	-0.027 (0.030)	-0.066*** (0.020)	-0.063* (0.034)	-0.052** (0.022)	-0.012 (0.017)	-0.021* (0.013)
Observations	13,840	40,450	13,003	37,333	12,620	36,549	13,840	40,450
R-squared	0.887	0.792	0.771	0.774	0.734	0.743	0.973	0.962
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
2-digit industry x year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

(Continued)

	less affec.	more affec.	less affec.	more affec.	less affec.	more affec.	less affec.	more affec.
	ln(White-collar emp.)		ln(Tangible fix.)		ln(Mac. Equip.)		ln(Land. Build. Furn. Other)	
	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
Blue-collar x Post	0.016 (0.016)	0.063*** (0.012)	0.025 (0.037)	0.021 (0.023)	-0.036 (0.045)	0.019 (0.034)	-0.007 (0.046)	0.035 (0.027)
Observations	13,840	40,450	13,840	40,450	13,524	38,249	13,726	40,261
R-squared	0.976	0.962	0.941	0.921	0.920	0.891	0.915	0.898
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 total factor productivity, employment, and capital over the period of 2009 to 2017. The dependent variables: (1)-(2) *TFP*, the residual from the estimation of a Cobb-Douglas production function through the methodology of [Akerberg et al. \(2015\)](#), using blue- and white-collar worker categories as separate inputs, defined in the natural log; (3)-(4) *ln(Separations)*, the natural log of the number of employees that left the firm; (5)-(6) *ln(Entering emp.)*, the natural log of the number of new hires; (7)-(8) *ln(Blue-collar emp.)*, the natural log of the number of blue-collar employees; (9)-(10) *ln(White-collar emp.)*, the natural log of the number of white-collar employees; (11)-(12) *ln(Tangible fix.)*, the natural log of total tangible fixed assets; (13)-(14) *ln(Mac., Equip.)*, the natural log of the sum of machinery and equipment; (15)-(16) *ln(Land, Build., Furn., Other)*, 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 average share of blue-collar employees was above its sample median, and to 0 otherwise. *Post* is a dummy variable equal to 1 for observations in the period of 2013 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 a 10-year tenure increased by less than 168 days (by 168 days or more). Appendix A 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 (2013-2017). Standard errors (in parentheses) are clustered at the firm level. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

F.9. Results across less versus more R&D intensive industries

Table F.9: The effect of firing costs: Heterogeneity across less versus more R&D intensive industries

	TFP	Ln(Separations)	Ln(Entering emp.)	Ln(Blue-collar emp.)	Ln(White-collar emp.)	Ln(Tangible fix.)	Ln(Mac. Equip.)	Ln(Land Build. Furn. Other)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Blue-collar x Post	-0.049*** (0.011)	-0.045** (0.022)	-0.038 (0.023)	-0.023* (0.014)	0.072*** (0.013)	0.042 (0.026)	0.026 (0.038)	0.051* (0.031)
Blue-collar x Post x High R&D	0.006 (0.016)	-0.031 (0.033)	-0.052 (0.037)	0.014 (0.020)	-0.049*** (0.018)	-0.056 (0.038)	-0.063 (0.053)	-0.071 (0.046)
Observations	54,452	50,528	49,371	54,452	54,452	54,452	51,939	54,150
R-squared	0.826	0.769	0.736	0.965	0.966	0.926	0.903	0.901
Blue-collar x High R&D (accounting for firms changing industries)	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 total factor productivity, employment, and capital over the period of 2009 to 2017. The dependent variables: (1) *TFP*, the residual from the estimation of a Cobb-Douglas production function through the methodology of [Akerberg et al. \(2015\)](#), using blue- and white-collar worker categories as separate inputs, defined in the natural log; (2) *ln(Separations)*, the natural log of the number of employees that left the firm; (3) *ln(Entering emp.)*, the natural log of the number of new hires; (4) *ln(Blue-collar emp.)*, the natural log of the number of blue-collar employees; (5) *ln(White-collar emp.)*, the natural log of the number of white-collar employees; (6) *ln(Tangible fix.)*, the natural log of total tangible fixed assets; (7) *ln(Mac., Equip.)*, the natural log of the sum of machinery and equipment; (8) *ln(Land, Build., Furn., Other)*, 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 employees was above its sample median, and to 0 otherwise. *Post* is a dummy variable equal to 1 for observations in the period of 2013 to 2017, and to 0 for observations in the period of 2009 to 2012. *High R&D intensive* is equal to 1 if the industry's R&D intensity score, based on the 2011 OECD taxonomy ([Galindo-Rueda and Verger, 2016](#)), is above its sample median, and to 0 otherwise. Appendix A 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 (2013-2017). Standard errors (in parentheses) are clustered at the firm level. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

F.10. Treating outsourced labor as a production input

Table F.10: Firing costs and productivity: Incorporating outsourced workers as an input

	Incorporating outsourced emp. in line with blue/white emp. ratio		Incorporating outsourced emp. as blue emp.		Incorporating outsourced emp. as a separate category using $\ln(1 + Outsourced\ emp.)$	
	TFP	TFP	TFP	TFP	TFP	TFP
	(1)	(2)	(3)	(4)	(5)	(6)
Blue-collar x Post	-0.048*** (0.008)		-0.038*** (0.008)		-0.045*** (0.008)	
Blue-collar x 2009 dummy		0.013 (0.013)		0.007 (0.013)		0.014 (0.013)
Blue-collar x 2010 dummy		-0.007 (0.011)		-0.008 (0.011)		-0.005 (0.011)
Blue-collar x 2011 dummy		-0.008 (0.009)		-0.007 (0.009)		-0.005 (0.009)
Blue-collar x 2013 dummy		-0.040*** (0.009)		-0.035*** (0.008)		-0.036*** (0.008)
Blue-collar x 2014 dummy		-0.042*** (0.010)		-0.033*** (0.010)		-0.037*** (0.010)
Blue-collar x 2015 dummy		-0.043*** (0.012)		-0.034*** (0.011)		-0.041*** (0.011)
Blue-collar x 2016 dummy		-0.062*** (0.013)		-0.050*** (0.013)		-0.058*** (0.013)
Blue-collar x 2017 dummy		-0.067*** (0.014)		-0.056*** (0.013)		-0.062*** (0.013)
Observations	54,348	54,348	54,451	54,451	54,176	54,176
R-squared	0.842	0.842	0.845	0.845	0.847	0.847
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 dependent variable, *TFP*, is the residual from the estimation of a Cobb-Douglas production function, defined in the natural log. When estimating the production function, blue- and white-collar worker categories are used as separate inputs, and outsourced workers are incorporated into the employment input as follows: In columns (1) and (2), outsourced workers are allocated between blue- and white-collar categories according to the firm's blue-/white-collar employment ratio in a given year. In columns (3) and (4), all outsourced workers are assumed to be blue-collar. In columns (5) and (6), outsourced workers are included as a separate input using $\ln(1 + Outsourced\ emp.)$. *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 2013 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. Appendix A 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 (2013-2017). Standard errors (in parentheses) are clustered at the firm level. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

F.11. 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 restrict the sample to firms that employ at least one blue-collar worker, at least one white-collar worker, and for which data on intermediate inputs are available. To ensure that our results are not sensitive to this selection, Table F.11 conducts our main analysis using three alternative samples.

Incorporating firms with only one worker type. In columns (1) and (2), we allow firms operating with only one worker type to be included in the sample. For this sample, we recover TFP through estimating the production function using $\ln(1+\text{blue-collar workers})$ and $\ln(1+\text{white-collar workers})$ as labor inputs.

Incorporating firms with missing intermediate inputs. In columns (3) and (4), while still focusing on firms operating with at least one blue-collar worker and at least one white-collar worker, we incorporate firms with missing intermediate inputs into our sample. Because intermediate inputs are typically reported by larger firms in Belgium, this inclusion allows us to incorporate smaller firms into the analysis. For this sample, we construct TFP by estimating our baseline production function through OLS (rather than following the approach of Akerberg et al. (2015) and using intermediate inputs as a proxy).

Incorporating both firms with only one worker type and those with missing intermediate inputs. In columns (5) and (6), we run our analysis using a sample that includes both firms employing only one type of worker and firms with missing intermediate inputs. For this sample, we use TFP from an OLS estimation of the production function with $\ln(1+\text{blue-collar emp.})$ and $\ln(1+\text{white-collar emp.})$.

The results indicate that our main effects are robust across all sample specifications. In fact, the estimated effects on blue- and white-collar workers are even stronger when smaller firms are included. This may reflect the greater internal flexibility of smaller firms, which could enable them to adjust their employment composition more aggressively in response to increased firing costs.

