Firm Performance and the Volatility of Worker Earnings

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Abstract: The notion that firms provide wage insurance to risk-averse workers goes back to Baily (1974). Guiso et al. (2005) use Italian data and find evidence of full wage insurance in the case of temporary shocks to firm output, although only partial insurance for permanent shocks. Using linked employer-employee data for the U.S. retail trade sector, we examine whether shocks to firm sales are transmitted to worker earnings. We examine both short-term (one-year) and long-term (five-year) changes. We also examine whether this relationship differs by gender or across workers in different parts of the earnings distribution. We find no impact for short-term changes, but small positive elasticities for longer-term changes.

I. Introduction

Recent work has put increased focus on the role of firms in generating earnings inequality. For example, Card, Heining and Kline (2013) utilize German administrative data to show that workers with similar attributes earn different amounts in different firms, and that increasing between-firm variance in worker earnings accounted for as much as 25 percent of the overall increase in earnings inequality in Germany. Similarly, Barth, Bryson, Davis and Freeman (2014) use U.S. data to show that much of the increase in earnings inequality since the 1970s resulted from increased dispersion in earnings across firms.

In this paper we examine a related but different question: what is the role of firms in generating earnings volatility for workers? Earnings volatility—the within-worker fluctuation in earnings from year to year— also contributes to cross sectional earnings inequality. The seminal paper by Gottschalk and Moffit (1994) showed that nearly one-third of the increase in male earnings inequality over the 1970-1987 period was due to a rise in within-worker volatility in earnings. The evidence for more recent decades has been mixed. Papers based on the Panel Study of Income Dynamics find further increases (Shin and Solon (2011)) while other papers find little change or even a declining trend using other data sets (Celik, Juhn, McCue, and Thompson (2012), Dahl, DeLeire, and Schwabish (2007)).

Earnings volatility, to the extent that it is not fully anticipated, is one source of financial risk for workers. There is some concern that American families may now be more subject to financial risk, given trends such as the switch in retirement accounts from defined benefit to defined contribution plans, rising health care costs, rising housing prices in certain areas of the country and even rising college tuition rates (Hacker (2006)). Given this context, one question of interest is whether employers shield workers from fluctuations in demand. That is, are shocks to firm performance transmitted to worker earnings?

A rich literature on implicit contracts posits that firms will shield workers from fluctuations in demand (Baily (1974), Azariadis (1975), Rosen (1985)). This makes sense from the perspective of risk, in that entrepreneurs or stockholders are likely to have better access to capital markets than workers as well as have more expertise in diversifying risk. According to Baily (1974), firms offer workers a joint product: employment and insurance. But empirical work directly testing these models with firm-level data is scant. An exception is a paper by Guiso et al. (2005) which tests for wage insurance using matched employer-employee data from Italy and finds that worker earnings are insulated from idiosyncratic shocks to firm performance, especially those shocks that are temporary in nature.

An alternative reason that employers might favor variable pay is to provide incentives when worker effort is unobserved. In such cases, performance pay based on worker output may increase productivity (Lazear (1986), Lazear (2000)). Performance pay may also have desirable sorting effects and attract higher quality workers. During a period of rising skill demand, firms may institute performance pay in order to attract more skilled workers (Lemieux, MacLeod, and Parent (2009)). These models focus on performance pay based on individual output. Why firms may vary pay with firm or group-level performance requires slightly different reasoning. Profit sharing may provide worker incentives to the extent that workers feel they have a stake in the firm, especially if opportunities to free-ride are limited (Weitzman and Kruse (1990)). In the CEO pay literature, tying CEO compensation to firm performance will help solve the principal-agent problem. While it may make sense to vary CEO pay with firm performance, tying pay to firm performance for low level managers or other workers whose effort has little direct impact on firm performance seems less sensible. These types of arguments are made by Lazear (1999) and Oyer and Schaefer (2005).

In this paper we make two contributions. First, we document the extent to which changes in worker earnings are influenced by shocks to firm outcomes using a set of matched employer-employee data for the U.S. retail trade sector. Our second contribution is to provide evidence on the extent to which this varies by worker characteristics. The performance pay literature suggests that the trade-off between insurance and incentives should be most stark for employees who have a larger direct impact on firm performance. We test whether this prediction is born out in the data by comparing wage insurance for highly paid (top 20%) vs. the low-paid (bottom 20%) workers in the firm.

Our preliminary findings show no impact of short term (1-year) changes in firm revenues on worker earnings for those who remain employed by the same firm. We find very small positive elasticities—0.010 and 0.014—for 3-year and 5-year changes respectively. Our estimates do not change when we correct for measurement error in firm revenues, which suggests that measurement error does not appear to be the key reason for the small coefficients. We do not find much evidence that the earnings of highly paid workers are more affected by firm performance than the earnings of other workers. While these results point in the direction of wage insurance, we are wary of drawing strong conclusions based on our preliminary results. We expect results from our parallel analysis of firms in manufacturing and in selected services sectors to provide a broader perspective on this question, but those results are not ready for inclusion in this draft.

II. Related Literature

Guiso et al. (2005) use matched employer-employee data from Italy to test for wage insurance. Separately identifying permanent and temporary shocks to value-added measures at the firm level, they report complete wage insurance in terms of temporary shocks and only partial wage insurance in terms of permanent shocks. We use a similar framework with matched employer-employee data for the U.S. One important difference for our paper relative to Guiso et. al (2005) is the institutional setting. Like most European countries, Italy is subject to high levels of unionization and collective wage bargaining. With a centralized wage bargaining process, firms may not be able to adjust worker wages to idiosyncratic firm-level shocks. This is less of an issue in the U.S., so U.S. data may provide more fertile ground for testing wage insurance models.

Comin, Groshen, and Rabin (2009) investigate the extent to which volatility of firm revenues are transmitted to average wages of workers in the firm using a sample of large publicly-traded firms in the COMPUSTAT data. They find that the relationship between firm and wage volatility has become more positively related over time which they attribute to a shift in composition of jobs with more bonus pay. They also investigate the variation across industries and find that the relationship is stronger in services than in manufacturing. One drawback to their study is the use of average firm wages which could be driven by changes in worker composition. Using matched employer-employee data bypasses this difficulty by measuring wage changes for individual workers who remain employed in the firm.

Strain (2013) examines the relationship between firm employment volatility and volatility of worker earnings and finds a robust positive relationship. But one difficulty for his findings is the use of firm employment as the measure of firm performance. While shifts in firm employment may reflect exogenous shifts in product demand, employment is more likely to be impacted by firm choices. Firm performance measures such as revenues or value-added are arguably more exogenous and so it is more likely that the direction of causality goes from firm volatility to volatility of worker earnings.

