How Responsive are Wages to Firm-Specific Changes in Labor Demand? Evidence from Idiosyncratic Export Demand Shocks*

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Abstract

Do firms adjust wages in response to changes in their own demand level, or to changes in competitive pressure from rival employers? We study how exporters adjust wages in response to unexpected product demand shocks during the 2008–2009 Great Recession. Using rich data on Portuguese firms' pre-recession export shipments, we measure firm-level shocks to export demand during the Recession. We show that shocks constructed at the firm level are not necessarily firm-specific and can be decomposed into a common component affecting all producers in a product market and an idiosyncratic component affecting individual firms within markets based on the locations of their pre-Recession customers. We demonstrate that while both components impact firms' output and their workers' wages, the common component spills over from firms to their labor market rivals, whereas the idiosyncratic component does not. We find that 10-15% of firms' idiosyncratic demand passes through to their employees' wage growth with no effect on retention rates, implying significant dependence of wages on noncompetitive quasi-rents. Moreover, we find that wages respond primarily to shifts in internal labor demand when labor markets are thin, but they respond more to competition from other employers when labor markets are fluid. These results indicate that employers' ability to set wages hinges on the underlying competitiveness of the labor market.

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1 Introduction

Trade shocks to product demand have significant effects on wages in affected labor markets (Revenga, 1992; Kovak, 2013; Autor et al., 2013, 2016; Bartik, 2018). But why do firms adjust wages in response to these product-market shocks—is it in direct response to changes in firms' own labor needs, or in response to changes in wage competition from other employers in the labor market? In perfectly competitive models of the labor market, firms set wages to match those of rival employers, not in reaction to output demand levels within the firm. For example, if labor markets within regions are modeled as perfectly competitive as in Autor et al. (2013), shocks to the product demand and derived labor demand of a subset of firms in a region will affect wages of *all* employees in the labor market alike, not just those at affected firms. Yet in frictional and imperfectly competitive labor markets, workers may be in a better position to negotiate wages with their employer when demand is high, even absent a change in the external labor market. In that case, the incidence of shocks to a subset of firms in a labor market may fall disproportionately on the employees of affected firms. Who sets wages matters for evaluating protectionary measures and policies that subsidize or target specific firms against policies that target broader regions or sectors. Moreover, to the extent that wages are determined non-competitively within firms' internal labor markets (Doeringer and Piore, 1971), variation in firms' product market conditions might explain the cross-firm wage dispersion documented around the globe in part (Abowd et al., 1999; Barth et al., 2016; Card et al., 2018; Alvarez et al., 2018; Song et al., 2019).

In this paper, we examine how firms adjust wages in response to firm-specific shocks to output demand and how behaviors depend on competition from rival employers. Implementing such a test is difficult in practice. One must identify idiosyncratic shocks to demand—which affect individual firms without systematically affecting rival employers—from common shocks that affect many employers competing in a labor market. A growing body of work documents that firms pay more when they sell or produce more (Guiso et al., 2005; Card et al., 2013a; Alvarez et al., 2018; Lamadon et al., 2019); however, conclusions drawn from such studies are limited by the possibility that changes in output may be simultaneously correlated with changes in labor supply inputs. Studies also find that demand shocks that impact industries (Revenga, 1992; Abowd and Lemieux, 1993), entire regions (Autor et al., 2014; Yagan, 2019), or the export sector as a whole (Verhoogen, 2008; Friedrich, 2014) have substantial incidence on wages.[1] Recent studies following Hummels et al. (2014) provide

[1] These studies provide evidence that barriers to competition divide labor markets into noncompeting segments,

evidence that firm-level export shocks lead firms to increase wages, even conditional on detailed industry fixed effects. However, most trade shocks simultaneously affect product demand and wage competition from other employers. Yet, to our knowledge, there is little direct evidence on whether the incidence of regional and industry trade shocks primarily falls on employees of firms whose sales are directly impacted or whether competition largely spreads the impacts of shocks through the labor market as a whole. More generally, we seek to understand how the structure of the labor market determines the incidence of such shocks among workers.

We shed light on these issues by separately identifying both idiosyncratic and common components of unexpected product demand shocks experienced by firms during the "Great Recession" of 2008–2009. We use several administrative datasets covering 2005–2013 to study Portuguese exporters that were exposed to large and unexpected shifts in foreign demand during this period. While Portugal did not experience a major domestic housing or financial crisis until the sovereign debt crisis at the end of 2011, Portugal is a small, open economy whose firms were nonetheless highly exposed to global fluctuations in trade demand that occurred in 2009 and 2010 (Eaton et al., 2016). The Great Recession is a particularly useful setting for our study because it generated significant unforeseeable sharp changes in demand for different products, which in turn varied significantly across countries. Accordingly, we use detailed data on Portuguese firms' pre-recession export shipments to implement a novel decomposition of firms' export demand shifts into a common component—which affects all firms in a product market—and an idiosyncratic component—affecting individual firms within markets based on the location of their pre-Recession customers. We demonstrate that while both components impact firms' output, employment, and wages paid to incumbent workers, the common component spills over from firms to similar firms in the same location and industry that likely hire workers from the same labor market segment, whereas the idiosyncratic component does not.

Our work advances a growing strand of literature that uses detailed data firms' export histories to construct firm-level demand shifters (Hummels et al., 2014; Berman et al., 2015; Mayer et al., 2016). Our methodology departs from prior studies in two important ways: First, we explicitly demonstrate that firm-level export demand measures combine idiosyncratic and common components and that these components can be distinguished empirically. Second, our study of trade shocks generated by the Great Recession provides a new and transparent source of quasi-experimental variation in export demand. The exogeneity of the demand shocks we study stems from the quasi-randomness of the

but do not necessarily imply that wages are determined within individual firms.

underlying import demand shifts that occurred around the world as a result of unforeseen recessions, not from ad hoc exclusion restrictions about "shift-share" trade exposure instruments in general. We implement direct tests of random sorting and parallel trends across shock levels to confirm the validity of our design. Our quasi-experimental analysis complements recent studies by Carlsson et al. (2016) and Lamadon et al. (2022), which use structural models to identify different types of demand shocks and to measure their pass-through to wages. Our approach also complements quasi-experimental studies of rent-sharing in specific labor market settings (Van Reenen, 1996; Kline et al., 2019; Howell and Brown, 2020) by examining the determinants of the wage incidence of demand shocks more generally.[2]

We find that workers bear part of the incidence of firms' trade shocks even when the broader market is unaffected. Employees working at firms that experienced a drop in demand saw lower wage growth, even when these shocks were idiosyncratic and had no impact on wages of employees of other firms in the same labor market. We find an idiosyncratic shock that lowers a firm's sales by 10 percent in the medium run reduces wages of incumbent employees by 1 to 1.5 percent relative to trend.[3] As baseline nominal wage growth for continuing employees was positive in Portugal during this period, these estimates imply that negative shocks dampened wage growth for continuing employees but would not have led firms to cut wages. In a broad class of models discussed in Section 2, these effects imply the costs of replacing employees are lower when firms' output demand levels decline—even if output demand at other firms remains constant. In the Portuguese setting, the threat points of workers likely stem in part from institutional restrictions on firing. Consistent with high firing costs, we find that Portuguese employers respond to product demand shocks by adjusting hiring, but not by firing existing employees.

The baseline results mask distinct mechanisms by which firms' trade shocks impact employees' wage growth. Consistent with theories of firm-specific human capital (Becker, 1962; Lazear, 2009), costly searches for workers with unique skills (Mortensen and Pissarides, 1994), and institutional firing costs (Lazear, 1990), we find that firm-specific trade shocks only impact wages in sectors where firms face substantial costs from employee turnover, which we infer based on low separation

[2] Earlier and contemporaneous studies examine rent-sharing as after firm innovations (Van Reenen, 1996), approvals of patent applications Kline et al. (2019), and government R & D grants (Howell and Brown, 2020). Though patent and cash grants may or may not directly impact the product demand or revenue productivity of firms, it is nonetheless the case that labor market imperfections may enable workers to directly capture some of the value of the assets gained by firms.

[3] We find that the common component of the shock has smaller effects on continuing employees, but these effects mask large extensive-margin effects on firm and worker exit. In addition, we find evidence that the common component of demand has greater impact on the wages of new hires.

rates and longer tenures. Meanwhile, we find no evidence that firms adjust wages based on their own demand conditions in sectors with higher degrees of fluidity. By contrast, we find that common shocks to demand have their largest incidence on wages in markets with higher degrees of fluidity. As implied by theory, firms and industries with lower turnover rates tend to have higher pay premiums indicated by firm fixed effects in Abowd et al. (1999) (henceforth AKM) wage models identified by movements of permanent-contract employees across firms. We find evidence that firms with higher AKM premiums pass idiosyncratic demand shocks onto wages to a higher degree. These findings support the hypothesis of Oi (1962) that turnover costs play an important role in wage determination—the easier replacing workers is, the less wages reflect internal labor demand within the firm as opposed to labor market competition.

These findings contribute to a burgeoning literature on wage determination in imperfectly competitive labor markets. Although standard models of monopsony (Manning, 2011; Card et al., 2018) and bargaining in frictional labor markets (Pissarides, 2000; Card et al., 2013a) imply that wage differences across firms may arise due to idiosyncratic differences in firms' demand levels, little causal evidence exists establishing a direct link between individual firms' demand and their employees' wages. In a simple framework, we show that idiosyncratic product demand shocks impact workers' bargaining position by changing the *firm's* outside option, while common product demand shocks also affect *worker's* outside options. Pass-through of idiosyncratic shocks requires a specific type of adjustment friction that makes the cost of replacing an employee—that employee's threat point at the bargaining table—increase when product demand is higher.[4] This study offers novel evidence that workers in frictional labor markets do gain stronger bargaining positions when employers face growing product demand—irrespective of conditions in the broader labor market.[5]

Our estimates can reconcile a puzzle in the earlier empirical literature on pay differences across firms: Card et al. (2018) found that while firms with 10 percent higher labor productivity levels have 1.3 percent higher AKM pay premiums in cross-sectional comparisons, non-experimental studies that correlated changes in pay with changes in firm output rarely find wage increases large

[4]This feature is a characteristic of labor markets with convex turnover costs (Manning, 2006; Acemoglu and Hawkins, 2014), of monopsonistic labor markets where firms face upward-sloping labor supply curves due to unobserved preference heterogeneity among recruits (Manning, 2011; Card et al., 2016), and of markets where insiders have the power to hold up the activity of the firm (Lindbeck and Snower, 2001).

[5]Our discussion of common shocks, which impact both revenue productivity and workers' outside options, is complemented by several recent studies that isolate shocks to workers' outside options from changes in productivity and test how the structure of workers' outside options affects their wages in imperfectly competitive labor markets (Beaudry et al., 2012; Caldwell and Danieli, 2018; Caldwell and Harmon, 2019; Jager et al., 2020).

enough to rationalize the general productivity-pay relationship. By contrast, in our quasi-experimental study, we find a causal relationship between firms' output growth and wages that is much larger than implied by correlational analyses, including our own, and that exactly matches the cross-sectional productivity-pay relationship. This difference between instrumental variables (IV) and ordinary least square (OLS) estimates is similar in nature to earlier findings by Abowd and Lemieux (1993) that industry-level rent-sharing estimates were attenuated in the OLS relative to the instrumental variables analysis; this suggests that using changes in output to infer underlying shocks introduces measurement error that attenuates estimates of wage incidence towards zero. Our estimates for low-turnover sectors are also consistent with findings from quasi-experimental studies of rent-sharing in specific settings where firm-specific human capital is likely important, including firm innovations (Van Reenen, 1996), approvals of patent applications Kline et al. (2019), and government R & D grants (Howell and Brown, 2020). Although our estimates can explain the well-established cross-sectional relationship between firm performance and firm pay premiums estimated from two-way fixed effects models, they are not large enough to explain the full wage variance attributable to AKM firm effects.[6] Nonetheless, our estimates imply that pay policy changes in response to employer performance can plausibly generate large cross-firm wage differentials—so long as there are sufficiently large barriers to replacing workers. These findings highlight the value of analyzing natural-experimental evidence in understanding the factors that drive wage determination.

The paper is organized as follows: Section 2 presents the conceptual framework that illustrates how labor market imperfections give rise to wage incidence of firm-specific shocks and how the objects we estimate relate to underlying frictions in the labor market. Section 3 provides background information about the context we study, and it describes the data sources we use. Section 4 presents the empirical strategy of the paper and provides evidence for its validity. Section 5 presents the main results about worker-level incidence of employer demand shocks. Section 6 examines heterogeneity across firms and individuals. In Section 7, we discuss the interpretation of our findings in the context of earlier studies. Section 8 concludes.

2 Competitive and Noncompetitive Channels of Wage Incidence

To motivate our analysis, we begin by presenting a stylized framework that highlights which types

[6] This is consistent with Sorkin (2018), which argues that a substantial fraction of the variance of AKM firm fixed effects reflects differences in non-wage amenities rather than rent-sharing.

of labor market frictions lead firms to pass-through idiosyncratic shocks to their workers' wages. We first show in a simple bargaining model where workers are costly to replace that the existence of labor-market rents does not in itself imply that idiosyncratic shocks will pass through to wages; rather, pass-through of firm-specific shocks specifically reflects changes in the firm's opportunity cost of replacing a worker. We then show this result generalizes across many alternate models and consider the interpretation of empirical elasticities.

We begin with a stylized single-period model. We consider a single-employee firm j that competes in monopolistic (or otherwise imperfectly competitive) product markets and thus faces a separate demand curve for a single product variety. Each firm's product is an imperfectly substitutable variety within an industry, and the level of demand is reflected as a firm-specific price level reflecting both a common group-level component and an idiosyncratic component:

$$P_j = \bar{P} \times p_j \tag{1}$$

where $\bar{P}$ is an industry-wide demand level, and p_j is a firm-variety-specific price premium.[7] For simplicity, we assume all producers in the industry use a specific type of skill and comprise a discrete labor market segment.[8] Firm j enters the period matched to one employee i. If they remain paired, the firm and worker can produce one unit of output and corresponding revenues $Y_{ij} = P_j$. However, either party can choose instead to seek an outside alternative in the labor market. We model workers' outside options as a competitive wage $\bar{w}(\bar{P})$ that reflects the demand for labor in the broader labor market, which may in turn depend on industry-wide product demand $\bar{P}$. Similarly, if firms access the outside market, they can find another employee at competitive wage $\bar{w}(\bar{P})$ and who will produce value P_j.

However, the labor market may be frictional or otherwise imperfectly competitive. We summarize labor market imperfections in a turnover cost $C_j(P_j)$ that each firm j must incur in order to access competitive alternatives.[9] This cost may either be a fixed cost of recruitment or retraining ($C_j(P_j) = \bar{c}_j$) or a cost that is higher when production demand is higher ($C_j'(P_j) > 0$),

[7] In Appendix B, we derive an equivalent set of parameters from consumer optimization under standard nested preferences.

[8] The analysis does not depend on the strong assumption that product markets and labor markets are segmented in identical ways, though this helps simplify the exposition.