Table F.11: The effect of firing costs: Incorporating firms with only one worker type as well as firms with missing intermediate inputs

	TFP	ln(Separations)	ln(Entering emp.)	ln(Blue-collar emp.)	ln(White-collar emp.)	ln(Tangible fix.)	ln(Mac., Equip.)	ln(Land, Build., Furn., Other)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. Incorporating firms with only one worker type								
Blue-collar x Post	-0.047*** (0.009)	-0.057*** (0.016)	-0.057*** (0.017)	-0.016 (0.011)	0.039*** (0.009)	0.014 (0.020)	-0.008 (0.026)	0.015 (0.023)
Observations	72,736	65,601	63,983	58,064	67,548	72,736	65,609	72,043
R-squared	0.849	0.764	0.735	0.960	0.961	0.926	0.903	0.903
B. Incorporating firms with missing intermediate inputs								
Blue-collar x Post	-0.044*** (0.005)	-0.040*** (0.010)	-0.046*** (0.011)	-0.044*** (0.006)	0.086*** (0.006)	0.002 (0.012)	0.002 (0.017)	0.001 (0.014)
Observations	161,858	139,152	136,058	161,858	161,858	161,858	153,445	160,782
R-squared	0.773	0.712	0.689	0.950	0.949	0.903	0.869	0.879
C. Incorporating both firms with only one worker type and firms with missing intermediate inputs								
Blue-collar x Post	-0.038*** (0.005)	-0.027*** (0.009)	-0.033*** (0.010)	-0.043*** (0.006)	0.089*** (0.006)	0.004 (0.011)	-0.020 (0.015)	0.008 (0.013)
Observations	268,285	225,199	220,278	222,994	215,601	268,285	246,123	265,332
R-squared	0.796	0.698	0.681	0.931	0.947	0.893	0.860	0.869
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 total factor productivity, employment, and capital over the period of 2009 to 2017. The dependent variables: (1) *TFP*, the residual from the estimation of a Cobb-Douglas production function, using the levels of blue- and white-collar workers as separate inputs, defined in the natural log; (2) *ln(Separations)*, the natural log of the number of employees that left the firm; (3) *ln(Entering emp.)*, the natural log of the number of new hires; (4) *ln(Blue-collar emp.)*, the natural log of the number of blue-collar employees; (5) *ln(White-collar emp.)*, the natural log of the number of white-collar employees; (6) *ln(Tangible fix.)*, the natural log of total tangible fixed assets; (7) *ln(Mac., Equip.)*, the natural log of the sum of machinery and equipment; (8) *ln(Land, Build., Furn., Other)*, the natural log of the sum of land, building, furniture, and other tangible fixed capital. TFP is constructed as follows. In columns (1) and (2), the production function is estimated through the methodology of [Akerberg et al. \(2015\)](#) and using *ln(1+blue-collar emp.)* and *ln(1+white-collar emp.)* as labor 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 emp.)* and *ln(1+white-collar emp.)* as labor 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 2013 to 2017, and to 0 for observations in the period of 2009 to 2012. Appendix A 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 (2013-2017). Standard errors (in parentheses) are clustered at the firm level. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

F.12. Results with controls

Table F.12: The effect of firing costs: Results with controls

	ln(Separations)	ln(Entering emp.)	ln(Blue- collar emp.)	ln(White- collar emp.)	ln(Tangible fix.)	ln(Mac., Equip.)	ln(Land, Build., Furn., Other)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Blue-collar x Post	-0.055*** (0.017)	-0.056*** (0.018)	-0.015 (0.010)	0.054*** (0.009)	0.015 (0.012)	-0.002 (0.025)	0.017 (0.017)
Observations	50,528	49,371	54,452	54,452	54,452	51,939	54,150
R-squared	0.775	0.738	0.967	0.969	0.953	0.912	0.925
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
2-digit industry x year FE	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, with control variables. The dependent variables: (1) $\ln(\text{Separations})$, the natural log of the number of employees that left the firm; (2) $\ln(\text{Entering emp.})$, the natural log of the number of new hires; (3) $\ln(\text{Blue-collar emp.})$, the natural log of the number of blue-collar employees; (4) $\ln(\text{White-collar emp.})$, the natural log of the number of white-collar employees; (5) $\ln(\text{Tangible fix.})$, the natural log of total tangible fixed assets; (6) $\ln(\text{Mac., Equip.})$, the natural log of the sum of machinery and equipment; (7) $\ln(\text{Land, Build., Furn., Other})$, 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 employees was above its sample median, and to 0 otherwise. *Post* is a dummy variable equal to 1 for observations in the period of 2013 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} , $\text{EBITDA-to-assets}_{t-1}$, $\text{Cash holdings}_{t-1}$, and $\text{Capital-to-labor}_{t-1}$. Appendix A 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 (2013-2017). Standard errors (in parentheses) are clustered at the firm level. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

F.13. Results with the matched samples

We create two matched samples with the aim of making blue- and white-collar firms as comparable as possible in terms of their observed pre-period characteristics.

F.13.1. Matching strategy 1

Matching strategy. To create the first matched sample, 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 (*size*, *age*, *leverage*, *cash holdings*, *capital-to-labor*) among our baseline firm controls. Additionally, we include the log of the total number of workers, 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 labor 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.⁶ 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).

Summary statistics. In total, we are able to successfully match 3,375 (around 98%) of the 3,451 blue-collar firms to white-collar firms. Table F.13.1 compares the pre-period sample means for blue- and white-collar firms before- and after the matching procedure. We assess the quality of matching by performing t-tests as well as by looking at standardized differences in means of the control variables. An absolute value of standardized mean difference that is less than 0.1 is considered to imply a negligible imbalance between samples. According to the t-tests, the two groups of firms significantly differ in covariates before the matching procedure. The standardized mean differences indicate a non-negligible imbalance for *size*, *EBITDA-to-assets*, and *capital-to-labor* between the non-matched blue- and white-collar samples. After the matching procedure, according to both the t-tests and 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

⁶Here, as well for matching strategy 2 below, our results hold when we retain two or three white-collar firms for each blue-collar firm.

non-matched sample.

Results. Using the matched sample, Panels A and B of Table F.13.2 present the main and dynamic estimates, respectively, for our main outcomes based on the baseline regression. The estimated effects on TFP and employment are qualitatively similar to our baseline results. The coefficient for tangible fixed capital, however, is noticeably larger in magnitude than our baseline estimate. Importantly, this increase in capital appears to be driven by land, buildings, furniture, and other non-machinery assets, rather than by machines and equipment. This finding is consistent with our main results based on the non-matched sample and suggests that the rise in firing costs did not lead to capital deepening.

F.13.2. Matching strategy 2

Matching strategy. We construct the second matched sample using the same strategy as for the first (i.e., nearest neighbor matching of propensity scores). The only difference is that, in this case, we match firms based only on their pre-period TFP average to more directly account for potential systematic differences in productivity between blue- and white-collar firms. As before, we require exact matches within NACE 2-digit industries, 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.

Summary statistics & results. Using the second strategy, we successfully match nearly all blue-collar firms to white-collar counterparts (3,438 out of 3,451). Table F.13.3 demonstrates that this matching approach leads to an overall improvement in covariate balance between the two groups. Panels A and B of Table F.13.4 illustrate the main and dynamic estimates, respectively, for our key outcomes. The results from this matched sample are broadly consistent with those from the non-matched sample and reveal even larger effects on the reduction in worker turnover and shifts in employment composition.

Table F.13.1: Comparing pre-period sample means for blue-collar and white-collar firms: Matched versus Non-Matched Sample (1)

	Non-matched sample						Matched sample					
	Blue-collar firms		White-collar firms				Blue-collar firms		White-collar firms			
	N	Mean	N	Mean	Diff.	SMD	N	Mean	N	Mean	Diff.	SMD
Dependent variables												
TFP	12,173	6.562	12,230	6.524	0.038***	0.065	12,036	6.563	12,129	6.456	0.107***	0.193
ln(Separations)	11,411	2.125	11,298	1.928	0.197***	0.175	11,296	2.128	11,371	2.126	0.002	0.002
ln(Entering emp.)	11,141	2.122	11,233	1.952	0.170***	0.148	11,017	2.125	11,364	2.104	0.021	0.018
ln(Blue-collar emp.)	12,173	3.506	12,230	2.392	1.115***	0.934	12,036	3.516	12,129	2.914	0.602***	0.494
ln(White-collar emp.)	12,173	1.949	12,230	2.881	-0.932***	-0.795	12,036	1.962	12,129	3.033	-1.071***	-0.884
ln(Total emp.)	12,173	3.737	12,230	3.483	0.254***	0.235	12,036	3.747	12,129	3.775	-0.029	-0.026
ln(Tangible fix.)	12,173	13.894	12,230	13.832	0.062	0.037	12,036	13.904	12,129	13.857	0.047	0.028
ln(Mac., Equip.)	11,696	12.197	11,678	11.823	0.375***	0.170	11,570	12.207	11,632	12.130	0.078	0.035
ln(Land, Build., Furn., Other)	12,107	13.404	12,178	13.431	-0.027	-0.015	11,970	13.414	12,095	13.367	0.047	0.026
Treatment												
(%) Blue-collar	12,173	0.799	12,230	0.405	0.394***	2.843	12,036	0.799	12,129	0.474	0.325***	2.584
Controls												
Size	12,173	15.654	12,230	15.979	-0.325***	-0.232	12,036	15.666	12,129	15.753	-0.088	-0.061
Age	12,173	27.676	12,230	29.136	-1.460***	-0.084	12,036	27.762	12,129	26.617	1.145	0.069
Leverage	12,173	0.592	12,230	0.607	-0.015**	-0.059	12,036	0.592	12,129	0.601	-0.009	-0.038
EBITDA-to-assets	12,173	0.119	12,230	0.106	0.013***	0.128	12,036	0.119	12,129	0.116	0.003	0.027
Cash holdings	12,173	0.090	12,230	0.095	-0.005*	-0.042	12,036	0.089	12,129	0.089	0.001	0.005
Capital-to-labour	12,173	10.157	12,230	10.349	-0.192***	-0.141	12,036	10.157	12,129	10.082	0.075	0.057

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 average share of blue-collar workers was above (below) its sample median. *, **, 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 $(Mean_{blue-collar} - Mean_{white-collar}) / \sqrt{(sd_{blue-collar}^2 + sd_{white-collar}^2) / 2}$. Appendix A defines all variables.

