

Firm Performance and the Volatility of Worker Earnings

Chinhui Juhn
University of Houston and NBER

Kristin McCue
U.S. Census Bureau

Holly Monti
Far Harbor, LLC

Brooks Pierce
U.S. Bureau of Labor Statistics

April 2017

Using linked employer-employee data for the U.S., we examine whether shocks to firm revenues are transmitted to the earnings of continuing employees. While full insurance is rejected, the elasticity of worker earnings with respect to persistent shocks in firm revenues is small and consistent with the notion that firms insulate workers from idiosyncratic shocks. Exploring heterogeneity of effects, we find the largest elasticity in professional services, among employees in the top 5% of their employers' earnings distribution, suggesting that in certain jobs performance pay may be a countervailing force to wage insurance.

Acknowledgements: We would like to thank Luigi Pistaferri, Edward Lazear, and Henry Hyatt for helpful comments on an earlier version.

Disclaimer: Any opinions and conclusions expressed herein are those of the authors and do not necessarily represent the views of the U.S. Census Bureau or the U.S. Bureau of Labor Statistics. Juhn (corresponding author): cjuhn@uh.edu; McCue: kristin.mccue@census.gov; Monti: hollymonti@gmail.com; Pierce: pierce.brooks@bls.gov.

All results have been reviewed to ensure that no confidential information is disclosed.

I. Introduction

What role do firms play in generating earnings volatility for workers? Earnings volatility—within-worker fluctuations in earnings from year to year—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 administrative data.¹

If not fully anticipated, earnings volatility 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 shift in retirement benefits 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 fluctuations in demand faced by employers are an important source of worker earnings volatility. 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

¹ See Gathright and Monti (2013), Celik, Juhn, McCue, and Thompson (2012), Dahl, DeLeire, and Schwabish (2007), Sabelhaus and Song (2010), DeBacker, Heim, Panousi, Ramnath, and Vidangos (2013), Guvenen, Ozcan, and Song (2014), among others.

perspective of risk, as entrepreneurs or stockholders are likely to have better access to capital markets than workers, and to have more expertise in diversifying risk. According to Baily (1974), firms offer workers a joint product: employment and insurance. Guiso et al. (2005) test for wage insurance using matched employer-employee data from Italy and find that firms insure workers against temporary shocks but not permanent shocks. Other papers have followed using employer-employee data from other European countries and found similar results.²

In contrast, employers might favor variable pay as a way 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 so 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. Firms may also vary pay with firm or group-level performance based on different reasoning. In the CEO pay literature, tying CEO compensation to firm performance will help solve the principal-agent problem. Lazear (1999) and Oyer and Schaefer (2005) argue that tying compensation to firm performance may help the firm sort workers who are more optimistic about the firm's prospects or have private information about the value of the firm. Whether incentives or sorting motivate firm compensation practices, these arguments suggest that variable pay is

²The list of papers include Fagereng et al. (2016), Cardoso and Portela (2009), Katay (2009), Guertzgen (2014), and Le Barbanchon and Tarasonis (2011). These papers generally find no transmission of temporary shocks and a partial transmission of permanent shocks which range from 0.03 to 0.10.

likely to be more prevalent for managers and other workers who have a more direct impact on firm performance.

The literature on wage insurance is also closely related to the rent-sharing literature, which explores the extent to which equally qualified workers are paid higher wages in firms that command economic rents.³ More recently available employer-employee data linking individual workers to firms has allowed for estimation of rent sharing parameters that control for worker fixed effects. This approach essentially uses evidence on stayers and hence avoids difficulties associated with highly able workers sorting into higher productivity firms. Card, Cardoso and Kline (2016) use linked employer-employee data from Portugal to estimate responses of worker earnings to changes in firms' value added. They find a small positive coefficient relating three-year changes in firm value added to three-year changes in worker earnings. They interpret the pass-through estimate mainly in terms of rent sharing. While motivated by different theories, empirically, the insurance story is the opposite of the rent-sharing story. A low elasticity suggests more insurance and risk-sharing between workers and firms. A higher elasticity suggests less risk-sharing and more rent-sharing. Card, Cardoso, Heining and Kline (2016) provides a survey of studies that relate wages to measures of firm performance. They find that parameters estimated from job stayers are smaller than parameters estimated using alternative estimation strategies. Wage insurance for workers who remain with the firm may be one reason for this gap in the two types of estimates.

³ See, for example, Hildreth and Oswald (1997), Blanchflower, Oswald and Sanfey (1996), Abowd and Lemieux (1993), and Van Reenen (1996).

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. Previous papers examining wage insurance have examined evidence from European countries with high levels of unionization and collective wage bargaining agreements. With a centralized wage bargaining process, firms may not be able to adjust worker wages to idiosyncratic firm-level shocks. These factors are less of an issue in the U.S., so U.S. data may provide more fertile ground for testing wage insurance models.⁴

Our second contribution is to explore important aspects of heterogeneity—by nature of the shock and by worker and firm characteristics. We examine whether the extent of wage insurance varies by the size of the shock and whether the impact of positive and negative shocks differ. The performance pay literature suggests that the trade-off between insurance and incentives should be most stark for employees who have a large direct impact on firm performance. We test whether this prediction is borne out by comparing effects for highly paid (top 5%) versus the low-paid (bottom 20%) workers in the firm. Finally we explore heterogeneity by worker tenure. While wage insurance predicts little or no transmission of firm-level shocks to worker earnings, a competing explanation is that labor markets are competitive and firms have little leeway in setting wages. This is more likely to be true for just-hired workers who have no accumulated firm-specific human capital. The just-hired provide a counterfactual for pay adjustments in a more competitive setting. To the extent that the coefficients differ

⁴ It may also be that lower firing costs in the U.S. induce firms to lay-off workers more readily so that focusing on wage insurance provided to stayers may provide an incomplete picture of the overall insurance provided to workers by firms.

substantially across these worker types, we may conclude that mobility of workers across firms is more important than any considerations of insurance.

Our findings show that the OLS coefficient on short term (1-year) changes in firm revenues on worker earnings is close to zero, ranging from 0.007 for manufacturing and 0.012 and 0.015 for retail and professional services respectively. We find the coefficient to be -0.003 and insignificant in finance. Estimates are generally larger for 3-year and 5-year changes which may be due to either reduced measurement error or isolating more persistent shocks. IV estimates, which are an alternative method for correcting for measurement error and isolating permanent shocks, produce small and positive coefficients which range from 0.02 in manufacturing to approximately 0.04 in professional services and retail, although the range of estimates is larger in those industries. While full insurance is rejected, the elasticity is small and consistent with the notion that firms insulate workers from idiosyncratic shocks. We find that the elasticity varies by sector and also by relative earnings rank of the worker in the firm. We find the largest coefficient—0.09—among highly paid workers (the top 5%) in professional services. For lower paid workers in professional services (those in the first quintile), the coefficient is close to zero.

II. Related Literature

Guiso et al. (2005) use matched employer-employee data from Italy to test for wage insurance. Other papers have followed their methods using matched employer-employee data from other European countries. However, we are not aware of other papers which have used matched employer-employee data for the U.S. with linked information on firm performance such

as revenues or value-added to examine this question. While our paper is the first that we know of to conduct this exercise, other papers examining the U.S. are nevertheless closely related to our paper. 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 positive over time which they attribute to a shift in the composition of jobs towards those with more bonus pay. They also investigate variation across industries and find that the relationship is stronger in services than in manufacturing. Lagakos and Ordonez (2011) use industry level time series data to show that there is less wage smoothing in industries with lower average schooling levels. They interpret this as suggesting that low-skilled workers have less wage insurance from employers. One drawback to these studies is the use of average firm wages, for which variation could be driven by changes in worker composition. Using matched employer-employee data bypasses this difficulty by measuring earnings 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 affected 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.

Barth et al. (2016) use the U.S. data from the Census's Longitudinal Business Database and economic censuses to estimate the relationship between establishment average payroll per employee and revenues per worker. Since they use establishment average earnings rather than changes in individual-level earnings, they find a somewhat larger positive coefficient relating firm revenues to worker earnings. As with Card, Cardoso, and Kline (2016), the authors interpret the coefficient as a rent-sharing parameter. Nevertheless, the authors conclude that the rent-sharing model accounts for only a small portion of the rise in earnings inequality between establishments.

III. Data

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

Firm data

The results presented here are based on data from three Census annual business surveys that collect information on revenues for a particular industry or industry group:

- The Annual Retail Trade Survey (ARTS) 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).

- The Annual Survey of Manufactures (ASM) collects information from employers in a wide set of manufacturing industries (NAICS 31-33).

- The Services Annual Survey (SAS) collects data from selected industries in other sectors.

The coverage of the SAS has expanded over time, so while it now covers a large portion of the services sector broadly defined, many of the currently included industries have relatively short time series. Rather than pooling all of the available data, we have produced estimates for selected industries, and only present two of them here: what we will refer to as finance (though it is based on a subset of industries in that sector)⁵ and professional services.⁶ We included finance based on the hypothesis that compensation practices in that industry would be more influenced by pay for performance and less influenced by wage insurance than most. Professional services was selected because it has substantial employment, and because we found a nontrivial relationship between worker earnings growth and firm revenue growth for workers in that industry.⁷

For each of the survey panels, a new sample is drawn every 5 years, once new information from the quinquennial Economic Census is incorporated into the sampling frame. Over the span of years in our data, new samples for ARTS and SAS started in 2001, 2006, and 2011, while new ASM panels began in 1999, 2004, and 2009. While new firms appear in the panel primarily in those years, large firms may be selected again in subsequent panels and so

⁵ The sample we use is made up of firms from NAICS 5231 (securities and commodity contracts intermediation and brokerage) and 5239 (Other financial activities).

⁶This samples from NAICS 541 (Professional, scientific, and technical services). A few examples of detailed industries in this sector are legal services, architectural and engineering services, and computer system design.

