Essays on macroeconomic policies after the crisis

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Chapter 4

Employment Protection Deregulation and Labor Shares in Advanced Economies

4.1 Introduction

Since the 1980s labor shares have trended downwards in many countries around the world (Karabarbounis and Neiman, 2013). This trend accelerated in the 1990s, and it has been particularly pronounced in advanced economies (IMF, 2017; OECD, 2012). Such a decline flies in the face of the predominant view in macroeconomics, since Kaldor (1957, 1961), that the labor share tends to be stable over the long run. This has triggered renewed interest in the drivers of labor shares, with particular focus on the roles of technological progress in equipment goods and implied substitution of capital for routine labor tasks (Acemoglu and Restrepo, 2016; Alvarez-Cuadrado, Van Long, and Poschke, 2018; Dao et al., 2017; Eden and Gaggl, 2015; Karabarbounis and Neiman, 2013), rising concentration and pricing power across markets (Autor et al., 2017; Barkai, 2017), globalization of trade, finance and production (Boehm, Flaaen, and Pandalai-Nayar, 2017; Dao et al., 2017; Elsby, Hobijn, and Şahin, 2013; Furceri, Loungani, and Ostry, 2018), and measurement issues (Bridgman, 2017; Koh, Santaeulalia-Llopis, and Zheng, 2015; Rognlie, 2016). This paper contends that, alongside these (non-mutually-exclusive) drivers, changes in institutions that weakened worker bargaining power have also played a role. While the point is general, for identification purposes, we focus narrowly on job protection deregulation aimed at enhancing the functioning of labor markets. We empirically show that such deregulation contributed to part of the observed decline in labor shares in many advanced economies — possibly a little over a tenth overall, keeping in mind that this estimate abstracts from other policy and non-policy changes that may also have weakened worker bargaining power in recent decades.

The analysis covers 26 advanced economies over the period 1970-2015. To capture labor market deregulation, we make use of a unique 'narrative' cross-country dataset of major reforms of employment protection legislation (EPL) for regular workers, compiled in Duval et al., 2018. Strikingly, in the five years after major reforms, the

aggregate labor share declined by more than seven-tenths of a percentage point in reforming countries, on average, compared to status quo countries.

We test empirically for this stylized fact applying the local projection method (Jordà, 2005) to trace out the response of the labor share to our reform events. To gauge the macroeconomic effects of EPL reforms on the labor share we first carry out the analysis at the country-time level. Then, to understand the underlying channels, we focus on the country-industry-time level. For the latter, we apply a difference-in-differences identification strategy ala Rajan and Zingales, 1996, using two alternative identifying assumptions that are grounded in theory. First, following Micco and Pages, 2006 and Bassanini, Nunziata, and Venn, 2009, stringent dismissal regulations are more binding, and therefore should have a more substantial impact, in industries where firms have a higher 'natural' propensity to regularly adjust their workforce — that is, a higher 'natural' layoff rate. Second, following Blanchard and Giavazzi, 2003 and Bentolila and Saint-Paul, 2003, insofar as EPL affects workers' bargaining power and wage bargaining conforms at least in part to a Right-to-Manage model, deregulation lowers wage rents and triggers substitution of labor for capital, with an impact on the labor share that depends on the elasticity of substitution between these factors. The upshot is that deregulation is more likely to reduce the labor share in industries characterized by a lower degree of substitutability between capital and labor.

There are two further advantages of having a three-dimensional (i industries, j countries and t time periods) dataset:

- First, it allows us to control for country- and industry-specific time-varying unobserved factors, such as macroeconomic shocks, as well as country-industry time-invariant characteristics by including country-time (j, t), industry-time (i, t) and country-industry (j, i) fixed effects. The inclusion of the country-time fixed effects is particularly important as it absorbs any unobserved cross-country heterogeneity in macroeconomic conditions and policies that affect labor shares in a similar way across industries. In a pure cross-country time-series analysis, this would not be possible, leaving open the possibility that the impact attributed to EPL reforms could be due to other unobserved factors. Similarly, the inclusion of industry-time (i, t) fixed effects absorbs any unobserved industry-specific developments that may affect industry labor shares in a similar way across countries, such as the adoption of new technologies.

- Second, it mitigates concerns about reverse causality. While it is typically difficult to identify causal effects using cross-country time-series data, it is much more likely that EPL reforms affect cross-industry differences in labor shares than the other way around. Since we control for country-time fixed effects — and therefore for aggregate labor shares — reverse causality in our set-up would imply that differences in labor shares across industries influence the probability of reforms at the aggregate level. Moreover, our primary independent variable is the interaction between job protection reforms and industry-specific factors (natural layoff rates

and/or elasticities of substitution); this makes it even less plausible that causality runs from the industry-level labor share to these composite variables.

To further strengthen the causal interpretation of our results, we verify their robustness to the inclusion of several additional controls whose omission could bias our estimates, including past and expected values of GDP growth and proxies for the other labor share drivers identified in the literature, such as technological progress and international trade.

Our key finding is that job protection deregulation reduces labor shares. In the country-level analysis, we find a major reform that liberalizes EPL to reduce the aggregate labor share by 0.6 to 0.8 percentage point, on average, over the medium term. In the country-industry-level analysis, the effect of that same reform is about 0.9 percentage point higher in high layoff-rate industries (defined as those in the 75th percentile of the cross-industry distribution of layoff rates in the United States) compared with their low layoff-rate counterparts (those in the 25th percentile).[1] The medium-term differential effect between industries with a low and high elasticity of substitution between capital and labor (defined as those in the 25th and 75th percentiles of the cross-industry distribution of elasticities) is more substantial, at 1.5 percentage point. We also find these effects to be mainly driven by a decline in the real wage; this further supports our interpretation that weaker bargaining power has been the principal channel through which EPL deregulation has lowered labor shares in reforming advanced economies.

Using both our country-level and industry-level estimates, we perform an illustrative back-of-the-envelope calculation of the impact of all past EPL reforms, both liberalizing and tightening ones, on the labor share. This exercise suggests a non-trivial impact; job protection deregulation may have contributed about 15 percent to the overall labor share decline. That reflects primarily the deregulation wave of the 1990s and 2000s, which is also the period over which labor shares declined the most in advanced economies.

Our paper relates to the extensive empirical literature on the drivers of labor shares which, somewhat surprisingly, has touched very little on the role of labor market regulation. Some papers study the impact of other drivers of labor shares, notably international trade and offshoring, via their effect on workers' bargaining power (see e.g. Kramarz, 2008, and the recent review by Hummels, Munch, and Xiang, 2016). Instead, our focus is on the direct role of labor market institutions. Blanchard, Nordhaus, and Phelps, 1997 and Blanchard and Giavazzi, 2003 provide theoretical support for a link between labor market deregulation, weaker bargaining power, and lower labor shares, and argue that such link is consistent with the decline observed across European countries during the 1990s. They also make a distinction

[1] Following Bassanini, Nunziata, and Venn, 2009, we use industry layoff rates computed from U.S. data to proxy for 'natural' layoff rates as in the U.S. contracts can typically be terminated at will. Hence, this country is the closest to a frictionless economy. For more details, see Section 4.3.2.

between the short- and long-term effects. They do not provide any formal evidence, however.

The few empirical studies that attempt to quantify the impact of labor market institutions on the labor share have typically failed to find any significant effect. Using cross-country industry-level data, Bentolila and Saint-Paul, 2003 explore a range of labor share drivers, including the frequency of labor conflicts, which they take as a proxy for workers' bargaining power. They find this variable to be insignificant, in a simple OLS regression without fixed effects. Elsby, Hobijn, and Şahin, 2013 exploit variation in the rate of unionization across US industries but do not find a significant association with the labor share. Checchi and García-Peñalosa, 2008 explore the impact on labor shares of several labor market institutions in a cross-country time-series set-up covering 16 OECD countries over the 1960-2000 period, but they do not consider EPL. (Deakin, Malmberg, and Sarkar, 2014) analyze the impact of EPL in an error correction framework for six OECD countries over 1970-2010 and do not find any statistically significant effect. Our sharper identification strategy — using a three-dimensional set-up with a rich set of fixed effects and two identification assumptions ala Rajan and Zingales, 1996 drawn from theory — and reliance on a new dataset of major job protection reforms is what radically distinguishes our analysis from these earlier contributions.