[9] This analysis abstracts from workers' search costs; however, Portuguese workers almost certainly cannot costlessly find alternative employment at a competitive wage. A more realistic formulation of the outside option would reflect both the duration of nonemployment while searching and the value of re-employment as in Jager et al. (2020)—in that case, market-wide demand shocks to firms could improve outside options either by reducing the duration of search or by increasing the re-employment wage. These considerations do not affect our analysis so long as as the replacement cost $C_j(P_j)$ is defined as the cost to the firm of replacing a worker *in excess of the worker's outside option.*

such as recruitment or training that requires forgone production time. Given this cost, firm j is willing to pay a wage greater than $\bar{w}$ in order to retain its incumbent employee i, even though the worker i could only get $\bar{w}_i$ if they were to walk away. This cost therefore governs the firm's outside option in the employment relationship. For any wage w_{ij} in the range $(\bar{w}(\bar{P}), \bar{w}(\bar{P}) + C_j(P_j))$, both the worker and firm enjoy a surplus in excess of their outside options. The firm and worker engage in a Nash bargain to choose a settlement wage w^*_{ij}, where the worker has bargaining weight $\beta \in (0,1)$.[10] In the Nash solution, each party receives their outside option, plus a share (β for the worker and $1-\beta$ for the firm) of the combined surplus value of the match: the cost $C_j(P_j)$, and the wage is given by:

$$w^*_{ij} = \underbrace{\bar{w}(\bar{P})}_{\text{Competitive Outside Option} \equiv \mathit{OutsideOption}_i} + \underbrace{\beta \times C_j(\bar{P} \times p_j)}_{\text{Noncompetitive Rents} \equiv \mathit{Rents}_{ij}} \tag{2}$$

Equation 2 highlights that when competition is imperfect, two firms j and j' in the same market may offer different wages to otherwise identical workers—either because of demand-invariant differences in firms' ability to access the external market $C_j(\cdot) \neq C_{j'}(\cdot)$ or, if $C'_j(P_j) > 0$, because of idiosyncratic differences in demand $p_j \neq p_{j'}$. The following proposition summarizes the impacts of idiosyncratic and common demand shocks:

Proposition 1. *A) The elasticity of wages with respect to idiosyncratic shocks to product demand p_j is given by:*

$$\epsilon^{w,p} \equiv \frac{\partial \ln w^*_{ij}}{\partial \ln p_j} = \underbrace{\frac{\beta \times C(P_j)}{w^*_{ij}}}_{\text{Rent Share of Wage}} \times \underbrace{\frac{d \ln C(P_j)}{d \ln P_j}}_{\text{Sensitivity of Rents to Demand}} \tag{3}$$

B) The elasticity of wages with respect to common demand shocks $\bar{P}$ is given by

$$\epsilon^{w,\bar{P}} \equiv \frac{\partial \ln w^*_{ij}}{\partial \ln \bar{P}} = \underbrace{\frac{\beta \times C(P_j)}{w^*_{ij}} \times \frac{d \ln C(P_j)}{d \ln P_j}}_{\text{Rent-sharing channel}} + \underbrace{\frac{\bar{w}(\bar{P})}{w^*_{ij}} \times \frac{d \ln \bar{w}(\bar{P})}{d \ln \bar{P}}}_{\text{Competitive channel}} \tag{4}$$

A key implication of Proposition 1A is that, while workers' outside options are an important determinant of wages, the passthrough of idiosyncratic firm shocks to workers' wages reflects changes

[10] The surplus for the worker is $w_{ij} - \bar{w}$, and the surplus for the firm is $(P_j - w_{ij}) - (P_j - \bar{w} - C_j(P_j)) = C_j(P_j) - (w^*_{ij} - \bar{w})$. Formally, the Nash wage solves $w^*_{ij} = \text{argmax}_w (w^*_{ij} - \bar{w})^{\beta} (C_j(P_j) - (w^*_{ij} - \bar{w}))^{1-\beta}$.

in *firms'* outside options in the bargaining game. Moreover, even in the presence of a baseline quasi-rent, idiosyncratic shocks to product demand only pass through to wages if the hold-up power of the worker (and thus the firms' outside option) *changes* as a result of the change in demand. Formally, the wage incidence of idiosyncratic shocks in Equation (3) can be written as the product of two terms. The first term is the fraction of the worker's wages that comprised of non-competitive rents due to the friction C, which reflects the level of competition in the labor market. Yet, incidence also depends on a second term—the elasticity governing how much costlier it is for firm j to lose worker i when product demand is higher. Some sources of worker hold-up power that generate quasi-rents may be invariant to the performance of the firm, thus this second term is crucial to keep in mind when comparing pass-through elasticities with cross-sectional measures of firm pay differentials. This highlights an important point: rent-sharing stems from underlying friction *in the labor market*, not the value of firms per se.[11]

Proposition 1 also highlights that while the pass-through of idiosyncratic demand shocks to wages provide direct evidence the existence of non-competitive wage differentials across firms, the incidence of demand shocks that are common to many firms in a market on wages might reflect either firm-specific pay increases or changes in competitive pressure on the worker's outside option in the labor market. Common shocks that affect entire industries (as in Autor et al. (2014) and Abowd and Lemieux (1993)), the entire export sector (as in Verhoogen (2008)), or entire regions (as in Yagan (2019)) could impact wages due to increases in outside options even with *no* labor market imperfection.[12] We also note that although common shocks to product markets affect workers' outside options, they are not pure shocks to workers' outside options as in Schubert et al. (2020) or Caldwell and Danieli (2018), as they might simultaneously increase outside options *and* quasi-rents within the firm.

Although we began with a simplistic model of the labor market, many other standard models of imperfect labor market competition yield wage equations of the same form:

$$w_{ij} = OutsideOption_i(a_i, \bar{P}) + Rents_{ij}(a_i, p_j, \bar{P}) \tag{5}$$

[11]Some papers assume $Rents_{ij} = \frac{\text{Value Added}}{\text{Worker}}$ and estimate a "rent-sharing elasticity" that is similar to the one in Equation (3), but only includes the first term (Card et al., 2018). This formulation stems from canonical collective bargaining models (Brown and Ashenfelter, 1986) in which unions can hold up the entire value added of the firm; in this case the per-worker quasi-rent is the entire per-worker value added of the firm. This is a special case of (3) discussed below.

[12]In this one-shot model, all shocks are permanent. In a model with many periods, transitory shocks to demand—idiosyncratic or common—may not lead to renegotiation if wages are set in longer-term implicit contracts (Beaudry and DiNardo, 1991; Guiso et al., 2005).

where quasi-rents stem from some cost of accessing alternative options in the labor market, and outside options and rents may also differ based on workers' skills and abilities a_i. Accordingly, our analysis applies directly to these models, as does the interpretation of the incidence elasticity:

$$\epsilon^{w,p} \equiv \frac{\partial \ln w_{ij}^*}{\partial \ln p_j} = \underbrace{\frac{Rents_{ij}}{w^*}}_{\text{Rent Share of Wage}} \times \underbrace{\frac{d \ln Rents_{ij}}{d \ln p_j}}_{\text{Sensitivity of Rents to Demand}} \tag{6}$$

Although alternative models in this class differ in the underlying friction (i.e. the cost C_j), the two main principles apply all cases: First, changes in non-competitive premiums are only identified by idiosyncratic demand shocks. And second, wages only respond to idiosyncratic changes in firm performance *if the cost of losing a worker changes as a result.* In Appendix B, we show how different models of imperfect labor markets map directly into this framework and discuss the underlying cost C_j and its sensitivity to demand P_j in each case. However, it is informative to briefly survey several important examples and discuss where they depart from our bargaining model:

Search with multiple-worker firms and multilateral bargaining (Stole and Zwiebel, 1996; Acemoglu and Hawkins, 2014): If individual employees cannot be replaced during a period, they can only hold up their *marginal* product.[13] Firms anticipate this, however, and can adjust recruitment in response to demand shocks accordingly—potentially even to to the point where the *per-worker* remains unchanged after the shock. In the appendix, we show that shocks only pass through to wages when firms face convex short-run recruitment costs.

Bargaining in single worker firms: If single-employee firms need to spend time searching for new hires (Pissarides, 2000) or training hires in firm-specific skills (Becker, 1962; Lazear, 2009), then the departure of an employee may prevent their firm from producing anything for a period of time. In each period, workers get their outside option plus a bargained share of the surplus productivity of a match in excess of the outside option. Shocks to this productivity directly affect the wage ($\frac{d \ln Rents_{ij}}{d \ln p_j} > 0$), but firm adjustment behaviors are not well-defined.

Efficient union bargains (Brown and Ashenfelter, 1986; Abowd and Lemieux, 1993): Unions can hold up the entire output of a firm, and jointly negotiate both employment levels

[13] The departure might additionally affect the bargaining position of remaining coworkers, this is accounted for in Stole and Zwiebel (1996).

and wages.[14] When product demand rises, the union trades off between higher per-worker surpluses and a larger number of workers who can share in the total surplus. Wages only respond to product demand shocks when unions limit employment growth to increase *per-employee* hold-up power—for example, when the output elasticity of labor is decreasing.

Monopsonistic wage-posting with upwards-sloping labor supply to the firm (Manning, 2011; Card et al., 2018): In wage-posting models, firms cannot contract with individual employees, and thus must offer to pay *all* employees more in order to grow employment. The cost of replacing an incumbent worker is governed by the cost of recruiting marginal hires (or retaining additional incumbents) who require increasingly high wages as firms expand employment in response to positive shocks $\frac{d\ln Rents_{ij}}{d\ln p_j} > 0$.

In each of these models, idiosyncratic product demand shocks impact workers' wages over a given time horizon only to the extent that the opportunity cost of replacing them is higher as a result. Accordingly, in all cases, the interpretation of the elasticity with which idiosyncratic product demand shocks pass through to wages is the same as in Equation 6.

This simple framework also highlights the potential threats to identification of $\epsilon^{w,p}$. In a regression framework, we seek to estimate:

$$\ln w_{ij,t} = \alpha + \epsilon^{w,p} \ln p_{j,t} + \epsilon^{w,\bar{P}} \ln \bar{P}_{s(j),t} + \nu_{ij,t} \tag{7}$$

where $w_{ij,t}$ are the period t wages of an employee i who begins at firm j before a shock, $p_{j,t}$ is shock to the idiosyncratic component of firm j's demand at time t, $\bar{P}_{s(j),t}$ is a time-varying component of firm j's demand that is common to all employers in some sector $s(j)$ over the same horizon, and $\nu_{ij,t}$ is a time-varying error term that reflects changes in skills and effort supply of employee i. To identify $\epsilon^{w,p}$, a valid shock to $p_{j,t}$ must satisfy two restrictions. First, a valid shock must be *idiosyncratic*, $E[p_{j,t} \times \bar{P}_{s(j),t}] = 0$. Any demand shifter that systematically affects other employers in the labor market may affect workers' outside options in addition to internal labor demand, confounding any estimate of $\epsilon^{w,p}$. Second, and more fundamentally, a valid shock must be *exogenous* to changes in worker characteristics $E[p_{j,t} \times \nu_{ij,t}] = 0$.[15] If product demand increases are related to increased

[14] The effective cost of replacing one worker is the average cost of attempting to replace all workers.

[15] This highlights one reason it is problematic to simply use variation in observed output to proxy for revenue supply: increases in output may reflect changes in worker characteristics, rather than firm revenue productivity.

ability or effort of employees that would be valued by competing employers as well, such changes would directly raise workers' outside options.[16] More generally, any sorting of firms with different latent trends towards customers with growing demand could lead to confounded results.

Although the pass-through of demand $\epsilon^{w,p}$ cannot be directly estimated because $p_{j,t}$ is not directly observed, one can use a valid instrument for $p_{j,t}$ to estimate a closely related object—the empirical *pass-through elasticity.* We define this elasticity as the log change in wages per log point change in firm output that occurs as a result of the demand shock:

$$\epsilon^{w,Y} \equiv \frac{\partial \ln w_{ij} / \partial \ln p_j}{\partial \ln Y_j / \partial \ln p_j} \leq \epsilon^{w,p} \tag{8}$$

This elasticity can be obtained from a regression

$$\ln w_{ij,t} = \alpha + \epsilon^{w,Y} Y_{j,t} + \nu_{ij,t} \tag{9}$$

where changes in output $Y_{j,t}$ are instrumented by a shifter to p_j that satisfies the exogeneity and idiosyncrasy restrictions. When the production function is linear, the empirical elasticity $\epsilon^{w,Y}$ is equivalent to the structural elasticity $\epsilon^{w,p}$. More generally the bounding inequality in (8) holds whenever the production function is concave: Firms have a strict incentive to increase (or decrease) their employment level in response to positive (or negative) shocks and thus changes in sales overstate changes in underlying demand.[17] In addition, benchmarking wage changes to output changes offers a useful normalization in its own right.

[16] Exogenous changes in effort supply should be distinguished from induced effort supply in wage-posting "efficiency wage" models (Katz, 1986). In these models, firms may raise wages after demand shocks to adjust the efficiency-units that incumbent workers supply. Although "per-efficiency-unit" quasi-rents may not change, *per-worker* quasi-rents do increase, since employees do *not* have the option to earn the higher per-worker wage at other firms. In this sense the resulting cross-firm difference in per-worker wages can be considered the result of a non-competitive quasi-rent.

[17] For example, if $Y_j = P_j f(L_j)$ where f is a concave function of labor, then $\frac{d \ln Yj}{d \ln p_j} = 1 + f'(L) \times \frac{dL}{dp_j} \geq 1$. By contrast, no such bounding result applies to elasticities with respect of changes in output *per-worker*, which can either understate or overstate the change in demand.

3 Background and Data

3.1 Setting and Institutions

In 2008, a housing bust and the resulting financial crisis in the United States triggered large recessions around the world, precipitating a sharp import demand drop in many countries in 2009 and 2010 (Eaton et al., 2016). Although Portugal faced no major domestic housing or financial crisis at the time, Portugal has a small, open economy whose firms were nonetheless highly exposed to global fluctuations in trade demand.[18] Figure 1 highlights how the recession that occurred in Portugal during this episode was marked by a dramatic decline in total exports that mirrored global trends and that remained depressed until 2011. Following this recovery, Portugal experienced a sovereign debt crisis in 2011, which triggered a second, distinct recession episode marked by dramatic increases in unemployment in 2012. To avoid concerns about confounds arising from this latter, domestic crisis, our study focuses on demand variation that occurred during the first recession episode through 2010.[19]

While Portugal provides an ideal setting to identify idiosyncratic demand shocks, key features of labor market institutions should be taken into account when considering the external validity of our results. As in many countries in Europe, most employees in the private sector work on permanent contracts that can only be terminated for just cause, and terminations are subject to court review. While legislative employment protection in Portugal is qualitatively similar to that in France and Italy, Portuguese regulation of individual dismissals is strict even by European standards. Dismissals—even for employees with less than one year tenure—require significant advance notice, can require large severance payments, and are subject to stringent rules about unfair dismissal with the burden of proof falling on the employer (OECD, 2020). Throughout our study period, the OECD consistently ranked Portugal as the country with the strongest restrictions among member nations (Appendix Figure A.1). The result is that layoffs—and job turnover more generally—are rare and individual incumbent workers have significant hold-up power in the firm.[20] There were significant efforts to relax these restrictions beginning in late 2011 as part of a debt relief package from the

[18] While Portugal's exports comprised only 11 percent of its GDP in 2007, exports amounted to over 31 percent of output (World Bank, 2016).

[19] During this initial recession, unemployment grew moderately, at a pace also comparable to other advanced European economies.

[20] Blanchard and Portugal (2001) observe that similarity in U.S. and Portuguese unemployment rates during the 1990s masked important differences in dynamism: transitions into unemployment were lower in Portugal, but unemployment durations were much higher.

European Commission, the European Central Bank, and the International Monetary Fund; these reforms did not go into effect until after 2011 and, in practice, only slightly reduced the restrictiveness of dismissal protections by 2013.

Similar to other countries in continental Europe with strict dismissal protections like France and Italy, Portuguese law allows firms to hire workers on fixed-term contracts in some cases; these employees must either be released or promoted to a permanent contract after a fixed period of time. Prior to the Great Recession, fixed-term contracts accounted for about 15 percent of total private-sector employment. While these contracts offer firms some degree of flexibility, it should be noted that even such temporary contracts provide substantially more protection to employees than "at-will" employment in the United States.[21]

In Portugal, unions engage in industry-wide collective bargaining that established occupation-specific floors for the monthly base wage; they do not negotiate with individual employers as in the United States. Importantly Portugal, like Spain, France, Italy, and other European countries, extends these wage floors to all workers in the occupation regardless of whether or not they are union members(Martins, 2014; Schulten, 2016). Addison et al. (2017) document that although only 11 percent of private-sector employees between 2010 and 2012 were union members, over 90 percent were covered by wage floors established collective bargaining agreements (CBAs). However, in practice, most workers earn wages above the floors set in CBAs. In recent work, Card and Cardoso (2021) match Portuguese employment records to the wage floors specified in CBAs and find that the typical covered worker has wages 20–25% above their CBA-established wage floor, which suggests firms have considerable scope to set wages.[22] These wage floors limit firms' ability to reduce nominal wages, and Appendix Figure A.2 shows that cuts to nominal monthly base wages are rare.[23] In principle, there may be less scope for demand shocks to pass through to workers with wages closer to the CBA floor; accordingly, we examine heterogeneity in pass-through by the size of the worker's 2007 wage "cushion" above the CBA floor.[24]

[21]Instead, these contracts offer firms an opportunity to learn about the quality of a match before committing to a permanent contract; accordingly, they fully account for 50 percent of all hires in Portugal (Portugal and Varejao, 2009).

