How Responsive are Wages to Demand within the Firm? Evidence from Idiosyncratic Export Demand Shocks*

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Abstract

How much do employees' wages directly reflect their employer's labor demand, rather than competition from other employers in the labor market? We test the wage incidence of product demand shocks by studying a quasi-experiment that idiosyncratically shocked individual firms' export demand without systematically affecting similar firms' product or labor demand. Our shocks measure how much Portuguese exporters' sales were impacted by *where*—but not *what*—they had been selling before the recession of 2008. These shocks predict changes in output, payroll, and hiring at affected firms, but not at rival employers in the same labor market segment. An idiosyncratic shock that changes output by 10 percent in the medium-run causes wages of pre-2008 employees to change proportionally by 1.5 percent, relative to trend. Consistent with a simple framework, we find that these pass-through effects are larger in industries with lower employee turnover rates and in firms with higher pay premiums. These findings offer evidence that heterogeneous firm dynamics can plausibly generate substantial cross-sectional wage dispersion, but only in less-fluid labor markets.

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1 Introduction

Why do employers adjust employees' wages: changes in their internal labor needs or changes in competition from other employers in the labor market? In perfectly competitive labor markets, wages are determined by competition among employees and employers, and employers always pay identical workers the same amount. However, when labor markets are imperfectly competitive, workers at firms facing expansionary product demand may be able to negotiate higher wages regardless of the state of the outside labor market. Understanding how and why employers behave in practice is important for assessing the impact of labor market interventions that target specific employers in large, global markets. Moreover, if wages are determined noncompetitively within firms in separate "internal labor markets" (Doeringer and Piore, 1971), the sharing of noncompetitive quasi-rents at firms performing differently could generate substantial cross-firm wage differentials, potentially explaining the cross-firm wage dispersion found around the globe (Abowd et al., 1999; Card et al., 2018; Song et al., 2015; Barth et al., 2016; Alvarez et al., 2018).

In this paper, we test how much and under what circumstances firms raise wages to match heightened production goals—even when wage competition from rival employers remains unchanged. Such a test is difficult to implement in practice. One must isolate a measurable source of variation in product demand conditions that is both uncorrelated with any changes in the skills or efforts employees bring to the table and also not associated with a general change in demand among rival employers. Several studies to date examine how changes in wages correlate with changes in output or productivity (Guiso et al., 2005; Card et al., 2013a; Mogstad et al., 2017; Alvarez et al., 2018); however, the conclusions that can be drawn from such studies are limited by the possibility that changes in output are driven either by labor supply (rather than labor demand) or by changes in labor demand that are common to entire markets.[1] This paper attempts to fill that gap.

To assess how much wages respond to firms' internal labor demand rather than labor market competition, this paper studies a novel natural experiment that idiosyncratically changed individual firms' product demand without affecting conditions at their closest labor-market competitors. We study how much Portuguese exporters' ability to sell during the global 2008–2009 recession was impacted by *where*—but not by *what*—they had been selling before 2008. As different countries'

[1] Previous work has also examined the wage incidence of quasi-experimental shocks to entire industries (Abowd and Lemieux, 1993; Revenga, 1992), entire regions (Autor et al., 2014; Yagan, 2016), or the export sector as a whole (Verhoogen, 2008; Friedrich, 2014). These studies provide evidence that barriers to competition divide labor markets into noncompeting segments, but do not necessarily imply that individual firms have wage-setting power.

import demands for any given product were jolted in unpredictable ways during this turbulent period, otherwise-similar exporters experienced unexpectedly differential changes in product demand based solely on who their customers were—even among those selling the same product.

Our approach builds on a growing strain of the international trade literature (Hummels et al., 2014; Berman et al., 2015; Mayer et al., 2016) that uses detailed data on how much each firm exported of specific product varieties to each overseas destinations to construct firm-level demand shifters.[2] However, our approach differs from earlier work in that we limit our focus to quasi-experimental variation from the 2008–2009 recession and argue that the exogeneity of this demand shock stems from the quasi-randomness of the underlying import demand shifts around the world, not the "shift-share" instrument form *per se*.[3] Moreover, we implement a novel decomposition of the export demand change into two components: one reflecting global changes in product demand that might have affected many firms in a product market and another that measures how much purchases of that product in the specific destination change compared to other countries. We find that differential exposure to specific countries within a product market affects firms' sales, value-added, payroll, and employment over the five years following 2008—but, importantly, not at their close competitors. Nonetheless, though the firm-specific shocks have no effect on product or labor demand at close competitors, they do have a significant impact on wage growth among pre-2008 employees of affected exporters.

We find that, even in the absence of a market-wide demand shift, the wages of pre-Recession employees adjust significantly in response to firm-level demand changes. A demand shift resulting in 10 percent more output results in a 1.5 percent larger rise in hourly wages for those incumbent workers. These effects arise primarily within continuous employment spells at the initial employer, suggesting that these wage changes reflect bargaining power within the firm—and not an increase in general human capital. In our Portuguese setting, this bargaining power may arise in part from institutional restrictions on firing. Consistent with high firing costs, we find that employers respond to production demand shocks by hiring more or fewer workers—but not by firing existing employees. In addition, we find wage effects are widely shared across workers of different pay levels, skill levels, genders, and contract types, though we only find significant effects on workers hired three or more

[2] In related work, Hummels et al. (2014) tests whether offshoring (input supply) shocks complement or substitute for incumbent workers. Their primary focus is substitutability/complementarity of low- and high-skill labor with off-shored inputs, though they simultaneously estimate import supply and export demand shocks as part of their analyses. Our paper differs in its emphasis on identification of a quasi-experimental shocks that was idiosyncratic to firms and the implications for wage determination.

[3] This approach to identification with a "Bartik-style" shift-share instrument follows Borusyak et al. (2018).

years prior to the recession.

However, wages do not respond in the same manner in all settings. 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 wages only respond to firm-level demand in sectors with higher barriers to employee turnover, which we infer based on low quit rates and longer tenures. In sectors with low turnover costs, wages are mostly pinned down by labor market competition, and we find no evidence that firms adjust wages based on their own demand conditions. We note that firms with lower turnover rates tend to have higher pay premiums based on Abowd et al. (1999) (henceforth AKM) models; moreover, we find evidence that firms with higher AKM premiums pass idiosyncratic 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 empirical findings provide empirical content to a long theoretical 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' labor demand in such markets, little causal evidence exists establishing a direct causal link between labor demand and wages. Our study offers novel evidence that firms with higher labor demand do in fact pay their employees significantly more—but only when labor markets are sufficiently uncompetitive. However, not all labor market frictions generate a causal link between firm-level labor demand and wages. In a simple framework, we show that wages only depend on firm-level labor demand in the presence of a specific type of adjustment friction that leads the marginal revenue product of a given employee—and thus that employee's "threat point"—to change along with total labor demand over a given time horizon.[4] Our findings are direct evidence that Portuguese workers have stronger bargaining positions when employers face growing product demand, irrespective of conditions in the broader labor market.

Moreover, 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

[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)

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 enough to rationalize the general productivity-pay relationship. By contrast, in our 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, suggesting that mismeasurement of labor demand biases estimates of wage incidence towards zero. Our results are also consistent with quasi-experimental studies of rent-sharing after firm innovations (Van Reenen, 1996) or unexpected approval of patent applications (Kline et al., 2018).[5] 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 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 and examines heterogeneity across firms and individuals. In Section 6, we discuss the interpretation of our findings in the context of

[5] While demand shocks and grants of intellectual property both boost profitability of firms, however, these windfalls also differ somewhat in nature from output demand shocks. Although the timing of a patent award may be quasi-random, the work that went into the patent can be directly attributed to individual workers and staff may be rewarded for past work—which had been of unclear value *ex ante*—after the fruits of the labor were revealed to be valuable. For example, (Kline et al., 2018) find the largest effects for employees listed on the patent. Although these effects indicate that firms share *ex post* windfall rents with some employees, it remains ambiguous how firms would respond to *ex ante* demand shocks.

[6] This is consistent with Sorkin (2017), which argues that a substantial fraction of the variance of AKM firm fixed effects reflects differences in non-wage amenities rather than rent-sharing.

earlier studies. Section 7 concludes.

2 Competitive and Noncompetitive Channels of Wage Incidence

We begin by studying the incidence of idiosyncratic product demand shocks in a stylized single-period model, which motivates a general framework for interpreting the incidence elasticity we estimate. 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. When workers access the outside market, they obtain a competitive wage $\bar{w}(\bar{P})$ that reflects the demand for labor in the market, which is in turn derived from market-level product demand $\bar{P}$. Similarly, if firms access the outside market, they can find another employee at competitive wage $\bar{w}(\bar{P})$ and product $Y_{i'j} = P_j$.

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 ($C'_j(P_j) > 0$), such as recruitment or training that requires forgone production time. Given this cost, firm j is

[7] Here, we effectively assume for simplicity that demand curves have an elasticity of zero, but this without loss of generality. In Appendix A, 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] The worker may also experience a cost $C^{worker} \geq 0$ due to foregone amenities or time searching while unemployed. The presence of such a cost does not impact the analysis unless P_j affects C^{worker} directly (rather than affecting the wage through bargaining and $\bar{w}(P^{comm})$). It is difficult to think how such a dependence could arise, and this is not the case in any of the models we review below.

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. For any wage w_{ij} in the range $(w^{out}(\bar{P}), w^{out}(\bar{P}) + C_j(P_j))$, 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 $C(P_j)$, and the wage is given by:

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

In our setting, perfect competition corresponds to a zero cost of accessing the outside market. In this case, all firms pay the competitive wage $\bar{w}(\bar{P})$, which depends on common demand but not on individual firms' idiosyncratic demand p_j. When competition is imperfect, however, two firms j and j' may offer different wages to otherwise identical workers due either to differences in firms' ability to access the external market $C_j(\cdot) \neq C_{j'}(\cdot)$ or—if losing an incumbent is costlier when product demand is higher—to idiosyncratic differences in demand $p_j \neq p_{j'}$. Thus, any incidence of idiosyncratic demand shocks p_j on wages operates through a firm-specific pay premium similar to the one modeled in Abowd et al. (1999), which is sustained by barriers to competition. By contrast, demand shocks that are common to many firms in a market cannot separately identify firm-specific pay increases from wage increases due to competitive pressure on the worker's outside option in the labor market. This is apparent in the following incidence elasticities:

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

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

B) The elasticity of wages with respect to common demand shocks P_j^{comm} is given by

[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 $w^*_{ij} = \text{argmax}_w (w^*_{ij} - w^{out})^{\beta} (C(P_j) - (w^*_{ij} - w^{out}))^{1-\beta}$

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

Idiosyncratic shocks are directly informative about firm-specific pay premiums. By contrast, common shocks that effect 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 (2016)) could impact wages due to increases in outside options even with *no* labor market imperfection.

Proposition 1 also facilitates interpretation of the elasticities we estimate. The incidence of idiosyncratic shocks in Equation (3) is the product of two terms. First, the fraction of the worker's wages that are non-competitive rents due to the friction C, which reflects the level of competition in the labor market. However, 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. The presence of this term is central to comparisons of elasticities with other measures of firm pay differentials. Even when wages incorporate large quasi-rents, there is no guarantee that these quasi-rents change in response to firm performance. This highlights an important lesson: rent-sharing stems from an underlying friction in the labor market, not the value of firms per se.[11]

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:

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

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

[11] In canonical collective bargaining models, unions can hold up the entire value added of the firm. In such a setting, the quasi-rent in the employment relationship is thus the *entire* per-worker value added of the firm, and employee bargaining positions thus directly respond to changes in demand or productivity. Building on these models, many papers assume $Rents_{ij} = \frac{\text{Value Added}}{\text{Worker}}$ and estimate a "rent-sharing elasticity" that is similar to the one in Equation (6), but only includes the first term (Card et al., 2018). Although the assumption that $Rents_{ij} = \frac{\text{Value Added}}{\text{Worker}}$ might be a realistic feature of union-firm bargaining, it is not general.

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 A, 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:

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 said period, the cost imposed by a departure of an employee is the firm's entire potential output $C_j(P_j) = P_j$. 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 ($\frac{d \ln Rents_{ij}}{d \ln p_j} > 0$).

Search with multiple-worker firms and multilateral Stole and Zwiebel (1996) bargaining (Acemoglu and Hawkins, 2014): If individual employees cannot be replaced during a period, they can only hold up their *marginal* product.[12] Firms anticipate this, however, and can adjust recruitment in response to demand shocks accordingly, potentially keeping the *per-worker* surplus. In the appendix, we show that when per-worker recruitment costs are constant, firms always choose employment levels in a manner that keeps per-worker rents constant ($\frac{d \ln Rents_{ij}}{d \ln p_j} = 0$). Workers are only able to extract more during periods of higher growth if firms' short-run recruitment costs are convex: in this case, employment adjustments are muted as in Oi (1962), and therefore marginal product and the corresponding per-worker surplus grow as firms attempt to adjust upwards ($\frac{d \ln Rents_{ij}}{d \ln p_j} > 0$).

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 and wages.[13] 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

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

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

respond to product demand shocks when unions limit employment growth to increase *per-employee* hold-up power .[14]

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 $\frac{d \ln Rents_{ij}}{d \ln p_j} > 0$. The contracting friction gives infra-marginal employees an information rent tied to the wage demands of marginal workers. As noted by as in Manning (2006), the wage-posting model is similar to the multilateral bargaining model when firms must rehire their full workforce each period and recruitment costs are convex.

In each of these models, demand shocks that are common to many producers affect wages through competition in the market. However, product demand shocks that are idiosyncratic to specific firms should affect their respective employees' wages over a given time horizon only if it becomes costlier to lose a worker when output demand is higher. 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 model highlights the potential threats to identification of $\epsilon^{w,p^{id}}$. In a regression framework, we seek to estimate:

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

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

[14] This occurs when the output elasticity of labor is decreasing.

estimate of $\epsilon^{w,p^{id}}$. Second, and more fundamentally, a valid shock must be *exogenous* to changes in worker characteristics $E[p_{j,t} \times \nu_{i,t}] = 0$.[15] If product demand increases are related to increased 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 $\epsilon^{w,p^{id}}$ 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:

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

This is the elasticity can be obtained from a regression

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

where changes in output $Y_{j,t}$ are instrumented by a shifter to $\ln P_{jt}^{id}$ that satisfies the exogeneity and idiosyncrasy restrictions. The empirical elasticity $\epsilon^{w,Y}$ gives a lower bound for the underlying structural elasticity $\epsilon^{w,p^{id}}$. In particular, the bounding inequality in (8) holds whenever the production function is concave: Firms have a strict incentive to increase their employment level in response to positive (or negative) shocks. Thus, putting increases in the denominator weakly overstates the demand change.[17] In addition, benchmarking wage changes to output changes offers a useful normalization in its own right.

[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.

[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 $\frac{d \ln Y_j}{d \ln P_j^{id}} = 1 + f'(L) \times \frac{dL}{dp_j^{id}} \geq 1$. 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

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. Portuguese labor markets are substantially more rigid than those in the United States. Blanchard and Portugal (2001) observed that although Portugal's unemployment rate in the 1990s was more similar to United States than to other southern European countries, this similarity masked important differences in dynamism: transitions into unemployment were lower in Portugal, but unemployment durations were much higher. One potential explanation for the low degree of turnover in Portugal is the presence of very strong job protections. Similar to systems in several other European countries, most employees work under permanent contracts that can only be terminated if the firm has legally defensible cause—and fired employees can sue for wrongful termination. As a result, layoffs are rare, and individual incumbent workers have hold-up power in the firm. In recent decades, firms have been granted limited scope to hire workers under fixed-term contracts, wherein after a set duration employees must either be released or promoted to a permanent contract.[20] 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, they are nonetheless still

[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] Fixed term contracts may be renewed up to two times under certain circumstances, but were not indefinitely renewable during the study period.

quite rigid compared to typical work arrangements in the United States because firms may not dismiss workers without cause until the full duration has elapsed.[21]

Collective bargaining institutions in Portugal are somewhat distinctive: although only 11 percent of private sector workers are unionized, under Portuguese law, any wage floor negotiated for a specific job title automatically extends to all workers with the same job title. Unions generally do not directly negotiate wages with individual firms, but rather set occupation-wide minimum wages—Addison et al. (2017) found that over 90 percent of private sector employees between 2010 and 2012 were covered by occupation-wide extensions.[22] Firms can freely raise wages above these floors but have very limited scope to reduce nominal wages.

3.1 Data and Sample

We study the universe of Portuguese firms that exported in each of the three years preceding the 2008 recession.[23] We measure firms' exposure to global demand and the resulting effects on export flows using annual administrative data covering 2005–2013 that reports firms exports and imports by destination country and six digit product (HS-6) level.[24] 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 using 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 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 each employee's 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 firm j

[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] Martins (2014) provides a detailed discussion the institutions that result in extension of bargained contracts—resulting in over 30,000 occupation-specific wage floors—and studies the impacts on the wage distribution.

[23] The sample includes all sectors that export. 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. Examples include as book and software publishing.

[24] 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.

in 2007, and study impacts on this cohort regardless of whether members remained at their 2007 firm. 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 B.

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 based on exposure to Spain and Angola may be therefore be associated with latent characteristics of firms and workers.[25] Third, we limit our primary focus to small- and medium-sized firms—those with average 2005–2007 employment of at least one and no greater than 100 employees—which are observably less diversified across export customers and more subject to idiosyncratic demand variation. 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.[26] 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).

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

[25] In all the primary analyses, we control for year-specific effects of the total 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.

[26] Table 1 shows that larger firms 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 ten destination 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.1 shows that in 2007, the small and medium-sized firms are larger than the median firm, but when tabulating the distribution of employer sizes amount *workers*, the these smaller exporters are central in the distribution.

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; 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.[27]

4 Identifying Idiosyncratic Export Shocks During the 2008 Global Recession

To identify demand shocks that are both exogenous and idiosyncratic, we exploit two important features of the recession and its impact on export demand. First, the recession led to differential changes in import demand that were sudden and unexpected, both across product groups and *within* product groups across importer countries. Appendix Figure A.2 shows that in bilateral trade flow data, measured at the country by six-digit product level, the growth of a country's imports of a product between 2004 and 2007mis not predictive of the change in importing that occurred following the 2008 recession. Second, firms had sticky relationships with their export customers, and, as a result there was substantial variation in firms' exposure to differently shocked foreign markets. Due to the unforeseeable nature of the global recession, it is unlikely that firms with unobservably different workers systematically sorted to differently shocked customers. Moreover, it is possible to test whether firms with declining export productivity systematically sorted to worse-shocked locales using firm-by-destination data, in the spirit of Khwaja and Mian (2008). Among firms with multiple export destinations, we find that firm exports to a specific destination are strongly predicted by changes in import demand at that destination. However, that this association is invariant to controls for firm fixed effects that capture latent productivity attributes, suggesting that changes in exports to preexisting customers during this period were driven by demand, not by changes in firm-side unobservables. In Appendix C, we discuss this test in greater detail.

4.1 Measuring Idiosyncratic Demand Shocks

We summarize firms' exposure to these quasi-experimental demand shocks with a measure of firms' predicted export demand change based on their pre-recession relationships. First, we define

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

the exposure weight of a firm j to the market for six-digit product module m in country $c$as the share $s_{j,mc}$ of the firm's total 2005–2007 exports that are exports of product p to country, $s_{j,mc} = \frac{Exports_{j,pm}^{2005-2007}}{\sum_{p \in P, c \in C} Exports_{j,mc}^{2005-2007}}$. We then calculate the symmetric growth rate of imports of product p by country c from all countries (*excluding* Portugal) 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.[28] The baseline predicted change in export demand for firm j, Δ_j, 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:

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

This shock is a variant of shift-share export shocks constructed in earlier papers (Berman et al., 2015; Hummels et al., 2014). However, our approach differs in that we limit our focus to quasi-experimental variation from the great Recession. Borusyak et al. (2018) highlight why this focus is important for identification: the exogeneity of this demand shock stems from the quasi-randomness of the underlying demand shifts during the recession Δ_{mc}, not the shift-share form *per se*.

Although firm-level in construction, the shock Δ_j may nonetheless contain demand variation that also affects other firms that produce similar products to firm j. Specifically, Δ_j reflects changes in demand for product module m that occur in *all* countries and therefore may affect *all* producers of m in Portugal. Formally, let $s_{p,j} = \sum_{c \in C} s_{mc,j}$ 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.[29] Thus, Δ_j can be decomposed as:

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

Whereas 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. Put differently, the second term isolates

[28] 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).

[29] In our baseline specification, we measure Δ_p as the average of Δ_{pc} taken across all countries.

