125 ANNI INPS

Istituto Nazionale Previdenza Sociale

luglio 2023 – numero 67

WorkINPS *Papers*

Heterogeneous Responses of Productivity to Labor Market Reforms*

Francesco Del Prato
Paolo Zacchia

ISSN 2532 -8565

Lo scopo della serie WorkINPS papers è quello di promuovere la circolazione di documenti di lavoro prodotti da INPS o presentati da esperti indipendenti nel corso di seminari INPS, con l'obiettivo di stimolare commenti e suggerimenti.
Le opinioni espresse negli articoli sono quelle degli autori e non coinvolgono la responsabilità di INPS.

The purpose of the WorkINPS papers series is to promote the circulation of working papers prepared within INPS or presented in INPS seminars by outside experts with the aim of stimulating comments and suggestions.
The views expressed in the articles are those of the authors and do not involve the responsibility of INPS.

In copertina: uno storico "Punto cliente" a Tuscania
INPS, Direzione generale, Archivio storico

I WORKINPS PAPER

Le basi dati amministrative dell'*INPS* rappresentano una fonte statistica unica per studiare scientificamente temi cruciali per l'economia italiana, la società e la politica economica: non solo il mercato del lavoro e i sistemi di protezione sociale, ma anche i nodi strutturali che impediscono all'Italia di crescere in modo adeguato. All'interno dell'Istituto, questi temi vengono studiati sia dai funzionari impiegati in attività di ricerca, sia dai *VisitInps Scholars*, ricercatori italiani e stranieri selezionati in base al loro curriculum vitae e al progetto di ricerca presentato.

I **WORKINPS** hanno lo scopo di diffondere i risultati delle ricerche svolte all'interno dell'Istituto a un più ampio numero possibile di ricercatori, studenti e policy markers.

Questi saggi di ricerca rappresentano un prodotto di avanzamento intermedio rispetto alla pubblicazione scientifica finale, un processo che nelle scienze sociali può chiedere anche diversi anni. Il processo di pubblicazione scientifica finale sarà gestito dai singoli autori.

Maurizio Franzini

Heterogeneous Responses of Productivity to Labor Market Reforms*

Francesco Del Prato

(Paris School of Economics)

Paolo Zacchia

(CERGE-EI (Charles University and the Czech Academy of Sciences)).

INPS.0072.23/05/2023.0000535

Heterogeneous Responses of Productivity to Labor Market Reforms*

Francesco Del Prato† Paolo Zacchia‡

May 2, 2023

Abstract

We present evidence that increased labor flexibility, achieved by granting firms more freedom to use temporary contracts, adversely impacted the total factor productivity (TFP) within the lower segment of the productivity distribution in manufacturing industries. By exploiting a 2001 Italian reform which progressively relaxed the regulations on temporary contracts, we reveal that firms positioned at the lower end of the pre-reform TFP distribution experienced a decrease in TFP compared to counterfactual firms, with a difference of 4 to 5 percentage points. Furthermore, within two years after the reform, these less productive firms faced a decrease in their exit rates by 20 to 30%. In contrast, firms situated in the middle to high segments of the productivity distribution exhibited no significant impact on TFP, and instead, experienced a 5 to 8% increase in labor productivity within three years. To provide a theoretical framework for interpreting our empirical evidence, we build a general equilibrium model with monopolistic competition. Our model relates the equilibrium productivity distributions across sectors to labor and capital markets frictions and highlights the role of labor wedges in driving selection dynamics at the lower end of the productivity distribution, with an ambiguous impact on welfare.

JEL classification: D21, D22, D24, E24, J08, O14.

Keywords: Productivity, TFP, labor flexibility, EPL, labor market policies.

*We are deeply grateful to Raffaele Saggio for having shared his data on collective agreements' renewals. We thank for their precious comments and suggestions Edoardo M. Acabbi, Bruno Cassiman, Luca Citino, Kenan Huremovic, Mattia Nardotto, Matteo Paradisi, and Santiago Pereda Fernández. We also thank the seminar participants at VisitINPS, KU Leuven, SIE's 63rd RSA. The realization of this paper was made possible thanks to sponsorships and donations in favor of the *VisitINPS Scholars'* program. We particularly thank Edoardo Di Porto and Paolo Naticchioni for their helpful comments and support with the data, as well as the entire INPS' *Direzione Studi e Ricerche*. The findings and conclusions expressed are solely those of the author and do not represent the views of INPS. All mistakes are our own.

†Paris School of Economics. Contact: francesco.delprato@psemail.eu.

‡CERGE-EI (Charles University and the Czech Academy of Sciences). Contact: paolo.zacchia@cerge-ei.cz.

Risposte Eterogenee della Produttività alle Riforme del Mercato del Lavoro*

Francesco Del Prato   Paolo Zacchia

May 2, 2023

Abstract

Questo contributo esamina l'effetto di una maggiore flessibilità nel mercato lavoro, in particolare un uso più agevole di contratti di lavoro temporanei da parte delle aziende, sulla loro produttività. In particolare, esso mostra come una maggiore flessibilità abbia influenzato negativamente la produttività totale dei fattori (PTF) del segmento inferiore della distribuzione della produttività dei settori manifatturieri. Attraverso l'analisi di una riforma italiana del 2001 che ha allentato le regolamentazioni sui contratti temporanei, si osserva come le aziende posizionate all'estremità inferiore della distribuzione della produttività pre-riforma abbiano subito una diminuzione del 4-5% della PTF rispetto alle aziende controfattuali. Due anni dopo la riforma tra tali aziende già meno produttive si riscontra una diminuzione dei tassi di uscita dal mercato tra il 20 e il 30%. Di contro, tra le aziende già collocate nella parte alta della distribuzione della produttività non si osserva un impatto significativo sulla PTF, a fronte di un aumento della produttività del lavoro tra il 5 e l'8% nell'arco di tre anni. Per razionalizzare l'interpretazione queste evidenze empiriche, il contributo sviluppa un modello di equilibrio generale che prevede concorrenza monopolistica tra imprese. Esso caratterizza la distribuzione della produttività di equilibrio come funzione delle frizioni nei mercati dei fattori di produzione, sottolineando il ruolo delle distorsioni nel mercato lavoro nella dinamica di selezione che avviene nella parte bassa della distribuzione della produttività. Tali distorsioni hanno un impatto ambiguo sul benessere sociale complessivo.

JEL classification: D21, D22, D24, E24, J08, O14.

Keywords: Produttività, TFP, flessibilità del lavoro, EPL, politiche del mercato del lavoro.

*La realizzazione del presente articolo è stata possibile grazie alle sponsorizzazioni e le erogazioni liberali a favore del programma 'VisitINPS Scholars'.

1 Introduction

It is often believed that increased labor market flexibility leads to increased productivity. More flexible labor markets should drive gains in efficiency as they reduce distortion in production choices by making workforce adjustments easier for firms. For example, temporary contracts can be considered drivers to incentivize productivity-enhancement investments (Grout, 1984; Card et al., 2014; Jäger et al., 2021). On the contrary, more rigidity is thought to depress job creation and destruction rates, theoretically reducing aggregate productivity (Lagos, 2006). However, although recent labor market reforms have reduced employment protection legislation (EPL) in many countries[1], productivity growth has experienced a severe slowdown, which in turn explains a relevant component of the observed decline in output growth (Bergeaud et al., 2017). Whether the EPL reduction truly acts as a *rising tide that lifts all boats* towards productivity has become a relevant question to address (Dew-Becker and Gordon, 2012).

This paper provides novel evidence that the relationship between labor market rigidity and firms' productivity is more nuanced than commonly believed and varies sharply along the productivity distribution itself. In particular, we show that increased flexibility—in our setting, by easing firms' access to temporary contracts—depresses the total factor productivity (TFP) within the group of already-unproductive firms. Such an adverse effect vanishes as one climbs the pre-labor market intervention productivity distribution. We rationalize these intriguing results through a full-fledged general equilibrium model that links the equilibrium TFP distribution across sectors to labor and capital frictions. We use the model to theoretically decompose the EPL reduction effect along the productivity distribution as a mix of a left tail-specific selection mechanism and an incentive in productivity-enhancement investments due to the downward pressure on labor cost.

To study the effect of the EPL reduction on productivity, we leverage a quasi-natural experiment provided by an Italian 2001 reform that reduced the requirements for the firms to start new temporary contracts: by easing the adoption of temporary jobs for firms, the reform increased the country's overall labor flexibility. We show that the reform caused a remarkably heterogeneous response across the four quartiles of the ex-ante TFP distribution. We do so through a battery of event studies that exploit the staggered implementation of the intervention across collective bargaining agreements (*Contratti Collettivi Nazionali del Lavoro*, CCNL) as in Daruich et al. (2020). This way, identification comes from the dynamic comparison of credibly as-good-as-random firms that adopt the reform early—based on the modal contract they employed right before the reform—with later-treated ones. We further complement these linear specifications with a quantile treatment effect approach that allows us to refine better this heterogeneity margin on productivity. In our empirical analysis, we exploit a rich, matched employer-employee administrative dataset from the Italian social security institute paired with firms' balance sheet data. This dataset allows

[1]In Europe, between 1995 and 2018, 25 reforms affected EPL of regular contracts and 27 of fixed-term across 11 countries. Of these reforms, 22 and 20, respectively, shifted the law toward a regulatory easing. See Aumond et al. (2022) for a quarterly narrative database of European labor market interventions.

us to reliably build three different measures of TFP based on distinct estimation procedures and to include worker, firm, and province-by-sector level outcomes in the analysis.

The increase in labor flexibility had a sizeable effect on the TFP among already-unproductive firms. Overall, more accessible use of temporary job arrangements caused a slight yet insignificant reduction in average TFP at the firm level, consistently across the three TFP measures. Still, this noisy effect masks substantial heterogeneity, as nearly all the signal is entirely driven by firms belonging to the bottom quartile of the pre-reform TFP distribution. Following the reform, only firms at the bottom of the ex-ante productivity distribution became less productive than the counterfactual. Our separate specifications show that the reform did not affect the rest of the distribution, leaving firms that were moderately-to-highly productive to start with virtually unaltered. Again, such a result remains strongly consistent across the three productivity measures. Through a dynamic triple differences specification on the whole sample, we provide further evidence of this heterogeneous effect by showing that firms at the bottom of the distribution systematically experience a much higher decrease in productivity compared to the firms belonging to the center. Firms at the top, on the contrary, tend to endure an effect on TFP which is more favorable than the one experienced by the mid-distribution ones.

This evidence relies on linear specifications that quantify the average impact of the EPL reduction *within* a certain quartile of the ex-ante TFP distribution. We thus refine our heterogeneity analysis by directly assessing the distributional impact of the reform through a non-parametric specification on quantile treatment effects (Callaway and Li, 2019). Intuitively, through this model, we compare the TFP distribution after the reform relative to the one we would have had if the intervention had never occurred. Our results show that the effect of the reform moves monotonously in the TFP distribution: it is firmly negative at the bottom deciles, which show productivity losses up to 10% right after the reform, and eventually flips sign for most productive firms, which experience a short-term increase in productivity up to 4%, albeit not precisely estimated.

One may wonder why our primary focus is on TFP rather than other related dimensions, such as labor productivity (LP). We do not neglect the latter. We show indeed that following the EPL-reducing reform, LP increases over a different segment of the ex-ante LP distribution—i.e., at the top—coherently with many competitive models of the labor market. Intriguingly, we show that increased flexibility has a mirror effect on TFP and LP, both in terms of parts of the distribution involved and the direction of the impact. Still, we mostly focus on TFP in order to highlight the overall implications of the reforms to the economy on both the output and welfare dimensions. These overall effects can be explained by mechanisms that differ from, or that complement, the simple adjustment of production factors to changes in their relative prices.

Moreover, we show that the reform impacted firms' survival of already-unproductive firms. While we find no effect along the rest of the ex-ante productivity distribution, the reduction in EPL caused a decrease from 20% up to 34% in the number of exit events after two and three years for firms at the distribution's bottom. In words, firms seemed to have used more flexible work arrangements to increase their probability of survival compared to the scenario in which the reform had not been implemented. This evidence is also sustained by the stark reduction in labor costs we document as implied by the reform. On average, our results show that firms experienced a cut of around 14% in total wages after three years.

We propose a novel interpretation of these strongly heterogeneous results on TFP as evidence of a mixed mechanism at play based on a simple intuition. We observe that a reduction in EPL translates into a decrease in labor's absolute and relative prices. In turn, this provides survival means to already unproductive firms that would have had a higher probability of exit had the reform not taken place. Thus, an adverse selection effect triggers on the left of the TFP distribution, where both firms that were not supposed to continue producing survive, entry barriers reduce, allowing for low-productivity firms to enter the market, and cheaper labor decreases incentives for capital deepening and investments. This combination, in turn, worsens allocative efficiency, depressing overall productivity. On the other side of the distribution, already-productive firms face higher incentives to keep investing—again, due to the relative price change of the production factors. Here, no negative selection applies; thus, firms *may* experience TFP increases thanks to the efficiency gains in labor force adjustments due to the more flexible labor market.

To provide more rigor to the arguments backing these results, we construct a model that delivers the mechanisms we postulate and builds on the tradition of general equilibrium models with monopolistic competition (Dixit and Stiglitz, 1977) with heterogeneous firms (Melitz, 2003). Our analysis features two key components already introduced in prior work, although our modeling approach is novel in both cases: financial frictions (see among the others Manova, 2013) and endogenous productivity (Bustos, 2011; Zhelobodko et al., 2012). We treat financial frictions as an asymmetric information problem: to enter a market, firms need financial intermediaries (FIs) to supply a credit for them; FIs only observe a noisy signal about firms' true productivity. Endogeneity of productivity is modeled similarly as in Bustos (2011), although we treat the cost of PEIs as a continuous, rather than binary, variable. A key prediction of our model is that stronger EPLs lead to the lower entry of low-productivity firms while also stifling productivity-enhancing investments (PEIs)—especially on the right tail. Our empirical results provide robust evidence in favor of the former mechanism and mixed evidence for the latter. Without considering the utility value of EPLs for workers, the net welfare effect of these two mechanisms is ambiguous and depends on the relative magnitude of the impact at the tails; we leave a full-fledged welfare analysis to future work.

Related literature Many papers have addressed the link between EPL—particularly the use of temporary jobs—and productivity. Among others, three, in particular, lie close to our work. Autor et al. (2007) used US plant-level data to investigate the effects of state courts adopting wrongful-discharge protection provisions, showing a decrease in job flows, entries, and TFP. Cappellari et al. (2012) exploit the same Italian reform of ours to show that it led to productivity losses due to the substitution of temporary employees for external staff and a reduction in capital intensity. Dolado et al. (2016) link the cost gap between permanent and temporary jobs to firms' TFP, arguing that the higher this gap, the lower the temp-to-perm conversion rate, which in turn depresses workers' effort and firm-level paid-for training—thus reducing productivity.

While our results stay coherent with many of the findings these three works offer, we differ from them along several dimensions. First, we focus on the heterogeneity of the effect of an increase in labor flexibility on TFP based on *ex-ante* productivity, exposing the risk of harming already-unproductive firms. Second, we rely on an institutional setting that allows us to claim a causal interpretation of our results. With respect to Cappellari et al. (2012), we have access to a remarkably rich administrative dataset that will enable us to observe the universe of the worker-firm matches, and the detail of the collective bargaining adopted by each worker, as in Daruich et al. (2020) and Acabbi and Alati (2021). Third, we provide a novel mechanism to explain the heterogeneous impact of the ease in temp jobs access on TFP that combines a selection effect with a change in the incentives to invest.

Other papers have addressed different firm-level margins, connecting them with reforms affecting EPL in general. Kugler and Pica (2008) show that an increase in dismissal costs for small firms decreased accessions and separations for workers in those establishments, reduced employment adjustments on the internal margin and entry rates, and increased exit rates. Bassanini et al. (2009) show a depressing impact of dismissal regulation in the OECD on productivity growth concentrated in industries where layoff restrictions are more likely to be binding—but no evidence of a productivity effect of temporary contracts regulation. Cingano et al. (2010) found that EPL depresses investment, capital-labor substitution, labor productivity, and job reallocation. The adverse effects are more pronounced in sectors with high reallocation rates and exacerbated by poor access to credit markets. Cingano et al. (2016) show that the introduction of unjust-dismissal costs for Italian firms below 15 employees caused an increase in the capital-labor ratio and a decline in TFP in small firms relative to larger firms. Acabbi and Alati (2021) exploit our same reform to show how firms use the contract composition to manage the risk determined by their labor-induced operating leverage: among firms with an ex-ante rigid labor cost structure, a more flexible workforce composition leads to an increase in profit margin. Other papers also focused on the economic effect of changes in EPL using cross-country analyses with aggregate data, mainly assessing the impact on unemployment and wages (Lazear, 1990; Bertola, 1990; Bertola and Rogerson, 1997; Garibaldi and Violante, 2005).[2] In particular, Dew-Becker and Gordon (2012) document a strong negative correlation between LP growth and employment per capita across European countries.

Gnocato et al. (2020) investigate a different channel in providing evidence of the heterogeneous effect of the easing of temporary contracts on the size-productivity covariance as a measure of allocative efficiency in the sense of Hsieh and Klenow (2009). Here, heterogeneity is driven by geographical differences in the length of labor court disputes. Our

[2]For a review of the different cost margins associated with structural reforms that improved labor flexibility, see Boeri et al. (2015).

results remain coherent with their proposed mechanism of heterogeneous gains in labor productivity, according to which more productive firms tend to gain market shares thanks to longer tenure at the workplace for fixed-term workers.

Different partial equilibrium approaches have discussed the ambiguous effect of easing access to fixed-term contracts on unemployment and wages from a theoretical standpoint. Bentolila and Bertola (1990) show that higher EPL increases average employment as the reduction in lay-offs dominates the adverse effect coming from lower hiring. Blanchard and Landier (2002) and Cahuc and Postel-Vinay (2002) argue that temp contracts may lead to higher turnover in entry-level jobs and, thus, higher unemployment, lower job ladder climbing, and lower matches' productivity. General equilibrium effects are ambiguous, with a relevant dependence on the model considered (Ljungqvist, 2002).