[22]Card and Cardoso (2021) further find that when CBAs are renegotiated, changes in wage floor levels covary with average productivity growth among covered firms and that increased wage floors compress the wage premiums above the floor, passing through to wages at a rate of about 50 percent.

[23]In our analysis, we find that, given baseline nominal wage growth, constraints on downward nominal wage adjustments were not binding in practice.

[24]We identify CBA wage floors in our sample following Cardoso and Portugal (2005), who show that modal wages within CBA-specified job titles closely match the wage floors specified in the actual text of the corresponding agreement.

3.2 Data and Sample

We study the universe of Portuguese firms that exported in each of the three years preceding the 2008 recession drawing on several administrative datasets covering the years 2005–2013.[25] We measure firms' exposure to global demand and the resulting effects on export flows using annual administrative data that reports firms exports and imports by destination country and six digit product (HS-6) level.[26] To infer demand in destination markets, we use bilateral trade flow data disaggregated at the six-digit product and country pair level from the publicly-available BACI database. We use anonymized firm identifiers to link these export data to profit and loss statements and balance sheets for the universe of firms operating in Portugal contained in the *Informação Empresarial Simplificada* (IES) database, with coverage for the years 2005–2013. We use the same identifiers to link firms to the *Quadros de Pessoal* (QP), a matched employer-employee dataset produced by the Ministry of Employment and based on a census of all private-sector employers during October of each year. Employers with at least one paid employee must report the baseline monthly contract wages, fringe payments, hours worked (regular and overtime), gender, detailed occupation, tenure, age, contract type, and education level of every worker actively employed during the reference month. We define the incumbent cohort at firm j as the full-time employees earning their primary income from firm j in 2007, and study impacts on this cohort. This ensures our results are driven by within-individual wage changes, and not by changes in employee composition within firms. These data sources are described in additional detail in Appendix C.

Our analysis sample consists of all pre-period exporters subject to three restrictions. First, we restrict to firms that both exported and employed more than one worker in 2005, 2006, and 2007, as these firms are most likely to export in future years. Second, we omit firms that only export to Spain (Portugal's only bordering neighbor) or Angola (Portugal's largest colony in the twentieth century), which experienced large decreases and increases in imports (respectively) after 2008. The decision to export to Spain or Angola is likely different than decisions to export to other destinations, and demand changes in Spain and Angola may be associated with other trends in the Portuguese

[25]The sample includes all sectors that export goods; while the majority of firms are manufacturers, the sample also includes firms in resource-extraction industries, wholesale and retail, and select service industries that produce intellectual property (such as book or software publishing). Complete data for other years was not made available to the researchers for this project.

[26]These data are derived from administrative customs records for exports outside the European Union and from mandatory reporting on all intra-EU shipments in excess of a certain threshold. In 2007, the threshold was 110,000 euros.

domestic market.[27] Third, we focus on small- and medium-sized firms that are observably less diversified across export customers and more subject to idiosyncratic demand variation. In the primary sample, we keep firms with average 2005–2007 employment of at least one and no greater than 100 employees. Among the 4,178 small- and medium-sized employers that exported in each of 2005, 2006, and 2007, the median firm exports to just three countries and ships products in only two major (HS2) and four detailed (HS6) product groups; accordingly, we use these firms as our core analysis sample.[28]

Table 1 summarizes the characteristics of the firms in our exporter sample. On average, exports reported in the shipment-level data (subject to reporting thresholds) account for 34 percent of sales reported on the firm's profit/loss statement, though the median is smaller and approximately 20 percent. Table 2 characterizes the sample of individuals employed at those same firms in 2007, and compares them to the broader population of workers in the QP. As firms may exit after 2007, we restrict certain analyses to the balanced panel of firms that never exit the *IES* and *QP* data; Appendix Table A.1 shows that this restriction does not significantly alter the characteristics of workers in the sample. The distribution of earnings, hours, and worker demographics in our samples closely matches the distribution workers in the broader private sector. In the samples and the broader population alike, the standard deviation of log wages is 0.5, though there is almost no variation in workers' normal working hours.[29]

4 Idiosyncratic and Common Demand Shocks During the 2008 Global Recession

To estimate the pass-through of idiosyncratic demand shocks to the wages of the incumbent cohort of workers present at the firm before the recession, we implement a dynamic difference-in-differences design comparing differential changes in incumbent wages across exporters that experienced different idiosyncratic demand shocks during the Great Recession. In this

[27]In all the primary analyses, we control for the pre-period share of exports that go to Spain and to Angola, respectively. The results are not sensitive to the inclusion or exclusion of these controls, in practice.

[28]Table 1 shows that larger firms are observably more diversified across customer markets and are less exposed to idiosyncratic demand risk. Among the approximately 1,000 firms with more than 100 employees in 2007 and exports in 2005, 2006, and 2007, the median firm exports to 10 destinations and exports 11 detailed (HS6) products. Although these 1,000 firms account for a majority of sales and employment among exporters, the 4,178 firms in the analysis firms are most representative of typical private sector employers in Portugal, since non-exporters are typically smaller. Appendix Figure A.3 shows that in 2007, the small and medium-sized firms in the sample are larger than the median firm in the population, but the median employer in our sample of *workers* is similar to the employer size of the median worker in the population.

[29]The statutory minimum monthly full-time wage of 403 euros (2007) is not binding for most workers in the sample.

section, we describe how we measure firm-level exposure to global export demand shocks during the recession and provide evidence that these firm-level shocks are exogenous. We then show how these measured shocks can be decomposed into distinct idiosyncratic and common (market-level) components and validate this decomposition.

4.1 Identification of Exogenous Export Shocks

We begin by measuring exposure to demand fluctuations in foreign markets during the recession using a firm-level shift-share instrument. Establishing overseas trading relationships entails fixed costs that generate stickiness in trading relationships (Startz, 2018). Small and medium-sized Portuguese firms with trading relationships in different overseas markets prior to 2008 were therefore differently exposed to the sharp swings in import demand that took place during the Recession. We measure the exposure of firms j to each six-digit product module market m in each country c based on their prior export histories. In particular, we calculate each firms' total 2005–2007 exports of each product m to each country c as a share of its total 2005-2007 exports $s_{j,mc} = \frac{Exports_{j,mc}^{2005-2007}}{\sum_{\mu \in M, \kappa \in C} Exports_{j,\mu\kappa}^{2005-2007}}$. We then calculate the symmetric growth rate of imports of product module m by country c from all other countries—but excluding imports from Portuguese firms—between the two years before the global recession (2006 and 2007) and the two trough years of the global decline in trade (2009 and 2010), $\Delta_{mc} = \frac{NPI_{mc}^{post} - NPI_{mc}^{pre}}{\frac{1}{2}(NPI_{mc}^{post} + NPI_{mc}^{pre})}$ where NPI denotes total non-Portuguese imports in real U.S. dollars.[30] Importantly, we leave out imports from Portuguese firms in the construction of the shock to avoid mechanical correlation between the shock and the exports of Portuguese firms. The baseline predicted change in export demand for firm j, Δ_j^{tot}, is calculated as the average change in each destination (country by product) market, weighted by the pre-period exposure of firm j to that market:

$$\Delta_j^{tot} = \sum_{m \in M, c \in C} s_{j,mc} \Delta_{mc} \tag{10}$$

This baseline shock is similar to firm-level shocks used in earlier work (Berman et al., 2015; Hummels et al., 2014). However, unlike previous work, our claims of exogeneity are specific to our focus on quasi-experimental variation demand generated by the global 2008 recession. In the econometric framework developed by Borusyak et al. (2018), the exogeneity of a shift-share instrument stems from the quasi-randomness of the underlying demand shocks to destination

[30] Since it is possible that some countries stopped importing some products altogether during this period, we approximate the percentage change using the symmetric growth rate (or "arc-elasticity") concept commonly used in literature on firm dynamics Davis et al. (1996).

markets during the recession Δ_{mc}, not the shift-share form *per se*.[31] Thus, identification comes from the sudden and unexpected nature of that recession and the resulting demand shifts both across product groups and within product groups across importer countries. Indeed, we show in Appendix Figure A.4 that there is no overall association between pre-recession (2003–2006) import growth and recession (2007–2010) import growth at the country-product level.[32]

The key identification assumption is that any association between changes in firm outcomes and their exposure to differently-shocked markets reflects the causal effects of changes to firms' product demand and not systematic selection of firms with different latent productivity growth into export markets with higher demand growth. This is a case of the standard parallel-trends assumption in difference-in-differences designs—it must be the case that all firm outcomes would have evolved in parallel had demand in all destination markets changed identically. It is possible to partially test this assumption among firms with multiple export destinations. Following Khwaja and Mian (2008), who conduct a similar test in the context of relationship lending, we test whether unobserved characteristics that drive exporters' performance systematically differ across choices of export destinations. First, we examine whether exporters with multiple destinations experienced changes in exports to *each destination* according to the change in imports *in that destination*. We regress the change in each firm's exports of each product to each destination on the change in total non-Portuguese imports of the corresponding product at the corresponding destination country from 2006-2007 to 2009-2010. The results, presented in Table 3, show that firms' export growth to each market is strongly predicted by non-Portuguese import growth in that market, both on the intensive and extensive margin. Yet these associations may still reflect firm-level unobservables that generate sorting of faster-growing firms to better-performing markets. We next include firm fixed effects in these relationship-level regressions to account for any such firm-level unobservables. The inclusion of firm fixed effects has a negligible impact on the estimated coefficients; this suggests the association between destination import growth and firm export growth reflects a causal shock and not sorting on unobservables. In Appendix D, we discuss this test in greater detail.

[31] Goldsmith-Pinkham et al. 2020 present an alternative framework in which the exogeneity of shift-share industries stems from random assignment of the exposure weights. They note that their framework is more appropriate in settings where units are differentially exposed to a limited number of common shocks, while the Borusyak et al. (2018) framework is better suited in situations where units are exposed to a large number of idiosyncratic shocks. We view our setting as an example of the latter case—the firms in our sample collectively have positive exposure to over 75,000 country-product (mc) markets across 157 countries and over 4,500 6-digit product categories, while the typical firm only has positive exposure to fewer than 20 mc-level markets.

[32] There is some evidence of mean reversion in the tails of the pre-recession growth distribution in Appendix Figure A.4. To ensure this mean reversion does not drive our results, we examine specifications that directly control for pre-recession growth in export destinations and construct a version of the shock that omits country-product markets in the tails of the 2003–2006 growth distribution (above the 95th percentile and below the 5th percentile). In combination with our pre-trend tests, we find no evidence that mean reversion of tail shocks influences our results.

Figure A.6 shows the baseline correlation of the demand shock Δ_j^{tot} with 2007 attributes of the firm. The shock is uncorrelated with most baseline attributes of sample firms. There is, however, a significant correlation with the firm's pre-period export level. This points to a possible concern particular to this setting: a large share of Portuguese exports go to neighboring Spain, which experienced a particularly adverse recession in the same years. Since the decision to export to Spain is surely different than decisions to export to other destinations given its immediate proximity to Portugal, worse shocks due to exposure to Spain may be reflective of latent characteristics of firms and workers. Similarly, Angola, Portugal's largest former colony, is a top trading partner that experienced particularly strong growth during 2009. Thus, in all the primary analyses, we control for year-specific effects of the total pre-period share of exports that go to Spain and Angola, respectively. Conditioning on exposure to Spain and Angola significantly reduces the association with export intensity. The remaining correlation might confound results if other latent drivers of performance and wage outcomes are correlated with firms' export intensity but not with total sales, productivity, and employee characteristics. To ensure such correlation does not drive the results, we control for year-specific effects of exports (in logs, levels, and as a share of sales) in some specifications. Additionally, we check that results are robust to controls for a wide range of pre-period covariates and industry-level fixed effects.

Although this baseline demand shock is constructed at the *firm-level,* there is no reason to believe the shock is *firm-specific.* The baseline shock Δ_j^{tot} may contain both idiosyncratic shocks to individual firms and market-level demand shocks that also affect other firms that produce similar products to firm j. Specifically, as constructed, the shock Δ_j^{tot} combines two types of variation: differential changes in demand for a given product m across different destination countries, and shifts in global demand for product m common to all destination countries. Idiosyncratic variation arising from cross-country differences in demand for the same product may idiosyncratically affect individual firms without systematically impacting other firms in the labor market. However, the component of demand for product m that is common to *all* countries (and potentially correlated with domestic demand in Portugal) may affect *all* producers of m in Portugal, many of which likely compete for workers in the same labor market segments.

Formally, let $s_{j,m} = \sum_{c \in C} s_{j,mc}$ be the total share of pre-period exports by firm j that are of product m (regardless of the destination), and let Δ_m denote the common decline in demand for product module m across all global markets. Then Δ_j^{tot} can be decomposed as:

$$\Delta_j^{tot} = \underbrace{\sum_m s_{j,m}\Delta_m}_{\text{Common Cross-Product Change}\equiv\Delta_j^{comm}} + \underbrace{\sum_{m,c} s_{j,mc}(\Delta_{mc} - \Delta_m)}_{\text{Differential Dest. Change Within Product}\equiv\Delta_j^{id}} \quad (11)$$

While the first component reflects global changes in product demand that are *common* to all producers, the second component measures how much demand for a given product in j's specific customer country changes in *excess* of the global average. This second term isolates changes in demand that differentially affect firms within a product market based on who their customers are, without shifting demand as a whole. Thus, so long as global shifts in imports of product m capture the part of export demand associated with market-wide demand changes in Portugal, the variation in the latter component (Δ_j^{id}) isolates *idiosyncratic* variation in export demand, which impacts individual employers without systematically shifting market demand.[33]

Accordingly, our approach is to decompose the predictor Δ_j^{tot}, tabulated in (10), into two components: the part reflecting product-level demand changes common to many firms and the residual variation in Δ_j^{tot} orthogonal to that common component. To do this, we attempt to directly measure changes in product-level import demand common to all countries Δ_j^{comm} based on global trade flow data. We proxy for the common demand shock to product module m using the observed change in imports of product module m averaged across all countries. In the baseline analysis, we use a simple unweighted average taken across all countries: $\Delta_m = \frac{1}{\#c}\sum_c \Delta_{mc}$; we also examine robustness to defining Δ_m as the global change in total imports or as the mean weighted by each country's total imports or imports from Portugal at the start of the period. We can then construct the predicted change in export demand based solely on the products exported by the firm $\Delta_j^{comm} = \sum_m s_{j,m}\Delta_m$, where $s_{j,m}$ is the share of each firm's total 2005-2007 exports that are of product module m. Using the baseline shock and this common component, we can then calculate the idiosyncratic component $\Delta_j^{id} = \sum_{m,c} s_{j,mc}(\Delta_{mc} - \Delta_m) = \Delta_j^{tot} - \Delta_j^{comm}$. This construction may generate some mechanical negative within-firm correlation between the two components; we therefore control for the common shock Δ_j^{comm} in our baseline specification when examining the effects of the idiosyncratic component Δ_j^{id}.