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 p capture 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.[30]

Our strategy is to use this latter component as our shifter of idiosyncratic demand P_j^{id}. Since $\Delta_j^{id} = \Delta_j - \Delta_j^{comm}$ and two components may be mechanically negatively correlated; we construct a version of the idiosyncratic shock that is orthogonal to the product-level variation in demand. To do this, we regress the baseline shock Δ_j on a fourth order polynomial in Δ_j^{comm}. We use the residual from this regression, labelled $\hat{\Delta}_j^{id}$, as our primary shock, and the prediction $\hat{\Delta}_j^{comm}$ as the corresponding common component. [31] We describe the construction of these components in detail in Appendix B. Figure 2 plots the distribution of both the baseline demand predictor Δ_j and the idiosyncratic shock component $\hat{\Delta}_j^{id}$. The idiosyncratic component $\hat{\Delta}_j^{id}$ accounts for 87 percent of the variation in Δ_j. Although the average levels of both Δ_j and $\hat{\Delta}_j^{id}$ are close to zero, these are adverse shocks relative to pre-recession export growth. Figure 2 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 5th and 95th percentiles in all of our subsequent analyses. Figure 3 shows the baseline correlation of the idiosyncratic demand shock $\hat{\Delta}_j^{id}$ with firms' 2007 attributes. Consistent with quasi-random assignment, the shock is uncorrelated with most measures of firm size, firm productivity, and employee characteristics. There is, however, a significant correlation with the firm's pre-period export level. This correlation could confound results it other latent drivers of performance and wage outcomes are correlated with firms' export intensity but not with total sales, productivity, and employee characteristics.[32]

[30]This condition would fail if firms in Portugal all exported to the same subset of countries, which may not be representative of the global average Δ_p. An alternative approach would be to match firms with identical product compositions, which is essentially a method for identifying the common variation $\sum_p s_{j,p}\Delta_p$ without measuring each Δ_p directly. Since matched firms have identical $s_{j,p}$, then $\sum_p s_{j,p}\Delta_p$ would be absorbed by a match-level fixed effect. In practice, given the limited sample size and the rich number of distinct products and the commonality of multi-product firms in the data, matching is not feasible.

[31]Formally, we construct $\Delta_j^{comm} = \sum_m s_{j,m}\Delta_m$, then estimate regressions $\Delta_j = \alpha + \beta_1\Delta_j^{comm} + \beta_2(\Delta_j^{comm})^2 + \beta_3(\Delta_j^{comm})^3 + \beta_4(\Delta_j^{comm})^4$. We define our primary idiosyncratic shock $\hat{\Delta}_j^{id}$ as the residual variation in Δ_j netting out the part predicted by Δ_j^{comm}, (which we denote $\hat{\Delta}_j^{comm}$), thus $\hat{\Delta}_j^{id} \equiv \Delta_j - \hat{\Delta}_j^{comm}$.

[32]To ensure such a correlation does not drive the results, we control for year-specific effects of exports (in logs, levels, and as a share of sales) in the primary analysis.

4.2 Effects on Exports, Sales, and Output

If exporting relationships are sufficiently difficult to adjust, then the shock $\hat{\Delta}_j^{id}$ should predict a change in firms' exports and a corresponding change in sales. We examine the "first-stage" effects on firms' performance by estimating the following firm-level difference-in-differences regressions:

$$Y_{jt} = \alpha_t + \delta_j + \beta \hat{\Delta}_j^{id} \times Post_t + \sum_{k \neq 2007} \gamma_k X_j^{pre} \times \mathbf{1}\{t = k\} + \nu_{jt} \ , \ t \in \{Pre, Post\} \tag{12}$$

where Y_{jt} is a firm-level outcome, 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 year-specific effects of firms' pre-period 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. The effect on *total* demand in turn depends on the share of firm j's sales that are exports—and in some analyses we use firms' average 2005–2007 export intensity $S_j^{pre} = \frac{Exports_j^{pre}}{Sales_j^{pre}}$ to predict a *sales* demand shock as $S_j^{pre} \times \hat{\Delta}_j^{id}$, although S_j^{pre} is a potentially endogenous object. To assess the timing by which the shocks impact firms, we also examine dynamic specifications of the form:

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

Each coefficient β_k is the year-specific effect of our shock in year k, relative to the omitted year 2007. 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 never exit the sample.

The difference-in-difference estimates presented in Table 3 show that relationships do matter—a one percent predicted change in export demand based on firms' pre-recession customers causes a 0.7 log point change in actual exports among those who continue exporting. This coefficient understates the total impact on exports, as negative shocks lead firms—even those that remain in business—to stop exporting.[33] The results in Panel B show the impact on total sales and production. The effect of a one-percent predicted change in export demand causes a corresponding change in sales

[33] We find that firms with worse shocks are in fact more likely to begin exporting to new destinations, but this effect is not large enough to significantly mitigate the shock.

by .21 log points in the balanced panel of firms that never exit. To interpret the magnitude of this effect, it is helpful to study the effect of a percentage change in total sales demand due to the export shock, measured by $S_j^{pre} \times \hat{\Delta}_j^{id}$. We find that a predicted changes in sales impacts actual sales almost one-for-one. The sales effect reflects a real change in production activity, as evidenced by comparable effects on firms' value added (which only includes capital and labor output and excludes intermediate inputs) and payrolls.[34] Although employment adjusts, the effect is smaller; accordingly, there is a significant and persistent effect on average labor productivity (measured by either sales per worker or value added per worker); this effect is displayed in Appendix Figure A.4. We present additional employment outcomes in Appendix Table A.3. Importantly, firms do not systematically exit the employer data nor the balance sheet data in response to the shock; this finding limits concerns about attrition biases in balanced-sample analyses.

The dynamic estimates plotted in Figure 4 show how the shock to export demand affects exports and total sales over time, both measured in common units as percentage of 2007 sales. An idiosyncratic export demand shock should only impact sales via changes in exporting; consistent with this, we find the effects on sales are almost fully accounted for by the change in exports caused by the shock.[35] Consistent with quasi-random assignment, the predicted export shock (which is constructed using 2006–2007 to 2009–2010 changes in imports) does not predict differential sales or export growth prior to 2007. The coefficient on the export shock grows through 2009, and then plateaus at an effect size of roughly 20 percent of firms' 2007 sales volumes, consistent with the difference-in-difference results. Although the shock is only defined through 2010, the effects of this shock on sales are highly persistent, and firms' sales do not recover much by 2013. In each year, the effect on value added growth as a percentage of the 2007 level is roughly proportional to the effect on sales growth in each year.

4.3 Testing Idiosyncrasy

If our shock Δ_j^{id} is in fact idiosyncratic, then it should predict changes in sales or payroll at affected firms without systematically affecting other firms in the same labor market. As a direct

[34] 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 the data on factor payments are more reliable than the data on inventories in the data, 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.

[35] Some discrepancy is to be expected due to reporting thresholds in the export data. Berman et al. (2015) argue that due to internal economies of scale, domestic sales may be impacted to some degree by export shocks as well.

test, we study the effect of firm j's shock on the average outcome level among the firms most likely to be in firm j's labor market: those in both the same municipality and the same five-digit industry (the finest classification available to us). Specifically, we estimate regressions:

$$\bar{Y}_{-j,t} = \alpha_t + \delta_{-j} + \beta \hat{\Delta}_j^{id} \times Post_t + \nu_{-jt} \ , \ t \in \{Pre, Post\} \tag{14}$$

where $\bar{Y}_{-j,t}$ is the employment-weighted outcome among other firms in j's specified group, corresponding to one observation per treated firm j in the balanced panel.[36]

The results, presented in Table 4, highlight the importance of isolating the idiosyncratic component of export demand. While the idiosyncratic component has no effect on other firms, both the baseline export demand predictor Δ_j and the common component $\hat{\Delta}_j^{comm}$ predict changes in sales, value added, and payroll at other similar firms. The dynamics of these effects are displayed in Figure 6. We find similar results both when using other definitions of the labor market and when studying only never-exiter firms in the balanced panel. Since small changes in common, product-wide demand have large effects on many firms in an industry, only the differential cross-country adjustments *within* product groups constitutes an idiosyncratic shock.

5 Incidence on Employees

If the idiosyncratic variation in export demand in the 2008 recession affected individual firms, without systematically affecting the labor market as a whole, how did employers adjust employees' wages? We now examine effects on the employment and earnings of the *individuals* who were employed by shocked firms in 2007. To consistently benchmark the change in wages to the magnitude of the effect on firms' output demand, we estimate the pass-through 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. Formally, we estimate $\epsilon^{w,Y}$ using a difference-in-differences IV approach, where the first stage is the effect on sales from Equation (12), and the second stage regresses the change in cohort average wages $\bar{w}_{jt}$ on the change in sales $\tilde{Y}_j$ predicted by the shock $\hat{\Delta}_j^{id}$:

[36] Formally, for group g (for example NACE code, municipality or both) we define $\bar{Y}_{-j,t} \equiv \frac{\sum_{g=g(j),k\neq j} L_k^{2007} \times Y_{k,t}}{\sum_{g=g(j),k\neq j} L_k^{2007}}$, as the 2007-employment-weighted average outcome among all firms in the same group as j, but excluding j itself. Our results are robust to using a simple unweighted mean.

$$\bar{w}_{jt}^{incumb} = \alpha_t + \delta_j + \epsilon^{w,R}\tilde{Y}_j \times Post_t + \sum_{k \neq 2007} \gamma_k X_j^{pre} \times \mathbf{1}\{t = k\} + \nu_{jt} \ , \ t \in \{Pre, Post\} \tag{15}$$

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. The outcomes $\bar{w}_{jt}^{incumb}$ are the period-t wages among the cohort of individuals whose primary full-time job in 2007 was at affected firm j, regardless of whether or not the individuals are employed by j in period $t \neq 2007$. Studying effects on a fixed cohort ensures that wage changes are not driven by changes in the composition.[37] Although outcomes are defined at the individual level, the treatment *only* varies at the 2007-employer-level. We therefore collapse observations to cohort averages and implement these regressions at the cohort level. We weight observations by the number of 2007 cohort members so that the analysis approximates a worker-level regression with a cohort-level treatment, while applying a fixed weighting in all analyses.[38]

5.1 Reduced-Form Results

Although IV estimates help quantify the magnitude of incidence, the effects on employees can be seen clearly in the reduced form. First, although we find that continuing firms adjust employment in response to shocks, the shocks do not lead to increased separations or nonemployment of the individuals employed by shocked firms in 2007. In Figure 5 and Appendix Table A.3, we show that the full effect on employment in percentage terms can be explained by changes in hiring, using a simple decomposition of employment changes into separations of incumbents and accessions after 2007.

Table 5 displays the reduced-form difference-in-differences wage effects of both the baseline shock to exports and the rescaled shock to total demand $S_j^{pre} \times \Delta_j^{id}$. Although the shock to demand is idiosyncratic, we find that firms respond by adjusting the wages they pay employees. Moreover, this effect is driven by the wages of the incumbent workers hired before 2007. The longitudinal nature of the employer-employee data allows us to study outcomes for workers who

[37] We consider both specifications that include worker outcomes at any firm in the post period and specifications where only workers remaining at the 2007 are considered. The former permits identification of changes in rents that are realized by moving out of the treated firm

[38] When workers do not appear in the data in post-period years, they are not included in the cohort-average outcome in that observation year, thus the underlying sample of workers is not balanced. However, we consider specifications where missing observations are treated as zeros and incorporated into the average; results are not sensitive to this adjustment. We fix observation weights over years as the number of full time workers in the 2007 cohort.

move firms, including cases where the initial employer exits.[39] We find that wages change by approximately 0.025 to 0.04 log points per each percent change in export demand and by 0.12 to 0.15 percent per each percent change in total sales demand (in comparison to effects on log sales of 0.15–0.2 and 0.6–1, respectively). Figure 7 plots the dynamic effects of the export demand shock on incumbent employees' monthly contract wages at firms that never exit, using the dynamic reduced-form specification in (13). Although the shock impacts wage growth after the recession, wages evolve similarly across differently-shocked firms prior to 2007. However, results in Figure 6 indicate that wages of 2007 employees at other firms within the same industry and municipality do not change in the same way—though the common component of demand affects wages of employees at treated firms and rival firms alike. The effect is similar whether we examine the monthly full-time contract wage specified in the employment contract or the effective hourly wage based on hours worked in the reference month; this reflects a minimal adjustment in working hours in response to the shock. Likewise, we find similar effects whether we study individuals' full wage histories or limit to within-spell changes, suggesting that the first-order effects occur within the initial employment relationship. Meanwhile, we find no evidence of an effect on the wages of employees hired after 2007.[40]

Although we are studying a recession, we find no evidence that wage responses were constrained by downwards nominal wage rigidities. In Appendix Table A.5, we present results of tests for asymmetric wage effects of the demand shocks. When we include a quadratic effect of the baseline export demand shock, the coefficient on the linear term is unaffected and the coefficient on the quadratic term is negative and not statistically significant. When we estimate a spline with a knot at the median shock value (-0.017), we find a steeper slope below the knot, though we cannot reject the hypothesis that the slope is constant throughout. In addition, we plot the residualized binned scatter plot corresponding to our main difference-in-differences specification in Appendix Figure A.5, and we find that the effect is linear for all levels of the shock. However, these findings are consistent with significant downward nominal rigidity previously documented in Portugal (Carneiro et al., 2013). We show in the addendum to Table 5 that inflation and inertia continued to drive up

[39] In our baseline specifications, we study effects on wages of employees who were employed by shocked firms in 2007, regardless of where they work in all other observation years, but only so long as they appear in the employment data. Workers may attrit even when firms survive, and vice versa; however, we find no evidence of differential attrition in response to shocks (both overall and separately among high-earning and low-earning subgroups). We only consider wages at the single job where individuals earn the most in a given year.

[40] However, this latter effect is highly imprecise—since we find adversely shocked firms made few hires during the recession, we have few observations of post-recession starting wages at these firms.

the average wages of employed workers' during the observation period, both in real and nominal terms: Conditional on being employed, nominal wages of workers in the sample grew by 10 percent on average between the "pre" and "post" years.[41] While bad shocks may depress wage growth, the shock variation is not large enough to push wage growth below zero.[42]

5.2 IV Results

We quantify the degree of incidence on wages using IV estimates of the elasticity in Equation (15). Table 6 presents our benchmark estimates of pass-through of changes in log output (either sales or value added) on wages of the individuals employed in 2007 by shocked firms. We find elasticities ranging between 0.12 and 0.25, roughly the ratio of the reduced-form effects on wages and output. The results are similar whether we use sales or value added as a first-stage output measure and whether we study the monthly contract wage or the calculated hourly wage. The first stage is strongest—and the interpretation of the elasticity is the clearest—in the balanced sample of firms that always sell. In this case, we find an elasticity of wages to value-added of 0.15. However, we find the largest effects on wage in specifications that include wage outcomes for workers who switch firms and employers that eventually exit, suggesting wage effects that outlive job spells.

Table 7 shows how the elasticities we estimate vary across measures of wages and firm output. A limitation of the baseline specifications is that they are not defined for cohort members who become nonemployed or for cohorts whose 2007 employer exits. Table 7 also presents alternative specifications that admit zeros values for wages (inverse hyperbolic sine, the logarithm of $(1+wage)$, and the symmetric growth rate), firm output (percentage changes), or both.[43] In addition, we study within-spell wage effects conditional on remaining at one's initial employer. In general, our results are similar in all specifications and therefore do not appear to be sensitive to selective attrition. Results tend to be larger when we include firm and worker exits; however, both the magnitude and precision of these estimates are sensitive to alternative methods of accounting for zeros. Meanwhile, among the firms in the balanced panel, we find nearly the entire effect on wages is explained by

[41] This wage growth occurs for those who are employed. However, the picture is different when one treats non-employment as a zero wage, in which case the average wage change for 2007 incumbents was negative. Although nominal wage growth during this period was positive conditional on employment, the baseline incidence of employment was declining.

[42] Even applying our largest estimates, a one standard deviation (23.6 percent) decrease in export demand would depress wages by less than 2 percent

[43] We assign zero values to observation-years when either firms or workers are absent from the data. In the balance sheet data, a missing entry should reflect inactivity (actual zeros). By contrast, missing entries in the employee data are more difficult to interpret, since government jobs and self-employment are not reported in our data; accordingly, we are more cautious in interpreting wage measures that treat missing entries as zeros. Average wage growth for 2007 incumbent workers can be negative depending on how we treat these imputed zeros.

within-spell changes in earnings. We find similar results when we include fringe benefits in total hourly compensation; however, as noted above, these wage measures are noisier and estimates are more imprecise.

The benchmark results are also robust across alternative regression specifications presented in Table 8 and Appendix Table A.6. The results do not change significantly if we omit our baseline controls or include additional controls such as exposure to specific destination countries, 2007 firm and workforce attributes, predict export demand growth prior to 2007, or five-digit industry-by-year fixed effects. The point estimates are not sensitive to alternative weighting assumptions, though pass-through estimates are less precise when small firms receive higher relative weight. We also consider robustness to inclusion of very large firms. The results remain similar when large firms are included and firms are weighted equally, but they become small and insignificant when we include large firms and weight by employees. These latter specifications are dominated by noisy zero effects for large firms, which in part reflect lower degrees of idiosyncratic variation and weaker first stages among those larger firms.

Our reduced-form findings in Figure 7 display a high degree of persistence. This could be because the underlying demand shocks persist and workers bear incidence in each year demand remains affects, or it could be because there are scarring effects on wages that outlive the underlying shock to the firm. We characterize the relative importance of these two forms of persistence by estimating year-specific pass-through elasticities of sales effects to wages across given post-period years, using the same variation in the initial shock during the recession.[44] Figure 8 and Appendix Table A.7 display the results. Even though the largest effects on both sales and wages occur in 2011, the *elasticity* is generally constant across years. Thus, the incidence on wages does not dissipate any faster than the shock itself.

We are also interested in the incidence of the common component of demand $\hat{\Delta}^{comm}$, which reflects changes in product demand worldwide. In practice, however, the effects of common shocks were qualitatively different than the effects of idiosyncratic shocks. In our reduced-form analysis, presented in Appendix Table A.8, we find that the common component of demand affects domestic and export sales alike—total sales change one-for one *without* rescaling by firms' export intensities. Moreover, these shocks have important effects both on firm exit and on the probability that 2007

[44] Formally, we estimate Equation (15) treating 2007 as the *pre* year and a single year $t \in \{2008, 2009, 2010, 2011, 2012, 2013\}$ as the post year—however, in all cases, we use the same shock Δ_j^{id} defined based on demand changes between 2006–2007 and 2009-2010.

incumbent employees exit the data completely. Since one cannot observe the underlying demand (or "potential sales") level for firms that exit the data or wages for nonemployed workers, it is difficult to assess how the incidence of these shocks on wages compares to the incidence of the idiosyncratic shocks. IV results in in Appendix Table A.9 are highly sensitive to how one accounts for exit and non-employment.

5.3 Heterogeneity

The incidence of idiosyncratic demand shocks on incumbent employees wages that we observe implies that employers face significant turnover costs and that these costs—and employees threat points—rise and fall with demand growth. Yet, turnover costs may vary across workers within firms. For example, 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., 2018). Accordingly, we estimate pass-through elasticities separately for members of different subgroups within our sample of 2007 incumbent employees.[45]

Results are presented in Table 9. In practice, we find pass-through effects for most types of workers. Interestingly, 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.[46] We do, however, find that significant incidence only occurs for "attached" employees who were employed by the shocked firm in each of 2005, 2006, and 2007. Workers hired after in 2006 or 2007—who have the highest baseline separation hazards—bear little incidence of shocks. This result is similar to our earlier finding that shocks do not affect wages of post-2007 new hires and suggests that market power in the employment relationship increases over the first few years of a job. These results are consistent with models where employers bargain with incumbent employees with firm-specific training (Becker, 1962; Jovanovic, 1979a; Lazear, 2009) or institutional protections that become stronger after a probationary period, but they are harder to

[45] If different types of workers are employed by different types of firms, estimated differences in pass-through to different types of workers may reflect heterogeneity across firms rather than heterogeneity across workers within firms. To mitigate this concern, we use the same baseline employment weights used in our main analysis in all subgroup specifications. This ensures our results do not inadvertently place higher weight on different subsets of firms. In Appendix Table A.10, we present alternative specifications where the weights on firm-subgroup-cohorts are allowed to vary depending on the number of employees in the firm-subgroup-cohort.

[46] Importantly, fixed-term contract employees are not employed "at-will", and employees 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.

reconcile with canonical wage-posting models where new hires and incumbent employees are treated symmetrically. To isolate heterogeneous effects across other subgroups, we focus on cross-group differences among "attached" workers. We find slightly higher rates of pass-through onto men than onto women, but the differences are sensitive to specification. Comparing workers' 2007 wages to the 2007 median at their firm, we divide individuals into high-wage and low-wage groups. The elasticities are similar for both groups, though slightly larger for the high-wage workers.[47] Finally, among firms that never exit, workers with or without high school diplomas are similarly affected by employer shocks. These findings suggest that both lower-skill and higher-skill workers have similar holdup power in the firm.[48]

Turnover costs and employee holdup power may also differ across firms and industries. The underlying holdup power of employees (the turnover cost C_j in our framework) is not directly observable; however, in most models with employee turnover costs, higher turnover costs lead to lower quit rates and longer spells.[49] 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.[50] 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.[51] To characterize the frictions in five-digit industries, we measure the pre-period separations rates of permanent contract workers, as well as their typical job tenures.[52] We study heterogeneity in pass-through by dividing the analysis sample

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

[48] The similar hold-up power of low-skill and high-skill workers in Portugal is likely in part a reflection of the strong employment protections that apply to all full-time employees alike.