Table F.13.2: The effect of firing costs: Results with the matched sample (1)

A. Main estimates

	TFP	ln(Separations)	ln(Entering emp.)	ln(Blue- collar emp.)	ln(White- collar emp.)	ln(Tangible fix.)	ln(Mac., Equip.)	ln(Land, Build., Furn., Other)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Blue-collar x Post	-0.058*** (0.013)	-0.056* (0.031)	-0.052* (0.028)	-0.025 (0.016)	0.087*** (0.016)	0.096*** (0.033)	0.047 (0.051)	0.087** (0.043)
Observations	53,461	49,904	48,817	53,461	53,461	53,461	51,109	53,147
R-squared	0.824	0.784	0.752	0.963	0.969	0.928	0.907	0.903
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
2-digit industry x year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

(Continued)

B. Dynamic estimates

	TFP	ln(Separations)	ln(Entering emp.)	ln(Blue-collar emp.)	ln(White-collar emp.)	ln(Tangible fix.)	ln(Mac., Equip.)	ln(Land, Build., Furn., Other)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Blue-collar x 2009 dummy	0.016 (0.017)	-0.006 (0.047)	-0.076 (0.048)	-0.042 (0.033)	0.001 (0.017)	-0.064 (0.055)	-0.076 (0.060)	-0.053 (0.065)
Blue-collar x 2010 dummy	-0.004 (0.016)	-0.059 (0.045)	-0.032 (0.046)	-0.036 (0.024)	-0.019 (0.015)	-0.050 (0.039)	-0.033 (0.057)	-0.045 (0.046)
Blue-collar x 2011 dummy	-0.006 (0.013)	0.010 (0.052)	-0.002 (0.040)	-0.016* (0.009)	-0.005 (0.012)	-0.020 (0.019)	-0.017 (0.040)	-0.022 (0.026)
Blue-collar x 2013 dummy	-0.038*** (0.013)	-0.043 (0.029)	-0.064 (0.043)	-0.034*** (0.011)	0.041*** (0.011)	0.027 (0.019)	0.001 (0.038)	0.037 (0.027)
Blue-collar x 2014 dummy	-0.046*** (0.016)	-0.017 (0.038)	-0.131*** (0.038)	-0.046*** (0.014)	0.050*** (0.017)	0.024 (0.038)	-0.022 (0.052)	0.031 (0.053)
Blue-collar x 2015 dummy	-0.060*** (0.018)	-0.103** (0.044)	-0.045 (0.044)	-0.040** (0.019)	0.097*** (0.020)	0.049 (0.046)	0.067 (0.094)	0.017 (0.048)
Blue-collar x 2016 dummy	-0.083*** (0.021)	-0.128*** (0.048)	-0.082* (0.046)	-0.047** (0.020)	0.117*** (0.020)	0.119*** (0.041)	0.048 (0.076)	0.098** (0.048)
Blue-collar x 2017 dummy	-0.070*** (0.021)	-0.074 (0.051)	-0.042 (0.056)	-0.072*** (0.024)	0.134*** (0.024)	0.156*** (0.051)	0.009 (0.086)	0.156*** (0.060)
Observations	53,461	49,904	48,817	53,461	53,461	53,461	51,109	53,147
R-squared	0.824	0.784	0.752	0.963	0.969	0.928	0.907	0.903
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 total factor productivity, employment, and capital over the period of 2009 to 2017, using a matched sample. In the matched sample, we retain one white-collar firm for each blue-collar firm. The dependent variables: (1) *TFP*, the residual from the estimation of a Cobb-Douglas production function through the methodology of [Akerberg et al. \(2015\)](#), using blue- and white-collar worker categories as separate inputs, defined in the natural log; (2) *ln(Separations)*, the natural log of the number of employees that left the firm; (3) *ln(Entering emp.)*, the natural log of the number of new hires; (4) *ln(Blue-collar emp.)*, the natural log of the number of blue-collar employees; (5) *ln(White-collar emp.)*, the natural log of the number of white-collar employees; (6) *ln(Tangible fix.)*, the natural log of total tangible fixed assets; (7) *ln(Mac., Equip.)*, the natural log of the sum of machinery and equipment; (8) *ln(Land, Build., Furn., Other)*, 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 2013 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. Appendix A 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 (2013-2017). Standard errors (in parentheses) are clustered at the firm level. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Table F.13.3: Comparing pre-period sample means for blue-collar and white-collar firms: Matched versus Non-Matched Sample (2)

	Non-matched sample						Matched sample					
	Blue-collar firms		White-collar firms				Blue-collar firms		White-collar firms			
	N	Mean	N	Mean	Diff.	SMD	N	Mean	N	Mean	Diff.	SMD
Dependent variables												
TFP	12,173	6.562	12,230	6.524	0.038***	0.065	12,128	6.563	12,183	6.562	0.001	0.001
ln(Separations)	11,411	2.125	11,298	1.928	0.197***	0.175	11,376	2.125	11,161	1.962	0.162***	0.142
ln(Entering emp.)	11,141	2.122	11,233	1.952	0.170***	0.148	11,103	2.122	11,166	1.959	0.162***	0.141
ln(Blue-collar emp.)	12,173	3.506	12,230	2.392	1.115***	0.934	12,128	3.508	12,183	2.698	0.810***	0.679
ln(White-collar emp.)	12,173	1.949	12,230	2.881	-0.932***	-0.795	12,128	1.952	12,183	2.856	-0.904***	-0.756
ln(Total emp.)	12,173	3.737	12,230	3.483	0.254***	0.235	12,128	3.738	12,183	3.573	0.165***	0.149
ln(Tangible fix.)	12,173	13.894	12,230	13.832	0.062	0.037	12,128	13.898	12,183	13.852	0.046	0.027
ln(Mac., Equip.)	11,696	12.197	11,678	11.823	0.375***	0.170	11,655	12.201	11,729	11.970	0.231**	0.105
ln(Land, Build., Furn., Other)	12,107	13.404	12,178	13.431	-0.027	-0.015	12,061	13.409	12,116	13.385	0.024	0.013
Treatment												
(%) Blue-collar	12,173	0.799	12,230	0.405	0.394***	2.843	12,128	0.799	12,183	0.467	0.332***	2.642
Controls												
Size	12,173	15.654	12,230	15.979	-0.325***	-0.232	12,128	15.657	12,183	15.996	-0.339***	-0.238
Age	12,173	27.676	12,230	29.136	-1.460***	-0.084	12,128	27.700	12,183	29.918	-2.218**	-0.127
Leverage	12,173	0.592	12,230	0.607	-0.015**	-0.059	12,128	0.592	12,183	0.610	-0.018	-0.072
EBITDA-to-assets	12,173	0.119	12,230	0.106	0.013***	0.128	12,128	0.119	12,183	0.112	0.007	0.062
Cash holdings	12,173	0.090	12,230	0.095	-0.005*	-0.042	12,128	0.089	12,183	0.096	-0.007	-0.060
Capital-to-labour	12,173	10.157	12,230	10.349	-0.192***	-0.141	12,128	10.160	12,183	10.279	-0.119**	-0.086

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 average share of blue-collar workers was above (below) its sample median. *, **, 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 $(Mean_{blue-collar} - Mean_{white-collar}) / \sqrt{(sd_{blue-collar}^2 + sd_{white-collar}^2) / 2}$. Appendix A defines all variables.

Table F.13.4: The effect of firing costs: Results with the matched sample (2)

A. Main estimates

	TFP	ln(Separations)	ln(Entering emp.)	ln(Blue- collar emp.)	ln(White- collar emp.)	ln(Tangible fix.)	ln(Mac., Equip.)	ln(Land, Build., Furn., Other)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Blue-collar x Post	-0.048*** (0.012)	-0.069*** (0.026)	-0.098*** (0.027)	-0.035** (0.016)	0.059*** (0.014)	0.066* (0.037)	0.015 (0.044)	0.061 (0.043)
Observations	54,274	50,191	49,148	54,274	54,274	54,274	51,885	53,925
R-squared	0.813	0.773	0.738	0.963	0.965	0.925	0.900	0.904
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
2-digit industry x year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

(Continued)

B. Dynamic estimates

	TFP	ln(Separations)	ln(Entering emp.)	ln(Blue-collar emp.)	ln(White-collar emp.)	ln(Tangible fix.)	ln(Mac., Equip.)	ln(Land, Build., Furn., Other)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Blue-collar x 2009 dummy	0.025 (0.016)	-0.085** (0.042)	-0.040 (0.049)	0.016 (0.017)	0.018 (0.016)	0.000 (0.043)	-0.043 (0.060)	0.022 (0.051)
Blue-collar x 2010 dummy	-0.007 (0.016)	-0.020 (0.037)	0.011 (0.043)	0.018 (0.014)	0.002 (0.013)	0.005 (0.037)	-0.023 (0.056)	0.030 (0.043)
Blue-collar x 2011 dummy	0.002 (0.014)	-0.025 (0.033)	-0.007 (0.037)	0.012 (0.009)	-0.004 (0.009)	-0.000 (0.026)	-0.048 (0.030)	0.007 (0.033)
Blue-collar x 2013 dummy	-0.037*** (0.012)	-0.065* (0.033)	-0.067* (0.038)	-0.011 (0.013)	0.032*** (0.008)	0.039** (0.018)	-0.017 (0.031)	0.056** (0.022)
Blue-collar x 2014 dummy	-0.041*** (0.014)	-0.092*** (0.033)	-0.163*** (0.043)	-0.025* (0.015)	0.052*** (0.016)	0.064** (0.027)	-0.032 (0.040)	0.085** (0.040)
Blue-collar x 2015 dummy	-0.025 (0.019)	-0.119*** (0.041)	-0.058 (0.045)	-0.006 (0.018)	0.082*** (0.017)	0.061* (0.035)	0.014 (0.053)	0.044 (0.037)
Blue-collar x 2016 dummy	-0.063*** (0.018)	-0.104** (0.044)	-0.130*** (0.042)	-0.029 (0.019)	0.076*** (0.018)	0.106** (0.042)	-0.003 (0.059)	0.120*** (0.046)
Blue-collar x 2017 dummy	-0.059*** (0.018)	-0.126*** (0.044)	-0.116** (0.047)	-0.062*** (0.023)	0.082*** (0.021)	0.082** (0.039)	-0.015 (0.061)	0.077 (0.051)
Observations	54,274	50,191	49,148	54,274	54,274	54,274	51,885	53,925
R-squared	0.813	0.773	0.738	0.963	0.965	0.926	0.900	0.904
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 total factor productivity, employment, and capital over the period of 2009 to 2017, using a matched sample. In the matched sample, we retain one white-collar firm for each blue-collar firm. The dependent variables: (1) *TFP*, the residual from the estimation of a Cobb-Douglas production function through the methodology of Akerberg et al. (2015), using blue- and white-collar worker categories as separate inputs, defined in the natural log; (2) *ln(Separations)*, the natural log of the number of employees that left the firm; (3) *ln(Entering emp.)*, the natural log of the number of new hires; (4) *ln(Blue-collar emp.)*, the natural log of the number of blue-collar employees; (5) *ln(White-collar emp.)*, the natural log of the number of white-collar employees; (6) *ln(Tangible fix.)*, the natural log of total tangible fixed assets; (7) *ln(Mac., Equip.)*, the natural log of the sum of machinery and equipment; (8) *ln(Land, Build., Furn., Other)*, 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 2013 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. Appendix A 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 (2013-2017). Standard errors (in parentheses) are clustered at the firm level. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