Our paper also relates to the extensive empirical literature on the macroeconomic effects of job protection legislation on economic outcomes, which has primarily focused on productivity and employment. While not entirely settled, the bulk of the evidence suggests that stringent regulation lowers productivity by distorting job turnover, and may also lower employment (for a comprehensive review, see e.g. OECD, 2013). However, except for the few studies mentioned earlier, this literature has not explored the impact of job protection on labor shares. Our paper fills this gap, thereby complementing recent research that has documented the macroeconomic effects of these and other labor market reforms.

The remainder of this paper is organized as follows. Section 4.2 discusses two stylized wage bargaining models to help guide our empirical strategy. Section 4.3 presents our new dataset of major employment legislation reforms, it illustrates the derivation of the layoff rates and elasticities of substitution that are used for the identification, and it provides some stylized facts concerning the decline of labor shares around EPL reform episodes. Section 4.4 sets up the econometric framework. In Section 4.5, we present the main regression results and perform several sensitivity analyses. Section 4.6 contains some extensions to decompose the channels driving our results. Section 4.7 concludes.

4.2 Theoretical Framework

To illustrate the theoretical impact of employment protection deregulation on the labor share and motivate our empirical approach, we use two stylized wage bargaining models, the Right-to-Manage and the Efficient Bargaining models (see e.g. Blanchard

and Fischer, 1989). For ease of exposure, and following others, such as Blanchard, Nordhaus, and Phelps, 1997, we assume that EPL deregulation directly weakens workers' bargaining power. For the rest, our theoretical analysis mostly follows Bentolila and Saint-Paul, 2003.

4.2.1 Competitive Labor Market

As a start, let's consider the case of a fully competitive labor market where labor is paid its marginal product. We assume that real output Y is produced using a constant elasticity of substitution (CES) production function with constant returns to scale:

$$Y = F(K, AL) = (\alpha(K)^{\varepsilon} + (1-\alpha)(AL)^{\varepsilon})^{(1/\varepsilon)} \tag{4.1}$$

where K, L and A denote capital, labor and labor-augmenting technical change, respectively, while the parameter ε relates to the elasticity of substitution, σ, according to: $\sigma = 1/(1-\varepsilon)$. Defining the labor-to-capital ratio in effective units as $l \equiv \frac{AL}{K}$, rewriting $F(K, AL) = Kf(\frac{AL}{K})$, and using that in competitive markets labor is paid its marginal product — such that $\frac{w}{p} = Af'(l)$, where w is the nominal wage and p the price level — we can write the labor share as:

$$LS = \frac{wL}{pY} = l\frac{f'(l)}{f(l)} = \frac{(1-\alpha)(AL)^{\varepsilon}}{\alpha(K)^{\varepsilon} + (1-\alpha)(AL)^{\varepsilon}} \tag{4.2}$$

For reasons that will become clear below, we want to express the labor share in terms of the capital-to-output ratio k which is $k = \frac{K}{\alpha(K)^{\varepsilon}+(1-\alpha)(AL)^{\varepsilon})^{1/\varepsilon}}$. After simple manipulations, we can rewrite Equation 4.2 as:

$$LS = 1 - \alpha k^{\varepsilon} \tag{4.3}$$

The key insight from Equation 4.3 is that when labor is paid its marginal product, any change in factor prices and/or quantities affects the labor share only through its effects on the capital-to-output ratio k.

4.2.2 Bargaining Under the Right-to-Manage model

To study the effects of EPL reforms on the labor share, we now introduce labor market frictions in the form of bargaining between employers and workers. We start by assuming that employers and workers first bargain over the wage, with employers then setting employment taking the wage as given. In this case, it remains optimal for employers to set employment such that labor is paid its marginal product (that is, $\frac{w}{p} = Af'(l)$). Equation 4.3 still holds.

What happens when easing EPL? Lower protection reduces workers' bargaining power, which in turn results in a lower wage. Employers respond by substituting

labor for capital, and therefore the capital-to-output ratio decreases. This drives a change in the labor share, whose sign depends on whether capital and labor are complements ($\varepsilon < 0$) or substitutes ($\varepsilon > 0$). To see this formally, take the derivative of the labor share expression in Equation 4.3 with respect to workers' bargaining power θ:

$$\frac{\partial LS}{\partial \theta} = -\alpha\varepsilon k^{\varepsilon-1}\frac{\partial k}{\partial \theta} \Rightarrow \begin{cases} > 0 \text{ if } \varepsilon < 0 \\ < 0 \text{ if } \varepsilon > 0 \end{cases} \tag{4.4}$$

where the inequalities follow from the fact that $\frac{\partial k}{\partial \theta} > 0$. Equation 4.4 shows that under the Right-to-Manage model, EPL deregulation that reduces workers' bargaining power ($\theta \downarrow$) will lower the labor share if capital and labor are relative complements ($\varepsilon < 0$) but increase it if they are substitutes ($\varepsilon > 0$).

4.2.3 Efficient Bargaining

Under efficient bargaining, firms and workers instead bargain over both employment and wages. They set employment efficiently by equalizing the marginal product of labor to its opportunity cost, which is the workers' reservation wage. The wage itself is a weighted average of the average and marginal products of labor, with the weight on the former reflecting the bargaining power of workers. Formally, under Nash bargaining the real wage follows:

$$\frac{w}{p} = \theta A\frac{f(l)}{l} + (1-\theta)Af'(l) \tag{4.5}$$

In this setting labor is paid more than its marginal product and Equation 4.3 does not longer hold. Recalling the definitions of l and k, it can be easily shown that:

$$LS = 1 - \alpha(1-\theta)k^{\varepsilon} \tag{4.6}$$

EPL deregulation then reduces workers' bargaining power and the real wage, but employment does not change since it is pinned down by the efficient bargaining condition that links the marginal product of labor to the reservation wage. Therefore, the labor share unambiguously declines, regardless of the elasticity of substitution between labor and capital. Formally:

$$\frac{\partial LS}{\partial \theta} = -\varepsilon\alpha(1-\theta)k^{\varepsilon-1}\frac{\partial k}{\partial \theta} + \alpha k^{\varepsilon} = \alpha k^{\varepsilon} > 0 \tag{4.7}$$

using $\frac{\partial k}{\partial \theta} = 0$, which in turn reflects the fact that changes in workers' bargaining leave unchanged the capital-to-output ratio due to the efficient bargaining condition.

4.2.4 Implications for the Empirical Analysis

Some of the insights from these two stylized models are similar. For example, in both cases, lower workers bargaining power unambiguously reduces the labor share if labor and capital are relative complements. Other predictions vary, particularly regarding whether lower bargaining power always reduces the labor share.

In practice, actual bargaining may combine elements of both models. The Right-to-Manage model has been regarded as describing rather well the actual functioning of labor markets in most European countries (see for instance Layard et al., 2005). At the same time, it has been argued that unions still play a part in determining the employment level, such that actual bargaining mixes up elements of both the right-to-manage and efficient bargaining models (see, among others, the theoretical contribution of Manning, 1987). This leads Cahuc, Carcillo, and Zylberberg, 2014, for example, to conclude that the right-to-manage and efficient bargaining models may ultimately "*represent limit cases of the same model*" (page 441).

Insofar as EPL increases worker bargaining power, the key implication for our empirical analysis is that deregulation is more likely to lower the labor share in countries and/or industries where capital and labor are less substitutable. In the next sections, we take these insights to the data.