⁷The other SAS industries that we produced estimates for but do not include were NAICS 621 (Ambulatory Health Care), 622 (Hospitals), 623 (Nursing and Residential Care), and 811 (Repair and Maintenance). Results based on data from those industries generally showed small and often insignificant coefficients.

remain in our data set for more than five years.⁸ The ARTS and SAS surveys sample firms, or in some cases the part of a firm that operates in an in-sample industry. The ASM differs from the other surveys in that it samples establishments (individual locations) rather than firms.

We use data from these annual business surveys primarily to measure changes in firm revenues. While some firms in the ARTS and SAS report sales separately for different parts of their organization or broken down by detailed industry, we use total sales reported for the firm within a three-digit NAICS industry as our measure of firm outcomes. In manufacturing, where we have information on revenues by location, we sum revenues up to the firm/state level, which is the most detailed level at which we can combine data on firms and their employees.

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. Earnings covered by UI reporting include wage and salary income, commissions, tips, bonuses, and other cash payments. The various forms of earnings are not separately identified. UI earnings do not include employer payments for benefits such as health insurance or retirement savings plans.

⁸We also use prior year revenue information, where available, to provide a 6th year of data.

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 business samples, as detailed in the next section. To provide some context for the variation captured in our primary analysis, 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 (or, in some cases, industry segments of firms) in the business samples that have non-missing and non-zero sales measures. Firms with multiple locations (“multi-unit”) firms may file information using more than one federal Employer Identification Number (EIN), so we pull all EINs used by the firm from Census’s Business Register (BR).⁹ The UI employer records also include EINs, so we then use the set of EINs from the business surveys to select UI employer records for sampled firms. Firms do not always use the same tax identifiers in filing federal payroll taxes (the primary source of EINs for the BR) and state UI taxes (the source of EINs for the LEHD files). As a consequence, it is likely that our sample of employees is incomplete for some of the firms included in our analysis. We find EIN match rates of 84-90 percent for the industries we use.

⁹ The Business Register (BR) contains information on essentially all businesses with employees who are subject to federal withholding. It serves as the sampling frame for all three surveys we use here.

In comparing our linked sample to economic census statistics for 2012, we find that the average firm in the industries sampled for the ARTS survey (NAICS 44/45 and 72) had \$4.3M in revenues, while the employment-weighted average was \$22,155M. This compares to linked-worker weighted average in our ARTS sample of \$119M. For professional services, those statistics are \$1.9M (firm mean), \$1,189M (worker-weighted mean), and \$1,687M (worker-weighted mean, our sample). For manufacturing: \$22M (firm mean), \$5,187M (worker-weighted mean), and \$788M (worker-weighted mean, our sample). For finance, they are \$9M (firm mean), \$6,154M (worker-weighted mean) and \$6,165M (worker-weighted mean, our sample). These statistics suggest that our sample is primarily made up of larger firms, making our results more representative from the perspective of a typical worker than from the perspective of a typical firm. At the same time, we have lower match rates for workers in large firms with complex structures, particularly in retail and in manufacturing, so that on a worker-weighted basis, our sample is smaller than the population of firms in these industries.¹⁰

Stayer samples

Once we have a list of UI employer identifiers, we select all employees aged 25-59 who are employed by a sample firm in at least one year during the period that the firm is in sample, and having at least one “full-quarter” of employment in that year. In tables below, we refer to

¹⁰ In Appendix Table 1, we investigate the robustness of our results with respect to size by examining separately firms that are below/above median in terms of revenues. The coefficient is somewhat larger in manufacturing for smaller firms while it is somewhat larger in retail for larger firms. Given that our worker-weighted sample firm size is somewhat smaller than the worker-weighted population firm size, this suggests that we may be overstating the pass-through in manufacturing and understating the pass-through in retail.

this set of workers as our cross-sectional sample. As is standard in using the LEHD earnings data, we treat quarter t as a full quarter if that worker has earnings reported in quarters $t-1$, t , and $t+1$ at that employer. 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. We use only full-quarter observations in measuring earnings. This restriction serves to eliminate variation in earnings due to the number of weeks worked, but note that we cannot eliminate variation due to changes in the hours individuals work, as we do not have data on worker hours. We do have establishment-level measures of hours in manufacturing, which we use in a robustness check discussed later in the paper.¹¹

Our regression analysis requires that we further restrict our sample of workers to those with two consecutive full years of earnings with a sample employer (“stayers”) so that we can measure the relationship between changes in worker annual earnings and the annual firm outcomes from the business data. In the least restrictive sample we use for our primary analysis, we define stayers in year t as those having positive 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 longer changes, we must further restrict our sample—for example, using those who are in the data for six adjacent calendar years when looking at five-year differences. Results presented here are

¹¹ We note, however, that we are not alone in not accounting for hours fluctuations. Several of the studies cited in Card, Cardoso, Heining, and Kline (2016) use annual or monthly earnings while others have daily or hourly wage measures. Guiso et al. (2005) delete part-time workers and those who worked less than 12 months but use annual earnings which include overtime pay. Their earnings measure therefore includes hours fluctuations along certain dimensions (work intensity among full-time full-year workers) but not along others.

primarily based on samples of jobs lasting long enough to use for either three-year or five-year changes, which we will refer to as our 3-year and 5-year samples (though they require 4 and 6 years of data, respectively).¹²

The length of the panel datasets we analyze depends on the availability of both LEHD and firm data. Our linked retail and manufacturing samples span years 1998-2011.¹³ For finance, our sample includes years 2002-2011, and for professional services it includes years 1992-2011. Because more states are included in the LEHD data in more recent years, our samples are larger for more recent years, and so our estimates weight recent years more heavily. We should also note that the fact that samples are refreshed every 5 years means that the 5-year samples are particularly large for intervals starting with the first year of data for a new panel.

Summary statistics

Table 1 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 linked to our sample of firms in that industry group with at least one full quarter of earnings. We include

¹² Note that it is possible for an individual to be employed by more than one sample employer in the same time period, but the restriction to stayers generally eliminates most such cases. In samples restricted to those who stay for at least one full year, 0.1 percent of individuals are in our sample more than once. Other papers in the wage insurance literature also use earnings changes among stayers which necessitates conditioning on workers who stay with the firm at a minimum two-years. Our 5-year sample is comparable to the sample in Guiso et al. (2005), who use lagged earnings values dated $t-4$ and earlier as instruments. Our 3-year sample is comparable to the stayer sample of Card, Cardoso, and Kline (2016), who focus on 3-year changes.

¹³ We have manufacturing data for all of the years with LEHD data, but have only used 1998 forward; including earlier data becomes computationally burdensome and limits comparability with other industry sectors.

these statistics to serve as a point of comparison for the more restrictive samples we use for our primary analysis. The age restriction and the requirement of one full quarter of earnings eliminates many transient and low earnings jobs. Annual average earnings for these cross-sectional samples range from \$51,300 in retail to over \$200,000 in finance.¹⁴ The ranking of pay by industries probably conforms with most readers' expectations, but the pay levels may seem surprisingly high. 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 in addition, these means are based on the full distribution of UI-covered earnings and are not topcoded, 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 revenues reflect the characteristics of larger firms.¹⁵

The rows labeled “1-year difference” further restrict the samples to observations on stayers at firms in-sample for adjacent match years.¹⁶ Restricting to stayers raises the average age by approximately 2 years, and raises average annual earnings substantially. Further restricting our samples to allow estimation based on longer-run changes of course reduces observation counts, and further raises average earnings in all sectors except finance. The 5-year samples are also more affected by the cycle of sample refreshment, which for our retail and finance samples

¹⁴Amounts are expressed in \$2012, and are rounded to the nearest 100.

¹⁵More precisely, they are weighted by matched employment for jobs that meet sample exclusion requirements, and so are informative only about the characteristics of our sample.

¹⁶Mean levels for the 1-year, 3-year, and 5-year change samples are respectively based on values for years 1, 2, and 3 of those intervals, so each in-sample change contributes a single observation to calculation of mean levels.

means that a large portion of the 5-year sample observations fall in the interval 2005-2010, and so reflect variation during the great recession.¹⁷

IV. Earnings Volatility of Stayers and Non-stayers

In estimating the effects of firm shocks on worker earnings in annual data, we end up with 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

¹⁷ The professional services sample is on the same sampling schedule, but covers a longer period of years.

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. Time series graphs of the volatility components are shown in Figure 1.

As shown in Table 2, 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 (columns 2-4). 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. In contrast, 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 (columns 5-7). While the relationship between firm shocks and job losses or job changes is clearly important, we leave it as the topic of future research. In this paper we focus on firm shock transmissions to stayers as an interesting topic in its own right.

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. We motivate this regression using a simplified version of Guiso et al. (2005), adapted to include measurement error in revenues.

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

$$R_{jt} = Z_{jt}\gamma + f_j + \epsilon_{jt} \quad (1)$$

The $Z_{jt}\gamma$ term captures time-varying observable factors, the f_j term captures unobserved firm fixed effects, and the error term captures other factors (including measurement error in revenues). First differencing eliminates the fixed effects,

$$\Delta R_{jt} = \Delta Z_{jt}\gamma + \Delta\epsilon_{jt} \quad (2)$$

Since our conceptual framework stresses wage insurance, we include industry, state and year controls in our first-differenced specifications. These capture any state- or industry-specific trends. The aim is to net out aggregate and industry-level shocks to isolate idiosyncratic shocks which are more likely to be insurable. For instance, industry and year controls help net out the effects of common input cost shocks.