[33]One benefit of our approach is that the idiosyncratic component is an *absolute* shock measure. If one knew the exact extent of the labor market, one could isolate an idiosyncratic shock using a labor-market level fixed effect—however, in that case, a positive idiosyncratic shock to a firm in a labor market would necessarily imply negative idiosyncratic shocks to other firms in the same market. By contrast, since we measure the common export demand shocks Δ_m using global trade flow data and not data on realized outcomes of Portuguese firms, any one firm's idiosyncratic component has no mechanical relationship to those of other firms in the same labor market when we partial out the global common components.

Figure A.5 plots the distribution of both the baseline demand predictor Δ_j^{tot} and the idiosyncratic shock component Δ_j^{id}. The idiosyncratic component Δ_j^{id} accounts for 87 percent of the variation in Δ_j^{tot}. The average levels of both Δ_j^{tot} and Δ_j^{id} are not significantly negative and are close to zero. Nonetheless, these are adverse shocks when compared to pre-recession export growth. Figure A.5 also plots the analogous pre-period demand shifter calculated for each firm j holding the exposure weights fixed, this time using the import demand change from 2003 and 2004 to 2006 and 2007 at j's destinations; the average pre-period "shock" is 28 percent. To limit the influence of outliers, we winsorize each component of the shock at the 5^{th} and 95^{th} percentiles in all of our subsequent analyses. Appendix Figure A.6 also shows the baseline correlation of each shock component with firms' 2007 attributes. While the idiosyncratic component of the shock is strongly balanced across total sales, productivity, and employee characteristics, the common component is less well-balanced. Differences in baseline levels of the outcomes and covariates across differently shocked firms do not in themselves violate the assumption that all firms and workers are on parallel trends; nonetheless, we take more caution when considering the effects of the common component.

4.2 Effects on Exports and Sales

The identification assumptions discussed above imply several sharp predictions. First, if exporting relationships are difficult to adjust, then each component of the export demand shock based on pre-recession exposure should predict a change in firms' exports and a corresponding change in sales. Second, if these shocks satisfy the parallel trends assumption, then the baseline shock and each component should be uncorrelated with pre-recession trends in firms' outcomes. Third, although the common component of demand may be correlated with domestic product demand, the idiosyncratic component should have little or no effect on sales beyond the effect on exports.[34] And fourth, the idiosyncratic component of the shock to one firm should not systematically predict changes in sales or wages paid at firms that are close competitors, though the common component may.[35]

[34]Berman et al. (2015) present evidence that, due to internal economies of scale, domestic sales may be impacted to some degree by export shocks as well. In practice, effects on exports may also be attenuated due to reporting thresholds in the export data.

[35]In theory, positive idiosyncratic shocks to other firms in the labor market could result in increased wage pressure or cause other pecuniary externalities onto un-shocked firms. However, our analysis sample of small- and medium-sized exporting firms represents only a small portion of the total workforce (even within manufacturing, see Appendix Table A.3) so the idiosyncratic shocks in our sample are unlikely to impact other firms in the same labor market through such channels.

We test for effects on firms' exports and sales by estimating the following firm-level difference-in-differences regressions:

$$Y_{jt} = \alpha_t + \delta_j + \beta \Delta_j^{\tau} \times Post_t + \sum_{k \neq 2007} \gamma_k X_j^{pre} \times \mathbf{1}\{t = k\} + \nu_{jt} \tag{12}$$

where Y_{jt} is a firm-level outcome, Δ_j^{τ} is a specified shock component where $\tau \in (tot, id, com)$, α_t is a year fixed effect, δ_j is a firm fixed effect, and $X_j \times \mathbf{1}\{t = k\}$ are controls for firms' 2005–2007 exposure to Spain and Angola and their 2007 export activity. In our baseline analysis, we define the pre-recession period as 2006 and 2007 and the post-recession period as 2009–2011. The coefficient β is the effect of a percentage change in *export* demand on the average level of Y_{jt} in the three post-recession years.[36]

To assess the timing by which the shocks impact firms, we also examine dynamic specifications of the form:

$$Y_{jt} = \alpha_t + \delta_j + \sum_{k \neq 2007} \beta_k \Delta_j^{\tau} \times \mathbf{1}\{t = k\} + \sum_{k \neq 2007} \gamma_k X_j^{pre} \times \mathbf{1}\{t = k\} + \nu_{jt} \tag{13}$$

Each coefficient β_k is the year-specific effect of our shock in year k, relative to the omitted year 2007. For specifications examining the idiosyncratic component of demand, we cluster standard errors at the firm level; when studying the baseline and common components we cluster standard errors at the 4-digit industry level. Graphical analysis of the β_k coefficients facilitates tests of differential pre-trends across differently treated firms. If the shock is exogenous, it should not predict differential evolution of outcomes before 2007. Since firms may exit our sample during the observation window, we study both the unbalanced panel of all firms and the balanced panel of only those firms that are present in all data years.

The difference-in-differences estimates presented in Table 4 and the dynamic estimates in Figure 2 show that the shocks work well. In Column 1 of Table 4, we find that a one-percent predicted change in export demand based on firms' pre-recession customers causes a 0.6–0.7 log point change in actual exports among those who continue exporting. Changes in exports are predicted well by each component individually and the results are similar in both the balanced panel and the full sample.

The results in Table 4 indicate that firms are partially able to mitigate negative idiosyncratic

[36] The corresponding percentage change in firms' total sales demand further depends on the share of firms' sales that are exports.

demand shocks by finding new customers in different countries, though negative common product-level shocks are more difficult to mitigate.

To illustrate how export changes pass through to total sales in each year, Figure 2 presents effects on exports and total sales in common currency units normalized by 2005–2007 average sales. Panel A shows that the idiosyncratic shock effect on exports passes through to total sales approximately euro-for-euro, which indicates that the idiosyncratic component is uncorrelated with demand for domestic sales. The effect grows through 2009 and then plateaus at approximately 15 percent of firms' pre-period sales, closely matching the log sales effect in Column 2 of Table 4. By contrast, Panel B shows that for every euro change in export sales caused by the common component total sales adjust by three times as much, suggesting that the common component of export demand is strongly correlated with domestic demand shocks. Importantly, the shocks (constructed using 2006–2007 to 2009–2010 changes in imports abroad) do not predict differential sales or export growth *prior to* 2007, consistent with quasi-random assignment.

While the two shock components vary in their idiosyncrasy, they differ in other ways in practice as well. Although the shocks are only defined through 2010, the effects of both components are persistent—though, while the effect of the idiosyncratic component on sales fully dissipates by 2013, the effect of the common shock is far more persistent and never dissipates in the observation window. While this difference in effect persistence might reflect differences in the underlying shocks, it might also reflect firms ability to respond to each kind of shock—the results in Columns 5 and 6 of Table 4 show that while firms are able to mitigate negative idiosyncratic demand shocks by finding new export destinations, they rarely export new products after negative common shocks. Moreover, Column 7 of Table 4 reveals another important difference across the two components: the common component has a nontrivial effect on whether firms shut down entirely and exit the sample, while the idiosyncratic component has no such effect on firm survival. This effect on firm exit should be kept in mind when examining common component effects in subsamples that condition on firm survival below, as selective exit of firms that would otherwise have had higher wage pass-through might attenuate estimates.

Table 5 displays estimates of effects on broader measures of firm activity and performance in the full sample. Columns 1 and 2 show that both shock components impact firms' value added in close proportion to sales.[37] We find that both payroll and employment adjust in Columns 3 and 4, but in

[37]Value added is defined as the total output of labor and capital factors in the firm during the year which, by an accounting identity, equals the value of sales less the cost of intermediates and inventory adjustments. Since our data on factor payments are more consistently reported than our data on inventories, we follow Card et al. (2018) and define value added based on the latter concept, so that VA = total labor costs + gross earnings before netting out interest, taxes, depreciation, and amortizations. This also ensures that the level of value added is not mechanically related to the level of sales.

smaller proportion to the change in total output, implying an increase in average labor productivity. While the idiosyncratic component effect on payroll and employment is not significantly different than zero in the full sample, the effects are significant when we examine the balanced panel of firms that never exit in Appendix Table E.8; in all cases, the effect on log payrolls is 1-2 percentage points larger than the effect on employment implying an increase in average pay. We examine dynamic versions of these analyses in Appendix Figure A.7 and find that while effects on output outstrip the effects on payroll and employment in the first years after the onset of the recession, the employment and payroll effects converge to the output effects in the longer term after 2011. In all analyses, we find no evidence of differential pre-period trends.

If the idiosyncratic component of the shock Δ_j^{id} is truly firm-specific and pecuniary externalities from sample firm responses are negligible, it should not systematically predict changes in sales or payroll at close competitor rivals in the product market. Conversely, if the common component of demand Δ_j^{comm} actually affects all firms in a product market, it should have discernible impacts on neighboring firms making the same goods—which in turn likely compete for the same workers. As a direct test, we study the effect of shocks to each firm j on its close competitors in the analysis sample, defined as other firms in the same four-digit industry and municipality. Specifically, we estimate the difference-in-differences specification in Equation 12 replacing the firm j's own shock and corresponding components with the leave-one-out mean averages taken over all other firms $-j$ in the same industry-geography cell. We present the results of these "placebo" tests for effects on un-shocked firms beneath the own-firm effects in Table 5.

The results presented in Table 5 highlight the importance of isolating the idiosyncratic component from the broader firm-level demand shock. In particular, we find in Panel A that prior to implementing any decompositions, the baseline demand shock significantly predicts changes in sales and production both at firms *and* their close competitors. Examining the effects of each component separately in Panels B and C, we find these effects on broader markets are driven entirely by the common component of demand Δ_j^{comm}, which strongly predict changes in sales and value added at similar competitor firms. By contrast, the idiosyncratic component appears to be firm-specific—the idiosyncratic component of one firm's shock does not predict changes in competitors' performance. We conduct similar tests for effects on employee wage growth at rival firms and find similar results, which we discuss. These findings support the validity of the research design.

5 Worker-Level Incidence

In this section, we examine the incidence of firm-specific and market-wide demand shocks on the employment and earnings of the *individuals* who were employed by shocked firms in 2007. Studying effects on a fixed cohort allows us to isolate changes in employee's wage growth from any endogenous changes in the composition of firms' workforces.

5.1 Effects on Employer Behavior

Before studying the evolution of wages within spells, we first examine whether the effects on firm-level employment documented in Table 5 reflect effects on employee turnover. To this end, we decompose changes in employment between 2007 and each prior and subsequent year as follows:

$$\Delta Emp_{j,t,2007} = \underbrace{\sum_{\tau=2007}^{t} \text{Accumulated Retained Hires}_{j,\tau}}_{Hires_{j,t}} - \underbrace{\sum_{\tau=2007}^{t} \text{Incumbent Separations}_{j,\tau}}_{Seps_{jt}}$$

This identity allows us to decompose percentage changes in employment (relative to 2007 levels) into a part due to incumbent retention (or layoffs) and subsequent (or prior) hiring $\frac{\Delta Emp_{j,t}}{Emp_j^0} = \frac{Hire_{t,j}}{Emp_j^0} + \frac{Retentions_{j,t}}{Emp_j^0} - 1$. We plot the dynamic effects of the idiosyncratic and market-level components of the demand shock on each margin in Figure 3 for the full sample and in Appendix Figure E.2 for the balanced panel of never-exiter firms. In both samples, we find that the observed adjustment in employment in response to the idiosyncratic demand shock is wholly attributable to differences in hiring behavior—there is a tight zero effect on the departure rate of incumbent employees. This finding is consistent with anecdotal evidence that firing costs are prohibitively large in Portugal. By contrast, the common component of demand has large effects on both employee retention and hiring in the full sample. The common component impacts employee separations primarily through firm exit—the point estimates in Appendix Table A.3 imply that firm exit accounts for 77% of the retention effect and Appendix Figure E.2 shows little effect on retention among firms that never exit. Notably, our employee-level results below imply that workers who separate from their baseline employer due to a shock rarely appear again at another job in subsequent years. If firms' decisions to exit the sample after market-wide shocks are based on potential outcomes, then selection biases may impact wage effects estimates conditioning on continued employment at surviving firms.

5.2 Effects on Employees' Wages

As a baseline test of how each type of shock impacts wage growth, we examine reduced-form impacts on the workers i whose main job in 2007 was at sample firms j using an individual-level version of the specification in Equation 12

$$w_{it} = \alpha_t + \delta_i + \beta \Delta^{\tau}_{j(i)} \times Post_t + \sum_{k \neq 2007} \gamma_k X^{pre}_{j(i)} \times \mathbf{1}\{t = k\} + \nu_{it} \ , \ t \in \{Pre, Post\} \tag{14}$$

In this specification, the sample is comprised of workers whose main job was at one of the sample firms in 2007, denoted $j(i)$. The primary specification includes the same firm-level controls as before. In our baseline analysis, we estimate effects on workers' contracted monthly wages *within* spells at their initial employer—the case in which pass-through is most well-defined—using the full sample of firms.[38] Nonetheless, we also examine impacts on hourly wages and earnings measures that allow for job mobility and zero earnings. To test whether the parallel-trends assumption holds at the worker-level, we estimate dynamic specifications analogous to Equation 13 that enable visual inspection of pre-period trends. In addition, to verify the idiosyncrasy and commonality of each component, we also examine the effect of each firm j's shocks on workers employed by its close competitors in the analysis sample, defined again as other firms in the same four-digit industry and municipality.[39] We also examine specifications restricting to firms that never exit in Appendix E and find highly similar results in both samples.

The estimates in Table 6 and corresponding event-study plots in Figure 4 indicate that as sales demand rises, employees are able to secure larger pay increases, even without a change in their labor market outside options. The idiosyncratic component has clear effects on the wages of incumbent employees of shocked firms but no effect on the "placebo" group of employees of rival firms that are close competitors in the same four-digit industry and municipality.[40] The dynamic estimates in Figure 4 indicate that, although workers' earnings evolved in parallel prior to 2007 regardless of their

[38]Since wages are only observed in a single reference month, we exclude overtime and fringe payments that may vary throughout the year. We begin with the monthly salary as this is the object most likely to remain constant across months, though we show robustness to studying hourly wages and total salaries in the following subsection. Workers who exit the data in a given year are treated as missing in that year, though workers whose initial firm exits the data, but who themselves find employment at other firms in subsequent years are included.

[39]For each worker i employed by j in 2007, we assign them the same leave-one-out average shock used in the firm-level analysis above.

[40]While it is unlikely that labor markets are fully segmented across four-digit industries (see, for example, Schubert et al. (2020)), we conjecture that firms in the same place and four-digit industry almost certainly compete for workers in the same labor market segments, making such firms a useful reference group for the placebo test. We have examined flow-based labor market definitions, but in practice job-to-job transition rates among permanent-contract employees are very low and these definitions are noisy, resulting in an under-powered placebo test.

own shock (or the shock of rival firms), their wages change by approximately 0.02 log points per each percent change in idiosyncratic export demand (associated with a log sales effect of 0.14).[41] This effect is similar whether examining the monthly full-time contract wage specified in the employment contract or the effective hourly wage calculated using the reported hours worked in the reference month—implying a minimal change in hours worked—and we find similar effects when restricting to firms that never exit the sample (Appendix E). The estimated wage impacts are slightly larger when we include observations at subsequent jobs in Columns 3 and 4, suggesting that employees do not escape wage changes by switching firms.

These results, combined with the finding of no effect on hours, imply a retention elasticity of *zero*—that is, labor is supplied completely inelastically to the firm by incumbent employees. Accordingly, we see bargaining models as providing a better description of the determination of wages for continuing employees in Portugal than wage-posting. Wage-posting models posit that firms raise wages in order to implement higher employment levels and therefore are less well-suited to settings where employment is highly rigid. The bargaining view is more appropriate to our setting where labor market institutions make it difficult for firms to either dismiss employees or find flexible replacement labor. In the lens of the model in Section 2, higher output demand (and hence labor demand) increases workers' hold-up power in the firm, which they can leverage to bargain for higher wages.