4.3 Dataset

In this section, we describe the data used in the empirical analysis. We start by illustrating the dataset of EPL reforms episodes that are the focus of the analysis. Next, we discuss the derivation of the layoff rates and the estimation of the elasticities of substitution. The section proceeds presenting the labor share and remaining data and it concludes providing some key stylized facts regarding the evolution of the labor share over the 1970-2015 period, with major emphasis on its behavior around EPL reform episodes.

4.3.1 Employment Protection Legislation Reforms

Major reforms to EPL are identified by examining legislative and regulatory actions reported in all available editions of the OECD Economic Surveys, as well as additional country-specific sources, for 26 advanced economies over the 1970-2013 period (for details, see Duval et al., 2018).[2] This methodology is related to the 'narrative' approach used by Romer and Romer (1989, 2004, 2010, 2017) and Devries et al., 2011 to identify monetary and fiscal shocks and periods of high financial distress.

[2] The 26 countries covered are: Australia, Austria, Belgium, Canada, Czech Republic, Denmark, Finland, France, Germany, Greece, Iceland, Ireland, Italy, Japan, Korea, Luxembourg, Netherlands, New Zealand, Norway, Portugal, Slovak Republic, Spain, Sweden, Switzerland, United Kingdom and United States.

In a first step, over 100 legislative and regulatory actions related to EPL are identified. In a second step, such actions are classified as major liberalizing or tightening reforms if one of the following three alternative criteria is met: (i) the OECD Economic Surveys uses strong normative language to define the action; (ii) the policy action is mentioned repeatedly across different editions of the Survey; or (iii) the OECD EPL indicator is in the 5th percentile of the distribution of the absolute changes in the indicator — or it would be if the OECD's scoring system were applied, but no OECD EPL indicator score is available for the country and year considered. When only the third condition is met, an extensive search through other available domestic and national sources is performed to identify the precise policy action underpinning the change in the indicator. Following this process, a variable is constructed that, for each country, takes value 0 in non-reform years, 1 in liberalizing reform years, and -1 in tightening reform years. Table C.1 in Appendix C.1 lists all reforms and tightening reforms identified in this way, while Figure C.1 shows the distribution of reforms over time.

An essential advantage of this approach vis-à-vis other existing databases is that it identifies major legislative reforms as opposed to just a long list of actions that in some cases would be expected to have little or no bearing on macroeconomic outcomes. Likewise, compared with an alternative approach that would infer major reforms from large changes in the OECD EPL indicators, we have a more extended time-series coverage — starting in 1970 rather than 1988 — and document precisely the timing of each action. These features are particularly useful for our empirical analysis that seeks to identify the dynamic effects of reforms.

4.3.2 Layoff Rates

To identify the effect of reforms at the industry level, we derive industry-specific measures of layoff rates. To compute those, we strictly follow the approach of Bassanini, Nunziata, and Venn, 2009, which in turn builds on Micco and Pages, 2006, and define them as the percentage ratio of laid-off workers over total wage and salary employment. Differently from Bassanini, Nunziata, and Venn, 2009, our rates are computed to match the ISIC Rev. 4 industry classification (the one used in the 2017 EU KLEMS database). To this purpose, we use data contained in the 2014 Displaced Workers Survey (DWS), conducted in the context of the more comprehensive IPUMS-CPS (see Flood et al. (2017)).[3] We use U.S. data given that employment protection legislation is virtually non-existent there. Hence, the U.S. is the closest empirical example of a frictionless economy in which employers can freely adjust the workforce in response to operational needs. Appendix C.2 describes in detail the construction of US layoff rates. Table C.2 lists the layoff rate for each

[3] Bassanini, Nunziata, and Venn, 2009 construct U.S. layoff rates using data contained in the 2004 CPS Displaced Workers Supplement for 22 industries classified to match the classification used in the EU KLEMS 2007 database (ISIC Rev. 3 classification).

industry in the sample. The industry with the highest layoff rate is Electrical and Optical Equipment, while that with the lowest is Coke and Refined Petroleum.

4.3.3 Elasticities of Substitution

While several papers provide estimates of the elasticities of substitution (EOS) for the aggregate economy, fewer focus on the industry level, and to our knowledge none does so for the ISIC Rev 4 industry classification, which is the one we use (see below). Therefore, we derive industry-specific EOS following standard practice in the literature, estimating the structural parameter directly from the solution to the firm's profit maximization problem (see, among others, Berndt, 1976, and Antras, 2004). In particular, we infer industry-specific EOS by estimating the following equation:

$$ln\left(\frac{P^K_{j,t}}{P_{j,t}}\right) = ln(\alpha_j) = \frac{1}{\sigma} ln\left(\frac{F_{j,t}(K_{j,t}, L_{j,t})}{K_{j,t}}\right) + \epsilon_{j,t} \tag{4.8}$$

Where P^K is the price of capital services; P is the price of the aggregate output $F(K, L)$; K and L, are capital and labor services, and σ is the elasticity of substitutions. Appendix C.3 provides details on the estimation, as well as an alternative measure of EOS that will be used as robustness checks. Table C.3 lists the EOS for each industry in the sample. The average is about 0.7, with the EOS varying between 0.3 (Construction) and 1.5 (Telecommunications). Overall, our estimates are in line with those of Antras, 2004, Oberfield and Raval, 2014 and Lawrence, 2015. Using different methodologies and data, these authors found the average EOS in the U.S. to be well below unity.

4.3.4 Labor Share and Other Data

Country-level time series of labor shares are taken from OECD Analytical Database. These data cover an unbalanced set of 26 advanced economies from 1970 to 2015. To derive industry-country labor shares, we use harmonized data on value added and labor compensation as contained in the EU KLEMS databases. To maximize the country-coverage, we use data from both the 2017 release (Jäger, 2017) and the 2012 release (O'Mahony and Timmer, 2009).[4] Overall, for the country-industry-time level analysis, we have an unbalanced panel comprising 32 industries in 22 advanced economies from 1970 to 2015.[5] Mean values of labor shares and value-added shares,

[4] The EU KLEMS database provides data on added value and labor compensation in 34 industries, classified according to the ISIC Rev. 4 classification. Next, we define the labor share as the percentage ratio of labor compensation relative to added value. We drop two industries from the sample, namely activity of households as employers and activities of extraterritorial organizations and bodies, as for most countries labor compensation and/or added value data are not available.

[5] The countries for which industry-level data are not available are Iceland, New Zealand, Norway, and Switzerland.

together with the layoff rates and estimated EOS used for the baseline analyses, are reported in Table C.4 of Appendix C.4.

Whereas below we present stylized facts for all the 32 industries, our baseline empirical analysis does not cover those that typically belong to the public sector, and it also excludes the agriculture and the construction industries. To motivate this choice, we observe that special EPL provisions typically apply to civil and public servants as well as seafarers.[6] Moreover, EPL generally does not apply to seasonal workers, who account for a sizable share of overall employment in the agriculture and construction industries. We also exclude from the analysis the (i) Coke, Refined Petroleum and Nuclear Fuel, and the (ii) Other Manufacturing industries due to potential issues in the measurement of added value. In a sensitivity analysis, we show that our baseline results do not hinge on the exclusion of these two industries.

To verify that our results are consistent with a reform-driven decline in worker bargaining power and real wages, we construct a measure of the real wage using data on average hourly earnings and hours worked (Jäger, 2017) as well as on the price level (IMF World Economic Outlook).

All specifications control for major reforms of EPL for temporary workers, which are identified following strictly the same approach used to construct the dataset of major reforms of EPL for regular workers (for details, see Duval et al., 2018). For robustness checks, we collect further data to be used as additional controls. Two variables capture the roles of technological change and globalization, which feature prominently in the recent literature on labor share drivers. Specifically, we proxy for openness to trade and technological change using respectively the ratio of imports and exports to GDP and the price of investment goods relative to output (both sourced from the Penn World Tables, version 9.0, see Feenstra, Inklaar, and Timmer, 2015). Moreover, since current and expected GDP growth rates could correlate with both EPL reforms and labor shares, we also control for them, using data from the OECD Economic Outlook. Finally, we control for trade union density, which we take from the ICTWSS database (Visser, 2016).

