Occupation Changes and Wage Changes*

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Abstract

Some occupations pay substantially higher average wages than others. If workers switch occupations, do their wages increase as much as predicted by differences in averages? We answer this question using a two-way fixed effect framework, with occupation and worker fixed effects, in the United States between 1983 and 2013, finding that workers who move to an occupation paying 10% higher average wages can only expect their own wages to increase by 3.1%. This means that, although occupational average wage differences explain about a third of wage inequality, occupation fixed effects explain only about 3%. Although fixed effects are small relative to differences in average wages, workers do gain an economically significant benefit by moving to certain occupations. We estimate fixed effects precisely; because of their economic significance, we therefore report them individually, along with their correlation with occupational characteristics: higher-skill jobs generally have higher fixed effects. We also show that fixed effects are relatively stable over time.

Keywords: Occupations, Wage growth, Variance decomposition

JEL Codes: J24, J31, J62, J63

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1 Introduction

Workers switching occupations are likely motivated in part by the wages they will face in their new professions. Indeed, government career guidance websites often prominently report average pay by occupation as a central piece of information for job seekers.[1] However, workers with different skills, experience, and other characteristics sort into different occupations and make different moves. Therefore, the difference between average occupational wages may not reflect the wage gain or loss workers can expect from switching.

We explore how much of a wage premium workers can expect when they change occupation, as well as the implications of these premia and the factors that predict them. To do this, we estimate wage premia using a two-way fixed effect framework, following Abowd et al. (1999) [AKM]. AKM proposed estimating whether a firm is high-pay or low-pay not by its average pay but with the firm's fixed effect, after controlling for the workers' fixed effects: whether workers' pay goes up or down when they move between it and other firms. We analyze occupational fixed effects rather than those of employers: the extent to which workers' hourly wage goes up or down when switching from one occupation to another, among those who do change occupations. Our analysis uses data from the Survey of Income and Program Participation (SIPP), a large, nationally-representative survey in the United States with data from 1983 through 2013.[2] Households are surveyed multiple times over the course of up to four years, which allows us to observe individuals who change occupations, sometimes multiple times.

We find that occupational wage premia are substantially smaller than differences in average wages. Differences in average wages account for about 29% of total wage variance; the occupation fixed effects only account for 3% of total variance, while the sorting of high-fixed effect workers into high-fixed effect occupations accounts for a further 11%. Indeed, workers who move to an occupation with 10% higher average wages can expect their wage to increase by only about 3.1%. Despite their modest contribution to overall inequality, occupation fixed effects are economically meaningful. For example, workers moving from an average occupation to electrical engineering can expect their wages to increase by approximately 20%; those moving to farm work can expect a drop of approximately 19%. We also document that occupation fixed effects are strongly related to skill requirements: occupations with higher fixed effects tend to require greater cognitive skills and problem-solving ability, while occupations with lower fixed

[1]For example, see https://www.bls.gov/ooh in the United States and https://www.jobbank.gc.ca in Canada.
[2]For more information about the SIPP, see https://www.census.gov/programs-surveys/sipp/about.html.

effects tend to involve physically demanding or less desirable working conditions, suggesting that compensating differentials are not the primary determinant of premia. Occupational fixed effects are relatively stable over time for most occupations: fixed effects before 2000 are highly predictive of those in and after that year. Following Di Addario et al. (2023)'s firm-based methodology, we also find that past occupations have very little predictive power for present wages after controlling for individual heterogeneity. Finally, although differences in occupational averages explain a large share of the wage gap between demographic groups (for example, 42% of the gender wage gap), we find that occupational premia explain little of this difference (6.8% for gender), suggesting that reallocating workers to different occupations would change little about these gaps.