F.14. Results with treatment intensity

Table F.14: The effect of firing costs: Results with treatment intensity

	TFP	ln(Separations)	ln(Entering emp.)	ln(Blue-collar emp.)	ln(White-collar emp.)	ln(Tangible fix.)	ln(Mac., Equip.)	ln(Land, Build., Furn., Other)
	(1)	(3)	(2)	(4)	(5)	(6)	(7)	(8)
(%) Blue-collar x Post	-0.114*** (0.020)	-0.094*** (0.036)	-0.128*** (0.039)	-0.016 (0.026)	0.149*** (0.021)	0.040 (0.044)	-0.035 (0.062)	0.061 (0.051)
Observations	54,452	50,528	49,371	54,452	54,452	54,452	51,939	54,150
R-squared	0.826	0.769	0.736	0.965	0.966	0.926	0.903	0.901
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 total factor productivity, employment, and capital over the period of 2009 to 2017. The dependent variables: (1) *TFP*, the residual from the estimation of a Cobb-Douglas production function through the methodology of [Akerberg et al. \(2015\)](#), using blue- and white-collar worker categories as separate inputs, defined in the natural log; (2) *ln(Separations)*, the natural log of the number of employees that left the firm; (3) *ln(Entering emp.)*, the natural log of the number of new hires; (4) *ln(Blue-collar emp.)*, the natural log of the number of blue-collar employees; (5) *ln(White-collar emp.)*, the natural log of the number of white-collar employees; (6) *ln(Tangible fix.)*, the natural log of total tangible fixed assets; (7) *ln(Mac., Equip.)*, the natural log of the sum of machinery and equipment; (8) *ln(Land, Build., Furn., Other)*, 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 2013 to 2017, and to 0 for observations in the period of 2009 to 2012. Appendix A 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 (2013-2017). Standard errors (in parentheses) are clustered at the firm level. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

F.15. Linear effects of firing costs

Table F.15: The effect of firing costs: Linear effects

	TFP	ln(Separations)	ln(Entering emp.)	ln(Blue-collar emp.)	ln(White-collar emp.)	ln(Tangible fix.)	ln(Mac., Equip.)	ln(Land, Build., Furn., Other)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Q2 (%) Blue-collar x Post	-0.009 (0.011)	-0.001 (0.021)	-0.019 (0.023)	-0.003 (0.014)	0.014 (0.011)	-0.009 (0.025)	-0.041 (0.035)	0.004 (0.029)
Q3 (%) Blue-collar x Post	-0.039*** (0.012)	-0.062*** (0.023)	-0.045* (0.025)	-0.022 (0.015)	0.037*** (0.013)	0.011 (0.027)	-0.020 (0.037)	0.019 (0.032)
Q4 (%) Blue-collar x Post	-0.072*** (0.013)	-0.052** (0.025)	-0.103*** (0.027)	-0.014 (0.016)	0.093*** (0.014)	0.021 (0.029)	-0.028 (0.040)	0.033 (0.034)
Observations	54,452	50,528	49,371	54,452	54,452	54,452	51,939	54,150
R-squared	0.826	0.769	0.736	0.965	0.966	0.926	0.903	0.901
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 total factor productivity, employment and capital over the period of 2009 to 2017. The dependent variables: (1) *TFP*, the residual from the estimation of a Cobb-Douglas production function through the methodology of [Akerberg et al. \(2015\)](#), using blue- and white-collar worker categories as separate inputs, defined in the natural log; (2) *ln(Separations)*, the natural log of the number of employees that left the firm; (3) *ln(Entering emp.)*, the natural log of the number of new hires; (4) *ln(Blue-collar emp.)*, the natural log of the number of blue-collar employees; (5) *ln(White-collar emp.)*, the natural log of the number of white-collar employees; (6) *ln(Tangible fix.)*, the natural log of total tangible fixed assets; (7) *ln(Mac., Equip.)*, the natural log of the sum of machinery and equipment; (8) *ln(Land, Build., Furn., Other)*, the natural log of the sum of land, building, furniture, and other tangible fixed capital. *Q_i* (%) Blue-collar is a dummy equal to 1 if the firm belongs to the *i*th 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 2013 to 2017, and to 0 for observations in the period of 2009 to 2012. Appendix A 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 (2013-2017). Standard errors (in parentheses) are clustered at the firm level. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

F.16. Results across industries with larger declines in blue-collar employment share

Table F.16: The effect of firing costs: Results across industries with larger declines in blue-collar employment share

	TFP	ln(Separations)	ln(Entering emp.)	ln(Blue-collar emp.)	ln(White-collar emp.)	ln(Tangible fix.)	ln(Mac., Equip.)	ln(Land, Build., Furn., Other)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Blue-collar x Post	-0.043*** (0.015)	-0.051* (0.028)	-0.093*** (0.031)	0.004 (0.019)	0.071*** (0.016)	0.038 (0.035)	0.010 (0.046)	0.023 (0.042)
Blue-collar x Post x High %blue-collar decline industry	-0.005 (0.018)	-0.010 (0.035)	0.054 (0.038)	-0.033 (0.022)	-0.028 (0.020)	-0.027 (0.042)	-0.014 (0.056)	0.000 (0.050)
Observations	54,452	50,528	49,371	54,452	54,452	54,452	51,939	54,150
R-squared	0.827	0.769	0.736	0.965	0.966	0.926	0.903	0.901
Blue-collar x High %blue-collar decline industry (accounting for firms changing industries)	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 total factor productivity, employment, and capital over the period of 2009 to 2017. The dependent variables: (1) *TFP*, the residual from the estimation of a Cobb-Douglas production function through the methodology of [Akerberg et al. \(2015\)](#), using blue- and white-collar worker categories as separate inputs, defined in the natural log; (2) *ln(Separations)*, the natural log of the number of employees that left the firm; (3) *ln(Entering emp.)*, the natural log of the number of new hires; (4) *ln(Blue-collar emp.)*, the natural log of the number of blue-collar employees; (5) *ln(White-collar emp.)*, the natural log of the number of white-collar employees; (6) *ln(Tangible fix.)*, the natural log of total tangible fixed assets; (7) *ln(Mac., Equip.)*, the natural log of the sum of machinery and equipment; (8) *ln(Land, Build., Furn., Other)*, 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 2013 to 2017, and to 0 for observations in the period of 2009 to 2012. *High %blue-collar decline industry* is equal to 1 if the industry's average change in the blue-collar worker share (weighted by firms' total number of employees in the industry) from 2009 to 2012 is below its sample median—capturing industries with the largest declines in blue-collar worker share—and to 0 otherwise. Appendix A 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 (2013-2017). Standard errors (in parentheses) are clustered at the firm level. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

F.17. Accounting for industry heterogeneity at a more granular level

Table F.17: The effect of firing costs: Granular industry controls

	TFP	ln(Separations)	ln(Entering emp.)	ln(Blue-collar emp.)	ln(White-collar emp.)	ln(Tangible fix.)	ln(Mac., Equip.)	ln(Land, Build., Furn., Other)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. Baseline analysis with 4-digit industry x year fixed effects								
Blue-collar x Post	-0.047*** (0.009)	-0.057*** (0.018)	-0.058*** (0.019)	-0.022** (0.011)	0.060*** (0.010)	0.042** (0.021)	0.029 (0.030)	0.041* (0.025)
R-squared	0.839	0.785	0.755	0.967	0.968	0.931	0.908	0.907
B. Defining Blue-collar within 4-digit industries								
Blue-collar (within 4-digit industry) x Post	-0.038*** (0.007)	-0.033** (0.014)	-0.044*** (0.016)	-0.012 (0.008)	0.053*** (0.008)	0.026 (0.016)	0.013 (0.024)	0.033* (0.019)
R-squared	0.839	0.785	0.755	0.967	0.968	0.931	0.908	0.907
Observations	53,987	50,046	48,872	53,987	53,987	53,987	51,470	53,688
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
4-digit industry x year FE	Yes	Yes	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 dependent variables: (1) *TFP*, the residual from the estimation of a Cobb-Douglas production function through the methodology of [Akerberg et al. \(2015\)](#), using blue- and white-collar worker categories as separate inputs, defined in the natural log; (2) *ln(Separations)*, the natural log of the number of employees that left the firm; (3) *ln(Entering emp.)*, the natural log of the number of new hires; (4) *ln(Blue-collar emp.)*, the natural log of the number of blue-collar employees; (5) *ln(White-collar emp.)*, the natural log of the number of white-collar employees; (6) *ln(Tangible fix.)*, the natural log of total tangible fixed assets; (7) *ln(Mac., Equip.)*, the natural log of the sum of machinery and equipment; (8) *ln(Land, Build., Furn., Other)*, the natural log of the sum of land, building, furniture, and other tangible fixed capital. In Panel A (Panel B), *Blue-collar* is a dummy variable equal to 1 if the firm's pre-period average share of blue-collar workers was above its sample median (above within 4-digit industry median), and to 0 otherwise. *Post* is a dummy variable equal to 1 for observations in the period of 2013 to 2017, and to 0 for observations in the period of 2009 to 2012. Appendix A 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 (2013-2017). Standard errors (in parentheses) are clustered at the firm level. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

F.18. Additional tests

In Table F.18, we carry out a set of additional robustness checks to confirm the validity of our main estimate in Table 6.