Many mechanisms can also explain negative correlations between EPL and productivity, particularly for LP. High EPL might indeed hamper efficient allocation of resources within the economy (Hopenhayn and Rogerson, 1993); depress workers' effort (Ichino and Riphahn, 2005; Engellandt and Riphahn, 2005; Dolado et al., 2016); decrease the incentive to acquire general skills rather than specific, thus hindering workers' reallocations across firms (Wasmer, 2006); reduce LP through an increase in substitution between permanent and temporary contracts (Cahuc et al., 2016). Similarly, a reduction in EPL (particularly when easing the use of temporary contracts) might be used as a screening device by firms—stepping stones into permanent contracts that increase the match quality (Ichino et al., 2008; Faccini, 2014). Moreover, an increase in EPL might depress TFP by reducing job-creation and job-destruction rates in an aggregative model of TFP that considers individual search behavior (Lagos, 2006).

On the other hand, two main reasons might link increases in EPL to increased productivity. First, employment protection might encourage workers to invest in match-specific human capital, thus benefitting LP, mainly when other labor market rigidities exist (Belot et al., 2007). Second, increasing EPL induces a selection of the most productive firms that can accommodate the increase in labor cost (Poschke, 2009). Although built on a very different mechanic, our model relies on an intuition that relates to the latter.

Paper's organization The paper is structured as follows. Section 2 outlines the data sources used in the paper, the cleaning choices, and the methods to measure TFP and provides summary statistics of the sample. Section 3 discusses the identification and provides empirical evidence of the causal relationship between labor market flexibility and productivity. Section 4 develops a general equilibrium model of the economy to rationalize the empirical results in a steady-state equilibrium. Section 5 concludes.

2 Data, TFP measures, and summary statistics

We leverage data from multiple administrative sources, which allow for building a panel of linked workers and firms, augmented by *i)* detailed information on national labor contracts

(CCNLs)[3] and their renewal dates and *ii)* data on firms' balance sheets we use to build different TFP measures. In this Section, we further discuss these data sources, explain the TFP measures we employ, and provide some descriptive statistics of our sample.

2.1 Data sources

We construct the empirical analysis of this paper on three different data sources:

– the firm-level balance sheet panel data for incorporated firms in Italy from 1996 to 2016 (*Cerved* dataset);

– the matched employer-employee panel data on the universe of employment relationships in the Italian non-agricultural private sector in the same period (*Uniemens* database);

– the information on national collective bargaining agreements (CCNLs) reporting their renewal dates.

The first two datasets come from the Italian Social Security Institute (*Istituto Nazionale di Previdenza Sociale*, INPS), which guaranteed us exclusive access through the *VisitINPS Scholars* program.

Firms' financial data (Cerved) We use the proprietary firm-level data on balance sheets contained in the *Cerved* database to build three different measures of TFP, as Section 2.2 details. We restrict the sample to the twenty years spanned by the 1996-2016 period, and we include standard account variables such as revenues, value-added, labor cost, tangible and intangible assets, and cost of materials. We deflate these measures using three indexes: money value, industry prices, and industry costs at the three-digit sector level. In doing so, we primarily concentrate on the manufacturing industry, as we restrict the sample to those firms belonging to sectors for which proper deflators are available from the Italian National Institute of Statistics (*Istituto Nazionale di Statistica*, ISTAT). This way, we primarily concentrate on the manufacturing industry. We detail the deflation and cleaning procedure in Appendix B.

MEE data (INPS' Uniemens) From INPS, we access the detailed matched employer-employee records for the entire population of non-agricultural firms in the Italian private sector with at least one worker. This unique panel contains the monthly workers' employment histories, including detailed employee-level information on demographic characteristics, labor earnings, contract type (*tempo determinato*, temporary; *tempo indeterminato*, permanent; *apprendistato*, apprenticeship), and working time arrangement (part-time or full-time). More-

[3]In Italian labor law, CCNLs are employment contracts stipulated at the national level between the unions representing the employees and their employers or by the respective social partners following collective bargaining and subsequent agreement. They regulate almost the whole employment relationship in the country. CCNLs mainly set minimum wage floors within occupations that apply to contracts regardless of whether the employee is a union member and allow businesses to provide supplements above them. Moreover, they do not differentiate between temporary and permanent contract employees.

over, it includes information on the collective contract applied to each worker[4]. On the firm side, we observe their demographics—including births, deaths, and suspensions—the industry in which they operate and, clearly, their workforce composition, size, and the total labor cost suffered as the sum of yearly workers' earnings. Cleaning this dataset, we first select the primary employment relationship for each worker-year pair, adopting a prevalence criterion based on duration and, subordinately, earnings. Moreover, we restrict our sample to the establishments that employed five workers for at least one year within our sample period. This lower bound in size allows us to exclude tiny firms from the sample, for which a reliable measure of TFP would be impossible to obtain. Since we need to match this information with firm-level data, we further curb our Uniemens sample to firms that appear in the Cerved database. Again, we further detail our cleaning choices for this dataset in Appendix B.

CCNL data We complement our panel with data on the renewal dates of each CCNL, from the *Centro Nazionale dell'Economia e del Lavoro* (CNEL).[5] This information allows us to exploit the staggered implementation of the reform of employment legislation, on which the identification of our reduced-form analysis relies—as Section 3.1 discusses.

2.2 Measuring TFP

Throughout the analysis, we rely on total factor productivity as the primary outcome variable, as we interpret it as a proxy of the establishment's efficiency in production. TFP allows us to assess the effect of an EPL reduction on a margin resulting from mechanisms that differ from (or complement) the simple adjustment of production factors that follow a change in their relative prices. For each measure, we leverage our panel structure to parametrically estimate the residual ω_{it} of a Cobb-Douglas firm-specific production function

$$Y_{it} = K_{it}^{\beta_K} L_{it}^{\beta_L} M_{it}^{\beta_M} \exp \omega_{it}$$

where Y_{it} are deflated sales, K_{it} is capital (assets), L_{it} is the labor force, and M_{it} is the deflated cost of materials for firm i at time t. We adopt a different estimation method that we run sector-by-sector on the firm panel for each TFP measure:

1. OLS two-way (firm-year) fixed effect estimation of a log-log specification of the production function;

2. semi-parametric estimation through the control function approach of Levinsohn and Petrin (2003);

3. output-based proxy variable approach (based on Olley and Pakes, 1996) as refined by Ackerberg et al. (2015, ACF).[6]

[4]We use this information to build the firm's most-used contract each year, thus assigning a univocal treatment at the firm level, as detailed in Section 3.1.

[5]We thank Raffaele Saggio and his co-authors for sharing this dataset, collected and used for the first time in Daruich et al. (2020).

[6]As discussed by Ackerberg et al. (2015), a production function which is purely Cobb-Douglas in all its

We decided to include three measures to reduce our results' model dependency, given that TFP can considerably vary across the many methods developed in the literature for its estimation. Moreover, we rely on output-based measures as they allow us to include more observations in the analysis, compared to value-added measures, which constrain the sample only to firm-year pairs in which the value is non-negative when taking the logarithm.

2.3 Descriptive statistics of the sample

After all the data cleaning detailed in Appendix B, we end up with a selected sample consisting of 50,000-to-70,000 firms per year across around 65 three-digit sectors, mainly in manufacturing. This variability in the sample (Figure A.1) is mainly due to the changes in coverage of the Cerved database, which included an increasing number of firms during the year. Furthermore, the dimension of the sample size is determined by two factors in particular: first, we keep the firms for which we can observe balance sheet data from Cerved; second, we restrict to sectors for which we can use industry-specific prices and costs indexes as meaningful deflators.

Figure A.2 illustrates the time trend of some selected statistics for the sample: mean, variance, inter-quartile rate (p75-p25), and inter-extreme rate (p90-p10) for the three considered TFP measures. As discussed in the Introduction, the TFP growth trend is remarkably negative across all three measures. The overall variance exhibits an increase in the early years of the sample and a consequent reduction that continued up to 2016, the last year we consider. The trend remains perfectly consistent across the TFP measures, given that the Log-log CD specification shows a higher variance at each point in time than the other two. The same consideration goes for the inter-quartile and the inter-extreme rates, which conversely exhibit a much more steady evolution over time. Table A.1 shows a detailed descriptive statistics breakdown by 2-digit industry in the sample for three selected years. While the average TFP trend presents some heterogeneity across sectors, it is clear that the use of temporary contracts grew sharply before and after the reform that eased their adoption—while declining later. The industry-specific variance is typically steady over time, if not occasionally on a declining trend.

Figure A.3 reports the firms' entry and exit dynamics in our sample, taken at the province-by-3-digit-sector level each year. [7] The number of exit events—considered as permanent ceases or potentially temporary suspensions—has increased over time. The same trend is present when looking at the rates. This may reflect the stagnation in growth the country has experienced in the last 30 years. Coherently, the number of entry events—again, defined as either new creations or possible reactivations—had started to decline since the 2008 financial crisis when looked at in absolute numbers; and even before considering the rates.

inputs, like the one reported above, is not identified via a control function approach if one of its inputs is used as a proxy variable, *unless* there are frictions that affect the timing of firms' input choices. An example of such frictions that affect the labor input is EPLs, which are the focus of this paper. Hence, we find the method introduced by ACF an appropriate one to estimate the entire production function in this setting.

[7] For details on the construction of the two groups of entry and exit measures, please refer to Appendix B.

Still, both panels show that new firms continue to be born and enter the market at a higher rate than the one at which they cease their activity. On top of these macro-trends, we will assess in this paper how changes in EPLs have impacted the firm dynamics and through which channels.

3 The effect of flexible labor on productivity

This section discusses the empirical strategy behind our reduced-form analysis of the impact of labor flexibilization on the firms' productivity and sets out our results.

We provide causal evidence that an increase in labor market flexibility—intended as their possibility to leverage temporary contracts more easily—harmed firms, decreasing their productivity on average. Moreover, we assess the heterogeneity margin of such an effect. First, we show that such flexibilization impacted firms differently across their size distribution prior to the policy: large firms report a statistically insignificant effect, while smaller ones are the most harmed. Second, we provide evidence of the reform's heterogeneous distributional impact, which negatively hit the bottom of the TFP distribution and moderately positively the top. Such a result shows that the average negative effect masks substantial heterogeneity and is pulled by the many already-unproductive firms.

We do so by following Daruich et al. (2020) and Acabbi and Alati (2021) as we leverage the quasi-experimental variation offered by a 2001 Italian reform that decreased the formal requirements needed by the firms to start a temporary contract, making it easier to adopt temporary employment relationships. We show that the liberalization of temporary contracts caused a significant decrease in average TFP at the firm level through an event study design that exploits the staggered implementation of such a reform. Moreover, we present evidence of heterogeneous effects depending on firm size and on the distribution of the TFP itself—i.e., the reform impacted differently across productivity quantiles.

3.1 Institutional setting and identification

The worker protection legislation in Italy differs dramatically between permanent and temporary contracts.[8] Prior to the critical changes made by decree 368—based on the EU directive 1999/70/CE, which was signed into law on September 6, 2001—Italian businesses could only utilize temporary contracts for very particular reasons, and they had to notify the social security institute of the underlying conditions explicitly. The reform removed many constraints on the use of temporary contracts while leaving permanent contracts unaffected, effectively liberalizing the use of fixed-term employment arrangements.[9]

[8]A permanent contract is one that does not have a specified end date. As a result, a firm that wants to part ways with a worker recruited under such a contract must pay hefty firing fees, which vary depending on the size of the company and the worker's tenure. On the contrary, a temporary contract is a work agreement that has a set end date after which the firm can fire the employee without incurring any costs.

[9]Such intervention did not affect present employment protection provisions for existing (and permanent) contracts, resulting in a significant disparity in worker protection across contractual regimes and further expanding labor market dualism. Moreover, even after the reform, firms are only permitted to retain employees on temporary contracts for a certain amount of time.

Although the reform was enacted on a specific date, it wasn't until the related CCNL's renewal that the new rule took effect in a particular industry. Each Italian union (or group of unions) negotiates its own CCNL. Thus, the expiration dates vary across contracts and are well-known in advance. It ends up that the new rules on temporary contracts have been implemented at various times across CCNLs, as the reform did not interfere with their renewal patterns.

Given this institutional mechanism, we exploit the staggered renewal of 181 Italian CCNLs, leveraging the effectively staggered implementation of the temporary jobs liberalization across these contracts. This timing provides a plausibly exogenous change in the reform execution, thus offering a quasi-experimental source of variation in labor flexibilization across collective agreements. Through this setting, we can claim to identify a causal effect of easing the use of temporary jobs on productivity and other relevant firm-level outcomes.

3.2 Empirical strategy

We assess the causal impact of increasing the labor market flexibility—i.e., easing the use of temporary employment contracts—through a firm-level event study that takes advantage of the 368/2001 decree's gradual implementation across CCNLs. Here, causal identification comes from comparing as-good-as-randomly early-treated firms to later-treated ones, given the covariates.

In particular, we first focus on the relationship between labor market flexibility and firms' total factor productivity. We estimate the causal effect of the former on the latter in three different steps. First, we quantify the average effect through a baseline event study that compares within-firm changes in productivity before and after the liberalization to a dynamic group of control firms that haven't renewed their modal CCNL yet in the same—i.e., that are still functioning in the same year under the pre-reform laws. Then, we expand these results through a heterogeneity analysis of the firms' position along the sector-specific TFP distribution *before* the reform. Taking into account the heterogeneity in the TFP before the reform allows us to test whether the improvement in labor flexibility affected the productivity of firms that were already less productive differently than those already more productive. This is a salient component of our analysis since, intuitively, labor market flexibilization may harm already-unproductive firms while helping others. To do so, we include an interaction term between the dynamic coefficients and the TFP quartile in the baseline event study, thus building a "dynamic triple differences" specification that lets us compare the effects at the productivity distribution extremes. Finally, to further delve into the heterogeneity of the flexibility effects, we also assess the *direct* distributional impact of the reform. This third specification estimates the Quantile Treatment Effect (Callaway and Li, 2019) of the labor flexibilization on firms' productivity. Through this non-linear approach, we can conceptually compare the TFP distribution across firms following the liberalization of the use of temporary contracts relative to what it would have been if the reform had not been implemented.

Finally, we include an extension of the empirical analysis that addresses other relevant outcomes potentially affected by a labor market flexibilization coherently with the mechanisms we postulate. In particular, we focus at the firm level on total labor cost, total assets, and per-worker value added—as a measure of labor productivity. Moreover, we test whether the gained flexibility impacted firms' entry and exit rates at the province-industry level.

3.2.1 Baseline event study (average effects)

We start our empirical analysis with a baseline event study to assess the average effects of the reform on firms' productivity.

Specification We quantify the effect of an increase in labor flexibility on the total factor productivity by estimating the following event study linear regression at the firm level:

$$\mathrm{TFP}_{f,t} = \sum_{k=a}^{b} \mathbb{I}\left\{t = t^*_{c(f,2001)} + k\right\} \beta_k + \psi_f + \tau_t + \lambda_{p(f),t} + \varepsilon_{f,t} \tag{1}$$

The dependent variable $\mathrm{TFP}_{f,t}$ measures the log-TFP of firm f in year t as specified in Section 2.2. The function $c(f, 2001)$ returns the modal CCNL used by firm f in 2001—i.e., before the reform was implemented for everyone. Thus, $t^*_{c(f,2001)}$ denotes the reform's adoption year assigned to firm f, and $\mathbb{I}\{\cdot\}$ is an indicator function that returns a time-of-event dummy k periods away from the reform. Summing over $[a, b]$ thus gives a set of event dummies spanning the window around the year in which contract c was implemented. We further include firms and time dummies ψ_f and τ_t to account for the respective fixed effects and a province-year dummies interaction $\lambda_{p(f),t}$ that allows for province-specific time trends with year-specific varying slopes.[10] Finally, $\varepsilon_{f,t}$ is the error term. We are interested in estimating the event time coefficients β_k for $k \in [a, b]$.

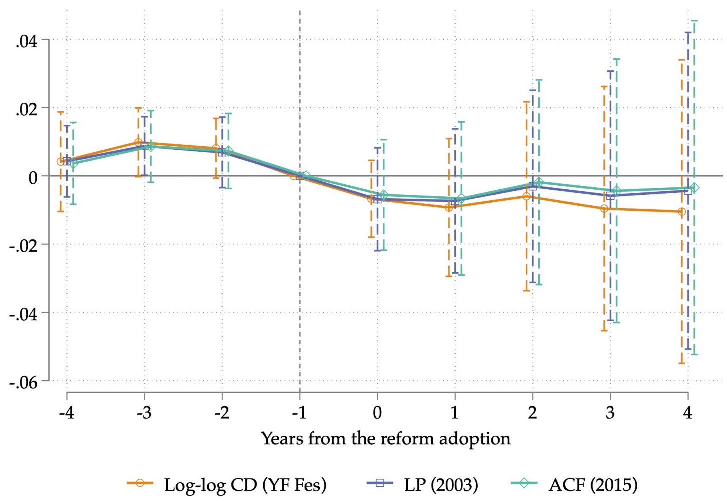
To correctly identify the coefficients of interest, we bin the event time dummies at $a = -6$ and $b = 6$ (thus avoiding the collinearity issues between event time and calendar time dummies described in Borusyak et al., 2021), and we normalize to zero two coefficients before the event: $\beta_{-5} = \beta_{-1} = 0$. Standard errors are clustered at the firm-by-year level. We leave the specification unweighted.[11]

Results On average, the liberalization of temporary contracts resulted in a slightly relative decrease in Total Factor Productivity at the firm level, statistically indistinguishable from a null effect. Figure 1 reports the estimated coefficients for the $[-4, 4]$ window around the reform for the three TFP measures discussed in Section 2.2. The results remain strongly consistent across the three measures and give rise to the natural question of whether this null average effect masks some degree of heterogeneity. Before the reform, the TFP trend is substantially flat—and pre-trend coefficients are positive when significantly different from

[10]For a more explicit interpretation of the non-linear province trends, the function $p(f)$ assigns to each firm f the province p in $k = -1$.

[11]This choice does not affect the qualitative margin of the exercise, which remains robust across alternative weighting decisions.

