Valuing Labor Market Power: The Role of Productivity Advantages

Bryan Seegmiller*

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Abstract

Consistent with the exercise of market power, firms with high labor productivity have low labor shares, high profitability, and high market valuations without high investment rates. I quantify the economic value that firms of different productivity levels derive from their labor market power by estimating the effect of unanticipated firm-level labor demand shocks on wages and employment at publicly listed U.S. firms. Productive firms face lower labor supply elasticities on average, and still lower elasticities for skilled workers, who are disproportionately employed at more productive firms. Using a dynamic wage posting model in which firms face upward-sloping labor supply and adjustment costs in hiring, I estimate that firms in the top and bottom quartiles of labor productivity pay 62% and 94% of marginal product, despite the fact that adjustment costs temper the exercise of labor market power. Markdown differentials can explain three-fifths of the average spread in log labor shares between high- and low-labor productivity firms, and the evolution of these differentials can explain most of the change in the aggregate labor share in the 1991–2014 period. Holding constant equilibrium labor demand, I estimate that about a third of capital income for the typical firm stems from wage markdowns. Aggregate wage markdowns are worth two-fifths of total capital income.

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Market power affects the distribution of firm cash flows between the various claimants on its output; in particular, increased market power is typically associated with a shift in the distribution of productive output away from the firm's workforce and towards owners of the firm's capital. A number of empirical patterns that have emerged over the past several decades suggest that US firms have been earning higher rents due to increased market power. This period of time has been characterized by a large rise in the value of the aggregate stock market and high firm profitability;[1] high firm valuations but with low rates of investment;[2] and, a decline in the share of output accruing to labor.[3] Much of the literature documenting these trends has focused on market power in the product market. In this paper I argue that a "superstar firms" view of labor market power—where firms with high labor productivity also face less labor market competition—can explain a significant share of the cross-sectional differences in firm labor shares and profitability, and can also speak to time series changes in the aggregate labor share. I test this view of labor market competition using a sample of firms containing wide variation in productivity and which features the largest and most highly productive firms in the world: publicly traded US corporations.

In support of the superstar firms view, I show empirically that labor productivity advantages confer a substantial competitive advantage in the labor market. In contrast with local labor market concentration, which has trended downward,[4] I find empirical patterns that parallel the recent shifts in the competitive structure of the US economy. An expanding productivity gap between the most- and least-productive firms has led to both an absolute and relative increase in productive firms' labor market power, resulting in a larger cross-sectional spread in labor shares as well as a lower aggregate labor share. Thus labor market power emerges as another factor in explaining recent macroeconomic trends in the competitive landscape. Furthermore, because labor compensation is the single largest cost of production for most firms, wage markdowns represent a substantial fraction of the capital income generated by US corporations.

I show all this in three steps. First, I motivate my choice to focus on productivity differentials by presenting evidence that firms with high labor productivity earn high economic rents relative to less productive firms. Next, I show empirically that labor market power is likely to play an important part in generating the rents earned by productive firms. Finally, I use a model combined with my empirical findings to value the cashflows that firms derive

[1] See Greenwald, Lettau, and Ludvigson (2021) and Barkai (2020).

[2] Gutierrez and Philippon (2017)

[3] Karabarbounis and Neiman (2013)

[4] Berger, Herkenhoff, and Mongey (2021), Rinz (2018), and Lipsius (2018) all find declining local labor market concentration over the past several decades using Census administrative data.

from their ability to set wages.

To show evidence suggesting that productive firms earn economic rents, I examine their labor shares, operating performance, financial valuation ratios, and investment rates, finding that productive firms have much lower labor shares; higher return on assets/equity, Tobin's Q, market-to-book, and market-to-sales ratios; but do not have significantly larger investment rates. Taken together, these features are consistent with productive firms earning higher economic rents than unproductive firms.

Because the supply elasticity governs wage markdowns, I next introduce a method for estimating supply elasticities that is applicable to a wide range of firms spanning multiple decades. The method provides enough statistical power to allow for heterogeneous cross-sectional and time series estimates, and accounts for the key sources of endogeneity that would cause me to make erroneously large inferences regarding the magnitude of labor market power. Specifically, I use the ratio of the firm-level employment and wage responses to stock returns as a measure of the supply elasticity, controlling for industry-by-year fixed effects and other firm level covariates. This quantity directly estimates the supply elasticity under certain assumptions, and provides an upper bound on the elasticity under a broader set of conditions, which causes my estimates to be a lower bound on the magnitude of labor market power. The most important potential source of confounding variation comes from common market level productivity shocks, which could bias my supply elasticity estimates downward. Accordingly, I show that my baseline estimates are insensitive to a wide variety of additional controls for common shocks, implying that my estimates are driven by the firm-specific component of stock returns. I find that my baseline average supply elasticity estimate is well within the range found in the literature and is highly robust to different sets of controls. I also find that alternate labor demand shock proxies—such as using the stock returns of firms' customers, patent grants, or only including stock returns near quarterly earnings announcements—yield supply elasticity estimates that are nearly identical to my baseline specification, though with less statistical precision.

Applying my method to firms sorted on labor productivity, I show that highly productive firms face much lower labor supply elasticities than less productive firms. In models of monopsony, wages move further below marginal product as the firm-specific supply elasticity decreases, so this suggests that productive firms enjoy more monopsony power, a key source of their economic rents. Decomposing wages into worker- and firm-specific heterogeneity to back out worker skill, I find that productive firms also face lower supply elasticities for workers of all skill levels; the most skilled workers have the lowest supply elasticities of all, implying that labor market power is increasing in worker skill. In the time series I find that labor supply elasticities have decreased overall and for each worker skill level, and especially so for skilled workers. The spread in supply elasticities between productive and unproductive firms has been widening over time, leading to an increased gap in firm-level labor shares between top and bottom firms.

The decline in my estimated supply elasticities matches the secular shift towards increased rents and falling labor shares. The key role that skilled workers play in these phenomena can possibly help explain why supply elasticity estimates trend downward, even as local labor market concentration has decreased. For skilled workers, labor markets are much less local (see Malamud and Wozniak, 2012; Amior, 2020, for example), diminishing the importance of concentration defined over narrow geographic boundaries. Instead, I suggest that the increased importance of human capital specificity may be a key contributor. Consistent with this notion, the wage premium for incumbent workers has increased and worker separations rates have decreased, suggesting that it has both become more costly to replace an incumbent worker and more difficult for an incumbent worker to leave the firm.

The empirical incumbent wage premium suggests that it may be costly to replace workers within the firm (Kline, Petkova, Williams, and Zidar, 2019). Prior research also uncovers direct evidence that firms find it costly to adjust their labor force.[5] I further find that firm hiring decisions respond to a labor force analogue of Tobin's Q, also suggesting labor is costly to adjust by analogy to a long literature in finance on investment with costly capital adjustment. In the presence of labor adjustment costs the supply elasticity may not be a sufficient statistic for wage markdowns from marginal product, and so quantifying the extent of labor market power also requires accounting for the impact of a labor force that is costly to adjust. Accordingly, I quantify my empirical findings in the context of a dynamic wage posting monopsony framework, which is based on the static models of Kline et al. (2019) and Card, Cardoso, Heining, and Kline (2018) and features costly hiring of workers from outside the firm. I use the model to estimate an adjustment cost factor that allows me to convert empirically estimated supply elasticities into markdowns from marginal product.

In the model firms face upward sloping labor supply curves for both incumbent workers and potential recruits. Firms may offer different wages to these two groups because of adjustment costs in hiring workers from outside the firm. These adjustment costs could stem from the need to train new hires to utilize the firm's production technology and/or the costliness of recruiting outside workers. I calibrate the model to match empirical labor supply elasticity estimates for incumbents and recruits; average worker separations rates;

[5]In the labor literatureJager and Heining (2019) show that incumbent workers are costly to replace; in asset pricing, Belo, Lin, and Bazdresch (2014) argue that labor adjustment costs can explain the empirical relationship between firm hiring and expected stock returns.

and, the average incumbent wage premium. The model is also able to generate the empirical pattern of monotonically decreasing supply elasticities and labor shares as firm productivity increases, without explicitly targeting these moments. My main calibration matches data moments for the full sample period from 1991-2014; I also separately calibrate the model for the 1991-2002 and 2003-2014 subperiods. I then use the model along with my empirical estimates to quantify the value of cashflows that firms derive from paying workers less than their marginal product.

Combining my empirical results and the model calibration, my main quantitative findings are the following. The full-sample calibration of the dynamic model with adjustment costs shows that wage markdowns are about 16% smaller relative to a static model without costly adjustment. However, the dollar value of wage markdowns still represent a substantial portion of firm cash flows. I find that the average firm pays workers about 83% of their marginal product, with firms in the top productivity quartile paying about 62%, and the least productive firms paying 94%. These wage markdowns in turn represent a meaningful portion of firm cash flows. I perform a counterfactual exercise where I reverse wage markdowns to remunerate back to workers their marginal products, holding constant firms' production decisions. Overall, wage markdowns are worth about a third of the average firm's operating income. This figure is 17% for firms in the bottom quartile of the labor productivity distribution and 43% for firms in the top quartile. Aggregating the dollar value of wage markdowns across firms, wage markdowns are worth on average about two-fifths of aggregate operating income.

I further show that wage markdowns can explain about three-fifths of the gap in average labor shares between the firms in the top and bottom quartiles of the labor productivity distribution; meanwhile, my supply elasticity estimates and model calibrations for the 1991-2002 and 2003-2014 subperiods imply that changes in markdowns can explain the majority of the observed decline in the labor share over that time period.

The implication that productive firms exert more labor market power despite paying high wages to their employees appears counterintuitive. But productive firms' wages are low relative to a very high marginal revenue product of labor, not low in the absolute sense, which is entirely consistent with the empirical fact that productive firms also have low labor shares. It also suggests the need for some nuance when considering the welfare implications of this type of labor market power. For example, is labor is inelastic because of valuable firm-specific human capital, then breaking up productive firms to reduce labor market power could have negative consequences for both employees and employers if it devalues the workers' human capital in some way. On the other hand, the welfare implications are more clearly negative if firms generate some of their labor market power by using their resources to artificially curb worker mobility. In section 6 I show evidence that both forces are likely to be important.

I organize the main body of the paper as follows. In Section, 1 I discuss the data sources and basic summary statistics; in Section 2 I examine stylized facts that are consistent with productive firms earning economic rents; I explain my method for estimating supply elasticities and apply it to firms sorted on labor productivity in Section 3; in Section 4 I then introduce and calibrate a dynamic wage posting model with labor adjustment costs; in Section 5 I combine my model and empirical estimates to quantify the contribution of labor market power to firm capital income and its impact on aggregate and firm-level labor shares; in Section 6, I discuss economic and policy implications and alternative explanations for my findings. Finally, Section 7 concludes.

Related Literature

This paper contributes simultaneously to several different literatures. First of all, this paper adds to a growing literature in macro-finance which examines recent macroeconomic trends in competition, firm valuations/operating performance, and labor shares.[6] Most related to my paper is Hartman-Glaser, Lustig, and Xioalan (2019), who document that large firms have obtained an increasing share of their sales as capital income over the past several decades and propose an explanation based on firms providing insurance to their workers against increased risk. I similarly find that labor shares have decreased by more among highly productive firms. I argue that this is in large part driven by productive firms marking down wages further from marginal product. I also provide the novel finding that firms generate a substantial portion of their operating income from wage markdowns, which is especially the case for firms with high labor productivity. This competitive advantage generates rents which are reflected in higher financial valuation ratios and relatively low investment rates among firms with high labor productivity.

A parallel area of research in macroeconomics connects the secular decline in labor shares with changes in market competition. I differ from most of this literature in primarily focusing on the role of labor market power instead of product market power.[7] While some papers in this literature have focused on trends in productivity dispersion as a driving force behind changes in product market competition, my findings imply that increases in the labor market power of the most productive firms have also played a crucial role in my sample of publicly

[6]These include Barkai (2020), Grullon, Larkin, and Michaely (2019), Covarrubias, Gutiérrez, and Philippon (2020), Greenwald et al. (2021), Corhay, Kung, and Schmid (2020), and Farhi and Gourio (2018).

[7]See Autor, Dorn, Katz, Patterson, and Van Reenen (2020), De Loecker, Eeckhout, and Unger (2020), Covarrubias et al. (2020) and Kehrig and Vincent (2021) for examples. One exception is Stansbury and Summers (2020), who argue that worker bargaining power has played an important role.

traded companies.

This paper also builds on recent research in labor economics. A number of prior papers estimate the elasticity of supply to the firm to infer the extent of monopsony power;[8]Another related empirical literature has examined how labor market concentration affects wages.[9] My paper differs from others in this literature by quantifying the impact that labor adjustment costs have on wage markdowns. I also estimate heterogeneity in labor supply elasticities based on firm characteristics, focusing on labor productivity in particular. Another large literature estimates the passthrough of firm-specific shocks to worker outcomes.[10] These papers typically analyze the response of worker pay to firm-specific shocks, whereas my focus is on estimating the ratio of the employment and wage responses to stock return shocks in order to estimate the supply elasticity.

In a related prior paper, Gouin-Bonenfant (2020) argues theoretically for labor productivity dispersion as a determinant of labor market power in a search and matching model, and he shows empirically that productivity dispersion depresses labor shares at the aggregated industry level in Canada. I find that productivity advantages also reduce labor shares at the firm level in the United States, and this is at least partly direct result of differences in monopsony power. Thus my findings complement those of Gouin-Bonenfant (2020). My paper is also the first, to my knowledge, to explicitly estimate heterogeneous supply elasticities for firms of different productivity levels both cross-sectionally and across time.

Finally, this paper adds to a growing body of work in financial economics that examines the interplay between labor markets and firm financial outcomes and decision making.[11] My findings suggest that accounting for imperfect competition in labor markets may be of first-order importance for future research in this area.

1 Data and Summary Statistics

My analysis relies primarily on two data sources. The first is employer–employee matched wage data from the US Census Bureau's Longitudinal Employer Household Dynamics

[8]See Lamadon, Mogstad, and Setzler (2019), Berger et al. (2021), Kroft, Luo, Mogstad, and Setzler (2020), Bassier, Dube, and Naidu (2020), and Ransom and Sims (2010) for a few examples.

[9]See Rinz (2018), Benmelech, Bergman, and Kim (2018), Schubert, Stansbury, and Taska (2020), and Jarosch, Nimczik, and Sorkin (2019), for example.

[10]See Abowd and Lemieux (1993), Van Reenen (1996), Guiso, Pistaferri, and Schivardi (2005), Kline et al. (2019), Kogan, Papanikolaou, Schmidt, and Song (2020), Chan, Salgado, and Xu (2021), Garin and Silverio (2020), Friedrich, Laun, Meghir, and Pistaferri (2019), Balke and Lamadon (2020).

[11]References in asset pricing include Eisfeldt and Papanikoloau (2013), Belo et al. (2014), Donangelo, Gourio, Kehrig, and Palacios (2019), Kuehn, Simutin, and Wang (2017), Liu (2019); see Matsa (2010), Jeffers (2019), Mueller, Ouimet, and Simintzi (2017), Kim (2020), and Shen (2021) for examples in corporate finance.

Database (LEHD). I link the firm identifiers in the LEHD to financial information in the CRSP/Compustat merged database obtained from Wharton Research Data Services. I describe the LEHD and CRSP/Compustat–LEHD merged data and samples below. Besides stock return and market cap data, which are from CRSP, all financial variables are from Compustat. Henceforth I refer to the CRSP/Compustat merged sample as the Compustat sample.

1.1 Employee-Employer Matched Wage Data from the LEHD

The Longitudinal Employer Household Dynamics Database (LEHD) contains restricted-use microdata with wage and employer information for individuals in the United States. Wage data in the LEHD are collected from firms' unemployment insurance filings, and they contain all forms of compensation that are immediately taxable, including stock options. Individuals in the LEHD are linked to their employers through their State Employer Identification Number (SEIN). The LEHD provides crosswalks between the SEIN and the federal Employer Identification Number (EIN), which is also available for firm-level data sources such as Compustat. The LEHD data begin in 1990, although most states join later as the LEHD coverage becomes more comprehensive; the LEHD covers the majority of jobs in the United States by the mid- to late-1990s, and coverage ends in 2015.[12] The wage data are reported on a quarterly basis, and cover nearly 100 percent of private employees in state-quarters where the data are available. See Abowd, Stephens, Vilhuber, Andersson, McKinney, Roemer, and Woodcock (2009) for a more detailed overview of the construction of the LEHD.

1.2 Matched Compustat-LEHD Sample

I link firm identifiers in each year of the the LEHD to Compustat records using a crosswalk created by Larry Schmidt.[13] I then assign individuals to their corresponding Compustat gvkey and retain worker-years in which at least one of the worker's employers was linked to a Compustat firm. Following Sorkin (2018), I convert quarterly LEHD wages to their full-year equivalents. Adjusting wages for a given year following the Sorkin (2018) procedure requires information on wages in the year before and the year after the current year. See appendix section A for more details. Given the availability of the LEHD wage data from 1990-2015, this means my effective sample period spans 1991-2014. In order to be in the sample, I require a Compustat firm to have at least 15 workers for whom that firm is their primary

[12] An overview of LEHD coverage is available at https://www2.vrdc.cornell.edu/news/data/qwi-public-use-data/.

[13] Thanks to Larry for sharing his crosswalk code.

employer (defined to be the firm where the worker earned the most income that year). I additionally exclude financial firms and regulated utilities from my analysis following common practice. Because I use NAICS industry codes throughout, this excludes the 2-digit NAICS codes 22, 52, and 53. In appendix Figure A1 I show the shares of employment, market cap, and sales represented in my LEHD-Compustat matched sample. On average, my matched sample covers about 62% of employment, 63% of market cap, and 50% of sales represented in Compustat in a given year. I compare the distribution of firm characteristics for my matched sample and the overall Compustat database in appendix Table A1. Not surprisingly, firms in my matched sample skew a little bit larger relative to the mean or median firm in Compustat based on various measures of firm size such as assets, employment, or market cap. The distributions of annual excess stock returns are about the same across the two samples. I obtain the risk-free rate from Ken French's data library in order to calculate excess returns.

1.3 Variable Construction

My proxy for labor productivity is given by log value-added per worker, following a large literature in labor economics (see Card et al., 2018, for a review). Firms can have higher labor productivity by enjoying higher total-factor productivity, being more capital intensive, or by hiring more skilled workers. I define value-added for Compustat firms following Donangelo et al. (2019), as the sum of operating income before depreciation, changes in inventories, and labor expenses. Firm level labor shares are the ratio of labor expenses to value added. In order to estimate the skill level of individual workers (or the average skill of a given firm), I follow a long literature starting with Abowd, Kramarz, and Margolis (1999)—from now on AKM—in decomposing observed wages into worker- and firm-specific heterogeneity. I start with a modification of the AKM decomposition proposed by Lachowska, Mas, Saggio, and Woodbury (2020) and Engbom and Moser (2020) that allows for the firm-specific component of wages to vary by time. I use LEHD data to create measures of firm wage offers and total labor expenses. Details on my construction of all these variables can be found in appendix section A.1.

2 Firm Labor Productivity and Labor Shares, Valuations, and Investment

In this section I document the following facts: productive firms 1) have lower labor shares; 2) have higher operating performance and valuation ratios; 3) but do not have high rates of investment. These facts in tandem are consistent with productive firms earning higher economic rents, and motivates my choice later on to examine supply elasticities for firms sorted on labor productivity. I emphasize that the evidence in this section is suggestive and correlational rather than causal, and do not by themselves constitute direct evidence for labor market power. Direct evidence on the magnitude of rents earned from labor market power instead come from when I estimate the labor supply elasticity to the firm in section 3. I focus on valuation ratios and labor shares because rents earned from market power should be associated with both higher valuations and lower shares of output accruing to labor, all else equal.[14] As shown by Crouzet and Eberly (2021) and Abel and Eberly (2011), the Tobin's Q valuation ratio reflects both rents and marginal Q, the marginal increase in the value of the firm with respect to an additional incremental unit of capital. In the presence of economic rents Tobin's Q overstates investment opportunities, and firms earning more rents have higher Tobin's Q without an attendant increase in investment.

In Table 1 I examine the relationship between labor productivity, labor shares, valuations, and operating profitability. In panel A I study the relationship between log value-added per worker and firm-level labor shares and log valuation ratios, respectively. Without controlling for industry-by-year fixed effects in the first column, the elasticity of the labor share with respect to labor productivity is a highly significant -.34. Size effects increase this slightly in the second column. Adding industry-by-year (where industry is defined at the 3-digit NAICS level) fixed effects increases the magnitude of this elasticity considerably, to about -.51 in column 3. Note that even without market power, labor shares can also vary negatively with productivity as a result of production function variation.[15] Production function variability is likely to be larger between rather than within industries, but the negative productivity-labor share relationship is stronger within industries. This is consistent with the market power of more productive firms playing a role in driving down firm level labor shares.

In panel B I look at the relationship between log value-added per worker and the log of four different valuation ratios, focusing on the specification with size controls and industry-by-year fixed effects. Log value-added per worker is strongly associated with increased log total Tobin's Q (which is taken from Peters and Taylor (2017) and includes both intangible and

[14]For labor shares, with a Cobb-Douglass production function the labor share is $(1-\alpha)$, where $(1-\alpha)$ is the labor returns to scale. When there is a price markup over marginal cost due to product market power, the labor share is (price markup)$^{-1} \times (1-\alpha)$, where the price markup is the ratio of the price to marginal cost. With labor market power the labor share is wage markdown $\times (1-\alpha)$, where the wage markdown is the ratio of the wage to marginal revenue product of labor.

[15]If the production function is CES then labor shares are naturally decreasing in the productivity level when capital and labor are complements (Donangelo, 2020). Returns to scale could also differ so as to be correlated with log value-added per worker.

physical capital) and in my employment-based Tobin's Q measure. Both have elasticities above 0.5 and highly significant t-stats. Productive firms have high enterprise values relative to their installed capital stock or skill-weighted labor force, as would be expected in the presence. The last two columns show that labor productivity is also associated with high market capitalization relative to book equity and sales.

Market power should also yield increases in profitability. I find that productive firms exhibit better operating performance in panel C of Table 1. This is true both contemporaneously and in the future, so the relationship is not merely a mechanical result arising from transitory increases in productivity that correlate with temporary contemporaneous increases in profitability. Instead, productivity and profitability show a persistent correlation intertemporally.

If the high valuations of productive firms are due to rents and not because of growth opportunities, we should not see a consummate rise in investment (or hiring) in response to valuation differences. In Panel A of Table 2 I sort firms into four quartiles of labor productivity and show the average Tobin's Q and average investment rates across these quartiles. In the first two rows I show the Peters and Taylor (2017) total (physical plus intangible) investment rates and average total Q. There is a strongly increasing average Q, with an average of about 1.70 for the top quartile of productivity and 0.73 for the bottom quartile. The average investment rates are only slightly higher for the top quartile relative to the bottom quartile, this difference is not statistically significant, and the middle two quartiles of productivity have lower average investment rates than the bottom quartile. The results are similar, when I examine the hiring rates relative to my measure of employment Q in the bottom two rows of Table 2. The employment Q is several times larger for highly productive firms, and yet their hiring rates are actually significantly lower.

These findings are consistent with the result in Crouzet and Eberly (2021) showing that when firms earn rents their Tobin's Q overstates marginal Q, and hence overpredicts their investment decisions relative to actual investment. I examine this relationship further in panel B of Table 2, by running a regression of investment rates on Q ratios with productivity quartile-specific slopes and for investment types $k = Total, Hire$. As in Peters and Taylor (2017) I include firm and year effects. I also add labor productivity quartile fixed effects and further control for firm cashflows.

$$\text{Inv Rate}_{t+1}^k = \alpha^k + \alpha_{q(j,t)}^k + \alpha_j^k + \alpha_t^k + \sum_{q=1}^{4} \mathbf{1}(q(j,t) = q) \times \left(\beta_q^k Q_{j,t}^k + \delta_q^k CF_{j,t}\right) + \epsilon_{j,t}^k \qquad (1)$$

The $q(j,t)$ denotes firm j's time-t productivity quartile, and $\alpha_{q(j,t)}^k$ are labor productivity quartile specific intercepts. Finally, $Q_{j,t}^k$ denotes the firm's total or employment Tobin's Q ratio, and $CF_{j,t}$ are the firm's cash flows, defined as in Peters and Taylor (2017). Panel B of Table 2 shows that β_q^k coefficients are lowest for the most productive firms and larger for the least productive firms, for both investment in capital and for hiring new workers. The fifth column gives the p-value on a test that the top and bottom labor productivity quartile slope coefficients are equal, which is rejected at high levels of statistical significance in both regressions. These findings could also be explained by productive firms facing higher adjustment costs, so in appendix Tables A3 and A4 I control for adjustment cost proxies, still finding the same basic result. I describe this test in appendix section B. In sum, Table 2 provides suggestive evidence that average Q ratios overstate marginal Q for productive firms because they reflect in part the value of economic rents earned from current assets in place and not future investment opportunities.

Finally, I look at trends in the spread in productivity and Tobin's Q ratios between the most and least productive firms. I again sort firms into labor productivity quartiles and take the average log value-added per worker and log total/employment Q ratios for each year. I then take the difference between firms in the top- and bottom-quartiles of labor productivity over a three-year moving average centered at the current year. I plot the resulting series in Figure 1. The spread in value-added per worker has increased, echoing the findings of De Loecker et al. (2020), Autor et al. (2020), and Gouin-Bonenfant (2020), among others. Lastly, the spread in both of the average log Q ratios has also increased, suggesting a widening gap in economic rents between the most- and least-productive firms, likely driven in part by the increasing gap in productivity between the two groups.