[49] 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, in 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 have substantial option value to maintaining a relationship once a good match is made.

[50] 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.

[51] We characterize industries based on permanent contract employee turnover, since 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).

[52] We calculate the typical tenure as the employment-weighted average of firm-median permanent employee tenure in 2003–2007, and the turnover rate as the ratio of total separations to the total number permanent workers across averaged across 2003–2007—both are calculated only for firms *outside* our analysis sample in each five-digit sector. We omit sample firms to avoid incorporating mean-reverting behaviors of firms in the sample. To calculate the degrees of fluidity most likely to characterize the sample firms, we calculate these averages for all firms with 100 employees or

into two equally sized subsamples with above-sample-median and below-sample-median levels of a specified industry turnover metric, and then by estimating an interacted version of our primary difference-in-differences IV specification:

$$\bar{w}_{jt}^{incumb} = \begin{array}{l} \alpha_t + \delta_j + \epsilon_H^{w,Y} Y_j \times Post_t \times Hi_j + \epsilon_L^{w,Y} Yj \times Post_t \times Lo_j \\ +\gamma^H \times Post_j \times Hi_j + \sum_{k \neq 2007} \gamma_k X_j^{pre} \times \mathbf{1}\{t = k\} + \nu_{jt} \end{array} \quad t \in \{Pre, Post\} \tag{16}$$

where Hi_j denotes an indicator for above-median ("high") level of the industry turnover cost proxy and $Lo_j = 1 - Hi_j$ is the complementary indicator. We estimate separate elasticities for each subgroup by interacting the treatment with each indicator and omitting the main effect of the treatment, always controlling for the main effect of Hi_j in interacted specifications. 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 $Y_j \times Post_t \times Hi_j$ using $\hat{\Delta}_j^{id} \times Post_t \times Hi_j$ and likewise for the interaction with Lo_j.

The results in 10 indicate that firm-specific demand does impact wage growth more when employee turnover is lower. Not only do wages respond more to shocks in sectors with lower separations, they also respond more than 2.5 times as much in these sectors *per realized percent change in output*. These differences are even more pronounced for workers with permanent contracts. In sectors with higher turnover of permanent contract employees, we find zero pass-through of idiosyncratic demand shocks to wages; by contrast, we find pass-through elasticities above 0.26 in sectors with low turnover.[53] We find qualitatively similar but smaller differences across industries with different typical tenure lengths. These findings suggest firm-level demand is more important for wage determination when markets are less fluid. However, these differences across industries are not randomly assigned and may be correlated with other underlying sources of heterogeneity across firms and sectors.

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 pay premiums reflect higher levels of rent-sharing in some firms, one should expect wages to be more sensitive to firm-level demand in firms with higher pay premiums. Accordingly, we estimate an AKM model during the pre-recession period (2003–2007) identified by job-to-job moves

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.

[53] Only in this specification do we obtain estimates precise enough to rule out equality of the elasticities at a five-percent level.

of permanent contract employees, and we study pass-through heterogeneity using the specification in (16).[54] The results in Table 10 provide evidence wages are more sensitive to firm-specific demand shifts in firms with higher AKM fixed effects. We note that there is a strong relationship between AKM pay premiums and the employee-turnover measures used above: Appendix Figure A.6 shows that both firm-level and industry-level turnover rates are highly correlated with AKM firm pay premiums.

6 Discussion

Our estimated elasticity of 0.15 is significantly larger than those found in earlier work, surveyed by Manning (2011) and Card et al. (2018), that estimate similar elasticities using non-experimental variation in firms' output or productivity, and that report typical OLS elasticities of 0.06 or less.[55] 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 6 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.

This finding is largely consistent with earlier empirical work. The difference between our OLS and IV estimates is similar to the findings in Abowd and Lemieux (1993), who found no wage effect of *industry-wide* shocks in OLS but found large effects in IV analysis using industry-level trade shocks.[56] 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., 2018). While these findings may

[54] 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 are age and eduction controls 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.

[55] For example, in Italian data Card et al. (2013a) find a .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.

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

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.[57] 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. These findings imply that OLS is significantly attenuated: thus, significantly larger IV estimates are plausible.

These results 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). 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.[58]

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).[59] Even under the extreme assumption that *all* 2007 cross-firm differences in value added were exogenous and pass-through

[57]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.

[58]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.

[59]For this exercise, we estimates the same AKM models as in footnote 54, but estimate them 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.

through to wages with an elasticity 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, 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.

7 Conclusion

We find that employers make significant adjustments to wages in response to idiosyncratic product market shocks, even without changes in labor market competition. In particular, we found that an 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. In our framework, these findings suggest substantial barriers to employee turnover that give incumbent workers higher threat points during periods of higher demand. Indeed, these effects were primarily found in labor markets characterized by more durable employment relationships and lower fluidity.

These finding provide direct experimental evidence that *where* one works can make a significant difference in how much one is paid, regardless of one's skills, abilities, or efforts brought to the job. This work adds to a growing body of research that 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—although we focus on medium-run pass-through, such effects would nonetheless generate 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, Portugal, is characterized by very strong labor market protections—though many countries have similar institutions, other countries like United States (where employment is "at-will") have fewer protections. It therefore may 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 employers would respond to idiosyncratic demand shocks during better times.

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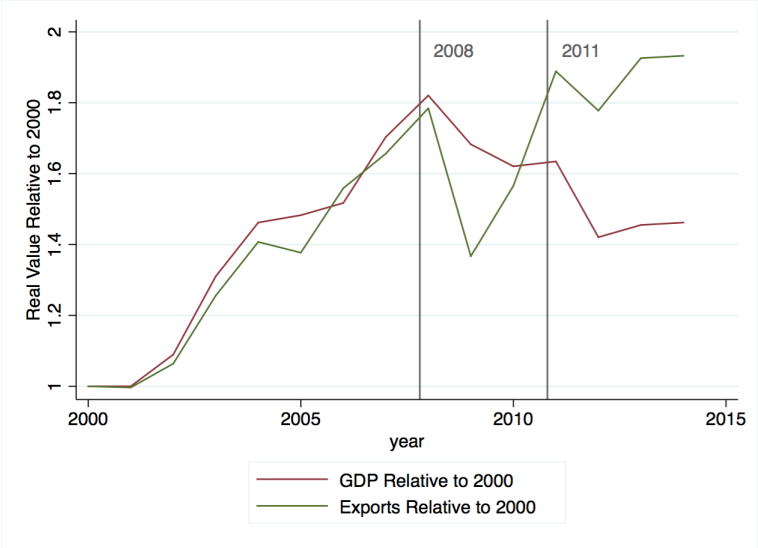
Figures and Tables

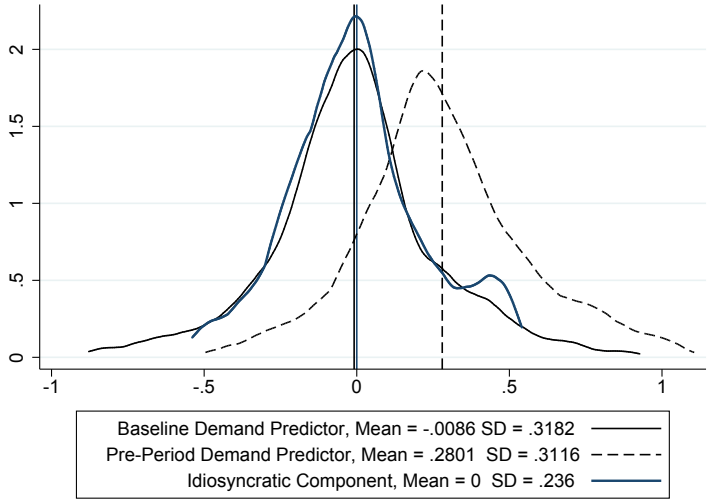
Figures

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



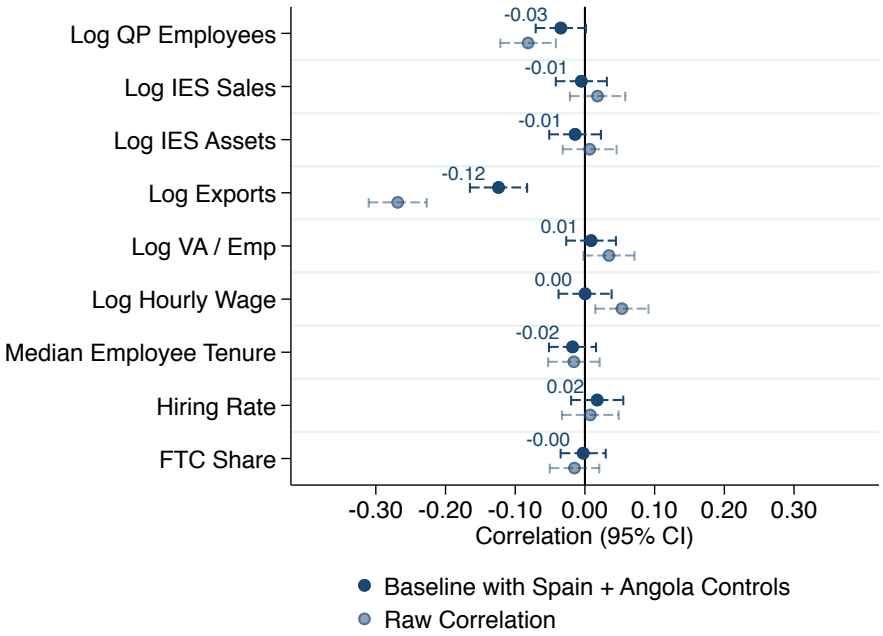
Source: World Bank (2016). Figure plots annual GDP and total exports for Portugal in real Euros, with each variable indexed to its 2000 level. Vertical lines indicate the start of the Great Recession in the US at the end of 2007 and the beginning of the Portuguese sovereign debt crisis in the spring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

Figure 2: Distribution of Demand Predictors and Idiosyncratic Component



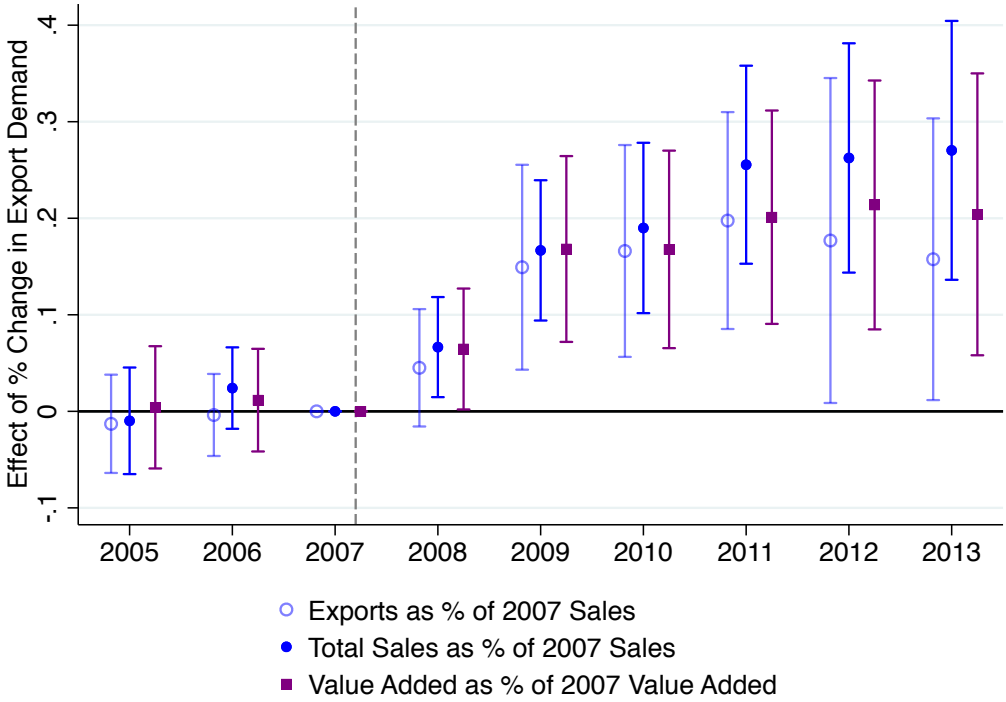
Notes: Figure displays kernel density plots, based on an Epanechnikov kernel, of total demand predictor Δ_j and the idiosyncratic demand shock Δ_j^{id}. Also displayed is the "pre-period" version of the total shock Δ_j^{03-07}, holding its exposure weights fixed as in the calculation of Δ_j, but using the symmetric growth rate from 2003-2004 to 2006-2007 of imports of product p to country c.

Figure 3: Correlation of Shock S_j with 2007 Covariates



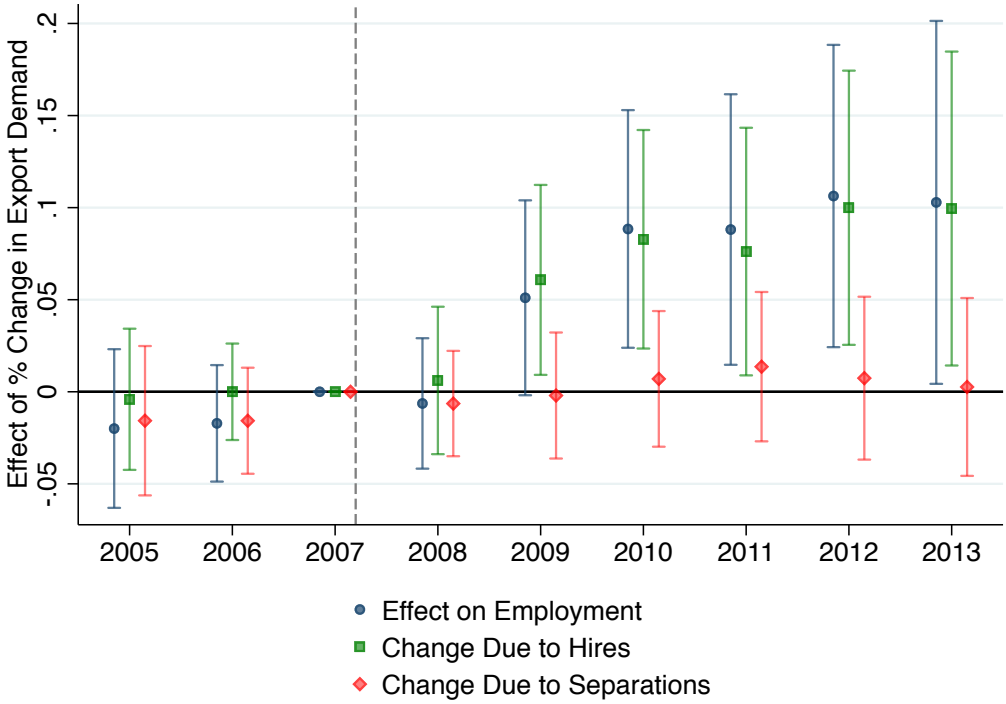
Notes: Figure displays standardized correlation coefficients between the 2007 level of the y-axis variable and the idiosyncratic shock $\hat{\Delta}_j^{id}$. Coefficients and confidence intervals are obtained from a standardized regression, either a simple bivariate regression or a regression with controls for baseline exposure of firms to each of Angola and Spain, as specified. Firms are weighted by pre-period average full-time employees, corresponding to the primary analysis.

Figure 4: Year-Specific Effects on Exports and Sales as Percent of 2007 Sales



Notes: Figure displays year-specific effects of the idiosyncratic shock to export demand $\hat{\Delta}_j^{id}$ on sales and exports in common Euro units, both as a percent of firms' 2007 total sales. Effects on value added in Euros as a percent of 2007 value added are displayed for comparison. Sample is balanced panel of firms that sell and employ at least one full-time worker in all years, N = 2,926. Year-specific coefficients and 95% confidence intervals are from regressions on interactions of $\hat{\Delta}_j^{id}$ 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 based on standard errors clustered at the firm level to account for potential serial correlation of errors. Exports are coded as zero in years when firms do not appear in the export data. Regressions are weighted by the average number of full-time employees in 2005, 2006, 2007. All regressions include year fixed effects, as well as controls for year-specific effects of 2005-2007 exports, log exports, the export share of sales, and the share of exports going to Spain or Angola in those years.

Figure 5: Year-Specific Effects on Employment Adjustment: Hiring Versus Incumbent Retention

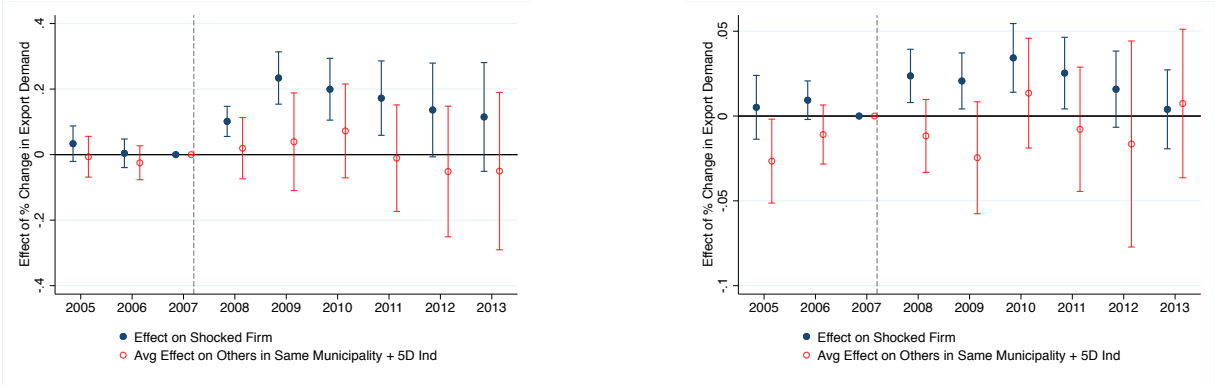


Notes: Figure displays year-specific effects of the idiosyncratic shock to export demand $\hat{\Delta}_j^{id}$ on on employment, and fully decomposes the main effect into two margins of adjustment. Sample is balanced panel of firms that sell and employ at least one full-time worker in all years, N = 2,926. Year-specific coefficients and 95% confidence intervals are from regressions on interactions of the $\hat{\Delta}_j^{id}$ 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 based on standard errors clustered at the firm level to account for potential serial correlation of errors. Outcomes are all tabulated from the employer-employee matched dataset, units are counts (based on full-time workers, including zeros) scaled by 2007 full-time employment at the firm. "Retention of 2007 Incumbents" is the percentage of 2007 incumbents present at the firm in the baseline year. "Accumulated hires" is the total number of hires made since 2007, less the number of new hires that have left the firm by the observation years, and is algebraically equal to the effect on employment less the effect on retention (prior to 2007 counts of hires are subtracted rather than added so this identity holds in all years). Regressions are weighted by the average number of full-time employees in 2007. All regressions include year fixed effects, as well as controls for year-specific effects of 2005-2007 exports, log exports, the export share of sales, and the share of exports going to Spain or Angola in those years.