FIGURE 1: Avg. effect of temp contracts liberalization on TFP



Note. This figure reports the event study coefficients β_k for $k \in [-4, 4]$ from Equation (1) for three different measures of TFP, showing the average effect of the flexibilization of temporary contracts on the log-TFP at the firm level. Confidence intervals at 95 percent are obtained from firm-by-year level cluster-robust standard errors. Source: Istituto Nazionale della Previdenza Sociale (INPS) and Cerved.

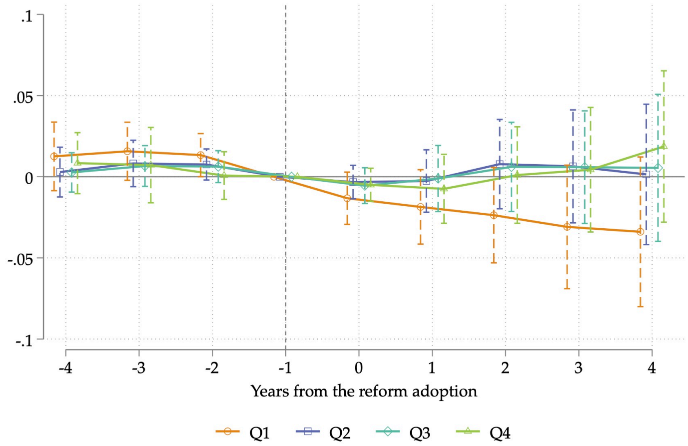
zero, meaning that the reform has switched the trend's sign. Our adverse mean result of an EPL reduction on TFP, although not significant, is consistent with the one of Cappellari et al. (2012), who leverage our same reform exploiting a much smaller CCNL sample and survey data on firms' sectors.

The question regarding the driver of this average effect is natural within our conceptual framework. We start to assess a raw heterogeneity margin by running the specification (1) separately for each TFP quartile *before* the reform's adoption. We indeed assign each firm to a time-constant quartile of TFP as in Devicienti et al. (2021). More precisely, we first compute the firm's position in the TFP distribution within a given sector-year pair; then, we assign to each firm the modal quartile to which the firm belonged in the last five periods before the enactment of the reform. Figure 2 reports the results of this exercise. Having reduced the constraints on temporary contracts impacted only the bottom quartile of the pre-reform TFP distribution: the firms that were already unproductive were damaged by the flexibility, which did not affect the other quantiles. Overall, all three panels do not show, again, sizeable differences between treated and yet-to-be-treated firms in the pre-period, offering suggestive evidence that the two lie on parallel trends before the reform.

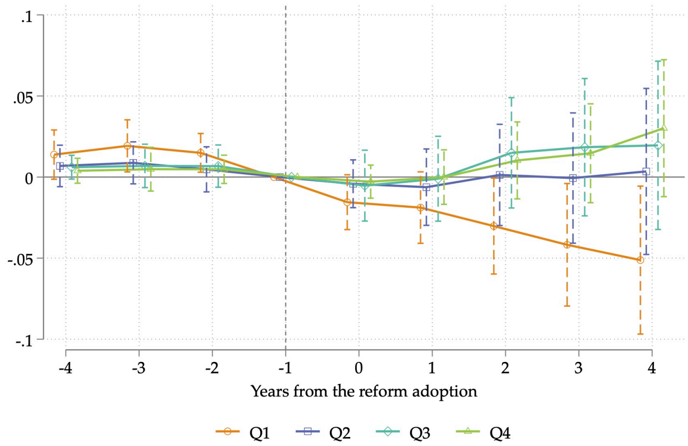
These results provide evidence of substantial heterogeneity behind the statistically null result shown in Figure 1. Our estimates show that the reform impacted negatively only within the bottom of the distribution, i.e., among those firms that were already unproductive at the time of the policy change. In contrast, the reform left the rest of the distribution untouched—with some positive yet insignificant effects within the top two quartiles.

Figure 2: Avg. effect of temp contracts liberalization on TFP, by pre-reform TFP quartiles

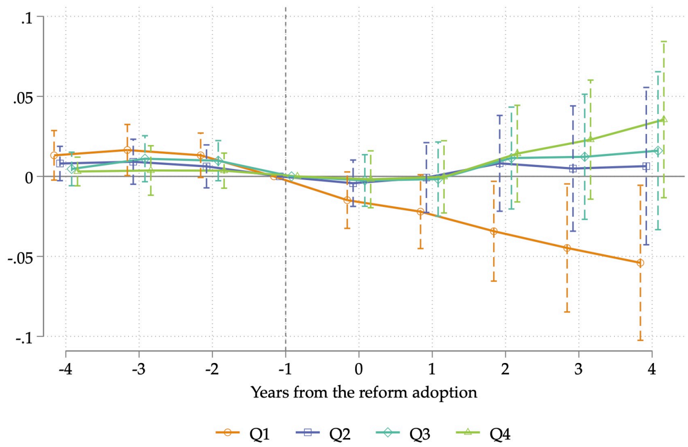
(A) Log-log CD (YF FEs)



(B) Log-log CD (YF FEs)



(C) Log-log CD (YF FEs)



Note. This figure reports the event study coefficients β_k for $k \in [-4, 4]$ from Equation (1), separately estimated across the quartiles of the TFP distribution before the event. Each panel shows the results using a different measure of TFP as the dependent variable: Log-log Cobb Douglas with year-firm fixed effects (A); Levinsohn and Petrin (2003) (B); Ackerberg et al. (2015) (C). The negative effect of temporary contract flexibilization on productivity is driven by the bottom quartile of the TFP distribution. Confidence intervals at 95 percent are obtained from firm-by-year level cluster-robust standard errors. Source: Istituto Nazionale della Previdenza Sociale (INPS) and Cerved.

3.2.2 Triple differences event study (heterogeneous effect by pre-reform TFP)

We further delve into the heterogeneity of the temporary contracts liberalization's effects along the pre-reform TFP distribution with a triple differences specification.[12] Here, we leverage the additional interaction to evaluate the differences in the reform's effects between the bottom and the top quartiles of the pre-reform productivity distribution, using the whole sample for the estimation.

Specification We investigate the possible heterogeneous effects due to differences in pre-reform TFP by including an interaction term between the event-time dummies and the quartiles of the pre-reform productivity, built as the previous paragraph discusses. In particular, we estimate the following specification:

$$\begin{aligned} \text{TFP}_{f,t} = & \sum_{k=a}^{b} \mathbb{I}\left\{t = t^*_{c(f,2001)} + k\right\} \theta_k + \sum_{k=a}^{b} \mathbb{I}\left\{t = t^*_{c(f,2001)} + k\right\} \times Q_{f,q}\beta_{q,k} \\ & + \psi_f + \tau_t + \lambda_{p(f),t} + \eta_{q,t} + \varepsilon_{f,t} \end{aligned} \tag{2}$$

Compared to Equation (1), we add a dummy $Q_{f,q}$ which takes value one if firm f belongs to the pre-reform TFP quartile $q = \{1, 4\}$, and $\eta_{q,t}$ are TFP quartile-by-year fixed effects that account for the quartile-specific heterogeneous time trends. The coefficient of interest is $\beta_{q,k}$, for $q = \{1, 4\}$, which has to be read as the performance of the top and bottom quartile in deviation from the two central ones.

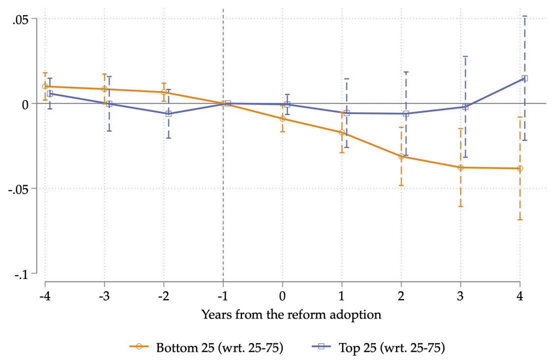
Results Figure 3 reports the results of the estimation of the triple differences. In particular, each panel shows the marginal effect of the temporary contracts liberalization on the two extreme quartiles of the TFP distribution in the years before the reform. The results should be read *in deviation* from the two central quartiles. All three groups of estimates indicate that the increase in market flexibility that followed the reform has damaged the bottom of the TFP distribution *more* than the two central quartiles. Similarly, the plots show that the top of the distribution experienced an effect on firms' TFP, which is slightly more positive than the median firm's—still, the difference between the top 25% and the second and third quartiles is statistically non-significant. We obtain robust results across all three measures. Again, the TFP specification based on the TWFE estimation (assuming the production function as a log-log Cobb Douglas) reports noisy results that become statistically significant when involving the other two more refined productivity measures. The reform effect on the top quartile is highly close to the one on the two central ones in the first two years after the reform and starts to diverge positively from the third year on.

Clearly, the results of the triple differences specification are closely related to the ones shown in Figure 2. There is strong evidence of substantial heterogeneity in the average impact of the reform on TFP: the negative effect is entirely driven by the bottom quartile

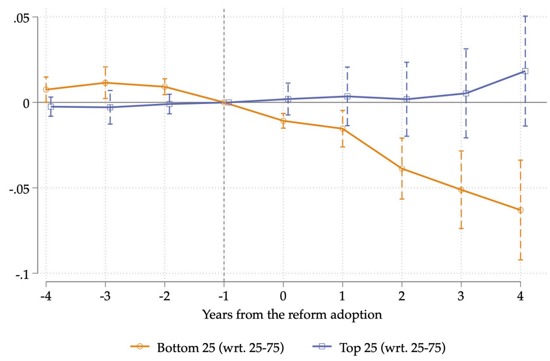
[12] As a matter of fact, we just add a second difference in this specification because the event study structure subsumes the two canonical differences of the diff-in-diff dynamically assigning control groups as collections of yet-to-treated units. This is why we consider adding an interaction term to the event-time dummies as moving to a triple differences specification.

FIGURE 3: Avg. marginal effects of temp contracts liberalization on TFP, by extreme pre-reform TFP quartiles

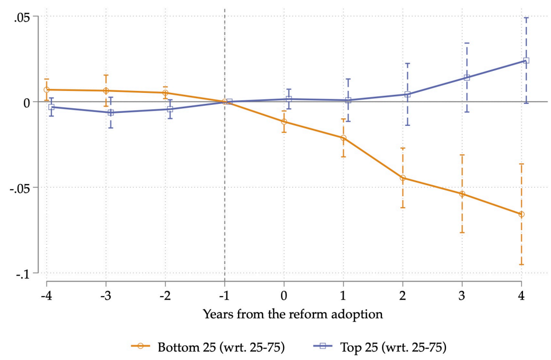
(A) Log-log CD (YF FEs)



(B) LP (2003)



(C) ACF (2015)



Note. This figure reports the event study coefficients $\beta_{q,k}$ for $q = \{1, 4\}$ and $k \in [-4, 4]$ from Equation (2), showing remarkable heterogeneity in the reform's effect: firms in the bottom quartile of pre-reform TFP experience a productivity drop significantly higher than in the second and third quartiles. Each panel shows the results using a different measure of TFP as the dependent variable: Log-log Cobb Douglas with year-firm fixed effects (A); Levinsohn and Petrin (2003) (B); Ackerberg et al. (2015) (C). Confidence intervals at 95 percent are obtained from firm-by-year level cluster-robust standard errors. Source: Istituto Nazionale della Previdenza Sociale (INPS) and Cerved.

of the pre-reform distribution, while the intervention left the rest nearly unaffected. We interpret this empirical evidence as an indication that the EPL reduction that occurred in the early 2000s harmed the TFP of the already-unproductive firms without having a sizeable impact on the rest of the distribution.

3.2.3 Quantile Treatment Effects (heterogeneous effects on the TFP distribution)

To understand whether reducing EPL by increasing labor market flexibility has impacted the TFP heterogeneously depending on how productive the firms were before the intervention, we discussed the effect heterogeneity along the pre-reform TFP distribution. Still, it is left to assess the *direct* distributional impact of the policy. In this section, we discuss the assumptions made and the steps involved to estimate the Quantile Treatment Effects (Callaway and Li, 2019) of the reform on the firms' TFP distribution—through which we aim to evaluate how having increased labor flexibility has heterogeneously impacted the TFP distribution itself. Intuitively, our exercise is meant to retrieve the TFP distribution across firms following the reform relative to the one that would have been if the liberalization had never occurred.

Identification We aim to estimate the Quantile Treatment Effect on the Treated (QTT) given by

$$\text{QTT}(\tau) = F^{-1}_{\text{TFP}_{1,t}\,|\,D=1}(\tau) - F^{-1}_{\text{TFP}_{0,t}\,|\,D=1}(\tau) \tag{3}$$

where τ is a quantile of the TFP distribution; $F_{\text{TFP}_{1,t}\,|\,D=1}$ and $F_{\text{TFP}_{0,t}\,|\,D=1}$ represent, respectively, the distribution of the firm's potential productivity $\text{TFP}_{1,t}$ and $\text{TFP}_{0,t}$, conditional on being reformed. To correctly estimate the QTT, we need to identify the marginal distributions of potential productivity—a task that requires the following empirical assumptions.

Empirical Assumption 1 (Distributional Parallel Trends)**.** Define $\Delta\text{TFP}_{0,t} = \text{TFP}_{0,t} - \text{TFP}_{0,t-1}$. Then,

$$\Delta\text{TFP}_{0,t} \perp\!\!\!\perp D$$

In words, the distribution of the change in the untreated potential TFP must not depend on the treatment status. This assumption is an extension of the standard diff-in-diff parallel trends assumption to a non-linear setting: conditioning on covariates, the distribution of the TFP path observed after the reform would not have changed if the temporary contracts had not been liberalized. As shown by Fan and Yu (2012), the Distributional Parallel Trends is not enough to point identify the counterfactual distribution of the outcome. As pointed out by Callaway and Li (2019), an additional assumption is needed.

Empirical Assumption 2 (Copula Stability)**.** Let $C\,(\Delta\text{TFP}_{0,t}, \text{TFP}_{0,t-1}\,|\,X, D = d)$ be the copula between the change in untreated potential TFP and its starting level, conditional on covariates X and being treated. Then,

$$C\,(\Delta\text{TFP}_{0,t}, \text{TFP}_{0,t-1}\,|\,X, D = 1) = C\,(\Delta\text{TFP}_{0,t-1}, \text{TFP}_{0,t-2}\,|\,X, D = 1)$$

Simply put, we assume the copula between the change in untreated potential TFP for the treated firms and its baseline level of untreated potential TFP for the same group—which summarizes the statistical dependence between these two variables—does not vary over time.[13] In other words, according to the Copula Stability Assumption, if firms with greater TFP tended to experience higher increases in TFP in the past, the same pattern will persist in the present in the absence of treatment.

Given these two assumptions, we can identify the counterfactual marginal distribution in (3) and, therefore, the QTT.[14] Intuitively, the Copula Stability Assumption is used to identify the joint distribution of $(\Delta\text{TFP}_{0,t}, \text{TFP}_{0,t-1}\,|\,D = 1)$, from which one can derive

[13]Notice that the assumption does not require any particular parametric copula, nor a specific form of dependence, as long as one exists and remains the same over time.

[14]To estimate the first term of equation (3), it is sufficient to invert the observed empirical distribution of the TFP for the firms the adopt the reform in a given year.

$F_{\mathrm{TFP}_{0,t}\,|\,D=1}$. Notice that both the marginal distributions of $\Delta\mathrm{TFP}_{0,t}$ and $\mathrm{TFP}_{0,t-1}$ are identified by the Distributional Parallel Trend Assumption and data, respectively—but this does not allow for the joint distribution's identification *per se*. As argued by Callaway and Li (2019), we use the observed dependence (the past copula) to build the information needed to identify $F_{\Delta\mathrm{TFP}_{0,t},\mathrm{TFP}_{0,t-1}\,|\,D=1}$ by exploiting the link between the joint distribution and the copula function established by Sklar's Theorem (Sklar, 1959).

Specification We framed this discussion as a standard diff-in-diff setting in which some units are treated, and others act as controls. Still, this is not the case in our setting, where we exploit the staggered implementation of a reform that eventually affects all the firms to build controls as yet-to-be-treated units dynamically. For this reason, we restrict the estimation of the QTT to two sub-experiments, as we would adopt a stacked-by-event design (Cengiz et al., 2019; Deshpande and Li, 2019). Conceptually, we limit our selection to firms that adopted the reform in 2002 and 2003—i.e., nearly 90% of our sample, as reported in Figure A.4, which shows the percentage distribution of the event years. For each cohort, we treat those employers who will incur the contract liberalization at least four years later as pure-control firms. This way, we obtain two balanced panels with no staggered implementation of the reform, and we run two separate QTTs on each of them.

Moreover, since the QTT is not linear, we cannot use the standard demeaning technique to rule out time- and unit-invariant heterogeneity. We then split the specification in two steps. First, we follow Canay (2011) and residualize the observed TFP from the unobserved heterogeneity we can address exploiting the rich panel structure of our data estimating predicted TFP values from the following two-way fixed-effect model:

$$\widehat{\mathrm{TFP}}_{f,t} = \psi_f + \lambda_{p(f),t}$$

Then, we use the residualized TFP measure to estimate the QTT as

$$\widehat{\mathrm{QTT}}(\tau) = F^{-1}_{\mathrm{TFP}_{1,t}-\widehat{\mathrm{TFP}}_{1,t}\,|\,D=1}(\tau) - F^{-1}_{\mathrm{TFP}_{0,t}-\widehat{\mathrm{TFP}}_{0,t}\,|\,D=1}(\tau) \tag{4}$$

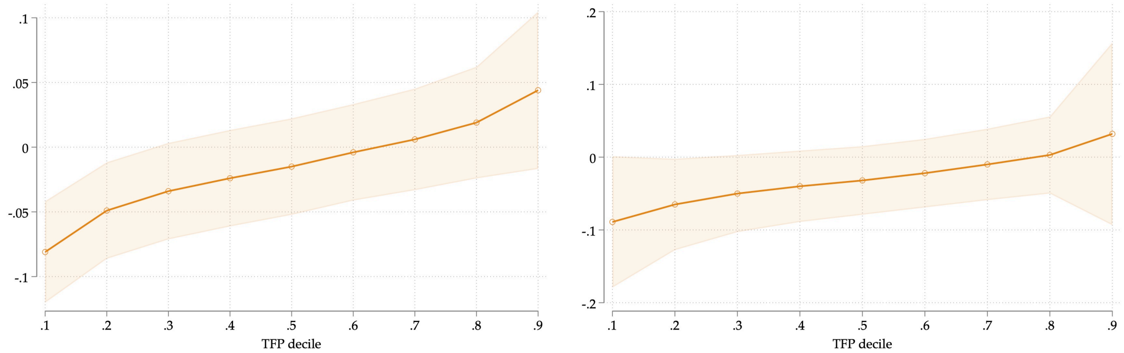
where the identification empirical assumptions now apply to the residuals $\mathrm{TFP}-\widehat{\mathrm{TFP}}|D = 1$. Finally, we compute the standard errors through empirical bootstrapping with 1000 iterations.