Our results are robust to a variety of concerns. First, occupations may be erroneously reported in survey data (Moscarini and Thomsson, 2007; Fujita et al., 2024), so some occupation changes may be spurious. To alleviate this concern, in our main specification, we restrict to occupational changes that occur at the same time as employer changes, which are less likely to be spurious. As a robustness check, we examine all occupation changes occurring in the middle of a single interview, including those that occur within firms; our results change little. Second, we perform a now-standard set of tests of the validity of the exogenous mobility assumptions that underlie a causal interpretation of the AKM model, such as pre-trends in wages before an occupation change and symmetry between up- and down-movers. Although our preferred interpretation of these premia is associational—they tell us the wage change workers who change occupations can expect—these tests generally do not reject the AKM assumptions. Third, AKM fixed effects are often imprecisely estimated, so they cannot be reported for individual firms, and estimated variances and covariances can be biased (see, for example, Andrews et al. (2008)). Because we examine only 318 occupations and over 50,000 workers in our data switch occupations, our occupational premia are precisely estimated, which we confirm using the variance correction proposed by Kline et al. (2020). Fourth, we present a new test to show that AKM fixed effects predict wage changes well out of sample: that is, the wage changes of workers going from occupation A to B, plus those of workers going from B to C, predicts the change for workers going from A to C. This result helps confirm that occupational fixed effects are a meaningful concept. Fifth, institutional barriers to mobility do not substantially affect our results, which we confirm by dropping workers in occupations that require licenses. Sixth, occupational movers may differ from those who don't move—for example, if most occupational movers are workers climbing a job ladder. To check if these differences drive our results, we estimate fixed effects restricting to only those who move to higher-wage occupations, and then separately those who move to lower-wage occupations. Results are only modestly different between the two groups.

1.1 Related Literature

Although we remain agnostic on the causal pathways that drive these fixed effects, the estimated premia are informative for several audiences. For workers, they provide a more accurate guide to the wage gains from occupational switches than raw average wages, which, we show, are heavily confounded by sorting. For researchers studying wage inequality, our decomposition clarifies that occupations account for less of the wage distribution than is commonly assumed. For policymakers considering interventions that reallocate workers across occupations, our results imply that the direct wage effects of such reallocation are modest.

Whether these premia reflect rents, compensating differentials, productivity differences, the characteristics of employers that populate certain occupations, or another factor remains an open question for future research. That said, our paper makes several contributions to the literature.

AKM frameworks and wage decompositions. Since Abowd et al. (1999), two-way fixed effect models have become a central tool for decomposing wages into worker and employer components. A large literature has applied this framework to firms (Card et al., 2013; Song et al., 2019; Bonhomme et al., 2019, 2023), and more recently to locations (Card et al., 2025) and industries (Card et al., 2024). However, the independent role of occupations—as distinct from firms or industries—has received remarkably little attention. The paper most closely related to ours is Hou and Milsom (2025); though it is not their main analysis, they estimate occupational and individual fixed effects in the United Kingdom, finding that occupation effects account for around 6% of earnings variance. We go substantially further: we provide occupation-by-occupation estimates of premia, examine which occupational characteristics predict high premia, test the stability of premia over time, develop a new out-of-sample validity test for AKM fixed effects, and apply the Kline et al. (2020) bias correction to confirm that our estimates are not driven by limited mobility.

Occupations and wage inequality. A large literature documents persistent wage disparities across occupations and links occupational change to wage inequality and polarization (Autor et al., 2003, 2006; Goos and Manning, 2007; Autor and Dorn, 2013; Acemoglu and Autor, 2011; Autor, 2019). These studies typically use average wages or task-based measures to characterize occupations, but they do not separate the occupation's direct contribution to pay from the sorting of different workers into different occupations. Our variance decomposition shows that this distinction matters: while between-occupation differences in average wages account for 29% of wage variance, occupational premia themselves account for only 3%, with sorting explaining a further 11%.

Gender wage gaps and occupational segregation. A long literature relates gender wage disparities to occupational segregation (Fawcett, 1918; Goldin, 2014; Blau and Kahn, 2017). Blau and Kahn (2017) find that occupations explain about 33% of the gender wage gap, while Goldin (2014) puts the figure at 22%. Our results challenge the magnitude of this channel: although occupational average wages account for 42% of the gender gap in our data, occupational premia account for only 6.8%. This suggests that equalizing the occupational distribution across genders would reduce the wage gap far less than raw averages imply.

Occupational mobility and human capital. Our work also relates to the literature on how wages change when workers switch occupations. Kambourov and Manovskii (2009, 2013) argue that human capital is largely occupation-specific. Gathmann and Schönberg (2010) and Cortes et al. (2024) find that wage changes upon switching depend on the similarity of skill requirements between origin and destination occupations. Altonji et al. (2016) show that initial occupational placement affects long-run earnings. By estimating occupation-level premia that control for individual heterogeneity, we provide a complementary perspective: the fixed effects tell us how much of the wage change upon switching reflects the destination occupation itself, rather than the worker's portable skills.