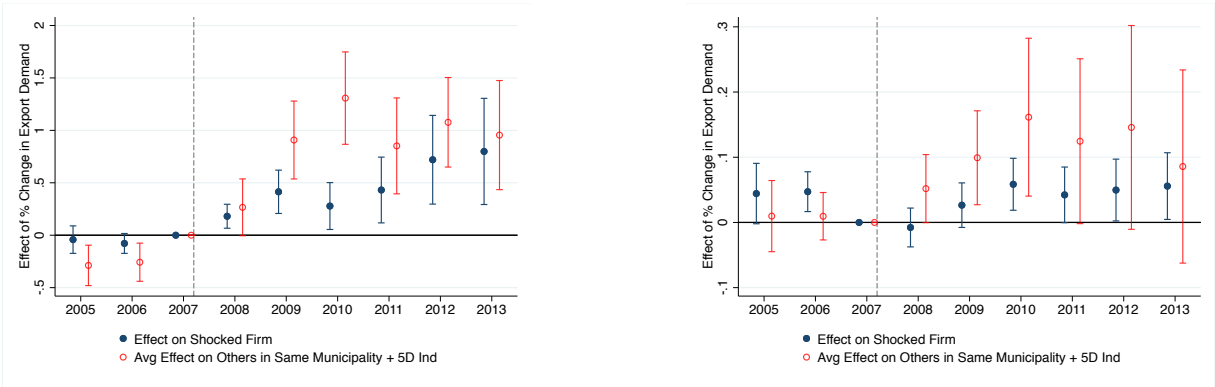
Figure 6: Effects of Export Shocks at Affected Firms and Other Similar Firms

Effect on Firm Sales **Effect on Monthly Contract Wage of 2007 Employees**

Idiosyncratic Component of Export Shock

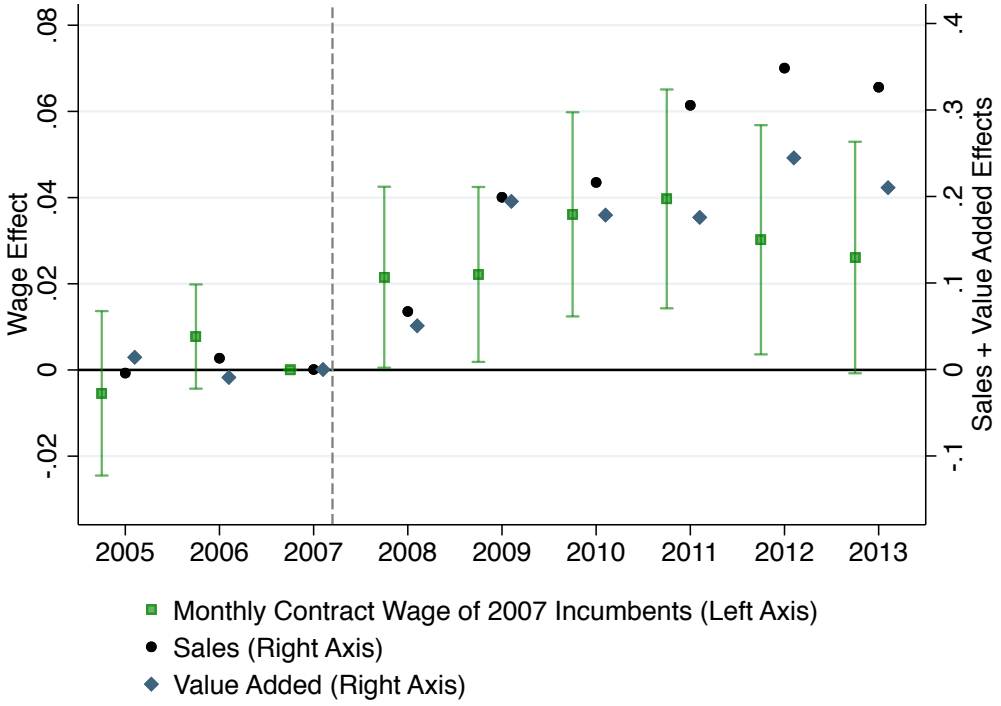


Common Component of Export Shock



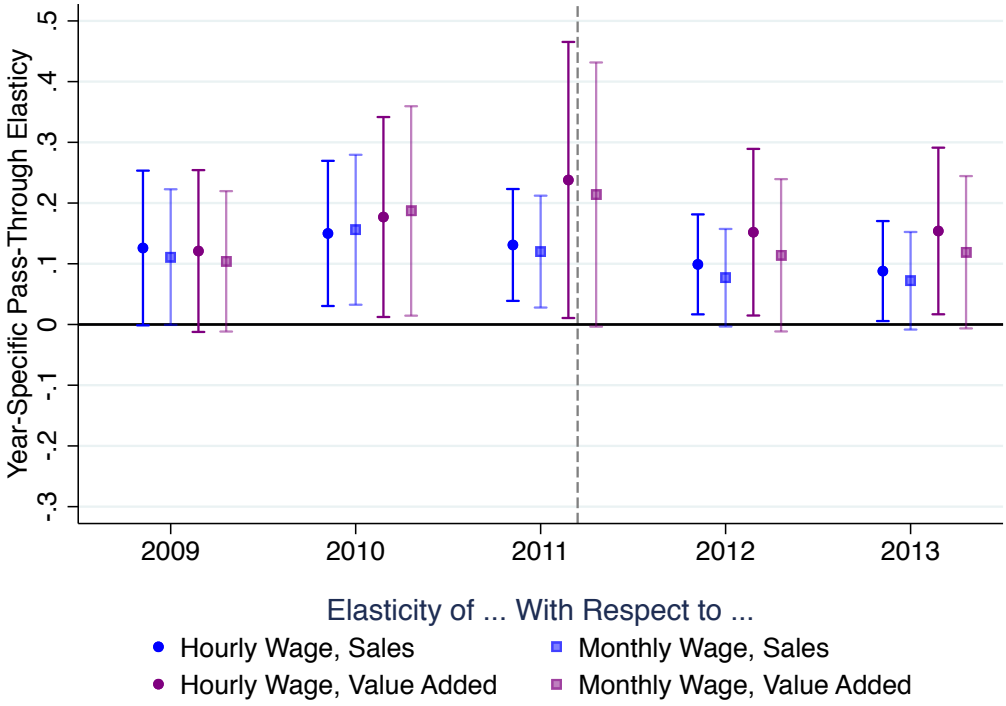
Notes: Figure shows year-specific effects of the idiosyncratic demand component $\hat{\Delta}_j^{id}$ and the common demand component $\hat{\Delta}_j^{comm}$ on outcomes for directly affected firms and for their 2007 employees, as well as for average outcomes for other firms (and 2007 employees of those firms) in the same five-digit industry and municipality. During the main post-period years (2009-2011), the idiosyncratic component affects labor demand at directly affected firms, but not other similar firms—however, the common component has similar effects on both the directly-affected firms and on other similar employers. The sample of affected firms is the balanced panel of firms that are employ at least one full-time worker in all years, N = 2,923. For this same sample, we calculate the 2007-employment-weighted average log sales of all other firms in the same 5-digit industry and the same municipality (one of 24 subregions in Portugal), as well as the weighted average log monthly contract wage of all workers employed by those firms in 2007. Figure displays year-specific coefficients from regressions of the form specified in equation (13). Estimates for each type of shock and each set of firms are from separate regressions. For comparability, we employ the same specification in (4) that omits all export controls when studying both own-firm and other-firm effects, since these controls are not defined for non-exporters. Regressions are weighted by the average number of affected-firm employees in 2007. Standard errors are clustered at the firm level for own-firm effects, and the 5-digit-industry-by-municipality level for the other-firm effects.

Figure 7: Dynamic Effects on Log Contract Wage of Attached Incumbents



Notes: Figure shows year-specific reduced-form effects of the idiosyncratic shock to export demand $\hat{\Delta}_j^{id}$ on the monthly contract wages for all individuals individuals whose primary full-time job (120-200 hours per month) was at one of the sample firms in the balanced panel of firms that never exit (N = 2,926). Since the treatment only varies at the 2007 employer level, we collapse all individual-level outcomes to cohort-level averages among cohort-members with outcomes defined in the given year (not necessarily constant), and weight regressions by 2007 cohort size. For comparison, year-specific effects on the log sales and log value added of the 2007 employer from Figure A.3 are plotted against the right y-axis. Year-specific coefficients and 95% confidence intervals are from regressions on interactions of the $\hat{\Delta}_j^{id}$ 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 based on standard errors clustered at the firm level to account for potential serial correlation of errors. Regressions are weighted by the average number of full-time employees in 2007. All regressions include year fixed effects, as well as controls for year-specific effects of 2005-2007 exports, log exports, the export share of sales, and the share of exports going to Spain or Angola in those years.

Figure 8: Year-Specific Pass-Through of Idiosyncratic Recession Shock



Notes: Figure displays estimates of year-specific pass-through elasticities $\epsilon^{w|Y}$, which coefficients on the interaction of the output measure Y_j with a post period indicator $Post_t$ in (15). Each estimate is from a separate regression where $Post_t$ is defined as zero for $t = 2007$ and one in the year specified in the row; all other years are omitted. Elasticities are estimated using the *same* idiosyncratic shock $\hat{\Delta}_j^{id}$—defined as the 2006–2007 to 2009–2010 change in export demand–as an instrument for the output measure Y_j. Results are also displayed in Appendix Table A.7. The sample of affected firms is restricted to the balanced panel of firms that are employ at least one full-time worker in all years, N = 2,923. Firms are weighted by the total number of attached incumbents present in pre-period, weights are fixed across years. Regressions are weighted by the average number of full-time employees in 2007. All regressions include year fixed effects, as well as controls for year-specific effects of 2005-2007 exports, log exports, the export share of sales, and the share of exports going to Spain or Angola in those years. Standard errors are clustered at the firm level. ** indicates $p < .05$, * indicates $p < .10$.

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: Effects on Firm Sales and Output

	Panel A: Effects on Export and Survival							
Dependent Var	Log Exports		Any Exports		Any Sales		Any Employees	
	(1)	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*	*(7)*	*(8)*
	Effect of Baseline % Shock to Exports							
Δ^{id} x Post	0.475**	0.555**	0.015**	0.016**	-0.007	-	-0.005	-
	(0.15)	(0.16)	(0.036)	(0.025)	(0.028)	-	(0.03)	-
Mean Pre-Post Change	*-0.093*	*0.002*	*-0.214*	*-0.080*	*-0.088*	-	*-0.123*	-
N Firms	*4,173*	*2,926*	*4,173*	*2,926*	*4,173*	*2,926*	*4,173*	*2,926*
N Firm-Year Obs	*17,408*	*13,436*	*20,865*	*14,630*	*20,865*	*14,630*	*20,865*	*14,630*
Full Sample (Unbalanced)	x		x		x		x	
Never-Exiters (Balanced)		x		x		x		x

	Panel B: Effects on Output and Employment							
Dependent Var	Log Total Sales		Log Value Added		Log Payroll		Log Employment	
	(1)	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*	*(7)*	*(8)*
	Effect of Baseline % Shock to Exports							
Δ^{id} x Post	0.161**	0.234**	0.176**	0.186**	0.111**	0.126**	0.076*	0.095**
	(0.064)	(0.054)	(0.069)	(0.057)	(0.044)	(0.043)	(0.041)	(0.038)
	Effect of Rescaled % Shock to Sales							
Δ^{id} x S x Post	.499**	0.955**	0.685**	0.960**	0.400**	0.544**	0.186	0.330*
	(0.312)	(0.232)	(0.286)	(0.249)	(0.185)	(0.197)	(0.172)	(0.183)
Mean Pre-Post Y Change	*-0.175*	*-0.041*	*-0.178*	*-0.079*	*-0.028*	*0.020*	*-0.078*	*-0.026*
N Firms	*4,173*	*2,926*	*4,142*	*2,923*	*4,173*	*2,926*	*4,173*	*2,926*
N Firm-Year Obs	*19,729*	*14,612*	*19,217*	*14,463*	*19,111*	*14,630*	*19,111*	*14,630*
Full Sample (Unbalanced)	x		x		x		x	
Never-Exiters (Balanced)		x		x		x		x

Notes: Sample is either full analysis sample ($N = 4,173$) 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. "Effect of Baseline Shock to Exports" estimates are coefficients on the interaction between the idiosyncratic shock to export demand $\hat{\Delta}_j^{id}$ and $Post_t$. "Shock to Sales" estimates are effects of $\hat{\Delta}_j^{id}$ interacted with the pre-period share of sales in exports $S_j \equiv \frac{Exports_j^{pre}}{Sales_j^{pre}}$; all specifications include $S_j \times Post_t$ controls. "Pre" years are 2006, 2007 (pre-period) and "Post" years 2009, 2010, 2011 (post-period). 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. Regressions are weighted by the average number of full-time employees in 2007. All regressions include year fixed effects, as well as controls for year-specific effects of 2005-2007 exports, log exports, the export share of sales, and the share of exports going to Spain or Angola in those years. Table also presents the average (real) change in the dependent variable from pre-to-post. Standard errors are clustered at the firm level. ** indicates $p < .05$, * indicates $p < .10$.

Table 4: Test of Effects of Idiosyncratic and Common Components on Other Firms

	Effect of Δ^{id} on Mean Outcome Level for Other Firms in Same:								
	5-Digit Industry and Municipality			5-Digit Industry			Municipality		
	(1)	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*	*(7)*	*(8)*	*(9)*
Log Sales	0.175	0.039	1.067	.135	-0.079	1.610	-0.002	-0.012	0.040
	(0.051)**	(0.050)	(0.223)**	(0.098)	(0.101)	(0.317)**	(0.005)	(0.007)*	(0.025)
Log Value Added	0.246	-0.022	1.84	0.165	-0.112	1.981	-0.003	-0.007	0.010
	(0.080)**	(0.072)	(0.352)**	(0.100)*	(0.098)	(0.387)**	(0.005)	(0.007)	(0.016)
Log Payroll	0.049	-0.010	0.452	-0.003	-0.107	0.706	-0.003	-0.008	0.019
	(0.024)**	(0.027)	(0.080)**	(0.073)	(0.081)	(0.243)**	(0.003)	(0.004)**	(0.014)
Log Employees	0.032	-0.008	0.300	0.027	-0.007	.274	-0.001	-0.004	0.010
	(0.019)*	(0.022)	(0.064)**	(0.028)	(0.032)	(0.092)**	(0.002)	(0.003)	(0.009)
Shock Component	*Total*	*Idiosyncratic*	*Common*	*Total*	*Idiosyncratic*	*Common*	*Total*	*Idiosyncratic*	*Common*

Notes: Table displays results from regressions of the leave-one-out (employment-weighted) average outcome level of all other firms the same specified group as each treated firm j (excluding j itself) on the demand shock to j, for all firms j in the the baseline sample with at least one other firm in the specified grouping. Results pertaining to subset of treated firms in balanced panel of never-exiter are displayed in Appendix Table A.4. Formally, for group g (for example NACE code, municipality or both) we define the outcome $\bar{Y}_{-j,t} \equiv \frac{\sum_{g=g(j),k\neq j} L_k^{2007} \times Y_{k,t}}{\sum_{g=g(j),k\neq j} L_k^{2007}}$, as the 2007-employment-weighted average outcome among all firms in the same group as j, but excluding j itself. Entries are regression coefficients on the interaction between the idiosyncratic shock S_j and $Post_t$, corresponding to β in equation (14) in the text; each estimate is from a separate regression. Regressions are run at treated firm level; regressions are weighted by firm-j employment to match specification in Table 3 but results are robust to using a simple unweighted mean. Specification includes year and treated-firm-group fixed effects, but no other controls. Municipalities are 24 subregions in Portugal; five-digit industry code is the most detailed NACE classification available. Standard errors are clustered at the treated-firm level. ** indicates $p < .05$, * indicates $p < .10$.

Table 5: Reduced-Form Effects on Wages

	Average Log Wage, All Employees		2007 Employees, Within Spell		2007 Employees, Including Movers		Post-2007 Hires	
Log Wage:	Monthly	Hourly	Monthly	Hourly	Monthly	Hourly	Monthly	Hourly
	(1)	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*	*(7)*	*(8)*
				A. All Firms (Unbalanced Sample of 4173 Firms)				
As Shock to Exports:								
Δ^{id} x Post	0.027**	0.017	0.025**	0.023**	0.043**	0.042**	-0.036	-0.023
	(0.012)	(0.012)	(0.010)	(0.010)	(0.11)	(0.10)	(0.048)	(0.038)
As Shock to Sales:								
Δ^{id} x S x Post	0.136**	0.089**	0.103**	0.103**	0.182**	0.174**	-0.165	-0.142
	(0.049)	(0.042)	-0.043	-0.043	-0.041	-0.042	(0.211)	(0.159)
	4173	*4173*	*4155*	*4155*	*4163*	*4163*	*3678*	*3678*
				B. Never-Exiter Firms Only (Balanced Sample of 2926 Firms)				
As Shock to Exports:								
Δ^{id} x Post	0.024*	0.015	0.031**	0.028**	0.028**	0.029**	-0.069	-0.049
	(0.013)	(0.012)	-0.012	-0.012	-0.11	-0.11	(0.053)	(0.042)
As Shock to Sales:								
Δ^{id} x S x Post	0.141**	0.088*	0.154**	0.112**	0.142**	0.121**	-0.098	-0.143
	(0.054)	(0.047)	-0.048	-0.047	-0.042	-0.043	(0.236)	(0.178)
N Firms	*2926*	*2926*	*2920*	*2920*	*2922*	*2922*	*2749*	*2749*

Addendum: Mean Pre-Post Hourly Wage Change in 2007 Cohort

	Ommitting Missing Observations		*Treating Missing as Zero*	
	Log Change, Nominal	Log Change, Real	Symmetric Growth	Inv. Hyp. Sine
All Firms	*0.103*	*0.065*	*-0.365*	*-0.189*
Never-Exiters	*0.102*	*0.071*	*-0.203*	*-0.107*

Notes: Table presents wage effects, both for workers employed by shocked firms j in observation year and also for all individuals individuals whose primary full-time job (120-200 hours per month) was at a treated firm in 2007—either at any firm, regardless of firm exit, or at a never-exiter firm in the balanced panel. Since the treatment only varies at the 2007 employer level, we collapse all individual-level outcomes to cohort-level averages among cohort-members with outcomes defined in the given year (not necessarily constant), and weight regressions by 2007 cohort size. Each point estimate is obtained from a separate regression. "Shock to Exports" effect estimates are coefficients on the interaction between the idiosyncratic shock to export demand $\hat{\Delta}_j^{id}$ and $Post_t$. "Shock to Sales" estimates are effects of $\hat{\Delta}_j^{id}$ interacted with the pre-period share of sales in exports $S_j \equiv \frac{Exports_j^{pre}}{Sales_j^{pre}}$; all specifications include $S_j \times Post_t$ controls. "Pre" years are 2006, 2007 (pre-period) and "Post" years 2009, 2010, 2011 (post-period). "Wages of 2007 Employees, Including Movers" is defined for full-time jobs at individual's highest paying job in a year, regardless of whether or not the employer is the same as the 2007 employer; "Within-Spell" is only defined for workers in years they are employed at their 2007 firm. Worker-year observations with missing data are omitted from cohort averages in that year. "Post-2007 Hires" in columns 7 and 8 exclude workers who were present at the firm in 2007. See Appendix B for further details on outcome construction. Table also presents the average change in wages from pre-to-post under alternative specifications. Regressions are weighted by the average number of full-time employees in 2007. All regressions include year fixed effects, as well as controls for year-specific effects of 2005-2007 exports, log exports, the export share of sales, and the share of exports going to Spain or Angola in those years. Standard errors are clustered at the firm level. ** indicates $p < .05$, * indicates $p < .10$.

Table 6: Pass-Through Elasticity: Effect in Wages for Given Change in Output

Outcome:	Log Monthly Wage		Log Hourly Wage	
	(1)	*(2)*	*(3)*	*(4)*
A. All Firms (Unbalanced Sample of 4173 Firms)				
IV Elasticity	0.227**	0.202**	0.233**	0.215**
	(0.110)	(0.091)	(0.113)	(0.096)
First stage F	*10.39*	*20.62*	*10.39*	*20.62*
OLS Elasticity	0.015**	0.015**	0.012**	0.011**
	(0.002)	(0.003)	(0.002)	(0.002)
N Workers	*116,258*	*115,218*	*116,258*	*115,218*
N Firms	*4,161*	*4,127*	*4,161*	*4,127*
N Cohort-Yr Obs	*19,374*	*18,899*	*19,374*	*18,899*
B. Never-Exiter Firms Only (Balanced Sample of 2926 Firms)				
IV Elasticity	0.122**	0.143**	0.123**	0.150**
	(0.050)	(0.066)	(0.050)	(0.066)
First stage F	*86.31*	*39.42*	*86.31*	*39.42*
OLS Elasticity	0.023**	0.019**	0.020**	0.017**
	(0.005)	(0.003)	(0.005)	(0.003)
N Workers	*85,229*	*84,955*	*85,229*	*84,955*
N Firms	*2,922*	*2,907*	*2,922*	*2,907*
N Cohort-Yr Obs	*14,518*	*14,371*	*14,518*	*14,371*
Output Measure				
Log Sales	x		x	
Log Value Added		x		x

Notes: Table displays estimates of pass-through elasticities, obtained from difference-in-difference regressions of average incumbent log monthly wages on the specified outcome variable; elasticity is coefficient $\epsilon^{w|Y}$ on the interaction of the output measure Y_j with the post period indicator $Post_t$ in (15). Elasticities are estimated from Equation (15) by OLS or instrumental variables (two stage least squares) estimation using the idiosyncratic shock $\hat{\Delta}_j^{id}$ as an instrument for the output measure Y_j. Output measure is either log total sales or log value added, as specified; value added is calculated as total factor payments (labor costs plus firm earnings before interest, depreciation, amortizations,and taxes). Sample includes for all individuals whose primary full-time job (120-200 hours per month) in 2007 was at a treated firm—either at any firm, regardless of firm exit, or at a never-exiter firm in the balanced panel. Since the treatment only varies at the 2007 employer level, we collapse all individual-level outcomes to cohort-level averages among cohort-members with outcomes defined in the given year (not necessarily constant), and weight regressions by 2007 cohort size, displayed in *N Workers* rows. Log wage outcomes are defined for full-time jobs at individual's highest paying job in a year, regardless of whether or not the employer is the same as the 2007 employer; worker-year observations with missing data are omitted from cohort averages in that year. Log-log specifications drop cohorts-year observations when shocked firms have exited. Firms are weighted by the total number of attached incumbents present in pre-period, weights are fixed across years. Regressions are weighted by the average number of full-time employees in 2007. All regressions include year fixed effects, as well as controls for year-specific effects of 2005-2007 exports, log exports, the export share of sales, and the share of exports going to Spain or Angola in those years. Standard errors are clustered at the firm level. ** indicates $p < .05$, * indicates $p < .10$.