Overall, the empirical patterns documented in this section are consistent with high and potentially increasing economic rents being earned by firms with high labor productivity. Productive firms have high market valuations without high investment rates, and also have lower labor shares. These stylized facts are correlational rather than causal, and could be consistent with the presence product market power, as in Barkai (2020), De Loecker et al. (2020), and Autor et al. (2020), as well as labor market power. In the following sections I examine to what extent, if any, labor market power could play a role in driving some of these empirical patterns. Direct evidence on the extent of labor market power comes from estimating the elasticity of labor supply to the individual firm, which I do in the next section.

3 Firm Productivity and Labor Market Power

In this section I first discuss my method for estimating supply elasticities. I then apply my method to firms sorted on labor productivity.

3.1 Relationship Between Wage Markdowns and the Supply Elasticity

The firm-specific elasticity of supply is the key quantity that determines wage markdowns in standard models of monopsony. To see this, consider the basic static monopsony framework (Robinson, 1969). The firm has a revenue function $F(L)$ and faces a labor supply function $L(w)$, where w is the wage offer. The firm solves:

$$\max_{w} \quad F(L(w)) - wL(w) \tag{2}$$

First order conditions give

$$w = \frac{\epsilon}{1+\epsilon} F_L \tag{3}$$

where

$$\epsilon = \frac{dL}{dW}\frac{w}{L} \text{ is the supply elasticity.} \tag{4}$$

Define

$$\frac{\epsilon}{1+\epsilon} \equiv \text{ wage markdown} \tag{5}$$

Thus the wage markdown represents the fraction of labor's marginal revenue productivity that workers obtain in wages. In perfect competition $\epsilon \to \infty$, and the wage equals F_L, the marginal revenue product of labor.

3.2 Estimating Supply Elasticities

As emphasized by Manning (2003), supply elasticities can be identified by shocks to the marginal revenue product of labor, holding the firm-specific labor supply curve constant. In order to estimate supply elasticities, labor demand shocks to should be: 1) a persistent, unanticipated shock to firm productivity; 2) firm-specific; 3) not correlated with shocks to firm-specific labor supply. An objective of this paper is to estimate heterogeneous supply elasticities for a panel of Compustat firms sorted on labor productivity, as well as to allow for time-variation in these estimates, so my labor demand shock also needs to be available for a wide variety of firms and for a long period of time.

3.2.1 Stock Returns As a Labor Demand Shock

Firm-specific stock returns satisfy the above criteria for a labor demand shock quite well. In particular, they reflect a revision in expected discounted future cashflows, and hence inherently correlate with changes in current and expected future revenue productivity.[16] Consider the standard asset pricing equation that determines the price of a stock:

$$P_t = E_t \left[\sum_{k=0}^{\infty} M_{t,t+k} D_{t+k} \right]$$

Here D_{t+k} is a cashflow occuring at time $t + k$ and $M_{t,t+k}$ is the time t discount factor for time $t + k$ cashflows. The time t stock return is

$$R_t = \frac{D_t + P_t}{P_{t-1}}$$

Most of the variation in stock returns is firm-specific, and the common components can be removed rather easily. Because stock returns react to new information, they are inherently unexpected, and because they are forward looking they represent persistent changes in the outlook of the firm going forward. All this suggests that stock returns are good candidates for unexpected shocks to firm revenue productivity, which in turn moves firm labor demand.

However, firms may also gain value from the willingness of workers to be employed at the firm for any given wage, which affects the level of the labor supply curve. Unexpected changes in the level of labor supply could bias supply elasticity estimates when using stock returns as an instrument for labor demand shocks. To build intuition about the form this bias may take, I derive closed form expressions for the bias in Appendix C with a simple model of the labor market that takes inspiration from Card et al. (2018) and Lamadon et al. (2019). There are two key insights from this exercise. The first is that failing to account for market-wide shocks is likely to bias stock-return based elasticity estimates towards zero, causing me to make erroneously large inferences about the importance of labor market power. Because of this I demonstrate the insensitivity of my baseline elasticity estimates to a host of

[16] Previous research provides evidence that while discount rate shocks drive much of the variation in the aggregate stock market, news containing information about future cashflows drives the vast majority of the variation in firm-level stock returns. For example, Vuolteenaho (2002) finds that the idiosyncratic component of individual stock returns is almost entirely driven by firm-specific cash flow news, while movements in discount rates are primarily common across firms. Recent research (Neuhierl, Scherbina, and Schiusene, 2013; Boudoukh, Feldman, Kogan, and Richardson, 2018) also shows that fundamental information about firms, such as unexpectedly high earnings, acquisitions, or new product announcements, are an important driving force behind stock price movements. All these shocks can be expected to relate to firms' marginal revenue productivity.

specifications using different proxies for market-level shocks.

The second insight is that once market shocks are accounted for, any remaining bias is likely to cause my estimates to be conservative. This follows from the fairly innocuous assumption that firms do not cut amenities (or workers don't reduce their common perception of firms' amenities) by too much on average when idiosyncratic productivity improves. Hence workers shouldn't view the firm as a worse place to work at when firm-specific productivity goes up. This assumption implies my elasticity estimates are an upper bound on the true elasticity. My estimation strategy correctly identifies the supply elasticity when I further invoke the identifying assumption that only market-specific shocks move the level of the labor supply curve. It should be noted that this assumption is invoked in most papers that estimate passthrough parameters of firm-specific shocks to workers.[17] This is because unobservable firm-specific amenities shocks bias passthrough coefficients. This is an issue even when using plausibly exogenous variation in firm revenue productivity, because one still cannot rule out in general that amenities could also move as a response to the shock.

Because of this potential bias, in robustness checks detailed in Appendix Section E I examine elasticity estimates implied by different shocks to revenue productivity, and discuss the different identifying assumptions each implicitly invokes in order to estimate the supply elasticity. Finally, I identify sets of firms that were likely to have experienced observable firm-specific labor supply shocks and check how sensitive elasticity estimates are to the inclusion of these observable controls.

My specification to obtain a baseline average supply elasticity estimate is as follows:

$$\log(Y_{j,t+1}) - \log(Y_{j,t}) = \alpha + \alpha_{I(j),t} + \beta \text{Stock Ret}_{j,t\to t+1} + \Gamma X_{j,t} + \epsilon_{j,t} \tag{6}$$

Here $Y =$ firm Compustat employment or firm average full-time full-year equivalent adjusted wage. The ratio of the estimates $\widehat{\beta}^{Emp}/\widehat{\beta}^{Wage}$ gives the supply elasticity. The $\alpha_{I(i),t}$ denote 3-digit NAICS industry $\times$ year fixed effects, which are intended to control for common market shocks. The controls $X_{j,t}$ include the contemporaneous change in log firm average AKM worker effects $\alpha_{j,t}$ (defined in equation (A.8) in appendix section A.1); and, lagged growth rates in employment, wages, and firm assets. I include the contemporaneous changes in worker effects in case stock returns lead to changes in the skill composition of the workforce. If this is the case, then failing to account for skill changes may cause employment and wage growth to be misstated in terms of constant efficiency units of labor. I include lagged growth

[17] For example: Lamadon et al. (2019), Kline et al. (2019), Garin and Silverio (2020). Papers in the asset pricing literature which incorporate labor market frictions (such as Kuehn et al. (2017), Belo et al. (2014), or Donangelo (2014)) also assume that labor supply shocks are determined at the market level.

rates in firm wages, employment, and assets to control for any pre-trends in labor demand or firm expansions that may be slightly correlated with future stock returns. The identifying assumption of the estimation strategy is that labor supply shocks operate at the market level, defined by industry-year. The annual stock returns in (8) are given by the sum of the firm's monthly returns in excess of the risk-free rate from July of year t through June of year $t+1$. Due to some extreme outliers in the tails of monthly returns, I cross-sectionally winsorize the outer 0.25% from each tail before summing excess returns to the annual level, although my findings are not meaningfully affected in any way by this decision.

Before estimating elasticities via (6), I first look at wages and employment responses over different horizons to see if there are pre-trends. Similar to Kogan et al. (2020), I examine the growth in average wages or employment over different forward- and backward-looking horizons of h years:

$$\log\left(\frac{1}{|h|}\sum_{k=1}^{|h|} Y_{j,t+k\times\text{sign}(h)}\right) - \log(Y_{j,t}) = \alpha + \alpha_{I(j),t} + \beta_h \text{Stock Ret}_{j,t\to t+1} + \Gamma X_{j,t} + \epsilon_{j,t} \quad (7)$$

Here Y = firm employment or average wage. This specification gives the growth of average employment or wages over the horizon and highlights persistent changes to wages and employment induced by stock return shocks. I estimate (7) for horizons $h=-5$ to $h=5$ years. For $h=1$ year the specification (7) is the same as (6). I plot the results in Figure 2. Because of the unforeseen nature of a shock to firm-specific stock returns, the pre-trends are non existent for both wages and employment. Meanwhile, both employment and wages display large, statistically significant and persistently positive responses at the time of and following the stock return shock. The joint positive employment and wage responses to movements in stock prices are consistent with my argument above that stock returns constitute a powerful and essentially unpredictable labor demand shock.

To examine pre-trends and shock responses at different horizons by worker skill, in Figure 3 I again estimate the employment and wage responses over forward- and backward-looking horizons as in (7), except broken down by each worker skill level. I define workers in the bottom two quintiles of estimated worker wage effects in (A.5) to be low skill, the third and fourth quintiles to be middle skill, and the top quintile to be high skill. Similar to Figure 2, I find no pre-trends and a strong positive wage and employment responses to a stock return shock. The skilled worker wage response is the largest in magnitude, generating a lower supply elasticity for skilled workers as found in Table 4.

3.2.2 Baseline Elasticity Estimates

In Panel A of Table 3 I estimate supply elasticities for workers of different skill levels. My baseline pooled average supply elasticity estimate is about 2.5, found in the first column of Panel A in 3. This is well in line with the range of estimates found in previous research, although on the slightly smaller end.[18] This is likely because of my focus on publicly traded firms, which are much larger than the typical firm in the US economy. The remaining columns show that supply elasticities decline in worker skill. This may cause one to worry that the larger wage passthroughs for skilled workers come because skilled workers who are insiders are simply able to appropriate more rents generated from positive firm specific shocks. In Panels B and C of Table 3 I split workers into incumbent and recruit status, where incumbents have been employed at the firm at least since the previous year. I find lower elasticities for incumbent workers. However, consistent with a monopsony explanation for the low elasticities skilled workers, I also find that skilled workers face the lowest supply elasticities among recruits. My wage posting model in section 4 matches the pattern of high high wage passthroughs and low incumbent supply elasticities, which is caused by the costliness of hiring workers from outside the firm and the presence of an incumbent wage premium.

3.2.3 Robustness Checks

I run a host of robustness checks for my estimates of (6), which I cover in more detail in in Section E of the appendix. Since a major source of confounding variation comes from aggregate shocks, I carefully check to see how my estimates vary with different controls for common market level shocks in E.1. One may also worry about the quantitative impact of other sources of endogeneity. In E.2 I instead estimate supply elasticities with several alternative labor demand shifters—including customers' stock returns, R&D tax credits, patenting, and earnings announcement returns—finding very similar estimates. In E.3 I also show that controlling for salient observable firm-specific labor supply shocks, such as unionization events or changes in non-compete enforceability, have no effect on the average supply elasticity estimate. The common finding from these exercises is that my baseline average supply elasticity estimate is very close economically and statistically to the various alternatives, suggesting that bias in my estimates is likely to be quantitatively small.

[18] Sokolova and Sorensen (2021) find the median supply elasticity estimate is around 1.7 in a meta-analysis. Bassier et al. (2020) point out that recent quasi-experimental evidence—such as Kroft et al. (2020), Cho (2018), and Dube, Manning, and Naidu (2018), and Caldwell and Oehlsen (2018)—has found supply elasticities between 2 and 5.

3.3 Supply Elasticities for Firms Sorted on Labor Productivity and Over Time

The stylized facts in Section 2 imply that such a sorting on labor productivity should induce wide variation in labor shares, firm valuations, and profitability, suggesting these firms earn rents but does not necessarily imply anything about their labor market. To provide direct evidence on differential labor market power across firms of varying labor productivity levels, I now estimate heterogeneous elasticities for firms sorted by labor productivity as proxied by log value-added per worker. In Table A2 I find that productive firms hire more skilled workers on average, and so I also break down my estimates by worker skill level. I sort firms into quartiles on log value-added per worker and estimate heterogeneous coefficients for each quartile of the productivity distribution. My main specification is the following:

$$\log(Y_{j,t+1})-\log(Y_{j,t}) = \alpha+\alpha_{q(j,t)}+\alpha_{I(j),t}+\sum_{q=1}^{4}\mathbf{1}(q(i,t)=q)\times\beta_q\text{Stock Ret}_{j,t\to t+1}+\Gamma X_{j,t}+\epsilon_{j,t} \tag{8}$$

where again Y = firm Compustat employment or firm average full-time full-year equivalent adjusted wage. Here $q(j,t)$ denotes the productivity quartile of firm j at time t. The controls $X_{j,t}$ are the same as introduced in (6), including lagged employment, wage, and asset growth, and the contemporaneous change in firm average worker skill. Finally, I add quartile-specific fixed effects $\alpha_{q(j,t)}$ to allow each productivity quartile to have a different intercept and slope. I also estimate (8) separately for workers of different skill levels. I define workers in the bottom two quintiles of the distribution of worker effects in the AKM decomposition (A.5) to be low-skilled; the third and fourth quintiles to be middle-skilled; and the top quintile to be high skilled. I separately compute the average wages, average AKM worker effects, and employment by worker skill level.[19]

In Table 4 I estimate (8) for the overall firm and for each skill type. Table 4 shows that more productive firms face lower supply elasticities at the firm level and at for each skill level. The overall supply elasticity for a highly productive firm is about 1.17, while it is 4.32 for a low productivity firm. These estimates mask heterogeneity by worker skill level; the

[19]Since I can't observe the share of Compustat employment by worker skill, I assume the number of workers of a given skill level are proportional to their observed shares in the LEHD. So if the firm has $N_{j,t}$ workers in the sample of the AKM decomposition (A.5) in year t, $N^s_{j,t}$ of which are from skill group s, then I assume Compustat employment in group s is equal to $EMP^s_{j,t} = EMP_{j,t}\times N^s_{j,t}/N_{j,t}$. I continue to use Compustat employment because it is reported at year end, which allows more time for employment to respond to a stock return shock than LEHD employment. Compustat employment also reflects workers at all the firm's establishments instead of just those at the establishments that are covered by the LEHD in that year. I make the same assumption when breaking down workers into their status as either incumbents or recruits.

highest skill workers have much lower supply elasticities, implying greater wage markdowns for the most skilled workers. However, even for low- and middle-skill workers the elasticities are decreasing in firm productivity. The test on coefficient differences between high- and low-productivity firms in fifth column also shows that differences in estimates elasticities are driven primarily by higher wage responses in the denominator—highly productive firms move wages by more to induce a given change in employment relative to unproductive firms.

The lower supply elasticities that productive firms face provide a competitive advantage, allowing them to markdown wages by more relative to marginal product, generating substantial economic rents to the firm. This finding squares with the results in Section 2 where I documented evidence that these same firms exhibit behaviors consistent with them earning rents derived from market power, such as having low investment and hiring rates given valuations and much lower labor shares.

Table 4 also shows that more skilled workers face lower supply elasticities at any given productivity level relative to less skilled workers. This implies that highly skilled employees have a greater difficulty of finding suitable substitutes among different potential employers. Skilled workers also exhibit much larger attachment to their employers. I find that the average separations rate for a firm's low-skilled workers is about 35.5%, whereas it is 24.6% for skilled workers. This suggests that firm-specific human capital could be a key driver of monopsony power among this group. Highly skilled workers are more educated and have generally made more specific human capital investments. This has the benefit of making skilled workers productive to their employers, but also leads to more difficulty in substituting their labor across firms. However, there is a flip side to this coin, which is that employees with specific skills are also hard for employers to replace. This can mitigate some monopsony power. In section 4 I present a dynamic wage posting model where hiring outside workers is costly, causing incumbent workers to enjoy a wage premium relative to the standard static marginal product markdown implied by the elasticity alone. I calibrate the model to quantify the attenuating influence such adjustment costs may have on firms' ability to fully exercise their monopsony power.

I now estimate time-varying heterogeneous supply elasticities. I estimate the baseline specification (6) for overlapping moving 3-year windows, both for the whole firm and each individual skill level. In Figure 4 I find that estimated supply elasticities have been decreasing for each skill group, and especially for skilled workers. Movements in labor market power may therefore have explanatory power for observed declines in the aggregate labor share documented by Karabarbounis and Neiman (2013), Autor et al. (2020), and numerous others. I examine how changes in elasticities impact the aggregate labor share in section 5.

My supply elasticity estimates can also explain cross-sectional dispersion in firm-level labor shares over time. To demonstrate this, I estimate (8) for overlapping moving 3-year windows and take the elasticity estimates for the top- and bottom-productivity quartiles. I use the standard markdown formula to estimate the elasticity-implied spread in log markdowns between the two productivity types and compare this to the spread in log labor shares. The log markdown spread is

$$\text{Log Markdown Spread}_t = \log\left(\frac{\widehat{\epsilon_{4,t}}}{1+\widehat{\epsilon_{4,t}}}\right) - \log\left(\frac{\widehat{\epsilon_{1,t}}}{1+\widehat{\epsilon_{1,t}}}\right) \tag{9}$$

where $\widehat{\epsilon_{q,t}}$ is the estimated supply elasticity for productivity quartile q in the 3-year moving window centered at time t. At the same time, I compute the average log labor share of the high- and low-productivity firms over the same 3-year window:

$$\text{Log Lshare Spread}_t = (1/3)\sum_{\tau=t-1}^{t+1}\left(E[\log(lshare_{4,\tau})] - E[\log(lshare_{1,\tau})]\right) \tag{10}$$

Figure 5 plots the two standardized series. Due to the availability of LEHD wage data and the requirements of my regression specification, I compute supply elasticity estimates for the years 1992-2013. There is a clear tight link between the cross-sectional dispersion in labor shares and the wage markdowns for firms sorted on labor productivity, with the two series tracking each other closely over time. The correlation is about 0.73, with a Newey-West t-stat of 4.10. Since there are only 22 observations I compute the small-sample adjusted t-stat, and I choose a lag length of 5 years due to the persistence of the two series. The widening gap in labor shares and elasticity-implied markdowns in Figure 5 echoes the changing relative valuations and productivity levels of high- and low-productivity firms in Figure 1, suggesting the phenomena may be jointly linked. In accordance with this, the model I introduce and calibrate in Section 4 forges a direct link between productivity advantages, labor market power, and firm labor shares.

My results in this section imply that labor market power is likely to be a quantitatively important determinant of firm rents and cash flows, especially for the most productive firms. While my estimates are informative about the magnitudes of labor market power, they may not be sufficient by themselves to quantify the cashflows firms derive from wage markdowns. As discussed before, in the presence of labor adjustment costs the supply elasticity is not the only determinant of markdowns from marginal product. I formalize this concept in section 4, where I propose a model to map my empirical supply elasticity estimates into quantitative markdowns in the presence of labor market adjustment costs.

3.3.1 Robustness Checks for Labor Productivity Sorted Supply Elasticities

I now briefly discuss robustness checks for labor productivity-sorted supply elasticity estimates; for further details on all these estimates see E.4 in the appendix. In appendix Table A11 I sort firms into labor productivity quartiles within 2-digit NAICS industry, and find that every key finding from my baseline sorts in Table 4 is unchanged. In appendix Table A12 I perform several more robustness checks on elasticities sorted on productivity. I first estimate elasticities at the 3-year instead of 1-year horizon to test whether short term frictions drive the lower elasticity estimates for unproductive firms. Next I replace replacing average log wage changes with time-varying AKM firm wage effects to verify that sorting patterns are not likely to be driven by differences in incentive pay or match components of wages. Then I sort firms on TFP estimates from İmrohoroğlu and Tüzel (2014) instead of value added per worker. Finally, I replace Compustat employment changes with LBD employment changes, finding that Compustat-based elasticity estimates are more conservative. In all cases I find the same strongly monotonic decreasing pattern in supply elasticities as productivity increases.

4 Dynamic Wage Posting Model

In this section I introduce a dynamic wage posting model where a single firm hires from a fixed pool of potential employees. My primary focus is on quantifying how much adjustment costs affect markdowns from marginal product relative to the standard static elasticity-based markdown formula. The model is a dynamic extension on the static framework in Kline et al. (2019), and is also closely related to the setup of Card et al. (2018).

4.1 Setup

Consider the dynamic wage posting problem of a labor market with a single firm. There are a fixed total of L workers in the market, with a fraction λ_t currently employed at the firm at the start of period t. The firm can make different wage offers to outside hires ("O") and incumbents ("I"). The firm faces labor supply functions for these two classes of worker. For incumbents:

$$L_t^I = \lambda_t L \frac{(w_t^I - b)^\beta}{\kappa} \tag{11}$$

For outside hires:

$$L_t^O = (1 - \lambda_t) L \frac{(w_t^O - b)^\beta}{\kappa} \tag{12}$$

This labor supply functional form is borrowed from Card et al. (2018) and Kline et al. (2019). The $\frac{(w_t^k - b)^\beta}{\kappa}$ represents the probability that a worker of type $k = I, O$ accepts the offer to work at the firm. Card et al. (2018) derive the above labor supply function in a model of worker discrete choice with unobserved taste shocks between firms; like Kline et al. (2019) I take the functional form in equations (11) and (12) as given. Unlike Kline et al. (2019), I assume that firms choose the wage offer for both their incumbent workers and outside hires. Card et al. (2018) show that this supply functional form has some nice properties. In particular, it has a representation of the optimal wage that is isomorphic to a bargaining model, where $\beta/(1+\beta)$ acts like the workers' bargaining weight and b functions like the workers' outside option. In Card et al. (2018) the parameter β is inversely related with the variance in workers' unobservable preferences among firms; higher values of β lead to more elastic demand. The parameter κ is simply a scaling factor to ensure that the probability of a given worker choosing to be employed at that firm is bounded between 0 and 1. In order to focus on the firm's decision, I deliberately make the worker employment decision simple. An incumbent or potential recruit takes the wage offer at time t and decides whether or not to be employed at the firm for that period according to the choice probabilities implied in (11) and (12). Though β and b could in theory be worker-type specific, I restrict the them to be the same for incumbent workers and outside hires in order to limit the number of free parameters when I calibrate the model.

The supply elasticity facing the firm for workers of type $k = I, O$ is

$$\frac{\beta w_t^k}{w_t^k - b} \tag{13}$$

This is decreasing in the wage and is increasing in β and b.

Firms may pay outside hires differently because they are imperfect substitutes for incumbents. Specifically, hiring outside workers incurs quadratic adjustment costs:

$$c_t(L_t^O) = \exp(\sigma_c z_t)\bar{C}\lambda_t L \left(\frac{L_t^O}{\lambda_t L}\right)^2 \tag{14}$$

The adjustment costs may represent the cost of training workers in firm-specific production technologies or recruitment/hiring costs. I include the term $\exp(\sigma_c z_t)$ to allow adjustment costs to be increasing in firm productivity. Given the Belo, Li, Lin, and Zhao (2017) and Jager and Heining (2019) findings that adjustment costs are likely higher for skilled labor, this feature is a reduced form way of getting adjustment costs to increase in firm "skill" in a setting where there is only one type of labor. The $\lambda_t L$ term gives the number of incumbent workers employed at the firm at the start of time t, so adjustment costs are quadratic in the ratio of outside hires to start of period incumbents. This is similar to the formulation in Kline et al. (2019).

The firm produces output according to a Cobb-Douglas production function in the sum of inside and outside hires. Firm log productivity z_t follows an AR(1) process. There is no aggregate risk and firm cash flows are priced at the constant risk-free discount rate r. The firm solves

$$V_t = \max_{w_t^I, w_t^O} F_t(L_t^I, L_t^O) - \left(w_t^I L_t^I + w_t^O L_t^O + c_t(L_t^O)\right) + \exp(-r) E_t[V_{t+1}] \tag{15}$$

Subject to $L_t^O = (1-\lambda_t) L \frac{(w_t^O - b)^\beta}{\kappa}$ and $L_t^I = \lambda_t L \frac{(w_t^I - b)^\beta}{\kappa}$

$$\lambda_{t+1} = (L_t^O + L_t^I)/L \tag{16}$$

Where

$$F_t(L_t^I, L_t^O) = \exp(z_t) \left(\bar{z}(L_t^I + L_t^O)\right)^{(1-\alpha)} \tag{17}$$

$$z_{t+1} = \rho_z z_t + \sigma_z \epsilon_{z,t+1} \tag{18}$$

and $c_t(L_t^O)$ is the cost of hiring L_t^O workers. The law of motion for λ_t means that a time t retained incumbent or outside hire becomes an incumbent at the start of $t+1$.