Validity of AKM models. Recent work has raised concerns about the interpretation of AKM fixed effects, particularly regarding limited mobility bias (Andrews et al., 2008; Kline et al., 2020) and violations of the exogenous mobility assumption (Bonhomme et al., 2023). We address these concerns through a comprehensive battery of specification tests, and we introduce a new out-of-sample test that, to our knowledge, has not been applied in the AKM literature.

Because we study only 318 occupations (rather than thousands of firms), our fixed effects are precisely estimated, and the Kline et al. (2020) correction confirms that limited mobility bias is negligible in our setting.

The remainder of this paper proceeds as follows. In Section 2, we introduce the data we use from SIPP and other sources, and describe the fixed effect methodology that underlies our results. In Section 3, we present estimates of occupational fixed effects and explore their importance in inequality, correlations with occupational characteristics, and other related results. Section 4 concludes with a discussion of the implications of our results.

2 Data and Methods

2.1 Data

To measure occupations' fixed effects, we use all the waves of the Survey of Income and Program Participation (SIPP) from National Bureau of Economic Research (2017). We use the panels from 1984 to 2008, which include data from 1983 to 2013. Each individual record contains detailed employment histories over a span of up to 4 years, including occupation codes, unique worker-level employer identifiers, average hours worked per week and average monthly earnings. The main outcome of interest in the two-way fixed effects model is the (log) average inflation-adjusted[3] hourly wage during an employment spell. An employment spell is defined as the period of time during which the individual works for the same employer. As earnings are measured at the month level and hours worked at the week level, we measure hourly wages by dividing the average monthly earnings during an employment spell by the product of 4.345—the average number of weeks in a month—and the average number of hours worked during the same employment spell.[4]

To ensure consistency and comparability across all the years of data, we use the occupations classification system developed in Dorn (2009) and Autor and Dorn (2013). This classification offers a standardized way to compare different occupations over time, taking into account

[3]We correct for inflation with the Consumer Price Index, from `https://fred.stlouisfed.org/series/CPIAUCSL`. The base year is 2002, around the middle of our dataset.

[4]Using the averages in earnings and hours worked helps in reducing measurement error associated with these variables. As we will detail below, we focus our analysis on individuals who change employers during the survey, and individuals tend to report fewer hours worked and earnings in the month before and after the change. The first and last month of an employment spell are therefore not considered when producing these variables.

variations in occupational titles and functions. It is important to note that there is a literature documenting errors in occupational codes in survey data; see for instance Kambourov and Manovskii (2013). To reduce the probability that an individual has one occupation but is misclassified as switching to another, we restrict our attention to individuals who change employers, as measurement error is less likely when occupation changes coincide with employer changes than when they occur mid-spell. Within this employer-change sample, occupation fixed effects are identified by those who also change occupations, while those who keep the same occupation help pin down individual fixed effects and improve precision. There still may be individuals who are erroneously classified as having switched occupations when, in fact, they did not—or, indeed, we may misidentify occupations in other ways, too. However, as long as measurement errors are independent of any errors in measured pay, we should still recover the fixed effect from being in a job that an interviewer would associate with a certain occupation, even if we do not identify the fixed effect of that occupation itself. As a robustness check, in Appendix Section D, we estimate occupation fixed effects considering all moves between occupations where both occupations are reported in the same interview (each interview covers a 4-month period), regardless of whether the person changed employers. This within-interview restriction greatly reduces our sample size, which is why we do not use it as our main specification.

We restrict the main sample to all individuals who experience at least one employer change during our observation window. We further limit the data to workers aged 25–65 who average at least 20 hours of work per week and whose spell-average wage exceeds the spell's average statutory minimum wage.[5] To ensure that extreme earners do not drive the dispersion of occupation effects, we trim the top and bottom 1 percent of the pay distribution. These filters yield a core sample of 184,987 employment spells for 74,658 unique workers. More details about the sample selections are provided in Appendix E.

Table 1 shows that all workers in the United States share many similarities with those we observe changing employers, and those changing occupations. Our primary sample consists of those changing employers at least once. We additionally present statistics on occupation switchers because this group alone drives occupation fixed effects calculations. These results, and all others unless otherwise noted, are weighted using SIPP individual weights. All groups work around 40-41 hours per week and are 53-54% male. The racial and ethnic compositions

[5]For each spell we compute the time-weighted average of the federal minimum wage in force and retain only those spells with mean earnings above that benchmark.

are also quite close, with White non-Hispanic individuals comprising roughly 75% of the full sample and 72-73% of switchers, and nearly identical proportions of Black non-Hispanic, and Hispanic individuals. Employer and occupation switchers are slightly younger, with an average age of 39 compared to 41 in the full sample. The primary difference is that the number of jobs is higher for those who switch employer (2.8) and occupation (3.0) than the full sample (1.6).