Table 7: Alternative Elasticity Specifications

	Within Spell			Including Moves			Including Movers and Zero Earnings		
Outcome:	Log Mth. Wage	Log Hr. Wage	Log Hr Tot Comp.	Log Mth. Wage	Log Hr. Wage	Log Hr Tot Comp.	arcsinh (Hr Wage)	Log (1+Hr wage)	%Δ Hr Wage
	(1)	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*	*(7)*	*(8)*	*(9)*
A. All Firms (Unbalanced Panel of 4,178 Firms)									
Pass-through of:									
Log Sales	0.131**	0.119*	0.129	0.227**	0.233**	0.216*	0.275*	0.185**	0.309
	(0.064)	(0.062)	(0.087)	(0.110)	(0.113)	(0.123)	(0.155)	(0.091)	(0.204)
Log Value Added	0.138*	0.127*	0.135	0.202**	0.215**	0.193*	0.310**	0.171**	0.351*
	(0.074)	(0.072)	(0.101)	(0.091)	(0.096)	(0.103)	(0.156)	(0.077)	(0.194)
Sales Pct Change	0.166**	0.151**	0.165	0.323**	0.320**	0.298**	0.402**	0.255**	0.373
	(0.079)	(0.077)	(0.109)	(0.124)	(0.126)	(0.134)	(0.185)	(0.101)	(0.259)
VA Pct Change	0.161**	0.148*	0.158	0.240**	0.255***	0.228**	0.372**	0.203**	0.422*
	(0.081)	(0.078)	(0.112)	(0.090)	(0.093)	(0.105)	(0.171)	(0.075)	(0.223)
B. Never-Exiter Firms Only (Balanced Sample of 2926 Firms)									
Pass-through of:									
Log Sales	0.134**	0.120**	0.134*	0.122**	0.123**	0.093*	0.147*	0.099**	0.221*
	(0.057)	(0.054)	(0.079)	(0.050)	(0.050)	(0.050)	(0.088)	(0.041)	(0.115)
Log Value Added	0.162**	0.150**	0.159*	0.143**	0.150**	0.100*	0.168	0.119**	0.254*
	(0.068)	(0.073)	(0.095)	(0.066)	(0.066)	(0.055)	(0.114)	(0.054)	(0.150)
Sales Pct Change	0.162**	0.144**	0.161	0.148**	0.149**	0.112*	0.175	0.119**	0.266*
	(0.076)	(0.065)	(0.104)	(0.059)	(0.059)	(0.060)	(0.107)	(0.049)	(0.141)
VA Pct Change	0.175**	0.161**	0.174	0.155**	0.161**	0.112*	0.177	0.129**	0.268*
	(0.078)	(0.075)	(0.108)	(0.069)	(0.068)	(0.061)	(0.121)	(0.056)	(0.159)

Notes: Table displays IV estimates of the same elasticity specifications in Table 6 under alternative variable definitions, see notes in that table for additional details. When the output change measure is a percent change, zeros are admitted and the first-stage outcome is defined in all years. In log wage specifications worker-year observations with missing data are omitted from cohort averages in that year; however, the outcomes in Columns 7–9 treat missing wage observations as zeros and are always defined. Firms are weighted by the total number of attached incumbents present in pre-period, weights are fixed across years. Regressions are weighted by the average number of full-time employees in 2007. All regressions include year fixed effects, as well as controls for year-specific effects of 2005-2007 exports, log exports, the export share of sales, and the share of exports going to Spain or Angola in those years. Standard errors are clustered at the firm level. ** indicates $p < .05$, * indicates $p < .10$.

Table 8: Robustness of Pass-Through Estimates

	Outcome: Log Hourly Wage, Any Job							
	(1)	*(2)*	*(3)*	*(4)*	*(5)*	*(7)*	*(8)*	*(9)*
Sales Elasticity IV	0.123**	0.099*	0.095*	0.123**	0.144	0.141	0.072	0.095*
	(0.050)	(0.046)	(0.046)	(0.054)	(0.077)	(0.285)	(0.046)	(0.051)
N firms	*2,922*	*2,922*	*2,922*	*2,922*	*2,802*	*3,585*	*2,922*	*3,585*
N workers	*85,229*	*85,229*	*85,229*	*85,229*	*81,716*	*318,814*	*85,229*	*318,814*
N Cohort-Year Obs	*14,518*	*14,518*	*14,518*	*14,518*	*13,916*	*17,820*	*14,518*	*17,820*
Value Added Elasticity IV	0.150**	0.120*	0.136**	0.152**	0.172*	0.027	0.090	0.094*
	(0.066)	(0.062)	(0.069)	(0.073)	(0.096)	(0.039)	(0.061)	(0.053)
N firms	*2,918*	*2,918*	*2,918*	*2,918*	*2,798*	*3,578*	*2,918*	*3,578*
N workers	*84,955*	*84,955*	*84,955*	*84,955*	*81,438*	*209,386*	*84,955*	*209,386*
N Cohort-Year Obs	*14,371*	*14,371*	*14,371*	*14,371*	*13,765*	*17,613*	*14,371*	*17,613*
Reduced Form Effect of Shock	0.029**	0.020**	0.021**	0.027**	0.021**	0.006	0.015	0.017**
	(0.011)	(0.009)	(0.010)	(0.011)	(0.010)	(0.009)	(0.010)	(0.085)
N firms	*2,922*	*2,922*	*2,922*	*2,922*	*2,802*	*3,585*	*2,922*	*3,585*
N workers	*85,229*	*85,229*	*85,229*	*85,229*	*81,716*	*318,814*	*85,229*	*81,716*
	14,536	*14,536*	*14,536*	*14,536*	*13,932*	*17,837*	*14,536*	*13,932*
Baseline Controls	x		x	x	x	x	x	x
Pre-Period Attribute Controls			x					
Destination Controls				x				
5 Digit Industry FE					x			
Including Large Firms						x		x
Firm-Weighted							x	x

Notes: Table displays robustness of instrumental variables estimates in Table 6 to alternative specifications. Sample is limited to balanced panel of 2926 firms that never exit, Appendix Table A.6 presents corresponding results for sample including all firms including those that exit at some point during the sample frame. Column 1 displays IV estimates from Columns 3 and 4 of Panel B in Table 6, see table notes for details. Baseline controls are year fixed effects, as well as controls for year-specific effects of 2005-2007 exports, log exports, the export share of sales, and the share of exports going to Spain or Angola in those years. "Pre-period attribute controls" include controls for year-specific effects of 2005-2007 average employment, sales, assets, hiring, labor productivity, wage levels, and fixed term contract employment. Destination controls include the share of pre-period exports going to each of 10 top destination countries, as well as predicted demand using 2003-2007 changes in imports at baseline destinations. 5-digit industry FE includes industry-by-year fixed effects for 5-digit industry codes, the most detailed NACE classification available. "Firm-weighted" indicates estimation of regressions where all cohorts have equal weight of one. Standard errors are clustered at the firm level. ** indicates $p < .05$, * indicates $p < .10$.

Table 9: Pass-Through Elasticity: Subgroups of Workers

					Attached and …					
	Perm. Contract *(1)*	Fixed-Term Contract *(2)*	Recent Hire *(3)*	Attached *(4)*	Male *(5)*	Female *(7)*	Low Wage *(8)*	High Wage *(9)*	No HS Degree *(10)*	HS Degree *(11)*
				A. All Firms (Unbalanced Sample of 4173 Firms)						
Sales Elasticity IV	0.215**	0.232*	0.153	0.218**	0.120	0.092	0.156*	0.168*	0.269*	0.123
	(0.106)	(0.141)	(0.142)	(0.111)	(0.086)	(0.085)	(0.088)	(0.093)	(0.140)	(0.098)
Vaue Added IV	0.207**	0.265*	0.144	0.198**	0.126*	0.093	0.129*	0.167*	0.262**	0.141
	(0.097)	(0.142)	(0.121)	(0.094)	(0.075)	(0.079)	(0.066)	(0.086)	(0.124)	(0.099)
N workers	*90630*	*23300*	*25256*	*90427*	*50212*	*40215*	*46693*	*43734*	*68789*	*30093*
				B. Never-Exiter Firms Only (Balanced Sample of 2926 Firms)						
Sales Elasticity IV	0.128**	0.227**	0.065	0.140***	0.120**	0.056	0.098**	0.147**	0.146***	0.132*
	(0.054)	(0.106)	(0.082)	(0.053)	(0.061)	(0.060)	(0.048)	(0.060)	(0.055)	(0.076)
Vaue Added IV	0.157**	0.263*	0.085	0.169**	0.142*	0.065	0.127*	0.175**	0.185**	0.170
	(0.072)	(0.139)	(0.103)	(0.073)	(0.079)	(0.079)	(0.066)	(0.081)	(0.080)	(0.106)
N workers	*65999*	*17229*	*18670*	*65929*	*37502*	*28427*	*33950*	*31979*	*50003*	*22119*

Notes: Table display pass-through elasticities corresponding to IV estimates from Columns 3 and 4 of Panel B in Table 6, pertaining to specific subgroups of workers. East estimate is obtained from a separate equation. Specification is identical to that in Table 6, except outcome is average log monthly wage taken only over the individuals working at shocked firms in 2007 who are members of the specified subgroup. Sample in each specification includes all 2007 cohorts in specified analysis sample with at least one 2007 incumbent full-time worker in the stated group. To maintain consistency across specifications, cohorts in all specifications are weighted by the *total* number of individuals in the 2007 cohort. We also analyze results where subgroup cohorts are weighted by the number of individuals in the 2007 cohort who are in the specified subgroup, results are displayed in Table A.10. Weights are fixed across years. "Attached" workers are those employed full-time at shocked firm j in each of 2005, 2006, and 2007; "recent hires" are employed in 2007 but not both of 2005 and 2006. "High-wage" and "Low-wage" indicate 2007 wage above/below 2007 firm median (taken over all workers, not just attached). Number of firms and incumbent workers included in each specification are displayed,All specifications include year fixed effects, as well as controls for year-specific effects of 2005-2007 exports, log exports, the export share of sales, and the share of exports going to Spain or Angola in those years. Standard errors are clustered at the firm level. ** indicates $p < .05$, * indicates $p < .10$.

Table 10: Heterogeneity by Industry Relationship-Durability

Friction Measure:	Separation Rate in Industry		Typical Tenure in Industry		Pre-Recession AKM Pay Premium	
	(1)	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*
Elasticity, All Workers:						
High Frictions	0.161**	0.166**	0.132**	0.174**	0.164**	0.174**
	(0.071)	(0.075)	(0.054)	(0.081)	(0.073)	(0.084)
Low Frictions	0.058	0.067	0.103	0.099	0.034	0.031
	(0.040)	(0.049)	(0.126)	(0.147)	(0.053)	(0.070)
Coeffs Equal, p-value	*0.207*	*0.271*	*0.835*	*0.662*	*0.135*	*0.176*
Elasticity, Permanent Contract						
High Frictions	0.267**	0.314**	0.144**	0.191**	0.127*	0.132*
	(0.113)	(0.147)	(0.060)	(0.091)	(0.069)	(0.078)
Low Frictions	0.004	0.007	0.080	0.077	0.047	0.054
	(0.059)	(0.077)	(0.128)	(0.141)	(0.055)	(0.073)
Coeffs Equal, p-value	*0.0373*	*0.0656*	*0.650*	*0.506*	*0.351*	*0.442*
Output Measure:						
Log(Sales)	x		x		x	
Log(VA)		x		x		x

Notes: Table displays estimates of subsample-specific pass-through elasticities, obtained from the interacted difference-in-difference regression specification in (16). Sample is limited to balanced panel of 2926 firms that never exit. 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 out-of-sample sample five-digit industry average of median tenure of permanent contract workers in 2003-2007. The second is the out-of-sample 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 54 for details. Interacted coefficients are estimated jointly, where interactions of the endogenous independent variable (Y_j) with the heterogeneity indicator are instrumented by interactions of the same indicator with $\hat{\Delta}_j^{id}$. All interacted specifications include controls for the categorical indicators times $Post_t$. Firms are weighted by the total number of 2007 employees, weights are fixed across years. All specifications include year fixed effects, as well as controls for year-specific effects of 2005-2007 exports, log exports, the export share of sales, and the share of exports going to Spain or Angola in those years. Standard errors are clustered at the firm level. ** indicates $p < .05$, * indicates $p < .10$.

A Theory Appendix

The framework in Section 2 is very general. In this appendix, we show how several standard models map into the the general wage equation (5) and elasticity (6), and discuss conditions in each model that govern how sensitive per-worker replacement costs are to product demand. We first develop a more detailed version of the model in the text that explicitly allows firms to adjust both employment and wages, and then survey a range of other models afterwards.

A.1 A Model of Wage Incidence in a Firm with Multiple Employees

Product Market Competition

We consider firms j that compete in monopolistic (or otherwise imperfectly competitive) product markets and thus face separate demand curve for a single product variety. A representative consumer with homothetic preferences of the following form: individuals have Cobb-Douglas over expenditure baskets in product groups (or industries) g

$$U = \sum_{g \in G} \alpha_g \ln E_g$$

where α_g is a product-group level demand shifter, and the expenditure basket E_p is a CES aggregator of differentiated sub-varieties $x_{p,j}$, each being produced by a different single-product firm j in product group g:

$$E_c = (\sum_{j \in J_g} \theta_{g,j} x_{g,j}^{\frac{\sigma-1}{\sigma}})^{\frac{\sigma}{\sigma-1}}$$

Here, J_g is the set of entrant firms producing in group g, $x_{g,j}$ is the quantity consumed of the sub-variety, σ is the elasticity of substitution between sub-varieties, and $\theta_{g,j}$ is an *idiosyncratic* taste for shifter for the sub-variety produced by firm j. In the representative-consumer model, this shifter represents all differences in demand for specific firms' products, not just tastes *per se*.

Following Krugman (1980), we assume firms face a fixed cost of entering to produce a variety, so that no two firms produce the same variety. Firms produce according to an increasing and convex function ϕ using a quantity of labor $L_{g,j}$ and a technical productivity shifter $\psi_{g,j}$, $x_{g,j} = \psi_{g,j} \times \phi(L_{g,j})$. Given customer optimization, each firm faces an inverse demand curve for its products given a price $P_{g,j}$:

$$P_{g,j} = x_{g,j}^{-\frac{1}{\sigma}} \frac{\theta_{g,j}^{\frac{1}{\sigma}} \alpha_g^{\frac{1}{\sigma}} I^{\frac{1}{\sigma}}}{P_g^{\frac{1-\sigma}{\sigma}}}$$

where I is the aggregate income level (constant for all firms in all product groups) and P_g is a product-group-level price index $P_g = (\sum_{j \in J_g} \theta_{g,j} p_{g,j}^{1-\sigma})^{\frac{1}{1-\sigma}}$. We assume there are a large number of firms in the product group, so that P_g is taken as exogenous by firms. Taking the demand curve as a constraint, firms therefore face the following *revenue* production function, which determines the

value of sales as a function of inputs alone:

$$Y_{g,j} = I^{\frac{1}{\sigma}} \times \frac{\alpha_g^{\frac{1}{\sigma}}}{P_g^{\frac{1-\sigma}{\sigma}}} \times \theta_{g,j}^{\frac{1}{\sigma}} \times \psi_{g,j}^{1-\frac{1}{\sigma}} \times (\phi(L_{g,j}))^{1-\frac{1}{\sigma}}$$

To see the connection to the revenue function in the main text we normalize units so that $I^{-\frac{1}{\sigma}} = 1$, then we let $\bar{P}_g \equiv \frac{\alpha_g^{-\frac{1}{\sigma}}}{P_g^{\frac{\sigma-1}{\sigma}}}$ be the industry-/product-group-level demand shifter (reflecting both consumer demand and the degree competition from other firms reflected in the price index), $p_{g,j} \equiv \theta_{g,j}^{\frac{1}{\sigma}}$ be the firm-level idiosyncratic demand shifter, $A_{g,j} \equiv \psi_{g,j}^{1-\frac{1}{\sigma}}$ be the shifter due to technical productivity, and $f(\cdot) \equiv (\phi(\cdot))^{1-\frac{1}{\sigma}}$ represent the conversion of labor inputs into revenue. Focusing on a single industry and dropping the subscript g, we obtain the firm's revenue function:

$$Y_j = \bar{P} \times p_j \times A_j \times f(L_j) \tag{A.1}$$

The single-worker revenue function in the main text corresponds to the case where $f(L) = \mathbf{1}\{L = 1\}$, and firms have identical productivity $A_j = 1 \ \forall \ j$. A direct implication of this formulation is that the incidence of idiosyncratic productivity shocks A_j is the same in the case of shocks to p_j. Accordingly, we suppress productivity A_j for simplicity in what follows.

In what follows, we assume the revenue function is homothetic with diminishing returns to scale, $f(L) - L^{1-\alpha}$, where $\alpha \in (0, 1)$, so that $Y_j(L; \bar{P}, p_j) = \bar{P} \times p_j \times L^{1-\alpha}$,

Wage Determination

We model wage determination in a single-period variant of the model in Acemoglu and Hawkins (2014). Firms begin a single period with a large stock of employees L_j^0. At the start of the period, shocks to $\bar{P}$ and p_j are realized, and a share $1-\delta$ of workers retire. The firm can recruit H_j additional workers at a cost $c(H_j)$ before production occurs, so that the total mass of workers available for production is $L_j =(\delta L_j^0 + H_j)$. For simplicity, we assume that δ is large enough that firms always hire $H_j > 0$.

If a worker leaves the firm after recruiting is complete, she cannot be replaced before production begins. To characterize wage bargaining, we denote the value of the firm at this stage be $J(L; \bar{P}, p_j) = Y_{g.j}(L; \bar{P}, p_j) - w_j(L; \bar{P}, p_j)L$. If a worker leaves, the loss to the firm is the foregone marginal revenue product of labor ($MRPL$) less the wage paid, plus any additional wages paid to other employees due to changes in their bargaining position: $J_L(L; \bar{P}, p_j) = Y_L(L; \bar{P}, p_j) - w(L; \bar{P}, p_j) - w_L(L; \bar{P}, p_j) \times L$. Meanwhile, a worker who leaves can obtain an outside option worth $\bar{w}(\bar{P})$. Since alternative opportunities depend on the level of labor demand among rival employers in the labor market, and labor demand is derived from product demand, growth in the common component of product demand can directly impact the outside option.

Wages are determined according to the Stole and Zwiebel (1996) multilateral bargaining solution, which corresponds to individuals striking a Nash bargain with their employer, which is renegotiated after any other employee leaves and thereby impacts the $MRPL$ of remaining employees. Formally, letting β be the worker's Nash bargaining weight, the settlement wage satisfies the differential

equation

$$\beta \times J_L(L; \bar{P}, p_j) = (1-\beta) \times [w(L) - \bar{w}(\bar{P})] \tag{A.2}$$

at all values of L. This recursively defines the symmetric wage paid to all employees in the bargaining equilibrium where no all workers accept a settlement wage:

$$w(L) = (1-\beta)\bar{w}(\bar{P}) + \beta \times \frac{\int_0^L n^{\frac{1-\beta}{\beta}} Y_L(L; \bar{P}, p_j) dn}{\int_0^L n^{\frac{1-\beta}{\beta}} dn} \tag{A.3}$$

Given our assumption that Y is homothetic, this can be solved analytically as

$$w(L) = (1-\beta)\bar{w}(\bar{P}) + \gamma \times Y_L(L; \bar{P}, p_j) \tag{A.4}$$

where $\gamma \equiv \frac{\beta}{1-\beta+(1-\alpha)\beta}$. Equivalently, the wage can be expressed as:

$$w(L) = \underbrace{\bar{w}(\bar{P})}_{\text{Outside Option}} + \beta \times \underbrace{(\tilde{\gamma} \times MRPL(L; \bar{P}, p_j) - \bar{w}(\bar{P}))}_{\text{Rent}} \tag{A.5}$$

where $MRPL(L; \bar{P}, p_j) = Y_L(L; \bar{P}, p_j)$ and $\tilde{\gamma} \equiv \frac{1}{1-\beta+(1-\alpha)\beta}$. Workers wages are therefore equal to their outside options plus a fraction γ of the quasi-rent—that is, the extent to which a rescaling of their marginal revenue product exceeds their outside option. (In a perfectly competitive market, $MRPL = \bar{w}$ and therefore workers are paid exactly their marginal product wherever they work.)