Permanent versus temporary shocks

It is useful to specify permanent and temporary components to the innovations $\Delta\epsilon_{jt}$, and to distinguish true from measured values. True error shocks ϵ_{jt}^* incorporate a random walk to capture a permanent component and a moving average process to capture transitory effects,

$$\epsilon_{jt}^* = \zeta_{jt} + (1 - \theta L)v_{jt} \quad (3)$$

$$\zeta_{jt} = \zeta_{j,t-1} + u_{jt} \quad (4)$$

where L denotes the lag operator. Together these imply a permanent-transitory distinction for the true shocks $\Delta\epsilon_{jt}^*$,

$$\Delta\epsilon_{jt}^* = u_{jt} + (1 - \theta L)\Delta v_{jt} \quad (5)$$

Adding measurement error m_{jt} to the true ϵ_{jt}^* implies

$$\Delta\epsilon_{jt} = \Delta\epsilon_{jt}^* + \Delta m_{jt} \quad (6)$$

Unless noted, error components are uncorrelated with each other and are serially uncorrelated, and we denote variances in obvious ways, so that $\text{var}(m_{jt}) = \sigma_m^2$, $\text{var}(\Delta m_{jt}) = 2\sigma_m^2$, $\text{var}(u_{jt}) = \sigma_u^2$, and so forth.

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} ,

$$\ln w_{ijt} = X_{ijt}\delta + \alpha P_{jt} + \beta T_{jt} + h_i + \psi_{ijt} \quad (7)$$

Here the P_{jt} and T_{jt} reflect the idiosyncratic permanent and temporary firm outcomes, and are implicitly defined to difference down to the components of (5). First differencing eliminates the worker fixed effects,

$$\Delta \ln w_{ijt} = \Delta X_{ijt}\delta + \Delta \omega_{ijt} \quad (8)$$

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

$$\Delta \omega_{ijt} = \alpha u_{jt} + \beta(1 - \theta L)\Delta v_{jt} + \Delta \psi_{ijt} \quad (9)$$

Estimation

Estimating (9) directly is difficult since firm side revenue growth includes measurement error and both temporary and permanent innovations. Earnings growth does not depend on the measurement error part of firm revenue growth, but even with no measurement error we would have difficulty distinguishing the α and β responses as we do not directly observe the separate components of $\Delta \epsilon_{jt}^*$. We adopt two complementary approaches which we think offer sensible information, particularly about α . In this section we relate our proposed OLS and IV model estimates to the parameters in the stochastic setup above.

One approach involves estimating OLS regressions of wage growth on firm sales growth, for various lengths of change (we estimate models for 1-, 3-, and 5-year changes). The idea is that correlations among longer differences should more heavily reflect the impact of permanent shocks rather than transitory shocks or measurement error. Consider an OLS regression on 5-year changes from $(t-3)$ to $(t+2)$, an interval that spans the one-period changes above. The stochastic setup implies

$$\epsilon_{j,t+2} - \epsilon_{j,t-3} = \left(\sum_{s=-2}^2 u_{j,t+s} \right) + (1 - \theta L)(v_{j,t+2} - v_{j,t-3}) + (m_{j,t+2} - m_{j,t-3}) \quad (10)$$

$$\omega_{ij,t+2} - \omega_{ij,t-3} = \alpha \left(\sum_{s=-2}^2 u_{j,t+s} \right) + \beta(1 - \theta L)(v_{j,t+2} - v_{j,t-3}) + (\psi_{ij,t+2} - \psi_{ij,t-3}) \quad (11)$$

The OLS coefficient is a ratio of the relevant covariances

$$\text{cov}(\omega_{ij,t+2} - \omega_{ij,t-3}, \epsilon_{j,t+2} - \epsilon_{j,t-3}) = 5\alpha\sigma_u^2 + 2\beta(1 + \theta^2)\sigma_v^2 \quad (12)$$

$$\text{var}(\epsilon_{j,t+2} - \epsilon_{j,t-3}) = 5\sigma_u^2 + 2(1 + \theta^2)\sigma_v^2 + 2\sigma_m^2 \quad (13)$$

Shorter changes look similar, but with fewer accumulated $u_{j,t+s}$ permanent terms in (10)-(11), and smaller leading terms in (12)-(13). In particular, coefficients on the leading terms in (12)-(13) equal the difference length, in this case 5.¹⁸

If there were no measurement error, the OLS coefficients would reflect a weighted average of α and β , with α more heavily weighted for longer changes. If there were no measurement error, and α equaled β , the ratio of (12) to (13) would give the common parameter $\alpha=\beta$. Measurement error biases coefficients toward zero, but less so as the difference length increases. Generally speaking, there is a difficulty in distinguishing between the effects of

¹⁸ The 1-period changes also have an additional component θ to the σ_v^2 term.

measurement error and different responses to permanent and transitory firm shocks ($\sigma_m^2=0$ and $\alpha>\beta$ could look the same in the data as $\sigma_m^2>0$ and $\alpha=\beta$). This seems intuitive since measurement error is transitory in the setup.

In our second approach, we follow Card, Cardoso, and Kline (2016) and use revenue growth at different dates as instruments, in particular choosing instrument dating that overlaps the independent variable but without common endpoints. For instance, the IV coefficient in a 1-period change model using the 5-period change ($\epsilon_{j,t+2} - \epsilon_{j,t-3}$) as an instrument is

$$cov(\Delta\omega_{ijt}, (\epsilon_{j,t+2} - \epsilon_{j,t-3}))/cov(\Delta\epsilon_{jt}, (\epsilon_{j,t+2} - \epsilon_{j,t-3})) = (\alpha\sigma_u^2)/\sigma_u^2 = \alpha. \quad (14)$$

If the instrument spans a long enough horizon and overlaps with but does not share endpoints with the independent variable, then the instrumentation filters out any variation having to do with the transitory terms, true or mismeasured, and the IV coefficient measures the earnings response to permanent shocks. One gets the same outcome from a 5-period change model when instrumenting with the one-period change $\Delta\epsilon_{jt}$, since $cov((\omega_{ij,t+2} - \omega_{ij,t-3}), \Delta\epsilon_{jt}) = cov(\Delta\omega_{ijt}, (\epsilon_{j,t+2} - \epsilon_{j,t-3}))$.¹⁹

Instrumenting with a just-barely-overlapping change would return α if $\theta=0$, but not otherwise. For example, a 3-period earnings growth model instrumented using a 5-period change in firm revenues returns

$$\frac{cov((\omega_{ij,t+1} - \omega_{ij,t-2}), (\epsilon_{j,t+2} - \epsilon_{j,t-3}))}{cov((\epsilon_{j,t+1} - \epsilon_{j,t-2}), (\epsilon_{j,t+2} - \epsilon_{j,t-3}))} = \frac{3\alpha\sigma_u^2 - 2\beta\theta\sigma_v^2}{3\sigma_u^2 - 2\theta\sigma_v^2} \quad (15)$$

¹⁹ This is a general symmetry result from this setup. For instance, the model suggests that estimating a 3-period growth model with a 1-period change as instrument would return the same parameter as running a 1-period growth model with a 3-period instrument (provided changes are dated so as to overlap but not share endpoints).

Instrumenting with a just-barely-overlapping change filters out the measurement error and the part of the transitory shock that disappears in one period, but not the part of the transitory shock with a lingering effect into a second period. If $\alpha=\beta$ this would return the common parameter.

To summarize, we expect OLS coefficients to rise with difference length, and to give a better measure of the earnings response to permanent shocks for longer changes. However, we cannot determine how much of any observed pattern in OLS coefficients to attribute to measurement error versus different responses to transitory and permanent shocks. We expect the IV strategies identifying α to return larger coefficients because they avoid the variation associated with measurement error and the transitory shocks (assuming $\alpha \geq \beta$). Higher IV coefficients are consistent with both measurement error in firm revenues and with a greater response to permanent than transitory shocks. Finally, we would take similar OLS and IV coefficients as some evidence in favor of small measurement error along with $\alpha \approx \beta$, meaning earnings responses to transitory and permanent shocks are similar.

VI. Results

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

$$\Delta \ln w_{ijt} = \Delta X_{ijt} \mu + \pi \Delta R_{jt} + \Delta \varphi_{ijt} \quad (16)$$

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.²⁰ Taking differences nets out worker-firm fixed effects. Other controls include separate dummies for worker age for men and for women, 4-digit industry dummies, year and state dummies, and 3-digit industry dummies interacted with year dummies.²¹

a. Baseline Results

Table 3 presents OLS regression results for four industries—manufacturing, retail, professional services and finance (as defined in section III). The table shows estimates for 1-year, 3-year, and 5-year changes. In order to compare shorter and longer-run changes on the same sample of workers and firms, we restrict our sample to firms and workers who remain in the sample long enough to be included in 5-year changes. The OLS estimates indicate that very little of the firm-level shocks in revenue are passed to worker earnings. In manufacturing, shown in the top left panel, the estimate is a highly significant 0.007 for 1-year changes. The estimates for 3-year and 5-year changes are larger (0.012 and 0.013) but still quite small. Estimates for professional services and retail are generally a bit larger than those for manufacturing, but still small. While we expected to find larger effects for finance because that is where we thought variable pay should be most important, there is little evidence that firm-level performance is transmitted to worker earnings in the finance sector.

²⁰ Various firm performance measures are possible. Value added is an attractive measure because it approximates the relevant pool of funds that is subject to rent capture or bargaining by labor. But revenues are likely to be better measured, as value added is typically derived from revenues by subtracting costs.

²¹ Our manufacturing and retail samples span multiple 3-digit industries but the other samples do not.

Table 4 reports results for instrumental variables models where the change in firm revenues is instrumented with non-overlapping shorter or longer changes. The columns refer to changes in worker log earnings (the dependent variables) while the rows refer to the changes in firm log revenues (the instruments). For example, the coefficient 0.023 reported for manufacturing is the estimate obtained from regressing 1-year changes in log worker earnings on 1-year changes in firm log revenues, with the 3-year change in log revenues used as an instrument. The IV estimates for manufacturing are generally larger than the OLS estimates shown in Table 3, and are now generally roughly 0.02. The IV estimates are also larger for professional services where some of the estimates are now close to 0.05. The results for retail vary rather substantially from .024 to .067 and appear sensitive to specification.²² Again, we find little evidence of transmission of firm level shocks to worker earnings in finance.