Results In Figure 4 we display the estimates of the effect of increasing the labor market flexibility on firm-level TFP.[15] The results should be interpreted as a short-term effect of the reform on the TFP distribution, as they are obtained using the first two years after the implementation. The average impact is estimated between -2.1% (treated in 2001) and -3.9% (treated in 2002), masking substantial heterogeneity along the TFP distribution since the effect is monotonically increasing in the TFP deciles for both treatment cohorts. In the two cases, at the 10th percentile, the firm-level TFP is nearly 8.5% lower than it would

[15]Here, we rely on the measure based on Levinsohn and Petrin (2003). Results remain consistent across the other TFP specifications.

FIGURE 4: Quantile Treatment Effect for selected years

(A) Reform adopted in 2002 (B) Reform adopted in 2003



Note. This figure reports the estimate of the QTT(τ) given in equation (4) for $\tau = (.1, \ldots, .9)$ on the residualized TFP. Results show substantial heterogeneity along the TFP distribution: the reform effect is strictly increasing in the quantiles, with an eventually-flipping sign of the coefficients. Confidence intervals at 95 percent are obtained through bootstrapping with 1000 iterations. Source: Istituto Nazionale della Previdenza Sociale (INPS) and Cerved..

have been had the increase in flexibility not taken place. Such an adverse effect disappears while climbing up the distribution and eventually flips for the last decile, where the reform is estimated to produce a TFP growth between 3.1% to 6.4%—although these effects are statistically not significant. Overall, the reform's impact remains negative along the majority of the distribution, even if it is non-significant almost everywhere: for example, the effect on the median ranges between -1.5% and -3.1%.

Notice that this result conveys additional pieces of information with respect to the heterogeneity analysis provided through linear models in the previous paragraphs. Through the event studies, we have been able to quantify the average marginal effect of belonging to a specific component of the pre-reform TFP distribution on the TFP. The QTT allows to address the direct effect of the reform *on* the quantiles of the TFP distribution, providing evidence of the heterogeneous distribution shifting that followed the increase in labor flexibility.

3.2.4 A brief discussion on the mechanism at play

Through the empirical analysis up to this point, we highlighted how having facilitated the use of temporary jobs ended up damaging the firms at the bottom of the TFP distribution—with nearly no effect on the rest of the employers. In particular, we showed that the reform damaged the firms that were already unproductive before the implementation of the new institutional setting and that the intervention impacted a firm's TFP increasingly in its position within the distribution.

In Section 4, we develop a theoretical model that formalizes our hypothesis of the economic mechanism behind this result at the equilibrium. Still, it is worth anticipating a brief discussion about our intuition. In the presence of the reform, the firms experienced a shrink in labor costs since they could use temporary contracts to adjust the workforce composition more flexibly, sparing the burdens connected to stable hirings and potential firings. This, in turn, triggers a double negative selection effect at the bottom of the distribution, as unproductive firms that should have left the market had the reform not taken place can survive for longer. At the same time, the entry productivity cutoff lowers, and more unproductive firms enter the market. Of course, this selection mechanism can operate only at the left of the TFP distribution, changing its composition in equilibrium.

If this is the case, we should observe *i)* a generalized drop in labor cost for all the firms due to the reform and; *ii)* a reduction of the exit events at the bottom of the distribution. Moreover, labor productivity (LP) can still be affected by the improved screening mechanisms that temporary contracts can provide (Faccini, 2014). Thus, we expect LP to increase. Notice that our results imply that the reform of temporary contracts widened the TFP distribution. Still, it is hard to get the intuition for this in the descriptives (see Figure A.2), as there probably exist other factors that countervail the reforms-induced mechanism.

3.2.5 Additional relevant outcomes

As just discussed, other outcomes beyond TFP are worth investigating, as they are supposed to vary consistently with the mechanism driving the relationship between the flexibility in the labor market and firms' TFP. Here, we focus in particular, on labor cost, productivity, and firms' exit events in province-by-sector cells.

Labor cost Labor cost is expected to reduce in the face of the temporary contract reform, as it is supposed to minimize frictions to labor cost adjustments.[16] Figure 5 reports the results of the estimation of the event study specification 1 using two measures of labor cost as the dependent variable: the total wages, computed as the raw sum of all the compensations paid by the employer in a year; and the labor cost as reported in the balance sheet. The reform has acted effectively as a downward pressure on labor costs, causing a reduction of around 13-to-14% after three years. This result is remarkably consistent across the two measures we used—the coefficients show really negligible differences—and is close to the findings of Acabbi and Alati (2021).

Firms' exits An increase in labor flexibility may create means for unproductive firms to survive more than they would have had, had the reform not been introduced. This, in turn, may explain at least part of the negative effect we have observed on post-reform productivity: an adverse selection mechanism might dominate within the lower component of the distribution.

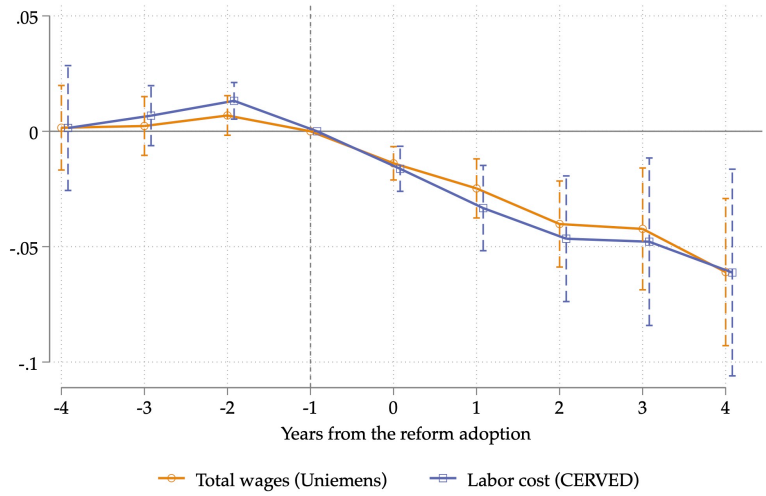
To test this hypothesis, we estimate the Poisson regression event study[17] specified as

$$\text{Exit}_{p,s,t} = \sum_{k=a}^{b} \mathbb{I}\left\{t = t^*_{c((p,s),2001)} + k\right\} \beta_k + \frac{1}{3}\sum_{j=-3}^{-1} m_{p,s,j}\gamma + \tau_t + \lambda_{p,t} + \eta_{s,t} + \varepsilon_{p,s,t} \tag{5}$$

[16]Daruich et al. (2020) show that firms increase their churn rate by substituting more often temporary workers with temporary workers after the reform.

[17]Since the number of panel disappearances and ceases is a count variable, we rely on a Poisson specification.

FIGURE 5: Avg. effect of temp contracts liberalization on labor cost



Note. This figure reports the event study coefficients β_k for $k \in [-4, 4]$ from specification (1) using two measures of labor cost as the dependent variable: the total wages, computed as the raw sum of all the compensations paid by the employer in a year; and the labor cost as reported in the balance sheet. Confidence intervals at 95 percent are obtained from firm-by-province-sector level cluster-robust standard errors. Source: Istituto Nazionale della Previdenza Sociale (INPS) and Cerved..

Here, $\text{Exit}_{p,s,t}$ is the number of exit events that happened in year t within a cell defined by province p and sector s. Similar to what we do in the baseline specification (1), we assign the treatment to the province-sector pair by taking the modal collective bargaining agreement used in the cell in 2001. Moreover, we control for the average number of firms in the cell in the three periods ahead of the reform, $\frac{1}{3}\sum_{j=-3}^{-1} m_{p,s,j}$, and we include a set of year-sector dummies that account for industry-specific heterogeneous trends in time. Standard errors are clusterized at the year-by-province-sector level.

We report the results of the estimation of specification (5) in Figure 6 for two different definitions of a firm's exit. The left panel uses the date on which a firm is certified dead in the INPS record (registered firms' ceases). The right panel uses disappearances from the firm-year panel: since a firm remains visible on it as long as it has at least one worker in the year, this way, we proxy both permanent exit and inactivity periods.[18] We run separate regression for the number of exiting firms that belong to each of the four quartiles of the ex-ante TFP distribution. This way, we can assess whether the reform impacted the number of activity terminations heterogeneously by the firms' position along the pre-reform TFP distribution.[19]

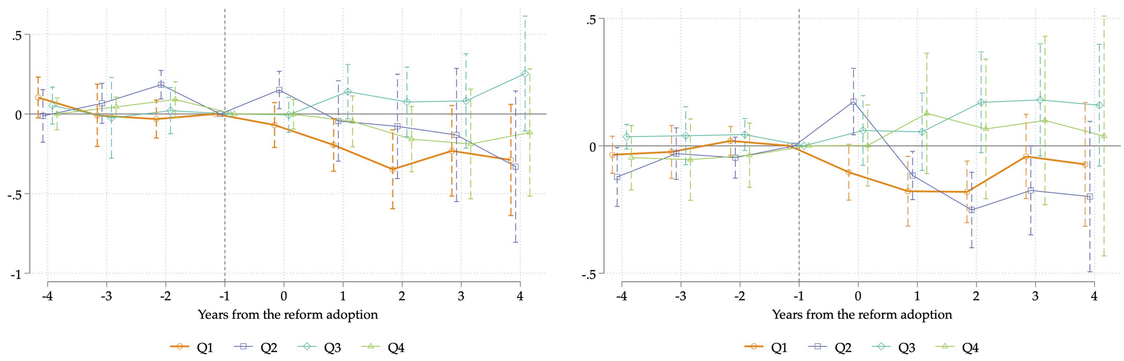
The reform reduced the exit events of firms belonging to the bottom quartile of ex-ante TFP distribution, and the effect remained consistent for both firms' cease and disappearances. The number of firms ceasing among the left tail dropped by 19% after one year and 34% after two. The effect remains negative three years after the reform, although no longer sta-

[18]Appendix B contains further detail on the building of the two exit measures.

[19]Again, we use here the TFP measure based on Levinsohn and Petrin (2003), but the results remain qualitatively the same across the other measures.

FIGURE 6: Avg. effect of temp contracts liberalization on exit events at the province-industry level, by ex-ante TFP quartile

(A) Firms' ceases (B) Firms' disappearance from the panel



Note. This figure reports the event study coefficients β_k for $k \in [-4, 4]$ from the Poisson specification (5). Each panel shows four separate regressions, one for every exit number of firms belonging to a specific ex-ante TFP quartile. The reform reduces the exits of the sole already-unproductive firms for two years before the effect fades out (thicker, orange line.) Confidence intervals at 95 percent are obtained from firm-by-province-sector level cluster-robust standard errors. Source: Istituto Nazionale della Previdenza Sociale (INPS) and Cerved..

tistically significant. The results stay surprisingly similar when considering the number of disappearances from the firm panel. In this latter case, the effect is again significant after one year and remains so for two periods, when the number of firms exiting drops by nearly 18%. Again, the effect fades out starting from three periods from the reform's adoption. The reform proves to have provided firms with a chap labor input, allowing the survival of low-productivity employers that would not have kept on producing otherwise.[20] This evidence supports our hypothesis of a link between the increase in labor flexibility and an adverse selection effect on the bottom of the productivity distribution—an intuition that we formalize in the model developed in the following Section 4.

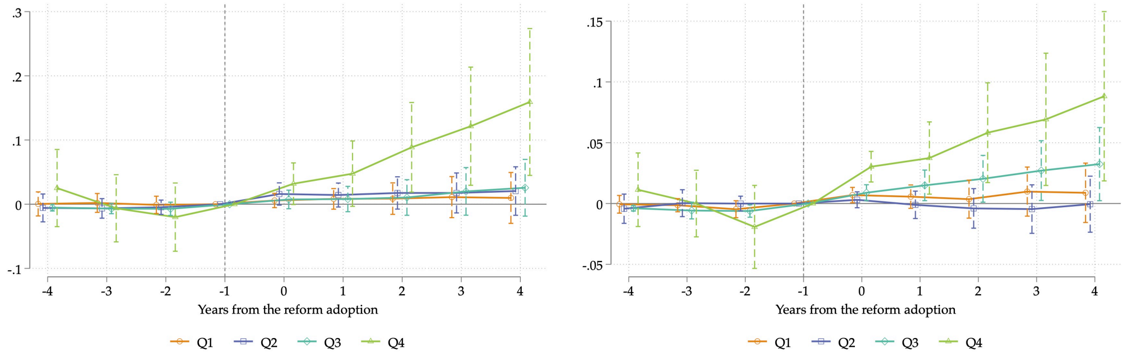
Labor productivity Labor productivity (LP) is another relevant proxy for firms' heterogeneity, which, albeit being often used in place of TFP, naturally leads to different measures (Sargent and Rodriguez, 2001; Tang, 2017). Through this paper, we chose to adopt TFP as the primary measure of productivity because the mechanism that links it to labor market reform is interestingly different from the straightforward adaptation of production factors to changes in their relative prices. Moreover, the link between EPL and TFP is more understudied than the one with LP. Still, we do not regret the latter.

To assess the reform's effect on firm-level LP, we estimate the baseline specification given in (1) on two measures of LP: per-worker sales and per-worker value added. We assess the heterogeneity margin of the reform's effect on LP similarly to what we have done for TFP: we run the estimation separately by pre-reform quartiles of the measure-specific LP

[20]Such a result is consistent with Daruich et al. (2020), who show that the reform had a decreasing effect on labor productivity among firms with an ex-ante low probability of surviving—an impact that they explain through the creation of low-quality jobs that would not have been formed in the absence of the reform.

Figure 7: Avg. effect of temp contracts liberalization on LP, by ex-ante LP quartile

(A) Per-worker sales (B) Per-worker value added



Note. This figure report the event study coefficients β_k for $k \in [-4, 4]$ from a version of Equation (1) in which the dependent variable is a measure of labor productivity. Panel A reports the estimates on the whole sample, and Panel B those obtained by running separate regressions across the quartiles of the pre-reform LP distribution. The reform's effect on LP is positive only for the firms at the top of the distribution (light green line) and null for the rest. Confidence intervals at 95 percent are obtained from firm-by-year level cluster-robust standard errors. Source: Istituto Nazionale della Previdenza Sociale (INPS) and Cerved..

distribution.[21] Figure 7 reports the results of the quartile-specific estimations for the two LP proxies: per-worker sales (pws, Panel A) and per-worker value added (pwva, Panel B). The reform impacted the firm-level LP with substantial heterogeneity but poles apart compared to TFP. Our results consistently show a sizeable positive effect of the increase in labor market flexibility on the top quartile of ex-ante LP across the two measures we employ. Firms at the top of the distribution experienced an increase in LP that varies between nearly 10% (pws) and 5% (pwva) three years after the reform. The rest of the distribution sustained no effect at all in the case of pws, while we registered a moderate yet significant increase for the third quartile in pwva, suggesting an increasing effect of the reform along the distribution.

These results can be explained by using temporary contracts as screening devices when a worker's productivity is observed imperfectly (Faccini, 2014). LP increases because of a compositional shift in labor quality due to more efficient screening.[22] In particular, our heterogeneity result pushes toward this direction: firms with ex-ante high labor productivity can be supposed to have already-good screening capacity of their candidates. Coherently, increasing the use of screening devices such as temporary contracts can disproportionally benefit those firms rather than the rest of the distribution.

Due to the substantial heterogeneity we documented, the average effect of the policy on LP when considering the entire sample is imprecisely estimated and statistically non-significant. Such a result is consistent with the evidence from Daruich et al. (2020), given the relevant

[21]We assign each firm to a time-invariant ex-ante LP quartile following the same procedure we adopted for TFP.

[22]Moreover, such a result can be easily reconciled both within a classical competitive model and an equilibrium unemployment model à la Mortensen and Pissarides (1994). Indeed, a decrease in EPL—i.e., a reduction in dismissal costs—increases the individual productivity threshold at which employees are willing to extinguish current matches, in turn increasing the number of efficient separations and, thus, labor productivity.

sample differences between ours and their study: the estimated average effect remains steadily noisy across the two. Moreover, the negative effect in Daruich et al. (2020) is driven by low-quality firms (with low-profit margins, value added per worker, and labor costs per worker), while in our case, the positive effect concentrates among firms with ex-ante high LP. These two results are qualitatively coherent, yet they differ in their quantitative assessment.

4 Model

To aid the interpretation of our empirical results, in this section, we introduce a full-fledged theoretical framework that relates the equilibrium productivity distributions across sectors of the economy to frictions in both the labor and capital markets. In addition to providing predictions that largely match our main empirical results, the model highlights how the labor wedges may have heterogeneous effects and ambiguous net impact, as they can potentially mitigate misallocation effects due to other kinds of distortions.

Our model builds on the familiar closed-economy monopolistic competition framework with heterogeneous firms developed by Melitz (2003). This helps disentangle several dimensions of firm behavior—such as entry, exit, and investment—from other features of the economy like labor supply. In our model, we introduce financial frictions (FFs) due to asymmetric information and post-entry productivity-enhancing investments (PEIs). While both FFs and PEIs have appeared in previous contributions, our *joint* treatment of them is both novel and tractable, as we are to elaborate. In what follows, we keep our notation as close as possible to that of the original Melitz (2003) model.