Importantly, to shifts in p_j only impact an *individual employee's* quasi-rents if marginal revenue product $MRPL(L; \bar{P}, p_j)$ changes. Firms, however, have a chance to adjust their hiring—and hence both employment L and $MRPL(L; \bar{P}, p_j)$—after shocks to p_j are realized but before wages are negotiated. When deciding on hiring, firms take into account not only the marginal revenue product and anticipated settlement wage, but also the cost of recruiting $c(H)$ and changes in bargaining power that impact settlement wage $w(L)$, as evidenced in the first-order condition for the optimal $H^*(\bar{P}, p_j)$ and implied $L^*(\bar{P}, p_j) \equiv H^*(\bar{P}, p_j) - \delta L_j^0$:

$$MRPL(L^*(\bar{P}, p_j)) = c'(H^*(\bar{P}, p_j)) - w(L^*(\bar{P}, p_j)) + \frac{dw}{dL}(L^*(\bar{P}, p_j)) \times L^*(\bar{P}, p_j) \tag{A.6}$$

Positive demand shocks mechanically increase $MRPL$, but also incentivize firms to hire more workers, thereby reducing $MRPL$. The net effect on rents thus ultimately depends on how employment adjusts given the recruiting friction characterized by $c(H_j)$. Total differentiation of (A.6) and algebraic manipulation yields reveals that the elasticity of $MRPL$ with respect to p_j is given by:

$$\epsilon^{MRPL,p} = \frac{1}{1+\chi}\ ,\ \chi \equiv \frac{-Y_{LL}(H^*(\bar{P}, p_j); \bar{P}, p_j)}{c''(H^*(\bar{P}, p_j))} \times (1 - \gamma(1-\alpha)) \tag{A.7}$$

The elasticity of $MRPL$ reflects the *relative curvature* of the revenue function compared to that of the hiring cost function, reflect in the second derivative of each. As the convexity of the hiring cost grows large, firms cease adjusting employment levels and p passes through to marginal product one-for-one. When the hiring cost function has zero convexity and is *linear*—that is, the cost of an additional recruit is a fixed constant—this ratio diverges to infinity and the elasticity is zero. In this latter case employment always adjusts to keep $MRPL$ constant; a direct implication is that and wages are invariant to product demand shocks.

We tie these insights to the framework in the main text in the following proposition:

Proposition. *The elasticity of wages with respect to idiosyncratic demand shocks is a case of the general formulation in (6)*

$$\epsilon^{w,p} \equiv \frac{\partial \ln w_{ij}^*}{\partial \ln p_j} = \underbrace{\frac{\gamma \times MRPL(\bar{P}, p_j)}{w(\bar{P}, p_j)}}_{\textit{Rent Share of Wage}} \times \underbrace{\frac{1}{1+\chi}}_{\textit{Sensitivity of Rents to Demand}} \tag{A.8}$$

and the empirical pass-through elasticity of wages with respect to sales is exactly:

$$\epsilon^{w,y} = \frac{\epsilon^{w,z}}{\epsilon^{y,z}} = \frac{\chi}{\frac{\alpha}{\chi} + \chi} = \frac{\gamma f'}{w} \frac{\chi}{\chi + \alpha} \tag{A.9}$$

where $\chi \equiv \frac{-Y_{LL}(H^*(\bar{P},p_j);\bar{P},p_j)}{c''(H^*(\bar{P},p_j))} \times (1 - \gamma(1-\alpha))$.

Wage incidence is determined as follows:

1. Constant Hiring Costs: *If there is a constant per-worker cost of recruitment* $c(H_j) = \psi \times H_j$*, or if the cost is zero, then firms always choose an employment level* L^* *such that* $MRPL = \bar{\mu}$*, where* $\bar{\mu}$ *is a constant. Wages are invariant to* p_j*, but include a strictly positive noncompetitive surplus* $\gamma \times a_i \times \bar{\mu}$*.*

2. Increasing Hiring Costs *If the cost of hiring is convex* $c'(H_j) > 0, c''(H_j) > 0$*, then firms choose an employment level* L^* *such that* $MRPL = \mu(\bar{P}, p_j)$*, where* $\frac{\partial \mu}{\partial P_j^{id}} > 0$*. Wages are strictly increasing in* P_j^{id}*, and includes a strictly positive noncompetitive surplus* $\gamma \times a_i \times \mu(P_j^{id}, P^{comm})$*.*

This proposition illustrates the key principle in our framework: although firms pay noncompetitive wage premiums in frictional labor markets, these premiums may not be affected by demand shocks if firms can adjust on other margins.

A.2 Relationship to Other Models

Union Bargains. In union bargaining models, employees can collectively hold up the entire output of a firm, and thus the entire value-added per worker is treated as a quasi-rent. Wage bargaining is anticipated when employment decisions are made—employment and wages are jointly negotiated with an industry union in "efficient bargain models" like Brown and Ashenfelter (1986), whereas it is unilaterally determined in "right-to-manage" models like in Van Reenen (1996) and bargained only with current employees in "insider-outsider" models following (Lindbeck and Snower, 2001; Solow, 1979). Here, we briefly analyze the efficient union bargaining setup in Abowd and Lemieux (1993).

Consider a large union with $\bar{L}$ members that bargains with a unionized "closed-shop" firm over both its employment level L_j and its wage level w_j during a single period. The union seeks to maximize the payroll of its members $w_j L_j + \bar{w}(\bar{L} - L_j)$ where $\bar{w}$ is the outside wage available to members. The union seeks to maximize a convex profit function $\pi_j = p_j \times f(L_j) - w_j L_j$, where $f(L_j)$ is am increasing and concave function. The firm and union engage a Nash bargain to choose w_j and L_j, where the union has bargaining weight γ. In the event of a breakdown in negotiations, the firm's outside option is zero production while the union holds up the firm, while union members can all get the outside wage $\bar{w}$. Formally, the settlement wage w^* and employment level L^* are chosen to solve

$$\max_{w,L} (w_j L_j + \bar{w}(\bar{L} - L_j))^{\gamma} (p_j \times f(L_j) - w_j L_j)^{1-\gamma} \tag{A.10}$$

The Nash bargaining solution for employment is characterized by an "efficient" employment level L^* that sets the marginal revenue product equal to the outside wage $p_j \times f'(L^*) = \bar{w}$. Meanwhile the wage is the *average* revenue product at employment level L^*, $w^* = \frac{p_j \times f(L^*)}{L^*}$. As workers bargain as unit, all workers can threaten to hold up their average product, which is more than their marginal product.

When a firm experiences an idiosyncratic shock to demand p_j, the union may respond by negotiate a higher per-worker wage, but it may also try to negotiate for higher employment given a constant wage premium. Thus, as in the model above, the incidence of shocks on wages depend crucially on how the employment adjustment is managed. Suppose f is homothetic so $f(L_j) = L_j^{\alpha}$, as above. In this case, the analysis is very similar to the discussion above—wages incorporate a potentially large quasi-rent and exceed outside option wages, *but the per-worker wage is fully invariant to $TFPR_j$ given fixed $\bar{w}$.* To see this, observe that since $f'(L_j) = \alpha \frac{f(L_j)}{L_j}$, the negotiated wage (given the negotiated employment level) can be expressed as $w^* = \frac{\bar{w}}{\alpha}$. This wage is marked up over the outside wage by a constant proportion reflecting how quickly returns to labor diminish. Thus, idiosyncratic shocks to p_j have no impact on *per-worker* quasi-rents $Rents_{ij} = \bar{w}(\frac{1-\alpha}{\alpha})$ and $\frac{d \ln Rents_{ij}}{d \ln p_j} = 0$.

In general, per-worker wages only increase when the output elasticity of $f(L_j)$ is decreasing in L_j. Although homotheticity is a special case, it highlights how the impact of demand on per-worker quasi-rents $\frac{d \ln Rents_{ij}}{d \ln p_j}$ depends strongly on the tradeoffs firm face when considering labor quantity adjustments beyond our baseline model.

Single Worker CRS Firm with Firm-Specific Training or Time Cost of Search. In standard Diamond-Mortensen-Pissarides search models (Pissarides, 2000) or models of bargaining with firm-specific skills (Lazear, 2009) single-worker firms with revenue productivity ϕ_j produce exactly ϕ_j so long as they have a matched and/or properly trained employee. At the beginning of each period, firms with no employees must engage in costly search or costly training of a new worker; if required skills are firm-specific, the firm must bear the cost of training (Becker, 1962). This cost may could be a fixed monetary cost c (for example a headhunting fee or a software license for a training program). Alternatively recruiting and training could take the form of a time/opportunity cost; for example, in the standard Diamond-Mortensen-Pissarides firms must spend entire periods searching for new workers and match with new workers in a given period with Pareto probability (one could similarly model training as taking full periods with success probability θin any period).

Given this cost, and workers' outside wages $\bar{w}_i$, firms and workers starting a period in previously-established match (or who match at the start of the period) engage in a Nash bargain to determine the wage. As above, this this entails each party receiving its outside option if the relationship were to dissolve, plus a fraction of the surplus value of the match that exceeds the sum of outside options. The relationship between the wage depends on the nature of the search/training cost. First, consider if the cost an opportunity cost, focusing on the DMP case where workers cannot be replaced at all during a period and free-entry drives the firm's outside continuation value down to zero. In this case, the entire output of the firm ϕ_j is at stake, and the total surplus value (or quasi-rent) is $Rents_{ij} = \phi_j - \bar{w}_i$. Workers receive a share γ of QR_{ij} reflecting their bargaining weight.[60] Since the firm cannot replace its workforce at all, increases in revenue productivity increase the quasi-rent one-for-one, and likewise $\frac{d \ln Rents_{ij}}{d \ln p_j} > 0$.

However, if firms can find a worker at the outside wage $\bar{w}$ subject to a fixed training/search cost c, then the firm's outside option is simply $(\phi_j - \bar{w}) - c$, and the total surplus is simply c.In this

[60] Formally, given a worker's bargaining weight of γ, the wage is the one that solves $w = \arg\max_{\omega}(TFPR - w)^{1-\gamma}(w - OO_i)^{\gamma} \implies w = \gamma(TFPR_j - OO_i) + OO_i$.

case $\frac{d \ln Rents_{ij}}{d \ln p_j} = 0$. This example highlights that the relationship between ϕ_j and wages in DMP models is *not* the presence of search frictions, but rather the specific assumption of frictions that take the form of an *opportunity cost* of search.

Monopsonistic Wage Posting and Efficiency Wage Models. In standard models of monopsonistic wage determination, firms unilaterally post wages and are subject to a firm-specific upward-sloping labor supply curve $L_j(w_j)$. In standard formulations, such as in Manning (2011) and Card et al. (2018), this upward-sloping supply curve reflects to unobservable heterogeneity in worker preferences over working for a specific employer. One can model this as heterogeneity in workers' outside options w^{out}, which are distributed according to some cumulative distribution function $G(w_i^{out})$. Workers therefore choose to work at firm j if $w_j > w_i^{out}$, and $L_J(w_j) = L^{tot} \times G(w_j)$, where L^{tot} is the size of the overall workforce and $G(w_j)$ are the share who choose to work at firm j. When a firm seeks to expand employment, it posts a higher wage so that more employees are willing to accept a job at the firm. Since firms cannot observe these preferences, *all* employees benefit from the higher wage, not just new hires. As a result, workers with $w_i^{out} < w_j$ enjoy an information rent reflecting the difference between their outside option and the cost of a marginal hire.

In the simplest formulation, firms post a wage in each period and never learn worker's types. A key property of this set up is that the cost of recruitment is convex, as noted by Manning (2006). Accordingly, there is a close relationship between model and the framework presented above. In particular, at any target employment level L_j^* and implied wage $w_j^*(L_j^*)$ (defined by the inverse labor supply curve), the cost of replacing a worker $w_j^*(L_j^*)$, the wage of the *marginal* worker. As a result, if $L^*(P_j)$ is increasing in the demand level P_j, and the labor supply curve is upward sloping, then the cost individual workers can impose on employers is also increasing in P_j—effectively, the information rent grows with product demand $\frac{d \ln Rents_{ij}}{d \ln p_j} > 0$.

This reduced-form of the wage posting model is isomorphic to the multilateral bargaining model presented above with a convex hiring cost and a 100 percent quit rate each period—the positive slope of the firm-specific labor supply curve (i.e. increasing costs of a marginal hire) corresponds to convexity in the recruiting cost. Like that case, the convexity of adjustment cost generates a dependence of wages on product demand and the desired employment level, $\frac{d \ln Rents_{ij}}{d \ln p_j} > 0$. However, the source of rent is different. Specifically, these models assume the only source of rents is the inability of employers to contract based on worker type and that employers have no other screening mechanisms beside the wage—as a result, changes in firm wages (relative to market-level outside options) should correspond to changes in employment, and wages of new hires should change the same as wages of existing employees. Other bargaining models do not share this assumption; for example, in models where jobs require time-intensive training in firm specific human capital, incumbent workers will enjoy quasi-rents that new hires will not.

However, upwards-sloping supply of workers is but one formulation on the wage-posting. Katz (1986) surveys alternative formulations in which higher wages induce higher effort or retention of incumbents, but do not necessarily affect recruiting. In these "efficiency wage models", firms with positive idiosyncratic demand shocks may find it worthwhile to increase wages in order to increase the effort inputs of incumbent workers—who may not be substitutable by external hires, as in (Jaeger, 2015). In these models, higher wages need not be accompanied by increased employment, but should be accompanied by higher effort supply. In this case, higher hourly wages may overstate the true change in wages *per efficiency/effort unit*; nonetheless, this constitutes a channel by idiosyncratic demand differences across firms can impact individuals *observable* hourly wages.

B Data Appendix

B.1 Data Sources

We use consistent, de-identified firm and worker identifiers to link detailed administrative records from the following datasets:

Export Data. The export data is 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.[61] Though the underlying data are transaction-level, for confidentiality purposes we were only able to view firms' export and import volumes disaggregated by destination country and six digit product (HS-6) level. Our data cover the period from 2005 to 2013. To relate firm behavior to demand condition in foreign markets, we use annual data on trade flows by six-digit product and country pair publicly available in the BACI database.[62]

Business Statistics from *Informação Empresarial Simplificada (IES)*. The IES database contains profit and loss statements and balance sheets for the universe of registered firms operating in Portugal, including those with no employees. Firms are required to report to the IES, which provides a unified reporting system used jointly for statistical and administrative purposes by several agencies. Our data has coverage for the years 2005–2013. This database includes annual information on the sales, input costs, factor payments, profits, assets, and liabilities of all Portuguese firms on an annual basis. We use items in this dataset to calculate the value added of the firm, defined as the the output of the firm net of purchases of intermediate goods in services. While gross sales are a useful measure of output as they are directly and reliably observed, value added is a more direct measure of the level of production in the firm.

Employer Employee Data from *Quadros de Pessoal (QP)* The QP is a matched employer-employee dataset produced by the Ministry of Employment based on a census of all private-sector employers during October of each year. Employers with at least one paid employee must report each employee's 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. Government workers, workers temporarily out of the labor force, or working as an independent contractor are excluded from these data. Because reported quantities pertain to a single reference month during the year, the most reliable outcome is the monthly contract wage, excluding fringe payments, bonuses, and overtime which may be inconsistent over the year, as this is most likely to stay constant throughout. We drop observations with invalid birthdates, identifiers, or zero hours.

These data contain consistent (anonymized) person and firm identifiers, enabling us to study individuals over time, even as they move firms. We define workers as *incumbents* at firm j if they worked full-time at firm j in 2007 and if this was their primary source of reported earnings. Unless otherwise specified, we study effects on the cohort of workers who were employed by a treated firm in 2007, observing the outcomes of those individuals *anywhere* they work in years besides 2007, so long as they appear in the QP. However, individuals within cohorts may exit the data over time. When studying effects on incumbents, we restrict to full-times jobs (where the individual works between 120 and 200 hours in the reference month). When individuals have multiple jobs in a year, we limit to jobs that are individual's highest-paying job in a given year. We also study employment

[61] 110,000 Euros in 2007.

[62] Gauiler and Zignago (2010) overview the underlying source data and discuss how the harmonized trade flow database was constructed.

B.2 Data Dictionary

B.2.1 Outcome Variables

Below, we describe the key outcome variables in our analysis and list their source. All monetary values are in Euros and are deflated into real terms unless otherwise noted. Select log wage variables (denoted by *) are kept in nominal Euro terms to facilitate tests of downwards rigidity, though annual inflation of log variables is absorbed by year fixed effects in most specifications). All outcome variables are winsorized by year at the 2nd and 98th percentiles to limit the influence of outliers.

Firm Level Variables:

- **Exports**: The annual sum of all firm export shipment values across all products and destinations, in Euros. This variable may be influenced by shipment-level reporting thresholds. Missing values are coded as zeros. *Source: Export Data*
- **Sales:** Total sales, directly reported by the firm. *Source: IES*
- **Export Share of Sales (S_j):** Ratio of total 2005-2007 exports from export data to total 2005-2007 sales in IES, all amounts in real Euros.
- **Value Added:** Following Card et al. (2018) and define value added as the sum of total labor costs and gross earnings before netting out interest, taxes, depreciation, and amortizations (EBITDA) reported on firms profit/loss statements. Labor costs are the balance sheet cost item including all wage and non-wage labor expenses. Value added can also be constructed during a specific period as total sales less the cost of intermediates used in production of goods sold during that period regardless of when the intermediates are purchased–this requires an adjustment for inventory stocks. Since value added is defined as the total output of labor and capital factors in the firm during the year, an accounting identity requires that the total value added of the firm equal the total factor payments (all costs of employed labor plus payments to capital including net profits and taxes on profits). Since the data on factor payments are more reliable than the data on inventories in the data, we use the latter definition. This also ensures value added is not mechanically correlated with sales. *Source: IES*
- **Employment:** The count of all employees at a firm with positive earnings reported in the *QP* during the reference month. In practice, this measure is highly consistent with employment reported in the IES, and results are not sensitive to the choice of variable. *Source: QP*
- **Payroll:** The sum of all payments (regular wages, overtime wages, other payments, and fringe subsidies) to all employees reported in the QP during the reference month. *Source: QP*
- **Accumulated Hires:** The count of employees at a firm in year t who were not present at the firm in 2007. (This variable is defined for years 2007 as well). *Source: QP*
- **Retained Incumbents:** The count of employees at a firm in year t who were present at the firm in 2007. *Source: QP*

- **Average Log Hours :** The average log hours including both regular *and* overtime hours for all full-time (120-200 hours per month) workers in a given year. *Source: QP*
- **Average Wages (*) :** The average log base hourly (non-overtime) wage for all full-time (120-200 hours per month) workers in a given year. *Source: QP*
- **Average Log Wages, Hires (*) :** The average log base hourly wage for all full-time workers at a firm in year t who were not present at the firm in 2007. *Source: QP*

Individual/Cohort Level Variables: All variables are defined for individuals whose primary full-time job (120-200 hours per month) was at a treated firm in 2007. Since the treatment only varies at the 2007 employer level, we collapse all variables to cohort-level averages among cohort-members with outcomes defined in the given year (not necessarily constant), and weight regressions by 2007 cohort size. *Source for all: QP*

- **Log Monthly Contract Wage (*) :** Employment contracts typically specify a full-time monthly base wage and an expected number of monthly work hours (this restricts firms' ability to reduce pay *ex-post* by cutting hours). We use this monthly contract wage as an individual-level outcome. The monthly contract base wage is the compensation variable most likely to be consistent throughout the year. This variable is defined for full-time jobs at individual's highest paying job in a year, regardless of whether or not the employer is the same as the 2007 employer (unless specified). Worker-year observations with missing data are omitted from cohort averages in that year.
- **Log Hourly Wage (*) :** The monthly contract wage divided by the number of regular non-overtime hours worked in the reference month. This variable is defined for full-time jobs at individual's highest paying job in a year, regardless of whether or not the employer is the same as the 2007 employer (unless specified). Worker-year observations with missing data are omitted from cohort averages in that year.
- **Inverse Hyperbolic Sine (arcsinh) of Hourly Wage:** This variable is defined in real terms and treats missing observations as zeros. This variable is defined for all workers in all years, even if the 2007 employer has exited the data.
- **Symmetric Growth Rate of Wages:** The real growth rate in wages relative to 2007, $\frac{w_t - w_{2007}}{.5(w_t + w_{2007})}$. This variable is defined in real terms and treats missing observations as zeros. This variable is defined for all workers in all years, even if the 2007 employer has exited the data.
- **Log Hourly Total Compensation:** For full-time workers, all payments in the QP including base wages, overtime wages, fringe subsidies, and other payments, divided by the total monthly hours worked including overtime.
- **Has Any Job:** An indicator for whether or not an individual has a full-time job in the QP data.

Heterogeneity Variables:

- **Typical Tenure in Industry:** We measure the median tenure of permanent contract employees of each firm in each year 2003–2007, then take the employment-weighted average of this over all firm-years in each five-digit NACE code that both A) are outside our exporter sample and B) have 0-100 employees. This gives a measure of the typical tenure at small- and medium0 sized firms in the broader industry.

- **Separation rate in Industry:** In each year 2003–2007, we measure the total number of permanent contract workers in firms in each five-digit NACE code that both A) are outside our exporter sample and B) have 0-100 employees, and then measure the total number of permanent contract workers who leave their firm between that year and the next. We calculate the annual turnover rate as the ratio of separations to the initial stock, and take the industry-level average over 2003–2007 as the average separation rate at small- and medium-sized firms in the broader industry.