Two other papers estimate empirical models that are closely linked to the models we estimate although the interpretations differ somewhat. Card, Cardoso, and Kline (2013) use matched employer-employee data from Portugal to estimate responses of worker earnings to within-firm changes in value-added. They find a small positive coefficient relating three-year changes in firm value-added to three-year changes in worker earnings. The authors interpret these results as profit sharing between firms and workers and interestingly, they find that the coefficient is higher for male workers than for female workers. In a similar vein, Barth et al. (2014) relate establishment level wages to establishment level sales per worker, controlling for firm fixed effects and find a positive coefficient. Again the authors interpret these results as a rent-sharing parameter between workers and firms.

III. Data

We base our empirical analysis on data collected from a sample of firms which we link to administrative records on the earnings, work histories, and demographics for their employees. Here we first describe the firm and employee data separately and then discuss how we join them.

Firm data

Our current results are based on data from Census's Annual Retail Trade Survey (ARTS), which collects information on sales and expenses for a panel of firms sampled from retail trade

(NAICS 44-45) and accommodation and food services (NAICS 72).[1] ARTS has two kinds of sampling units: a firm or the part of a firm associated with a particular federal Employer Identification Number (EIN) that reports activity under NAICS 44, 45, or 72. The purpose of collecting these data is to support timely estimates of aggregate retail trade activity, so firms with more sales have higher probabilities of selection. For this reason, our sample is primarily made up of large firms, making our results more representative of employment in retail trade than for the average firm in retail trade. The ARTS microdata are available for 1999 through 2012. A new sample for the panel is drawn every 5 years, once new information from Census's Economic Census of Retail Trade is incorporated into the sampling frame. Over the span of years in our data, new samples started in 2001, 2006, and 2011. While new firms appear in the panel primarily in those years, many large firms are selected again in subsequent panels and so remain in our data set for more than five years.

The ARTS data provide us with two measures of firm shocks: firm sales and gross margin. The latter measure is constructed by subtracting the cost of goods sold (purchases minus changes in inventories) from total sales, getting us closer to a measure of value added. We adopt this particular measure as our best approximation to value added for this sector because it is used in survey publications, and so is available in all years of our data and has relatively little missing

[1] We are working on parallel analyses based on employers in the Annual Survey of Manufactures (ASM) and in several industries that are included in the Services Annual Survey (SAS). Our services sample includes NAICS industries 621 (ambulatory health care), 622 (hospitals), and 623 (nursing and residential care facilities); NAICS 523 (securities, commodity contracts, and other financial investments and related activities); NAICS 541 (professional, scientific, and technical services), and NAICS 811 (repair and maintenance). We will include discussion of those samples in our next draft.

data. While some firms report sales separately for different parts of their organization, we use total sales (or total gross margin) reported for the firm as our measure of firm outcomes.

Employee data

Our data on employees come from Census's Longitudinal Employer-Household Dynamics (LEHD) database, which draws much of its data from complete sets of unemployment insurance (UI) earnings records for U.S. states. Workers' quarterly UI earnings records have been matched to characteristics of their employers drawn from quarterly administrative UI reports and to demographic and employer information from other Census data sources. While all states provided data for the LEHD program, the availability of data from earlier years varies by state. Our primary analysis sample of workers is selected based on their employment by the firms in our ARTS sample, as detailed in the next section. To provide some context, we also draw a 1% sample of all UI-covered workers in 39 states for years 2000-2011, and use that sample to estimate overall levels of earnings volatility.

Linked data

To construct our linked data set, we first take all of the firms and EIN units in the ARTS sample that have non-missing and non-zero sales measures. For multi-unit firms, which may file information using more than one EIN, we identify any additional EINs used by the firm. We then use that set of EINs to select UI employer records for ARTS firms, finding matches for 86 percent of the ARTS EINs.

Once we have a list of UI employer identifiers, we select all employees aged 25-59 who are employed by an ARTS firm in at least one year that they are in sample, and had at least one full-quarter of employment in that year. As is standard in using these data, we say the quarter t is a full quarter if that worker-employer pair has earnings reported in quarters t-1, t, and t+1. This definition is based on the notion that the individual was likely employed for the full 13 weeks of the quarter if they had earnings with the same employer both right before and right after the quarter. In tables below, we refer to this set of workers as our cross-sectional sample.

Our regression analysis requires that we further restrict our sample of workers to those with two consecutive full years of earnings with an ARTS employer ("stayers") so that we can measure the relationship between changes in worker annual earnings and the annual firm outcomes from the ARTS data. In year t, we define stayers as those having earnings with the same employer for each quarter in two adjacent calendar years (t and t+1), plus two quarters on either end of that spell (the fourth quarter of year t-1 and the first quarter of year t+2). When we examine 5-year changes, we further restrict our sample to long-term stayers--those who are in the data for six adjacent calendar years.

IV. Descriptive Statistics

Particularly in high-turnover industries like retail, our focus on stayers means that we exclude a large share of employment from our primary analysis. To give a sense of how our results relate to earnings volatility more generally, we present some descriptive evidence on earnings volatility for stayers relative to non-stayers based on a random sample of workers. We then present some descriptive statistics that illustrate how our sample exclusions affect the characteristics of our ARTS-based sample.

Earnings volatility of stayers and non-stayers

In estimating the effects of firm shocks on worker earnings in annual data, we end up with quite selective samples based on stayers, and in some instances only quite long-term stayers. From a broader perspective, while shocks to firm outcomes potentially affect earnings volatility through their effects on earnings changes among continuing employees, they also affect volatility by changing the likelihood that employees stay on their current job. Even for continuing employees, it may be that shared industry or local area shocks have more substantial effects than shocks that are idiosyncratic to the firm. But volatility among stayers provides an upper bound on how much individual firm outcomes could plausibly contribute to volatility. A straightforward accounting approximation gives the variance of earnings changes as the weighted average of variances for the stayer and non-stayer sets, with the weights equal to the relevant employment shares, as in

$$V_t \approx S_t * V_t^{stayers} + (1 - S_t) * V_t^{leavers}$$

where V_t is the variance of earnings growth for the relevant set and S_t is the stayer share. In forming this decomposition we use residualized earnings (net of age effects) and base our estimates on a separate one percent sample of workers drawn from the LEHD data without regard to industry, but satisfying our basic inclusion requirements. We plot trends separately for younger workers (ages 25-34) and older workers (ages 35-59).