4.1 Setup

We analyze a typical Melitz (2003) economy where preferences for individual goods are characterized by a Constant Elasticity of Substitution (CES) $\sigma > 1$: a representative consumer has a utility function $U^{\frac{\sigma-1}{\sigma}} = \int_{\omega\in\Omega} q(\omega)^{\frac{\sigma-1}{\sigma}} d\omega$, where Ω is the set of varieties available in equilibrium and $q(\omega)$ is the quantity of product $\omega \in \Omega$ that is consumed. Firms, which offer the varieties, are heterogeneous in productivity $\varphi(\omega) > 0$ and characterized by a linear cost function with increased returns caused by fixed costs $f > 0$, borne in every period when a firm operates in the economy. The labor demand function is also linear in quantity and writes as $l(q) = f + q/\varphi$. In this economy, labor is supplied inelastically by a mass of workers L; each unit of labor is paid a wage w, which we normalize as unitary ($w = 1$).

This economy inherits all the standard properties of the monopolistic competition model by Dixit and Stiglitz (1977) as developed later by Melitz (2003). In particular, for each firm optimal quantity is a power function of productivity with exponent σ, whereas both revenue and profit scale with exponent $\sigma - 1$. Hence, for any two firms with productivity φ_1 and φ_2, the ratio of their equilibrium revenues is $r(\varphi_1)/r(\varphi_2) = (\varphi_1/\varphi_2)^{\sigma-1}$. As in Melitz (2003), here, the probability distribution of productivity is endogenous and expressed by a density function $\mu(\varphi)$. However, how this distribution is determined in equilibrium differs in our model. In addition to a modified *entry decision* that firms take, the latter also must secure *setup financing* by financial intermediaries. In what follows, we call financial intermediaries more simply "banks."[23]

The set of varieties Ω with associated productivity $\varphi(\omega)$ is determined through the interaction between *entrepreneurs* and *banks*. We define an entrepreneur as a pair (φ, θ), where $\theta > 0$ is an individual signal about the productivity φ. These two random variables are drawn from a joint probability distribution $G(\varphi, \theta)$, which is common knowledge; however, they are initially unobserved by all the agents involved. Banks are instead described as a mass B of risk-neutral workers endowed with the ability to convert any unit of labor into a unit of "capital," a unique good used solely to set up firms.[24] These are, in turn, generated as follows.

1. A given mass of entrepreneurs decides whether to attempt setting up a firm. To do it, they must bear a one-shot—thus sunk—experimentation cost f_n. This provides information about the signal θ, which entrepreneurs and banks observe.

2. Thereafter, firms must secure capital financing equal to f_b units of labor, which only banks can provide. Only if both f_n and f_b are paid the true productivity φ is revealed. In exchange for paying f_b, banks demand a permanent claim over a share $b(\omega) \in (0, 1]$ of *all* future profits $\pi(\omega)$ of a firm supplying variety ω. The capital market is perfectly competitive: entrepreneurs can purchase capital from any bank without frictions.

3. Lastly, all extant firms set their prices and quantities; firms may even choose to exit and supply zero output if the optimal profits conditional on producing are negative (because of fixed costs). Firms then operate in the economy until an event occurring with exogenous probability δ forces them to exit.

This augmented entry-stage features FFs due to informational asymmetries since, at the financing stage, banks can neither see nor verify entrepreneurs' true productivity (it is irrelevant whether the latter can or not). Existing versions of the Melitz model that allow for FFs (see Manova, 2013 and Chaney, 2016) typically introduce liquidity constraints that firms are subject to only when they face costs for entering foreign markets. In our model, FFs also affect the entry into the domestic market, as our main objective is to explain potential sources of misallocation while abstracting from considerations about trade.[25] Our choice of modeling FFs through informational frictions makes the model fairly tractable. It helps isolate one specific channel: the firm selection on the extensive margin in the left tail of the productivity distribution.[26]

[23]This is fitting in the Italian setting, where commercial banks dominate capital markets.

[24]This is both a simplification and a normalization: a more elaborate production function for the capital good would not change the analysis substantively.

[25]One can see our distinction between the two fixed costs f_n and f_b as kind of liquidity constraint: of the full Melitz entry cost f_e, entrepreneurs are only able to pay $f_n < f_e$ upfront, with $f_b < f_e - f_n$ to be financed by banks.

[26]In a recent contribution, Unger (2021) introduced an augmented Melitz model where firms face financial frictions in the post-entry stage, as they need to anticipate part of both variable and fixed production costs in every period before realizing revenues. Contrary to our model, his framework (which features moral hazard)

For the sake of exposition, for the moment, we abstract from PEIs. Following our analysis of the (closed) economy with financial frictions at the entry stage, we discuss the implications of post-entry choices about PEIs made by firms.

4.2 Analysis

After the set of firms that paid up both entry fixed costs f_n and f_b is determined, firm behavior proceeds as in the Melitz model. To appreciate how financial frictions affect firms' selection, we solve the entry stage recursively, starting from the financing stage. To clarify the trade-offs that banks face, we introduce an innocuous assumption.

Assumption 1. *Signal informativeness*: if $\theta_1 > \theta_2$ are two different realizations of the signal θ, then $G(\varphi|\,\theta_1) \leq G(\varphi|\,\theta_2)$ for any $\varphi > 0$.

This assumption states that signals are ordered in a way that higher values lead to conditional distributions of productivity that first-order stochastically dominate those from lower values.[27]

There are two key implications of Assumptions 1: first, lower signals imply a higher risk for banks; second, as θ is the only information that banks receive about firms, set shares $b(\theta)$, that is the fraction of total equity they demand to entrepreneurs in exchange for f_b, which is only a function of the signal. Therefore, when financing an entrepreneur with signal θ, a bank's expected profit is $\widetilde{\pi}(\theta)\, b(\theta)/\delta - f_b$, where $\widetilde{\pi}(\theta)$ is the *unconditional* per-period profit (which incorporates the probability that a firm exits after observing φ) that one can expect from setting up a firm under signal θ.

Perfect competition in capital markets leads to an equilibrium where banks make zero profits in expectation. The reason is straightforward: there cannot be an equilibrium where $\widetilde{\pi}(\theta)\, b(\theta)/\delta > f_b$ for any value of $\theta > 0$, or else any subsets of banks with mass $B' < B$ would find it profitable to set a strictly lower share $b'(\theta) < b(\theta)$ and capture all the profits from firms generated by signal θ. Banks would not make negative profits either, as they would simply deny financing to all entrepreneurs with signal values such that $\widetilde{\pi}(\theta)\, b(\theta)/\delta < f_b$. If such a strict inequality is theoretically possible in the support of θ for some fixed primitives of the model, Assumption 1 implies the existence of a threshold signal that makes banks indifferent towards financing an entrepreneur under the assumption that they would capture all the profits of the resulting firm, i.e., the smallest positive number θ^* such that

$$\frac{\widetilde{\pi}(\theta^*)}{\delta} - f_b = 0. \tag{6}$$

We guess that a suitable value of θ^* exists; we verify *ex post* whether this is true.

This analysis implies that in equilibrium, only those firms with signal $\theta \geq \theta^*$ receive financing, $b(\theta^*) = 1$, and for any two signals $\theta_1 \geq \theta^*$ and $\theta_2 \geq \theta^*$, banks set shares that

predicts that financial frictions lead to a more intense selection effect, as the least productive firms face tighter access to credit.

[27]This comes without any loss of generality: as signals are abstract, they can always be transformed in such a way that Assumption 1 holds by construction.

yield zero profits in expectation with the property that $b(\theta_1)/b(\theta_2) = \widetilde{\pi}(\theta_2)/\widetilde{\pi}(\theta_1)$,[28] and $\widetilde{\pi}(\theta)\, b(\theta) = \widetilde{\pi}(\theta^*)$ for any $\theta \geq \theta^*$. Since (6) completely summarizes the trade-off faced by banks and the equilibrium in the capital markets, we call it (with some abuse of terminology) the Arbitrage Condition (AC), as it subsumes the fact that banks demand higher shares in exchange for riskier signals.

The initial entry decision by entrepreneurs is conceptually simpler. The expected value of generating a business idea is $v_n = \delta^{-1} \int_{\theta^*}^{\infty} \widetilde{\pi}(\theta) \left[1 - b(\theta)\right] dC(\theta)$, where $C(\theta)$ is the marginal cumulative distribution of the signal θ. Since entrepreneurs are free to attempt entering the economy and generate new signals, they would only refrain from doing so if the value of entry v_n falls shorter of the experimentation cost f_n. Thus, incorporating the equilibrium in the subsequent financing subgame and the value of the bank share $b(\theta)$ implies the following Free Entry (FE) condition in the economy:

$$\int_{\theta^*}^{\infty} \frac{\widetilde{\pi}(\theta)}{\delta} dC(\theta) - \left[1 - C(\theta^*)\right] f_b - f_n = 0. \tag{7}$$

Together with the Arbitrage Condition (6), this equation characterizes the economy's equilibrium. As (7) shows, entrepreneurs anticipate the probability of bearing the financing cost f_b, which they only bear if they receive a signal $\theta \geq \theta^*$.

To complete the analysis, it is necessary to characterize the function $\widetilde{\pi}(\theta)$. Following the analysis of the post-entry phase of the Melitz model, given a value of θ one has:

$$\widetilde{\pi}(\theta) = \mathbb{E}_{\varphi|\theta}\left[\pi(\varphi)|\, \theta\right] = f \left\{ \int_{\varphi^*}^{\infty} \left(\frac{\varphi}{\varphi^*}\right)^{\sigma-1} g(\varphi|\, \theta)\, d\varphi - \left[1 - G(\varphi^*|\, \theta)\right] \right\}, \tag{8}$$

where $g(\varphi|\, \theta)$ is a conditional density function derived from $G(\varphi|\, \theta)$ whereas φ^* is the threshold value of productivity below which, in equilibrium, firms find production unprofitable and exit. Note that (8) implicitly embeds a "Zero Profit Condition" *à la* Melitz, which is specific to θ. A pair of thresholds (θ^*, φ^*), one for the signal and one for productivity, completely determines the equilibrium—if one exists.

Here we show the existence and uniqueness of the equilibrium in a particular case: we formulate it through the following Assumption.

Assumption 2. *Log-normality*: $G(\varphi, \theta)$ is a cumulative bivariate (joint) log-normal distribution with standard log-normals as marginals. Let $\rho \equiv \mathbb{C}\mathrm{orr}\left(\log \theta, \log \varphi\right)$.

While it greatly facilitates the analysis, this normality assumption does not detract any realism content from the model, as a (truncated) normal distribution notoriously well approximates log-productivity distributions. Assuming that the marginals are standard is a normalization that comes with no loss of generality (again, recall that one can transform the signal at will). Note that Assumptions 1 and 2 imply that $\rho \geq 0$: the signal and the true productivity are non-negatively correlated.

[28]This result can be formulated formally as a Bayes-Nash equilibrium.

Proposition 1. An equilibrium pair (θ^*, φ^*) always exists, is unique and identified by the intersection between the curve of the points satisfying the AC, given by $\varphi^* = A(\theta^*)^\rho$ for an appropriate constant $A > 0$, and a globally concave curve tracing the points that satisfy the FE condition. The intersection always occurs at the global maximum of the implicit function of φ^* for θ^*, as traced out by the FE curve.

Proof. The proof can be found in Appendix C.1. □

Figure 8 depicts the equilibrium as the intersection between the two solid lines. The AC curve is constantly increasing because higher threshold values set by banks mechanically translate into higher average productivity as better firms are selected, and vice-versa. Instead, the FE curve is concave due to the interaction of two mechanisms. As in the Melitz model, the higher the productivity threshold, the higher the profits needed to motivate entry. The exact mechanism is at work for the signal threshold; hence for low values of θ^*, the latter increases alongside φ^* on the FE curve. At the same time, a higher signal threshold implies a lower probability that firms repay the financing cost f_b, thereby increasing the relative entry value. The latter effect dominates at high values of θ^* and makes the FE curve decrease in that section. Intuitively, the equilibrium lies at the maximum of the FE curve because of perfect competition between banks: the latter lends the financing cost f_b so long as the benefits exceed the costs.

Proposition 2. Adding a wedge $\tau > 0$ to firms' labor costs (but not to either entry cost f_n or f_b) such that the effective wage increases from $w = 1$ to $w_{(\tau)} = 1+\tau$, leads to an equilibrium $\left(\theta^*_{(\tau)}, \varphi^*_{(\tau)}\right) \gg (\theta^*, \varphi^*)$ in which both thresholds are higher with the wedge.

Proof. The proof can be found in Appendix C.2. □

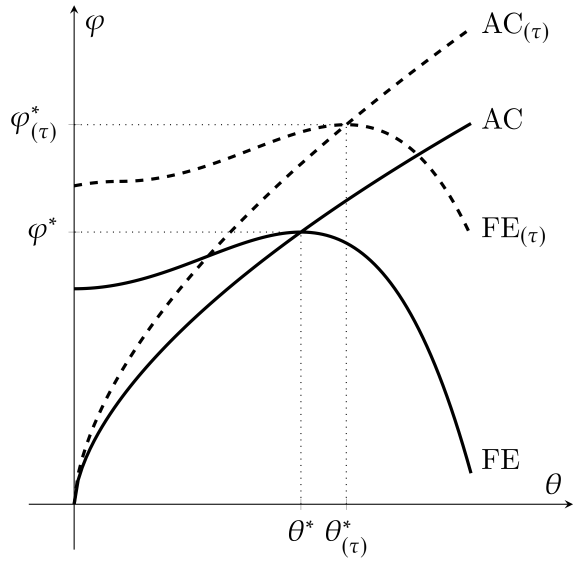
The intuition is straightforward: higher labor costs make it harder for firms to repay their fixed costs and survive in the market, leading to sharper selection. The Melitz model translates this effect in a downward rotation of the Zero Profit Condition curve. In our model, the wedge causes a leftward rotation of the AC curve and a rightward shift of the FE curve. As the two curves must still meet in the latter's maximum of the implicit function for θ^* as traced out by the FE curve, both equilibrium thresholds are inevitably higher. This is displayed graphically by the two dashed curves in Figure 8.

4.3 Welfare

This model has interesting non-trivial welfare implications. There are two key differences with respect to the Melitz model. First, there are three types of labor to be remunerated: entrepreneurial (L_n), bank (L_b) and production (L_p) labor, with the total labor force being $L = L_n + L_b + L_p$. Second, FFs lead to the optimality result by Dhingra and Morrow (2019), according to which the Melitz economy provides the optimal product diversity and firm size distribution to collapse. This is summarized as follows.

Proposition 3. Social welfare is increasing in ρ, and is maximum in $\rho = 1$.

FIGURE 8: Equilibrium of the model and comparative statics



Note. This picture portraits the equilibrium pair (θ^*, ϕ^*) as the intersection between the solid lines depicting the AC condition (6) and the FE condition (7). The AC curve is upward sloping because the higher the signal threshold, the higher the productivity due to selection, and vice-versa. For $\theta \leq \theta^*$, higher productivity, and signal thresholds call for higher profits to motivate entry, which are thus increasing alongside the two. At the same time, if $\theta \geq \theta^*$ the higher the signal threshold, the lower the probability of paying back the financing cost f_b, increasing the relative entry value. There, the FE curve is downward sloping. The dashed line represent the shift to the FE and the leftward rotation of the AC curves due to the introduction of a wedge $\tau > 0$ in the labor cost.

Proof. The proof can be found in Appendix C.3. □

Again, the intuition is simple: more informative signals lead to a more efficient allocation of the financing cost f_b. This is best illustrated by two extreme, degenerate cases, also discussed in the Appendix, whereby $\rho = 0$ and $\rho = 1$, respectively, which can be seen as two Melitz economies with different primitives. If $\rho = 0$, all entrepreneurs are financed in equilibrium, even if they leave the economy after their true productivity's revelation. If instead $\rho = 1$, no financial resources go wasted as all financed entrepreneurs stay in the economy.

A key implication is that price distortions may lead to second-best outcomes.

Proposition 4. Adding a wedge $\tau > 0$ to firms' labor costs (but not to either entry cost f_n or f_b), and which does not enter workers' utility, raises average productivity in equilibrium and has ambiguous effects on social welfare.

Proof. The proof can be found in Appendix C.4. □

This fact is illustrated in the Appendix, also making recourse to the two extreme cases mentioned above. The intuition goes as follows: in the presence of FFs, higher labor costs make entry less profitable (thus depressing social welfare), but they also raise the signal threshold θ^* as per Proposition 2. This in turns leads to a higher equilibrium productivity (due to a pure *selection* effect) and less "waste" on the financing cost f_b. Both the latter mechanisms are welfare-enhancing as opposed to the former, negatively impacting welfare by reducing entry. Note that this holds under the assumption that τ does not affect workers' compensation and social welfare *per se*. If τ is due to EPLs, workers likely extract utility from it.

4.4 Extensions

We next sketch a version of the model that features PEIs. In the analysis developed so far, the equilibrium productivity distribution of the model obtains as a truncated version of the distribution firms draw their productivity from, as in the Melitz model: $\mu(\varphi) = [1 - G_0(\varphi^*)]^{-1} g_0(\varphi)$, where $g_0(\varphi)$ and $G_0(\varphi)$ are the marginal p.d.f. and c.d.f. for φ, respectively. We now allow firms to adjust their productivity after entry. Specifically, we add a further, final stage of the model where firms are allowed to *set* a productivity level $\breve{\varphi}$ subject to a decreasing cost in their original draw φ.

We specify the firm optimization problem as the difference between the additional profits obtained by raising productivity from φ to $\breve{\varphi}$ and the cost of the raise:

$$\max_{\breve{\varphi}} B\left(\breve{\varphi}^{\sigma-1} - \varphi^{\sigma-1}\right) - \kappa\left(\frac{\breve{\varphi}}{\varphi}\right)^{\alpha}, \tag{9}$$

where B is a constant that comes from the Dixit-Stiglitz analysis of monopolistic competition, while κ and α are two technological constants. We assume $\alpha > \sigma - 1$ to ensure that the cost of the raise scales faster than the benefit, thereby making the problem salient. The problem is globally concave, and the solution is straightforward:

$$\breve{\varphi} = \left[\frac{B(\sigma-1)\varphi^{\alpha}}{\alpha\kappa}\right]^{\frac{1}{\alpha+1-\sigma}}. \tag{10}$$

This delivers a monotone increasing mapping $\breve{\varphi}(\varphi)$ and an equilibrium productivity distribution expressed by $\mu(\varphi) = [1 - G_0(\breve{\varphi}^*)]^{-1} g_0\left(\breve{\varphi}^{-1}(\varphi)\right) \frac{d}{d\varphi}\breve{\varphi}^{-1}(\varphi)$, where $\breve{\varphi}^*$ is the new productivity threshold that obtains in the new equilibrium where firms have enhanced their productivity.