- **AKM Firm Pay Premium (Permanent Contract Workers):** 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 are age and eduction controls 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. We use the estimated ϕ_j as the measured firm pay premium

B.2.2 Shock Construction

Exposure Weights: Using the export data, we measure the exposure weight of a firm j to the market for six-digit (HS6) product p in country $c$as their share $s_{j,pc}$ of exports of product module m to country c in the total 2005–2007 exports by the firm across all products (in set M) and countries (in set C):

$$s_{j,mc} = \frac{Exports_{j,mc}^{2005-2007}}{\sum_{p \in P, c \in C} Exports_{j,mc}^{2005-2007}} \tag{A.11}$$

The denominator is simply the total pre-period exports of firm j. We also measure firms' exposure to each product market m as

$$s_{j,m} = \frac{Exports_{j,m}^{2005-2007}}{\sum_{p \in P} Exports_{j,m}^{2005-2007}} \tag{A.12}$$

Recession Impact: We calculate the destination-based demand shock as the proportional change in imports of product p by country c—summing imports from all countries *excluding* Portugal—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). 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). Denoting the average 2006 and 2007 level of non-Portuguese imports (NPI) of product p to country c (in constant real US Dollars) as NPI_{mc}^{pre} and the corresponding average level during 2009 and 2010 as NPI_{mc}^{post} we calculate the destination change

in imports Δ_{mc} as

$$\Delta_{mc} = \frac{NPI_{mc}^{post} - NPI_{mc}^{pre}}{\frac{1}{2}(NPI_{mc}^{post} + NPI_{mc}^{pre})} \tag{A.13}$$

We also measure the aggregate change in demand for product m as the average in imports across all countries

$$\Delta_m = \frac{1}{\#C} \sum_{c \in C} \Delta_{mc} \tag{A.14}$$

Total Demand Shift. The baseline predicted change in export demand for firm j, Δ_j, 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:

$$\Delta_j = \sum_{m \in M, c \in C} s_{j,mc} \Delta_{mc} \tag{A.15}$$

This can be directly decomposed into a cross-product component $\Delta_j^{comm} = \sum s_{j,m} \Delta_m$ and within-product, cross-country component $\Delta_j^{id} = \sum s_{j,mc}(\Delta_{mc} - \Delta_m)$. However, these two objects are mechanically negatively correlated in fine samples, thus we use the following decomposition that does not have this problem.

Decomposition. We measure the predicted change in demand based on product market exposure alone:

$$\Delta_j^{comm} = \sum s_{j,m} \Delta_m \tag{A.16}$$

To isolate the variation in Δ_j that is orthogonal to this product-level variation *in our sample,* we regress the baseline shock Δ_j on a fourth-order polynomial in Δ_j^{comm} among the firms in our baseline sample to capture all variation in Δ_j predicted by product market movements:

$$\Delta_j = \alpha + \beta_1 \Delta_j^{comm} + \beta_2 (\Delta_j^{comm})^2 + \beta_3 (\Delta_j^{comm})^3 + \beta_4 (\Delta_j^{comm})^4 \tag{A.17}$$

We denote the prediction $\hat{\Delta}_j^{comm}$. We then define our idiosyncratic shock as the orthogonal residual:

$$\hat{\Delta}_j^{id} = \Delta_j - \hat{\Delta}_j^{comm}$$

In our analysis, we use the additive components $\hat{\Delta}_j^{id}$ and $\hat{\Delta}_j^{comm}$ as our idiosyncratic and common export demand shocks, respectively.

C Test of Selection of Firms into Export Markets

To motivate a test of exogenous assignment of firms to differently-shocked markets, consider a simple model of selection into export markets with better outcomes. Let Δ_{mc} be the symmetric growth rate of imports of product module m by country c from all countries, *excluding* Portugal, 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 denoted total non-Portuguese imports in real U.S. Dollar. Further, let the corresponding change in observed exports by the individual firm j to that same m, c market during the same period as $\tilde{E}_{j,mc}$. Suppose export growth $\tilde{E}_{j,mc}$ is determined by market level demand Δ_{pc}, unobservable firm productivity ϕ_j (denoted as such as it may reflect changes in worker productivity ϕ_i for workers i at firm j) that increases

export performance of firms across *all* destinations in constant proportions, and an idiosyncratic residual $\upsilon_{j,mc}$ according to:

$$\tilde{E}_{j,mc} = \phi_j + \beta\Delta_{j,mc} + \upsilon_{j,mc} \tag{A.18}$$

In this setting, selection occurs when firms with higher levels of ϕ_j tend to have relationships in markets with higher level of $\Delta_{j,mc}$–that is, firms experiencing larger labor productivity shocks select into relationships with customers in markets with better import demand. When this is the case, changes in the average level $\Delta_{j,mc}$ at firm j will be correlated with unobserved heterogeneity in ϕ_j, confounding identification of idiosyncratic shocks.

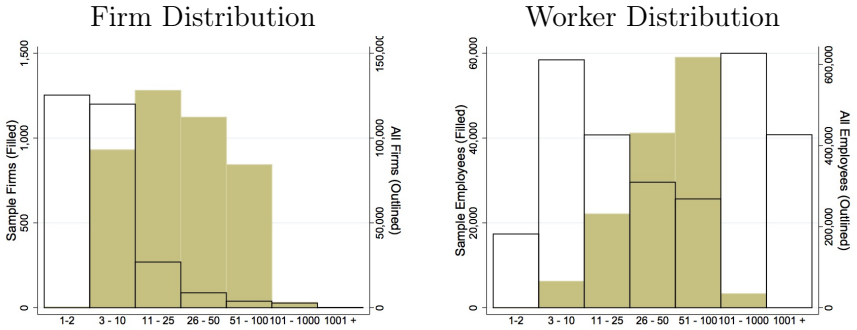
Although this type of selection can not be directly tested in firm-level data, it can be partially tested by studying firms with multiple export destinations in the relationship-level data in a test similar to that used by Khwaja and Mian (2008) to test for sorting of borrower firms to lending banks that experienced differential credit supply shocks. Under the assumption of constant effects of supply productivity across destinations, no selection implies $\Delta_{j,mc} \perp \phi_j$. If this is the case, then regression estimates of β in equation (A.18) for firms with multiple pre-period destinations should be both positive (necessary condition for a causal effect of demand shocks) and invariant to inclusion of a firm fixed effect ϕ_j. If inclusion of a firm fixed effect significantly reduces the estimated magnitude of β, this would imply a positive correlation between $\Delta_{j,mc}$ and ϕ_j, contradicting the no-selection condition.

Appendix Table A.2 presents estimates of equation (A.18) with and without fixed effects. The firm-by-market sample includes all firms with at least two export destinations (across product-country cells), and only includes relationships that had positive exports in the pre-period. For the benchmark estimates, the outcome is the symmetric growth rate of exports by the firm to the specified product-by-country market from 2006-2007 to 2009-2010, which incorporates export volumes of zero in the post period (which occurs in the majority of individual relationships). Import behavior at the destination is a strong predictor of exports by the firm–though the magnitude of the estimate is small, reflecting both imprecision in the relationship-level prediction and the preponderance of zero outcomes. However, the magnitude is very similar with or without inclusion of the firm fixed effect, consistent with no sorting on latent productivity. The following columns separately display effects on the intensive margin (adjustment conditional on exports) and the extensive margin (probability of zero). The magnitude of the intensive margin (measured as the effect on the log change in exports conditional on positive flows) is substantially larger and approximately 40 percent, and the probability of terminating the relationship is also responsive to import demand conditions. The intensive margin effect is somewhat smaller when firm fixed effects are included, but the extensive margin response is offsettingly larger when firm fixed effects are included. These findings support the claim that demand conditions have a causal effect on firms' sales to its trading partners, and that firms do not systematically sort to destinations with better demand growth based on latent productivity trends–and, therefore, also support the use of destination-level shocks to construct an exogenous firm-level demand predictor.

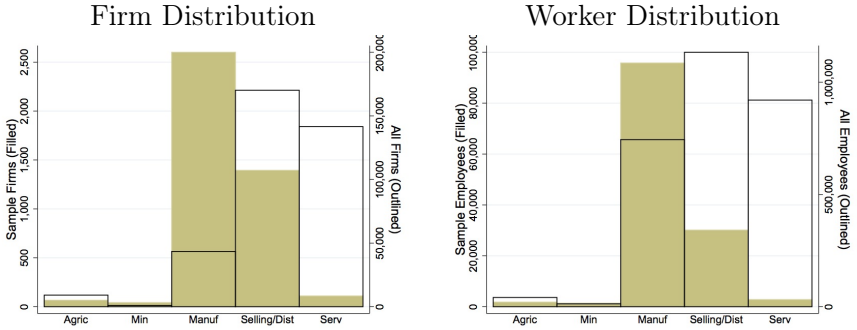
Appendix Figures

Figure A.1: Comparison of Sample and Population of Firms

Panel A: Firm Size Distribution

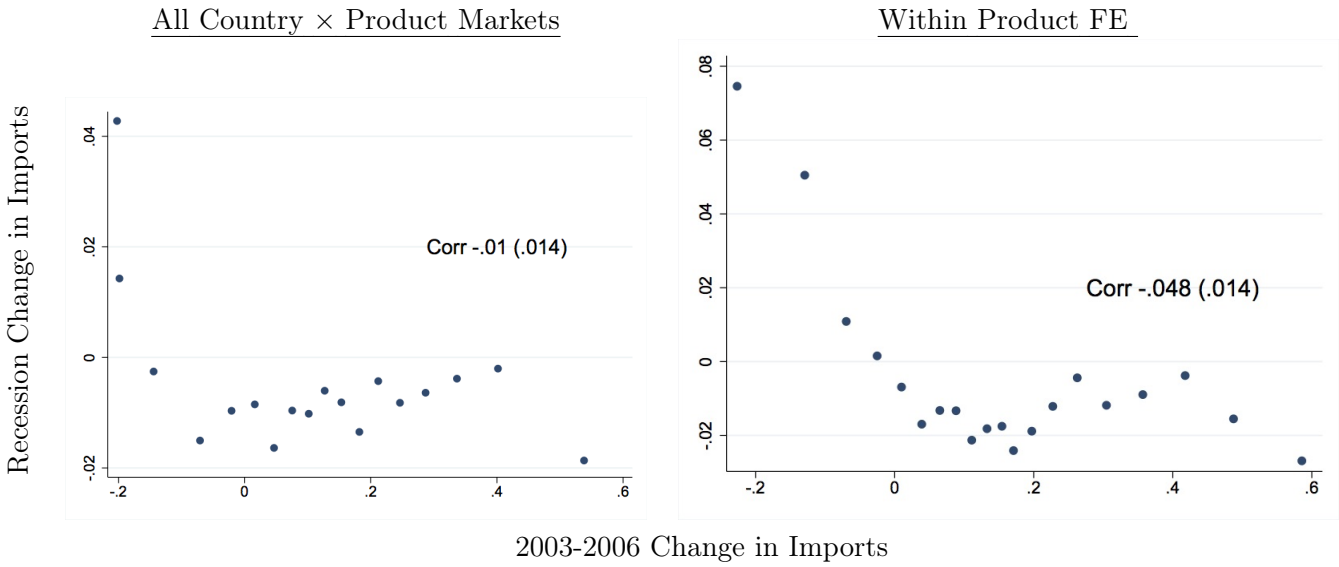


Panel B: Industry Distribution



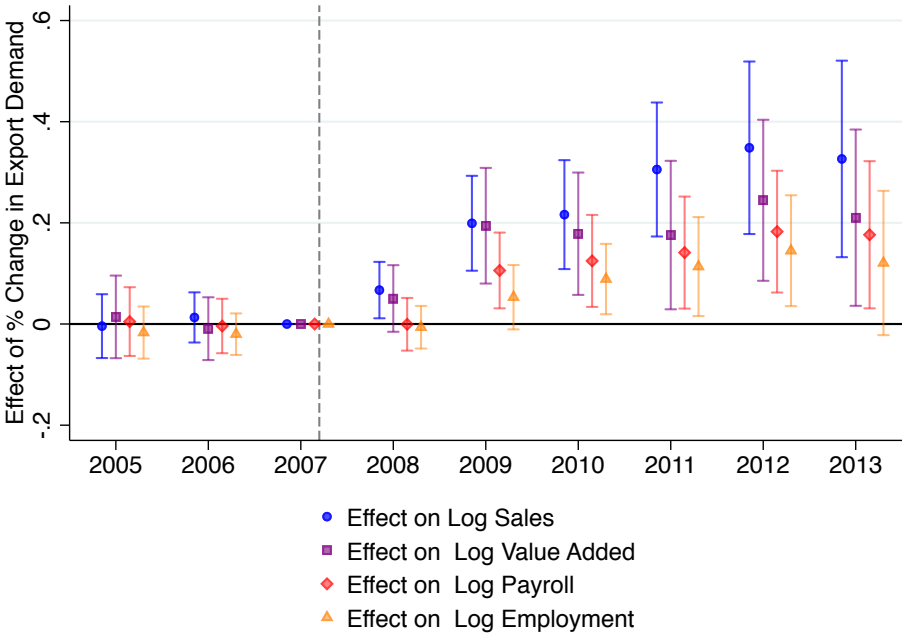
Notes: Figures plot distributions of firms in 2007 in the IES, "All firms" include all firms in the IES with positive employment, even if not reported in the matched employer-employee data. Employment bins are not equally sized. "Worker Distribution" plots present counts where firms are frequency weighted by the number of employees. "All" and "Sample" counts are plotted on separate axes.

Figure A.2: Recession Import Growth vs. Pre-Period Growth in Foreign Markets



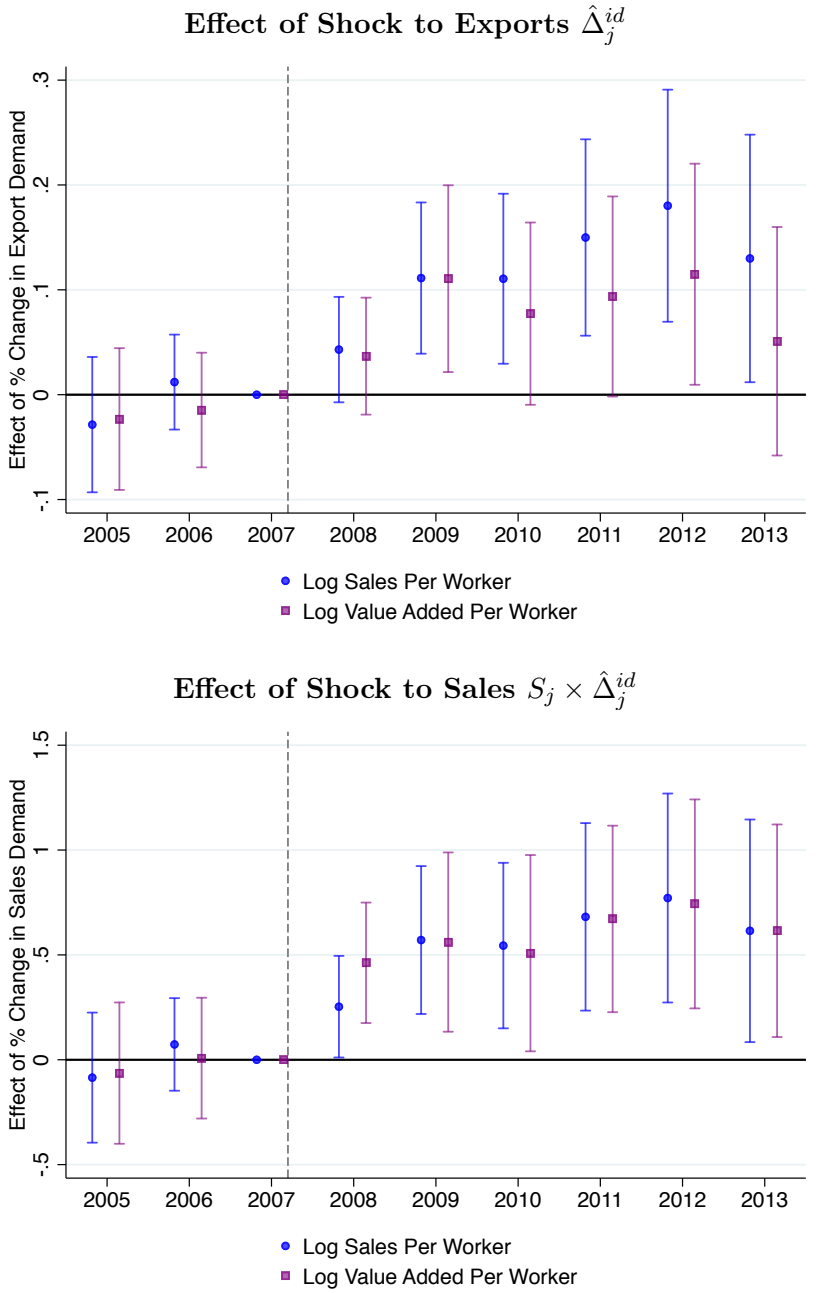
Notes: Figure plots the bin-average of the y-axis variable within 20 equally-sized quantile bins of the x axis variable. Observations are product-by-country markets. Y axis variable are the arc changes in non-Portuguese imports from 2006 and 2007 to 2009 and 2010 (Δ_{mc}). X axis variable is 2003-2006 arc change in non-Portuguese imports in the same markets, years are chosen so the two periods do not overlap.

Figure A.3: Year-Specific Effects on Sales, Value Added, and Payroll in Logs



Notes: Figure displays year-specific effects of the idiosyncratic shock to export demand $\hat{\Delta}_j^{id}$ on log sales, value added, payroll, and employment. Sample is balanced panel of firms that sell and employ at least one full-time worker in all years, N = 2,926. Year-specific coefficients and 95% confidence intervals are from regressions on interactions of $\hat{\Delta}_j^{id}$ 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 based on standard errors clustered at the firm level to account for potential serial correlation of errors. Regressions are weighted by the average number of full-time employees in 2007. All regressions include year fixed effects, as well as controls for year-specific effects of 2005-2007 exports, log exports, the export share of sales, and the share of exports going to Spain or Angola in those years.

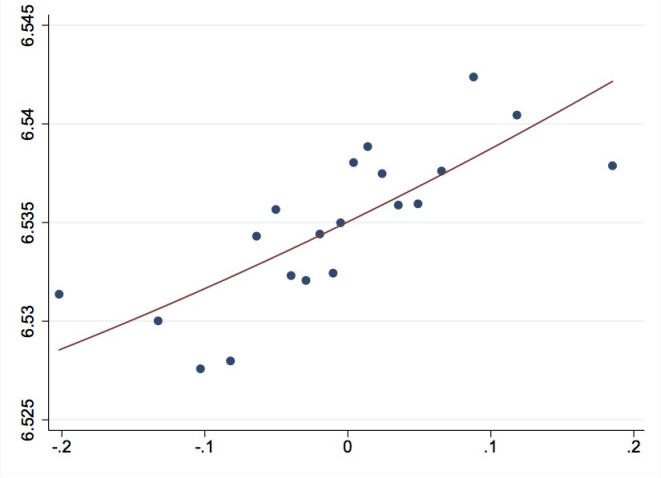
Figure A.4: Year-Specific Effects on Labor Productivity



Notes: Figure displays year-specific effects of the idiosyncratic shock to export demand $\hat{\Delta}_j^{id}$ or "Shock to Sales" estimates of effects of $\hat{\Delta}_j^{id}$ interacted with the pre-period share of sales in exports $S_j \equiv \frac{Exports_j^{pre}}{Sales_j^{pre}}$ on log sales per worker and log value added per worker. Sample is balanced panel of firms that sell and employ at least one full-time worker in all years, N = 2,926. Year-specific coefficients and 95% confidence intervals are from regressions on interactions of the Δ_j^{id} 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 based on standard errors clustered at the firm level to account for potential serial correlation of errors. Regressions are weighted by the average number of full-time employees in 2007. All regressions include year fixed effects, as well as controls for year-specific effects of 2005-2007 exports, log exports, the export share of sales, and the share of exports going to Spain or Angola in those years.