4.2 Wage Markdowns with Adjustment Costs

The presence of adjustment costs changes the markdown formula in the standard monopsony framework. To see this, we take the first-order conditions of (15) for worker types $k = I, O$:

$$w_t^k = \frac{\varepsilon_k(w_t^k)}{\varepsilon_k(w_t^k) + 1} \left(MRP_t^k - \frac{\partial c_t}{\partial L_t^k} + \exp(-r) E_t \left[\frac{\partial V_{t+1}}{\partial L_{t+1}^I} \right] \right) \tag{19}$$

Here MRP_t^k is $\frac{\partial F_t}{\partial L_t^k}$, which is the same for incumbents and recruits given the production function $F_t(L_t^I, L_t^O) = \exp(z_t) \left(\bar{z}(L_t^I + L_t^O)\right)^{(1-\alpha)}$. There are two additional terms in (19) relative to the static markdown formula. The first is $\frac{\partial c_t}{\partial L_t^k}$, the derivative of the adjustment cost function with respect to an additional unit of labor of type k. This term is zero for incumbents and positive for outside hires. The presence of adjustment costs leads the firm to prefer retaining incumbent workers over replacing them with outside hires, generating a wage premium for incumbent workers over recruits.

The second additional term relative to a standard static wage markdown in the dynamic wage offer equation is $\exp(-r)E_t\left[\frac{\partial V_{t+1}}{\partial L^I_{t+1}}\right]$, which is the time t expected marginal benefit of having an additional incumbent worker at time $t+1$. Because both a retained incumbent or recruit hired today becomes an additional worker in the available pool of incumbents tomorrow, this term is the same for both types of workers. This term is also always positive because entering the next period with more incumbents is strictly better than having fewer incumbents. This implies that wage markdowns from marginal product will be strictly smaller for incumbent workers than would be inferred by supply elasticities alone, and may be larger or smaller for recruits depending on the relative size of $\frac{\partial c_t}{\partial L^O_t}$ and $\exp(-r)E_t\left[\frac{\partial V_{t+1}}{\partial L^I_{t+1}}\right]$. Note also that since incumbents and outside hires face different wage offers, the supply elasticities themselves also differ because the elasticity (13) is a decreasing function of the wage offer for each worker type.

Define for worker types $k = I, O$

$$\textit{dynamic markdown}^k_t \equiv \frac{w^k_t}{MRP^k_t - \frac{\partial c_t}{\partial L^k_t} + \exp(-r)E_t\left[\frac{\partial V_{t+1}}{\partial L^I_{t+1}}\right]} = \frac{\varepsilon_k(w^k_t)}{\varepsilon_k(w^k_t)+1} \tag{20}$$

$$\textit{MRP markdown}^k_t \equiv \frac{w^k_t}{MRP^k_t} \tag{21}$$

$$\textit{markdown wedge}^k_t \equiv \frac{\textit{MRP markdown}^k_t}{\textit{dynamic markdown}^k_t} \tag{22}$$

The markdown expressions (20), (21), and (22), are worker type specific, so I introduce their firm-level aggregates:

$$\textit{dynamic markdown}_t \equiv \frac{\sum_k w^k_t L^k_t}{\sum_k \left(MRP^k_t - \frac{\partial c_t}{\partial L^k_t} + \exp(-r)E_t\left[\frac{\partial V_{t+1}}{\partial L^I_{t+1}}\right]\right) L^k_t} \tag{23}$$

$$\textit{MRP markdown}_t \equiv \frac{\sum_k w^k_t L^k_t}{\sum_k MRP^k_t L^k_t} \tag{24}$$

$$\textit{markdown wedge}_t \equiv \nu_t \equiv \frac{\textit{MRP markdown}_t}{\textit{dynamic markdown}_t} \tag{25}$$

I examine the properties of these objects in the calibration. The markdown wedge is a particularly important quantity, as it allows me to map my empirically estimated supply elasticities to model implied markdowns.

4.3 Calibration and Model Fit

Closed form solutions for the firm's objective function (15) are not readily available, so I solve the model for a given calibration through value function iteration. I calibrate the model to match the average separations rate; the incumbent wage premium; overall supply elasticity, incumbent/recruit specific supply elasticities; the relative stock-return wage passthroughs of incumbents and recruits; and the average log labor share.[20] I estimate model-implied supply elasticities by running a regression of model-generated employment and wage growth on stock returns (growth in the value function). I use the Rouwenhourst (1995) method to create a discretized grid of 9 points to approximate the AR(1) productivity process. For pre-calibrated parameters, I normalize the labor force size L to equal 1; set the annual discount rate to .02; set the supply scale factor κ to .741[21]; I set $\rho_z = 0.9$ to match the persistence of annual log value-added per worker in the data; and finally, I set both the baseline labor productivity $\bar{z}$ and convex adjustment cost volatility σ_c equal to 1.

Table 5 shows the model calibration of the externally and internally calibrated parameters, and Panel A of Table 6 displays the model fit to targeted moments. There are 5 internally calibrated parameters for 7 targeted moments. The fit quantitative fit is very good, with almost exact matches on average incumbent wage premium, separations rates, relative wage passthroughs, and average log labor shares; the model calibration does slightly underestimate average labor supply elasticities, but is still pretty close quantitatively. I calibrate the parameters $\beta = 0.38$, $b = 0.545$, and $\sigma_z = 0.145$ to help match the average supply elasticities and the incumbent/recruit stock return wage passthrough ratio; the parameter $\bar{C} = 0.55$ is identified primarily by the incumbent-recruit wage premium. The parameter $\alpha = 0.22$, which is one minus labor returns to scale, and productivity volatility $\sigma_z =$ jointly help me get close to the the average empirical supply elasticity and log labor share. Finally, the average wage levels implied by the combination of parameters help to jointly match the empirical average firm level separations rate.

The calibrated parameters are also in a reasonable range relative to analogous parameters in prior research. For comparison I look at the calibrations of the dynamic models in two

[20]The incumbent wage premium is skill-adjusted and represents the intercept from a regression of the wage premium on the incumbent minus recruit difference in average AKM worker effects. I obtain incumbent and recruit elasticities by estimating (6) just for incumbents and recruits, so the growth rates in the left hand side are the growth in wages for incumbents/recruits or growth in the number of incumbents/recruits. The independent firm-specific control variables $X_{j,t}$ are also their analogues just for incumbents and recruits, with the exception of lagged asset growth which is not worker type specific.

[21]The parameter κ is technically a function of a pre-calibrated parameter and an internally calibrated parameter. I obtain κ by setting $\kappa = (w_{max} - b)^{\beta}$, where w_{max} is pre-set to 1. in equilibrium this ensures that the probability of being employed at the firm is always bounded between 0 and 1 for both worker types and all productivity levels.

other papers, Kuehn et al. (2017) and Belo et al. (2017), which both have model features that are related to mine. Kuehn et al. (2017) calibrate the benefit of unemployment to be 0.71; my analogous parameter is the outside option of b, calibrated at 0.545. Kuehn et al. (2017) also have a worker bargain weight of 0.11. The most comparable parameter in my calibration is $\beta/(1+\beta) = 0.27$, which is a little higher. However, the parameter β also affects the supply elasticity, and so $\beta/(1+\beta)$ doesn't have the exact same structural interpretation as it would in a pure bargaining model because it also directly affects the endogenous labor supply response to wage changes. Belo et al. (2014) calibrate the high-skill convex adjustment cost parameter to 1.8 and the low-skill convex adjustment cost parameter to 0.17; my calibration of $\bar{C} = 0.55$ lies in between the two. Finally, my calibration of returns to scale of $1 - \alpha = 0.78$ lies in between those of Kuehn et al. (2017) and Belo et al. (2017) (0.75 and 0.85, respectively). Again note that these parameters don't exactly map between one another in the papers, although they are related in important ways; I merely present these parameter estimates to compare to some reasonably close analogues in recent prior literature.

A supply elasticity that decreases in the wage is a key feature of the model that helps match the data moments. Consistent with the data, recruits face higher average supply elasticities. In the model this is because their wage offers are closer to the reservation wage level b, and elasticities can be arbitrarily high as wage offers approach b. This feature of the labor supply function also implies that firms face lower elasticities in more productive states. In Panel B Table 6 I show that the model is able to match the monotonic pattern in supply elasticities and log labor shares when sorting by labor productivity without explicitly targeting these moments. Interestingly, although the model matches the monotonic pattern in firm labor shares it misses on the magnitude of the spread. The log labor share spread is 0.37 in the model but 0.72 in the data. In the next section I show that this is also the case for my empirical supply elasticity estimates–although they imply a spread in labor shares, the spread is wider in the data than implied by my empirical estimates. This motivates a decomposition of the labor share spread in Section 5, where I examine how much of a role there is left over for differences in markdowns or labor returns to scale after accounting for the implied spread in markdowns.

I now analyze the behavior of markdowns in the model. In the top panel of Figure 6 I compare average markdowns implied by estimating supply elasticities in a regression of employment and wage growth on stock returns versus the actual average markdown from the true supply elasticities determined by (13). Because of adjustment costs, the regression implied elasticities slightly underestimate the wages paid to incumbent and overestimate the wages paid to recruits. However, when aggregating to the firm level the linear regression elasticity-implied markdown closely reflect the average dynamic markdowns for the whole firm. In the second panel of Figure 6 I compare the dynamic markdowns the marginal revenue product markdowns. This comparison shows the difference between the markdowns that would be implied by supply elasticities alone versus the actual markdown from marginal product. At the firm level, the average dynamic markdown is 0.68, which implies that firm pays about 68% of the denominator in (23) to its workers in wages on average. However, this again masks heterogeneity by worker type. Because incumbents are costly to replace, their wages are only marked down by 83% from marginal product; without accounting for dynamics due to adjustment costs, supply elasticities alone would imply a wage of 64% of marginal product. The reverse is true for recruits: supply elasticities would imply a wage that is about 80% of marginal product, but recruits' wages are actually about 71% of marginal product.

A key object for empirical quantification is the average firm-level markdown wedge implied by the model. This is about $0.79/0.68 \approx 1.16$, so wage markdowns at the firm level are about 16% smaller in magnitude after accounting for adjustment costs than would be inferred by the simple static markdown formula $\epsilon/(1+\epsilon)$. I label the markdown wedge ν. In Table 7 I show the markdowns from marginal revenue product that result from my supply elasticity estimates in section 3 combined with the markdown wedge ν.[22] Even after adjusting for dynamics due to adjustment costs, the markdowns still imply a considerable amount of monopsony power. However, adjustment costs do negate labor market power quite a bit. The baseline elasticity estimate of 2.52 yields a markdown that is 72% of marginal product; after adjusting for dynamics this becomes 83% of marginal product. A supply elasticity of about 4.9 would give this markdown in a static setting. Table 7 combines the model markdown wedge with my empirical elasticity estimates. The table also shows that while low productivity firms do mark wages down, they still pay 94% of marginal product. This is reasonably close to a competitive benchmark. On the other hand, the most productive firms pay 62% of marginal product even after adjusting using the markdown wedge ν.

4.3.1 Calibration for 1991-2002 and 2003-2014 Subperiods

Given the downward trends in supply elasticities and estimated markdown spreads that I find in Figures 4 and 5, I introduce calibrations to match moments for the first and second halves of the sample (1991-2002 and 2003-2014, respectively). The calibrated parameters by subperiod are in appendix Table A5. The average cross-sectional standard deviation of

[22] For simplicity I report results assuming the markdown wedge is constant across productivity levels. Results are very similar quantitatively if I allow the markdown wedge to vary by productivity.

log value added per worker increases by about 5% over the two subperiods, so I decrease σ, the productivity dispersion parameter, by about 2.5% relative to the main calibration for the 1991-2002 period and increase it by 2.5% for the 2003-2014 calibration. I calibrate the remaining parameters to match the same moments as in my main calibration.

In A6 I give the corresponding model and data moments for the subperiods. Supply elasticities and separations rates decline empiricially across the two periods; I decrease in the parameter β, which governs the average supply elasticity, and b, which governs the worker outside options to match these moments. My calibration of β decreases from 0.5 to 0.34 from the first half to the second half of the sample, and b decreases from 0.549 to 0.515. This agrees in principle with Stansbury and Summers (2020), who suggest that worker power in the workplace has declined over recent decades.

The incumbent log wage premium increased by about 30% (from 0.13 to 0.17) across the two periods. This suggests increased adjustment costs, and I increase $\bar{C}$, the convex adjustment cost parameter, from 0.38 to 0.745 to match these moments. The resulting model-implied markdown wedge ν increases from 1.11 to 1.24 between the first and second halves of the sample period, mitigating some, but not all, of the impact of decreasing supply elasticities. I use these subperiod-specific elasticity estimates and markdown wedges when I quantify the model in the next section.

5 Quantifying Empirical Estimates

In this section I use the model-implied markdown adjustment parameter ν in conjunction with my empirical elasticity estimates from Section 3 to quantify the value that firms derive from their wage markdowns. I also use the model and estimated elasticities to back out the share of variation in labor shares across time and between firms of different productivity types that can be ascribed to differences in markdowns.

5.1 Value of Cashflows Derived from Labor Market Power

With the markdown wedges ν from the model and the empirical elasticity estimates for firms sorted on labor productivity, I now examine the counterfactual cash flows firms would bring in if they were not able to mark down wages from marginal product, but still held their production decisions constant. This is different from the competitive counterfactual, because in the competitive equilibrium both quantities and prices of labor would adjust. Instead, this counterfactual transfers the capital income attributable to wage markdowns from the actual owners of the firm's capital to the firm's workers after production has occurred. I use two measures of cash flows. My primary measure is based off the firm's operating income. I use the Compustat variable OIBDP adjusted for changes inventories, which was also used as the basis for my computation of firm value-added in (A.1). Hartman-Glaser et al. (2019) use OIBDP as a proxy for capital income available to the owners of debt and equity issued by the firm. The ratio of the value of wage markdowns to operating income can then be thought of as representing the share of capital income coming from wage markdowns. Going forward I use operating income and capital income interchangeably.

Denote $\widehat{\epsilon_{q,t}}$ the elasticity estimate for productivity quartile q at time t, where I use the elasticities estimates for either the 1991-2002 or 2003-2014 subperiods depending on which period year t falls in. Let $q(j,t)$ give the productivity quartile of firm j at time t. Finally, let ν_t denote the markdown wedge estimated over the subperiod where year t falls. Counterfactual labor expenses without markdowns are

$$\widetilde{LABEX}_{j,t} = \nu_t^{-1} \frac{\widehat{\epsilon_{q(j,t),\, t}} + 1}{\widehat{\epsilon_{q(j,t),\, t}}} LABEX_{j,t} \tag{26}$$

And the resulting markdown share of operating income:

$$\text{Markdown Share}^{OI}_{j,t} = \frac{\widetilde{LABEX}_{j,t} - LABEX_{j,t}}{OI_{j,t}} \tag{27}$$

I then compute the average markdown share of operating income:

$$\text{Average Markdown Share of OI} = \frac{1}{N} \sum_t \sum_j \text{Markdown Share}^{OI}_{j,t} \tag{28}$$

Here N denotes the total number of firm-year observations in the sample. Some firms report negative operating income, and occasional extreme values of Markdown Share$^{OI}_{j,t}$ skew the firm-level averages, so in (28) I focus on the subset of firms for which $OI_{j,t} > \widetilde{LABEX}_{j,t} - LABEX_{j,t}$, which are likely to have lower measurement error skewing the average. Instead of relying on a subsample of firms, I also compute the median fraction of operating income derived from wage markdowns among a all firms.

Finally, I look at the aggregate share of operating income that comes from wage markdowns by summing true and counterfactual income across the whole population of firms:

$$\text{Aggregate Markdown Share of OI} = \frac{1}{T} \sum_t \left[\frac{\sum_j \widetilde{LABEX}_{j,t} - LABEX_{j,t}}{\sum_j OI_{j,t}} \right] \tag{29}$$

with T the number of years in the sample. I compute (28) and (29) for all firms and separately for firms in the top and bottom quartiles of labor productivity.

I report these averages in Table 8 overall and for firms in the top and bottom quartiles of labor productivity. In Panel A I focus on results for the full sample period. The first two rows of Panel A show the firm-level mean and median shares of operating income derived from paying wages that are different from marginal product. The average firm in the sample earns about 34% of its operating income by paying wages lower than marginal product; this figure is 30% for the median firm. There is large heterogeneity between the most- and least productive firms, with those in the bottom quartile of productivity earning only 17% of their income from wage markdowns, while firms in the top quartile earn about 43%. Since the operating income represents earnings available to the firm's capital owners, these estimates suggest that about a third of the capital income generated by the typical firm comes from wage markdowns.

In the third row of Panel A in Table 8 I compute the aggregate operating income share of wage markdowns, as in equation (29). Because markdowns are concentrated amongst larger firms, the aggregate average is higher than the firm-level average, at about 40%. Hence two-fifths of the capital income generated by publicly traded firms were attributable to wage markdowns over the 1991-2014 period. Assuming these cash flows would be discounted at the same rate as capital income overall, this has the interpretation that the dollar value of wage markdowns at the monopsony equilibrium is worth roughly 40% of the total enterprise value of publicly traded firms. However, note that this is a very different statement than saying that the aggregate value of publicly traded firms would be 40% lower without wage markdowns, because the exercise here holds equilibrium labor demand constant. The decline in total enterprise value in the counterfactual competitive equilibrium would be substantially smaller than 40%. I discuss this point further later on in this section.

Panel B of Table 8 breaks down these figures by the 1991-2002 and 2003-2014 subperiods. At the firm level, the average markdown share of operating income increases slightly from 0.34 to 0.35 between the 1991-2002 and 2003-2014. The median increases slightly more, from 0.29 to 0.32; finally, the aggregate operating income share also increases from 0.39 to 0.41. These slight overall increases mask heterogeneity by productivity type: the mean, median, and aggregate average share of operating income generated from wage markdowns actually dropped for the least productive firms, while all three rose for the most productive firms. The largest increase was for the median high productivity firm, which saw their wage markdowns as a share of operating income increase from 0.45 to 0.52 between the two periods. This divergence parallels the increase in productivity dispersion between top and bottom firms over the sample period, as shown for example in Figure 5.

5.1.1 Interpreting the Magnitude of Cash Flows Generated from Wage Markdowns

Given the size of wage markdowns as a fraction of operating income in Table 8, it is helpful to provide some context for interpretation and to compare the magnitude against other results.

First of all, note that these markdown shares of capital income do not represent the change in the value of the firm relative to the competitive equilibrium. Constructing a reasonable competitive counterfactual would require imposing far more structure on my model, including assumptions about labor supply across firms and product market competition. Rather, I am asking the question "What is the total value of the gap between wages and marginal products relative to total capital income, *holding equilibrium quantities fixed*?" In a competitive equilibrium quantities as well as prices would adjust. Hence my counterfactual does not imply, for example, that the aggregate enterprise value of publicly traded firms would be 40% lower if we imposed perfect competition. To illustrate, in appendix Section D, I make a back of the envelope calculation in a simple static representative firm setting where wage markdowns are calibrated to be exactly 40% of operating income, and the production function is calibrated to roughly match the average aggregate labor share in my sample. In this setup, firm value declines by 16% in the counterfactual competitive equilibrium relative to the monopsony equilibrium.

How does the magnitude of my estimates compare with other estimates of market power? In the next subsection I introduce two measures of aggregate labor shares; depending on the measure the mean aggregate labor share is about 49-55% of value-added, which implies that wage markdowns average about 18-20% of total output. In comparison, Crouzet and Eberly (2021) point out that the De Loecker et al. (2020) price markup estimates imply rents from product market power were worth almost 40% of value added by the end of their sample period, so my estimates are about half as large as this figure. The De Loecker et al. (2020) estimates are also closely related to the ratio of sales to cost of goods sold, and they assume there is no monopsony power. Imperfect competition in labor markets can also affect the ratio of sales to costs of goods sold, so their estimates likely reflect both labor and product market power. Herschbein, Macaluso, and Yeh (2020) jointly estimate wage markdowns and price markups for manufacturing firms using a production function approach that is related to the De Loecker et al. (2020) method. They find that the average firm pays about 65% of marginal product. Since they estimate that markdowns increase in firm size, the value-weighted gap would be even wider. In comparison, I find that total aggregate wages are about 70% of aggregate marginal product, which is smaller but of a comparable magnitude.

5.2 Decomposition of Labor Share Differences in the Cross-Section and Time Series

Next, I look at what fraction of the gap in labor shares between high- and low-productivity firms can be explained by differences in wage markdowns; I also examine how changes in the aggregate labor share in the time series were impacted by changing markdowns. In this section I use two measures of labor shares. My baseline, defined previously in (A.4), comes from imputed labor expenses $LABEX_{j,t}$ derived from LEHD earnings and Compustat employment data. I find that $LABEX_{j,t}$ is on average a little lower than Compustat reported staff and labor expense (Compustat variable XLR), but the variable XLR is only available for a very small fraction of firms. To get around this I create an imputed version of XLR, which uses the fact that $LABEX_{j,t}$ and $XLR_{j,t}$ are very highly correlated when $XLR_{j,t}$ is observable. Details on the imputation are in appendix section A.3. I label this imputed version $\widehat{XLR}_{j,t}$, and create my alternative labor share measure by simply replacing $LABEX_{j,t}$ with $\widehat{XLR}_{j,t}$ in equations (A.1) and (A.4). I call this measure LShare $(\widehat{XLR})_t$. The logs of the two firm-level labor share measures share a high correlation of 0.91, though the time series behavior of the aggregate labor share from summing the two versions of labor expenses is a bit different; I elaborate on this point when I do the decomposition of aggregate labor share changes in the time series.

Table 6 shows that the model generates a monotonically decreasing pattern in labor shares as productivity improves, as is also found in the data. Still, the labor share spread in the model is not as wide as the empirical spread. This is also true when using the empirically estimated markdowns in Table 7, and leaves room for other factors, such as differences in product market power or production technologies, to explain the remainder of the labor share spread. For example, assume a Cobb-Douglas production function in labor, $F(L) = AL^{1-\alpha}$, but also suppose the firm chooses output according to inverse demand $P(Q) = P_0 Q^{-1/\gamma}$, where γ is the price elasticity of demand. Also assume that because of labor market power wages are a markdown μ from the marginal revenue product of labor. Then the revenue function is $R(L) = P_0 A^{1-1/\gamma} L^{(1-1/\gamma)(1-\alpha)}$, where $1 - 1/\gamma$ gives the inverse price markup. The log labor share satisfies

$$\begin{aligned}\log(\text{Labor Share}) &= \log\left(\frac{\mu R'(L)L}{R(L)}\right) = \log(\mu) + \log(1-1/\gamma) + \log(1-\alpha) \qquad (30)\\ &= \log(\text{wage markdown}) - \log(\text{price markup}) + \log(\text{labor returns to scale})\end{aligned}$$

The difference in average log labor shares can then be ascribed to differences in average log wage markdowns, price markups, and returns to scale. What percentage of the average labor share differences between high- and low productivity firms can be attributed to differential wage markdowns? In Panel A of 9 I answer this question using the wage markdown estimates from Table 6 and my two measures of firm labor shares. I show estimated log markdowns and average log labor shares. The difference in empirical log markdowns from Table 7 is $\log(0.62) - \log(0.94) \approx -.41$, which I report in the bottom row of the third column of Panel A. The differences in the average log labor shares between high and low productivity firms if -0.72 for my baseline and -0.65 for the $\widehat{XLR}$ based measure; markdown differences can account for roughly three-fifths of the difference in average log labor shares (57% for my baseline labor share measure and 63.5% for the alternate measure).

My findings still leave an important quantitative role for product market power to play in fully explaining the cross-sectional gap in labor shares. If labor returns to scale are approximately the same between the two groups of firms then the remaining two-fifths could be explained by differences in price markups. The increasing product market power among dominant firms found by Autor et al. (2020) and De Loecker et al. (2020) and the widening markdowns that I find are likely to be jointly important phenomena quantitatively. Also note that my model implicitly forges a link between product and labor market power, because increases in product market power increase labor productivity, which in turn increases labor market power, both in my model and in the data.

I next compute the aggregate labor share using either $\widehat{XLR}$ or $LABEX$. The aggregate labor share is given by

$$\text{Agg LShare } (LABEX)_t = \frac{\sum_j LABEX_{j,t}}{\sum_j VA_{j,t}} \tag{31}$$

for $LABEX$, and similarly for $\widehat{XLR}$:

$$\text{Agg LShare } (\widehat{XLR})_t = \frac{\sum_j \widehat{XLR}_{j,t}}{\sum_j VA_{j,t}} \tag{32}$$

Here $VA_{j,t}$ is the value-added for firm j at time t, computed by adding either $LABEX_{j,t}$ or $\widehat{XLR}_{j,t}$ to operating income adjusted for changes in inventory as in (A.1). Although the firm-level variation in log labor shares for the two measures is very highly correlated, Figure 7 shows that the downward trend in labor shares is more pronounced for Agg LShare ($\widehat{XLR}$). The average aggregate labor share drops from 0.579 to 0.524, when comparing the 1991-2002 and 2003-2014 subperiods; meanwhile Agg LShare ($LABEX$) shows a more modest decline of 0.506 to 0.485.