In contrast, the common component of demand impacts the wages of employees both at shocked firms and at close competitors. This highlights the importance of isolating the idiosyncratic variation in demand from the market-level variation—the baseline "firm-level" shock incorporates this market-level variation and is therefore associated with *market-wide* wage growth as well. Interestingly, we find that the within-spell wage effects of the common demand shock on workers' pay are not larger than the effects of the firm-specific component. This, however, reflects the qualitative differences in how the common component affects firms noted above: unlike the idiosyncratic shock, our common shock causes firms to exit and their employees to separate and even exit employment entirely. If the employees who lose jobs due to the common shock are those that otherwise would have experienced the largest wage declines, selective worker attrition might attenuate estimates of wage effects that

[41] Any downward nominal wage rigidity due to either explicit wage floors or other factors during the recession would attenuate our results. However, downward nominal wage rigidity constraints are likely not binding for most workers in our analysis. Table 6 reports that nominal wages of workers in the sample that remained with their initial employers grew by 10 percent on average between the "pre" and "post" years. Appendix Figure A.8 shows that even the most negative shocks in our sample would not have pushed wage growth all the way down to zero—the workers at firms in both the top and bottom quartile of the baseline shock distribution experienced significant nominal wage increases over this period. We also display the residualized binned scatter plot corresponding to our main difference-in-differences specification in Appendix Figure A.9 and find that the idiosyncratic component's effect is linear throughout its support, further suggesting downward rigidities do not bind.

condition on employment. Indeed, Columns 5-8 of Table 6 show that given large extensive-margin effects on employment, the large effects on the common shock are only visible using outcomes that admit zeros such as the inverse hyperbolic sine or the arc-percent change in base wages.[42] The large extensive-margin effects of the common shock limit our ability to directly compare the magnitude of the pass-through of the common shock to that of the idiosyncratic shock.

Are the wage gains for continuing employees shared by new hires? Since firms can respond to shocks by adjusting the *types* of workers they hire, it is difficult to distinguish differences in wages offered to fixed types from changes in the composition of hires. We cannot use individual fixed effects to control for changes in worker characteristics as in Equation 14 since it is impossible to determine which workers in the population were on the margin of being hired by firms in the sample. Nonetheless, we use the firm-level specification in Equation 12 to examine effects on the average wages paid by firms to new employees in their first year of employment—noting such effects may reflect changes in the composition of hires. Results are presented separately for the wages of new fixed-term-contract hires and for those of new permanent-contract hires in Appendix Table A.4. Though the estimates are imprecise, reflecting the small number of hires made after the recession, we find evidence that the wages of new permanent contract employees—who both gain significant bargaining power upon starting due to statutory firing restrictions and are typically poached from other firms—increase more in response to positive idiosyncratic shocks, and more so in response to common demand shocks. We find no evidence of effects on the starting wages of fixed-term hires.

5.3 Pass-Through Estimates

To consistently benchmark the change in wages to the magnitude of the effect on firms' output demand, we estimate the passthrough elasticity $\epsilon^{w,Y}$, introduced in Equation (8): this elasticity measures log change in wages per log point change in output induced by the shock. We estimate $\epsilon^{w,Y}$ using a difference-in-differences IV approach, where the first stage regresses the shocks to workers' employers on their sales growth, and the second stage regresses their wages on the variation in their employers' sales $\tilde{Y}_j$ predicted by the shock $\Delta^{\tau}_{j(i)}$:

$$w_{it} = \alpha_t + \delta_i + \epsilon^{w,Y} \tilde{Y}_{j(i)} \times Post_t + \sum_{k \neq 2007} \gamma_k X^{pre}_{j(i)} \times \mathbf{1}\{t = k\} + \nu_{it} \ , \ t \in \{Pre, Post\} \tag{15}$$

[42] We find slightly larger effects of the common shock on wages when we restrict the analysis to workers at firms that never exit (Appendix Table E.9), though we note this sample conditions on the firm survival outcome.

The two-stage-least squares IV estimator effectively rescales the reduced-form effect on wages by the magnitude of the "first-stage" effect on output. We implement the IV regressions at the worker level, clustering standard errors at the firm or industry level corresponding to the idiosyncrasy of the shock component.

Table 7 presents our benchmark estimates of passthrough of changes in log output (either sales or value added) to log wages of the individuals employed by sample firms in 2007, conditional on continued employment at the same firm. Firm-specific shocks have a substantial impact on the wages set by employers—passing through with an elasticity 0.14. While our baseline analysis uses 2009–2011 as the post period, we further explore how pass-through evolves over time by estimating Equation 15 using each individual year 2008–2013 as the post year.

Consistent with the reduced-form results, we find lower passthrough of the common component of the shock to wages within job spells. However, these intensive-margin effects mask the full impact of the common component, which has a large extensive-margin effect on employment. When examining earnings outcomes that admit zeros in Columns 3 and 4 of Table 7 we find that the common shock has larger pass-through to worker earnings than the idiosyncratic shock—and even larger pass-through when we use firm sales measures that admit zeros (incorporating years where firms close and workers exit employment, see Appendix E). These findings suggest that despite the smaller within-spell elasticities, the common component has greater total incidence on workers than the idiosyncratic component.

Our main passthrough elasticity for firm-specific shocks of 0.134 is significantly larger than those found in earlier work surveyed by Manning (2011) and Card et al. (2018) that estimates similar elasticities using non-experimental variation in firms' output or productivity. These early studies report typical OLS elasticities of 0.06 or less.[43] To better understand the source of the discrepancy between our results and earlier findings, we estimate the second stage equation (15) in our sample by simple OLS. Table 7 reports OLS estimates that are an order of magnitude smaller than the IV estimates: Across specifications, we consistently find an OLS elasticity of about 0.02. This implies the discrepancy between our work and earlier OLS estimates is due to our quasi-experimental research design, rather than factors specific to our setting. Despite concerns that OLS estimates might introduce upwards-pushing simultaneity bias due to unobserved changes in ability or market-level shocks, our findings imply the OLS estimates are significantly biased towards zero.

[43]For example, in Italian data Card et al. (2013a) find a 0.04 longitudinal elasticity of wages to output after adjusting for changes in outside options of workers. Studies by Cardoso and Portela (2009) and Guiso et al. (2005) find that wages appear to be invariant to transitory shocks but sensitive to permanent shocks to firm income, though only with an elasticity of 0.06. In the same QP data we study, Card et al. (2018) found a OLS regressions of wage changes on firm performance changes yield small elasticities of (0.06 or less) even over five year horizons.

This difference between our OLS and IV estimates is largely consistent with prior quasi-experimental studies. Abowd and Lemieux (1993) study industry-wide shifts in product demand and, though they find no wage effect in the OLS, they find large effects in IV analysis using exogenous variation from trade shocks.[44] The magnitude of our IV pass-through estimates are also of comparable magnitude to effects found in quasi-experimental studies of rent-sharing after firm innovations (Van Reenen, 1996) or unexpected approval of patent applications (Kline et al., 2019). While these findings may suggest OLS is confounded by shocks to labor supply, we think the most plausible explanation is that short-run fluctuations in output and in average productivity are poor measures of underlying product market conditions, leading to substantial attenuation bias in OLS estimates, in studies of both firm-level and industry-level variation.[45] This explanation is consistent with findings in Card et al. (2018) that the relationship between wage changes and output changes roughly doubles when one simply instruments for measurement error in output growth over a given horizon using output growth over a longer horizon.[46] These findings imply that OLS is significantly attenuated: thus, significantly larger IV estimates are plausible.

Our results are robust across alternative regression specifications. The findings presented in Table 8 show that results do not change significantly if we omit our baseline controls or include additional controls such as exposure to specific destination countries, 2007 firm performance, pre-period worker attributes, or predicted export demand growth prior to 2007. The point estimates are stable when we control for product market-level trends by including fixed effects for firms' main four-digit export product interacted with year dummies. The results are similar when large firms are included in the sample. We examine pass-through over alternate time horizons in Appendix Table A.5 and find that the pass-through rate increases slightly in each year after 2008, reflecting the persistence of the wage effects even as the impact on sales wanes. In additional robustness checks presented in Appendix E, we find that our results are robust to alternative methods of measuring the common component of demand and to omitting variation from markets with pre-Recession demand swings. We also find that our results are similar when we use firms' logged value added as

[44] Card et al. (2018) show that their estimates imply a pass-through elasticity of wages with respect to output per worker of 22 percent.

[45] For example, planned firm expansions, increased performance of other factors of production, and foreseen variation in revenues for long-run projects could all create large variations in firm performance that do not discretely increase the demand for incumbent labor at the same time.

[46] Card et al. (2013a) find a comparable increase in point estimates when instrumenting firms' own value-added with industry-wide measures of average revenue per worker.

the first stage output variable and when measuring compensation inclusive of fringe benefits and bonuses—though, fringe benefits and bonuses in the QP reference month are a noisier proxy of annual compensation than contract wages, which results in larger standard errors on the estimates.

6 Heterogeneity in Pass-Through: The Role of Relationship-Specific Surplus

6.1 Firm and Industry Heterogeneity

In our framework, the incidence of idiosyncratic demand shocks on incumbent employees wages we observe implies that employers face significant turnover costs and that these costs—and employees threat points—rise and fall with demand growth. To the extent that turnover costs and employee holdup power differ across industry labor markets, theory suggests idiosyncratic firm-level demand should only pass through to wages in cases where frictions are large. Although the underlying holdup power of employees (the turnover cost C_j in our framework) is not directly observable, most models featuring employee turnover costs imply that higher costs lead to lower separation rates and longer spells.[47] Likewise, although Portuguese institutions feature high firing costs that apply to all employers, these constraints on downward adjustment are more likely to bind when the baseline attrition rate is lower.[48]

Accordingly, we test whether the pass-through elasticity differs in sectors with shorter typical tenure lengths and higher separation rates of permanent contract workers, who cannot be temporarily laid off.[49] To characterize the frictions in firms' narrow five-digit industries, we measure the pre-period separation rates of permanent contract workers, as well as their typical job tenures.[50] We study heterogeneity in pass-through by dividing the analysis sample into two

[47] For example, when jobs require investment in firm-specific human capital that is less useful on the outside market (Becker, 1962; Jovanovic, 1979a; Lazear, 2009), workers and firms have an *ex post* incentive to maintain the employment relationship. Similarly, when search is frictional and there is substantial heterogeneity in *ex ante* unobservable match quality between firms and workers(Jovanovic, 1979b; Mortensen and Pissarides, 1994), both firm and worker derive substantial option value from maintaining a relationship once a good match is made.

[48] Employment protections are likely a significant source of employee hold-up power in Portugal. However, there is essentially no variation in regulation across firms, and unions set occupation-wide wages rather than negotiating with firms. However, even if protections cover all sectors, variation in natural rates of attrition would lead these same regulations to bind differentially across sectors.

[49] We characterize industries based on permanent contract employee turnover because fixed-term contracts in Portugal are often used a probationary arrangements to assess new matches. Sectors with higher turnover costs of permanent employees may have higher option value of churning through fixed-term contracts before committing to a long-run arrangement (Portugal and Varejao, 2009).

[50] We calculate the typical tenure as the employment-weighted average of firms' median permanent employee tenure in 2003–2007, and the turnover rate as the ratio of total separations to the total number of permanent workers averaged

equally sized subsamples with above-sample-median and below-sample-median levels of a specified industry turnover metric. First, we estimate an interacted version of the reduced-form event study specification used above:

$$w_{it} = \begin{array}{c} \alpha_t + \delta_i + \sum_{k\neq 2007} \beta^H_k \Delta^\tau_{j(i)} \times Hi_{j(i)} \times \mathbf{1}\{t=k\} + \sum_{k\neq 2007} \beta^L_k \Delta^\tau_{j(i)} \times Lo_{j(i)} \times \mathbf{1}\{t=k\} \\ + \sum_{k\neq 2007} \gamma^H_k Hi_{j(i)} \times \mathbf{1}\{t=k\} + \sum_{k\neq 2007} \gamma_k X^{pre}_{j(i)} \times \mathbf{1}\{t=k\} + \nu_{it} \end{array} \tag{16}$$

where $Hi_{j(i)}$ denotes an indicator of whether firm j employing worker i has a level of the industry turnover cost proxy above the median firm in the sample, and $Lo_{j(i)} = 1 - Hi_{j(i)}$ is the complementary indicator. Reduced-form effect heterogeneity may reflect both heterogeneity in pass-through and heterogeneity in the first stage, therefore, to isolate heterogeneity in pass-through rates, we estimate an interacted version of our primary difference-in-differences IV specification:

$$w_{it} = \begin{array}{c} \alpha_t + \delta_i + \epsilon^{w,Y}_H \tilde{Y}_{j(i)} \times Post_t \times Hi_{j(i)} + \epsilon^{w,Y}_L \tilde{Y}_{j(i)} \times Post_t \times Lo_{j(i)} \\ + \sum_{k\neq 2007} \gamma^H_k Hi_{j(i)} \times \mathbf{1}\{t=k\} + \sum_{k\neq 2007} \gamma_k X^{pre}_j \times \mathbf{1}\{t=k\} + \nu_{it} \end{array} \quad t \in \{Pre, Post\} \tag{17}$$

IV estimation requires separate instruments for each interaction with the endogenous output level Y_j; this accounts for the possibility that the first-stage effects of the shock differ across subsamples. We instrument for $Y_{j(i)} \times Post_t \times Hi_{j(i)}$ using $\Delta^\tau_{j(i)} \times Post_t \times Hi_{j(i)}$ and likewise for the interaction with $Lo_{j(i)}$.

The reduced form results in Figure 5 are consistent with turnover frictions playing a key role in determining the wage incidence of product-market shocks. There is a clear contrast in how each component of demand impacts employee wage growth in more and less frictional labor markets. We find that firm-specific shocks have clear effects on wages in sectors with lower employee turnover, but almost no effect in sectors with higher turnover rates. In contrast, we find that market-wide shocks have significant wage impacts in sectors with higher turnover, suggesting within-spell wage pressure from labor market competition is larger in these more-fluid settings.

The estimated passthrough elasticities in Table 9 reflect the same reduced-form facts. Not only do wages respond more to shocks in sectors with lower separations, they also respond at a higher

across 2003–2007. We calculate industry means excluding sample firms from the calculation. To calculate the degrees of fluidity most likely to characterize the sample firms, we calculate these averages for all firms with 100 employees or fewer within the industry. These industry-level indicators are highly predictive of the corresponding tenure lengths and separation rates at the sample firms in the same industry.

rate *per realized percent change in sales*. In sectors with lower turnover frictions, indicated by higher turnover rates of permanent contract employees, we find zero pass-through of idiosyncratic demand shocks to wages. By contrast, we estimate that the rate of pass-through to monthly base pay is 0.26 in more frictional sectors with low turnover rates. We find qualitatively similar but smaller differences across industries using typical tenure lengths as a measure of turnover frictions. Meanwhile, the common component of demand has larger within-spell pass-through to wages in less frictional sectors with higher turnover rates. However, this latter result does not take into account the extensive-margin effects of the common component. We show in Appendix Table A.6 that the story is more nuanced when examining effects on earning measures that admit zeros—here, the common shock has larger effects on high-friction sectors as workers lose jobs in firm closures and do not find re-employment. Together, these findings are consistent with the theoretical prediction that firm-level demand is more important for wage determination when markets are less fluid. Nonetheless, we acknowledge that these differences across industries are not randomly assigned and may reflect other differences across firms and sectors.[51]

We also test whether pass-through is higher at firms with higher pay premiums for permanent contract workers based on two-way fixed effects regressions as in Abowd et al. (1999). If higher AKM firm effects indicate firms with higher levels of rent-sharing, one should expect wages to be more sensitive to firm-level demand in these firms. Accordingly, we estimate an AKM model during the pre-recession period (2003–2007) identified by job-to-job moves of permanent contract employees, and we study pass-through heterogeneity using the specification in (17).[52] The results in Table 9 provide evidence that the wages of workers in firms with higher AKM fixed effects are more sensitive to firm-specific demand shifts but are more insulated from outside competition. We note that there is a strong relationship between AKM pay premiums and the employee-turnover measures used above: Appendix Figure A.10 shows that both firm-level and industry-level turnover

[51]In our sample, low-turnover sectors tend to be more mature with lower employment growth, larger firms, and higher employment concentration—all potentially related to drivers of labor market frictions—but feature similar average firm productivity and worker wage levels to high-turnover sectors. In Appendix Table A.7, we show that idiosyncratic shocks pass through to wages more in industries with more greater concentration of employment in a small number of firms. However, employment concentration is not a confound so much as a potential *source* of labor market friction—recent research highlights how increased concentration might lower the degree of competition in the labor market and increase the pass-through of idiosyncratic firm shocks as a result (Berger et al., 2022; Jarosch et al., 2019). Nonetheless, when we examine heterogeneity by industry separation rates *residualized* on each of these other industry characteristics (including employment concentration) in Appendix Table A.7 we continue to find larger effects in sectors with lower residual turnover, though the differences are somewhat muted.