It is useful to compare our results for the U.S. to others in the literature. Our estimates are in the neighborhood of those found by Arai (2003) for Sweden, Margolis and Salvanes (2001) for Norwegian manufacturing, and Martins (2009) for Portuguese manufacturing; these form the lower end of the range of estimates identified by Card, Cardoso, Heining, and Kline (2016). There are many reasons related to both data and measurement that could account for the lower rates we estimate in the U.S. compared to those from European countries. But issues of measurement aside, lower pass-through of firm-level shocks to workers is somewhat unexpected

²² In previous versions, we have allowed sparsely populated 3-digit industry cells to have their own industry-specific trends. In the current version, we pool the sparse industries with a residual category. The IV estimates we report in this version are generally larger than our previously reported estimates and somewhat more consistent across specifications. However, the sensitivity of the results to these types of data adjustments makes us somewhat cautious in reaching strong conclusions regarding our results in the retail sector.

given the existence of greater labor market flexibility in the U.S. For example, the U.S. has the lowest rates of unionization and collective bargaining agreements among OECD countries (Freeman 2007).

One explanation is that while unionization and collective bargaining may reduce wage flexibility at a more aggregate level, the presence of such institutions may not necessarily preclude wage adjustments at the firm level (Cardoso and Portugal 2005; Teulings 1997). Another possibility is crowd-out of private insurance by public insurance (Ellul, Pagano and Schivardi 2014). The U.S. offers less generous unemployment and disability insurance benefits compared to most OECD countries (OECD 2010; OECD 2016). This type of crowd out is unlikely, however, since these benefits are tied to non-employment rather than to wage cuts. Another possible explanation is that U.S. firms provide less employment insurance and are more likely to lay off workers in the event of a negative productivity shock.

In our paper as well as in Guiso et al. (2005), the focus has been on earnings changes among workers who are continuing with the firm. We show that stayers are a select sample when compared to a broader sample of employees working at least one full quarter. Thus a focus on earnings risk, conditional on employment, leaves open the important question of whether firms readily transfer employment risk by laying off workers even as they reduce wage risk for the workers who are retained.²³ Lamadon (2014) finds that a significant fraction of earnings risk

²³ It does appear to be case that firms are more inclined to lay off workers than cut nominal wages in response to aggregate shocks such as the business cycle. A large set of papers demonstrate downward rigidity of nominal wages at the individual level (see Card and Hyslop 1997; Kramarz 2001; and Dickens et al. 2006). Bewley (1999) also provides evidence from surveying managers that firms are reluctant to cut nominal wages due to the fear of reducing worker morale. It is not clear whether these findings translate to the case of idiosyncratic shocks at the firm level, however, which is our focus here.

is due to unemployment and job mobility. We view exploring employment risk in conjunction with wage insurance parameters as important topic for future research.

In the results reported above, we have restricted our sample to the 5-year change sample in order to compare shorter and longer-run changes holding sample composition constant. In doing so, however, we lose a large share of our original sample, particularly in the retail sector where the average tenure of workers is shorter. One concern is that our 5-year change sample is not very representative of typical workers or firms in the industry. We have examined how much this restriction affects our results by repeating our analysis of 1- and 3-year changes on a sample that requires staying in our data for 4 consecutive years (our 3-year change sample) rather than 6 consecutive years. These results are reported in Appendix Tables 2 and 3. The results using this more robust sample are qualitatively similar to those reported in Table 3 and Table 4.

a. Heterogeneity by Size and Direction of Shocks

While we find small effects overall, it is possible that our estimates of mean effects obscure different patterns at other parts of the distribution of firm-level shocks. In particular, it seems relevant to ask whether firms insure workers from particularly large shocks or are more likely to insure workers against negative shocks but allow earnings to vary with positive shocks. We explore this possibility in Figure 4 by graphing mean changes in log worker earnings against

changes in log firm-revenues.²⁴ We utilize the more robust 3-year change sample for this exercise. We order the firm revenue changes and divide them into 50 bins with approximately equal numbers of firm/year observations, and then average the changes in log revenues and changes in worker earnings within each bin. We have omitted the lowest and highest bins from the figure.

The figure shows suggestive evidence that firms insure workers against very large shocks in manufacturing and professional services. This seems to be true for both large positive shocks and for large negative shocks. The slope relating firm revenue shocks to changes in worker earnings is flatter at the extremes than in the middle. The pattern is less clear for retail and for finance; those figures do not point to obvious asymmetric effects of positive and negative shocks. We verify this by running regressions for the 3-year changes allowing the coefficients to differ for positive and negative shocks, which we report in Table 5. The coefficients are slightly larger for negative shocks although in the case of manufacturing, the difference is minimal (and in finance, none of the coefficients are significant). While we do not interpret our estimates as rent-sharing parameters, this type of asymmetry would be hard to reconcile with a rent-sharing story alone. The expectation is that firms do not cut wages in the case of negative shocks because workers may leave while they are likely to share the rents generated by positive shocks. One caveat is that we cannot separately identify hours variation in our data.²⁵ It is possible at

²⁴ We first residualize changes in both firm revenues and worker earnings by regressing out the set of controls used in our regressions: age dummies separately for men and women, year and state dummies, 4-digit industry dummies, and 3-digit industry dummies interacted with year dummies (for those sectors spanning multiple 3-digit industries).

²⁵ Unfortunately we do not have hours worked data for the retail, finance, or professional services. We do have total hours worked by production workers in manufacturing. We present results controlling for hours worked in manufacturing in a later robustness section.

least in the retail sector where part-time employment is common that firms cut hours for stayers in the case of negative demand shocks.

b. Heterogeneity by Worker Characteristics

Wage insurance and incentive pay are likely to be more relevant motives for structuring pay for some jobs than others. 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 years of age and a full set of year*quarter dummies to adjust for differences in the age and time periods in which earnings 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 again based on the 5-year change sample, we assign workers in the sample to quantiles based on order statistics calculated including any non-stayers that worked one or more full quarters. Therefore our quantiles do not split the sample of stayers into equal fractions, but they do preserve a reasonable ordering of our in-sample observations.

Table 6 shows the results for quintile 1, quintile 3 and for the top 5% of the distribution. The table reports OLS estimates based on 5-year changes and IV estimates that instrument 5-year changes in log revenues with 3-year changes in log revenues. In the retail sector the effects are similar across the wage categories. In manufacturing and professional services, there is more evidence that coefficients are larger for higher wage workers. In manufacturing, the IV

coefficient increases from being close to zero and insignificant for quintile 1 workers to 0.029 for the top 5% workers. The pattern is much more pronounced in professional services where the IV coefficient goes from being insignificant and close to zero for quintile 1 workers to 0.091 for the top 5%. The near 10% elasticity is the largest coefficient estimate we find in any of our specifications. This suggests that shifting composition of employment from manufacturing to professional services and from less-skilled jobs to higher skilled jobs may have contributed to increased volatility of worker earnings although we note that we do not find much evidence of performance-related pay (connected to firm-level outcomes) for vast majority of workers in our data. This is somewhat in contrast to the findings by Lemieux, MacLeod, and Parent (2009) who attribute a substantial role to the rising incidence of performance pay to increasing level of earnings inequality.

While we generally find small estimates and interpret this as evidence of partial wage insurance, a competing explanation is that labor markets are effectively competitive and firms have little leeway in setting wages. This is more likely to be true for workers who have just been hired and have no accumulated firm-specific human capital. We examine whether workers with greater tenure in the firm (and presumably more firm-specific human capital) experience larger transmission of firm-level shocks compared to the just hired.

We explore these differences in Table 7. We use our 3-year change sample here because requiring the longer 5-year interval is likely to have particularly large effects on the number of new hires who remain in sample. We distinguish between just-hired employees and longer-term employees, where the latter category is defined as workers who have 4+ quarters of tenure in the first year of the 3-year change. Table 7 shows that in retail and professional services, workers

with more tenure experience somewhat larger changes in their earnings in response to firm shocks than do the just hired, but even in these case the transmission of firm-level shocks to worker earnings continues to be fairly small. Estimated differences for both manufacturing and finance are insignificant. It thus appears that even for workers with greater tenure, there is little transmission of firm-level shocks to worker earnings suggesting that wage insurance is still a compelling explanation for the small size of the elasticities.

c. Additional Robustness Checks Using Manufacturing Data

We have focused on revenues as our measure of firm performance, presuming that revenue changes are driven by demand shocks or productivity changes so that the prospect of profits to be shared with workers grows. However, mergers or acquisitions could also lead to revenue increases without necessarily reflecting improved firm performance. To examine whether this is an important concern, we include controls for changes in firm employment for manufacturing so that our estimates should be less sensitive to such reorganizations.²⁶ We also include specifications that add a control for hours: log differences of the ratio of total production worker hours to production worker employment. We do this to see whether adding hours reduces our estimates, which would suggest that part of the adjustment is due to changes in hours rather than

²⁶ Unfortunately, the ARTS and SAS surveys do not collect employment data. While it is possible to obtain employment measures from Census's Business Register, uncertainty about the relationship between the sampling units used for these surveys and the units on the BR mean that using such a measure as a denominator for sales per worker would add considerable measurement error.

wages. We focus on five-year change models and the sample is similar to that in Table 4.²⁷ The first column of Table 8 reports OLS results while the second reports instrumental variables results. Compared to our baseline results reported in Table 4, we find somewhat larger coefficients when we add employment controls. For example, the IV estimates for manufacturing reported in Table 4 ranged between 0.016 and 0.024. Our estimate with employment controls is 0.038. On the other hand, we find that adding hours has very little effect on our estimates. This suggests that the interpretation of our estimates as referring to wage insurance is still valid and that, at least in manufacturing, the elasticity parameters do not simply reflect an hours adjustment. Finally, we replace sales with value added for the final row of Table 8. Card, Cardoso, Heining and Kline (2016) find that coefficients estimated using value-added are somewhat larger than those estimated using sales. We do not find that to be the case in Table 8—in fact, our estimates using value-added are somewhat lower than those using sales. On net, the results in Table 8 suggest that various specification changes may lead to slightly smaller or larger estimates but the overall similarity of these results to our baseline results are reassuring.