Table 1: Summary Statistics

	All Workers		**Employer Switchers**		**Occupation Switchers**	
	Mean	**SD**	**Mean**	**SD**	**Mean**	**SD**
Ln real wage	2.625	0.496	2.549	0.486	2.519	0.476
Average weekly hours	40.773	7.536	40.721	7.102	40.481	6.747
Number of jobs	1.618	1.038	2.841	1.174	2.953	1.242
Age	41.024	10.307	39.276	10.319	39.021	10.244
Male	0.529		0.544		0.542	
Female	0.471		0.456		0.458	
Ed: Less than high school	0.181		0.165		0.161	
Ed: High school	0.594		0.591		0.604	
Ed: College	0.225		0.244		0.235	
Race/Eth: White non-Hispanic	0.753		0.728		0.723	
Race/Eth: Black non-Hispanic	0.106		0.111		0.115	
Race/Eth: Hispanic	0.092		0.105		0.106	
Race/Eth: Other	0.049		0.055		0.055	
Num. of Individuals	261491		74658		52281	

Notes: Sample statistics from the Survey of Income and Program Participation. "Ln real wage" is the natural log of hourly wage at a worker's main job, adjusted for inflation to 2002 values. "Average weekly hours" is the average hours worked at their main job. "Number of jobs" is the number of distinct main jobs a worker has held. "Age" is average age while in the SIPP sample. Where there is variation, sex, education, and race/ethnicity are based on the modal value in the SIPP sample. "All Workers" includes those whose average age while in the sample is between 25 and 65, whose average wage is above the average federal minimum wage while they are in the sample, who average more than 20 hours per week, and whose wage is not in the top 1% or bottom 1%. "Employer Switchers" includes only those who we observe switching employers at the same time, and who satisfy similar restrictions to the full sample at the time of their move. "Occupation Switchers" is the subset of Employer Switchers who we additionally observe switching occupations.

To characterize each occupation, we use the Occupational Information Network (O*NET) data, a detailed source of information on occupational tasks and attributes, from National Center for O*NET Development (2021). O*NET details both the level and the importance of several factors for every occupation code. We extract the levels of four domains most relevant to our analysis—skills, abilities, knowledge and work styles—and map them to occupations. When several codes merge into a single occupation over time, we assign that occupation the mean of each O*NET item. The result is a vector of task requirements for every occupation in our sample.

2.2 Estimating Occupational Fixed Effects

To understand how much of the variation in wages can be attributed to occupations, we begin by decomposing individual wages within occupations. For an individual i in occupation j, the log wage y_{ij} can be expressed as:

$$y_{ij} = \underbrace{\bar{y}_j}_{\text{occupation mean}} + \underbrace{(y_{ij} - \bar{y}_j)}_{\text{individual deviation}}, \quad (1)$$

where $\bar{y}_j$ is the average log wage in occupation j.[6] This equation decomposes the total log wage into two components: the average wage within an occupation and the individual deviation from this average.

Using this decomposition, we can break down the variance of log wages into two parts:

$$\underbrace{\text{Var}_i\left(y_{ij}\right)}_{\text{total dispersion}} = \underbrace{\text{Var}_j\left(\bar{y}_j\right)}_{\text{between-occupation dispersion}} + \sum_j \omega_j \times \underbrace{\text{Var}_i\left(y_{ij}|i \in j\right)}_{\text{within-occupation-}j\text{ dispersion}},$$

where $\text{Var}_i(y_{ij})$ represents the total dispersion in log wages, $\text{Var}_j(\bar{y}_j)$ captures the variance between different occupations, and $\text{Var}_i(y_{ij}|i \in j)$ reflects the variance within a given occupation j. The term ω_j represents the weight of each occupation in the overall distribution: $\omega_j = N_j/N$ where N_j is the number of workers in occupation j among the workforce of N individuals.

In our data, the proportion of wage variance explained by the between-occupation component, $\frac{\text{Var}_j(\bar{y}_j)}{\text{Var}_i(y_{ij})}$, is approximately 29%. This suggests that a significant portion of wage dispersion is related to differences between occupations. The key question, therefore, is how large a slice of that 29% reflects true occupational effects as opposed to differences in the composition of workers across occupations.