Column (1) further restricts our main model by including NACE 2-digit industry $\times$ province $\times$ year fixed effects that net out any variation that is common to all firms in the same industry and location during the same year.⁷ Column (2) even further includes NACE 4-digit industry $\times$ province $\times$ year fixed effects. Including these additional fixed effects neither alters the sign nor the significance of the main result.

Columns (3) and (4) address the concern that standard errors from panel data models may be subject to autocorrelation. As a solution, Bertrand et al. (2004) 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. In column (3), we include NACE 2-digit industry fixed effects and cluster standard errors at the same level; column (4) does the same using the 4-digit industry classification. The coefficients of interest in both columns are negative and significant at the 1% confidence level, with magnitudes comparable to those from the panel data estimations.

Column (5) estimates our baseline specification but 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 (6) bootstraps our entire procedure, re-estimating TFP and re-running the DiD regression, with 1,000 replications. Given that TFP is an estimated outcome, any potential estimation error is absorbed into the regression error term, making our estimates consistent under our baseline specification. However, because this first-stage uncertainty may lead to understated standard errors, we perform bootstrapping. The resulting effect remains very similar to our baseline estimate.

Column (7) includes the interactions of the time-invariant pre-period control dummies and the post dummy.⁸ 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 our main result.

⁷There 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.

⁸For 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-labor*. We then interact these dummies with the post dummy and include them in Equation (3) as explanatory variables.

Column (8) defines the blue-collar dummy based on the firm's share of blue-collar workers at the end of 2012, rather than the average pre-period (2009-2012) share. The estimated effect remains qualitatively similar.

Table F.18: Firing costs and productivity: Additional tests

	2-digit industry x province FEs	4-digit industry x province FEs	Collapsed post minus collapsed pre	Collapsed post minus collapsed pre	Clustering standard errors at the industry-level	Bootstrapping	Pre-period control dummies x Post	Treatment based on 2012
	TFP	TFP	TFP	TFP	TFP	TFP	TFP	TFP
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Blue-collar x Post	-0.044*** (0.009)	-0.043*** (0.011)			-0.048*** (0.008)	-0.046*** (0.009)	-0.043*** (0.008)	-0.037*** (0.008)
Blue-collar			-0.050*** (0.017)	-0.037*** (0.012)				
Observations	53,641	46,995	6,822	6,053	54,452	54,452	54,452	54,452
R-squared	0.839	0.872	0.098	0.218	0.826	0.919	0.828	0.826
Pre-period control dummies x Post	No	No	No	No	No	No	Yes	No
Firm FE	Yes	Yes	No	No	Yes	Yes	Yes	Yes
2-digit industry x year FE	No	No	No	No	Yes	Yes	Yes	Yes
2-digit industry x province FE	No	No	Yes	No	No	No	No	No
2-digit industry x province x year FE	Yes	No	No	No	No	No	No	No
4-digit industry x province FE	No	No	No	Yes	No	No	No	No
4-digit industry x province x year FE	No	Yes	No	No	No	No	No	No

This table reports difference-in-differences estimates of total factor productivity over the period of 2009 to 2017. The dependent variable, *TFP*, is the residual from the estimation of a Cobb-Douglas production function through the methodology of [Akerberg et al. \(2015\)](#), using blue- and white-collar worker categories as separate inputs, defined in the natural log. In columns (4) and (5), the data is collapsed around the event (as opposed to using annual data), and the dependent variable is first differenced. In columns (1)-(7) (in column (8)), *Blue-collar* is a dummy variable equal to 1 if the firm's average share of blue-collar employees over 2009-2012 (only in 2012) was above its sample median, and to 0 otherwise. *Post* is a dummy variable equal to 1 for observations in the period of 2013 to 2017, and to 0 for observations in the period of 2009 to 2012. Column (7) includes the interactions of *Post* and time-invariant pre-period control dummies (*Size*, *Age*, *Leverage*, *EBITDA-to-assets*, *Cash holdings*, and *Capital-to-labor*). 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. Appendix A 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 (2013-2017). Standard errors (in parentheses) are clustered at the firm level in columns (1)-(2) & (6)-(8), 2-digit industry level in columns (3) and (5), and 4-digit industry level in column (4). *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

F.19. Results across firms with low average cost per employee

During our sample period (2009–2017), employers benefited from reductions in social security contributions for low-wage workers. Below, we provide the corresponding salary cutoffs for the reductions, the standard employer contribution rates, and the reduced rates after applying the reductions:

Worker type	Annual gross salary threshold (€)	Employer contribution Rate: standard	Employer contribution rate: effective (after reduction)
Before 1 April 2016			
Blue-collar workers	€22,241.96	≈34%	≈27%
White-collar workers	€22,241.96	≈31%	≈25%
After 1 April 2016			
Blue-collar workers	€27,600.00	≈32%	≈24%
White-collar workers	€27,600.00	≈30%	≈23%

Thanks to the reductions, an employer could save up to around €1,500 (€1,300) per year in contributions for a blue-collar (white-collar) worker before 1 April 2016, and up to around €2,200 (€1,900) per year after 1 April 2016.⁹ While the differences in saving rates across worker types are not substantial, the reductions might benefit blue-collar workers more, as they traditionally have lower salaries (Statbel, 2024). For this reason, this policy may have created a wedge in the demand for the two groups and thus potentially confounded our estimates.

To examine whether reductions in employer contributions for low-wage workers influenced our results, we analyze how the effects vary across low-wage firms. We do so by interacting the coefficients of interest in Equation (3) with a *low wage* firm dummy equal to 1 if the firm’s annual average employee cost in 2012 was below its sample median, and to 0 otherwise. In our sample, both the mean and median employee costs for *low wage* firms are around €43,000, capturing firms that predominantly employ low-wage workers.¹⁰ As presented in Table F.19, new hires declined less for low-wage firms, but we do not find statistically significant evidence that these firms experienced a smaller decline in TFP compared to their high-wage counterparts. Overall, our findings suggest that the benefits for employers related to low-wage workers did not largely drive our results.

⁹In 2018, salary cutoffs increased further, and effective employer contribution rates also declined. We do not discuss these changes in detail for brevity, as they fall outside our sample period.

¹⁰Our results remain unchanged when we instead define *low wage* firms as those with annual average employee costs below the 25th percentile of the distribution as of the end of 2012.

Table F.19: The effect of firing costs: Results across low-wage firms

	TFP	ln(Separations)	ln(Entering emp.)	ln(Blue-collar emp.)	ln(White-collar emp.)	ln(Tangible fix.)	ln(Mac., Equip.)	ln(Land, Build., Furn., Other)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Blue-collar x Post	-0.048*** (0.010)	-0.081*** (0.022)	-0.076*** (0.025)	-0.015 (0.014)	0.039*** (0.013)	0.038 (0.026)	0.019 (0.036)	0.027 (0.030)
Low wage x Post	0.001 (0.011)	0.050** (0.022)	-0.076*** (0.023)	0.038*** (0.014)	-0.013 (0.012)	0.021 (0.025)	0.005 (0.037)	0.025 (0.029)
Blue-collar x Post x Low wage	-0.000 (0.014)	0.016 (0.030)	0.066** (0.033)	-0.021 (0.018)	0.027 (0.017)	-0.038 (0.034)	-0.033 (0.050)	-0.018 (0.041)
Observations	54,452	50,528	49,371	54,452	54,452	54,452	51,939	54,150
R-squared	0.826	0.770	0.736	0.965	0.966	0.926	0.903	0.901
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 total factor productivity, employment, and capital over the period of 2009 to 2017. The dependent variables: (1) *TFP*, the residual from the estimation of a Cobb-Douglas production function through the methodology of [Akerberg et al. \(2015\)](#), using blue- and white-collar worker categories as separate inputs, defined in the natural log; (2) *ln(Separations)*, the natural log of the number of employees that left the firm; (3) *ln(Entering emp.)*, the natural log of the number of new hires; (4) *ln(Blue-collar emp.)*, the natural log of the number of blue-collar employees; (5) *ln(White-collar emp.)*, the natural log of the number of white-collar employees; (6) *ln(Tangible fix.)*, the natural log of total tangible fixed assets; (7) *ln(Mac., Equip.)*, the natural log of the sum of machinery and equipment; (8) *ln(Land, Build., Furn., Other)*, 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 2013 to 2017, and to 0 for observations in the period of 2009 to 2012. *Low wage* is equal to 1 if the firm's average wage cost in 2012 was below its sample median, and to 0 otherwise. Appendix A 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 (2013-2017). Standard errors (in parentheses) are clustered at the firm level. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