Our stayer definition is based on having 10 quarters in a row with a single employer. For stayers in this sample who work for more than one job, we also require that their highest earnings job in year t and t+1 are with the same employer. We term other workers meeting our basic inclusion restrictions and having positive earnings “non-stayers,” as “movers” would be inaccurate given that that set includes many different possible employment patterns. We summarize the results of two decompositions meant to give a sense of the bounds on the relative importance of earnings variation among stayers versus non-stayers, holding our stayers definition constant. In both sets we use the standard deviation of the change in log earnings as our measure of volatility, and so include only workers with earnings in both t and t+1. In the first set, we compute the difference in log average full-quarter earnings on the individual's highest paying job between year t and year t+1, and then calculate the variance of that log difference for groups defined by year, two age groups, and whether they are a stayer. By dropping any quarters with zero earnings, and any in which someone worked less than a full quarter, we exclude much of the effects of non-employment from our volatility measure, and also increase the share of stayers by treating some non-stayers as out of scope because they have no full quarters of earnings.

Using this approach, as shown in Table 1, volatility among stayers averages about 14 percent of overall earnings volatility for those aged 25-34, and about 19 percent of overall earnings volatility for those aged 35-59. Time series graphs of the volatility components are shown in Figures 1-3. These are substantial shares, even if they are considerably less than half. Unsurprisingly, stayers account for a larger share of volatility among older workers, primarily because older workers are more likely to be stayers. Age differences in the volatility of earnings are small when conditioned on whether or not the individual changes jobs.

Alternatively, if we include any workers with positive earnings in both t and t+1, adding in the full effects of non-employment spells among those with some labor force activity in both years, stayers account for only two or three percent of overall volatility, suggesting that shocks to employers are only important for individuals' earnings through their effects on employment. Figure 3 shows that the share of stayers rises during recessions, consistent with less turnover during bad economic times. While the relationship between firm shocks and job losses or job changes is obviously important, it is outside the scope of this paper. Rather, we focus on firm shock transmissions to stayers as an interesting topic in its own right and aim to give insight into this other dimension of worker risk.

Summary statistics for retail trade

Table 2 shows summary statistics for our analysis sample for the long-run stayers that are included in our regression analyses, alongside two other comparison samples. One comparison sample—the "cross-sectional" sample described above—includes, all workers aged 25-59 that are linked to our sample of retail firms with at least one full quarter of earnings. The age restriction and the requirement of one full quarter of earnings eliminate many transient and low earnings matches. Annual average earnings for this sample is about $51,800 (in 2012 dollars). Part of the high average is explainable by the sample restrictions—for example, staying with the same retailer for many years is probably much more common among store managers than cashiers, but these means are also based on the full distribution of earnings, and so will be influenced by workers with very high earnings such as executives. Because the statistics are worker-weighted, averages for firm-side variables such as sales revenue reflect the characteristics of large firms.

The middle column further restricts the sample to observations on stayers at firms in-sample during the adjacent match years. Restricting to stayers raises the average age by approximately 2 years, and raises average annual earnings substantially, by about $17,000. Further restricting the samples to longer-run stayers of course reduces observation counts and further raises average earnings by about $4,300 per year.

V. Empirical Framework for Regression Models

Our basic empirical approach is to regress innovations in worker earnings on innovations in a measure of firm performance as in the following:

$$\Delta\omega_{ijt} = \alpha\, \Delta\epsilon_{jt} + e_{ijt}$$

where *i* indexes workers, *j* indexes firms, *t* indexes time, and where $\Delta\omega_{ijt}$ and $\Delta\epsilon_{jt}$ are respectively first differences of log worker earnings and log firm revenues, net of other factors. Various firm performance measures are possible, including total revenues or value added, or those constructs on a per-worker basis. The results presented here use the log of annual sales revenues. Value added is conceptually a better measure for our purposes because it approximates the relevant pool of funds that is subject to rent capture or bargaining by labor. But revenues is likely to be better measured, as value added is typically derived from revenues by netting out various categories of costs from sales.

Firm shocks

Revenues are presumed to act as firm-side drivers affecting worker earnings. Letting *j* index firms and *t* index time, log revenues R_{jt} follow

$$R_{jt} = Z_{jt}\gamma + f_j + \epsilon_{jt}$$

The $Z_{jt}\gamma$ term captures time-varying observable factors and the f_j term captures unobserved firm fixed effects. First differencing eliminates the fixed effects,

$$\Delta R_{jt} = \Delta Z_{jt}\gamma + \Delta\epsilon_{jt}$$

Since our conceptual framework stresses wage insurance, we include industry and year controls in some of our specifications. The notion is to net out aggregate and industry-level shocks in an attempt to derive idiosyncratic shocks (which are more likely to be insurable). For instance, industry and year controls may help to net out the effects of any common input cost shocks.

Permanent versus temporary shocks

It is useful to specify permanent and temporary components to the innovations $\Delta\epsilon_{jt}$. We use as a baseline model a process for ϵ_{jt} that includes a random walk to capture a permanent component and a moving average process to capture transitory effects, as in

$$\epsilon_{jt} = \zeta_{jt} + \tilde{v}_{jt}$$

$$\zeta_{jt} = \zeta_{j,t-1} + \tilde{u}_{jt}$$

where we assume the $\tilde{u}_{jt}$ and $\tilde{v}_{jt}$ are serially and mutually uncorrelated. Together these imply a relatively simple permanent-transitory distinction for the idiosyncratic shocks $\Delta\epsilon_{jt}$,

$$\Delta\epsilon_{jt} = \tilde{u}_{jt} + \Delta\tilde{v}_{jt}$$

with $\tilde{u}_{jt}$ giving long-lived innovations and $\Delta\tilde{v}_{jt}$ giving short-lived innovations. One cannot separately derive these two components from $\Delta\varepsilon_{jt}$, but we ultimately hope to gauge their relative impacts on worker earnings. Guiso et al. (2005) tackle this problem (in a more complex setting) by deriving instruments for the permanent and temporary components of $\Delta\varepsilon_{jt}$. We hope to pursue that approach, but currently we adopt a simpler expedient of looking for effects over time horizons of differing lengths. Contrast the two changes

$$\varepsilon_{jt} - \varepsilon_{j,t-1} = \tilde{u}_{jt} + (\tilde{v}_{jt} - \tilde{v}_{j,t-1})$$

and

$$\varepsilon_{jt} - \varepsilon_{j,t-3} = \tilde{u}_{jt} + \tilde{u}_{j,t-1} + \tilde{u}_{j,t-2} + (\tilde{v}_{jt} - \tilde{v}_{j,t-3}).$$

The permanent shocks presumably compose a greater proportion of longer horizon changes. Therefore, looking at changes over various horizons might speak to a temporary/permanent distinction. We note that measurement error is also less likely to be important for longer horizon changes.