4.3.5 Stylized Facts

Appendix C.5 discusses some stylized facts about the evolution of labor shares over the period 1970-2015.[7] Three stand out. First, labor shares have generally been on a declining trend since the mid-1970s, with the decline accelerating in the 1990s. Second, there exist significant heterogeneities both across countries and industries.

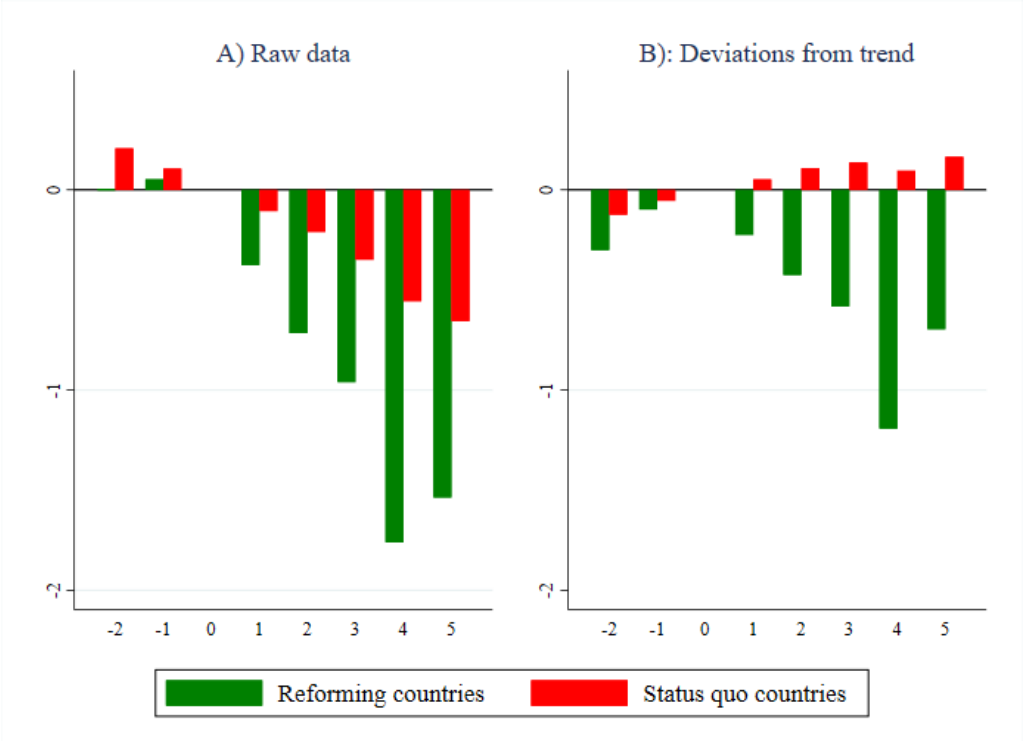
[6] Among the countries covered in our analysis, EPL for public and civil servants is governed by special laws in the following ones: Austria, Belgium, Czech Republic, Denmark, Finland, France, Germany, Greece, Italy, Japan, Korea, Luxembourg, Portugal, Slovakia, Spain, and the United States. For more information, see the ILO EPLex database.

[7] Since most of our stylized facts rely on data at the country-industry-level, for consistency, this section focuses on the 22-country sample for which such data are available. Country-level stylized facts for our full sample of 26 countries are available upon request.

Third, about 70 percent of the decline in country-level labor shares can be accounted for by within-industry changes.

Most importantly in the context of this paper, the decline in the labor share has been typically larger in periods following EPL reforms. To document this, we start by noting that liberalizing reforms were predominantly implemented during the 1990s and the 2000s, which is also the period over which the labor share declined the most (see Figure C.2 in Appendix C.5). Then, in Figure 4.1 we compare the mean cumulative change in country labor shares in the years before and after any EPL reform in reforming countries (solid green bars) versus non-reforming countries (red bars). Before EPL reforms, labor shares had typically been on a declining trend whose slope was similar between reforming and non-reforming countries. Crucially, the decline accelerated following EPL deregulation, while it did not in non-reforming countries.

Figure 4.1: Cumulative changes in country labor shares around reform years



Notes: the figure compares the mean cumulative change in country labor shares relative to reform years in (i) reforming countries (green bars), and (ii) status quo countries (red bars). Panel A reports changes in the raw data. Panel B reports changes in de-meaned and de-trended data. The y-axis measures the size of the mean cumulative change (in percentage points). The x-axis represents the number of years before (negative numbers) and after (positive numbers) the base year (denoted by 0).
Sources: Jäger, 2017, Duval et al., 2018 and own calculations.

The fact that the labor share was declining at a similar pace in reforming and non-reforming countries before reform years suggests that our reform episodes were orthogonal to labor share trends. We also formally verify this by running a simple

multinomial logit regression. Particularly, we regress our reform variable $R_{j,t}$ and regress it onto the contemporeaneous as well two lags of the labor share change, plus its own two lags and the variable capturing major reform to temporary contracts (similar to our baseline specification). The results (available upon request) do not suggest that endogeneity is an issue.

To check whether the decline in the labor share in the aftermath of EPL reforms displayed some heterogeneity across industries, we repeat the same analysis for within-industry labor shares by splitting the sample according to industry characteristics (Figure 4.2). Panel A (B) of Figure 4.2 shows the mean cumulative change in the labor share before and after EPL reforms for industries in the lower (upper) quartile of the distribution of US layoff rates. Panel C (D) shows the same statistics, but for industries in the lower (upper) quartile of the distribution of EOS. In line with priors, the decline in labor shares following EPL reforms observed at the macro level appears to be driven by industries with higher layoff rates and higher relative complementarity between capital and labor. This gives us comfort about the identification strategy that we adopt to establish the causal effects of labor market deregulation on labor shares, which we explain more in detail in the next section.

4.4 Econometric Framework

To estimate the dynamic response of labor shares to EPL reforms, we employ the local projection method proposed by Jordà, 2005 to derive impulse-response functions (IRFs). This approach has been advocated by Auerbach and Gorodnichenko, 2012 and Romer and Romer, 2017, among others, as a flexible alternative to vector autoregression (autoregressive distributed lag) specifications since it does not impose dynamic restrictions and it is better suited to estimate nonlinearities in the dynamic response.

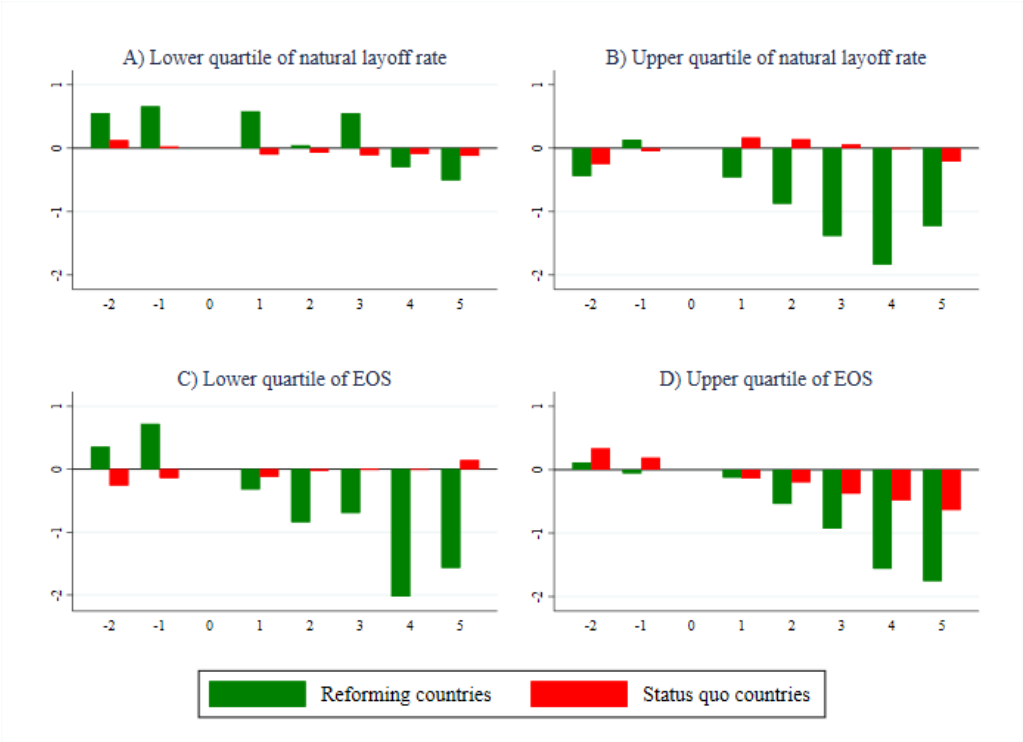
Starting with the country analysis, we estimate the following equation for each response horizon $k = 0, .., 5$:

$$\begin{aligned} y_{j,t+k} - y_{j,t-1} = \alpha_j + \tau_t + \beta_k R_{j,t} + \theta X_{j,t} + \\ + \sum_{h=1}^{k} \varphi_h R_{j,t+h} + \sum_{l=1}^{L} (\delta_l \Delta y_{j,t-l} + \gamma_l R_{j,t-l}) + \epsilon_{j,t} \end{aligned} \tag{4.9}$$

in which j and t denote country and time; y is the labor share of income; β_k denotes its response at each horizon (year) k after the reform; α_j are country fixed effects, included to take account of differences in countries' invariant characteristics; τ_t are time fixed effects, included to take account of global shocks; $R_{j,t}$ is our EPL reform variable, which takes value 0 in non-reform years, 1 in liberalizing reform years and -1 in tightening reform years; $X_{j,t}$ is a set of control variables; and Δ denotes the first difference operator.

Equation 4.9 includes forward reform dummies ($\sum_{h=1}^{k} R_j t + h$). This is to control for reforms that happen within the response horizon $t + k$ (for $k > 1$) that are not

Figure 4.2: Cumulative changes in industry labor shares around reform years by industry



Notes: the figure compares the mean cumulative change in country-industry labor shares relative to years of EPL reforms in (i) reforming countries (green bars), and (ii) status quo countries (red bars), and for industries in the lower (Panel A) and upper (Panel B) quartiles of the layoff rates as well as those in the lower (Panel C) and upper (Panel D) quartiles of the elasticities of substitution. The y-axis measures the size of the labor share change (in percentage points). The x-axis represents the number of years before (negative numbers) and after (positive numbers) the base year (denoted by 0).
Sources: Jäger, 2017, Duval et al., 2018 and own calculations.

captured by $R_{j,t}$. As shown by Teulings and Zubanov, 2014, not doing so would leave the model misspecified and bias our estimates. In our context, this is particularly important since EPL reforms are sometimes adopted in sequence or reversed after some years.

We also include recession dummies and dummies capturing reforms to temporary contracts. The former aim to address possible omitted variable bias that could stem from the fact that economic conditions may shape the likelihood of reform, as suggested by the 'crisis-induces-reform' hypothesis (Drazen and Easterly, 2001; Tommasi and Velasco, 1996), while the latter attempt to control for potential contemporaneous reforms that may also influence the labor share. In a sensitivity analysis, we add further controls, including, among others, trade openness and the relative price of investment goods, which have been put forward as prominent drivers of labor share trends in advanced economies.

(Elsby, Hobijn, and Şahin, 2013; IMF, 2017; Karabarbounis and Neiman, 2013). We find our results to be unaffected, reflecting that major EPL reforms are not

correlated with these drivers. The empirical specification is completed by two lags of the 1-period labor share change and of the EPL reform dummy.[8]

Equation 4.9 is estimated using OLS. IRFs are obtained by plotting the β_k coefficients for $k = 0, .., 5$, with 90 percent confidence bands computed using the associated standard deviations, based on clustered robust standard errors.

Next, to minimize any endogeneity concerns and explore the channels through which EPL reforms affect the labor share of income, we turn to country-industry-level analysis, using a difference-in-differences identification strategy in the spirit of Rajan and Zingales, 1996. Specifically, we estimate the following equation:

$$\begin{aligned} y_{i,j,t+k} - y_{i,j,t-1} = \tau_{j,t} + \alpha_{j,t} + \mu_{j,t} + \beta_k \vartheta_i R_{j,t} + \theta X_{i,j,t} + \\ + \sum_{h=1}^{k} (\varphi_h \vartheta_i R_{j,t+h}) + \sum_{l=1}^{L} (\delta_l \Delta y_{i,j,t-l} \gamma_l \vartheta_l R_{j,t-1}) + \epsilon_{i,j,t} \end{aligned} \tag{4.10}$$

in which $y_{i,j,t+k}$ is the labor share in industry i of country j in period $t+k$; $\tau_{j,t}$ are country-time fixed effects, which control for any variation that is common to all industries of a country's economy, such as country-wide macroeconomic shocks and reforms in other (non-EPL) areas; $\alpha_{i,j}$ are country-industry fixed effects, included to take account of cross-sectional differences in average changes in country-industry labor shares; $\mu_{i,t}$ are industry-time fixed effects that control for different labor share changes across industries $R_{j,t}$ our EPL reform variable; ϑ_i industry-specific characteristics, discussed below, which we use to identify the causal effects of EPL reform on the (country-industry-level) labor share; $X_{i,j,t}$ is a set of control variables including a temporary contracts reform dummy plus, in a sensitivity analysis, other labor share drivers. All controls are interacted with industry-specific characteristics (ϑ_i). As in the country-level analysis, we include forward reform dummies (see Teulings and Zubanov, 2014) as well as two lags of the first-difference of $y_{i,j,t+k}$ and of $R_{j,t}$.

This difference-in-differences specification relies on two alternative identification assumptions. The first is that stringent dismissal regulations are more binding, and therefore raise workers' bargaining power more, in industries characterized by a higher 'natural' propensity to adjust their workforce (that is, a higher 'natural' layoff rate). The second identifying assumption follows from our theoretical framework and suggests that job protection deregulation is likely to reduce the labor share more in industries where capital and labor are less substitutable. Hence, we estimate Equation 4.10 using three alternative industry-specific characteristics, ϑ_i: (i) the 'natural' layoff rate; (ii) $1-\varepsilon$ the inverse of the EOS ($1-\varepsilon = 1/\sigma$); and (iii) the interaction between these two, because the lower the EOS is, the more deregulation should reduce the labor share in industries where EPL is more binding. Since we include country-year dummies, which control for aggregate effects, our results should be interpreted as the cross-industry differential effects.

[8] As shown below, the results are robust to different lag specifications.

Equation 4.10 is estimated with OLS for each $k = 0, .., 5$. Similar to the country-level analysis, IRFs and the associated confidence bands are computed using the coefficients β_k and the respective standard errors. These are clustered at the country-industry-level and, for the identifications relying on (i) the EOS and (ii) the interaction between EOS and the layoff rates, they are obtained through bootstrapping.[9] The inclusion of the rich set of fixed effects and controls should largely address endogeneity concerns related to omitted variable bias. Besides, reverse causality is unlikely to be a concern in our set-up. First, the natural propensity to layoff in the U.S. is arguably orthogonal to industry-level labor share changes in other countries. A similar argument holds for the EOS between capital and labor. Second, it is highly unlikely that industry-level labor share patterns can influence EPL reform. Movements in the labor share at the aggregate level may well do so, but this potential source of reverse causality is addressed through the inclusion of country-time fixed effects. In other words, claiming reverse causality would mean arguing that differences in labor share changes across industries lead to economy-wide EPL reforms. This, we argue, is implausible.