The implications of adding PEIs to our analysis of labor market distortions differ slightly depending on the interpretation one gives to the cost side of (9). On the one hand, wedges to labor costs definitely reduce the benefit side of (9), as they depress equilibrium profits. On the other hand, they may also raise the cost of PEIs, if the latter depends, at least in part, on human labor. We summarize these considerations with the following statement.

Proposition 5. When firms can perform PEIs as in (9), adding a wedge $\tau > 0$ to firms' labor costs (but not to either entry cost f_n or f_b), has ambiguous effects on average productivity: the positive effect due to a higher threshold (per Proposition 4) is mitigated by a negative effect due to lower PEIs. This negative effect is larger if the wedge also leads to a multiplicative increase in the cost side of PEIs.

Proof. The proof can be found in Appendix C.5. ◻

Adding PEIs helps make sense of our empirical results at the distribution level. Under our specification of PEIs (9), firms that are *ex ante* highly productive (on the right tail of the distribution) benefit from labor market reforms that decrease effective labor costs. Conversely, on the left tail, the selection effect dominates, which contributes to depressing average productivity. Note that our analysis is silent on the overall welfare effects of the reform: even if the net impact on average productivity is lower, consumers may still benefit from lower product varieties. In future work, we plan to provide structural estimates of the model that would let us make preliminary conclusions about the overall welfare effects.

The analysis so far was confined to a closed economy and neglected considerations about trade, as this is not the key concern of this paper. In this regard, we plan to develop a suitable extension in future work, which is natural for an extension of the Melitz framework like ours. We expect to formalize the intuition according to which adding (removing) labor market distortions harms (helps) those firms in the right tail of the productivity distribution that are more likely to engage in foreign markets.

5 Conclusions

This paper presents novel evidence of the relationship between labor market flexibility and total factor productivity. Leveraging the staggered implementation of an Italian reform that lifted some constraints on using temporary contracts, we show that the reform had a negative and sizeable impact on the bottom of the ex-ante TFP distribution. Notably, the reform had no other effect than on the left tail of the distribution, showing that the increase in the use of temporary arrangements widened the productivity variance even when considering the direct effects on the post-intervention TFP distribution. We also show that the increased flexibility translated into a large general reduction in firm-level labor costs. Furthermore, it caused a significant reduction in the number of firms leaving the market within the bottom quartile of ex-ante TFP with, again, no effect on the rest. Finally, we provide evidence of an at-odds effect on labor productivity, which experienced a sharp increase only among the already-productive firms.

We rationalize our findings in steady-state through a general equilibrium model with monopolistic competition and heterogeneous firms. In the presence of financial frictions, firms need an upfront investment to enter the market, which financial intermediaries provide. Here an asymmetric information problem arises, as financial intermediaries only observe a noisy signal about firms' productivity. Our model shows that stronger EPLs lead to selection at the bottom of the productivity distribution, causing lower entries. This mechanism maps, in equilibrium, to our empirical evidence, as lower labor costs and lower exits among unproductive firms concur to explain the observed compositional heterogeneous effect on TFP. Our model also allows for productivity gains on the right tail, thanks to incentives to invest from the spares in labor costs.

Overall, this work shows that the grand effects of policy interventions aimed at improving labor market flexibility have ambiguous interpretations and that a multitude of labor market mechanisms intervenes in determining observable outcomes. Many of these are still left to be investigated deeper. We leave to future refinements a deeper analysis of the mechanisms linking LP and TFP, as well as a full-fledged welfare analysis of our model's implications.

References

ACABBI, EDOARDO MARIA AND ANDREA ALATI (2021) "Defusing Leverage: Liquidity Management and Labor Contracts," *Available at SSRN 3768825*. (Cited on pages 5, 10, and 20).

ACKERBERG, DANIEL A., KEVIN CAVES, AND GARTH FRAZER (2015) "Identification properties of recent production function estimators," *Econometrica*, 83 (6), 2411–2451. (Cited on pages 8, 14, and 16).

AUMOND, R., V. DI TOMMASO, AND G. RÜNSTLER (2022) *A narrative database of labour market reforms in euro area economies*: European Central Bank, doi/10.2866/430674. (Cited on page 2).

AUTOR, DAVID H, WILLIAM R KERR, AND ADRIANA D KUGLER (2007) "Does employment protection reduce productivity? Evidence from US states," *The Economic Journal*, 117 (521), F189–F217. (Cited on page 4).

BASSANINI, ANDREA, LUCA NUNZIATA, AND DANIELLE VENN (2009) "Job protection legislation and productivity growth in OECD countries," *Economic policy*, 24 (58), 349–402. (Cited on page 5).

BELOT, MICHÈLE, JAN BOONE, AND JAN VAN OURS (2007) "Welfare-improving employment protection," *Economica*, 74 (295), 381–396. (Cited on page 6).

BENTOLILA, SAMUEL AND GIUSEPPE BERTOLA (1990) "Firing costs and labour demand: how bad is eurosclerosis?" *The Review of Economic Studies*, 57 (3), 381–402. (Cited on page 6).

BERGEAUD, ANTONIN, GILBERT CETTE, AND REMY LECAT (2017) "Total factor productivity in advanced countries: A long-term perspective," *International Productivity Monitor* (32), 6. (Cited on page 2).

BERTOLA, GIUSEPPE (1990) "Job security, employment and wages," *European Economic Review*, 34 (4), 851–879. (Cited on page 5).

BERTOLA, GIUSEPPE AND RICHARD ROGERSON (1997) "Institutions and labor reallocation," *European Economic Review*, 41 (6), 1147–1171. (Cited on page 5).

BLANCHARD, OLIVIER AND AUGUSTIN LANDIER (2002) "The perverse effects of partial labour market reform: fixed-term contracts in France," *The Economic Journal*, 112 (480), F214–F244. (Cited on page 6).

BOERI, TITO, PIERRE CAHUC, AND ANDRÉ ZYLBERBERG (2015) "The Costs of Flexibility-Enhancing Structural Reforms," (1264), https://doi.org/https://doi.org/10.1787/5jrs558c5r5f-en. (Cited on page 5).

BORUSYAK, KIRILL, XAVIER JARAVEL, AND JANN SPIESS (2021) "Revisiting event study designs: Robust and efficient estimation," *arXiv preprint arXiv:2108.12419*. (Cited on page 12).

BUSTOS, PAULA (2011) "Trade liberalization, exports, and technology upgrading: Evidence on the impact of MERCOSUR on Argentinian firms," *American Economic Review*, 101 (1), 304–40. (Cited on page 4).

Cahuc, Pierre, Olivier Charlot, and Franck Malherbet (2016) "Explaining the spread of temporary jobs and its impact on labor turnover," *International Economic Review*, 57 (2), 533–572. (Cited on page 6).

Cahuc, Pierre and Fabien Postel-Vinay (2002) "Temporary jobs, employment protection and labor market performance," *Labour economics*, 9 (1), 63–91. (Cited on page 6).

Callaway, Brantly and Tong Li (2019) "Quantile treatment effects in difference in differences models with panel data," *Quantitative Economics*, 10 (4), 1579–1618. (Cited on pages 3, 11, 16, 17, and 18).

Canay, Ivan A. (2011) "A simple approach to quantile regression for panel data," *The econometrics journal*, 14 (3), 368–386. (Cited on page 18).

Cappellari, Lorenzo, Carlo Dell'Aringa, and Marco Leonardi (2012) "Temporary employment, job flows and productivity: A tale of two reforms," *The Economic Journal*, 122 (562), F188–F215. (Cited on pages 4, 5, and 13).

Card, David, Francesco Devicienti, and Agata Maida (2014) "Rent-sharing, holdup, and wages: Evidence from matched panel data," *Review of Economic Studies*, 81 (1), 84–111. (Cited on page 2).

Cengiz, Doruk, Arindrajit Dube, Attila Lindner, and Ben Zipperer (2019) "The effect of minimum wages on low-wage jobs," *The Quarterly Journal of Economics*, 134 (3), 1405–1454. (Cited on page 18).

Chaney, Thomas (2016) "Liquidity constrained exporters," *Journal of Economic Dynamics and Control*, 72, 141–154. (Cited on page 25).

Cingano, Federico, Marco Leonardi, Julian Messina, and Giovanni Pica (2010) "The effects of employment protection legislation and financial market imperfections on investment: evidence from a firm-level panel of EU countries," *Economic Policy*, 25 (61), 117–163. (Cited on page 5).

——— (2016) "Employment protection legislation, capital investment and access to credit: evidence from Italy," *The Economic Journal*, 126 (595), 1798–1822. (Cited on page 5).

Daruich, Diego, Sabrina Di Addario, Raffaele Saggio et al. (2020) "The effects of partial employment protection reforms: Evidence from Italy." (Cited on pages 2, 5, 8, 10, 20, 22, 23, and 24).

Deshpande, Manasi and Yue Li (2019) "Who is screened out? Application costs and the targeting of disability programs," *American Economic Journal: Economic Policy*, 11 (4), 213–48. (Cited on page 18).

Devicienti, Francesco, Bernardo Fanfani et al. (2021) "Firms' Margins of Adjustment to

Wage Growth: The Case of Italian Collective Bargaining,"Technical report, Institute of Labor Economics (IZA). (Cited on page 13).

Dew-Becker, Ian and Robert J Gordon (2012) "The role of labor-market changes in the slowdown of European productivity growth," *Review of Economics and institutions*, 3 (2), 45. (Cited on pages 2 and 5).

Dhingra, Swati and John Morrow (2019) "Monopolistic competition and optimum product diversity under firm heterogeneity," *Journal of Political Economy*, 127 (1), 196–232. (Cited on pages 28 and 46).

Dixit, Avinash K. and Joseph E. Stiglitz (1977) "Monopolistic competition and optimum product diversity," *American Economic Review*, 67 (3), 297–308. (Cited on pages 4 and 24).

Dolado, Juan J., Salvador Ortigueira, and Rodolfo Stucchi (2016) "Does dual employment protection affect TFP? Evidence from Spanish manufacturing firms," *SERIEs*, 7 (4), 421–459. (Cited on pages 5 and 6).

Engellandt, Axel and Regina T. Riphahn (2005) "Temporary contracts and employee effort," *Labour Economics*, 12 (3), 281–299. (Cited on page 6).

Faccini, Renato (2014) "Reassessing labour market reforms: Temporary contracts as a screening device," *The Economic Journal*, 124 (575), 167–200. (Cited on pages 6, 20, and 23).

Fan, Yanqin and Zhengfei Yu (2012) "Partial identification of distributional and quantile treatment effects in difference-in-differences models," *Economics Letters*, 115 (3), 511–515. (Cited on page 17).

Garibaldi, Pietro and Giovanni L. Violante (2005) "The employment effects of severance payments with wage rigidities," *The Economic Journal*, 115 (506), 799–832. (Cited on page 5).

Gnocato, Nicolò, Francesca Modena, and Chiara Tomasi (2020) "Labor market reforms and allocative efficiency in Italy," *Labour Economics*, 67, 101938. (Cited on page 5).

Grout, Paul A. (1984) "Investment and wages in the absence of binding contracts: A Nash bargaining approach," *Econometrica*, 449–460. (Cited on page 2).

Hopenhayn, Hugo and Richard Rogerson (1993) "Job turnover and policy evaluation: A general equilibrium analysis," *Journal of political Economy*, 101 (5), 915–938. (Cited on page 6).

Hsieh, Chang-Tai and Peter J Klenow (2009) "Misallocation and manufacturing TFP in China and India," *The Quarterly Journal of Economics*, 124 (4), 1403–1448. (Cited on page 5).

Ichino, Andrea, Fabrizia Mealli, and Tommaso Nannicini (2008) "From temporary help jobs to permanent employment: What can we learn from matching estimators and their sensitivity?" *Journal of applied econometrics*, 23 (3), 305–327. (Cited on page 6).

Ichino, Andrea and Regina T. Riphahn (2005) "The effect of employment protection on worker effort: Absenteeism during and after probation," *Journal of the European Economic Association*, 3 (1), 120–143. (Cited on page 6).

Jäger, Simon, Benjamin Schoefer, and Jörg Heining (2021) "Labor in the Boardroom," *The Quarterly Journal of Economics*, 136 (2), 669–725. (Cited on page 2).

Kugler, Adriana and Giovanni Pica (2008) "Effects of employment protection on worker and job flows: Evidence from the 1990 Italian reform," *Labour Economics*, 15 (1), 78–95. (Cited on page 5).

Lagos, Ricardo (2006) "A model of TFP," *The Review of Economic Studies*, 73 (4), 983–1007. (Cited on pages 2 and 6).

Lazear, Edward P. (1990) "Job security provisions and employment," *The Quarterly Journal of Economics*, 105 (3), 699–726. (Cited on page 5).

Levinsohn, James and Amil Petrin (2003) "Estimating production functions using inputs to control for unobservables," *The Review of Economic Studies*, 70 (2), 317–341. (Cited on pages 8, 14, 16, 18, 21, and 38).

Ljungqvist, Lars (2002) "How do lay-off costs affect employment?" *The Economic Journal*, 112 (482), 829–853. (Cited on page 6).

Manova, Kalina (2013) "Credit constraints, heterogeneous firms, and international trade," *Review of Economic Studies*, 80 (2), 711–744. (Cited on pages 4 and 25).

Melitz, Marc J. (2003) "The impact of trade on intra-industry reallocations and aggregate industry productivity," *Econometrica*, 71 (6), 1695–1725. (Cited on pages 4 and 24).

Mortensen, Dale T. and Christopher A. Pissarides (1994) "Job creation and job destruction in the theory of unemployment," *The review of economic studies*, 61 (3), 397–415. (Cited on page 23).

Olley, G. Steven and Ariel Pakes (1996) "The Dynamics of Productivity in the Telecommunications Equipment Industry," *Econometrica*, 64 (6), 1263–1297. (Cited on page 8).

Owen, Donald Bruce (1980) "A table of normal integrals: A table," *Communications in Statistics-Simulation and Computation*, 9 (4), 389–419. (Cited on page 44).

Poschke, Markus (2009) "Employment protection, firm selection, and growth," *Journal of Monetary Economics*, 56 (8), 1074–1085. (Cited on page 6).

Sargent, Timothy C. and Edgard R. Rodriguez (2001) "Labour or total factor productivity: Do we need to choose?"Technical report. (Cited on page 22).

Sklar, M (1959) "Fonctions de repartition an dimensions et leurs marges," *Publ. inst. statist. univ. Paris*, 8, 229–231. (Cited on page 18).

Tang, Meng-Chi (2017) "Total factor productivity or labor productivity? Firm heterogeneity and location choice of multinationals," *International Review of Economics & Finance*, 49, 499–514. (Cited on page 22).

Unger, Florian (2021) "Credit frictions, selection into external finance and gains from trade," *Canadian Journal of Economics/Revue canadienne d'économique*, 54 (3), 1206–1251. (Cited on page 25).

Wasmer, Etienne (2006) "General versus specific skills in labor markets with search frictions and firing costs," *American Economic Review*, 96 (3), 811–831. (Cited on page 6).

Zhelobodko, Evgeny, Sergey Kokovin, Mathieu Parenti, and Jacques-François Thisse (2012) "Monopolistic competition: Beyond the constant elasticity of substitution," *Econometrica*, 80 (6), 2765–2784. (Cited on page 4).