F.20. Results within initially exempt industries

Table F.20: The effect of firing costs: Results within initially exempt industries

A. Main estimates

	TFP	Ln(Separations)	Ln(Entering emp.)	ln(Blue- collar emp.)	ln(White- collar emp.)	Ln(Tangible fix.)	Ln(Mac., Equip.)	ln(Land, Build., Furn., Other)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Blue-collar x Post	-0.037** (0.015)	-0.059* (0.031)	-0.099*** (0.035)	-0.029 (0.019)	0.074*** (0.019)	-0.031 (0.043)	-0.084 (0.059)	0.007 (0.051)
Observations	8,746	8,239	8,059	8,746	8,746	8,746	8,329	8,726
R-squared	0.713	0.787	0.738	0.963	0.964	0.913	0.883	0.880
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
2-digit industry x year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

(Continued)

B. Dynamic estimates

	TFP	Ln(Separations)	Ln(Entering emp.)	ln(Blue-collar emp.)	ln(White-collar emp.)	Ln(Tangible fix.)	Ln(Mac., Equip.)	ln(Land, Build., Furn., Other)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Blue-collar x 2009 dummy	0.037 (0.024)	-0.033 (0.054)	-0.004 (0.062)	-0.033 (0.025)	-0.012 (0.025)	0.034 (0.051)	0.042 (0.084)	-0.004 (0.063)
Blue-collar x 2010 dummy	-0.006 (0.018)	-0.062 (0.049)	0.020 (0.055)	-0.022 (0.019)	-0.027 (0.021)	0.013 (0.043)	0.078 (0.069)	0.004 (0.053)
Blue-collar x 2011 dummy	0.011 (0.017)	0.008 (0.045)	0.011 (0.049)	-0.012 (0.013)	-0.022 (0.015)	-0.012 (0.029)	0.025 (0.047)	0.003 (0.044)
Blue-collar x 2013 dummy	-0.013 (0.018)	-0.016 (0.043)	-0.036 (0.050)	-0.034** (0.016)	0.020 (0.014)	-0.033 (0.030)	-0.030 (0.045)	0.008 (0.040)
Blue-collar x 2014 dummy	0.014 (0.020)	-0.085* (0.049)	-0.086 (0.055)	-0.038* (0.021)	0.037* (0.019)	-0.049 (0.043)	-0.042 (0.063)	-0.005 (0.055)
Blue-collar x 2015 dummy	-0.038* (0.023)	-0.069 (0.050)	-0.140** (0.058)	-0.045** (0.022)	0.063*** (0.022)	-0.044 (0.049)	-0.056 (0.074)	-0.013 (0.060)
Blue-collar x 2016 dummy	-0.074*** (0.027)	-0.122** (0.054)	-0.161*** (0.060)	-0.062** (0.025)	0.094*** (0.027)	0.030 (0.059)	-0.095 (0.088)	0.065 (0.070)
Blue-collar x 2017 dummy	-0.051* (0.027)	-0.128** (0.056)	-0.042 (0.059)	-0.046* (0.028)	0.112*** (0.033)	-0.001 (0.062)	-0.039 (0.099)	-0.005 (0.074)
Observations	8,746	8,239	8,059	8,746	8,746	8,746	8,329	8,726
R-squared	0.714	0.787	0.738	0.963	0.964	0.913	0.883	0.880
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 total factor productivity, employment, and capital over the period of 2009 to 2017, only for the construction, upholstery and woodworking industries (NACE codes 41–43, 16, and 31). Panels A and B show the main estimates and dynamic estimates, respectively. The dependent variables: (1) *TFP*, the residual from the estimation of a Cobb-Douglas production function through the methodology of [Akerberg et al. \(2015\)](#), using blue- and white-collar worker categories as separate inputs, defined in the natural log; (2) *ln(Separations)*, the natural log of the number of employees that left the firm; (3) *ln(Entering emp.)*, the natural log of the number of new hires; (4) *ln(Blue-collar emp.)*, the natural log of the number of blue-collar employees; (5) *ln(White-collar emp.)*, the natural log of the number of white-collar employees; (6) *ln(Tangible fix.)*, the natural log of total tangible fixed assets; (7) *ln(Mac., Equip.)*, the natural log of the sum of machinery and equipment; (8) *ln(Land, Build., Furn., Other)*, 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 2013 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. Appendix A 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 (2013–2017). Standard errors (in parentheses) are clustered at the firm level. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

F.21. Firing costs, input choices, and bias in estimating TFP when using a pooled-workers production function

In this section, 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 logs of blue- and white-collar workers. Table F.1 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 distinction is likely to be misspecified. Below, we first derive analytically the bias in estimating TFP when worker types are pooled in the production function. Then, we compare our benchmark results with those obtained when pooling worker types.

Suppose a researcher only observes total employment $L = W + B$ and assumes that the production function is:

$$VA = TFP^{biased} \cdot K^{\tilde{\alpha}} L^{1-\tilde{\alpha}} \quad (5)$$

where we allow for $\tilde{\alpha}$ to be different from α in Equation (2) due to the misspecification of the production function. Revenue TFP will be measured as:

$$\log TFP^{biased} = \log VA - \tilde{\alpha} \log K - (1 - \tilde{\alpha}) \log L \quad (6)$$

while the true revenue TFP can be obtained from Equation (2):

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

The bias Ω is the difference between both revenue TFP expressions, $\Omega \equiv \log TFP^{biased} - \log TFP^{true}$, which implies:

$$\Omega = (\alpha - \tilde{\alpha}) \log K + (1 - \alpha) \log W^\gamma B^{1-\gamma} - (1 - \tilde{\alpha}) \log(W + B)$$

If the error in the estimated elasticity $\hat{\alpha} = \alpha - \tilde{\alpha}$ is small, the bias is approximately equal to:

$$\Omega \approx (1 - \alpha) \ln \left[\frac{W^\gamma B^{1-\gamma}}{W + B} \right], \quad (7)$$

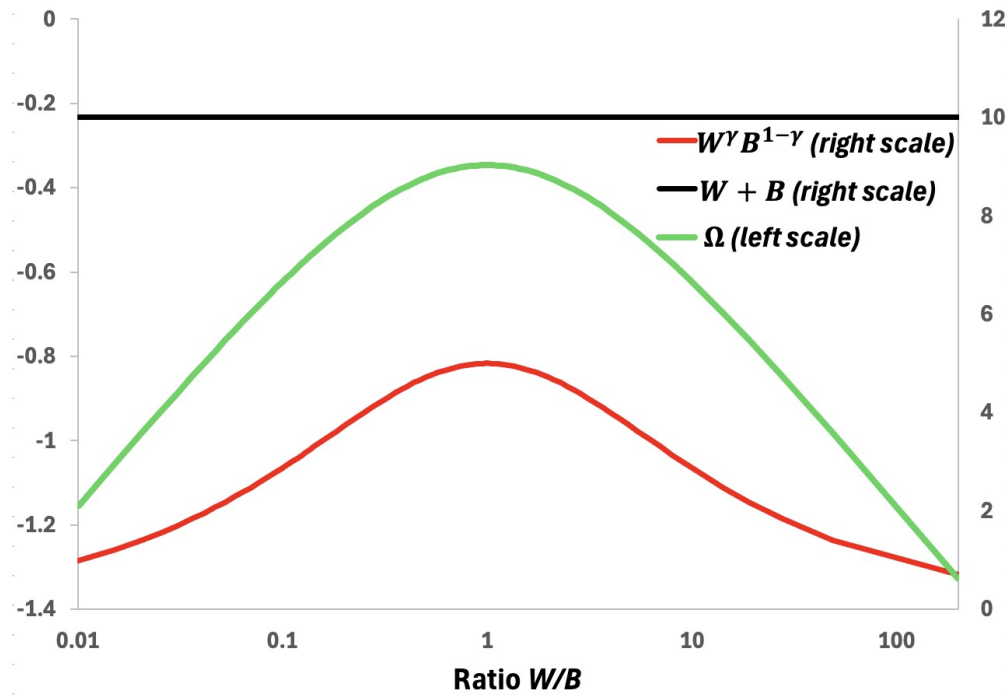
which, using $r = \frac{W}{B}$, can be written as

$$\Omega \approx (1 - \alpha) \ln \left[\frac{r^\gamma}{1 + r^\gamma} \right]$$

It follows that Ω is always negative, since the term $\left[\frac{r^\gamma}{1+r^\gamma}\right]$ is smaller than one. Furthermore, it is easy to see that Ω is smaller in absolute value when $\frac{W}{B} = \frac{\gamma}{1-\gamma}$. In order to understand the intuition, assume for simplicity that γ is equal to 0.5, and therefore that Ω is minimized when $W = B$. In this case, notice that the marginal blue- and white-collar workers have the same marginal return. Therefore, adding these workers in the biased production function (5), which implies that different workers are considered equally productive at the margin because they are assumed to be perfect substitutes, generates a relatively small misspecification error. However, now suppose the number of white-collar workers increases by one. This implies W increases by 1 in Equation (2), while L increases by one in Equation (5). It is easy to see that the misspecified Equation (5) implies output should grow by more than the correct increase implied by (2), and this difference becomes larger as $\frac{W}{B}$ increases further. The same reasoning can be applied to any deviation from $W = B$, implying that the bias increases whenever $\frac{W}{B}$ deviates from 1, or more generally, from $\frac{\gamma}{1-\gamma}$ (when $\gamma \neq 0.5$). In other words, the more the ratio $\frac{W}{B}$ becomes distant from $\frac{\gamma}{1-\gamma}$, the more adding one additional worker in Equation (5) implies a larger error relative to adding one in Equation (2).