VII. Conclusions and Future Work

U.S. evidence on the extent to which changing firm conditions affect the earnings of continuing employees is scant, largely because data to address this question has only recently

²⁷ In estimating the models in Table 8 we require observations to have non-missing values for log changes in firm value added, firm employment, and average production worker hours. The estimation sample is identical for all models in Table 8. About 1.5 percent of our 5-year change sample is dropped in estimating Table 8; about two-thirds of the dropped observations had negative values for firm value added.

become available. In this paper, we use firm level data in manufacturing, retail and selected service industries to examine this question. IV estimates which isolate persistent shocks and correct for measurement error are small and positive—ranging from 0.02 in manufacturing to approximately 0.04 in professional services and retail. The range of estimates is large in the retail sector and appear to be somewhat sensitive to specification. In professional services, we find important heterogeneity across workers, with the coefficient being essentially zero among the lowest wage workers in the first quintile of the firm earnings distribution while the coefficient is 0.09 among those in the top 5%. The highest paid workers in professional services, however, are likely to be in precisely the sort of jobs in which we would expect performance pay objectives to override insurance concerns. While our finding of non-zero estimates is technically a rejection of the notion of full insurance, these coefficients are economically small enough that it seems sensible to conclude that the transmission of firm-level shocks to earnings of stayers is minimal in the U.S. labor market.

To what extent have firms contributed to the recent increases in financial risk experienced by American families? Our evidence here, based on workers who remain with the firm, suggests that firms had a relatively limited role. Even if firms offer less wage insurance to workers than in the past, our estimates suggest that even in the 2000s, very little of the volatility in firm performance is passed through to workers. However, there is an important caveat, in that our paper focuses on earnings changes among workers who are continuing with the firm. It is possible that firms are more likely to lay-off workers in adverse circumstances and employment risk has increased for American workers. We view this as an important topic for future research.

References

- Abowd, John M., and Thomas Lemieux. 1993. The effects of product market competition on collective bargaining agreements: the case of foreign competition in Canada. *Quarterly Journal of Economics* 108, no.4: 983-1014.
- Arai, Mahmood. 2003. Wages, profits and capital intensity: evidence from matched worker-firm data. *Journal of Labor Economics* 21, no.3: 593-618.
- Azariadis, Costas. 1975. Implicit contracts and underemployment equilibria. *The Journal of Political Economy* 83, no.6: 1183-1202.
- Baily, Martin Neil. 1974. Wages and employment under uncertain demand. *The Review of Economic Studies* 41, no.1: 37-50.
- Barth, Erling, Alex Bryson, James C. Davis, and Richard Freeman. 2016. It's where you work: increases in earnings dispersion across establishments and individuals in the United States. *Journal of Labor Economics* 34, suppl.2: S67-S97.
- Bewley, Truman F. 1999. Why wages don't fall during a recession. Cambridge and London: Harvard University Press.
- Blanchflower, David G., Andrew J. Oswald, and Peter Sanfey. 1996. Wages, profits and rent sharing. *Quarterly Journal of Economics* 111, no.1: 227-252.
- Card, David, Ana Rute Cardoso, and Patrick Kline. 2016. Bargaining, sorting, and the gender wage gap: quantifying the impact of firms on the relative pay of women. *The Quarterly Journal of Economics* 131, no. 2: 633-686.
- Card, David, Ana Rute Cardoso, Jörg Heining, and Patrick Kline. 2016. Firms and labor market inequality: evidence and some theory. Working Paper no. 22850, National Bureau of Economic Research, Cambridge, MA.
- Card, David, and Dean Hyslop. 1997. Does inflation grease the wheels of the labor market? In *Reducing inflation: motivation and strategy*, ed. Christina D. Romer and David H. Romer. Chicago, IL: University of Chicago Press.
- Cardoso, Ana and Miguel Portela. 2009. Micro foundations for wage flexibility: wage insurance at the firm level. *Scandinavian Journal of Economics* 111, no.1: 29-50.
- Cardoso, Ana and Pedro Portugal. 2005. Contractual wages and the wage cushion under different bargaining settings. *Journal of Labor Economics* 23, no.4:875-902.

- Celik, Sule, Chinhui Juhn, Kristin McCue, and Jesse Thompson. 2012. Recent trends in earnings volatility: evidence from survey and administrative data. *The BE Journal of Economic Analysis & Policy* 12, no.2.
- Comin, Diego, Erica L. Groshen, and Bess Rabin. 2009. Turbulent firms, turbulent wages? *Journal of Monetary Economics* 56, no.1: 109-133.
- Dahl, Molly W., Thomas C. DeLeire, and Jonathan A. Schwabish. 2007. Trends in earnings variability over the past 20 years. Congressional Budget Office, Washington, D.C.
- DeBacker, Jason, Bradley Heim, Vasia Panousi, Shanthi Ramnath, and Ivan Vidangos. 2013. Rising inequality: transitory or persistent? New evidence from a panel of U.S. tax returns. *Brookings Papers on Economic Activity* no.2: 67–142.
- Dickens, William T., Lorenz Goette, Erica L. Groshen, Steinar Holden, Julian Messina, Mark E. Schweitzer, Jarkko Turunen, and Melanie Ward. 2006. The interaction of labor markets and inflation: analysis of micro data from the International Wage Flexibility Project. Brookings Institution.
- Ellul, Andrew, Marco Pagano, Fabiano Schivardi. 2014. Employment and wage insurance within firms: worldwide evidence. Working Paper.
- Fagereng, Andreas, Luigi Guiso, and Luigi Pistaferri. 2016. Back to background risk? Discussion Paper no. 11051, Center for Economic and Policy Research.
- Freeman, Richard. 2007. Labor market institutions around the world. Working Paper no. 13242, National Bureau of Economic Research, Cambridge, MA.
- Gathright, Graton, and Holly Monti. 2013. Measuring earnings instability using survey and administrative data. Working Paper no. 260, Survey of Income and Program Participation.
- Gottschalk, Peter, and Robert Moffitt. 1994. The growth of earnings instability in the U.S. labor market. *Brookings Papers on Economic Activity* no.2: 217-54.
- Guertzgen, Nicole. 2014. Wage insurance within German firms: do institutions matter? *Journal of the Royal Statistical Society* 177, no.2: 345-369.
- Guiso, Luigi, Luigi Pistaferri, and Fabiano Schivardi. 2005. Insurance within the firm. *Journal of Political Economy* 113, no.5: 1054-1087.
- Guvenen, Fatih, Serdar Ozcan, and Jae Song. 2014. The nature of countercyclical income risk. *Journal of Political Economy* 122, no.3: 621–660.

- Hacker, Jacob S. 2006. *The great risk shift: the assault on American jobs, families, health care, and retirement—and how you can fight back*. New York: Oxford University Press.
- Hildreth, Andrew K.G., Andrew Oswald. 1997. Rent sharing and wages: evidence from company and establishment panels. *Journal of Labor Economics* 15, no.2: 318-337.
- Katay, Gabor. 2009. Do firms provide wage insurance against shocks? Evidence from Hungary. Working Paper no. 964, European Central Bank.
- Kramarz, Francis. 2001. Rigid wages: what have we learned from microeconomic studies? In *Advances in macroeconomic theory*, ed. J. Drèze. Oxford, U.K.: Oxford University Press.
- Lagakos, David and Guillermo L. Ordonez. 2011. Which workers get insurance within the firm? *Journal of Monetary Economics* 58, no.6: 632-645.
- Lamadon, Thibaut. 2014. Productivity shocks, dynamic contracts and income uncertainty. Working Paper.
- Lazear, Edward P. 1986. Salaries and piece rates. *Journal of Business* 59, no.3: 405-431.
- Lazear, Edward P. 1999. Output-based pay: incentives or sorting? Working Paper no. 7419, National Bureau of Economic Research, Cambridge, MA.
- Lazear, Edward P. 2000. Performance pay and productivity. *American Economic Review* 90, no.5: 1346-1361.
- Le Barbanchon, Thomas, and Linas Tarasonis. 2011. Wage responsiveness within the firm: evidence from France. Working Paper.
- Lemieux, Thomas, W. Bentley MacLeod, and Daniel Parent. 2009. Performance pay and wage inequality. *Quarterly Journal of Economics* 124, no.1: 1-49.
- Margolis David, Kjell G. Salvanes. 2001. Do firms really share rents with their workers? *Discussion Paper* no. 330, IZA- Institute of Labor Economics.
- Martins, Pedro S. 2009. Rent sharing before and after the wage bill. *Applied Economics* 41, no.17: 2133-2151.
- OECD. 2010. *Sickness, disability, and work: breaking the barriers*. Paris: OECD.
- OECD, 2016. Benefits and wages statistics. <http://www.oecd.org/els/benefits-and-wages-statistics.htm>.