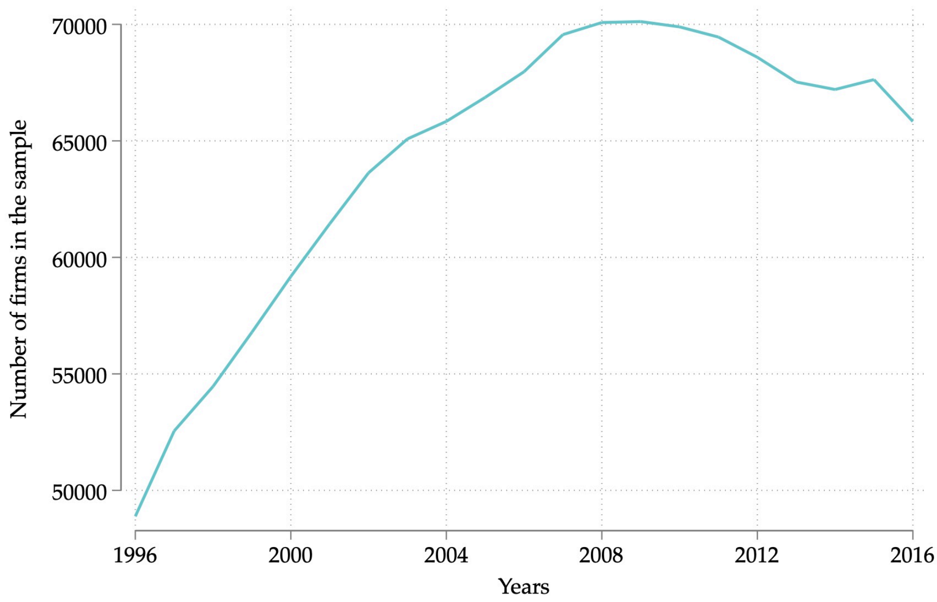
Appendix A Additional figures and tables

Table A.1: Summary statistics of the sample for selected years, by 2-digit industry

2-digit NACE industry	Year	Perm workers	Temp workers	Avg. TFP	TFP variance	Median TFP
	1998	172667	27652	.1284216	.2161534	.1329527
Manufacture of food products	2006	182135	42376	.0977126	.2039971	.0982962
	2014	193205	36770	-.1207272	.1610168	-.131798
	1998	203251	17358	-.0282327	.2293279	-.0408628
Manufacture of textiles	2006	145741	15966	.0318249	.2151859	.0274992
	2014	96058	11211	.0108087	.2154965	.0076907
	1998	95842	8067	-.0281261	.2237059	-.0059295
Manufacture of leather and related products	2006	85690	13919	.0371724	.2118929	.0297861
	2014	85843	16251	-.0029151	.2412991	-.0067139
	1998	60475	6169	.0575304	.2131559	.0448575
Manufacture of wood and of products of wood and cork	2006	69850	9809	-.018065	.1777057	-.0406096
	2014	51510	6780	-.0244748	.2048674	-.0346851
	1998	65799	7105	-.0345502	.1609966	-.0360556
Manufacture of paper and paper products	2006	73648	8038	.0514081	.1495572	.0409253
	2014	67121	6120	-.0359246	.1259497	-.0542607
	1998	58319	4477	-.1418616	.2411323	-.1501567
Printing and reproduction of recorded media	2006	64460	6889	-.022144	.2482745	-.0345538
	2014	48120	4285	.1439818	.2665306	.1373963
	1998	17316	1204	.0590195	.6170318	.0766721
Manufacture of coke and refined petroleum products	2006	17586	1618	-.0777462	.3545822	-.0558596
	2014	16079	856	-.1644919	.2347989	-.1666942
	1998	134756	10514	.1041821	.210599	.1119909
Manufacture of chemicals and chemical products	2006	115205	11419	-.0093579	.214623	-.0152116
	2014	100635	8074	-.1090271	.1780349	-.1454096
	1998	161121	21403	.0512851	.1377124	.051466
Manufacture of rubber and plastic products	2006	168897	23682	-.0016266	.1355466	-.0103579
	2014	147487	14470	-.0493059	.1369119	-.0582962
	1998	171197	14553	.0489248	.2333839	.0443273
Manufacture of other non-metallic mineral products	2006	178310	21533	-.026132	.1849009	-.0370715
	2014	123448	11219	.001261	.2020404	.0014682
	1998	126283	13791	.1089616	.1766382	.0841942
Manufacture of basic metals	2006	132343	15953	-.0974397	.1417552	-.1042986
	2014	92872	6348	-.050852	.1685186	-.0533013
	1998	96231	11678	.0591453	.1858619	.0610647
Manufacture of fabricated metal products	2006	122137	18650	-.0366059	.1703661	-.0349894
	2014	120580	17263	-.0069171	.1854015	-.0065837
	1998	65162	7030	-.1955914	.2487922	-.1863854
Manufacture of computer, electronic and optical products	2006	60192	5321	.0469366	.207455	.0288072
	2014	48757	3527	.1662638	.2088284	.170682
	1998	122394	15349	-.0511751	.15904	-.0507491
Manufacture of electrical equipment	2006	127603	16008	.0062777	.2016724	-.0109329
	2014	110928	11229	.0464987	.1781607	.0321269
	1998	276533	28455	.0411117	.1822258	.0456848
Manufacture of machinery and equipment n.e.c.	2006	290845	29306	.0110233	.1655855	.0047696
	2014	268757	24187	-.0295066	.1682832	-.0361953
	1998	51604	6894	.0110476	.1975234	.0091438
Manufacture of motor vehicles, trailers and semi-trailers	2006	71001	7820	-.0097178	.1674892	-.0267239
	2014	51548	3564	.0515928	.1800885	.0203576
	1998	35826	3971	-.0984418	.4623626	-.0849557
Manufacture of other transport equipment	2006	33995	4599	.0517861	.3086032	.0460749
	2014	27648	3800	.0455361	.441531	.0770729
	1998	87929	9192	.0764356	.1858246	.0792017
Manufacture of furniture	2006	104543	14089	.0057076	.1858041	-.0077348
	2014	75072	9557	-.0443373	.1874094	-.0605059
	1998	105636	10447	.0535403	.2309012	.0380507
Other manufacturing	2006	112926	15503	.0382077	.1816311	.0264225
	2014	105322	11072	-.04065	.168244	-.0568869
	1998	2761	223	.1045582	.682135	.1069984
Water collection, treatment and supply	2006	13993	1782	.0645613	.4773931	.0549474
	2014	19128	1072	-.0775508	.5319856	-.1021981

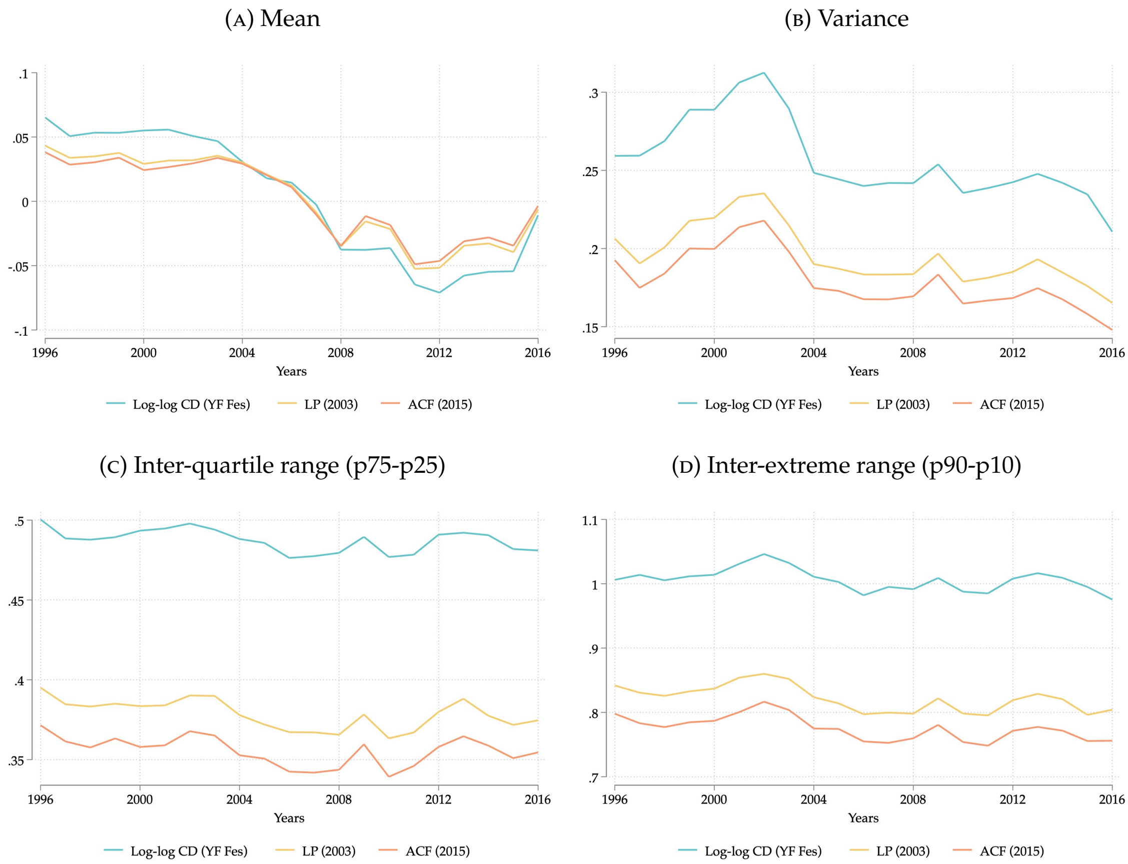
Note. This table reports descriptive statistics for three selected years in the sample, by 2-digit industry. The TFP measure is based on Levinsohn and Petrin (2003). Source: Istituto Nazionale della Previdenza Sociale (INPS) and Cerved.

FIGURE A.1: Sample size evolution



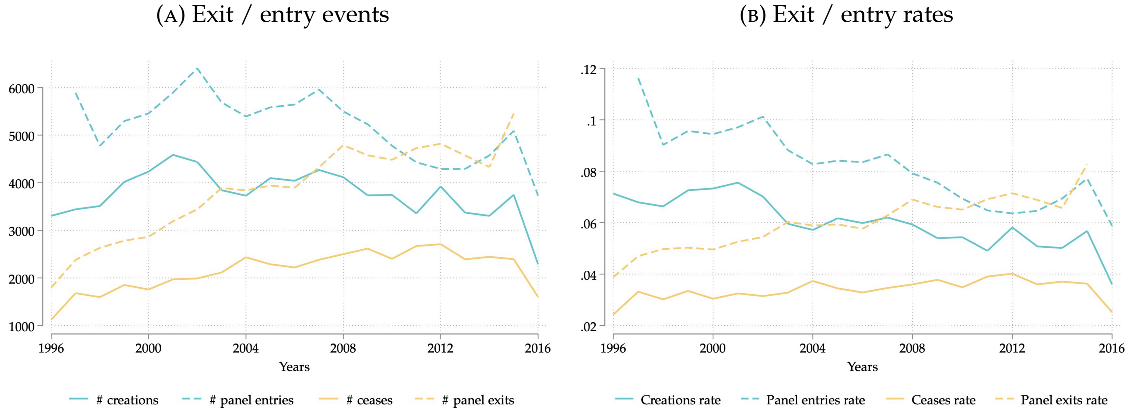
Note. This figure reports the sample size evolution over the sample period 2006-2016. The observed change is mainly due to the enlargement of firms' balance sheets recorded in the Cerved database. Source: Cerved.

FIGURE A.2: Descriptive statistics of the sample



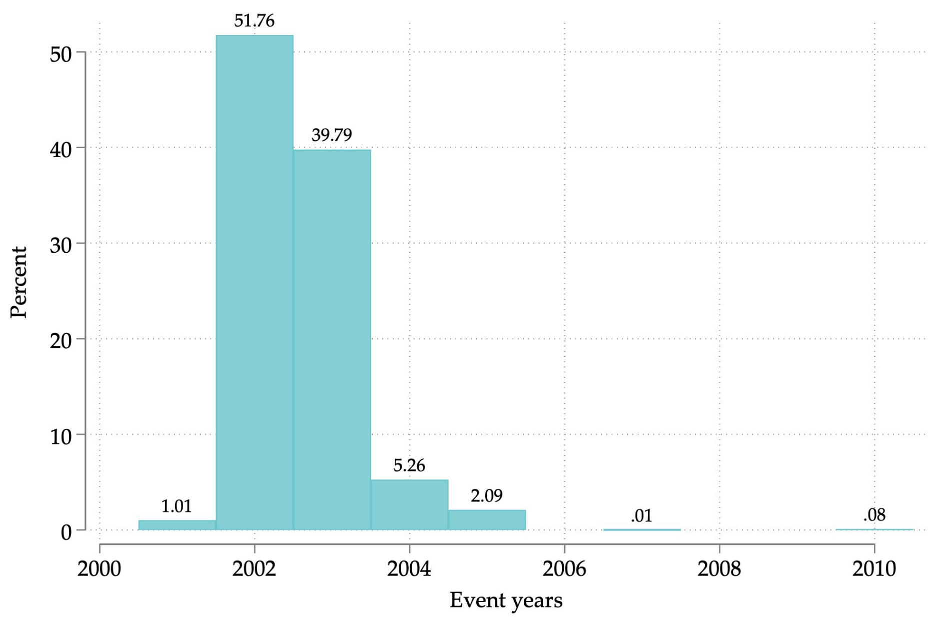
Note. This figure reports time series of different descriptive statistics of the sample—mean (A), variance (B), inter-quartile range (C), inter-extreme range (D)—for the three TFP measures employed in the paper. Source: Istituto Nazionale della Previdenza Sociale (INPS) and Cerved.

Figure A.3: Firm dynamics in the sample



Note. This figure reports the entry and exit events (Panel A) and the entry and exit rates (defined as the ratio between the events out of the firms population in each year; Panel B) for the sample. Source: Istituto Nazionale della Previdenza Sociale (INPS) and Cerved.

Figure A.4: Event years distribution



Note. This figure reports the relative percentages of the event years at the firm level. An event year is defined as the renewal year of the most used CCNL among a firm's workforce in 2001. Source: Istituto Nazionale della Previdenza Sociale (INPS) and Consiglio Nazionale dell'Economia del Lavoro (CNEL).

Appendix B Additional information on data cleaning and deflation

B.1 Cerved measures deflation

We deflate the firm-level balance sheet measures with three different indexes relying on ISTAT, the Italian National Institute of Statistics. First, we deflate capital measures—fixed assets and liquidity—with a purchasing power index. Second, we deflate revenues with an industry-specific price index that we match at the finest available digit each year. Third, we use an imputed cost index at the industry level to deflate production inputs—net purchases and labor costs. Again, we match these indexes at the best possible digit. More in detail, we build this latter measure through the following steps. First, we normalize the input-output table such that each matrix element represents the relative weight an input has in the output costs in a given year. Second, for each output sector-year pair, we build a cost index through a weighted sum of the cost indexes of the input sectors. Each cost index is assigned to the best available industry-specific price index in ISTAT. We set the base year of all three indexes in 2015.

Whenever the industry price indexes are unavailable from 1996 to 1999, we retropolate the available points of the series to predict these observations. More in detail, we use an ARIMA$(0,1,0)$ with a subset of external predictors, mainly the series of the lagged salary index.

B.2 Data cleaning details

We detail here all the data-cleaning operations undertaken on the different datasets.

We build a firm-year panel dataset starting from the Uniemens database. Here, we assign a singular province and industry for each observation—as the same firm might operate in more than one sector or geographical region with some branch—keeping the observation with the highest number of employees.

For the matched employer-employee dataset, we first drop the contracts that lasted less than nine weeks in a year. Then, we assign to each worker-year firm only one establishment. First, we solve multiple spells *within* the same employer in a year by keeping the one that pays more. Then, we solve multiple spells in different employers within the same year by adopting a double criterion: we keep the one that pays more and, subordinately, the one that involves more worked months. Finally, we drop contracts with no wage, and we winsorize the wage outliers on the right at the 99.7 percentile.

We clean the Cerved firm-level panel dataset by winsorizing all the relevant balance sheet variables at the 0.1 and 99.9 percentiles to remove outliers and by replacing negative values among variables that could not contain one (costs, revenues, purchases, and assets) as missing values: this way, we can still use the observation in its valid information. Besides the labor cost measure that this dataset contains, we also add the one obtained by the matched employer-employees, where we can collapse individual wages at the firm-year level. We then remove industries with less than forty firms in the entire period and industry-province-year cells that do not contain at least three establishments. Finally, after each TFP estimation, we drop the estimates that report at least one negative coefficient, and we further restrict the sample to industries for which we have non-missing estimates of all three productivity measures.

B.3 Exit and entry events details

We use the record of a registered ceasing of a firm in a specific year from the INPS dataset to identify permanent exit events, i.e., those cases in which a firm permanently exits the markets, communicating this fact to the Social Security Institute. Similarly, we use the registered firms' creations to identify entries of newborn employers.

Moreover, since firms show up in the INPS panel as long as they employ at least one worker (either full- or part-time; either with a temporary or permanent contract), we consider disappearances from the dataset in specific years as a signal of firms' inactivity in those periods. More in detail, we flag as a *disappearance* event a period of at least two consecutive years in which the firm does not occur on the panel. Again, we consider a reappearance in the panel as an indication of a firm's reactivation—with the exact same argument just exposed for the disappearances from the panel.

Of course, in both cases, the second definition of an exit or entry event includes the first one.

Appendix C Additional analysis of the model

C.1 Analysis of Proposition 1

It is useful to establish some auxiliary notation first. Let:

$$\begin{aligned}
t &= \log\theta \\
p &= \log\varphi \\
u &= -\log\theta \\
u' &= -\log\theta + \rho(\sigma-1) \\
z &= \frac{\log\varphi - \rho\log\theta}{\sqrt{1-\rho^2}}
\end{aligned}$$

and use asterisks to denote the values of these transformations evaluated at the corresponding threshold value of their argument(s): thus, $t^* = \log\theta^*$, $p = \log\varphi^*$, *et cetera* (but $z^* = (p^* - \rho t)/\sqrt{1-\rho^2}$ is a function of $t = \log\theta$). In addition, let $\phi(x)$ be the probability density function of the standard normal distribution and $\Phi(x)$ the corresponding cumulative distribution – both evaluated at a given point x—and $\Phi_\varrho(x, y)$ be the cumulative bivariate normal distribution with standard normal marginals and correlation parameter ϱ—evaluated at point (x, y).

We start by elaborating expression (8) under the model's assumptions:

$$
\begin{aligned}
\frac{\widetilde{\pi}\left(e^{t}\right)}{f} &= \int_{p^*}^{\infty} \frac{e^{(\sigma-1)(p-p^*)}}{\sqrt{1-\rho^2}}\phi\left(\frac{p-\rho t}{\sqrt{1-\rho^2}}\right)dp - \left[1-\Phi\left(\frac{p^*-\rho t}{\sqrt{1-\rho^2}}\right)\right]\\
&= \int_{z^*}^{\infty} e^{(\sigma-1)\left(\sqrt{1-\rho^2}z+\rho t-p^*\right)}\phi\left(z\right)dz - \left[1-\Phi\left(z^*\right)\right]\\
&= e^{(\sigma-1)(\rho t-p^*)+\frac{1}{2}(\sigma-1)^2\left(1-\rho^2\right)}\int_{z^*}^{\infty}\phi\left(z-\left(\sigma-1\right)\sqrt{1-\rho^2}\right)dz - \Phi\left(-z^*\right)\\
&= e^{(\sigma-1)(\rho t-p^*)+\frac{1}{2}(\sigma-1)^2\left(1-\rho^2\right)}\Phi\left(\left(\sigma-1\right)\sqrt{1-\rho^2}-z^*\right) - \Phi\left(-z^*\right)\\
&= e^{(\sigma-1)(\rho t-p^*)+\frac{1}{2}(\sigma-1)^2\left(1-\rho^2\right)}\Phi\left(\frac{\rho t-p^*+\left(\sigma-1\right)\left(1-\rho^2\right)}{\sqrt{1-\rho^2}}\right) - \Phi\left(\frac{\rho t-p^*}{\sqrt{1-\rho^2}}\right).
\end{aligned}
$$

Therefore, the Arbitrage Condition (6) reads:

$$
e^{(\sigma-1)(\rho t^*-p^*)+\frac{1}{2}(\sigma-1)^2\left(1-\rho^2\right)}\Phi\left(\frac{\rho t^*-p^*+\left(\sigma-1\right)\left(1-\rho^2\right)}{\sqrt{1-\rho^2}}\right) - \Phi\left(\frac{\rho t^*-p^*}{\sqrt{1-\rho^2}}\right) - \frac{\delta f_b}{f} = 0,
$$

with an associated implicit function $p^* = \rho t^* + a$ where $a = \log A$—as one can verify by setting the total differential at zero. It is also possible to verify that plugging this implicit function back into the right-hand side of the above AC delivers a decreasing monotone function of a which cuts the x-axis so long as $\delta f_b / f > 0$. Therefore, a (and hence A) is unique, and it is both decreasing in f_b and increasing in f.