[52]We estimate wage equations of the form $w_{ijt} = \alpha_i + \phi_j + \beta X_{it} + \delta_t + \epsilon_{ijt}$, as in Abowd et al. (1999) and Card et al. (2018), on the largest connected set of firms and permanent-contract workers for the period 2003–2007, where X_j is a cubic in age interacted with dummies for years of education completed, and the firm fixed effects of interest (ϕ_j) are estimated off of permanent-contract workers who move jobs. We focus on permanent-contract employees because these workers cannot be selectively laid off, so transitions are more likely to be exogenous.

rates are highly correlated with AKM firm pay premiums in our sample. We additionally examine heterogeneity by workers' 2007 wage cushions above CBA wage floors in Appendix Table A.6 to test whether the "bite" of such floors limit the scope for firm-specific pass-through as discussed in Section 3, but find no evidence that pass-through of either shock component is impacted by wage floor bite.

6.2 Worker Heterogeneity

Employee hold-up power may vary across workers within firms as well. Workers with more experience or specific skills may be more difficult to replace quickly, as might workers with stronger institutional protections. Previous work has suggested rent-sharing may differ across gender groups (Card et al., 2016) or across income subgroups (Kline et al., 2019). Accordingly, we estimate pass-through elasticities separately for members of different subgroups within our sample of 2007 incumbent employees. Results are displayed in Table 10. In practice, the incidence on workers with permanent contracts and fixed-term contracts is similar, suggesting even fixed-term contract employees in Portugal have a high degree of attachment to their employers.[53] Among workers with permanent contract, we find that firm-specific shocks affect the wages of higher-paid and lower-paid workers within firms similarly.[54] In contrast to Card et al. (2016), we find that the wage growth of women is slightly more sensitive to firm-specific demand than that of men, though we cannot reject equal pass-through rates.

We do, however, find large differences between blue-collar and white-collar workers and between workers with less than a high-school-level education (a majority of adult workers in Portugal) and those with at least a high school degree. Specifically, we find large pass-through of demand shocks to wages of less-educated and blue-collar workers and little to no incidence on the wages of higher-educated and white-collar workers. Our estimates of the incidence of demand shocks in Portugal stand in contrast to the findings in (Kline et al., 2019), who document that patent grant shocks in the US have larger incidence on high-end workers than on low-end workers. This likely reflects key differences in the institutional settings and the nature of the shocks—export demand shocks increase the need for production workers, who have significant hold-up power as a result of the strict

[53]Importantly, fixed-term contract employees are not employed "at-will", and employer face costs of terminating workers before the contracted date. As these contracts typically last two to three years and are renewable, fixed-term contract workers in Portugal can have substantial attachment to their employers, and have stronger institutional protections than most employees in the United States.

[54]We find that among high-wage and low-wage workers alike, shocks have zero effect on the probability that individuals exit the employment data. Combined with our finding that high-wage and low-wage workers bear similar incidence of the shocks, this finding suggests selective attrition of individuals is minimal.

labor market regulations in Portugal (in contrast to the US setting). We further split blue-collar workers into semi-skilled occupations (craftsmen, foremen) and less-skilled occupations (operators, laborers), and find that only the more-skilled blue-collar workers—who likely have more intimate knowledge of the production process and have more hold-up power in the firm—experience larger pass-through rates.

7 Discussion

Our findings establish a causal channel by which exogenous differences in firm performance can lead to cross-firm wage differentials, even among otherwise identical workers. In this sense, they directly relate to previous work following Abowd et al. (1999) that decomposes wages into firm fixed effects, worker fixed effects, and match-specific results, and compares heterogeneity in firm performance to firm wage premiums (Card et al., 2013b; Barth et al., 2016; Alvarez et al., 2018; Card et al., 2018; Lamadon et al., 2019). Although prior OLS rent-sharing elasticities have been small, the *cross-sectional* relationship between log labor productivity and AKM firm log wage premiums is generally larger: Studying the same employer-employee data in Portugal during the period 2005–2009, Card et al. (2018) found that the coefficient in a regression of AKM firm wage effects on log value added per worker across firms was approximately 0.13. Unlike earlier OLS estimates, our IV estimates—which establish the causal relationship between output and wages due to idiosyncratic shocks—match the cross-sectional relationship very closely.[55]

Although our estimates can explain the cross-sectional relationship between firm performance and AKM pay premiums, they are not large enough to explain the full wage variance attributable to AKM firm effects. In our analysis sample, the 2007 variance of log wages was 0.202, and the variance due to AKM firm effects was 0.036 (17.8 percent of total).[56] Even under the extreme assumption that *all* 2007 cross-firm differences in value added were exogenous and pass through to wages with an elasticity of 0.15, the resulting dispersion in wages would only generate log wage variance of 0.013 (6.4 percent) in total—less than one-third the variance in firm AKM effects. There are, however,

[55]We do not use per-worker measures of output to proxy for revenue productivity *changes* in response to well-defined shocks, since per-worker changes reflect endogenous employment responses. Consistent with our reduced-form results, we generally find that elasticities with respect to output per worker are generally larger than our baseline estimates—however, we note that the bounding result above only holds for elasticities with respect to total output. Per-worker measures are arguably better for cross-sectional comparisons, as these measures reflect static differences in outputs given a fixed number of inputs.

[56]For this exercise, we estimate the same AKM models as in footnote 52, but we estimate the models over all workers in order to study the full variance in wages. In all variance calculations, we restrict to the firms and workers in our sample in the connected set used for AKM estimation.

important caveats to such a calculation. The relationship between *permanent* productivity/demand differentials and long-run wages may be different than medium-run elasticity we estimate with respect to potentially transient demand shocks (though, *cross-sectional* variation in wages may reflect both transient and permanent dispersion). In addition, AKM estimates are identified off pay changes for job-movers that are realized quickly upon joining a firm, which may identify forms of rent-sharing different from those we identify (in particular, sharing of quasi-rents not tied to product demand). Nonetheless, the causal relationship between wages and firm productivity or demand would have to be significantly larger than our estimates—or than the cross-sectional relationship in Card et al. (2018)—to attribute the full variation in firm-pay differences to differences in firm performance.

While we find significant incidence of idiosyncratic firm demand shocks in Portugal during the Great Recession, pass-through behavior may differ in other institutional settings or in response to other shocks. In particular, our finding that pass-through in less-fluid labor market segments points to the likelihood that rent-sharing behavior differs substantially across contexts. Our setting is characterized by very strong labor market protections—in other countries where employment protections for low-wage workers are significantly weaker, employment is "at-will," and job turnover is significantly higher, it is plausible that a larger portion of the labor market behaves like the more fluid sectors in Portugal. Further, the pass-through of firm-specific shocks to wages in other contexts likely depends on the size and expected persistence of the shock. If explicit or implicit long-run contracts are an important feature of wage-setting, then firms will be more likely to adjust wages in response to permanent shocks than in response to transitory shocks (Beaudry and DiNardo, 1991; Guiso et al., 2005). Firms may also respond differently to similar shocks in more or less expansionary economies. While our focus on the Great Recession allows us to empirically disentangle the causal effects of firm-specific and market-level shocks, employers may behave differently in response to idiosyncratic shocks in higher-growth environments.

8 Conclusion

This paper documents that firms play an important role in transmitting trade shocks and other product demand shocks to workers wages. Our findings indicate that employers—particularly those in labor markets with higher barriers to replacing incumbent employees—make significant adjustments to wages in response to idiosyncratic trade shocks, even though unaffected close competitors do not. We found that an idiosyncratic export demand shock that changes firm output by 10 percent leads to a 1.5 percent change in the wages of attached incumbent employees.

These empirical pass-through elasticities provide a lower bound for the magnitude of the incidence of the underlying demand shocks on worker's wages. Our results are consistent with models in which the cost of replacing incumbent workers is higher during periods of higher demand, which lowers employers' outside options and thus increases workers' threat points.

These findings provide direct evidence from a natural experiment that *where* one works can make a significant difference in how much one is paid, regardless of job amenities or one's skills, abilities, or efforts brought to the job. This work adds to a growing body of research documenting that a significant amount of wage dispersion can be attributed to cross-firm pay differentials, even conditional on worker attributes or fixed effects. Moreover, our study provides evidence that cross-sectional dispersion in wages can directly result from firm-level heterogeneity in productivity or demand. Even if the medium-run wage effects we observe revert in the long run, repeated shocks would nonetheless generate a cross-sectional correlation between firms' pay premiums and their revenue productivity. We interpret our findings as evidence that prior non-experimental dynamic rent-sharing estimates were significantly attenuated. Nonetheless, our results are not large enough to provide a full accounting of the variance of AKM firm pay premiums based on differences in firms' revenue productivity.

An important question for future research is whether and how wage determination behavior differs in institutional settings outside of Portugal. Our finding that pass-through effects only occur in industries with higher levels of relationship durability raises the possibility that rent-sharing behavior may differ substantially in alternative contexts. Our setting is characterized by very strong labor market protections—though many European countries have similar institutions, Portuguese regulation is stringent even by European standards. It may therefore be the case that even the "high-durability" industries would feature less rent-sharing in alternative regulatory contexts. In addition, firms may adjust differently to different types of shocks. Wage incidence may differ in response to demand shocks when the economy is expansionary, when shocks differ in their persistence, or when shocks are to productivity rather than demand. We focus on the Great Recession for purposes of identification, as we believe import changes were harder to foresee during this period; we nonetheless believe that it would be useful to understand how employers would respond to idiosyncratic demand shocks during better times.

Data Availability Statement

The data underlying this article cannot be shared publicly, as the paper uses confidential administrative data. However, instructions on how to obtain these data along all replications scripts, including detailed explanations of data construction, are available at https://doi.org/10.5281/zenodo.7901559.

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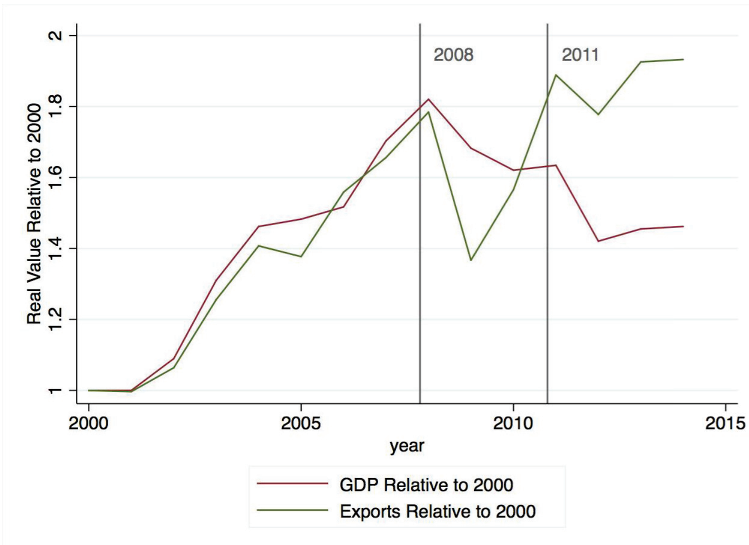
Figures and Tables

Figures

Figure 1: Growth and Exports in Portugal Around the Great Recession

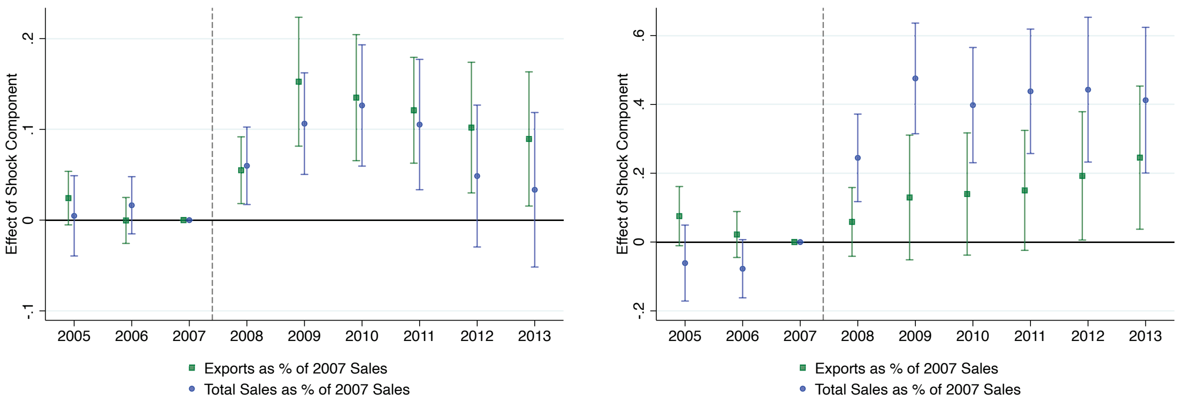


Source: World Bank (2016). Figure plots annual GDP and total exports for Portugal in real euros, with each variable indexed to its 2000 level. Vertical lines indicate the start of the Great Recession in the US at the end of 2007 and the beginning of the Portuguese sovereign debt crisis in the spring of 2011.

Figure 2: Year-Specific Effects on Exports and Sales in Comparable Currency Units

(a) Idiosyncratic Component

(b) Common Component

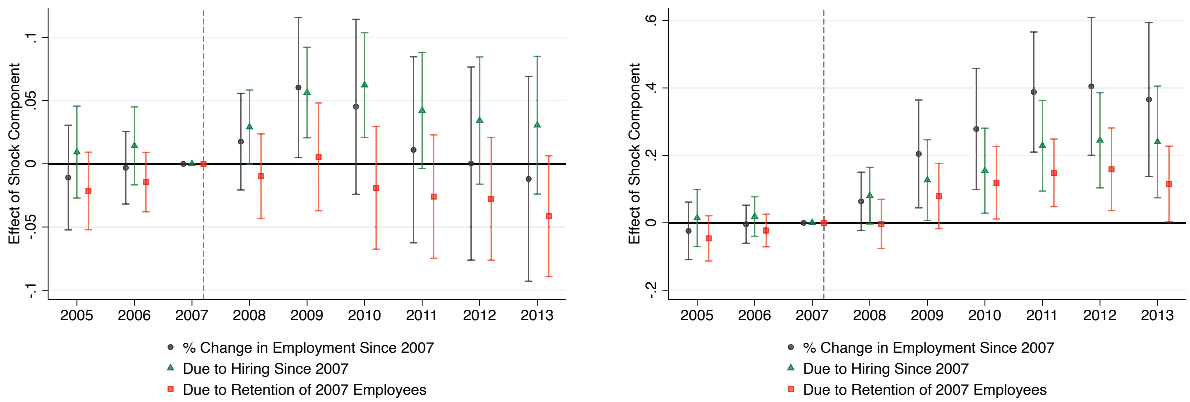


Notes: Figure displays year-specific effects of the specified shock to export demand Δ_j^τ on sales and exports in common euro units, normalized by firms' 2007 total sales. Sample is full analysis sample of firms ($N = 4,173$). Year-specific coefficients and 95% confidence intervals are from regressions on interactions of Δ_j^τ and an indicator for each year, with all interactions estimated jointly as in equation (13). Estimates for each outcome are from separate regressions. Confidence intervals are based on standard errors that are clustered at the firm level when Δ_j^{id} is the dependent variable and at the 4-digit industry level when Δ_j^{com} is the dependent variable to account for potential serial correlation of errors. Exports are coded as zero in years when firms do not appear in the export data. All regressions include year fixed effects, as well as controls for year-specific effects of the share of exports going to Spain or Angola in 2005–2007. When the Δ_j^{id} is the shock, we include a control for Δ_j^{com}. Corresponding estimates for sample of never-exiter firms are presented in Appendix Figure E.1.