This is shown graphically in the figure below, which compares the value of the numerator $W^\gamma B^{1-\gamma}$ and the denominator $W + B$ of the term in square brackets in Equation (7), for different values of W and B , so that the sum is constant and equal to 10, while the ratio $\frac{W}{B}$ goes from 0.1 to 200 (the x-axis scale is logarithmic). The chosen parameter values are $\gamma = 0.5$ and $\alpha = 0.5$. The figure shows how the numerator is largest when $\frac{W}{B} = \frac{\gamma}{1-\gamma} = 1$. When $\frac{W}{B}$ deviates from 1 in either direction, the value of the numerator declines because of the imbalance between the two complementary inputs. Therefore, the ratio $\frac{W^\gamma B^{1-\gamma}}{W+B}$ also declines and, since it is smaller than one, the Bias Ω becomes more negative and larger in absolute value.

Bias Ω , an illustrative example with $\gamma = 0.5$ and $W + B = 10$



Why is this bias relevant for our exercise? Because firing costs alter the optimal ratio between factor inputs, and imply that the ratio $\frac{W}{B}$ changes relative to $\frac{\gamma}{1-\gamma}$, thereby changing the bias. Since we find that firing costs change $\frac{W}{B}$ differentially for blue- and white-collar firms, they also change the bias differentially, and this effect would distort the measured effect of the reform on productivity when using a production function that pools worker types. This can, in part, explain why, when using blue- and white-collar workers pooled in Table F.21, columns (3) and (4), we no longer find a negative effect of the reform.

Table F.21: Firing costs and productivity: Using separate employment inputs versus total employment input

	Separate emp. inputs		Combined emp. input	
	TFP	TFP	TFP	TFP
	(1)	(2)	(3)	(4)
Blue-collar x Post	-0.048*** (0.008)		0.002 (0.008)	
Blue-collar x 2009 dummy		0.015 (0.013)		-0.001 (0.012)
Blue-collar x 2010 dummy		-0.005 (0.011)		-0.020* (0.010)
Blue-collar x 2011 dummy		-0.006 (0.009)		-0.016* (0.009)
Blue-collar x 2013 dummy		-0.039*** (0.009)		-0.009 (0.008)
Blue-collar x 2014 dummy		-0.039*** (0.010)		0.001 (0.009)
Blue-collar x 2015 dummy		-0.043*** (0.012)		-0.006 (0.011)
Blue-collar x 2016 dummy		-0.060*** (0.013)		-0.011 (0.013)
Blue-collar x 2017 dummy		-0.064*** (0.014)		-0.012 (0.013)
Observations	54,452	54,452	54,391	54,391
R-squared	0.826	0.826	0.859	0.859
Firm FE	Yes	Yes	Yes	Yes
2-digit industry x year FE	Yes	Yes	Yes	Yes

This table reports difference-in-differences estimates of total factor productivity over the period of 2009 to 2017. The dependent variable, *TFP*, is the residual from the estimation of a Cobb-Douglas production function through the methodology of [Akerberg et al. \(2015\)](#), defined in the natural log. When estimating the production function, blue- and white-collar worker categories are used as separate inputs in columns (1) and (2), and these worker categories are pooled into a single employment input in columns (3) and (4). *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 2013 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. Appendix A 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 (2013-2017). Standard errors (in parentheses) are clustered at the firm level. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

F.22. The effect on firm exit & entry

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

$$Y_{s,t} = \alpha \text{Blue-collar}_s * \text{Post}_t + \mu_s + \theta_t + \varepsilon_{s,t} \quad (8)$$

where the dependent variable is either *exit share* (defined as the ratio of the total number of firm exits to the total number of firms in industry s and year t), or *entry share* (defined as the ratio of the total number of firm entries to the total number of firms in industry s and year t). We define firm exit based on legal status, identifying firms that ceased activity due to bankruptcy, liquidation or judicial dissolution¹¹, whereas entry is defined by the year of incorporation. *Blue-collar* $_s$ is a dummy variable equal to 1 if the industry's weighted average share of blue-collar workers (where weights correspond to firms' total number of employees) over the pre-period (2009-2012) was above the sample median, and to 0 otherwise.¹² *Post* is a dummy variable equal to 1 for observations in the period of 2013 to 2017, and to 0 for observations in the period of 2009 to 2012. We include industry (μ_s) and time (θ_t) fixed effects. Standard errors are clustered at the industry level.

For this analysis, we focus on the industries used in our baseline analysis (see Table F.1) but include all firms without applying additional firm-level restrictions—that is, we do not require firms to be observed in both 2012 and 2014, to have at least five employees in 2012, or to employ both a blue-collar and a white-collar worker.

Table F.22 presents the results from the estimation of Equation (8) at the 2-digit (columns (1) and (3)) and 3-digit (columns (2) and (4)) industry levels. The evidence indicates that the reform increased the exit share by 0.2 and 0.3 percentage points—corresponding to 12% and 18% increases relative to the sample average of 1.7%—at the 2- and 3-digit industry levels, respectively. However, only the latter effect is significant at the 5% confidence level. In contrast, we do not observe any significant impact on firm entry. Overall, these results provide some evidence that increased firing costs lead to an increase in firm exits.

¹¹Our definition of firm exit does not include voluntary exits, but our results are robust to their inclusion.

¹²Our results are qualitatively similar if we construct the industry-level treatment based on the simple average of firms' blue-collar worker shares within each industry.

Table F.22: The effect of firing costs on firm exit & entry

	2-digit industry	3-digit industry	2-digit industry	3-digit industry
	Exit share		Entry share	
	(1)	(2)	(3)	(4)
Blue-collar (Industry level) x Post	0.002 (0.001)	0.003** (0.002)	-0.000 (0.001)	0.000 (0.002)
Observations	447	1,470	447	1,470
R-squared	0.835	0.484	0.500	0.296
Industry FE	Yes (2-digit)	Yes (3-digit)	Yes (2-digit)	Yes (3-digit)
Year FE	Yes	Yes	Yes	Yes

This table reports difference-in-differences estimates of firm exit and entry over the period of 2009 to 2017. In columns (1) and (2), the dependent variable, *exit share*, is the ratio of the total number of firm exits (defined through firms' legal status) to the total number of firms in the industry in a given year. In columns (3) and (4), the dependent variable, *entry share*, is the ratio of the total number of firm entries (defined through the firm's year of incorporation) to the total number of firms in the industry in a given year. *Blue-collar* is a dummy variable equal to 1 if the industry's weighted average share of blue-collar workers (where weights correspond to firms' total number of employees) 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 2013 to 2017, and to 0 for observations in the period of 2009 to 2012. Appendix A defines all variables. Standard errors (in parentheses) are clustered at the industry level used in the analysis. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

F.23. The role of credit access

In this section, we investigate how the effect of firing costs on TFP varies with financial constraints. Because 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).

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 (National Bank of Belgium, 2012). 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. 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). We first run the following regression on a sample of borrowing firms at the bank-firm-month level separately for each pre-period year:

$$\Delta Lending_{bft} = \alpha_{lst} + \beta_{bt} + error\ term \quad (9)$$

where $\Delta Lending_{bft}$ is the annual percentage growth rate of credit at the intensive margin at month t , α_{lst} 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.¹³ We estimate Equation (9) by omitting one firm dummy, and demean

¹³Since 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

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.

Results. Using TFP as the dependent variable, we then estimate Equation (3) 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 F.23 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.

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.

Table F.23: Firing costs and productivity: The role of credit access

	Higher debt capacity			Lower debt capacity		
	Full sample	Low credit supply	High credit supply	Full sample	Low credit supply	High credit supply
	TFP	TFP	TFP	TFP	TFP	TFP
	(1)	(2)	(3)	(4)	(5)	(6)
Blue-collar x Post	-0.026* (0.015)	-0.031 (0.022)	-0.031 (0.020)	-0.041*** (0.012)	-0.049*** (0.015)	-0.027 (0.018)
Observations	16,764	7,849	8,855	22,001	11,554	10,391
R-squared	0.830	0.826	0.843	0.850	0.852	0.855
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. A firm is assumed to have *higher debt capacity* (*lower debt capacity*) if its long-term financial debt-to-fixed assets ratio in 2012 end year was below (above) the sample median. *Low credit supply* (*High credit supply*) sample includes firms whose estimated credit supply shock over the pre-period (2009-2012) 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 [Akerberg et al. \(2015\)](#), using blue- and white-collar worker categories as separate inputs, defined in the 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 2013 to 2017, and to 0 for observations in the period of 2009 to 2012. Appendix A 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 (2013-2017). Standard errors (in parentheses) are clustered at the firm level. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

F.24. 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 neighboring countries of Belgium) that are not affected by the Belgian labor 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 labor 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, implemented in response to the sovereign debt crisis, 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 ([Bureau van Dijk, 2018](#)). As in our main sample, we use historical files and incorporate both surviving and failed firms into our analysis. Unlike 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 German and French firms in the same industries, assuming that workforce composition in Belgium, Germany, and France is similar within the same industries. We also conduct the same comparison 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 aggregate share of blue-collar workers, measured as the ratio of total blue-collar workers to total workers in the industry, was above the sample median) and white-collar industries (i.e., NACE 4-digit industries whose corresponding share was below the sample median):

$$\ln(\text{Value added})_{i,s,c,t} = \beta \text{Belgian}_c * \text{Post}_t + \Pi \text{Controls}_{c,t} + \theta_i + \mu_{st} + \varepsilon_{i,s,c,t} \quad (10)$$

where the dependent variable is the natural log of value-added for firm i , in industry s , country c , and year t . Belgian_c 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_t is a dummy variable equal to 1 for observations in the period of 2013 to 2017, and to 0 for observations in the period of 2009 to 2012. $\text{Controls}_{c,t}$ include *GDP growth* and *GDP per capita* to account for time-varying macroeconomic conditions across countries.¹⁴ 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 firm fixed effects (θ_i) and 4-digit industry $\times$ year

¹⁴We obtain data on *GDP growth* and *GDP per capita* from the World Bank ([World Bank, 2017](#)).