To analyze the Free Entry Condition, it is helpful to define $\widetilde{v} \equiv \int_{\theta^*}^{\infty} \widetilde{\pi}\left(\theta\right) dC\left(\theta\right)$ as the expected joint profits that accrue to both the entrepreneur and the bank following the experimentation stage. This quantity can be expressed as the following function of the two threshold values (t^*, p^*):

$$
\begin{aligned}
\widetilde{v}\left(t^*, p^*\right) &= f\int_{t^*}^{\infty}\pi\left(e^{t}\right)\phi\left(t\right)dt\\
&= f\int_{t^*}^{\infty} e^{(\sigma-1)(\rho t-p^*)+\frac{1}{2}(\sigma-1)^2\left(1-\rho^2\right)}\Phi\left(\frac{\rho t-p^*+\left(\sigma-1\right)\left(1-\rho^2\right)}{\sqrt{1-\rho^2}}\right)\phi\left(t\right)dt\\
&\qquad - f\int_{t^*}^{\infty}\Phi\left(\frac{\rho t-p^*}{\sqrt{1-\rho^2}}\right)\phi\left(t\right)dt\\
&= f e^{\frac{1}{2}(\sigma-1)^2-(\sigma-1)p^*}\int_{-\infty}^{-t^*+\rho(\sigma-1)}\Phi\left(\frac{-\rho u'-p^*+\left(\sigma-1\right)}{\sqrt{1-\rho^2}}\right)\phi\left(u'\right)du'\\
&\qquad - f\int_{-\infty}^{-t^*}\Phi\left(\frac{-\rho u-p^*}{\sqrt{1-\rho^2}}\right)\phi\left(u\right)du\\
&= f\left[e^{\frac{1}{2}(\sigma-1)^2-(\sigma-1)p^*}\Phi_{\rho}\left(-p^*+\sigma-1, -t^*+\rho\left(\sigma-1\right)\right) - \Phi_{\rho}\left(-p^*, -t^*\right)\right],
\end{aligned}
$$

where the last line follows from the analysis of the moments of the standard normal cumulative distribution as in Owen (1980). Write the Free Entry condition as follows:

$$\mathcal{H}(p^*,t^*) = e^{\frac{1}{2}(\sigma-1)^2-(\sigma-1)p^*}\Phi_\rho(-p^*+\sigma-1,-t^*+\rho(\sigma-1)) - \\ -\Phi_\rho(-p^*,-t^*) - \frac{\delta f_b}{f}\Phi(-t^*) - \frac{\delta f_n}{f} = 0.$$

The derivative of the above with the respect to the log-productivity threshold p^* is, following some manipulation, shown to be always negative:

$$\frac{\partial\mathcal{H}(p^*,t^*)}{\partial p^*} = -(\sigma-1)e^{\frac{1}{2}(\sigma-1)^2-(\sigma-1)p^*}\Phi_\rho(-p^*+\sigma-1,-t^*+\rho(\sigma-1)) < 0.$$

Instead, the derivative with respect to the log-signal threshold t^* is shown to be:

$$\frac{\partial\mathcal{H}(p^*,t^*)}{\partial t^*} = -\left[e^{(\sigma-1)(\rho t^*-p^*)+\frac{1}{2}(\sigma-1)^2(1-\rho^2)}\Phi\left(\frac{\rho t^*-p^*+(\sigma-1)(1-\rho^2)}{\sqrt{1-\rho^2}}\right) - \\ -\Phi\left(\frac{\rho t^*-p^*}{\sqrt{1-\rho^2}}\right) - \frac{\delta f_b}{f}\right]\phi(t^*)$$

which is not a monotone function of t^*. However, an analysis of this derivative shows that, for a fixed p^*, it is $\lim_{t^*\to-\infty}\partial\mathcal{H}(p^*,t^*)/\partial t^* = \lim_{t^*\to\infty}\partial\mathcal{H}(p^*,t^*)/\partial t^* = 0$; that the derivative equals exactly 0 whenever $t^* = (p^*-a)/\rho$ (observe that the expression in brackets matches the Arbitrage Condition); and that to the left of this value, the derivative is positive, while on the right, it is negative. These results give rise to the pattern depicted in Figure 8, with the interpretation given in the text. Also, observe that the line $p^* = \rho t^* + a$ can only intersect the implicit function of p^* with respect to t^* based on the Free Entry condition at a stationary point of the implicit function because a is unique. Since there is only one such stationary point, there is only one intersection point and, therefore, only one equilibrium of the model.

C.2 Analysis of Proposition 2

This is straightforward: as already mentioned a (and thus A) is increasing in f, while $\partial\mathcal{H}(p^*,t^*;f)/\partial f = \delta[f_b\Phi(-t^*)+f_n]f^{-2} > 0$. Hence, the AC and FE curves shift, following an increase of the fixed cost of production from f to $f(1+\tau)$—with f_n and f_b staying unchanged—according to the pattern depicted in Figure 8. Since the two curves must always meet at the maximum of the implicit function of p^* over t^*, both threshold values are higher in the new equilibrium.

C.3 Analysis of Proposition 3

This is a particular instance where informational frictions lead to a deadweight welfare loss, which is larger the more marked frictions are. As in the original Melitz model, we analyze the welfare implications of the model's steady state. First, define:

$$\mathcal{P}_\theta^* \equiv \mathbb{P}\text{r}\left(\theta \geq \theta^*\right)$$
$$\mathcal{P}_\varphi^* \equiv \mathbb{P}\text{r}\left(\varphi \geq \varphi^*\right)$$

as the two unconditional probabilities that in equilibrium, before the draw of (θ, φ) pair, a firm-entrepreneur passes either threshold. Also define:

$$\widetilde{\pi} \equiv \mathbb{E}\left[\pi | \theta \geq \theta^*\right]$$

that is the expected market profits (including the share to be paid out to banks) that firms expect in equilibrium conditional upon passing the signal threshold. Thus, in steady state the mass of entering firms M_e and that of active firms M must comply to $\delta M = \mathcal{P}_\varphi^* M_e$; the total remuneration of entrepreneurial labor is $L_n = M_e f_n$; and bank labor amounts in equilibrium to $L_b = M_e \mathcal{P}_\theta^* f_b$. Moreover, free entry implies the following relationship in steady state:

$$\mathcal{P}_\theta^* \left(\widetilde{\pi} - \delta f_b\right) - \delta f_n = 0.$$

Lastly, recall that $\bar{r}$ and $\bar{\pi}$, in the original Melitz model, indicate average equilibrium revenues and profits conditional on successful entry, respectively.

Combining everything, it is:

$$\begin{aligned}
L = L_p + L_b + L_n &= M\left(\bar{r} - \bar{\pi}\right) + M_e\left(\mathcal{P}_\theta^* f_b + f_n\right) \\
&= M\left[\bar{r} - \bar{\pi} + \frac{\delta}{\mathcal{P}_\varphi^*}\left(\mathcal{P}_\theta^* f_b + f_n\right)\right] \\
&= M\left(\bar{r} - \bar{\pi} + \widetilde{\pi}\frac{\mathcal{P}_\theta^*}{\mathcal{P}_\varphi^*}\right)
\end{aligned}$$

where the first line exploits $L_p = M\left(\bar{r} - \bar{\pi}\right)$ as in Melitz; the second line leverages stationarity, and the third lines makes use of the Free Entry condition in steady state. Therefore, welfare per worker $\mathcal{W}$ equals the inverse of the price level, that is:

$$\mathcal{W} = \frac{\sigma - 1}{\sigma} L^{\frac{1}{\sigma-1}} \left(\bar{r} - \bar{\pi} + \widetilde{\pi}\frac{\mathcal{P}_\theta^*}{\mathcal{P}_\varphi^*}\right)^{-\frac{1}{\sigma-1}} \widetilde{\varphi}$$

where $\widetilde{\varphi}$, using the same notation as in Melitz, is the productivity of the representative firm. Note that $\widetilde{\varphi}$ is increasing in ρ: an argument akin to that of Proposition 2 would show that a higher ρ leads to higher equilibrium thresholds (θ^*, φ^*), and hence to a higher average productivity (thanks to a sharper selection by banks).

Further observe (although this is tedious to show), that for $\rho \geq 0$ it is:

$$\frac{\mathcal{P}_\theta^*}{\mathcal{P}_\varphi^*} \geq \frac{\bar{\pi}}{\widetilde{\pi}} \geq 1,$$

with both relationships becoming equalities if and only if $\rho = 1$. In addition, the two inequalities widen the closer ρ gets to zero. To conclude, social welfare is maximized under the perfect information case $\rho = 1$ when the economy reduces to Melitz's, and hence the optimality result by Dhingra and Morrow (2019) is restored. A deviation of ρ from the optimal benchmark leads to two sources of inefficiency: first, representative productivity $\widetilde{\varphi}$ falls due to a selection effect; second, the number of available varieties decreases by a factor $\bar{r}/\left(\bar{r} - \bar{\pi} + \widetilde{\pi}\mathcal{P}^*_\theta/\mathcal{P}^*_\varphi\right) < 1$, as some resources in the economy are wasted to finance entrepreneurs-firms that pass the signal threshold θ^* but fail to meet the productivity threshold φ^*.

C.4 Analysis of Proposition 4

Adding a wedge τ to firms labor costs, holding everything else equal, raises the two equilibrium threshold are raised (per Proposition 2), hence $\widetilde{\varphi}$ increases while the gap between $\bar{\pi}$ and $\widetilde{\pi}\mathcal{P}^*_\theta/\mathcal{P}^*_\varphi$ also narrows. At the same time, fewer firms can repay production costs and survive in the economy, leading to higher average revenues $\bar{r}$ and fewer product varieties. This makes the overall welfare effects of the wedge ambiguous and dependent on the specific parametrization of the model.

C.5 Analysis of Proposition 5

Use the (τ) subscript to denote the values of the constants featured in (9) following the addition of a wedge τ to labor costs. From the Dixit-Stiglitz analysis of monopolistic competition one has:

$$B_{(\tau)} = \frac{B}{(1+\tau)^{\sigma-1}},$$

as both revenues and profits decrease because of higher labor costs. Let the wedge τ also cause the cost side of (9) to increase, say because part of the cost of enhancing productivity involves human resources, as follows:

$$\kappa_{(\tau)} = \kappa\,(1+\tau)^{\zeta},$$

for some $\zeta \geq 0$. Therefore, by (10) the wedge leads to a multiplicative transformation of the equilibrium productivity distribution, which is expressed as follows:

$$\breve{\varphi}_{(\tau)} = (1+\tau)^{\frac{\sigma-1+\zeta}{\sigma-1-\alpha}}\,\breve{\varphi} < \breve{\varphi},$$

where $\breve{\varphi}$ is as in (10), while $\breve{\varphi}_{(\tau)}$ is the updated value of post-investment productivity following the addition of the wedge.

C.6 Analysis of the two extreme cases

The critical properties of the model are perhaps best appreciated by looking at two "extreme" cases about the statistical relationship between the signal θ and productivity φ. In one case, signals are not informative at all, and the two random variables are fully independent. In the other case, the signal is fully informative, and the two random variables are perfectly correlated. The analysis of these two cases can be conducted without maintaining either Assumptions 1 or 2. Under these assumptions, however, the cases in question correspond to those where $\rho = 0$ and $\rho = 1$, respectively.

Signals are not informative. If signals deliver no information about productivity, the two random variables are independent: $G(\varphi|\,\theta) = G_0(\varphi)$ for all pairs (φ, θ), and $\widetilde{\pi}(\theta) = (1 - G_0(\varphi^*))\,\bar{\pi}$, where $\bar{\pi}$ are the expected profits conditional upon successful entry as in Melitz, for all values of θ. At stage 2. of the model, banks set their share uniformly for all firms: hence $\theta^* = 0$ and $b(\theta) = \delta f_b/(1 - G_0(\varphi^*))\,\bar{\pi}$. Back in the firm entry stage (stage 1.) it is $C(\theta^*) = 0$ and free entry reduces to:

$$\frac{(1 - G_0(\varphi^*))}{\delta}\bar{\pi} - f_b - f_n = \frac{f}{\delta}(1 - G_0(\varphi^*))\,k(\widetilde{\varphi}(\varphi^*)) - f_e = 0$$

for $f_e = f_b + f_n$ and where $\bar{\pi} = f\,k(\widetilde{\varphi}(\varphi^*))$ is the Zero Profit Condition (ZPC) as in Melitz. This is precisely the equilibrium condition of the original Melitz model.

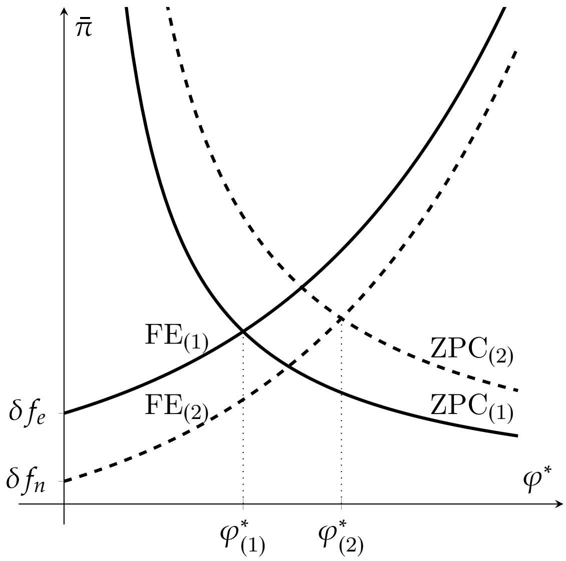
Signals are fully informative. If signals predict productivity with probability one, θ^* and φ^* are jointly determined, hence one can safely focus on productivity φ only while disregarding the signal θ. To solve the model, observe that at stage 2. banks will finance only firms that are able to repay f_b in present value. This translates, relative to the Melitz benchmark, into an actual *per-period* post-entry fixed cost of $f + \delta f_b$: therefore the Zero Profit Condition becomes as $\bar{\pi} = (f + \delta f_b)\,k(\widetilde{\varphi}(\varphi^*))$. In the firm entry stage (stage 1.) entrepreneurs only need to bear their own entry cost f_n, and Free Entry implies $\bar{\pi} = \delta f_n/(1 - G_0(\varphi^*))$. Combining everything, the equilibrium solution is given as follows, and it shown to be unique by Appendix B in Melitz.

$$\frac{(1 - G_0(\varphi^*))}{\delta}\bar{\pi} - f_n = \frac{f + \delta f_b}{\delta}(1 - G_0(\varphi^*))\,k(\widetilde{\varphi}(\varphi^*)) - f_n = 0$$

One can show analytically that the second scenario leads to a higher threshold productivity value φ^*. An easier way to appreciate this is by comparing Melitz's ZCP and FE curves between the two cases: when moving from the first scenario (no information) to the second (full information), both curves shift outward in such a way that leads to a higher value of φ^*, as it is shown in Figure C.5. As in Proposition 4, the second scenario is more efficient for two reasons: entering firms are, on average, more productive, and no intermediary-specific fixed entry cost f_b is wasted on firms that eventually fail to pass the final threshold and produce.

It is interesting to analyze the effect of adding a wedge τ to labor costs in the first of the two extreme scenarios, where signals provide no information. Conditional on expected post-entry profits $\bar{\pi}$ staying constant, the wedge does not affect the cost side of firm entry decisions. However, it obviously affects the benefits side, in a way that is summarized by the ZPC, which becomes $\bar{\pi} = (1 + \tau)\,f\,k(\widetilde{\varphi}(\varphi^*))$. Hence, graphically the ZPC curve shifts

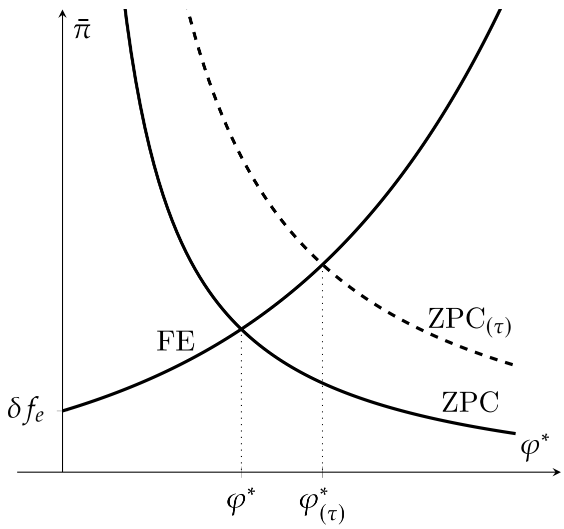
FIGURE C.5: Analysis of the two extreme cases



Note. Numbers apposed to curves or variables denote one of the two "extreme" scenarios, as described above.

outward thus leading to a higher productivity threshold (from φ^* to $\varphi^*_{(\tau)}$ in the representation given by Figure C.6). Thus, the wedge τ can in principle be tailored to make the resulting productivity threshold equal to that of the "full information," efficient outcome shown in Figure C.5. Observe that this would not, however, restore the full efficiency properties of the model! The intuition is that by introducing the wedge, only the ZCP curve shifts, but the FE curve does not. In equilibrium, the lower expected profits dissuade some firms from entering, thus decreasing the number of varieties and increasing average profits. Therefore, the overall welfare effect is ambiguous, as expressed by Proposition 4 for the general case.

FIGURE C.6: Introduction of labor frictions in the Melitz model



Note. τ included in a curve's or variable's subscript represents the implications of introducing labor price wedges equal to τ on it.