- Oyer, Paul, and Scott Schaefer. 2005. Why do some firms give stock options to all employees? An empirical examination of alternative theories. *Journal of Financial Economics* 76, no.1: 99-133.
- Rosen, Sherwin. 1985. Implicit contracts: a survey. *Journal of Economic Literature* 23, no.3: 1144-1175.
- Sabelhaus, John, and Jae Song. 2010. The Great moderation in micro labor earnings. *Journal of Monetary Economics* 57, no.4: 391-403.
- Shin, Donggyun, and Gary Solon. 2011. Trends in men's earnings volatility: what does the Panel Study of Income Dynamics show? *Journal of Public Economics* 95, no.7: 973-982.
- Strain, Michael. 2013. Do volatile firms pay volatile earnings? Evidence using linked worker-firm data. Policy Working Paper No. 2013-01, American Enterprise Institute.
- Teulings, Coen N. 1997. A new theory of corporatism and wage-setting. *European Economic Review* 41, no.3-5: 659-669.
- Van Reenen, John. 1996. The creation and capture of rents: wages and innovation in a panel of U.K. companies. *Quarterly Journal of Economics* 111, no.1: 195-226.

Table 1
Summary Statistics

Sector	Sample	N	Unique firms	Unique firm years	Change in log revenue, per year	Change in log earnings, per year	Annual earnings (\$2012)	Revenues (M \$2012)	Age	Share female	Change in log sales per worker (annualized)
Manufacturing	Cross-section	65,192,500	213,700	616,700			56,900	788	42	32	
	1-year difference	41,349,200	64,100	288,900	0.010	0.019	65,500	1,045	44	29	0.025
	3-year difference	22,706,400	39,100	163,700	0.010	0.014	67,200	1,166	44	29	0.026
	5-year difference	12,505,100	20,500	90,100	0.023	0.015	68,500	1,259	44	28	0.033
Retail	Cross-section	18,129,900	63,000	217,800			51,300	119	41	45	
	1-year difference	5,326,000	29,300	105,400	0.020	0.022	68,800	139	43	44	
	3-year difference	1,981,800	19,400	50,000	0.030	0.019	72,200	123	43	45	
	5-year difference	496,000	12,300	14,500	0.091	0.019	72,500	111	44	46	
Finance	Cross-section	2,568,100	3,600	10,500			207,300	6,165	39	42	
	1-year difference	840,400	1,600	5,100	0.065	0.057	264,600	4,856	41	43	
	3-year difference	315,600	700	2,200	0.070	0.045	256,900	3,031	42	45	
	5-year difference	128,700	500	1,000	0.059	0.046	240,300	2,816	42	47	
Professional Services	Cross-section	17,113,700	26,700	89,500			78,700	1,687	39	46	
	1-year difference	5,024,600	12,100	45,100	0.061	0.038	102,900	1,789	42	47	
	3-year difference	2,250,600	7,200	23,000	0.053	0.035	107,100	1,724	42	49	
	5-year difference	1,021,600	4,700	9,900	0.048	0.035	108,300	1,687	43	50	

Notes. Manufacturing includes NAICS industries 31-33; source is Annual Survey of Manufactures. Retail data includes NAICS industries 44-45 (retail trade) and 72 (accommodation and food services); source is the Annual Retail Trade Survey. Finance includes NAICS 523 and Professional Services includes NAICS 541; source is Services Annual Survey. Samples are for workers aged 25-59. Averages are worker-weighted averages.

Table 2
Earnings volatility of stayers and non-stayers, by age group

Age group	Stayers	Non-stayers, full-quarter earnings			Non-stayers, any earnings		
	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	.467	.486	.137	1.10	.394	.019
35-59	.174	.456	.620	.193	1.06	.539	.031

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.

Table 3
OLS Models for Changes in Log Worker Earnings

Change in log revenues over:		Dependent variable = Change in log earnings over:					
		Manufacturing			Retail		
		1-year	3-year	5-year	1-year	3-year	5-year
1-year	0.007*** (0.001)				0.012** (0.004)		
3-year			0.012*** (0.002)			0.033*** (0.007)	
5-year				0.013*** (0.003)			0.021*** (0.004)
		Professional Services			Finance		
		1-year	3-year	5-year	1-year	3-year	5-year
1-year	0.015*** (0.004)				-0.003 (0.008)		
3-year			0.019*** (0.005)			-0.004 (0.009)	
5-year				0.010 (0.007)			0.006 (0.010)

Notes. Samples are for workers aged 25-59 who stayed with the same employer for 6 years. Samples are consistent across specifications. Statistics are OLS coefficients from regressions of log earnings growth on firm log revenue growth. Column and row headings indicate whether growth is calculated as 1-, 3- or 5-year changes. Additional controls include single year of age indicators for men and for women; state, year, and four-digit industry indicators; and, in Manufacturing and Retail, interactions between year and three-digit industry indicators. Standard errors are clustered at the firm level and reported in parentheses (** p<0.01 *** p<0.001).

Table 4
Instrumental Variables Models for Changes in Log Worker Earnings

Dependent variable = Change in log earnings over:						
Manufacturing				Retail		
Instrument:	1-year	3-year	5-year	1-year	3-year	5-year
1-yr change in log(revenues)		0.020*** (0.004)	0.012* (0.005)		0.024* (0.010)	0.067** (0.026)
3-yr change in log(revenues)	0.023*** (0.004)		0.016*** (0.004)	0.043*** (0.010)		0.034*** (0.007)
5-yr change in log(revenues)	0.024*** (0.006)	0.020*** (0.004)		0.057** (0.017)	0.039*** (0.008)	
Professional Services				Finance		
1-yr change in log(revenues)		0.041*** (0.011)	0.020 (0.013)		-0.002 (0.013)	-0.003 (0.015)
3-yr change in log(revenues)	0.048*** (0.008)		0.022** (0.008)	-0.010 (0.009)		0.011 (0.023)
5-yr change in log(revenues)	0.051* (0.020)	0.041** (0.015)		0.003 (0.015)	0.009 (0.024)	

Notes. Samples are for workers aged 25-59 who stayed with the same employer for 6 years. Samples are consistent across specifications. Statistics are IV coefficients from regressions of log earnings growth on firm log revenue growth over the same interval. Column headings indicate whether growth is calculated as 1-, 3- or 5-year changes. The length of the change in the instrument varies across rows; instrument dating is chosen so that the midpoint of the instrument change coincides with the midpoint of the independent variable change. Additional controls include single year of age indicators for men and for women; state, year, and four-digit industry indicators; and, in Manufacturing and Retail, interactions between year and three-digit industry indicators. Standard errors are clustered at the firm level and reported in parentheses (** p<0.01 *** p<0.001).

Table 5
OLS Models with Asymmetric Effects

Change in log revenue	Dependent variable = 3-year change in log earnings	
	Manufacturing	Retail
3-yr change * (indicator for positive change)	0.009*** (0.002)	0.018*** (0.005)
3-yr change * (indicator for negative change)	0.013*** (0.002)	0.039*** (0.010)
	Professional Services	Finance
3-yr change * (indicator for positive change)	0.016** (0.005)	-0.002 (0.009)
3-yr change * (indicator for negative change)	0.031*** (0.007)	0.013 (0.011)

Notes. Samples are for workers aged 25-59 who stayed with the same employer for 4 years. Statistics are OLS coefficients from regressions of log earnings growth on firm log revenue growth, interacted with indicators for whether the change in firm revenues is positive or negative. Growth is calculated as a 3-year change. Additional controls include single year of age indicators for men and for women; state, year, and four-digit industry indicators; and, in Manufacturing and Retail, interactions between year and three-digit industry indicators. Standard errors are clustered at the firm level and reported in parentheses (** p<0.01 *** p<0.001).

Table 6
Changes in Log Worker Earnings, by Earnings Rank

	Manufacturing		Retail	
	OLS	IV	OLS	IV
Quintile 1	0.007 (0.006)	0.008 (0.010)	0.015* (0.007)	0.037** (0.013)
Quintile 3	0.015*** (0.002)	0.018*** (0.003)	0.020*** (0.004)	0.038*** (0.007)
Top 5%	0.019*** (0.005)	0.029*** (0.006)	0.019*** (0.003)	0.031*** (0.007)
	Professional Services		Finance	
Quintile 1	0.005 (0.004)	0.008 (0.007)	-0.008 (0.009)	-0.006 (0.015)
Quintile 3	0.011** (0.004)	0.017** (0.006)	0.002 (0.007)	0.004 (0.013)
Top 5%	0.042** (0.015)	0.091*** (0.015)	-0.010 (0.033)	-0.039 (0.030)

Notes: Results are for workers aged 25-59 who stayed with the same employer for 6 years; samples are split by workers' earnings rank within the firm, determined after controlling for age and time. The dependent variable is the 5-year change in log worker earnings. Statistics are estimated coefficients on the log change in revenues over a 5-year interval. The IV results instrument using the 3-year change in log sales. Additional controls include single year of age indicators for men and for women; state, year, and four-digit industry indicators; and, in Manufacturing and Retail, interactions between year and three-digit industry indicators. Standard errors are clustered at the firm level and reported in parentheses (* p<0.05 ** p<0.01 *** p<0.001).