Figure 3: Year-Specific Effects on Employment Adjustment: Hiring Versus Incumbent Retention

(a) Idiosyncratic Component

(b) Common Component



Notes: Figure displays year-specific effects of the specified shock to export demand Δ_j^τ on employment, total accumulated hires, and total retentions, all as a percent of firms' 2007 total employment. Sample is full analysis sample of firms ($N = 4,173$). Year-specific coefficients and 95% confidence intervals are from regressions on interactions of Δ_j^τ and an indicator for each year, with all interactions estimated jointly as in equation (13). Estimates for each outcome are from separate regressions. Confidence intervals are based on standard errors that are clustered at the firm level when Δ_j^{id} is the dependent variable and at the 4-digit industry level when Δ_j^{com} is the dependent variable to account for potential serial correlation of errors. All regressions include year fixed effects, as well as controls for year-specific effects of the share of exports going to Spain or Angola in 2005–2007. When the Δ_j^{id} is the shock, we include a control for Δ_j^{com}. The change "Due to Hiring" is the total number of workers at the firm who were not present in 2007, as a percentage of 2007 employment; change "Due to Retention" is the number of employees at the firm who were employed at the firm in 2007, also as a percentage of 2007 employment. By construction, changes in these two variables related to 2007 sum to the the total percent change in employment since 2007. Corresponding estimates for sample of never-exiter firms are presented in Appendix Figure E.2.

Figure 4: Dynamic Effects on Log Contract Wage of Attached Incumbents

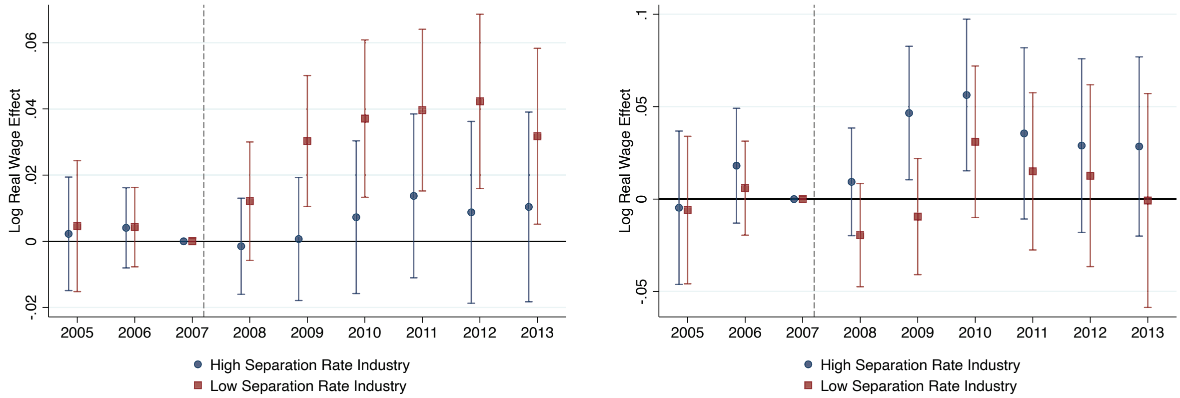
(a) Idiosyncratic Component (b) Common Component

Notes: Sample is all individuals i whose primary full-time job (120-200 hours per month) in 2007 was at a firm j in the main analysis sample of 4,173 firms. Figure shows year-specific reduced-form effects of the specified shock to export demand $\Delta^{\tau}_{j(i)}$ on individual' hourly wages conditional on remaining at the 2007 Firm. Estimates for each outcome are from separate regressions. The "at Other Sample Firm" estimates in each panel replace $\Delta^{\tau}_{j(i)}$ for each worker i at firm $j(i)$ with the average $\Delta^{\tau}_{-j(i)}$ taken over all other close-competitor firms in the same four-digit industry and the same municipality as $j(i)$, exclusive of $j(i)$. Confidence intervals are based on standard errors that are clustered at the firm level when $\Delta^{id}_{j(i)}$ is the dependent variable and at the 4-digit industry level when $\Delta^{com}_{j(i)}$ is the dependent variable to account for potential serial correlation of errors. All regressions include year fixed effects, as well as controls for year-specific effects of the share of exports going to Spain or Angola in 2005–2007. When the $\Delta^{id}_{j(i)}$ is the shock, we include a control for $\Delta^{com}_{j(i)}$. Corresponding estimates for 2007 employees of firms in the never-exiter sample are presented in Appendix Figure E.3.

Figure 5: Incidence of Shocks by Industry Relationship-Durability

(a) Idiosyncratic Component

(b) Common Component



Notes: Sample is all individuals whose primary full-time job (120-200 hours per month) in 2007 was at a firm in the main analysis sample of 4,173 firms. Figure shows year-specific reduced-form effects of the interaction of the specified shock to export demand $\Delta^{\tau}_{j(i)}$ and a dummy indicating either a high separation-rate industry or a low quit-rate industry on individual' hourly wages conditional on remaining at the 2007 Firm. Specifications included controls for year specific effects of separation-rate indicators. High (low) separation rate indicates the firms' five-digit industry has a leave-one-out average annual separation rate of permanent contract workers (averaged across years) that is above (below) that of the median firm in the sample. All coefficients are estimated jointly using specification in Equation 16. All regressions include year fixed effects, as well as controls for year-specific effects of the share of exports going to Spain or Angola in 2005–2007. Confidence intervals are based on standard errors that are clustered at the firm level when $\Delta^{id}_{j(i)}$ is the dependent variable and at the 4-digit industry level when $\Delta^{com}_{j(i)}$ is the dependent variable to account for potential serial correlation of errors. When the $\Delta^{id}_{j(i)}$ is the shock, we include a control for $\Delta^{com}_{j(i)}$. Corresponding estimates for 2007 employees of firms in the never-exiter sample are presented in Appendix Figure E.4.

Tables

Table 1: Summary Statistics: Pre-period Exports of Sample Firms

	Analysis Sample					Large Exporters	
	Mean	P25	P50	P75	SD	Mean	P50
2007 Exports, euros	1,132,340	41,718	321,375	1,187,745	2,364,478	18,500,000	5,312,642
Pre-Period Export Exposure	0.342	0.027	0.201	0.650	0.345	0.464	0.464
# Destination Countries	5.21	2	3	6	5.7	14.27	10
# Major (2D) Products	3.6	1	2	4	4.9	6.9	4
# Detailed (6D)Products	10.3	2	4	10	22.0	25.1	11
Number of Firms	4,173					934	

Notes: Table displays export statistics for firms appearing in the export data in each of 2005, 2006, and 2007, tabulated from the firm-product-destination-year level data. Analysis sample contains all such firms with 100 or fewer employees during pre-period (2005-2007 average). Large exporters are remainder of firms with over 100 employees. Exports are measured in constant 2007 euros. Exports/Sales is the ratio of total exports to total sales from the balance sheet data (a distinct source) averaged across years 2005-2007. Counts of destination countries and products (HS2 and HS6) pool 2005, 2006, and 2007 exports of each firms, to reflect construction of the shock.

Table 2: Comparison of Firms and Workers in Sample and Population

	Analysis Sample					Full Count Data				
	Mean	P10	P50	P90	SD	Mean	P10	P50	P90	SD
Firms:										
Employees	27.9	5	21	64	23.16	3.75	0	0	6	51.67
Sales/Worker if Emp>0, euros	173,226	33,803	103,085	374,312	238,679	98,055	0	29,892	180,425	2,894,556
Value Added / Worker if Emp>0, euros	34,769	11,935	26,575	61,969	40,939	20,397	0	11,019	42,658	203,807
N Firms, Emp>0			4,173					278,226		
N Firms			4,173					714,212		
Workers:										
Monthly Wage, euros	760.80	403	575	1310	533.58	753.12	403	561.74	1,318	530.46
Hourly Wage, euros	4.45	2.34	3.35	7.67	3.14	4.52	2.33	3.33	8.15	3.27
Log Monthly Wage	6.49	6.00	6.35	7.18	0.49	6.47	6	6.33	7.18	0.50
Log Hourly Wage	1.34	0.85	1.21	2.04	0.49	1.35	0.85	1.20	2.10	0.50
Fixed Term, Percent of Sample	0.20					0.25				
Tenure, Months (All Workers)	121	9	93	260	105.82	90.01	4.00	57	227	98.58
Female, Percent of Sample,	0.44					0.43				
Regular Hours Per Month	171	162	173	176	7.55	168	154	173	176	11
N Workers			115,526					2,490,452		

Notes: Table compares firms and workers in the analysis sample to the population data for year 2007. All figures in currency units are in 2007 euros. Data on firm sales and output is from the balance sheet data (IES) with N=278,226. Employment counts are tabulated from the matched employer-employee data (QP) for firms that appear in both the QP and IES, N=278,226. Worker statistics are tabulations of employee records in the cleaned QP, restricting to the highest paying full-time job (more than 120 hours per month) per worker, N=2,490,452. "Monthly Wage" and "Hourly Wage" are the base contract pay earned during regular hours during the reference month in the QP, excluding overtime, fringe payments, and bonuses. "Percent FTC" is the percent of workers at the firm with fixed-term contracts.

Table 3: Test of Sorting for Firms with Multiple Destinations

	Outcome is Change in Exports by Firm j of Product m to Country c, Measured by:					
	Symmetric Growth Rate		Log Change (if > 0)		Any Exports	
	(1)	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*
Coefficient: Change in destination c imports of product module m	0.0600***	0.0677***	0.476***	0.397***	0.0179	0.0324***
	(0.0152)	(0.0112)	(0.0481)	(0.0507)	(0.0154)	(0.0107)
Baseline Controls	x		x		x	
Firm Fixed Effects		x		x		x
N	101,344	101,344	35,193	34,804	101,344	101,343

Notes: Table reports results from regressions of changes in exports by firm j of product m to destination c on the change (symmetric growth rate) of imports of m to country c from all other countries (excluding Portugal) during the same period, as in equation (A.11). Observations are firm-market pairs (markets are country x 6-digit product). Sample includes all firms in primary analysis sample with exports to at least two distinct markets in the pre-period. Changes are taken from 2006-2007 to 2009-2010. When no exports occur in the post period, log values are treated as missing. Regressions are unweighted. Standard errors are two-way clustered at the firm and market level. *** indicates $p < .01$, ** indicates $p < .05$, * indicates $p < .10$

Table 4: Effects on Firm Sales and Output

	Log Exports *(1)*	Log Sales *(2)*	Exports/ 2007 Sales *(3)*	Sales/ 2007 Sales *(4)*	New Exp Dest *(5)*	New Exp Product *(6)*	Firm Active *(7)*
			A. Full sample (Full Sample of 4,173 firms)				
Baseline	0.635***	0.234***	0.126***	0.160***	−0.036	0.049	0.010
	(0.122)	(0.050)	(0.040)	(0.037)	(0.037)	(0.046)	(0.022)
Idiosyncratic	0.592***	0.143***	0.136***	0.104***	−0.077**	0.085***	−0.003
	(0.115)	(0.048)	(0.029)	(0.033)	(0.034)	(0.029)	(0.023)
Common	1.167***	0.693***	0.129	0.476***	0.080	−0.114	0.119**
	(0.221)	(0.111)	(0.090)	(0.083)	(0.077)	(0.153)	(0.055)
N Firm-Year Obs	*17,408*	*19,729*	*20,865*	*20,865*	*12,685*	*12,685*	*20,865*
			B. Never-Exiters (Balanced Sample of 2,926 firms)				
Baseline	0.669***	0.241***	0.120***	0.186***	−0.015	0.043	-
	(0.141)	(0.038)	(0.037)	(0.030)	(0.042)	(0.046)	
Idiosyncratic	0.624***	0.184***	0.122***	0.145***	−0.061	0.083***	-
	(0.127)	(0.038)	(0.030)	(0.030)	(0.038)	(0.032)	
Common	1.185***	0.526***	0.157*	0.400***	0.108	−0.130	-
	(0.213)	(0.102)	(0.084)	(0.078)	(0.083)	(0.151)	
N Firm-Year Obs	*13,436*	*14,612*	*14,630*	*14,630*	*10,331*	*10,331*	

Notes: Sample is either full analysis sample of firms ($N = 4,173$) or sample of firms that always report positive employment ("never-exiters" $N = 2,926$), as specified. Each point estimate is obtained from a separate regression. Estimates are coefficients on the interaction between the specified shock to export demand Δ_j^{τ} and $Post_t$. "Pre" years are 2006, 2007 (pre-period) and "Post" years 2009, 2010, 2011 (post-period), 2008 is omitted. Firm-year observations with zeros are treated as missing when the outcome is in logs—therefore, the baseline sample is not a balanced panel, but the never-exiter sample is. All regressions include year fixed effects, as well as controls for year-specific effects of the share of exports going to Spain or Angola in 2005–2007. When the Δ_j^{id} is the shock, we include a control for Δ_j^{com}. Standard errors are clustered at the firm level when Δ_j^{id} is the dependent variable and at the 4-digit industry level when Δ_j^{com} or Δ_j^{tot} is the dependent variable. *** indicates $p < .01$, ** indicates $p < .05$, * indicates $p < .10$.

Table 5: Own-Firm and Close-Competitor Effects of Shock Components, Full Sample

	Log Sales (1)	Log Value Added (2)	Log Payroll (3)	Log Employees (4)
Mean Pre-Post Change	*-0.252*	*-0.194*	*-0.021*	*-0.073*
A. Baseline				
Baseline	0.234***	0.194***	0.092***	0.075***
	(0.050)	(0.045)	(0.030)	(0.029)
N Firm-Year Obs	*19,729*	*19,217*	*19,111*	*19,111*
Placebo	0.223**	0.148*	0.040	0.076
	(0.096)	(0.085)	(0.063)	(0.060)
N Firm-Year Obs	*14,857*	*14,490*	*14,408*	*14,408*
B. Idiosyncratic component				
Idiosyncratic	0.143***	0.112**	0.051	0.035
	(0.048)	(0.047)	(0.033)	(0.030)
N Firm-Year Obs	*19,729*	*19,217*	*19,111*	*19,111*
Placebo	0.039	−0.000	−0.022	0.031
	(0.076)	(0.077)	(0.058)	(0.052)
N Firm-Year Obs	*14,857*	*14,490*	*14,408*	*14,408*
C. Common component				
Common	0.693***	0.584***	0.277***	0.287***
	(0.111)	(0.124)	(0.085)	(0.084)
N Firm-Year Obs	*19,729*	*19,217*	*19,111*	*19,111*
Placebo	0.896***	0.677***	0.281**	0.257**
	(0.173)	(0.183)	(0.139)	(0.130)
N Firm-Year Obs	*14,857*	*14,490*	*14,408*	*14,408*

Notes: Sample is full analysis sample of firms ($N = 4,173$). Each point estimate is obtained from a separate regression. The estimates in the first row of each panel are coefficients on the interaction between the specified shock to export demand Δ_j^τ and $Post_t$. The close-competitor "placebo" estimates in each panel replace Δ_j^τ for each firm j with the average Δ_{-j}^τ taken over all other firms in the same four-digit industry and the same municipality as j, exclusive of j. "Pre" years are 2006, 2007 (pre-period) and "Post" years 2009, 2010, 2011 (post-period), 2008 is omitted. Firm-year observations with zeros are treated as missing when the outcome is in logs—therefore, the baseline sample is not a balanced panel, but the never-exiter sample is. All regressions include year fixed effects, as well as controls for year-specific effects of the share of exports going to Spain or Angola in 2005–2007. When the Δ_j^{id} is the shock, we include a control for Δ_j^{com}. Table also presents the average (real) change in the dependent variable from pre-to-post. Standard errors are clustered at the firm level when Δ_j^{id} is the dependent variable and at the 4-digit industry level when Δ_j^{com} or Δ_j^{tot} is the dependent variable. *** indicates $p < .01$, ** indicates $p < .05$, * indicates $p < .10$. Corresponding estimates for sample of never-exiter firms are presented in Appendix Table E.8.