Figure A.5: Linearity/Symmetry of Wage Effects

Residualized Bin Scatter + Quadratic Fit, Log Nominal Hourly Wage

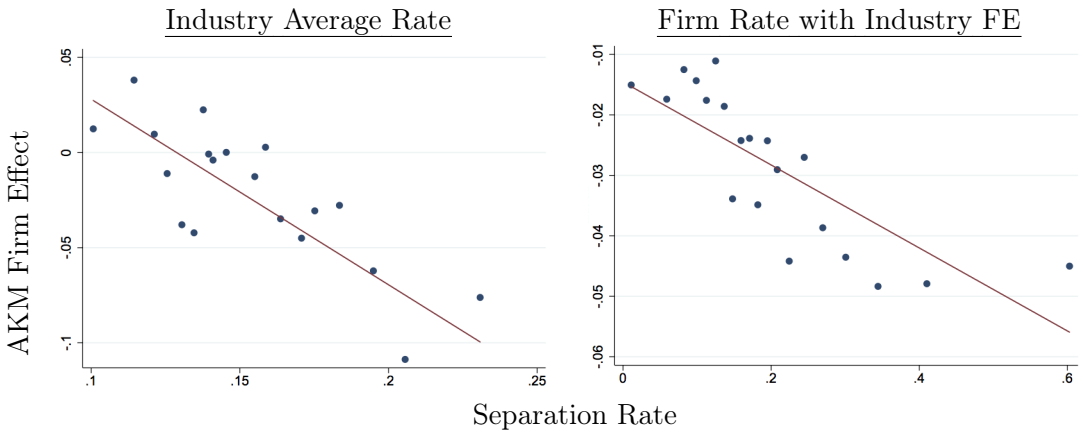


Idiosyncratic Shock

Average nominal w change in sample is 9.2%

Notes: Sample includes all firms with attached incumbent workers in 2007, N = 4,100. Figure plots mean residual values of the outcome within 20 equally-sized quantile bins of the resdiualized shock levels, where residuals are constructed to reflect all specification details of the reduced-form difference-in-difference specification in equation (12). The average slope line represents the differences-in-differences coefficient that would be obtained from the regression, with firms weighted by the number of attached incumbents in 2007. Also displayed is the best fit quadratic polynomial to the residuals given the specification details. Specification details are as in Table 7 (estimating the reduced-form effect of the shock on the outcome, rather than IV estimation using output as the explanatory variable).

Figure A.6: Firm Pay Premiums and Job Durability



Notes: Figure plots the bin-average of the y-axis variable within 20 equally-sized quantile bins of the x axis variable, including the best fit line. Sample is largest connected set of firms and full-time male workers over 18 years old who change jobs, based on years 2003-2007 in the QP. "AKM" firm effects are firm fixed effects from a wage regression with firm fixed effects, person fixed effects, year effects, and age × education group dummies. X-axis variable in "Industry Average Rate" pane is the 5-digit industry index used in Table 10 calculated for all industries in the QP, common to all firms in same 5-digit industry. X-axis variable in "Firm Rate" pane is the ratio of current-year employees not present at the firm in the following year divided by all current-year employees, averaged across 2003-2007; both X and Y variables are residuals conditional on 5-digit industry fixed effects.

Appendix Tables

Table A.1: Comparison of Firms and Workers in Sample and Population

	Industry Heterogeneity Subsamples								
	High Separation Rate			Low Separation Rate			Never-Exiter Firms		
	Mean	P50	SD	Mean	P50	SD	Mean	P50	SD
Firms									
Employees	27	19	23.34	29	23	22.9	29	23	23.33
Sales/Worker if Emp>0, Euros	180,041	104,549	265,262	164,039	101,298	199,688	178,275	108,638	221,231
Value Added / Worker if Emp>0, Euros	35,250	25,224	49,015	34,030	27,524	28,558	33,765	28,409	33,764
N Firms, Emp>0		2,223			1,944			2,926	
N Firms									
Workers:									
Monthly Wage, Euros	747.48	545.00	540.85	774.81	600.00	524.96	769.47	582.46	541.78
Hourly Wage, Euros	4.37	3.17	3.20	4.53	3.49	3.08	4.50	3.41	3.20
Log Monthly Wage	6.46	6.30	0.50	6.51	6.40	0.48	6.40	6.27	0.44
Log H ourlyWage	1.32	1.15	0.51	1.37	1.25	0.48	1.26	1.13	0.44
Fixed Term, Percent of Sample	0.20			0.20			0.20		
Tenure, Months (All Workers)	115	90	99.75	128	95	125.83	127	99	108.07
Female, Percent of Sample,	0.51			0.36			0.43		
Regular Hours Per Month	171	173	7.45	171	173	7.66	171	173	7.56
N Workers		59,446			55,910			84,520	

Notes: Table compares firms and workers in full sample to firm subsamples examined in the main analysis. All items are exactly as in Table 2.

Table A.2: Test of Sorting for Firms with Multiple Destinations

	Outcome is Change in Exports by Firm *j* of Product *p* 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*	0.0674***	0.0682***	0.463***	0.396***	0.0266*	0.0324***
	(0.0150)	(0.0114)	(0.0486)	(0.0506)	(0.0148)	(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: Tables reports results from regressions of changes in exports by firm j of product p to destination c on the change (symmetric growth rate) of imports of m to country c from all other countries during the same period, as in equation (A.18). Observations are firm-markets 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.

Table A.3: Reduced-Form Effects on Labor Adjustment

				% Change in Employment Since 2007: Decomposition			
	Log Payroll	Log Employees	Log Hours Per Worker	Total	Post-2007 Hires	Incumbent Retention	% 2007 Incumbs Have Any Job
	(1)	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*	*(7)*
			A. All Firms (Unbalanced Sample of 4173 Firms)				
Δ^{id} x Post Effect:	0.111**	0.076*	0.011*	0.057	0.041*	0.016	0.016
	(0.044)	(0.041)	(0.007)	(0.041)	(0.025)	(0.027)	(0.019)
Mean Change	*0.020*	*-0.077*	*0.000*	*-0.139*	*.138*	*-0.276*	*-0.178*
			B. Never-Exiter Firms Only (Balanced Sample of 2926 Firms)				
Δ^{id} x Post Effect:	0.126**	0.095**	0.005	0.084**	0.069**	0.014	0.014
	(0.043)	(0.038)	(0.006)	(0.032)	(0.030)	(0.017)	(0.012)
Mean Change	*.070*	*-0.029*	*0.000*	*0.010*	*0.177*	*-0.170*	*-0.110*

Notes: Sample is either full analysis sample ($N = 4,173$) 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 idiosyncratic shock to export demand $\hat{\Delta}_j^{id}$ and $Post_t$. "Pre" years are 2006, 2007 (pre-period) and "Post" years 2009, 2010, 2011 (post-period). 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. See Appendix B for additional outcome definitions. Regressions are weighted by the average number of full-time employees in 2007. All regressions include year fixed effects, as well as controls for year-specific effects of 2005-2007 exports, log exports, the export share of sales, and the share of exports going to Spain or Angola in those years. Table also presents the average (real) change in the dependent variable from pre-to-post. Standard errors are clustered at the firm level. ** indicates $p < .05$, * indicates $p < .10$.

Table A.4: Test of Effects of Idiosyncratic and Common Components on Other Firms

	Effect of Δ^{id} on Mean Outcome Level for Other Firms in Same:								
	5-Digit Industry and Municipality			5-Digit Industry			Municipality		
	(1)	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*	*(7)*	*(8)*	*(9)*
Log Sales	0.179	0.038	1.165	0.112	-0.101	1.577	-0.000	-0.009	0.043
	(0.052)**	(0.053)	(0.240)**	(0.097)	(0.98)	(0.327)**	(0.005)	(0.007)	(0.024)*
Log Value Added	0.227	-0.037	1.901	0.155	-0.122	1.984	-0.002	-0.005	0.009
	(0.084)**	(0.076)	(0.367)**	(0.100)	(0.096)	(0.385)**	(0.005)	(0.007)	(0.016)
Log Payroll	0.049	-0.023	0.542	-0.015	-0.130	0.734	-0.003	-0.007	0.026
	(0.027)*	(0.030)	(0.096)**	(0.071)	(0.077)*	(0.245)**	(0.003)	(0.005)	(0.015)*
Log Employees	0.033	-0.017	0.359	0.017	-0.020	.267	-0.002	-0.004	0.008
	(0.022)*	(0.024)	(0.075)**	(0.027)	(0.029)	(0.095)**	(0.003)	(0.003)	(0.009)
Shock Component	*Total*	*Idiosyncratic*	*Common*	*Total*	*Idiosyncratic*	*Common*	*Total*	*Idiosyncratic*	*Common*

Notes: Table displays results from regressions of the leave-one-out (employment-weighted) average outcome level of all other firms the same specified group as each treated firm j (excluding j itself) on the demand shock to j, for all sample firms j that never exit, which also have at least one other firm in the specified grouping. Results pertaining to all treated firms in sample are presented in Table 4. Formally, for group g (for example NACE code, municipality or both) we define the outcome $\bar{Y}_{-j,t} \equiv \frac{\sum_{g=g(j),k\neq j} L_k^{2007} \times Y_{k,t}}{\sum_{g=g(j),k\neq j} L_k^{2007}}$, as the 2007-employment-weighted average outcome among all firms in the same group as j, but excluding j itself. Entries are regression coefficients on the interaction between the idiosyncratic shock S_j and $Post_t$, corresponding to β in equation (14) in the text; each estimate is from a separate regression. Regressions are run at treated firm level; regressions are weighted by firm-j employment to match specification in Table 3 but results are robust to using a simple unweighted mean. Specification includes year and treated-firm-group fixed effects, but no other controls. Municipalities are 24 subregions in Portugal; five-digit industry code is the most detailed NACE classification available. Standard errors are clustered at the treated-firm level. ** indicates $p < .05$, * indicates $p < .10$.

Table A.5: Tests for Asymmetric Wage Effects

Outcome:	Log Hourly Wage		
	(1)	*(2)*	*(3)*
Δ^{id} x Post	0.029**	0.030**	
	(0.011)	-0.01	
$(\Delta^{id})^2$ x Post		-0.041	
		(0.036)	
Spline Components			
Δ^{id} x Post, Baseline			0.054**
			(0.018)
Δ^{id} x (Δ^{id} > Median) x Post			-0.047
			(0.030)

Notes: Table presents nominal hourly wage effect for all individuals individuals whose primary full-time job (120-200 hours per month) was at a never-exiter treated firm in 2007, as in Column (1) in Panel B of Table 5. The spline estimates in Column (3) include a baseline slope and then a marginal slope change above a knot at the median shock level of -0.017; the slope above the knot is the sum of the baseline slope plus the slope change. See notes to Table 5 for additional details on specification.

Table A.6: Robustness of Pass-Through Estimates, All Firms Including Exiters

	Outcome: Log Hourly Wage, Any Job							
	(1)	*(2)*	*(3)*	*(4)*	*(5)*	*(7)*	*(8)*	*(9)*
Sales Elasticity IV	0.233**	0.167	0.191*	0.213**	0.250*	0.162	0.170**	0.140*
	(0.113)	(0.105)	(0.098)	(0.106)	(0.135)	(0.210)	(0.085)	(0.071)
N firms	*4,161*	*4,161*	*4,161*	*4,161*	*4,056*	*5,089*	*4,161*	*5,089*
N workers	*116,258*	*116,258*	*116,258*	*116,258*	*112,935*	*388,195*	*116,258*	*388,195*
N Cohort-Year Obs	*19,374*	*19,374*	*19,374*	*19,374*	*18,844*	*23,705*	*19,374*	*23,705*
Value Added Elasticity IV	0.215**	0.151*	0.199**	0.207**	0.194*	0.057	0.183**	0.142*
	(0.096)	(0.082)	(0.100)	(0.101)	(0.107)	(0.041)	(0.086)	(0.070)
N firms	*4,127*	*4,127*	*4,127*	*4,127*	*3,997*	*5,008*	*4,100*	*5,008*
N workers	*115,399*	*115,399*	*115,399*	*115,399*	*111,856*	*377,635*	*115,399*	*377,635*
N Cohort-Year Obs	*18,899*	*18,899*	*18,899*	*18,899*	*18,344*	*23,090*	*18,899*	*23,090*
Reduced Form Effect of Shock	0.042**	0.026**	0.033**	0.037**	0.033**	0.012	0.033**	0.026*
	(0.010)	(0.009)	(0.010)	(0.011)	(0.010)	(0.008)	(0.010)	(0.009)
N firms	*4,163*	*4,163*	*4,163*	*4,161*	*4,058*	*5,091*	*4,161*	*5,091*
N workers	*116,288*	*116,288*	*116,288*	*90,298*	*112,965*	*388,255*	*90,298*	*5,091*
N Cohort-Year Obs	*20,426*	*20,426*	*20,426*	*20,426*	*20,426*	*19,901*	*24,968*	*20,426*
Baseline Controls	x		x	x	x	x	x	x
Pre-Period Attribute Controls			x					
Destination Controls				x				
5 Digit Industry FE					x			
Including Large Firms						x		x
Firm-Weighted							x	x

Notes: Table displays robustness of instrumental variables estimates in Table 6 to alternative specifications. Sample includes all 4173 firms, including those that exit at some point during the sample frame; Table 8presents corresponding results for sample including only firms that never exit. Column 1 displays IV estimates from Columns 3 and 4 of Panel B in Table 6, see table notes for details. Baseline controls are year fixed effects, as well as controls for year-specific effects of 2005-2007 exports, log exports, the export share of sales, and the share of exports going to Spain or Angola in those years. "Pre-period attribute controls" include controls for year-specific effects of 2005-2007 average employment, sales, assets, hiring, labor productivity, wage levels, and fixed term contract employment. Destination controls include the share of pre-period exports going to each of 10 top destination countries, as well as predicted demand using 2003-2007 changes in imports at baseline destinations. 5-digit industry FE includes industry-by-year fixed effects for 5-digit industry codes, the most detailed NACE classification available. "Firm-weighted" indicates estimation of regressions where all cohorts have equal weight of one. Standard errors are clustered at the firm level. ** indicates $p < .05$, * indicates $p < .10$.

Table A.7: Year-Specific Elasticities

Wage Outcome:	Log Hourly Wage		Log Monthly Wage	
	(1)	*(2)*	*(3)*	*(4)*
Pass-Through Elasticity in:				
2008	0.154	0.225	0.327	0.485
	(0.155)	(0.226)	(0.203)	(0.420)
2009	0.126*	0.121*	0.111*	0.104*
	(0.065)	(0.068)	(0.057)	(0.059)
2010	0.150**	0.177**	0.156**	0.187**
	(0.061)	(0.084)	(0.063)	(0.088)
2011	0.131**	0.238**	0.120**	0.214*
	(0.047)	(0.116)	(0.047)	(0.111)
2012	0.099**	0.152**	0.077*	0.114*
	(0.042)	(0.070)	(0.041)	(0.064)
2013	0.088*	0.154*	0.072	0.119
	(0.042)	(0.070)	(0.041)	(0.064)
Output Measure				
Log Sales	x		x	
Log Value Added		x		x

Notes: Table displays estimates of year-specific pass-through elasticities $\epsilon^{w|Y}$, which coefficients on the interaction of the output measure Y_j with a post period indicator $Post_t$ in (15). Each estimate is from a separate regression where $Post_t$ is defined as zero for $t = 2007$ and one in the year specified in the row; all other years are omitted. Elasticities are estimated using the *same* idiosyncratic shock $\hat{\Delta}_j^{id}$—defined as the 2006–2007 to 2009–2010 change in export demand–as an instrument for the output measure Y_j. The sample of affected firms is the balanced panel of firms that are employ at least one full-time worker in all years, N = 2,923. Firms are weighted by the total number of attached incumbents present in pre-period, weights are fixed across years. Regressions are weighted by the average number of full-time employees in 2007. All regressions include year fixed effects, as well as controls for year-specific effects of 2005-2007 exports, log exports, the export share of sales, and the share of exports going to Spain or Angola in those years. Standard errors are clustered at the firm level. ** indicates $p < .05$, * indicates $p < .10$.

Table A.8: Reduced-Form Effects of Common Shock

						% Change in Employment Since 2007: Decomposition			
Outcome:	Log Sales *(1)*	Log VA *(2)*	Log Payroll *(3)*	Log Employees *(4)*	Any Employees *(5)*	Total *(7)*	Post-2007 Hires *(8)*	Incumbent Retention *(9)*	% 2007 Incumbs Have Any Job *(10)*
				A. All Firms (Unbalanced Sample of 4173 Firms)					
Δ^{id} x Post Effect:	0.857**	0.867**	0.380**	0.372**	0.131*	0.365**	0.185**	0.180**	0.099**
	(0.161)	(0.203)	(0.095)	(0.096)	(0.072)	(0.086)	(0.048)	(0.056)	(0.040)
				B. Never-Exiter Firms Only (Balanced Sample of 2926 Firms)					
Δ^{id} x Post Effect:	0.414**	0.506**	0.292**	0.254**	-	0.200**	0.161**	0.040	0.041
	(0.132)	(0.132)	(0.102)	(0.100)	-	(0.070)	(0.058)	(0.037)	(0.030)

Notes: Sample is either full analysis sample ($N = 4,173$) 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 *common* shock to export demand $\hat{\Delta}_j^{comm}$ and $Post_t$. "Pre" years are 2006, 2007 (pre-period) and "Post" years 2009, 2010, 2011 (post-period). 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. See Appendix B for additional outcome definitions. Regressions are weighted by the average number of full-time employees in 2007. All regressions include year fixed effects, as well as controls for year-specific effects of 2005-2007 exports, log exports, the export share of sales, and the share of exports going to Spain or Angola in those years. Table also presents the average (real) change in the dependent variable from pre-to-post. Standard errors are clustered at the firm level. ** indicates $p < .05$, * indicates $p < .10$.

Table A.9: IV Pass-Through of Common Demand Shocks to Wages: Conditioning on Survival Matters

				Treating Missing/Nonemployed as Zero					
Wage Measure:	Log Hourly Wage			asinh(Wage)			Symmetric Wage Growth Rate		
	(1)		*(2)*	*(3)*		*(4)*	*(3)*		*(4)*
A. All Firms (Unbalanced Sample)									
Elasticity Instrument									
Common Component	0.027	0.043	0.024	0.172**	0.347**	0.078**	0.197**	0.460**	0.156**
	(0.021)	(0.036)	(0.022)	(0.055)	(0.092)	(0.008)	(0.073)	(0.133)	(0.072)
First stage F	170.2	177.4	174.4	169.6	182.0	181.8	169.6	182.0	181.8
Total Shock	0.115**	0.185**	0.114**	0.202**	0.358**	0.208**	0.222**	0.391**	0.229**
	(0.038)	(0.054)	(0.039)	(0.072)	(0.104)	(0.076)	(0.099)	(0.149)	(0.099)
First stage F	64.74	95.43	85.52	66.42	93.57	86.02	66.42	93.57	86.02
B. Never-Exiter Firms Only (Balanced Sample)									
Elasticity Instrument									
Common Component	0.082	0.106	0.066	0.311**	0.411**	0.226**	0.309**	0.408**	0.212*
	(0.051)	(0.067)	(0.040)	(0.118)	(0.166)	(0.092)	(0.142)	(0.197)	(0.113)
First stage F	68.81	66.38	73.87	73.98	68.51	78.14	73.98	68.51	78.14
Total Shock	0.105**	0.128**	0.113**	0.177**	0.216**	0.176**	0.228**	0.278**	0.223**
	(0.037)	(0.044)	(0.041)	(0.070)	(0.087)	(0.078)	(0.091)	(0.115)	(0.102)
First stage F	143.8	150.4	83.77	147.4	152.2	86.16	147.4	152.2	86.16
Output Measure:									
Log Sales	x			x			x		
Sales Pct Change		x			x			x	
Log Value Added			x			x			x

Notes: Table presents specifications from Tables 6 and 7, using the common component of the demand shock $\hat{\Delta}_j^{comm}$ as the instrument for output, instead of the idiosyncratic shock Δ_j^{id}. See notes to Tables 6 and 7 for specification details.

Table A.10: Pass-Through Elasticity: Subgroups of Workers, Alternative Weighting

					Attached and …					
	Perm. Contract *(1)*	Fixed-Term Contract *(2)*	Recent Hire *(3)*	Attached *(4)*	Male *(5)*	Female *(7)*	Low Wage *(8)*	High Wage *(9)*	No HS Degree *(10)*	HS Degree *(11)*
				A. All Firms (Unbalanced Sample of 4173 Firms)						
Sales Elasticity IV	0.228**	0.219	0.226	0.226**	0.136*	0.282	0.195*	0.191**	0.337*	0.081
	(0.115)	(0.159)	(0.210)	(0.107)	(0.073)	(0.229)	(0.107)	(0.086)	(0.193)	(0.066)
Vaue Added IV	0.220**	0.199*	0.253	0.205**	0.153**	0.218	0.158**	0.195**	0.294*	0.128*
	(0.108)	(0.121)	(0.241)	(0.089)	(0.067)	(0.181)	(0.079)	(0.080)	(0.152)	(0.074)
N workers	*90630*	*23300*	*25256*	*90427*	*50212*	*40215*	*46693*	*43734*	*68789*	*30093*
				B. Never-Exiter Firms Only (Balanced Sample of 2926 Firms)						
Sales Elasticity IV	0.102**	0.244*	0.150	0.124***	0.090*	0.141*	0.098**	0.140***	0.132***	0.121
	(0.048)	(0.136)	(0.141)	(0.048)	(0.047)	(0.077)	(0.047)	(0.052)	(0.050)	(0.077)
Vaue Added IV	0.126**	0.311	0.176	0.154**	0.112*	0.169	0.127**	0.173**	0.168**	0.136
	(0.062)	(0.223)	(0.171)	(0.066)	(0.061)	(0.109)	(0.064)	(0.072)	(0.070)	(0.092)
N workers	*65999*	*17229*	*18670*	*65929*	*37502*	*28427*	*33950*	*31979*	*50003*	*22119*

Notes: See notes to Table 9.