Table 7
Recent Hires versus Longer Term Employees

	Manufacturing	Retail	Finance	Professional Services
3-yr change in log(revenues)	0.017*** (0.004)	0.005 (0.010)	0.012 (0.010)	0.013 (0.008)
3-yr change in log(revenues) * Longer term worker	0.012 (0.011)	0.013*** (0.003)	0.007 (0.020)	0.026*** (0.005)
Longer term worker	-0.041*** (0.002)	-0.050*** (0.003)	-0.067*** (0.008)	-0.033*** (0.003)

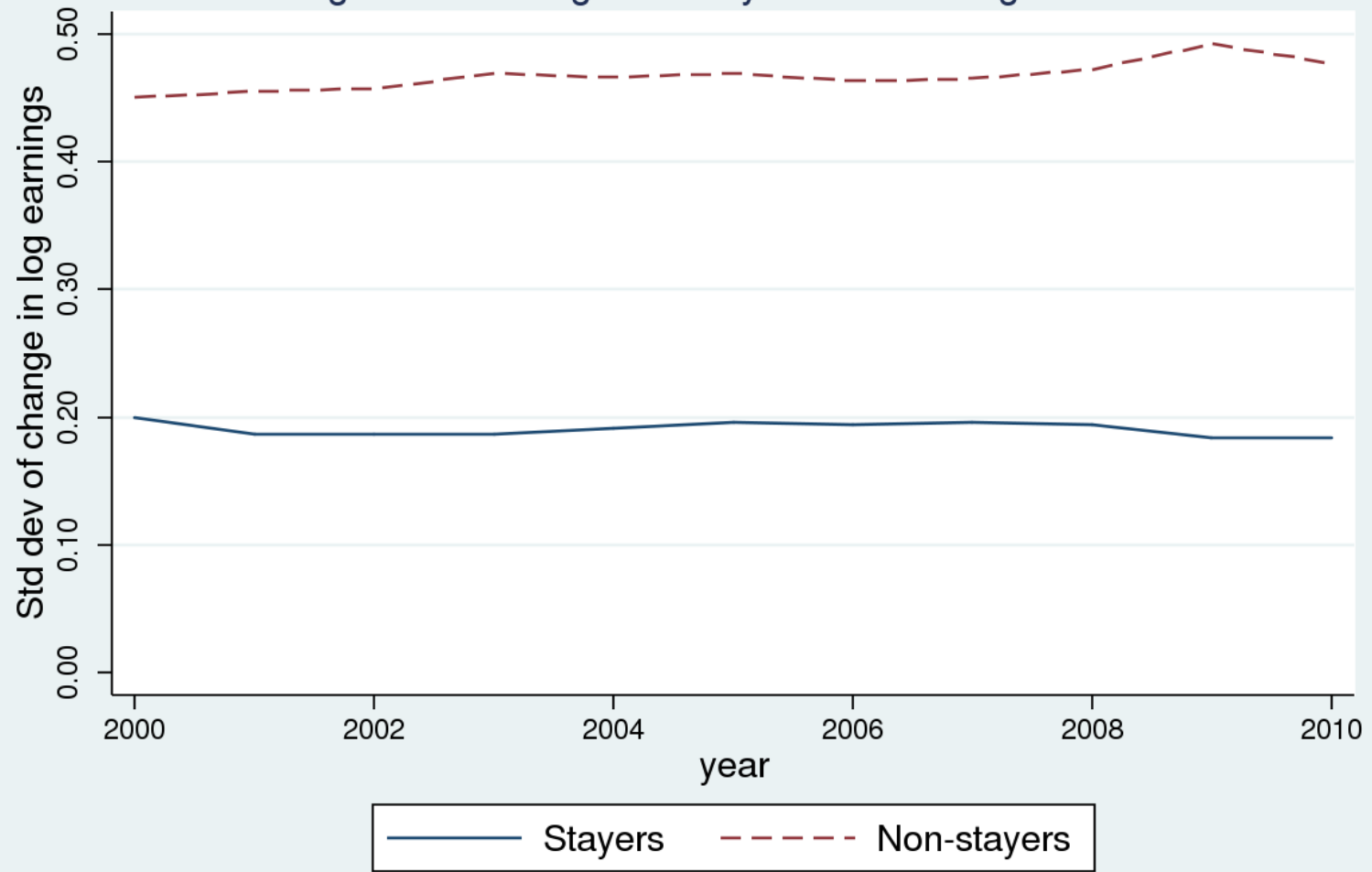
Notes. Statistics are IV regression coefficients from 3-year log earnings growth models. Longer term workers are defined as those with 4+ quarters of tenure at beginning of the 3-year interval. Instruments are the 1-year change in log revenues and the 1-year change in log revenues interacted with the longer term worker indicator. Additional controls include single year of age indicators for men and for women; state, year, and four-digit industry indicators; and, in Manufacturing and Retail, interactions between year and three-digit industry indicators. Standard errors are clustered at the firm level and reported in parentheses (** p<0.01 *** p<0.001).

Table 8
Additional Models, Manufacturing

Additional controls	Coefficient on 5-year change in:	Dependent variable: 5-year change in log earnings	
		OLS	IV
employment change	log revenues	0.027*** (0.004)	0.038*** (0.011)
employment change hours change	log revenues	0.026*** (0.004)	0.037*** (0.010)
employment change hours change	log value added	0.017*** (0.002)	0.028*** (0.004)

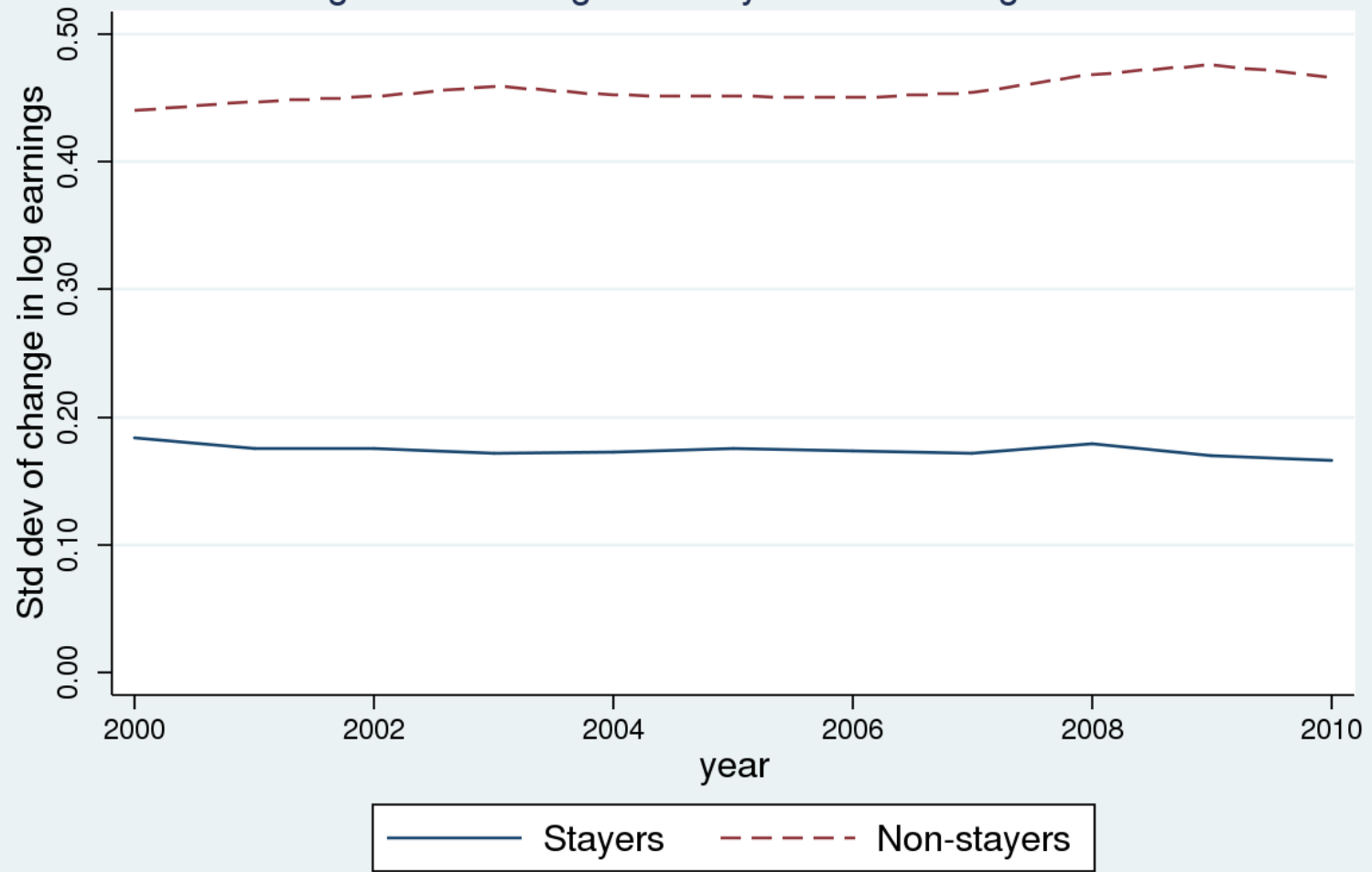
Notes. Samples are for workers aged 25-59 who stayed with the same employer for 6 years, and where observations have non-missing, positive values for firm employment, hours per worker among production workers, and value added. Statistics are coefficients from regressions of 5-year log earnings growth on 5-year growth in log firm revenues or value added over the same interval. For IV models the instrument is the 3-year change in the relevant independent variable with dating chosen so that the midpoints of the independent variable change and the instrument change coincide. Base controls include single year of age indicators for men and for women; state, year, and four-digit industry indicators; and, interactions between year and three-digit industry indicators. Additional controls are the 5-year change in log firm employment and in the latter two models the 5-year change in log hours per worker among production workers. Standard errors are clustered at the firm level and reported in parentheses (***) $p < 0.001$.