Table 6: Reduced-Form Effects on Employee and Close-Competitor Employee Wages, Full Sample

	Average Log Wage, Within Spell		Average Log Wage, Including Moves		Monthly Wage, Including Zeros			
	Monthly	Hourly	Monthly	Hourly	IHS	Arc Δ	At Firm	Any Job
	(1)	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*	*(7)*	*(8)*
				A. Idiosyncratic component				
Idiosyncratic	0.022***	0.019**	0.027***	0.024***	0.041	0.033	−0.003	0.004
	(0.008)	(0.008)	(0.008)	(0.008)	(0.050)	(0.037)	(0.025)	(0.018)
N	*432,845*	*432,845*	*485,055*	*485,043*	*582,975*	*582,975*	*582,975*	*582,975*
Placebo	0.010	0.002	0.013	0.005	−0.013	0.031	0.031	0.011
	(0.012)	(0.012)	(0.012)	(0.012)	(0.087)	(0.064)	(0.041)	(0.030)
N	*329,117*	*329,117*	*367,710*	*367,704*	*441,295*	*441,295*	*441,295*	*441,295*
				B. Common component				
Common	0.026*	0.020	0.023	0.018	0.318***	0.193**	0.181***	0.087**
	(0.014)	(0.014)	(0.016)	(0.016)	(0.104)	(0.080)	(0.060)	(0.036)
N	*432,845*	*432,845*	*485,055*	*485,043*	*582,975*	*582,975*	*582,975*	*582,975*
Placebo	0.074***	0.072***	0.074***	0.070***	0.497***	0.309***	0.202**	0.117**
	(0.026)	(0.025)	(0.026)	(0.026)	(0.153)	(0.116)	(0.079)	(0.056)
N	*329,117*	*329,117*	*367,710*	*367,704*	*441,295*	*441,295*	*441,295*	*441,295*
Mean Pre-Post Change	*0.061*	*0.065*	*0.060*	*0.064*	*-0.487*	*-0.342*	*-0.277*	*-0.178*

Notes: Sample is all individuals i whose primary full-time job (120-200 hours per month) in 2007 was at a firm j in the main analysis sample of 4,173 firms. Each point estimate is obtained from a separate regression. The estimates in the first row of each panel are coefficients on the interaction between the specified shock to export demand $\Delta^{\tau}_{j(i)}$ and $Post_t$. The close-competitor "placebo" estimates in each panel replace $\Delta^{\tau}_{j(i)}$ for each worker i at firm $j(i)$ with the average $\Delta^{\tau}_{-j(i)}$ taken over all other firms in the same four-digit industry and the same municipality as $j(i)$, exclusive of $j(i)$. "Pre" years are 2006, 2007 (pre-period) and "Post" years 2009, 2010, 2011 (post-period), 2008 is omitted. Worker-year observations with zeros are treated as missing when the outcome is in logs, and observations when workers are not employed by their 2007 employer are treated as missing in "Within-Spell" specifications. All regressions include year fixed effects, as well as controls for year-specific effects of the share of exports going to Spain or Angola in 2005–2007. When the $\Delta^{id}_{j(i)}$ is the shock, we include a control for $\Delta^{com}_{j(i)}$. Table also presents the average (real) change in the dependent variable from pre-to-post. Standard errors are clustered at the firm level when $\Delta^{id}_{j(i)}$ is the dependent variable and at the 4-digit industry level when $\Delta^{com}_{j(i)}$ is the dependent variable. *** indicates $p < .01$, ** indicates $p < .05$, * indicates $p < .10$. Corresponding estimates for sample of 2007 employees of never-exiter firms are presented in Appendix Table E.9.

Table 7: Pass-Through Elasticity: Effect in Wages for Given Change in Output, Full Sample

	Log Wage Within Spell		Monthly Wage, Including Zeros		
	Monthly	Hourly	IHS	Arc Δ	Any Job
	(1)	*(2)*	*(3)*	*(4)*	*(5)*
A. Idiosyncratic component					
IV	0.134**	0.115**	0.245	0.225	0.026
	(0.054)	(0.052)	(0.320)	(0.229)	(0.116)
N	*430,703*	*430,703*	*552,106*	*552,106*	*552,106*
First stage F	*15.05*	*15.05*	*4.51*	*4.51*	*4.51*
B. Common component					
IV	0.050**	0.040*	0.285***	0.159**	0.067**
	(0.022)	(0.023)	(0.098)	(0.070)	(0.033)
N	*430,703*	*430,703*	*552,106*	*552,106*	*552,106*
First stage F	*23.53*	*23.53*	*33.60*	*33.60*	*33.60*
C. OLS					
OLS	0.026***	0.023***	0.213***	0.159***	0.073***
	(0.003)	(0.003)	(0.016)	(0.012)	(0.005)
N	*430,703*	*430,703*	*552,106*	*552,106*	*552,106*

Notes: Table displays estimates of pass-through elasticities, obtained from IV and OLS difference-in-differences regressions of workers i's log wages on the sales of their 2007 employer $j(i)$. Elasticities are estimated from Equation (15) by OLS or instrumental variables (two stage least squares) estimation using the specified shock component $\Delta^{\tau}_{j(i)}$ interacted with $Post_t$ as an instrument for the firm output measure $Y_{j(i)}$ interacted with $Post_t$. Sample is all individuals whose primary full-time job (120-200 hours per month) in 2007 was at a firm in the main analysis sample of 4,173 firms. Each point estimate is obtained from a separate regression. "Pre" years are 2006, 2007 (pre-period) and "Post" years 2009, 2010, 2011 (post-period). Worker-year observations with zeros are treated as missing when the outcome is in logs, and observations when workers are not employed by their 2007 employer are treated as missing in "Within-Spell" specifications. All regressions include year fixed effects, as well as controls for year-specific effects of the share of exports going to Spain or Angola in 2005–2007 When the $\Delta^{id}_{j(i)}$ is the shock, we include a control for $\Delta^{com}_{j(i)}$. Table also presents the average (real) change in the dependent variable from pre-to-post. Standard errors are clustered at the firm level when $\Delta^{id}_{j(i)}$ is the dependent variable and at the 4-digit industry level when $\Delta^{com}_{j(i)}$ is the dependent variable. *** indicates $p < .01$, ** indicates $p < .05$, * indicates $p < .10$. Corresponding estimates for sample of 2007 employees of never-exiter firms are presented in Appendix Table E.10.

Table 8: Robustness of Idiosyncratic Shock Pass-Through Estimates

	(1)	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*	*(7)*	*(8)*	*(9)*	*(10)*
					A. Full Sample					
Sales Elasticity IV	0.134**	0.094**	0.148**	0.089**	0.138***	0.127**	0.145**	0.123**	0.112	0.151**
	(0.054)	(0.048)	(0.066)	(0.041)	(0.054)	(0.051)	(0.063)	(0.050)	(0.089)	(0.059)
Reduced Form Effect	0.022***	0.013**	0.021**	0.014**	0.023***	0.021***	0.021**	0.021***	0.010	0.032**
	(0.008)	(0.006)	(0.008)	(0.006)	(0.008)	(0.008)	(0.008)	(0.008)	(0.007)	(0.015)
N firms	*4,156*	*4,156*	*4,156*	*4,156*	*4,156*	*4,156*	*4,155*	*4,156*	*4,152*	*5,204*
N workers	*109,413*	*109,413*	*109,413*	*109,413*	*109,413*	*109,413*	*109,397*	*109,413*	*109,409*	*389,897*
					B. Never-Exiters					
Sales Elasticity IV	0.102**	0.070	0.102*	0.072*	0.103**	0.096**	0.101*	0.094**	0.070	0.127**
	(0.049)	(0.044)	(0.055)	(0.040)	(0.048)	(0.046)	(0.057)	(0.046)	(0.060)	(0.057)
Reduced Form Effect	0.020**	0.011	0.018*	0.012*	0.020**	0.019**	0.017*	0.019**	0.009	0.030*
	(0.010)	(0.007)	(0.010)	(0.007)	(0.009)	(0.009)	(0.010)	(0.009)	(0.008)	(0.017)
N firms	*2,921*	*2,921*	*2,921*	*2,921*	*2,921*	*2,921*	*2,920*	*2,921*	*2,919*	*3,712*
N workers	*80,639*	*80,639*	*80,639*	*80,639*	*80,639*	*80,639*	*80,623*	*80,639*	*80,637*	*328,108*
Angola/Spain Exposure	x		x		x	x	x	x	x	x
Common Component	x			x	x	x	x	x	x	x
Pre-Period Exports					x					
Pre-Period Firm Performance						x				
Destinations + 03-06 Demand							x			
Worker Attributes								x		
Main 4-Digit Product FE									x	
Including Large Firms										x

Notes: Table displays robustness of instrumental variables estimates of the pass-through of the idiosyncratic shock $\hat{\Delta}^{id}_{j(i)}$ to wages in presented in Table 7 to alternative specifications. The outcome is log monthly contract wage within spell. Worker-year observations with zeros are treated as missing when the outcome is in logs, and observations when workers are not employed by their 2007 employer are treated as missing in "Within-Spell" specifications. Each point estimate is obtained from a separate regression. Sample in Panel A is all individuals whose primary full-time job (120-200 hours per month) in 2007 was at a firm in the main analysis sample of 4,173 firms; and sample in Panel B is limited to those employed by the 2,926 firms that never exit the sample. Column 1 replicates IV estimates from Column 1 of Panel A in Table 7, and includes controls for for the common shock and pre-period exports to Spain and Angola—all specifications except Columns 2–4 include these controls. Column 2 includes no controls, while Columns 3 and 4 include the Angola/Spain controls and common shock control individually. Column 5 includes controls for firms 2005-2007 exports in logs and as a share of sales. Column 6 includes controls for year-specific effects of 2005-2007 average employment, sales, assets, hiring, labor productivity, wage levels, and fixed term contract employment. Column 7 includes controls for the share of pre-period exports going to each of 10 top destination countries, as well as predicted demand using 2003-2006 changes in imports at baseline destinations. Column 8 includes controls for workers' tenure, age, pre-period pay, gender, and educational attainment. Column 9 includes product-class-by-year fixed effects for the 4-digit HS product category that constitutes the largest share of each firms' 2005-2007 exports. Column 10 replicates the baseline specification in Column 1 in the extended sample including large firms. All controls are interacted with year dummies to allow for year-specific effects. Standard errors are clustered at the firm level. *** indicates $p < .01$, ** indicates $p < .05$, * indicates $p < .10$.

Table 9: Heterogeneity by Industry Relationship-Durability, Full Sample

	Separation Rate in Industry		Typical Tenure in Industry		Pre-Recession AKM Pay-Premium	
	Log Hourly Wage	Log Monthly Salary	Log Hourly Wage	Log Monthly Salary	Log Hourly Wage	Log Monthly Salary
	(1)	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*
			A. Idiosyncratic component			
High Frictions	0.294**	0.310**	0.120**	0.146**	0.204**	0.225***
	(0.148)	(0.151)	(0.060)	(0.063)	(0.083)	(0.084)
Low Frictions	−0.008	0.011	0.075	0.079	0.074	0.087
	(0.057)	(0.054)	(0.103)	(0.102)	(0.059)	(0.061)
Coeffs Equal, p-value	0.068	0.074	0.701	0.567	0.177	0.160
			B. Common component			
High Frictions	0.010	0.031	0.028	0.037	0.019	0.040
	(0.029)	(0.026)	(0.031)	(0.028)	(0.048)	(0.043)
Low Frictions	0.079**	0.079**	0.051	0.067*	0.116	0.072
	(0.035)	(0.034)	(0.037)	(0.035)	(0.103)	(0.091)
Coeffs Equal, p-value	0.135	0.262	0.650	0.514	0.440	0.772

Notes: Table displays estimates of subsample-specific pass-through elasticities, obtained from the interacted IV difference-in-differences regression specification in (17), where interactions of sales $Y_{j(i)}$, $Post_t$, and each of the group indicators $Hi_{j(i)}$ and $Lo_{j(i)}$ are jointly instrumented with interactions of $Post_t$, the specified shock component $\Delta^{\tau}_{j(i)}$, and each of $Hi_{j(i)}$ and $Lo_{j(i)}$. Wage outcomes are defined within spell, and observations when workers are not employed by their 2007 employer are treated as missing. The sample is split into high and low friction subsamples based on whether each of three different measures of potential frictions is above or below median for the sample. The first measure is the leave-one-out five-digit industry average of firms' median tenure of permanent contract workers in 2003-2007 (low turnover rate indicates high frictions). The second is the leave-one out average annual separation rate of permanent contract workers (averaged across years). The third is a firm-level AKM firm effect identified off of permanent contract workers who switch firms, see footnote 52 for details. All interacted specifications include controls for the categorical indicators $Hi_{j(i)}$ and $Lo_{j(i)}$ times $Post_t$. See notes to Table 7 for addition specification details. Standard errors are clustered at the firm level when $\Delta^{id}_{j(i)}$ is the dependent variable and at the 4-digit industry level when $\Delta^{com}_{j(i)}$ is the dependent variable. *** indicates $p < .01$, ** indicates $p < .05$, * indicates $p < .10$.

Table 10: Idiosyncratic Shock Pass-Through among Subgroups of Workers, Full Sample

	Sales Elasticity IV		
	Log Hourly Wage	Log Monthly Salary	N Workers
	(1)	*(2)*	*(3)*
Perm. Contract	0.109^{**}	0.130^{**}	355,398
	(0.052)	(0.054)	
Fixed-Term Contract	0.181	0.193^{*}	69,371
	(0.113)	(0.112)	
Male	0.080	0.094^{*}	199,962
	(0.054)	(0.055)	
Female	0.118^{*}	0.151^{**}	155,436
	(0.069)	(0.077)	
Low Wage	0.139^{**}	0.153^{**}	217,981
	(0.061)	(0.063)	
High Wage	0.090^{*}	0.114^{**}	212,722
	(0.052)	(0.054)	
No High School	0.143^{**}	0.169^{**}	267,194
	(0.064)	(0.068)	
High School	0.025	0.034	109,153
	(0.056)	(0.057)	
Blue Collar	0.147^{**}	0.169^{***}	220,827
	(0.061)	(0.064)	
White Collar	0.027	0.041	119,265
	(0.062)	(0.064)	
BC Lower-Skilled	0.054	0.072	88,698
	(0.065)	(0.064)	
BC Semi-Skilled	0.195^{**}	0.221^{***}	132,129
	(0.078)	(0.084)	

Notes: Table displays idiosyncratic shock pass-through elasticities corresponding to IV estimates from Columns 1 and 2 of Table 7, pertaining to specific subgroups of workers. Wage outcomes are defined as within-spell, and observations when workers are not employed by their 2007 employer are treated as missing. The sample is split into high and low friction subsamples based on whether each of three different measures of potential frictions is above or below median for the sample. Each estimate is obtained from a separate equation. Specification is identical to that in Table 7. "High-wage" and "Low-wage" indicate 2007 wage above/below 2007 firm median (taken over all workers, not just attached). "Blue Collar" and "White Collar" positions are distinguished based on the occupational coding of the job. We code crafstman and foreman jobs as "Semi-Skilled" blue-collar work and operator and laborer jobs as "Lower-Skill" blue-collar jobs. Number of firms and incumbent workers included in each specification are displayed. All regressions include year fixed effects, as well as controls for year-specific effects of the share of exports going to Spain or Angola in 2005–2007. When the $\Delta^{id}_{j(i)}$ is the shock, we include a control for $\Delta^{com}_{j(i)}$. Standard errors are clustered at the firm level. *** indicates $p < .01$, ** indicates $p < .05$, * indicates $p < .10$.