Since the trend in Agg LShare ($\widehat{XLR}$) is much closer to the Compustat labor share trends found by Hartman-Glaser et al. (2019), in the next exercise I focus primarily on changes in Agg LShare($\widehat{XLR}$), but show results for both versions. I compute the counterfactual labor share if markdowns remained at their 1991-2002 levels in the years 2003-2014. In particular, let $\eta_q^{1991-2002}$ denote the estimated wage markdown for productivity quartile q for the 1991-2002 period, and $\eta_q^{2003-2014}$ the estimated wage markdown for 2003-2014. The counterfactual labor share is given by

$$\widetilde{\text{Agg LShare}}\,(\widehat{XLR}) = \frac{\sum_j \widehat{XLR}_{j,t} \times \eta_{q(j,t)}^{1991-2002} \times \left(\eta_{q(j,t)}^{2003-2014}\right)^{-1}}{\sum_j VA_{j,t}} \tag{33}$$

It's important to be clear about what this counterfactual represents and what it does not. Similar to my prior exercise in section 5.1, this counterfactual asks the question: "Holding the distribution of workers across firms constant, how would the observed aggregate labor share change in the 2003-2014 period if enough final output was reallocated to workers to keep the gap between wages and marginal products constant at 1991-2002 levels?"

I compute $\widetilde{\text{Agg LShare}}\,(LABEX)_t$ analogously. I then compute the counterfactual log change in the average labor share across the two periods. Panel B of Table 9 shows the results. In the first two columns I show the actual average labor shares for the two measures, and in the third column (labeled $\widetilde{2003-2014}$) I give the counterfactual aggregate labor share. The first row shows that if markdowns had held constant at their 1991-2002 levels in the latter period, the average labor share Agg LShare $(\widehat{XLR})_t$ would have only declined from 0.579 to 0.554 instead of the observed level of 0.524. The actual log change, in the column Δ log(Lshare), was -0.10, whereas the counterfactual log change ($\widetilde{\Delta\log(\text{Lshare})}$) would have been -.044. This implies that about 56% percent of the decline ($\frac{0.10-0.044}{0.10} \approx 0.558$) can be explained by the change in wage markdowns. In the second row of panel B I compute the same counterfactual for $\widetilde{\text{Agg LShare}}\,(LABEX)_t$. Because the labor share decline from this measure is less stark to begin with, the counterfactual actual predicts an *increase* in the labor share of about 1.8% instead of the observed 4.2% decrease.

In either case, I estimate that the change in wage markdowns can explain the bulk of the decline in average labor shares over the two periods. The results using my preferred measure $\widetilde{\text{Agg LShare}}\,(\widehat{XLR})_t$ suggest that there is still some room for other proposed factors, such as increased price markups or labor-substituting technological change, to explain the remaining 44%.

6 Discussion

In this section I discuss potential causes of the rise in labor market power, economic and policy implications of my findings, and how my results can be distinguished with alternative explanations.

6.1 What Has Caused the Rise In Labor Market Power?

I uncover two key features of the rise in labor market power. I document the rising spread in labor productivity in Figure 1, and similar patterns have been documented in numerous other papers.[23] The rise in productivity dispersion has led highly productive firms to mark wages down by more relative to low productivity firms. At the same time, in appendix Table A6 I find decreases in the supply elasticities for firms of all productivity levels between the 1991-2002 and 2003-2014 periods. This also has the tendency to push wages down.

The rise in productivity dispersion could be driven by a number of phenomena, such as changing economies of scale arising from the shift towards intangible capital and information technology, increased global competition which causes firms to exit, larger barriers to entry, or shifts in consumer taste towards specific brands or products. Whatever the causes, the dispersion in productivity allows productive firms to operate on a more inelastic portion of their labor supply curve, generating monopsony rents in equilibrium.

The overall decrease in supply elasticities may be caused by very different economic forces than the cross-sectional spread in elasticities. On a fundamental level, the supply elasticity a firm faces captures the willingness or ability of workers to substitute away from that firm and towards other firms. This suggests that workers have found it more difficult to substitute away from particular employers. Consistent with this Molloy, Smith, Trezzi, and Wozniak (2016) find that the US labor market has become less dynamic over the past several decades, with transitions between firms to and from employment being less frequent. Similarly, in appendix Table A6 I show that the average firm-level separations rate has declined. My calibration for the 1991-2002 and 2003-2014 periods suggests reduced outside options (model parameter b) and bargaining power (parameter β). Song, Price, Guvenen, Bloom, and von Wachter (2018) show that the 1990-2015 period has seen increased sorting of productive workers to high wage firms; I find a similar result in appendix Figure A2, which shows that the spread in average worker skill between productive and unproductive firms has widened, especially in the latter half of my sample period. Skill-biased technological change has induced an increasing share

[23] See Autor et al. (2020), Hartman-Glaser et al. (2019), De Loecker et al. (2020), Kehrig and Vincent (2021), and Gouin-Bonenfant (2020) for example.

of individuals to delay entry into the workforce in favor of obtaining higher education college, potentially resulting in more specialized human capital in the process. Appendix Table A6 shows that the wage premium for incumbent workers has increased, suggesting that firms have also found it more difficult to substitute workers from outside the firm.

All these patterns are consistent with specific human capital rising in importance. There are two potential opposing effects from increased human capital specificity: 1) employers find it harder to replace skilled workers, which tends to raise the wages of incumbent workers; and 2) workers are less able (or less willing) to substitute their labor across particular employers, which leads to a larger gap between wages and marginal products. My model calibration and empirical estimates suggest that the second effect has dominated.

Berger et al. (2021), Rinz (2018) and Lipsius (2018) all find that local labor market concentration has been decreasing, which initially seems at odds with increasing aggregate labor market power. The above discussion could also help explain why monopsony power has increased even as local labor market concentration has decreased. Figure 4 and Table 4 show the importance of skilled workers in driving firm-level supply elasticities. The overall downward trend in supply elasticities is driven in large part by skilled workers, and the productive firms with the most labor market power also hire more skilled workers. This in turn means that local labor market concentration may be less important for determining monopsony power for skilled workers, because for them labor markets are far less local. For example, although skilled workers have lower rates of separation, Malamud and Wozniak (2012) and Amior (2020) document that conditional on moving, more educated workers make job moves that are far more geographically distant on average, and Diamond (2016) shows that educated workers tend to live in larger metropolitan areas which naturally have less local concentration. Supporting a human capital specificity explanation, Nimczik (2020) finds that more skilled workers tend to be employed in labor markets that are geographically dispersed but more concentrated in a set of particular industries.

The overall rise of non-compete agreements, which now affect a large portion of the labor force and are focused on skilled workers (Starr, Prescott, and Bishara, 2021), could also be playing a role in reducing supply elasticities. These appear to be successful in reducing both worker mobility and wages, especially among skilled workers.[24]

[24]See Garmaise (2011), Balasubramanian, Chang, Sakakibara, Sivadasan, and Starr (2020), Jeffers (2019), Johnson, Lavetti, and Lipsitz (2020), for example.

6.2 Economic and Policy Implications of Labor Market Power

My findings suggest an important subtlety to consider for policy interventions intended to curtail labor market power: in the cross-section, firms who exercise more labor market power tend to pay higher wages, not lower wages, and they are likely to be firms where many workers would want to be employed. It is precisely these firms' success in terms of productivity—an amalgam of high product demand, innovative success, productive efficiency, etc.—which grants them their monopsony rents. While I find that firms which have become productive earn substantial rents from labor market power ex post, many of these firms have likely made risky investments ex ante which allowed them to become productive. The prospect of earning rents may compensate firms for undertaking risky R&D or other types of investments earlier on in their life cycle. These investments are also beneficial for workers because they generate growth and can improve wages in absolute terms, even if the gap between the marginal product of labor and wages widens. Accordingly, it is important to consider whether policy interventions geared towards reducing labor market power may deter firms from making these types of investments, or more generally act as a deterrent to potential market entrants.

Although I do not explicitly model product market power in this paper, my results also imply that labor and product market power are likely to comove. Because firms with high product market power charge high prices, part of what I measure as labor productivity (value added per worker) could in practice reflect firms' product market power. This implies that policy interventions, such as antitrust enforcement, that are meant to curb product market power could also be effective in reducing labor market power at the same time. However, it's not necessarily the case that this is guaranteed to make workers better off. For example, it's possible that breaking up productive firms could reduce productivity or devalue firm-specific human capital, leading to lower wages for workers.

Perhaps of most concern from the perspective of worker welfare is the aggregate decline in the overall supply elasticity across productivity and skill levels, which reflects workers' diminished ability to substitute employment across firms. As I argue regarding human capital specificity in the previous subsection, some of the forces that have led to this change may also have made it more costly for firms to replace existing workers. These labor adjustment costs create a deadweight loss. Consequently, policies that are geared towards reducing the costliness of hiring new workers while helping workers substitute their labor across potential employers may be mutually beneficial for both workers and firms. This could include the subsidization of new skill acquisition or easing regulatory burdens which make hiring employees costly.

Working to reduce artificial barriers to worker mobility stands to be directly beneficial to workers, and targets the types of forces generating labor market power that are more clearly negative from a welfare perspective. This could include reducing the scope of or banning entirely the use of non-compete agreements. Pursuing antitrust action to prevent employers from colluding to reduce worker mobility could also be helpful to workers. There is some precedent for such legal action already, as the US Department of Justice Antitrust Division filed a suit against a number of prominent Silicon Valley tech firms in 2010 for colluding from 2005 to 2009 to not poach employees from one another and a settlement forcing the firms to end this practice was quickly reached.[25]

Another interesting economic implication of my findings relates to the rise in wage inequality and the polarization of the US labor market driven by technological change (Acemoglu and Autor, 2011; Autor and Dorn, 2013). Since I find the lowest and most decreasing supply elasticities for skilled workers, who also have high earnings, this suggests that skilled workers' wages are marked down relative to a very high and increasing marginal product of labor relative to low skill workers. Thus my estimates imply an even larger role for technological change in generating wage inequality. However, it is also necessary to note that in the model I estimate dynamic markdown wedges for a single labor input, and not separately for different skill levels. If I added workers of different skill levels to the model the dynamic markdown wedge would be larger for the most skilled workers, who are most costly to replace, meaning that wage markdown differences between high- and low-skill workers are not as large as the simple supply elasticity difference would imply. Still, the elasticity difference is wide enough that it is highly unlikely this would reverse the of the skilled workers' lower supply elasticities entirely.

6.3 Alternative Explanations for My Findings

Some previous papers find similar patterns in firm-level labor shares but argue for different economic mechanisms. Here I discuss two specific examples. Hartman-Glaser et al. (2019) also focus on Compustat firms, and they similarly show that the share of output accruing to capital owners has increased by the most for highly productive firms. They also show that firm level idiosyncratic risk rose substantially over the 1960-2014 period and propose a model based on optimal contracting to explain the capital share dynamics. In their model skilled workers demand wage contracts with embedded insurance for bearing firm-specific risk, and productive firms are able to allocate more output to capital. Increasing idiosyncratic risk amplifies this mechanism, leading to a drop in the aggregate labor share driven by productive

[25]See court documents for the case "U.S. V. Adobe Systems, Inc., Et Al." found at https://www.justice.gov/atr/case/us-v-adobe-systems-inc-et-al.

firms in the right tail of the productivity distribution.

They calibrate the model to match changes in the labor share between the 1960-1970 and 1990-2014 periods. In appendix Figure A3 I plot time trends in their idiosyncratic risk measures before and during my sample period. Although firm-specific risk has indeed risen over that time horizon, idiosyncratic risk has actually been flat or slightly declining over the 1991-2014 period that my data covers. Therefore while their mechanism does speak to broad patterns in labor shares over a longer time horizon, it is incomplete as an explaination for the change in the aggregate labor share *within* the 1991-2014 period that is the focus of this paper.

Kehrig and Vincent (2021) document that large and productive manufacturing firms with low labor shares have driven the aggregate decline in the manufacturing sector labor share. They argue that this is more consistent with demand side forces than with wage markdowns because in their framework monopsony should primarily depress labor shares by reducing wages. However, I find that monopsony power is larger for productive firms precisely because they can pay high wages, which allows them to operate on a relatively inelastic portion of their labor supply curve. From the perspective of my findings, the demand side forces they find exaggerate productivity advantages and increase the spread in labor market power. Thus the results in this paper and those in Kehrig and Vincent (2021) are compatible explanations for the aggregate labor share decline.

Finally, I stress that my findings still leave plenty of room for other forces, such as increasing price markups or technological change, to explain cross-sectional differences and time series changes in labor shares, and these could also speak to the evidence on firm profitability, valuations, investment, and economic rents that I presented in section 2. I do argue, however, that labor market power has played a more important role in these phenomena than prior literature may suggest.

6.3.1 Monopsony Through Upward Sloping Labor Supply Curves or Wage Bargaining?

Models of rent-sharing via wage bargaining can also explain passthrough of firm-specific productivity shocks to wages, even without upward sloping firm-specific labor supply curves. A few patterns suggest monopsony-based explanations over a bargaining. For example, in most models of monopsony a shock to the wages of a competitor acts like a firm-specific labor supply shock to the firm. The result is that the firm must raise wages in order to retain workers, meaning competitor wage growth should be expected to raise wages without raising employment at the firm. This is exactly what I find in appendix Table A7. In the fourth column of this table I find that competitor wage growth is indeed strongly associated with own firm wage growth, but has no relation to employment growth. In Table 3 I find that skilled recruits also face lower supply elasticities compared to lower skilled recruits, suggesting that the low supply elasticity caused by the wage response is related to monopsony power rather than, say, rent sharing due to bargaining with skilled insiders who have a better bargaining position. Finally, my simple dynamic wage posting monopsony model with labor adjustment adjustment costs immediately can explain empirical patterns in recruit and incumbent wage passthroughs and supply elasticities; sorting patterns in supply elasticities across firms of different productivity types; and cross-sectional differences in firm level labor shares and profitability. Purely bargain based explanations would have a difficult time matching all the above patterns simultaneously.

7 Conclusion

In this paper I find evidence for a "superstar firms" view of labor market power: firms with productivity advantages face much lower supply elasticities and hence earn higher monopsony rents from wage markdowns. Differences in supply elasticities have widened over time, leading to an increased gap in labor shares between productive and unproductive firms in the cross section and contributing to the decline in the aggregate labor share. The value of wage markdowns is substantial. The average firm earns about a third of its capital income from wage markdowns, and wage markdowns are worth about 40% of capital income in aggregate. Consistent with economic rents earned from labor market power, productive firms have higher valuation ratios but not significantly higher investment or hiring rates.

Although my findings suggest that labor market power generates substantial value to productive firms in the form of economic rents, my results suggest some caution in interpreting welfare or policy implications. In my model and in the data, productive firms actually pay higher wages despite having larger markdowns, since they operate on a steeper portion of the labor supply curve. Because of the opportunities they afford their employees–especially in the form of higher wages—highly productive firms are likely to be coveted employers. Any policy recommendations to curb labor market power ought to consider that it is exerted by the most economically successful firms who actually tend to pay above average wages to their employees.

Skilled workers play an important role in creating these patterns, as they face the lowest supply elasticities of any skill group. There is some evidence in increased role for firm-specific human capital could be driving these patterns, but artificial barriers to mobility—such as the rising prevalence of non-compete agreements—may also play a role. Exploring these possibilities in more depth could be a particularly useful avenue for further research.

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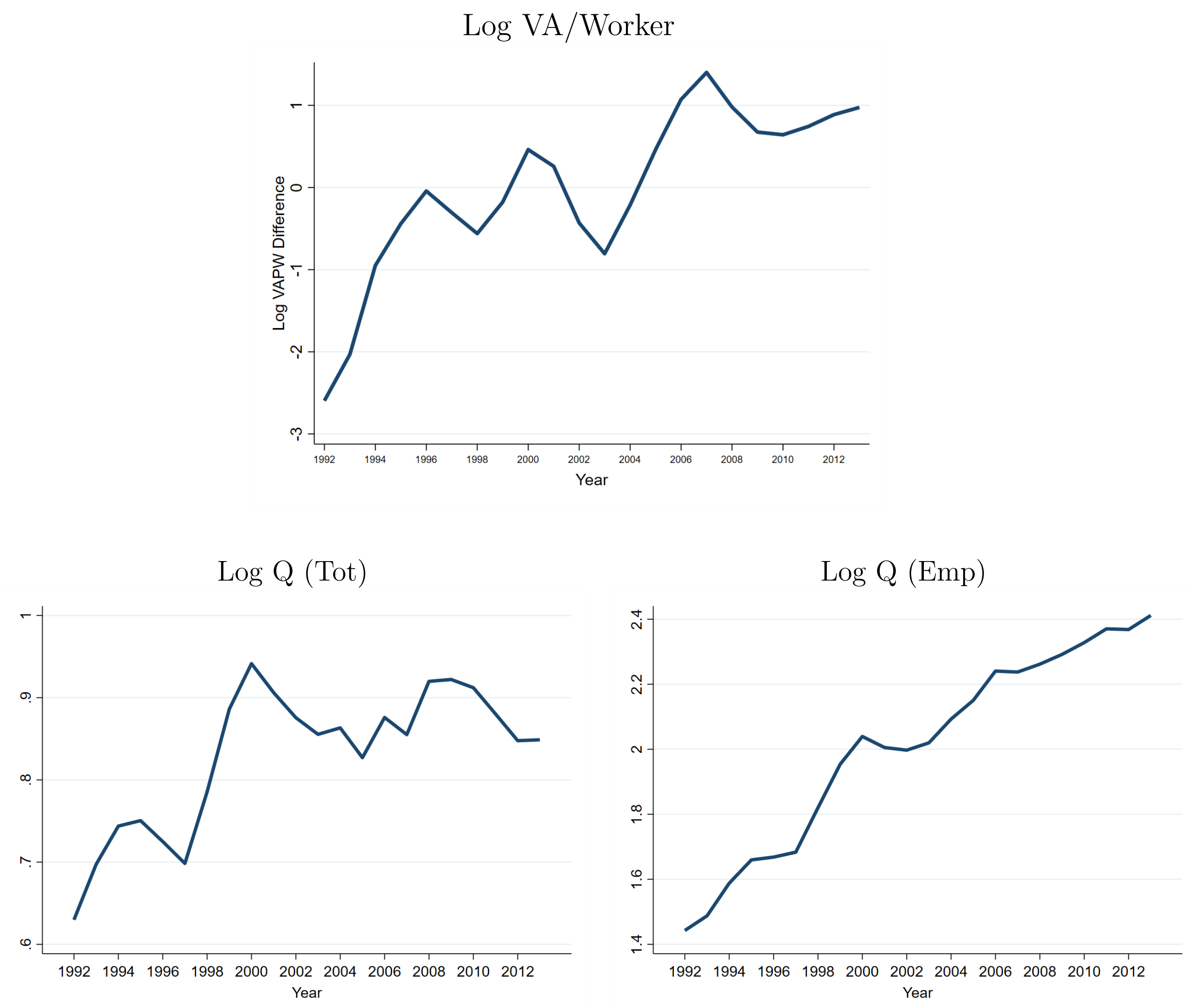
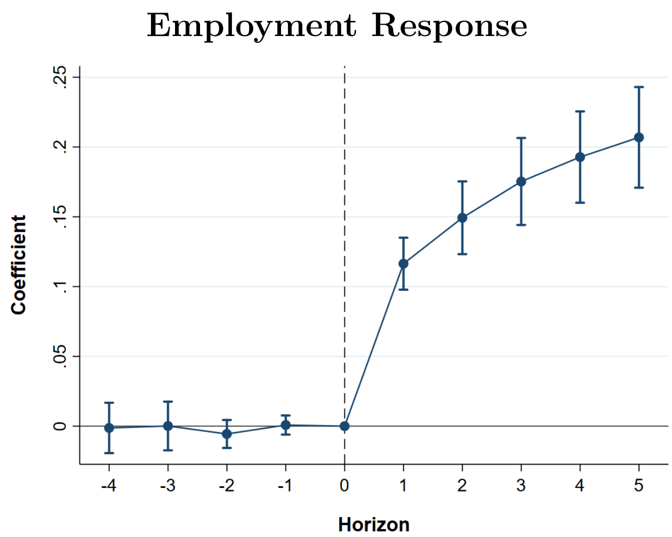
Figures

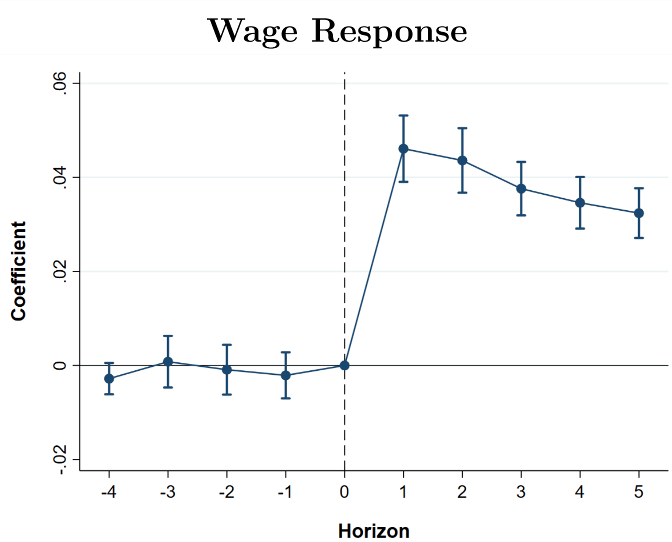
Figure 1: High- minus Low-Labor Productivity Firm Spreads in Average Log VA/Worker, AKM Worker Skill, Log Q (Total), and Log Q (Emp) Trend Upward Over Time



Note: This figure shows the differences between firms in the top- and bottom-quartiles of productivity for the average log value-added per worker; firm worker skill level from (A.8) in the text; log total Tobin's Q ratio from Peters and Taylor (2017); and log employment-based Q ratio from (A.9) . Sample period spans 1991-2014.

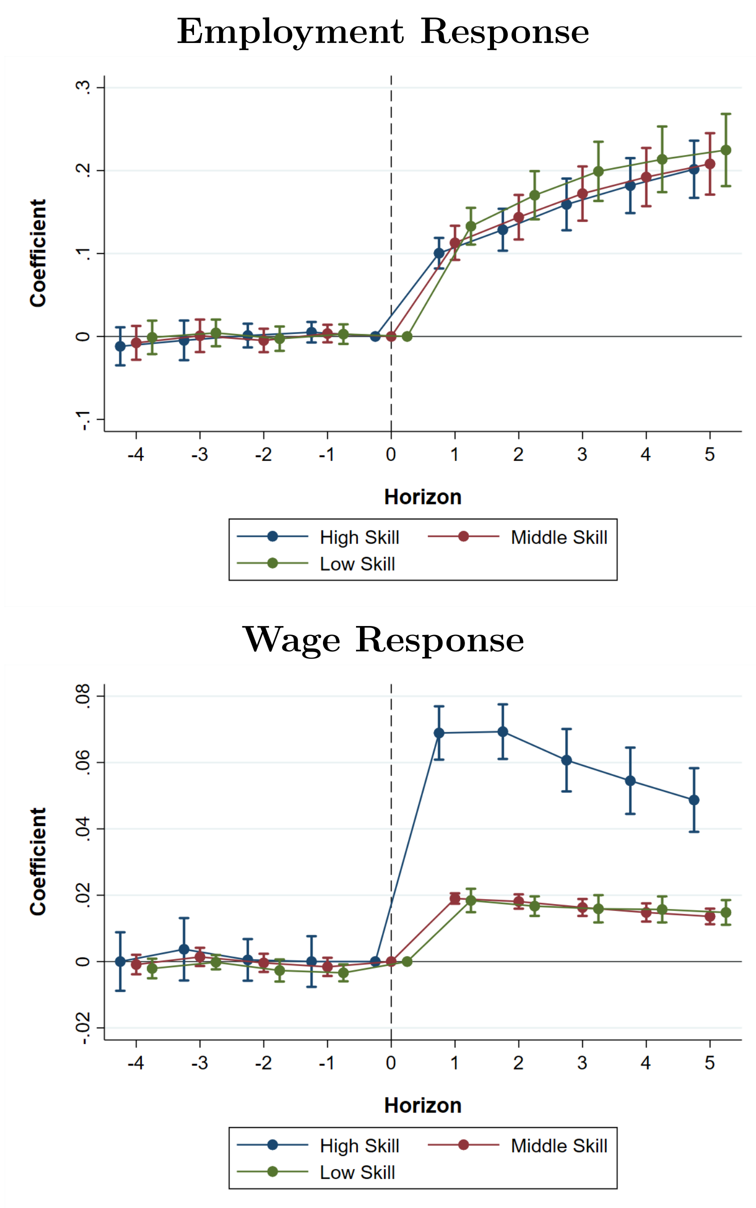
Figure 2: Employment and Wage Responses to a Stock Return Shock





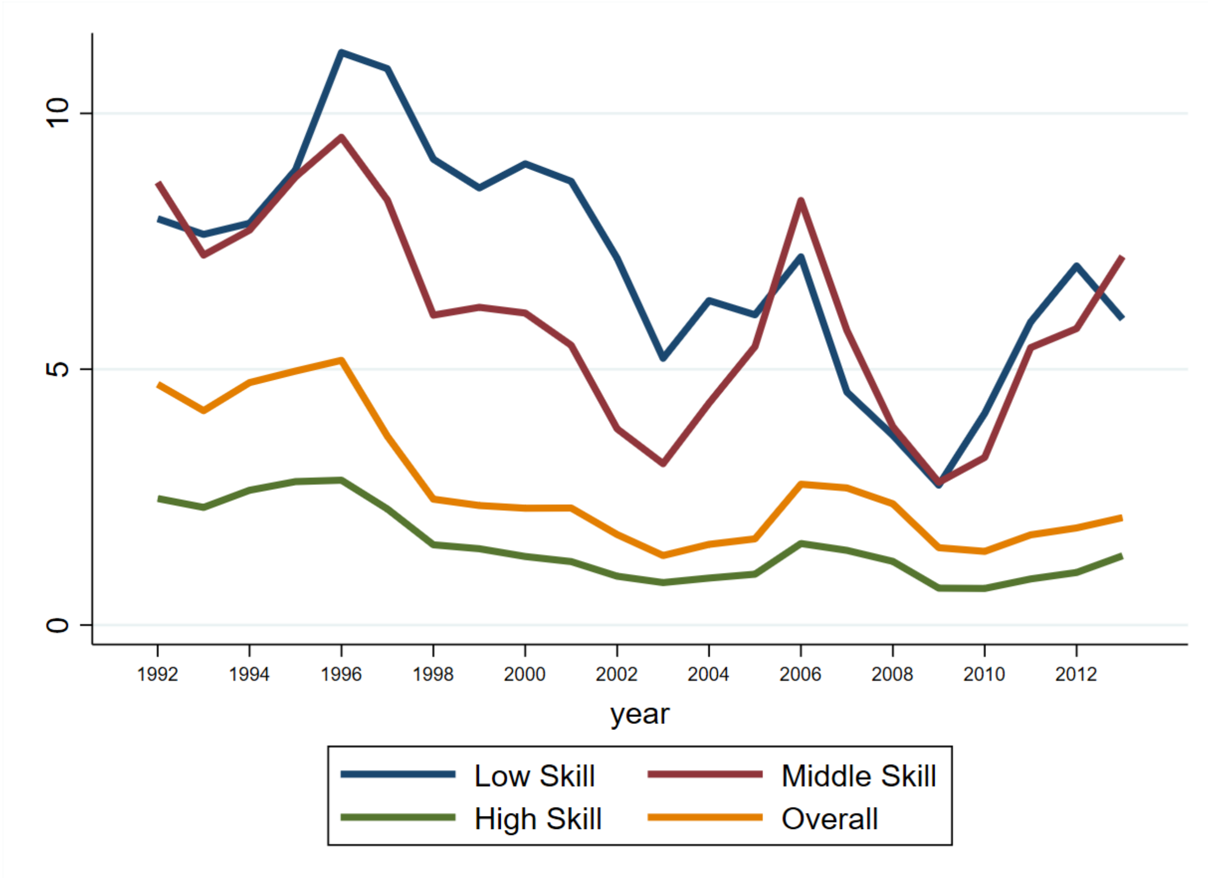
Note: This figures shows the employment and wage responses from estimating (7) in the main text for $h = -5$ to 5 years. Confidence intervals are based off standard errors double clustered by industry and year.