Worker earnings

Log earnings for worker *i* are presumed to depend on time-varying observable factors, permanent and temporary firm-side shocks, worker fixed effects h_i, and a shock ψ_{ijt},

$$lnw_{ijt} = X_{ijt}\delta + \alpha P_{jt} + \beta T_{jt} + h_i + \psi_{ijt}$$

Here the P_{jt} and T_{jt} reflect the idiosyncratic permanent and temporary firm outcomes, which can differently affect log earnings. First differencing eliminates the worker fixed effects,

$$\Delta lnw_{ijt} = \Delta X_{ijt}\delta + \Delta\omega_{ijt}$$

where the composite error term $\Delta\omega_{ijt}$ includes terms related to the permanent and temporary firm shocks and the idiosyncratic wage shock ψ_{ijt},

$$\Delta\omega_{ijt} = \alpha\tilde{u}_{jt} + \beta\Delta\tilde{v}_{jt} + \Delta\psi_{ijt}$$

where $\Delta\epsilon_{jt} = \tilde{u}_{jt} + \Delta\tilde{v}_{jt} = (\Delta P_{jt} + \Delta T_{jt})$. If α=β, there is no temporary-permanent difference in wage effects and the composite error term above takes the form

$$\Delta\omega_{ijt} = \alpha\,\Delta\epsilon_{jt} + e_{ijt}.$$

If there are temporary/permanent differences then they are not separately identified but looking at changes across different horizons may help establish that such differences exist.

VI. Results for Changes in Worker Earnings

As described above, our basic framework for analyzing the effect of firm shocks on worker earnings growth is a simple set of regressions of the form

$$\Delta lnw_{ijt} = \Delta X_{ijt}\delta + \alpha\Delta R_{jt} + \Delta\varphi_{ijt}$$

where j indexes firms, i indexes workers, and t indexes time. The dependent variable is the change in log annual worker earnings and the independent variable of interest is ΔR_{jt}, the change in log firm revenues. Taking differences nets out worker-firm fixed effects. Other controls include worker age and gender, industry effects, and time effects.

One of our primary goals is to estimate the effects of interest for different kinds of workers and firms. We do this by estimating this model with a variety of specifications and on different subsets of the data. We view the resulting estimates as a set of summary statistics describing how firm-side shocks correlate with worker earnings growth.

The specifications we choose vary along several important dimensions. One dimension is the length of the changes incorporated. In particular, we examine 1-, 3- and 5-year changes. Varying this window is motivated in part as a way to reduce attenuation bias due to measurement error in ΔR_{jt}, and in part as a way to allow for differential effects of temporary and permanent shocks. For some longer-change specifications we also instrument the change ΔR_{jt} with a non-overlapping shorter change, as an alternative way to gauge potential measurement error effects. For example, a longer run change $(R_{j,t+k} - R_{j,t-k})$ can be instrumented with $(R_{j,t+k-1} - R_{j,t-k+1})$, provided measurement error is very transitory. In that case, the instrumentation captures much of the real growth and knocks out the transitory measurement error. In order to compare longer-run and shorter-run changes on similar data sets, we restrict our sample in all models to include only firms and workers who remain in sample long enough to be included in the longer-term changes. This means that our samples in these models are more restrictive than the 10-quarter stayer requirement discussed above when describing the volatility of earnings. We also generate estimates of α by quintiles of the earnings distribution, for positive versus negative firm shocks, by gender, by industry, and for different measures of firm ΔR_{jt}. We pursue these different dimensions of the estimates because some theories motivating the analysis may be more applicable for some parts of the populations of firms and workers than they are for others. For example, theories emphasizing variable pay as a means to induce greater effort might apply more for high earners within the firm, or in certain industries such as finance.

Baseline results for retail trade

Table 3 gives our first set of results, which is based on the ARTS sample data along with LEHD data on employees of sample firms. We use retail trade as a jumping off point primarily because it is the simplest data to construct, so we have our most complete set of results for this sector. The table shows estimates of α based on 1-, 3-, and 5-year changes from the same sample. Controls include 3-digit industry dummies, gender and single year of age dummies, and two-digit industry-by-year dummies. Standard errors are clustered at the level of the firm. The longer-run estimates are small but statistically significant at conventional levels, and coefficients are reasonably precisely measured. The differences across columns suggest that either transitory measurement error is an issue, or that long-run effects are larger than short-run effects. For example, the near-zero coefficient for one-year changes could reflect measurement issues or it could reflect a situation where firms shelter worker earnings against utterly transitory shocks.

Table 4 gives results for instrumental variables models where the change in firm revenues is instrumented with a shorter-run change. For the 3-year model this means instrumenting $(R_{j,t+1} - R_{j,t-2})$ with $(R_{jt} - R_{j,t-1})$. For the 5-year model this means instrumenting $(R_{j,t+2} - R_{j,t-3})$ with $(R_{j,t+1} - R_{j,t-2})$. The first stage results for both models show strong correlations between the independent variable and instrument. The IV estimates did not result in larger positive coefficients, which would be the expectation based on the simplest measurement error logic.

Tables 5 and 6 give estimates of the same models for men and women separately. As a general rule, we see larger point estimates in this industry for men than for women.[2] This is not unlike the results from Card, Cardoso and Kline (2013), although on a very different sample. As before, longer-run effects are larger, due to now larger IV estimates for the 5-year changes. Although not as consistently clear here, the measurement error logic still does not appear to fit with the difference between our OLS and IV sets of results.

While we find small effects overall, it is possible that the relevance of wage insurance and incentive pay motives for structuring pay differs across workers. Given what can be measured in our data, grouping workers by relative position in their firm's earnings distribution seems the most useful way to try to distinguish groups that might be more or less affected. To do this, we pool information on all workers (stayers or not) who work at least one full quarter for a firm in the sample. We then regress log quarterly earnings on dummies for single year of age and a full set of year*quarters to adjust for differences in the age and time periods in which earnings

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[2] In future drafts of this paper, we will add a test for significant differences here.

are observed. We take residuals from this regression and average them across all quarters that an individual is in sample. We use these average residuals to rank workers within each firm. While our analysis sample is based only on stayers, we assign sample workers to quintiles based on order statistics calculated including any non-stayers that worked one or more full quarters. Therefore our "quintiles" do not split the data into equal sample fractions, but they do preserve a reasonable ordering of our in-sample observations.

Table 7 shows the distribution of our sample across quintiles by sex. These shares do not equal 20 percent because the quintiles are not gender specific and because non-stayers are over-represented in the lowest quintile in particular. Tables 8 and 9 show regression results for quintiles 1, 3, and 5, for men and women respectively. We restrict attention here to the 3- and 5-year changes. The estimates here are more variable than those shown in previous tables, so any differences in point estimates should be interpreted more cautiously. But generally speaking, there are not obvious patterns by earnings position. Perhaps the largest difference by quintile is in the 3-year IV changes, for women, where the sample of quintile=1 workers have a larger coefficient (.035) than the quintile=3 or 5 groups (.002 and -.009, respectively). But even here, the differences across quintiles among the 3-year OLS models are relatively minor. We have also estimated our models using only workers with average residuals that put them in the top five percent of their firm's residual distribution. Estimates for this group of high earners tend to be slightly larger than for more general groups, but the differences are not dramatic.