4.5 Baseline Results and Robustness Checks

4.5.1 Country-level Analysis

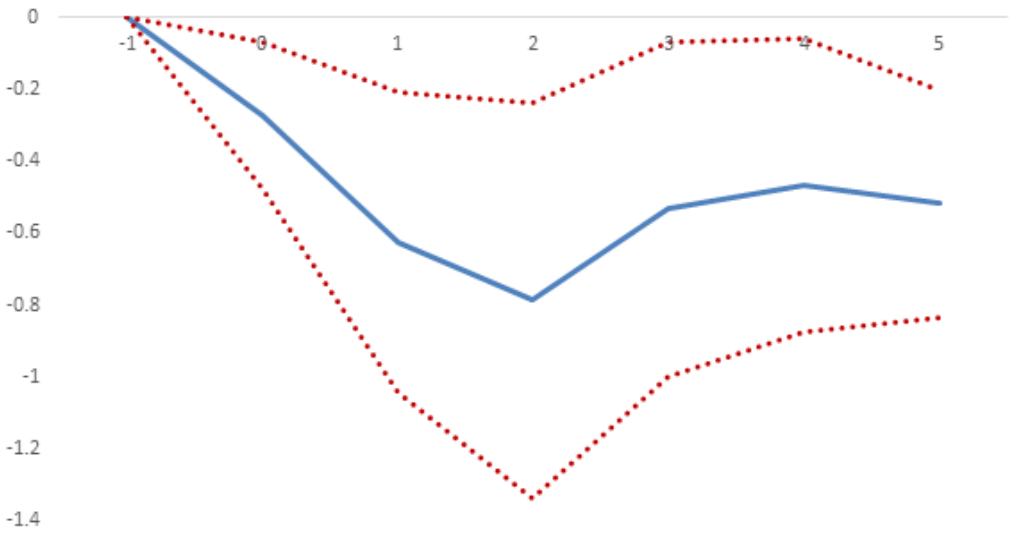
Figure 4.3 shows the estimated dynamic response of the labor share to a liberalizing EPL reform over the five-year period following implementation, together with the 90 percent confidence interval around the point estimate. Major deregulation episodes have a statistically significant and persistent detrimental effect on the labor share. This effect reaches 0.8 percentage point two years after the reform, before declining marginally to 0.6 percentage point. It eventually levels off at this level eight years after the reform.[10]

Next, we perform a few back-of-the-envelope calculations to get a rough sense of the share of the overall decline in labor shares that may be ascribed to EPL deregulation, based on our estimates. We found a major EPL liberalizing reform to cause the labor share to decline by about 0.6 percentage point over the four years following the reform. By calculating the net number of liberalizing reforms over the period considered for each country, we can compute an illustrative estimate of the overall impact of EPL deregulation on the change in the labor share. Taking the average of these estimates across countries, we find that deregulation may

[9] When we estimate Equation 4.10 using the EOS and the interaction between EOS and layoff rates as identification variables the regressor are derived from estimated variables themselves and standard t-statistics may be biased upwards. Hence, we compute standard errors via bootstrapping method (with 500 replications). However, our results are robust to not using bootstrapped standard errors.

[10] We also separately estimated the effect of liberalizing and tightening EPL reforms. As expected, the magnitude of the estimated response is similar (although of opposite sign). This indicates that our results are not driven by tightening reform episodes.

Figure 4.3: Country-level analysis – baseline results



Notes: estimates based on Equation 4.9. Solid line denotes the percentage point response of labor share to EPL reforms. Dotted lines indicate 90 percent confidence interval based on clustered standard errors. The X-axis reports the horizon, with 0 indicating the reform year. The Y-axis reports the magnitude of the estimated coefficients (in percentage points).
Sources: OECD Analytical Database, Duval et al., 2018 and own calculations.

have accounted for about 14 percent of the overall labor share decline in advanced economies over 1970-2015.

Our figures implicitly assume that the labor share decline estimated over the four years following the reform persists in the long run. Indeed, this is what our analysis suggests if we extend its horizon beyond four years; in our baseline regression, the effect of EPL reforms is found to stabilize at about -0.8 percentage point after eight years. Given that the magnitude of the trend decline in the labor share depends on the period considered, including on whether the end year falls within a recession or an expansion period (Kehrig and Vincent, 2017), we also perform the same calculation over the periods 1970 to 2007 — thereby excluding the Great Recession — and from 1990 to 2015, when the trend decline in the labor share was steepest. Remarkably in line with the overall estimate above, we find that changes in EPL contributed about 14 percent and 15 percent to the overall labor share decline over the 1970-2007 and 1990-2010 periods, respectively.

Robustness checks

To check the sensitivity of these results to potential sources of endogeneity, we estimate two additional specifications with a richer set of control variables. First, we control for factors that have been put forward as fundamental forces behind the trend labor share decline in advanced economies, namely technological progress and international trade, as well as other potential drivers such as changes in trade union density. Second, we also estimate a specification including past GDP growth as

well as expected future GDP growth between periods t and $t+k$ — the horizon over which the impulse response functions are computed — at time $t-1$. Table 4.1 summarizes the results from these two robustness checks; they turn out to be very similar to, and not statistically different from, our baseline, suggesting that the potential sources of endogeneity listed above are not empirically important in practice.

Table 4.1: Country-level analysis – robustness checks

	Impact	1y	2y	3y	4y	5y
Baseline	**-0.27**	**-0.63**	**-0.78**	**-0.53**	**-0.47**	**-0.55**
Other labor share drivers	**-0.26**	**-0.58**	**-0.85**	**-0.69**	**-0.62**	**-0.69**
(Exp.) GDP	**-0.28**	**-0.61**	**-0.73**	**-0.47**	**-0.47**	**-0.49**