Figure 3: Employment and Wage Responses to Stock Return Shock, by Worker Skill



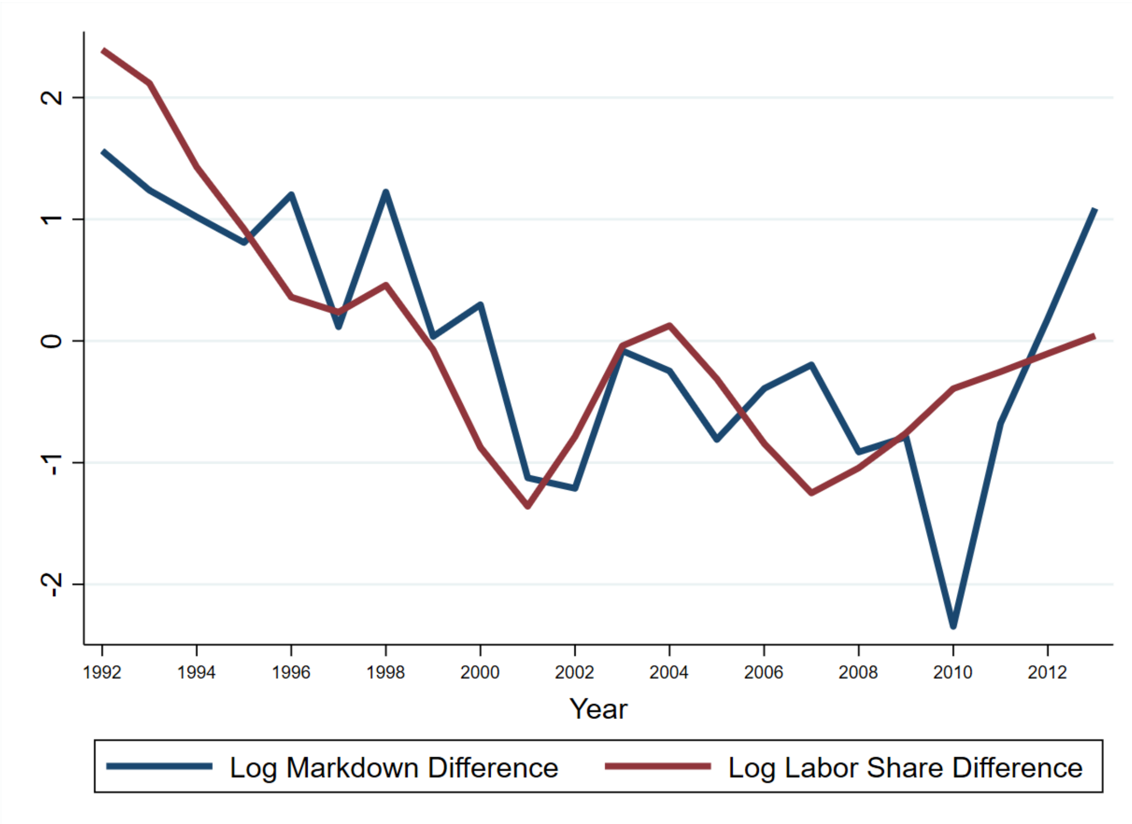
Note: This figures shows the employment and wage responses from estimating (7) in the main text for $h = -5$ to 5 years for workers of different skill levels. Individuals in the bottom two quintiles of the cross-sectional distribution of worker effects are considered low-skilled, the third and fourth quintiles middle-skilled, and the top quintile high-skilled. Confidence intervals are based off standard errors double clustered by industry and year.

Figure 4: Estimated Supply Elasticities Trend Downward Over Time For All Skill Groups



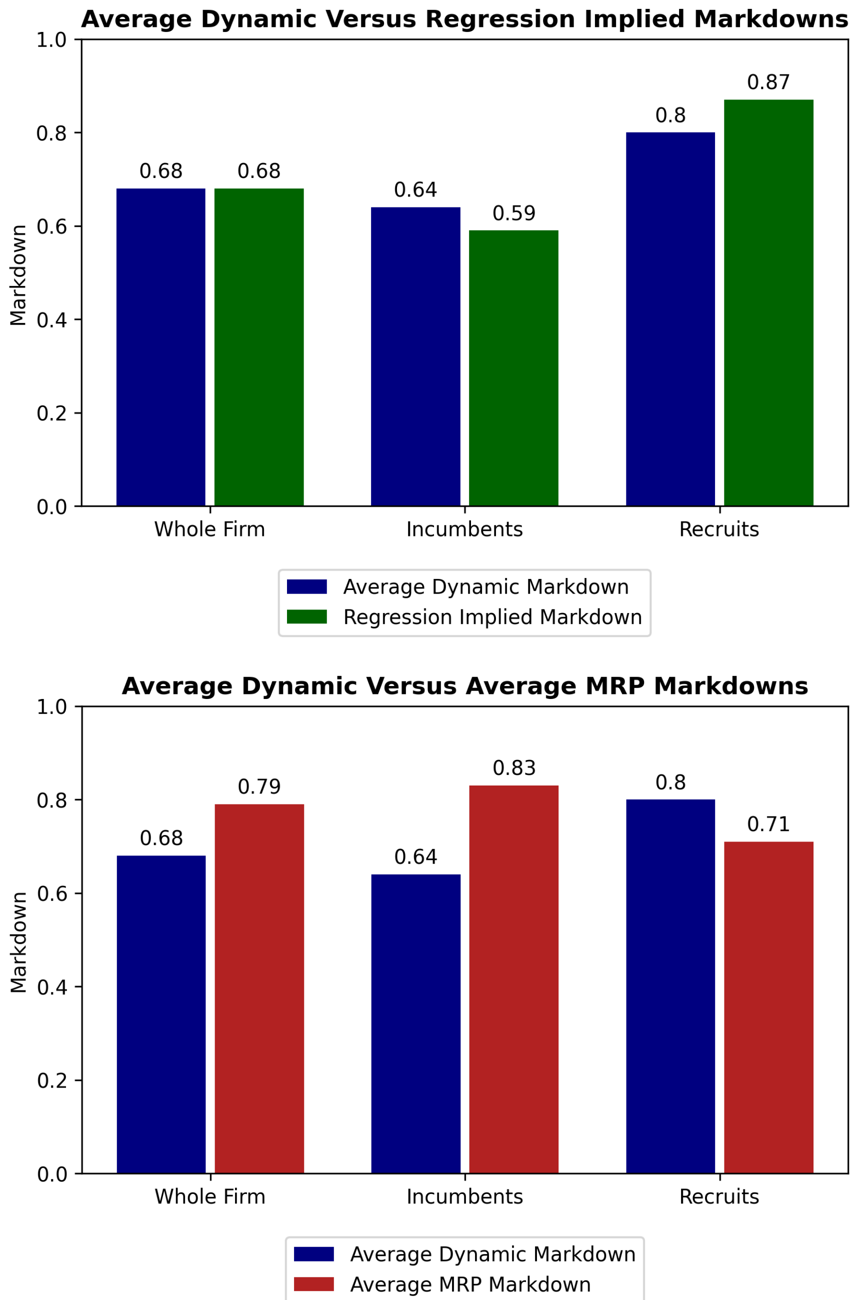
Note: This figure shows estimates of supply elasticities implied by estimating employment and wage responses from (6) for workers of different skill levels and for overlapping moving 3-year windows. The elasticity for a given year is for the moving window centered at that year (except for the start and end years, which are respectively the first or last year of 2-year window). The sample spans 1992-2013. High skill workers are in the top quintile of individual fixed effects from a wage of individual earnings into worker- and firm-specific components; low skill workers are in the bottom two quintiles and middle skill the third and fourth quintiles.

Figure 5: The Spread in Labor Shares and Estimated Markdowns Between High- and Low-Labor Productivity Firms Trends Downward Over Time



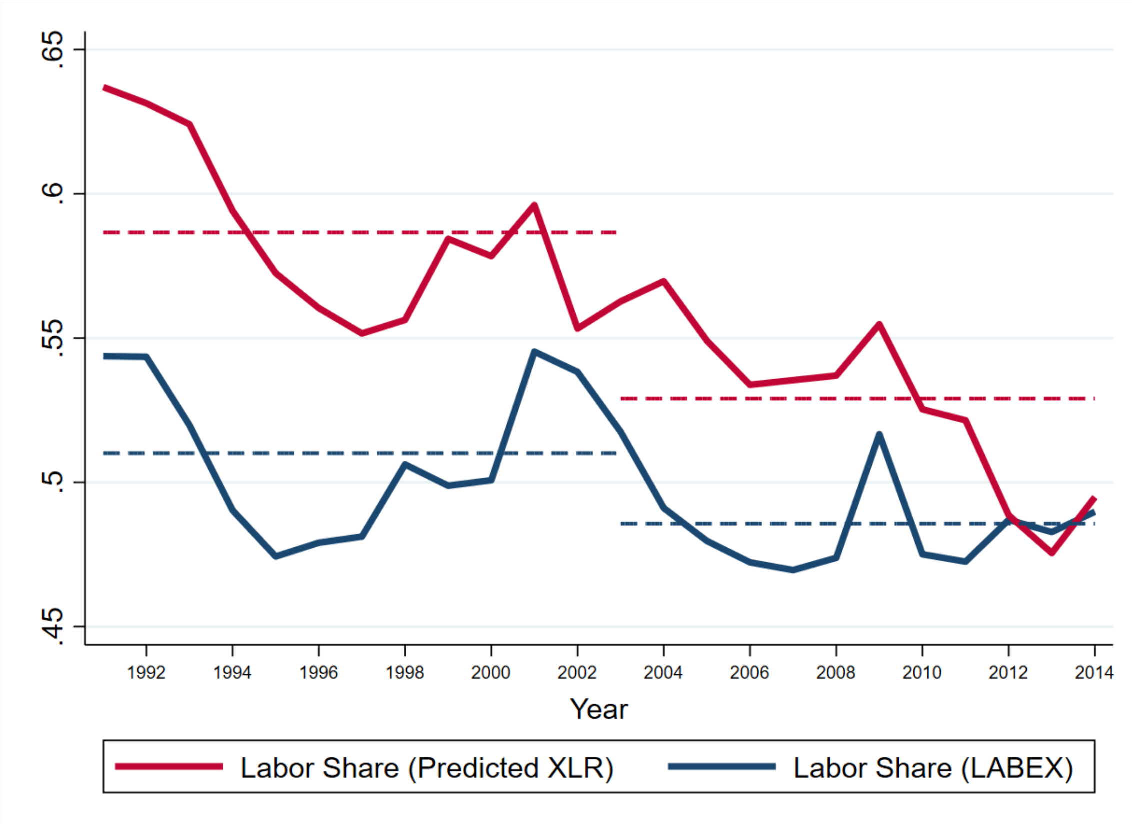
Note: This figure shows differences elasticity implied log markdowns between top- and bottom-quartile labor productivity firms from (9) in the main text, and log labor share differences following (10). The elasticity for a given year is for the moving window centered at that year (except for the start and end years, which are respectively the first or last year of 2-year window). Both series are standardized to unit standard deviation and zero mean. The sample period spans 1992-2013. The two series have a correlation of 0.73.

Figure 6: Model Implied Markdowns



Note: This figure shows markdowns implied by the calibration of the model. The dynamic markdown is the markdown adjusted for model dynamics due to adjustment costs, and is given by (20) in the main text. The regression implied markdown is markdown implied by estimating the supply elasticity by running the regression of model employment/wage growth on stock returns and taking the ratio of the two coefficients. The MRP markdown is the markdown from marginal revenue product. See section 4 in text for details.

Figure 7: Aggregate Labor Shares Over Time



Note: This figure shows the aggregate labor shares for firms in my Compustat-LEHD matched sample. The dashed lines give the mean aggregate labor share for the given measure over the 1991-2002 and 2003-2014 subperiods. I report aggregate labor shares using two measures of firm labor expenses. Labor Share (LABEX) uses my LEHD-based measure of firm labor expenses, while Labor Share (Predicted XLR) is constructed by imputing Compustat staff and labor expenses. I describe my imputation of XLR in section A.3 of the appendix. Labor shares are computed as the ratio of total labor expenses to total value-added, where I define valued-added following Donangelo et al. (2019). See section 5.2 in main text for further details.

Tables

Table 1: Productive Firms Have Lower Labor Shares, Higher Valuations, and Better Operating Performance

Panel A: Log Labor Share				
log VA/Worker	-0.343	-0.389	-0.514	-0.555
	(0.050)	(0.050)	(0.025)	(0.036)
Size Controls		X		X
Industry X Year FE			X	X
N	57500	57500	57500	57500
R^2 (within)	0.297	0.335	0.458	0.476
Panel B: Log Valuation Ratios				
	log Q (Tot)	log Q (Emp)	log Mkcap/Book	log Mkcap/Sales
log VA/Worker	0.524	0.540	0.254	0.412
	(0.048)	(0.060)	(0.029)	(0.033)
Size Controls	X	X	X	X
Industry X Year FE	X	X	X	X
N	48500	47500	55000	57000
R^2 (within)	0.125	0.276	0.069	0.315
Panel C: Operating Performance				
	ROA_t	ROA_{t+1}	ROE_t	ROE_{t+1}
log VA/Worker	0.107	0.098	0.277	0.2456
	(0.006)	(0.006)	(0.016)	0.0221
Size Controls	X	X	X	X
Industry X Year FE	X	X	X	X
N	57500	53500	55500	51500
R^2 (within)	0.168	0.128	0.085	.0665

Note: This table displays coefficients on log value added per worker for predicting firm log labor shares in panel A; firm log valuation ratios in panel B; and current and future firm operating performance in panel C. Controls for size include log employment, assets, and sales. Log Q (Tot) is the log of intangible-adjusted total Q from Peters and Taylor (2017). Log Q (Emp) is an employment-based analogue of Tobin's Q, where the denominator is the skill-weighted workforce of the firm. ROA is the return on assets (income before extraordinary items plus deprecation over total assets) and ROE is return on equity (income before extraordinary items plus deprecation over book equity). Operating performance and valuation ratios are winsorized at the 1% level by year. See section 2 in main text for more details. Standard errors double clustered by year and industry in parentheses.

Table 2: Average Investment Rates/Tobin's Q and Investment-Q Relation for Firms Sorted on Labor Productivity

Panel A: Investment Rates and Tobin's Q

Productivity:	Quartile 1	Quartile 2	Quartile 3	Quartile 4	P-val 4-1
Inv Rate (Total)	0.18	0.16	0.16	0.20	0.33
Q (Total)	0.73	0.79	1.04	1.70	0.00
Hiring Rate	0.40	0.33	0.29	0.29	0.00
Q (Emp)	0.78	0.97	1.67	4.83	0.00

Panel B: Investment-Q Relation

Productivity:	Quartile 1	Quartile 2	Quartile 3	Quartile 4	P-val 4-1	R-sq (Within)	N
Dep Variable: Investment Rate (Total)							
Q (Tot)	0.039	0.030	0.026	0.022			
	(0.004)	(0.003)	(0.002)	(0.001)	0.000	0.287	48000
Cash Flow	0.177	0.182	0.204	0.251			
	(0.028)	(0.020)	(0.021)	(0.027)	0.000		
Dep Variable: Hiring Rate							
Q (Emp)	0.067	0.077	0.048	0.032			
	(0.009)	(0.011)	(0.007)	(0.004)	0.000	0.073	43500
Cash Flow	0.260	0.225	0.120	-0.026			
	(0.053)	(0.048)	(0.024)	(0.035)	0.000		

Note: Panel A of this table shows average investment rates in capital and in new hires for firms of different productivity quartiles. Panel B includes results from estimating (1) in the main text. Q (Total) is the intangible-adjusted total Q from Peters and Taylor (2017) and the total investment rate is the dollar investment in total (physical plus intangible) capital divided by lagged replacement value of total capital. Q (Emp) is an employment-based analogue of Tobin's Q, where the denominator is the skill-weighted workforce of the firm. "P-val 4-1" gives the p-value from a test that the coefficients for firms in the top and bottom quartiles have equal values. All specifications include firm, year, and productivity quartile fixed effects. The sample period spans 1991-2014. Standard errors double clustered by industry and year in parentheses.

Table 3: Baseline Elasticity Estimates, By Worker Skill and Incumbent or Recruit Status

	All	Low Skill	Middle Skill	High Skill
Panel A: Whole Firm				
Employment	0.116	0.128	0.110	0.102
	(0.01)	(0.011)	(0.011)	(0.01)
Wages	0.046	0.016	0.019	0.084
	(0.004)	(0.002)	(0.001)	(0.005)
Implied Elasticity	2.53	7.97	5.92	1.22
Panel B: Incumbents				
Employment	0.091	0.082	0.083	0.099
	(0.009)	(0.009)	(0.01)	(0.01)
Wages	0.053	0.018	0.020	0.082
	(0.004)	(0.002)	(0.001)	(0.004)
Implied Elasticity	1.73	4.55	4.10	1.20
Panel C: Recruits				
Employment	0.203	0.220	0.200	0.152
	(0.016)	(0.017)	(0.016)	(0.015)
Wages	0.028	0.022	0.018	0.038
	(0.002)	(0.002)	(0.002)	(0.003)
Implied Elasticity	7.19	9.98	11.32	3.99

Note: This table shows estimates of the supply elasticities implied by the employment and wage responses to stock returns from estimating (6) in the text. Controls include 3-digit NAICS industry by year and productivity quartile fixed effects; lagged growth rates in wages, employment, and total assets; and the contemporaneous change in average worker skill level at the firm (see (A.8) for definition). Workers are grouped into skill groups based on their estimated worker effects from a modified Abowd et al. (1999) style wage decomposition with time-varying firm fixed effects. Individuals in the bottom two quintiles of the cross-sectional distribution of worker effects are considered low-skilled, the third and fourth quintiles middle-skilled, and the top quintile high-skilled. Changes in average worker skill are computed within the population of workers considered in the specification. Panel A estimates (6) for the whole firm and for workers of different skill levels. Panels B and C restrict (6) to incumbents (workers who were employed at the firm the previous year) and recruits (workers who joined the firm in the given year). Standard errors clustered by industry and year are in parentheses.

Table 4: Productive Firms Face Lower Supply Elasticities For Workers of All Skill Levels

Productivity:	Quartile 1	Quartile 2	Quartile 3	Quartile 4	P-val 4-1	R-sq	N
			Whole Firm				
Employment	0.119	0.089	0.120	0.106			
	(0.013)	(0.010)	(0.011)	(0.011)	0.249	0.127	43500
Wages	0.028	0.033	0.051	0.091			
	(0.003)	(0.003)	(0.004)	(0.010)	0.000	0.423	43500
Elasticity	4.324	2.708	2.336	1.166			
			Low Skill Workers				
Employment	0.128	0.102	0.143	0.139			
	(0.015)	(0.010)	(0.013)	(0.015)	0.522	0.110	43500
Wages	0.012	0.016	0.021	0.030			
	(0.002)	(0.003)	(0.003)	(0.003)	0.000	0.289	43500
Elasticity	10.510	6.369	6.768	4.663			
			Middle Skill Workers				
Employment	0.116	0.084	0.118	0.103			
	(0.016)	(0.010)	(0.010)	(0.013)	0.307	0.085	43500
Wages	0.014	0.016	0.022	0.029			
	(0.002)	(0.002)	(0.002)	(0.003)	0.000	0.182	43500
Elasticity	8.199	5.298	5.305	3.546			
			High Skill Workers				
Employment	0.102	0.078	0.106	0.087			
	(0.014)	(0.010)	(0.009)	(0.010)	0.148	0.076	43500
Wages	0.050	0.056	0.076	0.119			
	(0.006)	(0.004)	(0.006)	(0.010)	0.000	0.454	43500
Elasticity	2.042	1.388	1.389	0.735			

Note: This table contains supply elasticity estimates for firms sorted on log value-added/worker quartiles as in (8) in the text. Controls include 3-digit NAICS industry by year and productivity quartile fixed effects; lagged growth rates in wages, employment, and total assets; and the contemporaneous change in average worker skill level at the firm (see (A.8) for definition). Workers are grouped into skill groups based on their estimated worker effects from a modified Abowd et al. (1999) style wage decomposition with time-varying firm fixed effects. Individuals in the bottom two quintiles of the cross-sectional distribution of worker effects are considered low-skilled, the third and fourth quintiles middle-skilled, and the top quintile high-skilled. Changes in average worker skill are computed within the population of workers considered in the specification. "P-val 4-1" gives the p-value from a test that the coefficients for firms in the top and bottom quartiles have equal values. Wage data are from the LEHD, and the sample period spans 1991-2014. Standard errors double clustered by industry and year in parentheses. See section 3 in main text for more details.

Table 5: Model Calibration

Parameter	Explanation	Value
Externally Calibrated		
L	Labor Force Size	1.0
r	Discount rate	0.02
κ	Supply scale factor	0.741
ρ_z	Productivity persistence	0.9
$\bar{z}$	Fixed Labor Productivity	1.0
σ_c	Convex adjustment cost volatility	1.0
Internally Calibrated		
α	(One minus) Labor returns to scale	0.22
β	Supply Elasticity Shifter	0.38
b	Reservation Wage	0.545
σ_z	Labor productivity volatility	0.145
$\bar{C}$	Convex adjustment cost level	0.57

Note: This table shows the parameter calibration for the model in section 4 of the text. Externally calibrated parameters are calibrated beforehand without trying to match target model moments, while internally calibrated parameters are explicitly set in order to match target moments.

Table 6: Model vs Data Moments

Panel A: Targeted Moments

Moment Name	Model	Data
Separations Rate	0.31	0.30
Incumbent Premium	0.15	0.15
Supply Elasticity	2.09	2.52
Elasticity (Inc.)	1.44	1.73
Elasticity (Rec.)	6.66	7.19
Inc/Rec Wage Pass. Ratio	1.93	1.86
Log Labor Share	-0.50	-0.51

Panel B: Sorting on Productivity

Moment Name	Model	Data
Elasticity (Q1)	4.85	4.32
Elasticity (Q2)	2.53	2.71
Elasticity (Q3)	1.80	2.34
Elasticity (Q4)	1.36	1.17
Log Labor Share (Q1)	-0.31	-0.22
Log Labor Share (Q2)	-0.46	-0.40
Log Labor Share (Q3)	-0.56	-0.54
Log Labor Share (Q4)	-0.68	-0.94

Note: This table compares model implied moments with their empirical counterparts. “Inc” and “Rec” denote incumbents and recruits, respectively. In order to mimic the empirical estimates, the model supply elasticities are computed by running a regression of model-generated stock returns on employment and wage growth and taking the ratio of the responses. See section 4 in text for details.