Table 10 presents a final set of estimates that allows the effects of firm sales increases and decreases to differ. We implement this by adding an interaction term ΔR_{jt}*($\Delta R_{jt} \geq 0$) which turns on for non-negative log sales growth, along with an indicator variable for $\Delta R_{jt} \geq 0$ to allow for a level shift. We show OLS models only and, because there are no discernible effects for the one-year change models, present results only for the 3- and 5-year changes. Although patterns differ for the 3- and 5-year changes, and the estimates are not very precisely measured, one commonality is that wage growth does not correlate strongly with positive sales increases. We interpret this to mean that, at least in the retail trade sector, the wage responsiveness to firm-side shocks occurs most strongly when the shock is negative. One caveat is that we cannot separately identify hours variation in our data. It is likely—particularly in the retail sector where part-time employment is common—that firms cut hours for stayers in the case of negative demand shocks but hire additional workers in the case of positive demand shocks.

VII. Conclusions and Future Work

U.S. evidence on the extent to which changing firm conditions affect the earnings of continuing worker earnings is scant, largely because data to address this question has only recently become available. In this paper, we use data on a sample of retail trade firms and their employees to examine this question. We find no evidence of such effects for one year changes in firm sales, but some evidence of small positive effects when looking at changes over 3 or 5 years. Differential findings for short-run and long-run changes may reflect different responses to temporary and permanent changes. We examine whether effects differ for workers at different points in a given firm's earnings distribution, but do not find evidence of systematic variation along this dimension.

Economists have proposed several different theories that have implications for the empirical relationships we document. Competitive forces in labor markets will work against correlations between wages and firm-side drivers, with little or no relationship expected where spot markets prevail. But our samples are based on larger firms, and on workers with substantial attachment to particular firms–situations in which we would expect career considerations to be quantitatively important. For these sorts of samples, it seems reasonable to suppose that theories such as implicit contracts and bargaining over rents are also relevant. We have gravitated toward a wage insurance view but we admit that other views are possible.

We plan to extend this analysis along several lines in future drafts of this paper. We are currently focused on estimating these effects for a broader range of industries (adding manufacturing and several services industries) and determining how best to use data on gross margins for retail (where negative values are an issue).   Finally, we think it would be worthwhile to pursue methods of distinguishing between the effects of short-run and long-run changes, for example in the vein of Guiso et al. (2005), as our preliminary findings suggest the distinction may be important.

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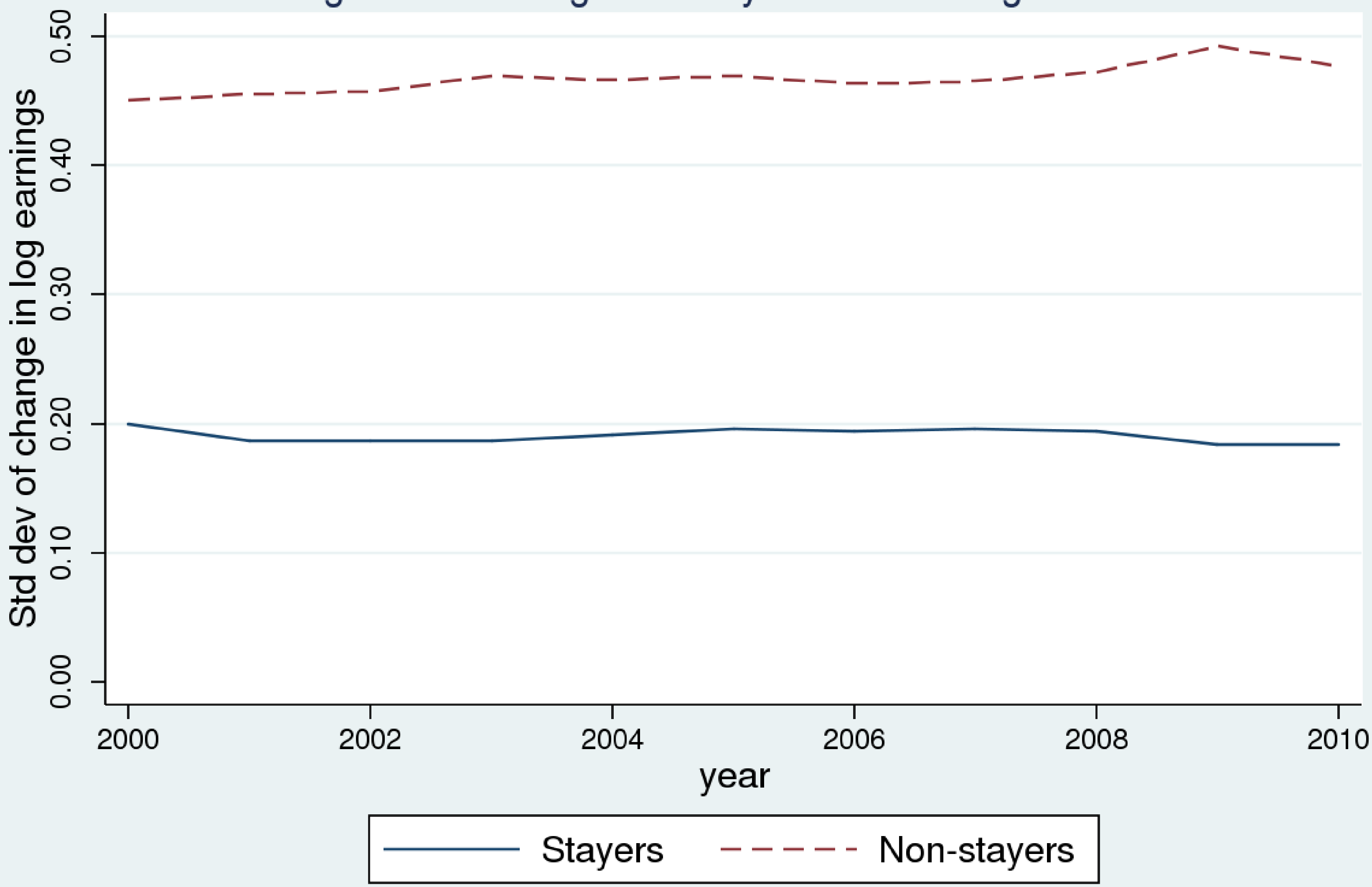
Table 1. Earnings volatility of stayers and non-stayers, by age group