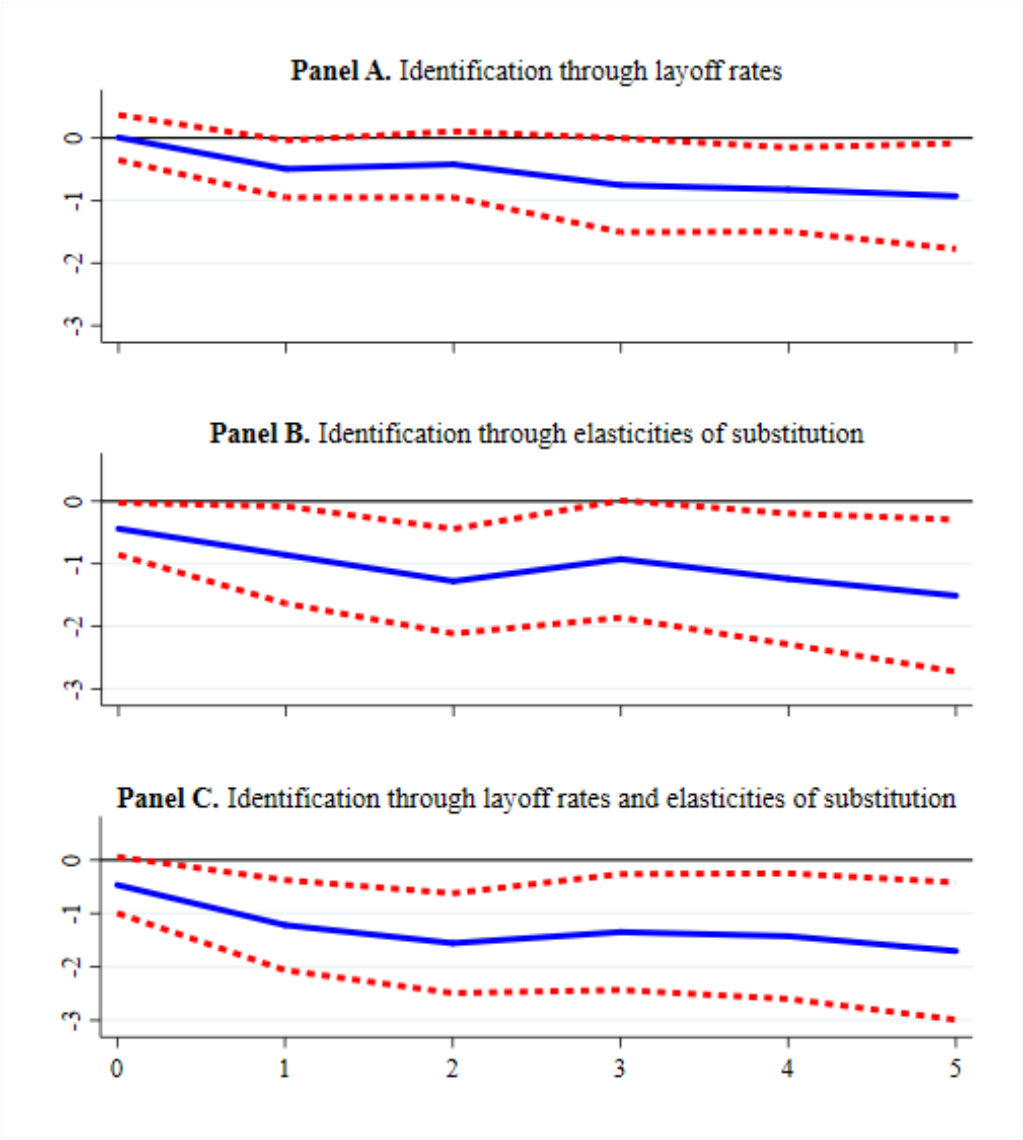
Notes: estimates based on Equation 4.9. Coefficients are in percentage points. Bold numbers indicate significance at the 90 percent confidence interval, based on clustered standard errors. The row "Other labor share drivers" reports estimates from a regression including the change in union density, the relative investment price, the trade openness as controls. The row "(Exp. GDP)" reports estimates based on a regression including current, past and expected future GDP growth as controls.
Sources: OECD Analytical Database, Duval et al., 2018, Feenstra, Inklaar, and Timmer, 2015, Visser, 2016, and own calculations.

4.5.2 Country-industry-level Analysis

Figure 4.4 presents the results from the country-industry analysis, that is, from estimating Equation 4.10. Panels A, B and C show the IRFs when the effect of the reform is identified, respectively, through the layoff rates, the EOS and the interaction between these two. The estimated coefficients are rescaled by interacting them with the difference between the values of the 75th and 25th percentiles of the relevant industry characteristics. Therefore, the IRFs show the estimated differential effect of the reform between industries in these percentiles. The same approach is applied to construct the confidence bands. The results are qualitatively similar across all specifications, indicating a relative decline in the labor share in high layoff rates and low substitutability industries. They are also quantitatively larger when we identify the reform using both the layoff rates and the EOS, which is our preferred specification.

Panel A shows that over the medium term — five years after the reform takes place — job protection deregulation tends to reduce the labor share in industries with a high layoff rate relative to those with a low-layoff-rate. The differential medium-term reduction in the labor share following an EPL reform between an industry with a relatively high natural layoff rate (at the 75th percentile of the cross-industry distribution of layoff rates in the U.S) and one with a relatively low natural layoff rate (at the 25th percentile of the distribution) is about 0.9 percentage point.

Figure 4.4: Country-industry-level analysis – baseline results



Notes: estimates based on Equation 4.10. Solid lines denote the estimated average differential labor share effect of EPL reforms between industries in the 75th percentile and 25th percentile of the layoff rates distribution (Panel A), in the 25th percentile and 75th percentile of the distribution of the elasticities of substitution (Panel B) and in the 25th percentile and 75th percentile of the distribution of the interaction between the two (Panel C). Dotted lines indicate 90 percent confidence interval based on standard errors clustered at the country-industry-level. For Panels B and C standard errors are obtained through bootstrapping (500 replications. The Y-axis reports the magnitude of the estimated coefficients (in percentage points), while the X-axis reports the response horizon (in years).
Sources: Jäger, 2017, Duval et al., 2018, Flood et al. (2017) and own calculations.

In line with theory, the results also suggest that the effect of EPL reforms on the labor share tends to be larger in industries with a lower EOS between capital and labor (Panel B). The medium-term differential reduction in the labor share between an industry with a relatively low EOS (at the 25th percentile of the ε's distribution)

and one with a relatively high EOS (at the 75th percentile of the ε's distribution) is about 1.5 percentage point.

Finally, and as expected, the identification through the interaction between the natural layoff rate and the EOS yields the largest and sharpest estimate of the differential impact of EPL reforms across industries (Panel C). In short, the effect tends to be larger in industries with a higher natural layoff rate and a lower elasticity of substitution. Quantitatively, the joint effect of moving from the 25th to the 75th percentile of the layoff rate and from the 75th to the 25th percentile of the EOS' distribution is about -1.7 percentage point 5 years following a liberalizing EPL reform. Except upon impact, the effect is statistically significant over the entire horizon considered.

Under a number of simplifying assumptions, discussed in Appendix C.7, we perform a back-of-the envelope calculation similar to the one based on our country-level estimates. Reassuringly, we find that EPL deregulation might have accounted for about 15 percent of the overall labor share decline in the average advanced economy, which is very similar to what we calculated using our country-level estimates.

Robustness checks

We now check the sensitivity of our results to several different specifications. Relevant results are reported in Appendix C.6. We start verifying that our findings are not driven by any given country or industry. To do so, we estimate Equation 4.10 excluding first one country and then one industry at a time. Figures C.10 and C.11 report the corresponding impulse responses, together with the baseline estimates and relative confidence bands. All the newly obtained impulse responses lie close to the baseline and always fall within its confidence bands.

Since the data we used for the labor share comes from two different vintages of the EU KLEMS database (2012 and 2017), there might be statistical differences across them. Therefore, we verify that our results hold when only the latest version is used (Table C.5). In Table C.5 we also show that the baseline estimates do not depend on the exclusion of the Coke, Refined Petroleum and Nuclear Fuel, and the Other Manufacturing industries. Finally, exploiting that the EPL reforms we analyze generally do not apply to the public sector, agriculture and construction, we estimate an alternative specification in which these industries are used as a control group by setting $\vartheta_i R_{j,t}$ to 0 for them. In line with our expectation, the results point to even larger differential effects across industries (Table C.5). The distribution of industries according to natural layoff rates relies on US layoff rates that might be imprecisely estimated. To address this potential concern, we rerun the specifications that rely on the layoff rates for the identification using an alternative measure. Specifically, we divide industries into two categories depending on whether their layoff rates were above the median in all the three years covered by the 2014 Displaced Workers Survey (see Table C.2), and construct a dummy variable that takes value 1 (0) for industries in which EPL is (is not) binding. According to this rather conservative classification, EPL only binds in seven industries, and in an equal manner across

them. In another robustness check, we use the layoff rates calculated for the year 2013 instead of the average over 2011-2013. The impulse responses obtained using these alternative measures, reported in Table C.6, are qualitatively similar to the baseline results. When the dummy variable is used, the estimated coefficients of the reform variable are quantitatively lower than in our baseline, but they cannot be readily compared. Importantly, when our preferred identification strategy based on the interaction between layoff rates and EOS is used, the negative impact of EPL reforms on the labor share is statistically significant at least form the third year onward in all cases.

We also run a sensitivity analysis on our measure of the EOS between labor and capital, re-estimating our specifications using the alternative sets of EOS discussed in Appendix C.3. We employ in turn the EOS estimated (i) using data on the real capital stock as a proxy for capital services, (ii) using the nominal capital stock divided by capital services to proxy for the rental rate of capital, and (iii) relaxing the assumption of Hicks-neutral technical change. The results, presented in Table C.7, are very similar to our baseline results. Again, they are most statistically significant when using our preferred identification strategy based on the interaction between layoff rates and EOS.