Table 7: Adjusted Markdowns for Firms Sorted on Labor Productivity Implied By Empirical Elasticity Estimates and Markdown Wedge ν From Model

	Elasticity	Unadjusted Markdown	Adjusted Markdown
Overall	2.52	0.72	0.83
Quartile 1	4.32	0.81	0.94
Quartile 2	2.71	0.73	0.85
Quartile 3	2.34	0.70	0.81
Quartile 4	1.17	0.54	0.62

Note: This table compares markdowns implied by my empirical supply elasticity estimates before adjusting for the model markdown wedge ν estimated from the model and after. For supply elasticity ϵ, the unadjusted markdown is $\epsilon/(1+\epsilon)$; the adjusted markdown is $\nu \times \epsilon/(1+\epsilon)$.

Table 8: Valuing Labor Market Power—Wage Markdowns as a Fraction of Operating Income

Panel A: Full Sample (1991-2014)

	Overall	Low Productivity	High Productivity
Mean Firm-Level Operating Income Share	0.34	0.17	0.43
Median Firm-Level Operating Income Share	0.30	0.08	0.49
Mean Aggregate Operating Income Share	0.40	0.10	0.45

Panel B: By Subperiod (1991-2002 and 2003-2014)

	Overall	Low Productivity	High Productivity
1991-2002			
Mean Firm-Level Operating Income Share	0.34	0.19	0.42
Median Firm-Level Operating Income Share	0.29	0.10	0.45
Mean Aggregate Operating Income Share	0.39	0.12	0.43
2003-2014			
Mean Firm-Level Operating Income Share	0.35	0.14	0.44
Median Firm-Level Operating Income Share	0.32	0.07	0.52
Mean Aggregate Operating Income Share	0.41	0.07	0.46

Note: This table shows the dollar value of wage markdowns as a fraction of operating income; these are broken down by the full sample period in panel A and by subperiod in panel B. See section 5.1 of the main text for more details.

Table 9: Differences in Labor Shares Explained by Markdowns, Cross-Section and Time Series

Panel A: High Minus Low Productivity Cross-Sectional Average Labor Share Spread

	Low Productivity	High Productivity	High - Low	% Due to ΔMarkdown
Log Markdown	-0.05	-0.46	-0.41	
Avg Log LShare ($\widehat{XLR}$)	-0.18	-0.83	-0.65	63.5
Avg Log LShare ($LABEX$)	-0.22	-0.94	-0.72	57.1

Panel B: Time Series Change in Aggregate Labor Share

	$1991-2002$	$2003-2014$	$\widetilde{2003-2014}$	$\Delta\log(\text{Lshare})$	$\Delta\,\widetilde{\log(\text{Lshare})}$	% Due to ΔMarkdown
Agg LShare ($\widehat{XLR}$)	0.579	0.524	0.554	-0.10	-0.044	55.8
Agg LShare ($LABEX$)	0.506	0.485	0.515	-0.042	0.018	143.2

Note: Panel A of this table decomposes the fraction cross-sectional average labor share differences between high- and low-productivity firms that can be attributed to wage markdowns. In panel A I use markdown estimates for the full 1991-2014 period reported in Table 8. Panel B of this table decomposes the time series change in the aggregate labor share between the 1991-2002 and 2003-2014 period using markdowns estimated separately for the two subperiods. The column $\widetilde{2003-2014}$ denotes the counterfactual average labor share for the 2003-2014 period if markdowns were held constant at their 1991-2002 estimates, as defined in (33) in the main text. The column $\Delta\log(\text{Lshare})$ gives the actual log change in the average labor share between the two periods, while $\Delta\,\widetilde{\log(\text{Lshare})}$ gives the counterfactual log labor share change holding markdowns constant. The last column of panel B gives the fraction of the observed change in the labor share that is attributable to wage markdown changes. See section 5.2 in main text for further details.

Appendices

A Data Appendix

A.1 Constructing Labor Productivity, Wages, Labor Shares, and Other Firm-Level Variables

Firm Value-Added and Labor Shares

I follow Donangelo et al. (2019) in defining value-added for Compustat firms following as the sum of operating income before depreciation, changes in inventories, and labor expenses.

$$\mathrm{VA}_{j,t} = \mathrm{OIBDP}_{j,t} + \Delta\mathrm{INVFG}_{j,t} + \mathrm{LABEX}_{j,t} \tag{A.1}$$

Changes in inventories are set to zero when missing. Here

$$\mathrm{LABEX}_{j,t} = \widetilde{W}_{j,t} \times (\mathrm{EMP}_{j,t} + \mathrm{EMP}_{i,t-1})/2 \tag{A.2}$$

Instead of imputing wages as industry-size cell averages of Compustat item XLR as in Donangelo et al. (2019), I create a measure of the average wage ($\widetilde{W}_{j,t}$) paid to workers using my LEHD-Compustat match. I detail my computation of the average wage later on in this section. Because coverage of the LEHD varies by year and may not contain all establishments from a given Compustat gvkey in a given year, I multiply the inferred LEHD wage by the average of the firm's employment in years t and $t-1$, rather than directly summing up LEHD wage compensation. Because Compustat employment is reported at year-end, I also follow Donangelo et al. (2019) in taking the average employment in adjoining years. Labor productivity is given by

$$\log(\mathrm{VA}/\mathrm{Worker})_{j,t} = \log(\mathrm{VA}_{j,t}/\mathrm{EMP}_{j,t}) \tag{A.3}$$

Unless otherwise specified, I refer hereafter to labor productivity, productivity, and log value-added per worker interchangeably. Finally, I define the labor share of firm i at time t by

$$\mathrm{LSHARE}_{j,t} = \frac{\mathrm{LABEX}_{j,t}}{\mathrm{VA}_{j,t}} \tag{A.4}$$

AKM Wage Decomposition

I now detail how I decompose wages into worker- and firm-specific heterogeneity in the tradition of Abowd et al. (1999) (AKM). I start with a modification of the AKM decomposition proposed by Lachowska et al. (2020) and Engbom and Moser (2020) that allows for the firm-specific component of wages to vary by time. Let i index individual workers; $j(i,t)$ a function indicating the firm j that employs individual i at time t; and $X_{i,t}$ a third-degree polynomial in worker age that is flat at age 40, as in Sorkin (2018) and Card, Heining, and Kline (2013). I then estimate the wage decomposition

$$\log(w)_{ijt} = \alpha_i + \phi_{j(i,t),t} + \beta X_{i,t} + \epsilon_{i,t} \tag{A.5}$$

I estimate (A.5) for matched Compustat firms for overlapping 5-year moving windows. Because my analysis is at the annual frequency and LEHD earnings are quarterly, each wage w_{ijt} represents a full-year equivalent real wage for individual i in year t, as in Sorkin (2018). In order to be included in the sample for estimating (A.5), firm j must be worker i's primary employer for that year (the firm with highest earnings), worker i must have been employed at that firm for at least two consecutive quarters within the year, and the worker must have earned more than \$3250 in 2011 US dollars. Papers performing AKM wage decompositions on LEHD earnings data set a lower threshold on annual earnings because earnings because hours worked are not observed in the LEHD. Due to the large size of the LEHD data, I estimate (A.5) for four disjoint 25% subsamples of my original sample of individuals found in the LEHD. All firm level aggregates taken from these estimates represent averages across these four 25% subsamples. Estimating (A.5) also requires that firm-years and individual worker pairs must belong to a set satisfying connectedness conditions in order for the fixed effects α_i and $\phi_{j(i,t),t}$ to be separately identified. I provide more details on this connectedness requirement and the sampling procedure in appendix A.

I use the sample of workers in (A.5) to create my inferred firm average wage $\widetilde{W}_{j,t}$. Let $N^{ins}_{j,t}$ denote the total number of workers mapped to firm j that are in the sample of (A.5) in year t. Let $N^{not-ins}_{j,t}$ denote the number of unique individuals that show up on the payrolss of firm j in year t that are not included in the sample (either due to earnings below the required threshold or firm j not being the primary employer for that year). Let $\widetilde{w}_{ijt}$ denote the actual (not full-year adjusted) year t earnings of worker i at firm j. Then I compute the firm-specific wage as

$$\widetilde{W}_{j,t} = \frac{\sum_i \widetilde{w}_{ijt}}{N^{ins}_{j,t} + N^{not-ins}_{j,t}} \tag{A.6}$$

I contrast this unadjusted wage $\widetilde{W}_{j,t}$ with a full-time full year equivalent adjusted wage $W_{j,t}$. Let $\Gamma_{j,t}$ denote the set of workers that are in the sample of (A.5) for firm j in year t:

$$W_{j,t} = \frac{\sum_{i \in \Gamma_{j,t}} w_{ijt}}{N^{ins}_{j,t}} \tag{A.7}$$

I use the actual average earnings $\widetilde{W}_{j,t}$ multiplied by Compustat employment for computing firm level value-added, labor expenses, and labor shares, as in (A.1), (A.2), and (A.4). Meanwhile, I focus on the employment response to log changes in the firm's full-time, full-year equivalent adjusted wage $W_{j,t}$ when I estimate supply elasticities in Section 3. I make these choices in order to ensure that my computation of labor expenses represents actual spending on labor per Compustat employee, while estimated supply elasticities correspond to the employment response induced by a change in the wage offer for a consistently defined period of employment.

Firm-Level Measures Derived From AKM Estimates

I introduce three measures that I derive from the wage decomposition in (A.5). The first is a measure of the firm's skill:

$$\alpha_{j,t} = \log\left(\frac{\sum_{i\in\Gamma_{j,t}} \exp(\widehat{\alpha}_{i,t})}{N^{ins}_{j,t}}\right) \tag{A.8}$$

Thus $\alpha_{j,t}$ is the log of the average component of wages that is due to the worker-specific heterogeneity of the firm's labor force. Firms with more skilled workers will have a higher $\alpha_{j,t}$. Next, I introduce an employment based analogue of the Tobin's Q valuation ratio. Let $V_{j,t}$ denote the total enterprise value of firm j in year t. Then define $Q^{Emp}_{j,t}$ as

$$Q^{Emp}_{j,t} = \frac{V_{j,t}}{\sum_{i\in\Gamma_{j,t}} \exp(\widehat{\alpha}_{i,t})} \tag{A.9}$$

I compute firm value $V_{j,t}$ following Peters and Taylor (2017). Finally, I introduce a skill-adjusted measure of the firm hiring rate. Denote by $\Gamma^{Recruit}_{j,t}$ the set of workers i that are recruited to firm j in year t, where a recruit is any worker whose primary employer is j in year t, but not in year $t-1$. Then define

$$\text{Inv Rate}^{Hire}_{j,t} = \frac{\sum_{i\in\Gamma^{Recruit}_{j,t}} \exp(\widehat{\alpha}_{i,t})}{\sum_{i\in\Gamma_{j,t-1}} \exp(\widehat{\alpha}_{i,t-1})} \tag{A.10}$$

Hence $\text{Inv Rate}^{Hire}_{j,t}$ represents the ratio of the skill-weighted number of workers hired in year t relative to the skill-weighted number of workers employed by the firm in the previous year.

A.2 Sampling/Cleaning LEHD Wage Data and Estimation of Wage Decomposition

The basic person-level identifier variable in the LEHD data is the PIK, which has a one-to-one correspondence with and individual's Social Security number. My baseline LEHD sample comes from the list of unique PIK identifiers obtained from the union of all individuals found in the 2000 Decennial Census; the SIPP and Current Population Survey; and a 10% subsample of the Numident sample. I further generate four disjoint 25% subsamples of this list of PIKs intersected with the list of PIKs in the LEHD.

I repeat the following steps for each of the four disjoint 25% subsamples. All estimates are taken separately across these disjoint subsamples; all firm level aggregates represent averages across these four subsamples. For each year in the LEHD 1990-2015 I retain all individuals who were found to be employed at a Compustat-linked firm in that year. This forms my LEHD-Compustat match. I then clean LEHD earnings data using a procedure that follows very closely with Sorkin (2018). Because days and hours worked in the LEHD are not observed, these steps are meant to convert quarterly LEHD earnings to their full-year wage equivalents. I categorize quarterly earnings observations into three groups: full, continuous, and discontinuous. Quarter q is a full earnings occur when individual i is linked to firm j at quarters q, $q-1$ and $q+1$; quarter q is a continuous is when individual i is linked to firm j at quarters q and on of $q-1$ and $q+1$, but not both; finally, discontinuous quarters occur when individual i is linked to firm j at during quarter q but not at $q-1$ or $q+1$.

Because full quarters are most likely to represent full-time employment, they are prioritized as follows. If individual i has any full quarters of employment at firm i in the given year, then annual wages are taken to be

$$4 \times \text{Total Earnings in Full Quarters/Number of Full Quarters.}$$

If there are no full quarters of employment, then annual earnings are

$$8 \times \text{Total Earnings in Continuous Quarters/Number of Continuous Quarters.}$$

Finally, if there are no continuous quarters, annual earnings are given by

$$12 \times \text{Total Earnings in Discontinuous Quarters/Number of Discontinuous Quarters.}$$

Assuming separations occur uniformly within a quarter, a continuous quarter represents a half quarter of employment at the firm and a discontinuous quarter represents a third of a quarter of employments, so this adjusts wages to full-year terms. Full quarters require no such adjustment, which is the reason for prioritizing full quarters of employment over others. Earnings are further adjusted to real equivalents. I follow Sorkin (2018) in using the CPI from the 4th quarter of 2011 as the baseline for this real wage adjustment.

I link each individual to their primary in each year. The primary employer is the one where their unadjusted earnings are highest. I define the employer at the gvkey level for worker-firm-years linked to a Compustat firm, and at the EIN level for worker-firm-years with no such link. Since I restrict to persons linked to Compustat in the current year, every individual in my sample will have at least one job in year t linked to a Compustat gvkey, although this will not always be their primary employer. Using the adjusted earnings I estimate the modified AKM decomposition for individuals' primary employers:

$$\log(w)_{ijt} = \alpha_i + \phi_{j(i,t),t} + \beta X_{i,t} + \epsilon_{i,t} \tag{A.11}$$

As in Sorkin (2018), I require individuals to have earned more than \$3250 in real earnings at their employer in the year in order to be considered part of the sample for estimating (A.11). Since hours or days worked are not observed, this drops individuals likely to have had a minimal attachment to the firm in the year due to part-time employment. I estimate (A.11) for overlapping 5-year periods starting in 1991 and ending in 2010. Because I have estimates for overlapping 5-year intervals, I take estimates for the final year from the sample (i.e. firm and person effects in 2013 come from the 2009-2013 subsample, 2014 comes from 2010-2014). The exception is for 1991-1994, which do not have a full 5-year period ending in the given year, so estimates for these years come from the 1991-1995 subsample. This gives me estimates of model parameters for the 1991-2014 period (though the LEHD covers 1990 and 2015, I use these years to determine full/continuous/discontinuous quarters in 1991 and 2014, respectively, leading my sample period to span 1991 to 2014).

Fixed effects in (A.11) are only defined for the collection of firm-years connected by worker flows across firms and time. Because I have time-varying firm effects, the connectivity requirements are slightly different than in the classic AKM decomposition with time-invariant firm effects (see Lachowska et al. (2020) and Engbom and Moser (2020) for a detailed discussion on the connectivity requirements in order for parameters to be identified). Accordingly, when estimating (A.11) I restrict the sample to the largest connected set of worker-firm-year observations following these two papers. Because Compustat firms are large, this connectivity restriction drops a tiny fraction of the data.

A.3 Imputing Compustat Staff and Labor Expense (XLR)

Here I describe my imputation of Compustat variable XLR. I use $LABEX_{j,t}$ from (A.2) in the main text. I use $LABEX_{j,t}$ to predict XLR out-of-sample (I also use predicted XLR even when actual XLR is available for consistency). To ensure the predicted XLR is positive I run the following regression in logs:

$$\log(XLR_{j,t}) = \alpha + \alpha_{I(j),t} + \beta_t \log(LABEX_{j,t}) + \epsilon_{j,t} \tag{A.12}$$

Here $\alpha_{I(j),t}$ are two-digit NAICS by year fixed effects and β_t are time-varying coefficients on $\log(LABEX_{j,t})$.

My predicted level of XLR is then just $\widehat{XLR}_{j,t} = \exp\left(\widehat{\alpha} + \widehat{\alpha}_{I(j),t} + \widehat{\beta}_t \log(LABEX_{j,t})\right)$. When estimation error is taken into consideration, simply taking the exponential of the predicted log of the variable in question often leads to better forecasts (Brardsen and Lütkepohl, 2011). A Jensen's inequality adjustment term for a scaling factor, using the variance of the residuals and assuming lognormality leads to a non-trivial overestimate of the actual XLR, while the average level of $\widehat{XLR}_{j,t}$ is a much closer to actual $XLR_{j,t}$. Because of this I use the exponential of the log to create a strictly positive predicted $XLR_{j,t}$. In-sample $\widehat{XLR}_{j,t}$ has a correlation of 0.95 with actual $XLR_{j,t}$.

A.4 K-Means Clustering to Estimate Empirical Labor Market Boundaries

In this section I describe my method for grouping firms into empirical labor market boundaries based on k-means clustering of flows of workers across firms. Let $R_{i,j,t}$ be the numer of workers firm i has hired from firm j in year t, and $L_{i,j,t}$ the number of workers firm i loses to firm j in year t. I then construct a matrix A_t of flows across firms by assigning the the i,jth entry as follows:

$$A_{i,j,t} = \log\left(1 + \sum_{\tau=t-2}^{t} R_{i,j,\tau} + L_{i,j,\tau}\right) \tag{A.13}$$

I then perform k-means clustering on the columns of A to group firms into clusters that hire from one another, using 1 minus the cosine similarity of column vectors of A_t as the distance metric between vectors. The cosine distance between column $A_{j,t}$ and $A_{j',t}$ is defined as

$$1 - \frac{\sum_i A_{i,j,t} \times A_{i,j',t}}{\sqrt{\sum_i A_{i,j,t}^2}\sqrt{\sum_i A_{i,j',t}^2}}, \tag{A.14}$$

which is one minus the uncentered correlation between the vectors $A_{j,t}$ and $A_{j',t}$. Thus the distance metric accounts for the differences in the size of the two comparison firms. I take the log of the sum of inflows and outflows in order to downweight extremely large firms but still allow entries to be increasing in the number of workers that flow between the two firms. Because k-means clustering has a random component, I perform the routine 5 times each year using different starting points for the clusters, selecting the cluster assignment which explains the largest share of workers flows across firms for that year. Because the number of clusters must be pre-specified, I perform the routine for $k = 10$ and $k = 20$ labor market clusters per year.

I find that the method explains labor empirical labor markets quite well. In appendix table A8 I examine how much variation labor market-by-year fixed effects explain in the levels of and changes in log wage and employment as well as the fraction of firm-to-firm worker transitions occurring within the given labor market boundary. I compare the $k = 10$ and $k = 20$ clusters with 2-digit and 3-digit NAICS industrys. The table shows that about half of worker flows occur within the $k = 10$ version of the empirical labor markets, while also explaining three-fifths of log wages and having very comparable explanatory power for wage and employment growth, and stock returns as the other empirical labor market boundaries. In the final column of the table I show that, while the empirical labor market boundaries do a good job of capturing empirical labor markets, they capture similar variation in stock returns, and employment/wage growth as my baseline 3-digit NAICs fixed effects. In particular, there is a very small change in explanatory power when both labor market-by-year and 3-digit NAICs-by-year fixed effects are included at the same time.

B Controlling for Proxies of Labor and Capital Adjustment Costs in Investment-Q Regressions

The smaller investment response to Tobin's Q for productive firms are suggestive that Tobin's Q proxies relatively more for economic rents and not investment opportunities for these firms; however, the lower investment rates could also be due to higher adjustment costs for a given level of investment.[26]. Adjustment costs are likely different skilled and unskilled workers. For example, Belo et al. (2017) argue that adjustment costs are likely to be considerably higher for firms with a more skilled workforce, and Jager and Heining (2019) shows direct empirical evidence that firms have difficulty substituting for skilled workers. In appendix

[26]In the case where convex adjustment costs are quadratic and returns to scale are constant so that Tobin's Q = marginal Q, the coefficient on Tobin's Q is precisely one divided by the multiplicative convex adjustment cost parameter.

Table A3 I regress hiring on employment Q, and interact employment Q separately with labor productivity and estimated log firm average worker effects ($\alpha_{j,t}$ from (A.8)). The interaction term still has a strongly negative coefficient even after accounting for firm skill, suggesting that at least for hiring rates, the patterns are not likely to be driven purely by differences in adjustment costs. The coefficient on the interaction between average worker effects and employment Q is also negative, suggesting that these firms do face higher adjustment costs.

In appendix Table A4 I devise a similar exercise for capital investment based on the intangible share of capital, where I interact total Q with the log intangible share of total capital as proxy for variation in capital adjustment costs. To the extent that intangible and physical capital have differential convex adjustment costs, these findings are also not driven by productive firms facing disparate costs of adjusting their capital stock on account of having differing amounts of intangible capital.

C Elasticity Estimate Bias in a Simple Model of the Labor Market

Similar to Card et al. (2018) or Lamadon et al. (2019), suppose that L workers choose employers among a market of N firms, and normalize $L = 1$ for simplicity. Worker i's utility from working at firm j is increasing in firm-specific amenities $a_{j,t}$, the log of the wage offer $w_{j,t}$, and an unobservable taste shock $\epsilon_{i,j,t}$.

$$u_{i,j,t} = \theta \log(w_{j,t}) + a_{j,t} + \epsilon_{i,j,t} \tag{A.15}$$

Assuming the taste shocks follow a type I extreme value distribution, then standard results from McFadden (1973) imply the firm-specific labor supply curve:

$$L(w_{j,t}, a_{j,t}) = \lambda_t^{-1} \exp(a_{j,t}) w_{j,t}^{\theta} \tag{A.16}$$

Here I assume each firm views itself as atomistic in the market, and so they take the constant $\lambda_t = \left(\sum_{j'=1}^{N} \exp(a_{j',t}) w_{j',t}^{\theta}\right)$ as given. The parameter θ gives the firm-specific supply elasticity. Let $A_{j,t} = \lambda_t^{-1} \exp(a_{j,t})$ denote the level of the supply curve.

Firms choose the wage offer $w_{j,t}$ to maximize the following:

$$V_{j,t} = \max_{w_{j,t}} Z_{j,t}(L_{j,t})^{1-\alpha} - w_{j,t} L_{j,t} \tag{A.17}$$

subject to the functional form of the labor supply curve (A.16). Denote the wage markdown by $\mu \equiv \frac{\theta}{\theta+1}$ and define constant $c \equiv \frac{1}{1+\theta\alpha} \log\left(\mu(1-\alpha)\right)$. Solving (A.17) yields the expressions for the log optimal employment, wage, and firm value:

$$\log(L_{j,t}) = \theta c + \frac{\theta}{1+\theta\alpha}\log(Z_{j,t}) + \frac{1}{1+\theta\alpha}\log(A_{j,t}) \tag{A.18}$$

$$\log(w_{j,t}) = c + \frac{1}{1+\theta\alpha}\log(Z_{j,t}) - \frac{\alpha}{1+\theta\alpha}\log(A_{j,t}) \tag{A.19}$$

$$\log(V_{j,t}) = \log(1-(1-\alpha)\mu) + (1-\alpha)\theta c + \frac{1+\theta}{(1+\theta\alpha)}\log(Z_{j,t}) + \frac{1-\alpha}{1+\theta\alpha}\log(A_{j,t}) \tag{A.20}$$

For simplicity I look at the bias from running a regression of the levels of log employment/wages on log firm value. My empirical strategy of running the regression of stock returns on the growth rates in wages and employment is essentially the same except in differences instead of levels. The parameter estimate obtained from regressing log employment and wages on firm value and taking the ratio of the two coefficients is given by

$$\frac{\theta(1+\theta)\sigma_z^2 + \theta(1-\alpha)\sigma_{az} + (1+\theta)\sigma_{az} + (1-\alpha)\sigma_a^2}{(1+\theta)\sigma_z^2 + (1-\alpha)\sigma_{az} - \alpha(1+\theta)\sigma_{az} - \alpha(1-\alpha)\sigma_a^2} \tag{A.21}$$

Here σ_z^2 is the variance of $\log(Z_{j,t})$; σ_{az} is the covariance of $\log(Z_{j,t})$ and $\log(A_{j,t})$; and, σ_a^2 is the variance of $\log(A_{j,t})$. When there are no labor supply shocks, so that $\sigma_a^2 = \sigma_{az} = 0$, equation (A.21) collapses to the true supply elasticity θ.

For (A.21) to represent an upper bound on the supply elasticity—which implies a conservative estimate of the magnitude of wage markdowns—we must have

$$\rho_{az}\sigma_z > -\frac{(1-\alpha)}{1+\theta}\sigma_a \tag{A.22}$$

where ρ_{az} is the correlation of $\log(A_{j,t})$ and $\log(Z_{j,t})$. The intuition behind (A.22) is simple. Increases in the level of the supply curve $\log(A_{j,t})$, (increases in "amenities") allow firms to hire more workers at a given wage, which reduces wages, increases employment, and increases firm value, all else held constant. This tends to bias the wage response downward and the employment response upward, leading to an upward biased elasticity estimate. However, if $\log(A_{j,t})$ is sufficiently negatively correlated with firm productivity, then increases in $\log(A_{j,t})$ reduce firm value, and the elasticity estimate becomes downward biased.