	Stayers	Non-stayers, any earnings			Non-stayers, full-quarter earnings		
Age group	standard deviation of log earnings	standard deviation of log earnings	fraction stayer	stayer share of volatility	standard deviation of log earnings	fraction stayer	stayer share of volatility
25-34	.191	1.10	.394	.019	.467	.486	.137
35-59	.174	1.06	.539	.031	.456	.620	.193

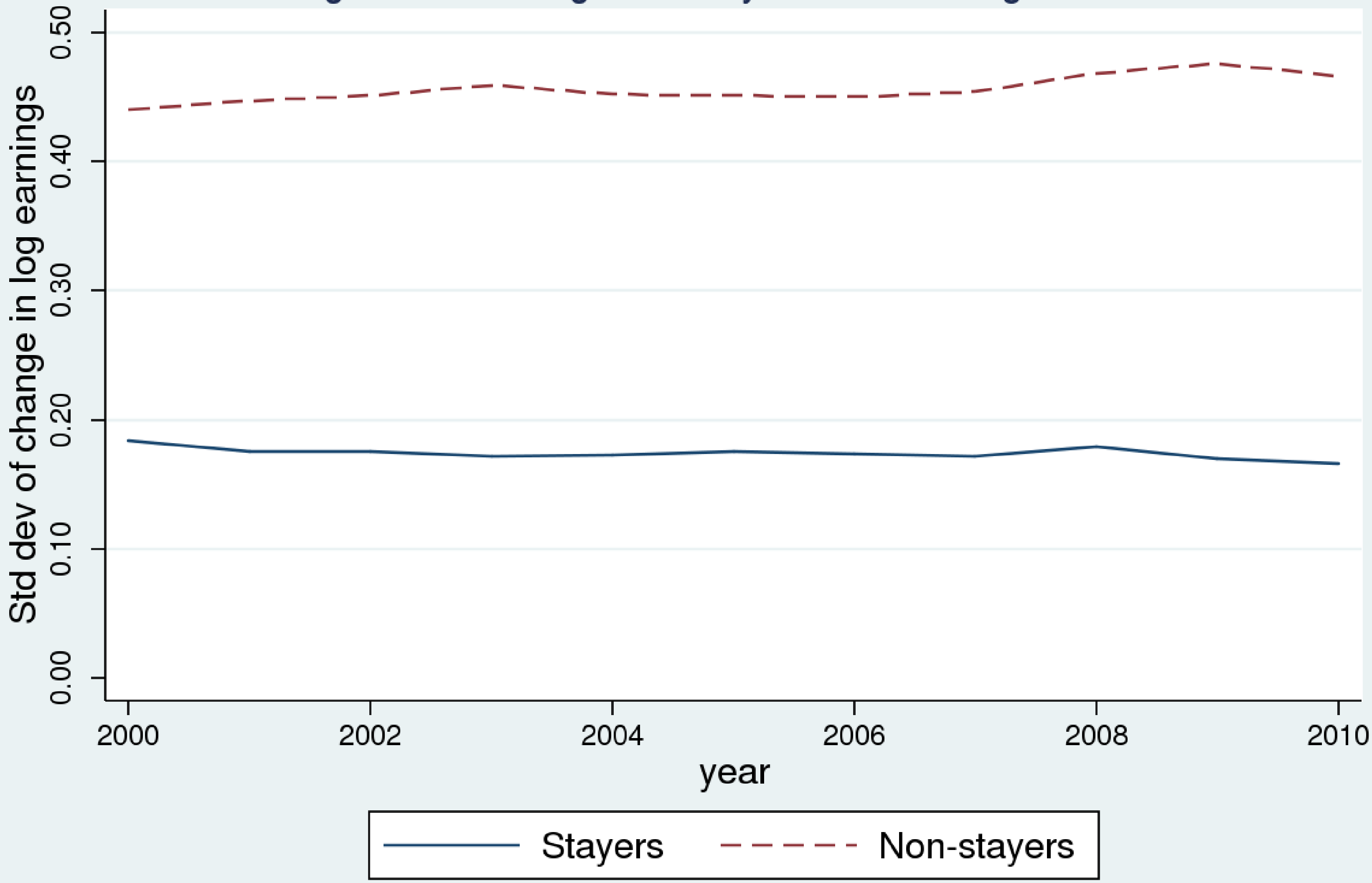
Notes. Columns labeled "Non-stayers, any earnings" are based on samples keeping workers with earnings in both periods t and t+1. Columns labeled "Non-stayers, full-quarter earnings" are based on samples keeping workers with full-quarter earnings in both periods t and t+1, and calculate standard deviations using quarterly averages of (full quarter) earnings. The full quarter definition for non-stayers is meant to abstract from quarters with zero or partial earnings.

Figure 1: Earnings volatility for workers aged 25-34



Based on 1% random sample of workers.

Figure 2: Earnings volatility for workers aged 35-59



Based on 1% random sample of workers.