(μ_{st}) fixed effects. Lastly, standard errors are clustered at the country $\times$ 4-digit industry level.¹⁵

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 Sections 5.6 and F.21 for a detailed discussion).

Our intensive set of fixed effects considerably accounts for the heterogeneity between Belgian firms (i.e., treated firms) and German and French firms (i.e., control firms). Additionally, since the treated and control firms are drawn from different countries, we implement a propensity score matching procedure to improve comparability between these two groups. To obtain propensity scores, we consider 2012 as a baseline year and estimate a logit regression using *Belgian* as the dependent variable. As explanatory variables, we include *size*, *age*, *leverage*, *cash holdings*, and *capital-to-labor* among baseline firm controls. To further ensure that our matched samples exhibit similar pre-treatment trends in labor and capital, we also include the log growth of tangible fixed assets, the log growth of the total number of employees, as well as both the lagged and contemporaneous values of the natural log of the total number of employees. Finally, we include NACE 4-digit industry dummies to control for industry-level differences. Then, we carry out a nearest neighbor matching of propensity scores with exact matches in NACE 4-digit industries. We match each Belgian firm with a non-Belgian firm, and allow that a non-Belgian firm can be used as a match for multiple Belgian firms.¹⁶ Table F.24.1, in Panel A, demonstrates the summary statistics for the full baseline (i.e., unmatched) sample. Panel B compares the pre-treatment sample means of Belgian and non-Belgian firms in both the unmatched and matched samples. As evidenced by the t-tests and the standardized mean differences, the matching procedure improves the balance in pre-period covariates between the Belgian and non-Belgian groups, relative to the unmatched sample.

Results. Panel A of Table F.24.2 illustrates the results from the estimation of Equation (10). Columns (1) and (2) use the unmatched sample and report the results for blue-collar and white-collar industries, respectively. Columns (3) and (4) use the matched sample and repeat the same estimations. The point estimates for blue-collar industries are negative and statistically significant, with magnitudes that are consistent across both the unmatched and matched samples. Specifically, the evidence indicates that Belgian firms in blue-collar industries (i.e., those facing an increase in firing costs) on average experienced a 2% decline in value-added relative to their German and French counterparts. This result is consistent with the estimated effect from the within-Belgium analysis (see Column (8) of Table 5).

At the same time, the point estimates for firms in white-collar industries are not significant,

¹⁵We obtain nearly similar results when we cluster standard errors at the firm level.

¹⁶Our results are also robust when two non-Belgian firms are retained for each Belgian firm.

suggesting that the reduction in the notice periods for white-collar workers did not lead to an increase in output. This is consistent with the possibility that the reduction in notice periods might have been offset by increased protection through other channels, including the abolition of trial periods, improved outplacement rights, and enhanced safeguards against unfair dismissals.

Using the full sample of firms, we also conduct a triple difference-in-difference approach by interacting the Belgian and post dummy variables in Equation (10) 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 F.24.2. Columns (1) and (2) demonstrate the estimates from the unmatched sample, without and with the inclusion of the triple interaction terms, 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 the 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 using the matched sample, and the results remain consistent.

Overall, we find that Belgian firms in blue-collar industries on average experienced a decrease in output relative to their non-Belgian counterparts, whereas we do not observe any significant effect for firms in white-collar industries. These results corroborate the negative effect of higher firing costs on output identified in the within-Belgium analysis. Moreover, by comparing equally affected European firms within the same industries, our cross-country estimates mitigate the concern that financial programs implemented during the sovereign debt crisis may have confounded our results by differently affecting blue- and white-collar firms.

Table F.24.1: Cross-country analysis of firing costs: Summary statistics

A. Full sample						
	N	Mean	SD	p(25)	p(50)	p(75)
<i>Firm level</i>						
Ln(Value added)	248,139	15.127	1.692	13.739	15.028	16.291
Size	248,139	15.241	1.920	13.701	15.247	16.549
Age	248,139	26.905	21.933	13.000	22.000	35.000
Leverage	248,139	0.630	0.265	0.450	0.631	0.794
EBITDA-to-assets	248,139	0.109	0.122	0.045	0.099	0.168
Cash holdings	248,139	0.149	0.170	0.020	0.083	0.224
Capital-to-labour	248,139	9.392	1.583	8.410	9.427	10.444
<i>Country level</i>						
GDP growth (%)	248,139	0.853	1.763	0.420	0.956	1.949
GDP per capita (in thous. \$)	248,139	39.765	3.639	37.441	39.524	42.291

B. Comparing pre-period sample means for treatment and control firms								
	Non-matched sample				Matched sample			
	Belgian N=20,972	non-Belgian N=94,014	Diff.	SMD	Belgian N=19,835	non-Belgian N=18,666	Diff.	SMD
<i>Firm level</i>								
Ln(Value added)	15.612	14.925	0.687***	0.469	15.612	15.824	-0.212***	-0.151
Size	16.097	14.964	1.133***	0.680	16.098	16.165	-0.067	-0.042
Age	29.326	24.143	5.182***	0.256	29.459	30.171	-0.711	-0.030
Leverage	0.593	0.641	-0.048***	-0.185	0.593	0.615	-0.022***	-0.089
EBITDA-to-assets	0.109	0.113	-0.004***	-0.032	0.110	0.111	-0.001	-0.012
Cash holdings	0.087	0.168	-0.081***	-0.541	0.087	0.095	-0.009***	-0.073
Capital-to-labour	10.295	9.178	1.116***	0.770	10.301	10.294	0.007	0.005
<i>Country level</i>								
GDP growth (%)	0.878	0.666	0.212***	0.099	0.889	0.798	0.091***	0.038
GDP per capita (in thous. \$)	40.363	37.765	2.599***	1.246	40.332	38.423	1.909***	0.840

This table presents summary statistics for the main variables used in the cross-country analysis over the period of 2009 to 2017. Panel A shows the summary statistics for the entire sample. Panel B compares the sample means of Belgian and non-Belgian (German or French) firms in the unmatched and matched samples over the pre-period (2009-2012). *Belgian* 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. Appendix A 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 (2013-2017). *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively, for *t*-test of whether the Belgian and non-Belgian 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 non-Belgian firms are matched to Belgian firms more than once. The standardized mean difference (SMD) for a variable is calculated as $(Mean_{Belgian} - Mean_{non-Belgian}) / \sqrt{(sd_{Belgian}^2 + sd_{non-Belgian}^2) / 2}$.

Table F.24.2: Cross-country analysis of firing costs: Results

A. Split sample analysis

	Non-matched sample		Matched sample	
	Blue-collar industries	White-collar industries	Blue-collar industries	White-collar industries
	ln(Value added)			
	(1)	(2)	(3)	(4)
Belgian x Post	-0.020*** (0.007)	0.005 (0.006)	-0.027*** (0.009)	0.005 (0.008)
Observations	119,125	129,014	40,973	40,890
R-squared	0.988	0.988	0.981	0.983
Macro controls	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
4-digit industry x year FE	Yes	Yes	Yes	Yes

(Continued)

B. Triple-difference

	Non-matched sample			Matched sample		
	ln(Value added)					
	(1)	(2)	(3)	(4)	(5)	(6)
Belgian x Post	0.001 (0.006)			-0.009 (0.007)		
Blue-collar industries x Belgian x Post		-0.024** (0.010)			-0.031*** (0.012)	
Blue-collar industries x Belgian x 2009 dummy			0.017 (0.011)			0.019 (0.015)
Blue-collar industries x Belgian x 2010 dummy			0.001 (0.008)			0.000 (0.012)
Blue-collar industries x Belgian x 2011 dummy			-0.006 (0.006)			-0.011 (0.007)
Blue-collar industries x Belgian x 2013 dummy			-0.017*** (0.005)			-0.021** (0.009)
Blue-collar industries x Belgian x 2014 dummy			-0.026*** (0.008)			-0.034*** (0.011)
Blue-collar industries x Belgian x 2015 dummy			-0.024** (0.011)			-0.029** (0.014)
Blue-collar industries x Belgian x 2016 dummy			-0.029** (0.012)			-0.051** (0.020)
Blue-collar industries x Belgian x 2017 dummy			-0.011 (0.014)			-0.009 (0.022)
Observations	248,139	248,139	248,139	81,863	81,863	81,863
R-squared	0.988	0.988	0.988	0.982	0.982	0.982
Firm FE	Yes	Yes	Yes	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

This table reports difference-in-differences (Panel A) and triple-difference (Panel B) estimates of value-added over the period of 2009 to 2017. *Blue-collar industries* (*White-collar industries*) are 4-digit industries whose pre-period aggregate share of blue-collar workers (i.e., the ratio of the total number of blue-collar workers to the total number of workers in the industry) was above (below) the sample median. The dependent variable, $\ln(\text{Value added})$, is the natural log of value added. *Belgian* 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 2013 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. Macro controls are $GDP\ growth_{t-1}$ and $GDP\ per\ capita_{t-1}$. Appendix A 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 (2013-2017). Standard errors (in parentheses) are clustered at the country $\times$ 4-digit industry level. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

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