The most obvious candidate for reversing the inequality (A.22) is market-specific productivity shocks. This is because $A_{j,t}$ is decreasing in the market-wide wage index λ_t =

$\left(\sum_{j'=1}^{N}\exp(a_{j',t})w_{j',t}^{\theta}\right)$:

$$A_{j,t} = \lambda_t^{-1}\exp(a_{j,t}) = \left(\sum_{j'=1}^{N}\exp(a_{j',t})w_{j',t}^{\theta}\right)^{-1}\exp(a_{j,t}) \tag{A.23}$$

Suppose that firm-specific productivity $Z_{j,t}$ is given by an aggregate market component and an idiosyncratic component: $Z_{j,t} = \widetilde{X}_t X_{j,t}$. Equation (A.19) then implies that the wage offer can be expressed as

$$w_{j,t} = C_{j,t}\widetilde{X}_t^{\frac{1}{1+\alpha\theta}}\lambda_t^{\frac{\alpha}{1+\alpha\theta}} \tag{A.24}$$

where the firm-specific term $C_{j,t}$ depends on $\exp(a_{j,t})$, $X_{j,t}$, and model parameters. Define $\widetilde{C}_{j,t} = \exp(a_{j,t})C_{j,t}^{\theta}$. Then

$$\lambda_t^{\frac{1}{1+\alpha\theta}} = \left(\sum_{j'}\widetilde{C}_{j',t}\right)\widetilde{X}_t^{\frac{\theta}{1+\alpha\theta}} \equiv \widetilde{C}_t\widetilde{X}_t^{\frac{\theta}{1+\alpha\theta}} \tag{A.25}$$

or

$$\lambda_t = \widetilde{C}_t^{1+\alpha\theta}\widetilde{X}_t^{\theta} \tag{A.26}$$

Hence $\log(A_{j,t}) = a_{j,t} - (1+\alpha\theta)\log\left(\widetilde{C}_t\right) - \theta\log\left(\widetilde{X}_t\right)$ is decreasing in $\log(\widetilde{X}_t)$. If enough of the variance in $\log(A_{j,t})$ is driven by $\log(\widetilde{X}_t)$ this can reverse the inequality in (A.22), causing downward biased supply elasticities. Aggregate productivity shocks do have meaningful variance and firm-specific amenities may be much more slow moving, so this case is feasible empirically. Since I assume firms are "small" in the market, the correlation of $\log(\widetilde{C}_t)$ and $\log(C_{j,t})$ is approximately zero and so I don't consider the impact of this term. The $\log(\widetilde{C}_t)$ term nets out with market-wide fixed effects along with $\log(\widetilde{X}_t)$. In summary, this means that without market-specific controls I am likely to overestimate the importance of labor market power.

After netting out market-specific productivity shocks (via market-by-year fixed effects or other controls) the condition (A.22) becomes highly plausible. When market shocks are netted out, (A.22) says that workers' perceptions of firm amenities should not *decrease* by too much when idiosyncratic productivity *improves*, or that firms should not cut their amenities by a lot on average when they experience a positive productivity shock. The more likely scenario is that firm amenities also improve when their productivity goes up, which guarantees that (A.22) holds, implying my estimates would be conservative if biased at all.

D Simple Example: Comparing Firm Value in Monopsony and Counterfactual Competitive Equilibria

To see how firm value might change in a competitive equilibrium relative to the monopsony equilibrium, consider the following simple setting. There is a representative firm which solves the following:

$$V = \max_{w} AL(w)^{1-\alpha} - wL(w) \tag{A.27}$$

where $L(w) = w^{\varepsilon}$ and ε is the supply elasticity. The firm's optimal wage and employment are

$$w = \left(\frac{\varepsilon}{\varepsilon+1}(1-\alpha)A\right)^{1/(1+\alpha\varepsilon)}, \qquad L = \left(\frac{\varepsilon}{\varepsilon+1}(1-\alpha)A\right)^{\varepsilon/(1+\alpha\varepsilon)} \tag{A.28}$$

And the value of wage markdowns as a share of operating income is

$$\text{markdown share} = \frac{\left(\frac{\epsilon+1}{\epsilon} - 1\right) w^{\varepsilon+1}}{V} \tag{A.29}$$

Consider the calibration $A = 1$, $\varepsilon = 2.5$, and $\alpha = 0.3$. This yields an aggregate labor share of about 0.5 and a markdown share of operating income of 40%, both of which are very close to their empirical counterparts. The firm value in this calibration is $V = 0.25$.

Now suppose that the firm takes wages as given and equilibrium wages are determined by the same labor supply function:

$$V^c = \max_{L} AL^{1-\alpha} - wL \tag{A.30}$$

where $L = w^{\varepsilon}$ gives the market clearing condition for wages. The resulting competitive wages and employment are

$$w^c = ((1-\alpha)A)^{1/(1+\alpha\varepsilon)}, \qquad L^c = ((1-\alpha)A)^{\varepsilon/(1+\alpha\varepsilon)} \tag{A.31}$$

Both the wage and employment increase in the competitive equilibrium. In the same calibration as before with $A = 1$, $\theta = 2.5$, and $\alpha = 0.3$, the competitive firm value is $V^c = 0.21$. So there is a 16% reduction in firm value in the counterfactual competitive equilibrium, even though wage markdowns are worth 40% of firm income in the equilibrium where the firm takes advantage of its labor market power.

E Supply Elasticity Robustness Checks: Empirical Specifications and Data

E.1 Different Controls for Market Level Shocks

Failing to account for common market shocks could bias my supply elasticity estimates downward, leading to an upward bias in the extent of labor market power. In Table A7 I present the elasticity estimate from my baseline specification (6) , as well as for a set of alternative specifications with different controls for common market shocks. The top panel of the table shows the employment responses to a stock return shock, while the bottom panel shows the wage responses. My estimate of the average elasticity—obtained by taking the ratio of the employment and wage responses—in the baseline specification (column 1) is about 2.5. In columns 2-6 of Table A7 I include different variations of controls for common market shocks. At the bottom of these columns I include a p-value for the test that the coefficient in the given column is different from my baseline estimate in column 1.

I find that each set of controls for common shocks yields estimates that are economically and statisically close. From this exercise I conclude that the main specification in column 1 of Table A7 does a reasonably good job of capturing the relevant variation in common market shocks that could bias my estimates downward. Consequently I focus on this baseline specification, noting that any quantitative changes from using a different specification would be minor.

In column 2 of Table A7 I drop the industry-by-year fixed effects. Consistent with the discussion above and the simple model in appendix C, this reduces my supply elasticity estimate. The difference has a p-value of .052, but economically the difference is not that large (2.525 in column 1 versus 2.397 in column 2). In the third column I add empirical labor market-by-year fixed effects instead of the industry-by-year fixed effects from column 1. I obtain these empirical labor market boundaries by performing k-means clustering on the flows of workers between firms; I explain the method in more detail in section A.4 of the appendix and show that it does a good job of capturing variation in worker flows between firms, as well as in wages and employment levels and growth rates. I choose $k = 10$ labor market clusters per year as my baseline.[27] See appendix table A8 for more details on the comparative performance of different definitions of labor market boundaries. In column 3 the elasticity is about 2.54 when controlling for these empirical labor markets.

I drop the labor market fixed effects in column 4 of Table A7, instead controlling for

[27] Using a different clustering method, Nimczik (2020) finds that $k = 9$ empirical labor markets does a good job of capturing empirical market boundaries in Austria.

the weighted average employment and wage growth of a firms' labor market competitors. To create this measure I weight the employment or wage growth by the number of workers a competitor has hired from the given firm, divided by the total number of workers that have been hired away by other firms in the sample. I similarly do this for the number of workers a given firm hires from a candidate firm. I then take the average of the inflow- and outflow-weighted measures. This column demonstrates that wages and employment do respond to competitor labor demand shocks, but controlling for these variables do little to change employment or wage marginal effects, and hence do not move the estimated elasticity.

In column 5 I control for both industry-by-year and labor market-by-year fixed effects, and in column 6 I use $k = 20$ labor market clusters per year instead of $k = 10$. This does not substantially change the supply elasticity estimate in quantitative terms and again the difference is not statistically significant.

E.2 Alternative Labor Demand Shocks

I now perform a number of robustness checks in order to gauge how much unobserved firm-specific labor supply shocks may be biasing my estimates. I first estimate supply elasticities using several different proxies for firm labor demand shifters. Results are found in appendix Table A9. I report the estimated supply elasticity using the alternative measure, the supply elasticity from my baseline specification , and the p-value on the test that my baseline supply elasticity estimates are different from the given estimate. Because a couple of the measures are available only for selected subsamples of the data, I re-estimate my baseline specification for these same subsamples of the data before comparing elasticity estimates.

In the first column of A9 I use stock returns of firms' customers rather than the firms themselves as shock to labor demand. In the next column I use the firm-specific R&D tax credits from Bloom, Schankerman, and Van Reenen (2013) (with data updated by Lucking, Bloom, and Van Reenen (2019)) as a labor demand shifter; since this is a level variable rather than a flow, I estimate (6) in levels rather than first differences in this column. To test if firm-specific exposure to common stock market risk factors matters, in the third column I use return residuals backed out from a regression on the 5 Fama and French (2015) factors, plus the momentum risk factor. In the fourth column I use patent-induced shocks from Kogan, Papanikolaou, Seru, and Stoffman (2017) as a shifter of firm labor demand; and finally, in the last column I follow Daniel, Hirshleifer, and Sun (2019) in using stock returns in excess of the market return in a four day window around earnings announcements, which isolates periods of time when information about fundamental firm cash flows is revealed.

Each different labor demand shifter in Table A9 yields elasticity estimates that are very close quantitatively and statistically indistinguishable from my baseline estimates. Because each of these measures can be expected to covary differently with possible unobserved firm-specific labor supply shocks, this suggests that any bias from such shocks is not likely to have large a quantitative effect on my stock-return based supply elasticity estimates.

In the first column of Table A9 I show the response to stock returns of firms' customers using cleaned Compustat customer-supplier data provided by Wharton Research Data Services. Cohen and Frazzini (2008) show that stock return shocks to customers eventually propagate to upstream to their suppliers as a demand shock, and consistent with this notion I find that the wages and employment at supplier firm both respond significantly positively to the stock returns to their customers. The identifying assumption is that these shocks to customers constitute a pure demand shock to the firm, and are orthogonal to firm-specific labor demand shocks after accounting for industry-by-year fixed effects. Customers' returns are likely much less affected by any idiosyncratic labor supply shocks of their suppliers than the suppliers' own returns. Still, a potential concern is that customers may also be labor market competitors, and so in unreported results I consider a version where I exclude customer-supplier links between firms who have hired from one another within a 5 year window centered at the current year, or who are in the same 3-digit NAICS industry. In both cases the estimated supply elasticity is nearly the exact same as the estimate based on own-firm stock return and is statistically indistinguishable, with both elasticity estimates for the customer return sample being close to 2.

Column 2 of Table A9 uses variation in labor demand induced by the federal tax treatment of R&D expenses from Bloom et al. (2013), who show that differential exposure causes firms to undertake more R&D. Firms with differing tax-induced incentives to engage in more or less R&D may also be induced to adjust their labor forces to meet this incentive, which constitutes a labor demand shock. Because this measure is a level rather than a flow, I estimate a version of (6) in levels rather than changes when using the tax credit measure, now controlling for lagged log employment, wages, and assets and contemporaneous firm skill composition. I continue to include industry by year fixed effects to soak up market level variation. Again we find an elasticity estimate that is similar to the baseline and statistically indistinguishable, with the tax-credit induced elasticity being a little lower than my baseline for this sample of firms (1.73 to 1.96).

In the third column of Table A9 I use the firms' cumulative abnormal log excess returns with respect to the Fama and French (2015) five factor model augmented with the momentum factor.[28] I construct stock return residuals as follows. In each year and for each firm i I

[28] All risk factor and risk-free rate data are obtained from Ken French's website.

regress daily log excess returns on the 6 risk factors:

$$\log(R_{i,t}) - \log(R_{f,t}) = \alpha_i^\tau + \beta_i^\tau F_t + \epsilon_{i,t} \tag{A.32}$$

Here τ denotes the year corresponding to day t and F_t is a vector of risk factors. I take the estimated $\hat{\beta}_i$ for year τ and take these as the risk exposures for year $\tau + 1$ to get cumulative abnormal log returns. A literature starting with Vasicek (1973) suggests that out-of-sample risk exposures are more accurately estimated when applying Bayesian shrinkage to deflate the estimates towards a common average. I shrink the $\hat{\beta}_i$ estimates towards the cross-sectional average $E_\tau[\hat{\beta}_i]$, using the cross-sectional variance of the $\hat{\beta}_i$ estimates, $\mathrm{Var}_\tau(\hat{\beta}_i^\tau)$, and the standard error of the own firm's estimate, $SE^2_{\beta_i}$, to get the shrinkage weights:

$$\widetilde{\beta_i^\tau} = (1-\omega)\hat{\beta}_i + \omega E_\tau[\hat{\beta}_i^\tau] \tag{A.33}$$

where

$$\omega = \frac{SE^2_{\beta_i}}{SE^2_{\beta_i^\tau} + \mathrm{Var}_\tau(\hat{\beta}_i^\tau)} \tag{A.34}$$

I similarly deflate the intercepts α_i. Finally, the cumulative abnormal returns for year $\tau + 1$ are given by

$$\text{Abnormal Return}_{i,\tau} = \sum_{t\in\tau} \log(R_{i,t}) - \log(R_{f,t}) - \widetilde{\beta_i^{\tau-1}} F_t - \widetilde{\alpha_i^{\tau-1}} \tag{A.35}$$

I use Abnormal Return$_{i,\tau}$ in place of the excess return and re-estimate (6). I follow the same timimg convention as in mys baseline spec so that abnormal returns are aggregated from July until June of the following year. My inclusion of this measure relates primarily to the need to control for market shocks discussed previously and demonstrated in Table A7. In particular, a long literature in asset pricing argues that these factors represent systematic shocks, exposure to which demands compensation with higher returns. My inclusion of industry-by-year fixed effects implicitly imposes constant betas within industry on these systematic shocks, but it's possible the heterogeneity in exposure to systematic factors could matter. This procedure accordingly allows firms to have differential exposures to common factors. Using this measure again yields a very similar elasticity estimate to my baseline (2.34 relative to a baseline of 2.52, difference statistically insignificant).

In the fourth column I show the supply elasticity implied by employment and wage responses to patent induced shocks to firm value from Kogan et al. (2017), who show that the measure predicts changes in firm productivity, employment, and sales, all consistent with a marginal revenue productivity shock. Patent values are estimated from stock price movements in a small window around patent grants, and capture information in price movements related to firm innovation. I follow Kogan et al. (2020) in looking at the response of valuable of patents from the year they are filed rather than granted and use patenting in year t as a shock to labor demand from year t to $t+1$. This specification is related in spirit to Kline et al. (2019), who estimate passthroughs of patent-induced shocks to worker and firm outcomes based on predicted Kogan et al. (2017) patent values. Again I find an elasticity estimate that is close to my baseline.

Finally, I use the response to own-firm stock return shocks, except I only use stock price responses in a small time window around earnings announcements. Following Daniel et al. (2019) I use cumulative daily returns in excess of the market starting over the four day period starting the day before the announcement, summing up all 4-day announcement returns over the July to June period. The idea here is to isolate stock price movements that are highly likely to be related to information about firm productivity unrelated to information about labor supply. Stock price movements around earnings announcement are driven primarily by firms announcing unexpectedly high or low income; thus restricting to these small windows is more likely isolates price movements related to information about firm demand that is unrelated to firm-specific labor supply shocks. Though the measures are different, this follows a similar intuition to my use of Kogan et al. (2017) patent induced shocks to the firm—both measures isolate movements in prices due to information revealed to the market that is highly related to firm revenue productivity, and hence instruments for shifts in labor demand.

E.3 Controlling for Observable Labor Supply Shocks (Union Elections and Changes in Non-Compete Enforceability)

While these findings are useful in establishing the plausibility of my baseline supply elasticity estimates, it would still be helpful to control for potential firm-specific labor supply shocks, if they can be made observable. Although one can never definitively say that every firm-specific labor supply shock has been accounted for, I now include specifications where I control separately for two labor market shocks that have featured prominently in prior literature: union elections and changes in non-compete contract enforceability. Results are in appendix Table A10, which shows that bias from excluding these more salient observable firm-specific labor supply shocks is not important quantitatively. This lends credence to my argument that this sort of confounding variation is not likely to substantially bias my estimates in general.

I control for unionizations using data on union elections from the National Labor Relations

Board and matched to Compustat records by Knepper (2020).[29] Changes in non-compete enforceability come from the lists of changes compiled by Ewens and Marx (2017) and Jeffers (2019). Following Ewens and Marx (2017) I assign non-compete changes by firms' headquarters.

Union elections may be one of the single best candidates for the type of firm-specific labor supply shock that could bias my estimates. For example, Lee and Mas (2012) find that union elections wins induce significant negative stock return responses; I verify the same result in the last row of Panel A of Table A10. However, even among the firms who have experienced large union elections, the unionization event accounts for a small amount of variation in firm-specific stock returns, so that controlling for unionizations leads to negligible changes in my estimates. Meanwhile, Jeffers (2019) argues that firms use non-competes successfully to diminish the mobility of their skilled workers, and so changes in non-compete enforceability could also in theory constitute a labor supply shock that bias my estimates. .

In Panel A of appendix Table A10 I re-estimate supply elasticities based off (6) for firms who ever experienced a union election during my sample; the first column is from the baseline specification without controlling for unionizations, and the others include different sets of unionization controls. Consistent with Lee and Mas (2012), I find a significantly negative stock return response in the year a union wins an election. Despite this fact, allowing for this supply shock to be observable has no effect on my estimated elasticities, despite the fact that I restrict the sample to only firms who have experienced a sufficiently large union election.

Following Knepper (2020), I focus on firms experiencing union elections where at least 20 employees voted in the election. In order to give maximal explanatory power to the union elections, I restrict the sample to just the set of firms identified at some point to have experienced a sufficiently large union election.

In the second column of Table A10 I include dummies for whether a union election win occurs in year t, $t+1$, or $t-1$ (as well as dummies for whether any election is occurs); in the third column I ascribe all variation in stock returns in a union election year to the election by adding interactions of stock returns with the full set of union election dummies in the second columnn. In all cases the elasticity is quantitatively close to the baseline elasticity and statistically indistinguishable, even when assuming all variation in stock returns during in the 3 years surrounding surrounding a union election are due to the election.

Non-compete agreements make it more difficult to move to a competing employer; Jeffers (2019) shows that they are quite common and especially prevalent among skilled workers. Changes in non-compete enforceability are therefore another good candidate for firm-specific

[29] Thanks to Matthew Knepper for generously sharing his data.

labor supply shocks that could bias my estimates. Following Ewens and Marx (2017) I assign non-compete changes by firms' headquarters. Panel B of Table A10 shows the resulting supply elasticities when controlling for non-compete changes. In the first column I include indicators for whether a non-compete increases or decreases in enforceability in years t, $t+1$, or $t-1$. In the next columns I add interactions of all non-compete dummies with the stock return in that year. As was the case with union elections, after controlling non-compete changes, supply elasticity estimates are quantitatively very close and statistically indistinguishable.

E.4 Robustness Checks for Labor Productivity Sorted Supply Elasticity Estimates

Since production methods and labor markets vary from industry to industry, one concern with my Table 4 could be reliance on unconditional labor productivity sorts that use between rather than within industry variation. In Table A11 I instead sort firms into productivity quartiles within their 2-digit NAICS industry and re-estimate (8) for employment and wages to back out supply elasticities for workers of all skill levels. All my basic findings from Table 4 are unchanged for the within industry sorts in A11, and all the elasticity point estimates are very similar.

In appendix Table A12 I address a few more potential concerns with elasticity estimates for firms sorted on productivity. One possibility is that wage responses are larger in the short-run for productive firms because they have more immediate flexibility in adjusting their wages, and so the monotonically decreasing pattern in elasticities sorted on productivity could be driven by the horizon. For example, unproductive firms may be more constrained in their ability to adjust wages in the short horizon. To address this, I re-estimate elasticities from (8) for the 3-year horizon. Specifically, I replace stock returns and employment/wage growth (as well as the control for contemporaneous changes in firm skill composition) with their 3-year equivalents:

$$\log(Y_{j,t+3})-\log(Y_{j,t}) = \alpha+\alpha_{q(j,t)}+\alpha_{I(j),t}+\sum_{q=1}^{4}\mathbf{1}(q(i,t)=q)\times\beta_q\text{Stock Ret}_{j,t\to t+3}+\Gamma X_{j,t}+\epsilon_{j,t} \tag{A.36}$$

Panel A of Table A12 shows that productive firms similarly have much lower supply elasticities at this horizon. Thus the cross-sectional sorting in elasticities is not merely driven by the time horizon. Elasticities in general are also a little higher for this horizon, implying more elastic labor supply over the long run.

Another concern may be that results are driven by the equity-based compensation of the most skilled workers. For example, Eisfeldt, Falato, and Xiaolan (2021) document a large rise in equity compensation over my sample period, which may be due to contracting issues unrelated to a wage posting, monopsonistic model of the labor market. The LEHD includes all compensation that is immediately taxable, which includes equity based pay upon exercise. However, note also that such equity-based incentive pay is not incompatible with a monopsony framework, as it intrinsically ties the compensation of employees with their marginal revenue productivity, as would be implied by a monopsony model. Another issue is that a non-trivial fraction of equity-based compensation may not show up in LEHD earnings because they are taxed as capital gains, in which case the wage responses for supply elasticities may be mismeasured for skilled workers. That being said, Table 4 already alleviates these concerns in part, because productive firms face significantly lower supply elasticities even for the least skilled workers, whose compensation is far less tied to equity-based incentive pay.[30] Another factor that helps alleviate this concern is the fact that elasticity estimates are by far the lowest for skilled workers, and this is due to the larger wage passthrough of firm-specific stocks to skilled workers' compensation. Deferred compensation that does not immediately show up as taxable earnings would tend to bias down the wage passthrough of skilled workers, but I find that skilled workers have by far the highest wage passthroughs of all. To address whether differences in deferred compensation are likely to drive the sorting patterns in supply elasticities, in Panel B of Table A12 I re-estimate (8) with changes in the time-varying firm wage fixed effects from (A.5), instead of the log average firm wage. This is the component of the log wage that is entirely firm-specific and is paid to all workers, regardless of their skill, incumbent status, or worker-firm match quality, and hence is less likely to be attached to firm- or worker skill-specific tendencies towards higher equity-based compensation. Again the most productive firms face by far the lowest supply elasticites. These elasticity estimates are not surprisingly a little higher than in Table 4, because they aren't able to take into account that the most highly paid workers make up a large share of the overall firm average wage and also have the least elastic labor supply.

Value-added per worker may be a noisy productivity proxy. In panel C I instead sort on estimates of firm total factor productivity from İmrohoroğlu and Tüzel (2014). Findings similarly go through in this case, as sorting on TFP also generates a decreasing pattern in supply elasticities. In unreported results I find a strong negative relationship between estimated TFP and firm labor shares. Hence my findings aren't driven by my choice of the log value-added per worker measure, but are instead driven by productivity advantages in

[30] Eisfeldt et al. (2021) find that 97% of equity-based incentive pay is accrues to the top 10% of workers in the manufacturing sector.

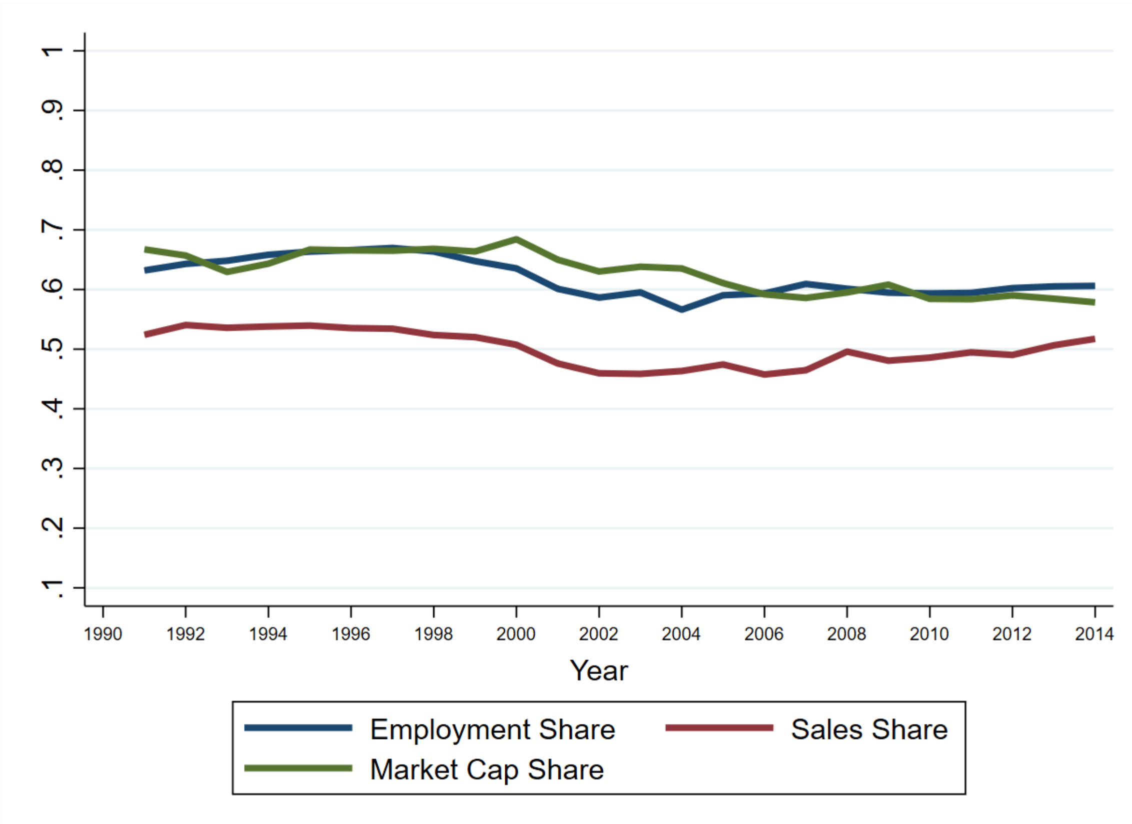
general.