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

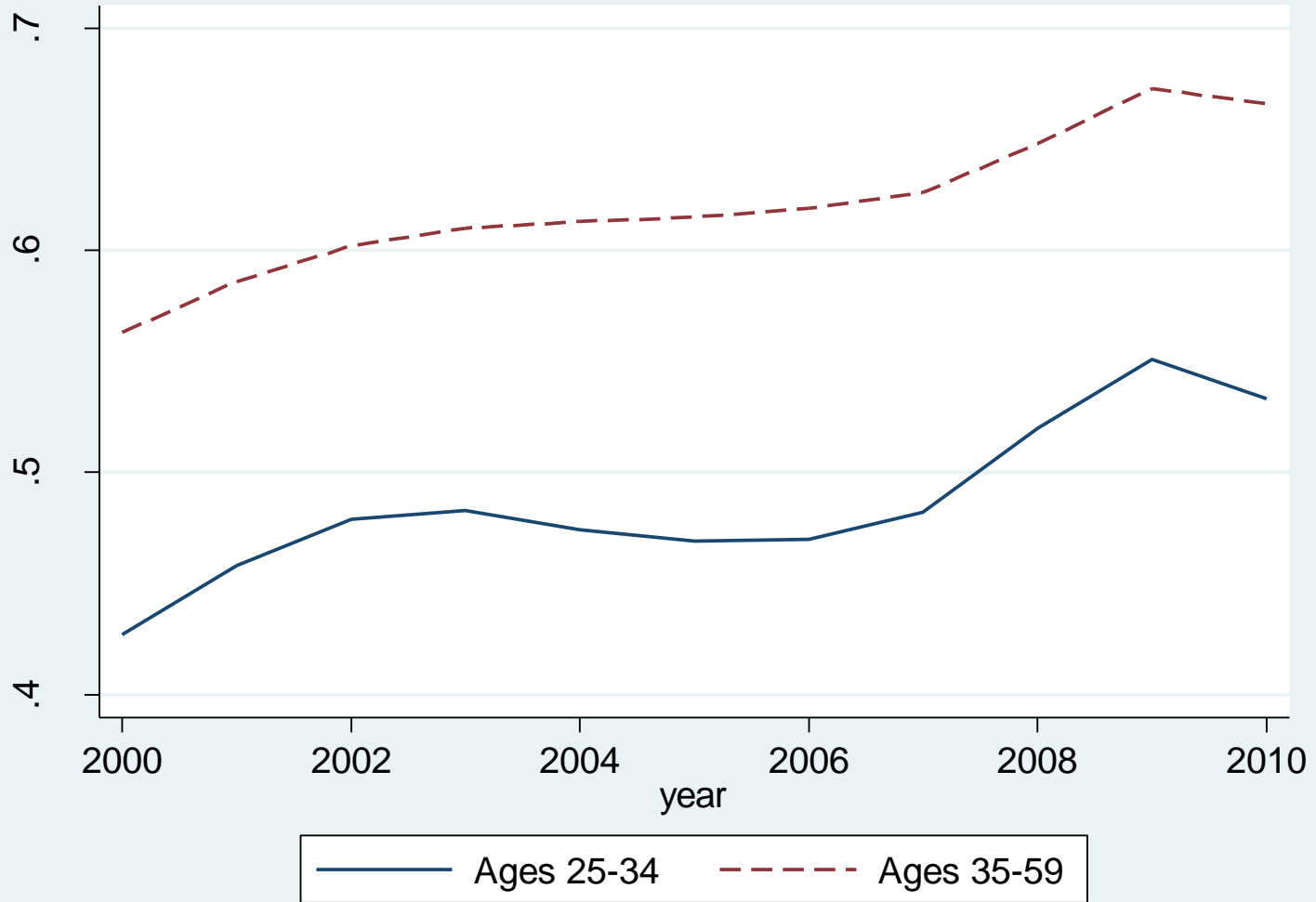
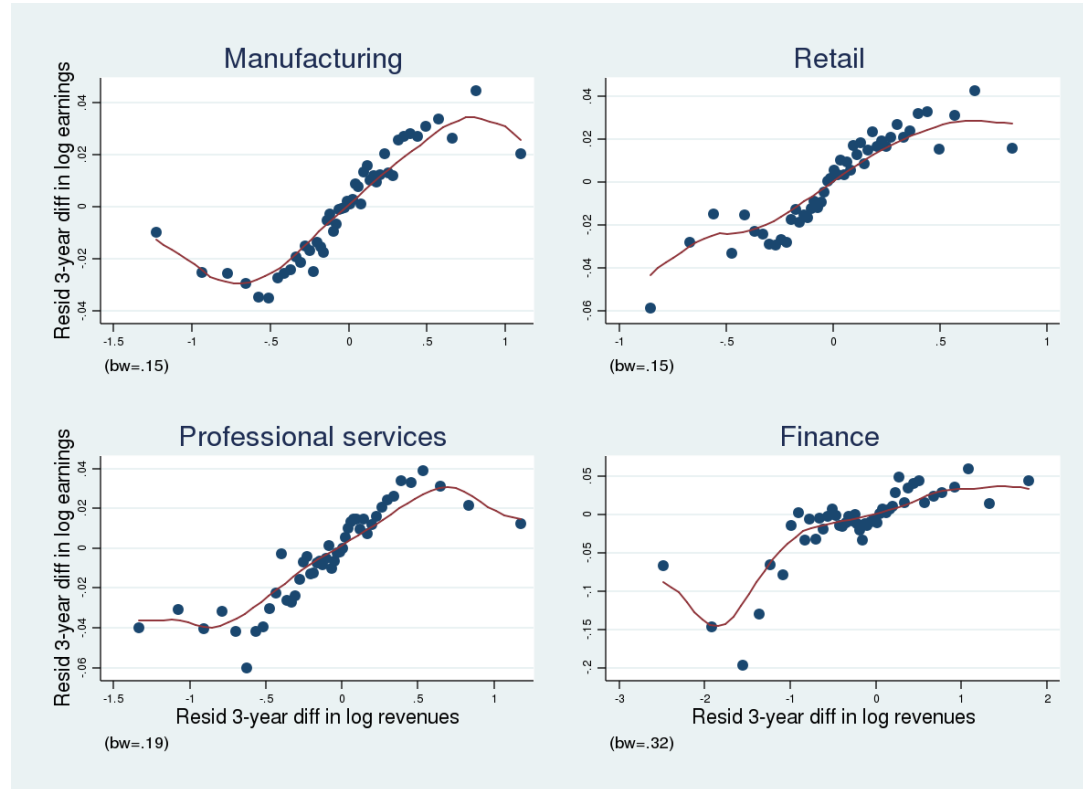


Figure 4. Earnings Growth and Revenues Growth, by Industry



Notes. Plots show binned averages (scatter points) and local polynomial regression smoothed values (red lines) for 3-year changes in worker log earnings as related to 3-year changes in firm log revenues. Samples are for 4-year stayers. Each point represents 2 percent of the relevant sample; the smallest and largest revenue change values are not plotted. The (rounded) number of firm/year observations per bin for these figures are 5,900 for manufacturing, 2,500 for retail, 1,300 for professional services, and 140 for finance. Rounded worker/year observations are 437,100 for manufacturing, 39,800 for retail, 45,300 for professional services, and 6,300 for finance.

Table A1
Changes in Log Worker Earnings by Firm Size

Firm size	Manufacturing		Retail	
	OLS	IV	OLS	IV
Below median size	0.020*** (0.002)	0.028*** (0.002)	0.025*** (0.006)	0.036** (0.012)
Above median size	0.010** (0.003)	0.011 (0.006)	0.048*** (0.006)	0.074*** (0.009)
	Professional Services		Finance	
Below median size	0.010 (0.010)	0.024* (0.010)	0.003 (0.009)	0.015 (0.026)
Above median size	0.007 (0.005)	0.016* (0.008)	0.030 (0.016)	-0.007 (0.027)

Notes: Median size is based on within-sector worker-weighted sales revenues for our sample of linked 1-year stayers. Regression results are for workers aged 25-59 who stayed with the same employer for 6 years. The dependent variable is the 5-year change in log worker earnings. Statistics are estimated coefficients on the log change in revenues over a 5-year interval. The IV results instrument using the 3-year change in log sales. Additional controls include single year of age indicators for men and for women; state, year, and four-digit industry indicators; and, in Manufacturing and Retail, interactions between year and three-digit industry indicators. Standard errors are clustered at the firm level and reported in parentheses (* p<0.05 ** p<0.01 *** p<0.001).

Table A2
OLS Models for 3-Year Difference Samples

Independent variable	Dependent variable = Change in log earnings over:			
	Manufacturing		Retail	
Change in log revenues over:	1-year	3-year	1-year	3-year
1-year	0.009*** (0.002)		0.015** (0.005)	
3-year		0.022*** (0.004)		0.027*** (0.004)
	Professional Services		Finance	
	1-year	3-year	1-year	3-year
1-year	0.012*** (0.003)		-0.005 (0.008)	
3-year		0.034*** (0.009)		0.015 (0.015)

Notes. Samples are for workers aged 25-59 who stayed with the same employer for 4 years. Samples are consistent across specifications. Statistics are OLS coefficients from regressions of log earnings growth on firm log revenue growth. Column and row headings indicate whether growth is calculated as 1- or 3-year changes. Additional controls include single year of age indicators for men and for women; state, year, and four-digit industry indicators; and, in Manufacturing and Retail, interactions between year and three-digit industry indicators. Standard errors are clustered at the firm level and reported in parentheses (** p<0.01 *** p<0.001).

Table A3
IV Models for 3-Year Difference Samples

Instrument	Dependent variable = Change in log earnings over:			
	Manufacturing		Retail	
Change in log revenues over:	1-year	3-year	1-year	3-year
1-year		0.021*** (0.004)		0.016 (0.010)
3-year	0.011*** (0.002)		0.033*** (0.007)	
	Professional Services		Finance	
	1-year	3-year	1-year	3-year
1-year		0.021*** (0.003)		0.004 (0.008)
3-year	0.047** (0.017)		0.013 (0.015)	

Notes. Samples are for workers aged 25-59 who stayed with the same employer for 4 years. Samples are consistent across specifications. Statistics are IV coefficients from regressions of log earnings growth on firm log revenue growth over the same interval. Column headings indicate whether growth is calculated as 1- or 3-year changes. The length of the change in the instrument varies across rows; instrument dating is chosen so that the midpoint of the instrument change coincides with the midpoint of the independent variable change. Additional controls include single year of age indicators for men and for women; state, year, and four-digit industry indicators; and, in Manufacturing and Retail, interactions between year and three-digit industry indicators. Standard errors are clustered at the firm level and reported in parentheses (** p<0.01 *** p<0.001).