Another possible concern with an OLS estimation of Equation 4.10 might be that the results could be biased due to the omission of other macroeconomic developments that may affect industry-level labor shares through their interaction with industry-specific natural layoff rates or/and the elasticities of substitution, and that may at the same time correlate with EPL reforms. A candidate is the change in union density, whose trend decline may have reduced workers' bargaining power and affected the labor share through the same channels as EPL reforms. While changes in union density are not correlated with EPL reforms — the correlation is only -0.01 — we nonetheless check the robustness of our results by adding to Equation 4.10 an interaction term between the change in union density and the industry-specific natural layoff rate (or/and the elasticity of substitution).

Likewise, while the effects of technological progress — proxied by the relative price of investment — and trade openness on labor shares are controlled for through country-time fixed effects, they could still be a source of omitted variable bias if (i) they were correlated with EPL reforms, and (ii) their impact varied with industry-specific characteristics. Therefore, we check the robustness of our results by also adding to Equation 4.10 the interaction of these variables with industry-specific characteristics. Table C.8 shows the results from these sensitivity analyses. The effects of EPL reforms on country-industry labor shares when controlling for the additional factors described above are very close to, and not statistically different from, our baseline estimates.

4.6 Extensions

What factors drive the negative response of the labor share to job protection deregulation? Wage bargaining models of the type we presented in Section 4.2 imply that, insofar as EPL reform reduces worker bargaining power, it should lower the real wage, all else equal. Implications for the capital-to-output ratio and the employment level are more ambiguous a priori, as they depend on whether bargaining takes place only over the wage or also over employment.

As a cross-validation exercise, we test whether EPL reforms are also associated with a decline in the real wage. To this end, we re-estimate Equation 4.10 using the change of the (log) hourly real wage as the dependent variable. Since there are no theoretical reasons to expect that the effect of deregulation on the real wage should depend on the elasticity of substitution, the identification relies exclusively on the layoff rate. We also apply the same approach to estimate the effect of deregulation on the employment level and the capital-to-output ratio.

Table 4.2 shows the estimated coefficients. In line with theoretical priors, EPL deregulation leads to a relative fall in the real wage in industries with a high natural layoff rate — where EPL is more binding, and deregulation thus has a greater impact on worker bargaining power — relative to those with a low rate. This negative differential effect between industries at the 75th and 25th percentiles of the layoff rate distribution gets larger over time, reaching about -1.5 percent four years after the reform. Consistent with this finding, employment growth instead shows a positive differential response, which becomes significant two years following the reform. The capital-output ratio displays a negative medium-term response, although this is not significant at conventional confidence levels. Overall, these results are supportive of a significant role of bargaining power in driving the impact of EPL deregulation on the labor share, in line with our illustrative theoretical framework.

Table 4.2: Country-industry-level analysis – extension on labor share drivers

	Impact	1y	2y	3y	4y	5y
Identification through layoff rates						
Labor share	0.01	**-0.5**	-0.42	-0.76	**-0.83**	**-0.93**
Real wage	0.22	**-0.96**	**-1.22**	**-1.38**	**-1.47**	**-1.3**
Employment	0.11	**0.41**	0.42	**0.83**	0.66	0.19
Capital-to-output ratio	1.45	2.84	0.49	-2.3	-4.66	-3.87

Notes: estimates based on Equation 4.10 and using layoff rates for the identification. The rows "Labor share", "Real wage", "Employment" and "Capital-to-output ratio" report estimates obtained using, respectively, the labor share, the log hourly wage deflated by the price index, the log of engaged individuals and the ratio of the nominal capital stock to value added as dependent variables. Bold numbers indicate significance at the 90 percent confidence interval, based on clustered standard errors at the country-industry-level.
Sources: Jäger, 2017, Duval et al., 2018, Flood et al. (2017) and own calculations

Next, we perform another extension to check whether the direction of the effect of EPL reforms depend on whether labor and capital are complement or substitute. Our baseline specification does not allow for any switch in the sign of the impact of EPL reforms on the labor share depending on whether the EOS is above or instead below 1. This is because we did not want to tie our empirical strategy to the Right-to-Manage model — or any other specific wage bargaining model — since actual wage bargaining is likely to be more involved and combine elements from various models. Yet, if bargaining took place only over wages following the Right-to-Manage model, the sign of the impact of EPL deregulation on the labor share should depend strictly on whether capital and labor are relative complements or instead substitutes. To test this formally, we split the sample in two according to whether the EOS is above or below 1 and run Equation 4.10 on the two restricted samples. For the identification, we rely on the natural layoff rates. Results are presented in Table 4.3.

Table 4.3: Extension on sample split according to the elasticity of substitution

	Impact	**1y**	**2y**	**3y**	**4y**	**5y**
Identification through layoff rates						
Full sample	0.01	**-0.56**	-0.48	-0.85	**-0.93**	**-1.05**
Elasticity above 1	**0.96**	**1.3**	**1.51**	**1.83**	1.39	0.86
Elasticity below 1	0.01	**-0.61**	-0.47	**-0.92**	**-0.92**	**-0.98**

Notes: estimates based on Equation 4.10 and using layoff rates for the identification. The rows "Full sample", "Elasticity above 1", "Elasticity below 1" report estimates obtained using, respectively, the full sample, the restricted sample of industries with elasticity of substitution above 1 and the restricted sample of industries with elasticity of substitution below 1. Bold numbers indicate significance at the 90 percent confidence interval, based on clustered standard errors at the country-industry-level.
Sources: Jäger, 2017, Duval et al., 2018, Flood et al. (2017) and own calculations

The new estimates are broadly in line with the Right-to-Manage model's predictions: relative to industries with a low natural layoff rate, those with a high layoff rate experience an increase in the labor share following a liberalizing EPL reform when the EOS above 1 (substitutability), and a drop if the EOS is below 1 (complementarity).

Quantitatively, the effect of moving from the 25th to the 75th percentile of the layoff rate distribution among industries characterized by substitutability is significant already upon impact and reaches about +1.8 percentage point after 3 years, whereas the corresponding effect is negative and significant, at about -0.9, in the sub-sample of industries with EOS below 1. At longer horizons, the effect becomes statistically insignificant in the former group of industries (possibly owing to loss of statistical power due to the small sample), while it remains significant in the latter.

4.7 Conclusions

This paper explored the impact of job protection deregulation on labor shares using both country-time-level and country-industry-time-level data and a new dataset of major reforms of regular contracts covering 26 advanced economies over the past four decades. We applied the local projection method to estimate the dynamic response of labor shares at both the country- and country-industry-level. For the latter analysis, we used two alternative identifying assumptions ala Rajan and Zingales, 1996 derived from theory, namely that job protection reforms should have more substantial effects in industries characterized by a high 'natural' propensity to regularly adjust their workforce and a low elasticity of substitution between capital and labor.

Unlike previous literature, we found a statistically and economically significant adverse effect of weaker job protection on labor shares. In line with theory, this effect is concentrated in industries with a higher propensity to regularly adjust the workforce and a lower elasticity of substitution between capital and labor, and it is likely driven by a reduction in wage rents. To account for country-specific macroeconomic shocks and other aggregate drivers of labor shares, as well as for industry-specific developments, our country-industry-level analysis included country-time and industry-time fixed effects, and country-industry fixed effects as well. Our findings are also robust to a variety of alternative specifications controlling for potential omitted variable bias and reverse causality as well as including different deterministic components.

Our results call for more research on the role of labor market deregulation, alongside those of technology and globalization, in the extensive literature on the drivers of the decline in labor shares. On the policy front, they also point to the need for assessing the effects of labor market reform plans on a wide range of macroeconomic outcomes — including productivity, employment, and output, but also wages and labor shares — and for addressing trade-offs between efficiency and equity when designing such reforms.