Finally, due to concerns some have raised regarding employment reported in Compustat (Davis, Haltiwanger, Jarmin, Miranda, Foote, and Nagypál, 2006), in panel D I replace the Compustat-based employment with Longitudinal Business Database employment. I obtain LBD employment by aggregating reported employment across all LBD establishments linked to a given Compustat gvkey in that year.[31] I still find strongly monotonically decreasing supply elasticities across productivity types, but the LBD employment is not as responsive to stock return shocks and so I get slightly lower elasticity point estimates. This suggests that LBD-based supply elasticities would if anything increase the quantitative magnitude of my findings.

[31]LBD employment is collected in March while Compustat employment is almost always reported in December, and so I use LBD figures from the March nearest to the Compustat December employment report date to compute employment growth. I find that this yields larger employment responses than taking the previous March observation or an average of the two.

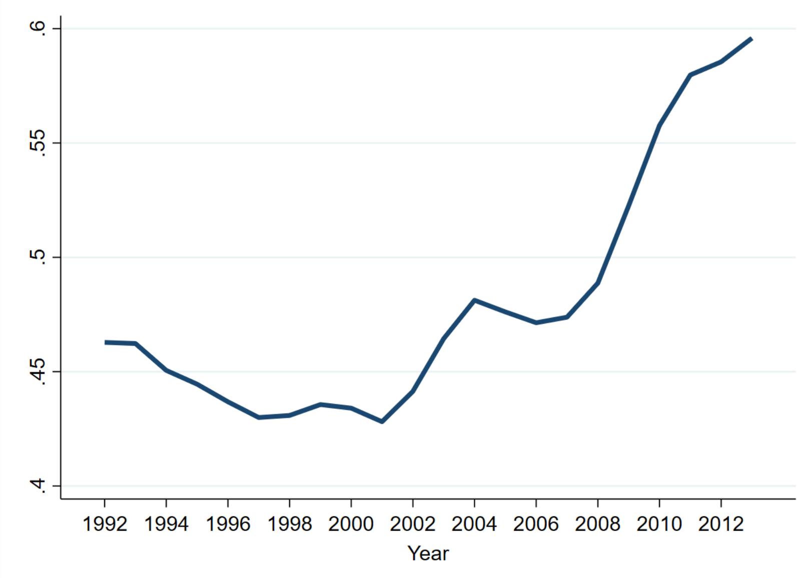
F Appendix Figures and Tables

Figure A1: Compustat-LEHD Matched Sample: Shares of Employment, Market Cap and Sales by Year



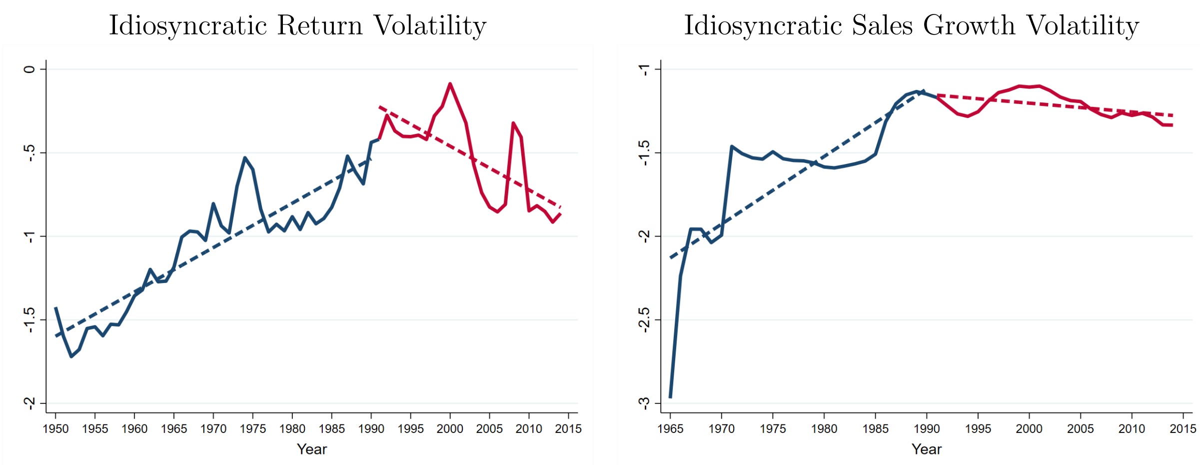
Note: This figure gives the shares of total Compustat sales, employment, and stock market cap that are represented in my Compustat-LEHD matched sample. The sample period spans 1991-2014.

Figure A2: Spread in Average Worker Skill Between Productive and Unproductive Firms Trends Upward



Note: This figure shows the differences for the average firm worker skill level (from (A.8) in the text) between firms in the top- and bottom-quartiles of labor productivity. The sample period spans 1991-2014.

Figure A3: Idiosyncratic Risk Has Increased Over the Long Term, But is Declining or Flat Within the 1991-2014 Period



Note: This figure plots the log average idiosyncratic stock return and sales growth volatility from Hartman-Glaser et al. (2019). The data span 1950-2014 for returns and 1965-2014 for sales. The 1991-2014 period covered in my paper is shown in red and the period before is shown in blue. Dashed lines give a separate linear time trend for the 1991-2014 and pre-1991 periods.

Table A1: Compustat Matched Sample and Overall Compustat Summary Stats

Panel A: LEHD-Compustat Matched Sample

	N	Mean	SD	P5	P50	P95
Log Assets	64000	5.862	1.946	2.887	5.752	9.327
Log Market Cap	63000	12.690	2.070	9.457	12.610	16.280
Log Sales	63500	0.646	1.861	-2.254	0.588	3.845
Log Employment	64000	5.872	2.009	2.769	5.843	9.260
Log Phys Capital	64000	4.914	2.205	1.542	4.808	8.734
Log Int Capital	63500	5.058	1.981	2.099	4.924	8.501
Excess Return	61500	0.119	0.542	-0.717	0.108	0.982

Panel B: Full Compustat Sample

	N	Mean	SD	P5	P50	P95
Log Assets	110000	5.396	2.202	1.991	5.246	9.334
Log Market Cap	164000	12.170	2.076	8.930	12.060	15.790
Log Sales	133000	-0.136	2.231	-3.689	-0.211	3.648
Log Employment	142000	5.180	2.408	1.380	5.133	9.200
Log Phys Capital	122000	4.428	2.643	0.366	4.284	8.992
Log Int Capital	137000	4.318	2.287	0.858	4.172	8.312
Excess Return	159000	0.104	0.518	-0.700	0.092	0.924

Note: This table provides basic summary stats for the full Compustat sample and my LEHD-Compustat merged sample. The sample period spans 1991-2014.

Table A2: Productive Firms Hire More Skilled Workers on Average

Dep Var: Log Firm Average AKM Worker Effects				
log VA/Worker	0.189	0.147	0.163	0.110
	(0.011)	(0.018)	(0.011)	(0.014)
Size Controls		X		X
Industry X Year FE			X	X
N	57500	57500	57500	57500
R^2 (within)	0.344	0.386	0.217	0.263

Note: This table shows regressions of log firm average AKM worker fixed effects (defined in (A.8)) on firm labor productivity as proxied by log value-added per worker. Controls for size include log employment, assets, and sales. Standard errors double clustered by year and industry in parentheses.

Table A3: Highly Productive Firms Have Lower Hiring Response to Employment Q After Accounting for Worker Skill

Dep Var: Hiring Rate	(1)	(2)	(3)	(4)
Q (Emp)	0.119	0.116	0.112	0.109
	(0.017)	(0.018)	(0.016)	(0.017)
Q (Emp) × log VA/Worker	-0.016	-0.014	-0.014	-0.013
	(0.003)	(0.004)	(0.003)	(0.004)
log VA/Worker	-0.033	-0.034	-0.039	-0.041
	(0.011)	(0.011)	(0.012)	(0.012)
Q (Emp) × Avg Skill		-0.01		-0.012
		(0.009)		(0.009)
Avg Skill		-0.017		-0.006
		(0.08)		(0.085)
Cash Flow			0.783	0.819
			(0.123)	(0.149)
Cash Flow × log VA/Worker			-0.155	-0.172
			(0.027)	(0.04)
Cash Flow × Avg Skill				0.124
				(0.117)
R-sq (within)	0.074	0.075	0.078	0.078
N	44000	44000	44000	44000

Note: This table shows regressions of investment in new hires on employment Q. The employment Q measure is interacted with a proxy for the costliness of labor/capital adjustment, the skill level of the firm's workforce (average worker AKM fixed effect). The last two columns add controls for cash flows as defined in Peters and Taylor (2017). All specifications include firm and year fixed effects. Standard errors double clustered by industry and year are in parentheses.

Table A4: Highly Productive Firms Have Lower Investment Response to Tobin's Q After Accounting for Intangible Capital Share

Dep Var: Total Invest Rate	(1)	(2)	(3)	(4)
Q (Tot)	0.066	0.059	0.054	0.048
	(0.009)	(0.008)	(0.008)	(0.009)
Q (Tot) $\times$ log VA/Worker	-0.007	-0.006	-0.006	-0.006
	(0.002)	(0.002)	(0.002)	(0.002)
log VA/Worker	0.011	0.01	-0.009	-0.01
	(0.003)	(0.003)	(0.002)	(0.002)
Q (Tot) $\times$ Log Intangible Share		-0.006		-0.005
		(0.003)		(0.002)
Log Intangible Share		0.008		0.008
		(0.004)		(0.004)
Cash Flow			0.223	0.203
			(0.045)	(0.044)
Cash Flow $\times$ log VA/Worker			-0.003	-0.002
			(0.01)	(0.01)
Cash Flow $\times$ Log Intangible Share				-0.021
				(0.01)
R-sq (within)	0.23	0.235	0.286	0.293
N	48500	48500	48500	48500

Note: This table shows regressions of investment in total capital on total Q from Peters and Taylor (2017) (panel B). The total Q is interacted with the firm's log intangible capital share of total capital as a potential proxy for the costliness of capital adjustment. The last two columns add controls for cash flows as defined in Peters and Taylor (2017). All specifications include firm and year fixed effects. Standard errors double clustered by industry and year are in parentheses.

Table A5: Calibration by Subperiod

Parameter	**Explanation**	**1991-2002**	**2003-2014**
α	(One minus) Labor returns to scale	0.22	0.22
β	Supply Elasticity Shifter	0.5	0.34
b	Reservation Wage	0.549	0.515
σ_z	Labor productivity volatility	0.141	0.149
$\bar{C}$	Convex adjustment cost level	0.38	0.745

Note: This table gives two alternative calibrations of the dynamic wage-posting monopsony model from section 4 in the main text. The calibrations are made to fit data moments for the 1991-2002 and 2003-2014 subperiods instead of the full sample moments from 1991-2014.

Table A6: Model Versus Data Moments by Subperiod

	1991-2002		2003-2014	
Moment	Model	Data	Model	Data
Separations Rate	0.36	0.32	0.29	0.27
Incumbent Premium	0.13	0.13	0.17	0.17
Inc/Rec Wage Pass. Ratio	1.67	2.10	2.11	1.59
Elasticity (Overall)	2.48	2.92	1.66	1.88
Elasticity (Inc.)	1.82	2.04	1.21	1.20
Elasticity (Rec.)	5.68	8.17	5.13	5.99
Elasticity (Q1)	5.04	4.86	3.16	3.25
Elasticity (Q2)	2.84	3.00	1.93	2.18
Elasticity (Q3)	2.20	2.95	1.51	1.55
Elasticity (Q4)	1.65	1.34	1.14	0.85
Log Labor Share	-0.49	-0.49	-0.52	-0.53
Log Labor Share (Q1)	-0.35	-0.21	-0.31	-0.23
Log Labor Share (Q2)	-0.46	-0.40	-0.48	-0.38
Log Labor Share (Q3)	-0.53	-0.50	-0.57	-0.53
Log Labor Share (Q4)	-0.64	-0.90	-0.72	-0.98

Note: This table compares model and data moments for my subsample calibrations of the dynamic wage-posting monopsony model from section 4 in the main text. ”Inc” and ”Rec” denote incumbents and recruits, respectively. The calibrations are made to fit data moments for the 1991-2002 and 2003-2014 subperiods instead of the full sample moments from 1991-2014. The label Q1 denotes the bottom quartile of labor productivity, Q4 denotes the top quartile, etc. The calibration of 5 model parameters explicitly targets the 7 moments that are not labor productivity quartile-specific, while the remaining 8 productivity quartile moments are not targeted.

Table A7: Baseline Elasticity Estimates Are Insensitive to Additional Controls for Common Market Shocks

Specification:	(1)	(2)	(3)	(4)	(5)	(6)
Employment						
Excess Return	0.116	0.115	0.116	0.115	0.116	0.116
	(0.009)	(0.010)	(0.009)	(0.010)	(0.009)	(0.009)
Competitor Emp Growth				0.043		
				(0.015)		
Competitor Wage Growth				0.002		
				(0.038)		
N	45000	45000	45000	45000	45000	45000
R^2	0.115	0.069	0.077	0.069	0.120	0.127
Wages						
Excess Return	0.046	0.048	0.046	0.046	0.045	0.045
	(0.004)	(0.004)	(0.003)	(0.003)	(0.004)	(0.004)
Competitor Emp Growth				0.022		
				(0.004)		
Competitor Wage Growth				0.155		
				(0.030)		
N	45000	45500	45500	45000	45000	45000
R^2	0.413	0.383	0.398	0.389	0.421	0.425
Base Controls	X	X	X	X	X	X
Year FE		X		X		
Industry × Year FE	X				X	X
Labor Market × Year FE			X		X	X*
Implied Elasticity	2.525	2.397	2.536	2.492	2.568	2.585
	(0.305)	(0.291)	(0.289)	(0.300)	(0.307)	(0.303)
P-value (Difference)		0.052	0.850	0.527	0.202	0.114

Note: This table shows estimates of (6) in the main text when different sets of controls are considered. Baseline control variables include the contemporaneous change in AKM worker effects (worker skill), and lagged wage, employment, and asset growth at the firm level. Industry fixed effects are defined at the 3-digit NAICS level. Labor market fixed effects are from empirically defined labor market clusters (estimation described in section A.4 of the appendix) with $K = 10$ labor market clusters per year. The "X*" in the 6th column indicates that I alternatively use $K = 20$ labor market clusters per year. Competitor emp and wage growth are an average of the growth rates of competitor firms that either hire from or whose employees are hired by the given firm. Local market controls include average changes in local labor market concentration, unemployment rates, and stock returns of firms operating in the same labor market. Wage data are from the LEHD, and the sample period spans 1991-2014. See section 3 in text for more details. "P-value (Difference)" denotes the p-value from the test that the given supply elasticity estimate is different from the baseline estimate in column 1. Standard errors double clustered by industry and year are in parentheses.

Table A8: Explanatory Power of Labor Market Proxies for Worker Flows, Wages, Stock Returns, and Employment

	K = 10	K = 20	NAICS2	NAICS3	K = 10 + NAICS3
Worker Flow Share	0.50	0.42	0.34	0.23	
Log Wage	0.62	0.65	0.51	0.60	0.70
Wage Growth	0.03	0.04	0.03	0.03	0.04
Excess Return	0.14	0.15	0.11	0.16	0.18
Log Emp	0.19	0.26	0.10	0.18	0.26
Emp Growth	0.03	0.03	0.03	0.03	0.04

Note: This table shows the explanatory power of different candidate labor market boundaries for several different variables. The first row reports the fraction of worker transitions between Compustat firms that occur within the same candidate market definition. The remaining rows report the adjusted R^2 from a regression of the given variable on market × year fixed effects. The first two columns show results for labor market clusters estimated with 10 or 20 clusters and the next two columns instead use either 2- or 3-digit NAICS codes. The last column reports adjusted R^2 values for labor market × year FEs and 3-digit NAICS × year FEs included simultaneously. Sample spans 1992-2013.

Table A9: Baseline Supply Elasticity Estimates are Robust to Alternative Shocks to Labor Demand

	Customer Ret	R&D Tax Credit	FF Resid	Patents	Earnings Ret
Elasticity	2.01	1.73	2.34	2.77	2.67
	(0.53)	(0.54)	(0.29)	(1.11)	(0.42)
Baseline Elasticity	2.12	1.96	2.53	2.53	2.53
	(0.33)	(0.18)	(0.30)	(0.30)	(0.30)
P-Value (Diff)	0.79	0.65	0.25	0.83	0.55
N	11500	21500	45000	45000	44500

Note: This table gives supply elasticity estimates using different labor demand shifters. All specifications have the baseline controls from (6) in main text, including industry × year fixed effects. "Customer Ret" uses the stock returns of the firm's customers instead of the firm itself. "R&D Tax Credit" uses federal treatment of R&D expenses from Bloom et al. (2013) and Lucking et al. (2019) as a labor demand shifter. "FF Resid" uses cumulative log abnormal returns relative to the Fama-French 5-factor model augmented with momentum. "Patents" uses patent induced shocks from Kogan et al. (2020). Finally, "Earnings Ret" follows Daniel et al. (2019) in using stock returns in excess of the market return in a 4 day window around earnings announcement instead of stock returns. Elasticities computed as the ratio of the employment and wage responses to the given shock. The baseline elasticity is the elasticity estimate for the my main method using annual excess returns as a labor demand shock. In the first two columns I report baseline elasticities for the selected subsample for which the given shock is available. "P-Value (Diff)" gives the p-value on the differences between the elasticity using the given shock and for my baseline estimate. See appendix section E for further details. Standard errors double clustered by industry and year are in parentheses, and are computed by estimating the elasticity via two-stage least squares where wages are predicted in the first stage and employment is then regressed on predicted wages.

Table A10: Labor Supply Shocks from Union Elections and Changes in Non-Compete Enforceability Do Not Affect Supply Elasticity Estimates

Panel A: Union Elections

	(1)	(2)	(3)
Elasticity	1.65	1.64	1.72
	(0.33)	(0.32)	(0.34)
P-Val (Diff)		0.57	0.35
N	4200	4200	4200
Baseline Controls	X	X	X
Union Dummies		X	X
Union Dummies × Excess Return			X
Industry × Year FE	X	X	X
Union Win Excess Return	-0.08***		

Panel B: Non-Compete Changes

	(1)	(2)	(3)
Elasticity	2.37	2.36	2.4
	(0.50)	(0.50)	(0.52)
P-Val (Diff)		0.29	0.70
N	11000	11000	11000
Baseline Controls	X	X	X
Non-Compete Dummies		X	X
Non-Compete Dummies × Excess Return			X
Industry × Year FE	X	X	X

Note: Panel A of this table shows how elasticity estimates change when including controls for a union election occuring and for a union win in years t, $t-1$, or $t+1$. Elections are taken from a list compiled by Knepper (2020). The third column adds interactions of union dummies with the excess return in that year. Panel B of this table uses non-compete changes from the lists compiled by Jeffers (2019) and Ewens and Marx (2017). Following Ewens and Marx (2017) firms are considered treated if their headquarters state changes non-compete laws in a given year; non-compete controls include separate dummies for non-compete increases and decreases in the years t, $t-1$, and $t+1$, and the third column interacts these dummies with stock returns. "P-Value (Diff)" gives the p-value on the differences the elasticity using the given shock and for my baseline estimate. The sample is restricted to only those firms who ever had a unionization/non-compete event, and the baseline estimate is computed for the same sample. Standard errors double clustered by industry and year are in parentheses, and are computed by estimating the elasticity via two-stage least squares where wages are predicted in the first stage and employment is then regressed on predicted wages.

Table A11: Productive Firms Face Lower Supply Elasticities For Workers of All Skill Levels—Within Industry Productivity Sorts

Productivity:	Quartile 1	Quartile 2	Quartile 3	Quartile 4	P-val 4-1	R-sq	N
			Whole Firm				
Employment	0.114	0.103	0.106	0.109			
	(0.012)	(0.012)	(0.010)	(0.011)	0.641	0.127	43500
Wages	0.028	0.039	0.047	0.088			
	(0.004)	(0.003)	(0.004)	(0.010)	0.000	0.421	43500
Elasticity	4.028	2.620	2.241	1.234			
			Low Skill Workers				
Employment	0.123	0.118	0.131	0.138			
	(0.013)	(0.013)	(0.016)	(0.014)	0.285	0.109	43500
Wages	0.014	0.017	0.020	0.028			
	(0.003)	(0.003)	(0.002)	(0.004)	0.016	0.289	43500
Elasticity	8.786	6.837	6.660	4.950			
			Middle Skill Workers				
Employment	0.111	0.098	0.107	0.103			
	(0.014)	(0.012)	(0.010)	(0.013)	0.444	0.084	43500
Wages	0.015	0.018	0.019	0.029			
	(0.002)	(0.002)	(0.002)	(0.003)	0.005	0.182	43500
Elasticity	7.283	5.281	5.743	3.580			
			High Skill Workers				
Employment	0.099	0.092	0.094	0.086			
	(0.013)	(0.012)	(0.008)	(0.011)	0.266	0.076	43500
Wages	0.049	0.063	0.075	0.116			
	(0.005)	(0.004)	(0.006)	(0.011)	0.000	0.453	43500
Elasticity	2.047	1.458	1.250	0.738			

Note: This table contains supply elasticity estimates for firms sorted on log value-added/worker quartiles as in Table 4, except I now sort firms on labor productivity within 2-digit NAICS industry. Controls include 3-digit NAICS industry by year and productivity quartile fixed effects; lagged growth rates in wages, employment, and total assets; and the contemporaneous change in average worker skill level at the firm (see (A.8) for definition). Workers are placed into skill groups based on their estimated worker effects from a modified Abowd et al. (1999) style wage decomposition with time-varying firm fixed effects. Individuals in the bottom two quintiles of the cross-sectional distribution of worker effects are considered low-skilled, the third and fourth quintiles middle-skilled, and the top quintile high-skilled. Changes in average worker skill are computed within the population of workers considered in the specification. "P-val 4-1" gives the p-value from a test that the coefficients for firms in the top and bottom quartiles have equal values. Wage data are from the LEHD, and the sample period spans 1991-2014. Standard errors double clustered by industry and year in parentheses. See section 3 in main text for more details.

Table A12: Elasticities for Firms Sorted on Productivity—Robustness

Productivity:	Quartile 1	Quartile 2	Quartile 3	Quartile 4	P-val 4-1	R-sq	N
Panel A: 3-Year Horizon							
Employment	0.203	0.173	0.202	0.193			
	(0.016)	(0.016)	(0.017)	(0.017)	0.563	0.202	34000
Wages	0.026	0.036	0.046	0.079			
	(0.002)	(0.002)	(0.004)	(0.008)	0.000	0.506	34000
Elasticity	7.778	4.793	4.418	2.441			
Panel B: Wage Growth Using Changes in AKM Firm Effects							
Employment	0.119	0.089	0.120	0.106			
	(0.013)	(0.010)	(0.011)	(0.011)	0.240	0.127	43500
Wages	0.017	0.021	0.029	0.043			
	(0.002)	(0.002)	(0.002)	(0.005)	0.000	0.252	43500
Elasticity	6.953	4.238	4.068	2.474			
Panel C: TFP Sort							
Employment	0.109	0.087	0.102	0.120			
	(0.011)	(0.008)	(0.008)	(0.015)	0.482	0.139	37500
Wages	0.034	0.040	0.053	0.085			
	(0.004)	(0.004)	(0.007)	(0.011)	0.000	0.427	37500
Elasticity	3.212	2.194	1.906	1.424			
Panel D: LBD Employment Growth							
Employment	(0.085)	(0.084)	(0.095)	(0.092)			
	0.013	0.009	0.013	0.013	0.683	0.082	40000
Wages	(0.028)	(0.033)	(0.051)	(0.091)			
	0.003	0.003	0.004	0.010	0.000	0.422	43500
Elasticity	3.105	2.552	1.854	1.011			

Note: This table shows robustness checks for the elasticity estimates obtained from estimating variants of (8) in the main text. All equations use the baseline set of controls from (8), which includes industry × year fixed effects. Panel A estimates a variant of (8) where stock returns and employment/wage growth are at the 3-year horizon. In Panel B changes in AKM firm effects $\phi_{j,t}$ from estimating (A.5) replace growth in the firm-level average wage in estimating the supply elasticity. Panel C sorts on firm total-factor productivity from İmrohoroğlu and Tüzel (2014) instead of log value-added per worker. Panel D uses employment growth from Longitudinal Business Database employment figures instead of Compustat. "P-val 4-1" gives the p-value from a test that the coefficients for firms in the top and bottom quartiles have equal values. Wage data are from the LEHD, and the sample period spans 1991-2014. Standard errors double clustered by industry and year in parentheses. See section 3 in main text for more details.