Figure 3. Fraction stayers, by age group

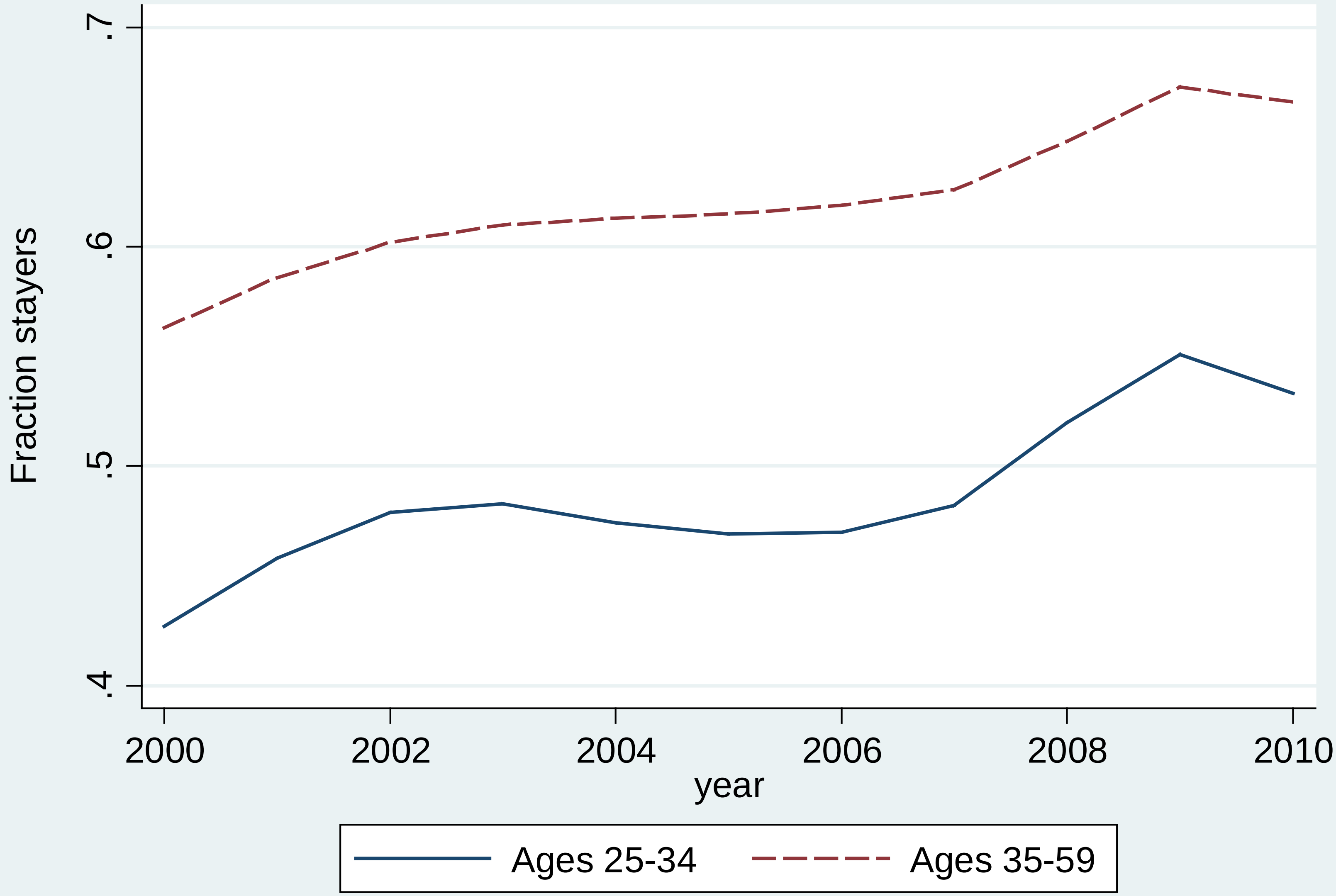


Table 2. Summary Statistics, Retail Trade 1999-2011

	Cross-section	2-year sample	6-year sample
Observation counts			
Person years	17,438,800	5,430,900	485,700
Unique persons	6,434,300	2,016,100	342,500
Firm years	201,300	105,200	14,200
Unique firms	48,000	29,200	12,300
Worker-weighted averages			
Firm sales (M$2012)	$122	$138	$109
Change in log sales		1.91%	6.97%
Annual earnings	$51,817	$68,720	$73,000
Average quarterly earnings	$13,759	$17,180	$18,250
Change in log annual earnings		2.25%	2.61%
Share female	45%	44%	46%
Worker age	41	43	44

Notes. The cross-sectional firm sample includes all firms in the retail trade sample with positive sales in current year, and with a link to employees in LEHD data in current year. The cross-sectional sample of individuals includes all individuals aged 25-59 with at least 1 full-quarter of earnings in the current year who are employed by a firm that is in the retail trade sample at some point in 1999-2012. The 2-year sample differs from the cross-section sample by requiring 10 quarters of continuous employment with the same firm. The 6-year sample requires that 10-quarter requirement to be met both in the current year t and year t+5. For the 2-year and 6-year samples the person-year and firm-year counts refer to observational counts in difference regressions on the relevant samples. All counts are rounded to the nearest 100. All measures relating to earnings and sales in this table refer to worker-weighted averages.

Table 3. OLS Models for Changes in Log Worker Earnings – Retail Trade

	(1) One-year change	(2) Three-year change	(3) Five-year change
One-year change in log revenues	0.004		
	(0.003)		
Three-year change in log revenues		0.010***	
		(0.003)	
Five-year change in log revenues			0.013**
			(0.004)
RMSE	0.159	0.247	0.298

Notes: Results are based on workers aged 25-59 who stayed with the same employer for 6 years. Samples are consistent across the specifications. Column (1) regresses 1-year change in worker log earnings on 1-year change in firm log revenues. Column (2) regresses 3-year change in worker log earnings on 3-year change in firm log revenues. Column (3) regresses 5-year change in worker log earnings on 5-year change in firm log revenues. Standard errors in parentheses. Additional controls include age and gender dummies, 3 digit industry dummies, 2 digit industry X year dummies. Standard errors are clustered at the firm level. .
** $p<0.01$ *** $p<0.001$

Table 4. OLS vs IV Models for Changes in Log Worker Earnings - Retail Trade

	(1) OLS Three-year change	(2) IV Three-year change	(3) First stage Three-year change	(4) OLS Five-year change	(5) IV Five-year change	(6) First stage Five-year change
3-year change in log revenues	0.010***	-0.005				
	(0.003)	(0.017)				
1-year change in log revenues			0.658***			
			(0.037)			
5-year change in log revenues				0.013**	0.014**	
				(0.004)	(0.005)	
3-year change in log revenues						0.709***
						(0.049)
RMSE	0.247	0.247	0.670	0.298	0.298	0.644
R2	0.045	0.043	0.671	0.045	0.045	0.802

Notes: Results are based on workers aged 25-59 who stayed with the same employer for 6 years. Samples are consistent across the specifications. Columns (1) and (4) regress 3-year change (5-year change) in worker log earnings on 3-year change (5-year change) in firm log revenues. Columns (2) and (5) regress 3-year change (5-year change) in worker log earnings on with 3-year change (5-year change) in firm log revenues using 1-year change (3-year change) in firm log revenues as instrument. Columns (3) and (6) report the first stage. Standard errors in parentheses. Additional controls include age and gender dummies, 3 digit industry dummies, 2 digit industry X year dummies. Standard errors are clustered at the firm level. ** $p<0.01$ *** $p<0.001$

Table 5. OLS Models for Changes in Log Worker Earnings, by Gender

A. Men	(1) One-year change	(2) Three-year change	(3) Five-year change
One-year change in log revenues	0.009		
	(0.005)		
Three-year change in log revenues		0.037***	
		(0.008)	
Five-year change in log revenues			0.027***
			(0.005)
RMSE	0.152	0.231	0.279
R2	.028	.070	.070
B. Women			
One-year change in log revenues	0.002		
	(0.003)		
Three-year change in log revenues		0.024***	
		(0.006)	
Five-year change in log revenues			0.018***
			(0.004)
RMSE	0.166	0.262	0.315
R2	.010	.035	.029

Notes: Results are based on workers aged 25-59 who stayed with the same employer for 6 years. Samples are consistent across the specifications. Column (1) regresses 1-year change in worker log earnings on 1-year change in firm log revenues. Column (2) regresses 3-year change in worker log earnings on 3-year change in firm log revenues. Column (3) regresses 5-year change in worker log earnings on 5-year change in firm log revenues. Standard errors in parentheses. Additional controls include age and gender dummies, 3 digit industry dummies, 2 digit industry X year dummies. Standard errors are clustered at the firm level. ** p<0.01 *** p<0.001

Table 6. OLS vs IV Models for Changes in Log Worker Earnings, by Gender

	(1) OLS Three-year change	(2) IV Three-year change	(3) First stage Three-year change	(4) OLS Five-year change	(5) IV Five-year change	(6) First stage Five-year change
A. Men						
3-year change in log revenues	0.037*** (0.008)	0.025 (0.014)				
1-year change in log revenues			0.741*** (0.046)			
5-year change in log revenues				0.027*** (0.005)	0.041*** (0.009)	
3-year change in log revenues						0.852*** (0.029)
RMSE	0.231	0.231	0.410	0.279	0.280	0.435
R2	.070	.070	.754	.070	.069	.819
B. Women						
3-year change in log revenues	0.024*** (0.006)	0.006 (0.010)				
1-year change in log revenues			0.758*** (0.049)			
5-year change in log revenues				0.018*** (0.004)	0.028*** (0.007)	
3-year change in log revenues						0.857*** (0.037)
RMSE	0.262	0.262	0.415	0.315	0.315	0.456
R2	.035	.034	.915	.029	.029	.931

Notes: Results are based on workers aged 25-59 who stayed with the same employer for 6 years. Samples are consistent across the specifications. Standard errors in parentheses. Additional controls include age and gender dummies, 4 digit industry dummies, 2 digit industry X year dummies. Standard errors are clustered at the firm level. ** p<0.01 *** p<0.001

Table 7. Distribution across earnings quintiles for regression sample, by gender

Quintile	Women	Men
1	0.180	0.091
2	0.227	0.147
3	0.221	0.202
4	0.203	0.249
5	0.169	0.311

Notes. Quintiles are based on distribution of average quarterly earnings for all workers with at least 1 full quarter of earnings with one of the firms in our sample. We pool information on all workers with at least one full quarter of earnings; regress log quarterly earnings on dummies for single year of age and a full set of year*quarters to adjust for differences in the age and time periods in which earnings are observed. We take residuals from this regression, average them across all quarters that an individual is in sample; and rank workers within firm based on their average residuals. While our analysis sample is based only on stayers, we assign sample workers to quintiles based on order statistics calculated including any non-stayers that worked one or more full quarters. Therefore our "quintiles" do not split the data into equal sample fractions, but they do preserve a reasonable ordering of our in-sample observations.

Table 8. Models for Changes in Log Worker Earnings, by Wage Position for Men, in Retail Trade

	Three-year changes		Five-year changes	
	(1)	(2)	(3)	(4)
	OLS	IV	OLS	IV
A. Quintile 1				
change in log revenues	0.032*	0.037	0.017	0.026
	(0.014)	(0.022)	(0.011)	(0.018)
RMSE	0.376	0.375	0.442	0.440
r2	0.039	0.035	0.034	0.032
B. Quintile 3				
change in log revenues	0.039***	0.022	0.028***	0.048***
	(0.008)	(0.018)	(0.005)	(0.010)
RMSE	0.198	0.198	0.238	0.238
r2	0.082	0.079	0.079	0.076
C. Quintile 5				
change in log revenues	0.039***	0.024	0.030***	0.030**
	(0.011)	(0.016)	(0.007)	(0.011)
RMSE	0.247	0.247	0.296	0.296
R2	0.088	0.086	0.103	0.102

Notes: Results are based on workers aged 25-59 who stayed with the same employer for 6 years. Samples are consistent across the specifications. Columns (1) and (3) regress 3-year change (5-year change) in worker log earnings on 3-year change (5-year change) in firm log revenues. Columns (2) and (4) regress 3-year change (5-year change) in worker log earnings on the 3-year change (5-year change) in firm log revenues using 1-year change (3-year change) in firm log revenues as an instrument. Standard errors in parentheses. Additional controls include age and gender dummies, 4 digit industry dummies, 2 digit industry X year dummies. Standard errors are clustered at the firm level. ** $p<0.01$ *** $p<0.001$

Table 9. Models for Changes in Log Worker Earnings, by Wage Position for Women, in Retail Trade

	Three-year changes		Five-year changes	
	(1)	(2)	(3)	(4)
	OLS	IV	OLS	IV
A. Quintile 1				
change in log revenues	0.034***	0.035*	0.014	0.031*
	(0.008)	(0.014)	(0.010)	(0.013)
RMSE	0.432	0.431	0.520	0.518
r2	0.026	0.024	0.024	0.022
B. Quintile 3				
change in log revenues	0.022***	0.002	0.019***	0.028***
	(0.006)	(0.012)	(0.004)	(0.008)
RMSE	0.233	0.233	0.280	0.280
R2	0.041	0.037	0.034	0.032
C. Quintile 5				
change in log revenues	0.025**	-0.009	0.019**	0.013
	(0.009)	(0.010)	(0.007)	(0.009)
RMSE	0.222	0.222	0.266	0.266
R2	0.101	0.096	0.082	0.080

Notes: Results are based on workers aged 25-59 who stayed with the same employer for 6 years. Samples are consistent across the specifications. Columns (1) and (3) regress 3-year change (5-year change) in worker log earnings on 3-year change (5-year change) in firm log revenues. Columns (2) and (4) regress 3-year change (5-year change) in worker log earnings on the 3-year change (5-year change) in firm log revenues using 1-year change (3-year change) in firm log revenues as an instrument. Standard errors in parentheses. Additional controls include age and gender dummies, 4 digit industry dummies, 2 digit industry X year dummies. Standard errors are clustered at the firm level. ** $p<0.01$ *** $p<0.001$

Table 10. Allowing Effects to Differ for Positive and Negative Changes

	Men		Women	
	(1) Three-year change	(2) Five-year change	(3) Three-year change	(4) Five-year change
Indicator for positive 3-year revenue growth	0.027		0.005	
	(0.016)		(0.014)	
3-year change in log revenues	0.047*		0.059**	
	(0.021)		(0.021)	
Interaction between 3-year change log revenues and positive indicator	-0.045*		-0.052*	
	(0.021)		(0.020)	
Indicator for positive 5-year revenue growth		0.051***		0.041***
		(0.012)		(0.010)
5-year change in log revenues		0.006		0.012
		(0.010)		(0.009)
Interaction between 5-year change log revenues and positive indicator		0.004		-0.009
		(0.012)		(0.012)
RMSE	0.231	0.279	0.262	0.315
R2	0.073	0.073	0.036	0.031

Notes. Models are OLS regressions on 3- and 5-year changes. Standard errors in parentheses cluster on firm. Asterisks ** indicates p<0.01; *** indicates p<0.001.