To separate those channels, we estimate several two-way fixed effects regressions. For an individual i in occupation j, we estimate

$$y_{ijt} = \lambda_i + \lambda_j + \mathbf{x}'_{it}\beta + \epsilon_{ijt}, \quad (2)$$

where y_{ijt} is the log of the average hourly wage during the employment spell. The regression incorporates both individual and occupation fixed effects, represented by λ_i and λ_j, respectively.

[6] Note that all variables are a function of time t, and occupation j is additionally a function of individual i. Except where they are needed, we suppress these arguments to improve readability.

The individual fixed effect, λ_i, captures time-invariant worker attributes. The occupation fixed effect, λ_j, captures the wage premium or penalty attached to the occupation itself. In our preferred specification, $\mathbf{x}_{it}$ includes year fixed effects (corresponding to the year the worker switched employers) and age.[7] With worker heterogeneity held fixed, the estimated occupation effects $\hat{\lambda}_j$ tell us how much a worker's log wage would change if they moved to occupation j from a job with the sample-average occupation effect.

Estimated fixed effects are of independent interest, but we also perform a variance decomposition exercise as in AKM. Based on the model specified above, we decompose the variance in wages into three main components (for now, ignoring components related to $\mathbf{x}'_{it}$ and ϵ_{ijt} for tractability):

$$Var(y_{ij}) = Var(\lambda_i) + Var(\lambda_j) + 2\,Cov(\lambda_i, \lambda_j). \tag{3}$$

$Var(\lambda_i)$ represents the variance due to individual worker effects, capturing differences in wages due to individual characteristics such as skills and knowledge. $Var(\lambda_j)$ captures differences in wages due to characteristics of the occupations themselves. $Cov(\lambda_i, \lambda_j)$ captures the extent to which individual worker characteristics are correlated with occupational characteristics—for instance, the extent to which high-skilled workers tend to be in high-paying occupations.

Occupational fixed effects are estimated based on the wage changes we observe for people who move to or from each occupation. If few workers in our data set move to or from certain occupations, the fixed effects associated with those occupations will be estimated noisily. If that is the case, the estimated variance of occupational fixed effects includes the variance of the true fixed effects, but also includes the variance of this noise. To deal with this limited mobility bias, we use the correction outlined in Kline et al. (2020) and the accompanying package.[8] This method first estimates the occupation fixed effects using the full sample, then applies a leave-one-out correction to obtain unbiased estimates of the variance components.

Some care is needed when interpreting the fixed effects because not all workers can transition into any given occupation. For example, only some workers have the option of becoming electrical engineers—the occupation with the highest fixed effect. We should therefore consider estimated fixed effects to be local to the workers for whom a given occupation is in their choice

[7] Individual and year fixed effects cannot be combined with age because of perfect collinearity. We therefore include the terms $\left(\frac{\text{age}-40}{40}\right)^2$ and $\left(\frac{\text{age}-40}{40}\right)^3$ in the regressions.

[8] Available here: https://github.com/rsaggio87/LeaveOutTwoWay

set. In general, the difference in fixed effect between any two occupations A and B is identified through chains of occupations connecting them: the sum of fixed effect differences along all paths from A to B that do see movement. If there are workers who move between A and B, these are included in this set of chains, but such moves are not needed for identification. As a robustness check, in Appendix Table A.3, we re-estimate key outcomes after removing workers in occupations that universally require occupational licensing, finding that our results change little.

2.3 Validity of Fixed Effects Assumptions

Interpreting results from the AKM model as causal relies on the assumption of exogenous mobility, meaning that match effects are uncorrelated with occupations. Although we remain agnostic on the interpretation of fixed effects and cannot test exogenous mobility directly, we test several of its implications in Appendix C. Following Card et al. (2013), we find no differential pre- or post-trends in wages around occupation moves (Appendix Figures C.1 and C.2), approximate symmetry between wage gains for up-movers and wage losses for down-movers (Appendix Figure C.3), and no systematic relationship between residual errors and occupational fixed effects (Appendix Figure C.4). Following Card et al. (2024), changes in residuals are uncorrelated with changes in occupational fixed effects (Appendix Figure C.5). Overall, these tests do not reject the AKM assumptions.

In Appendix Section C.2, we also develop a new out-of-sample test: we show that the wage changes of workers going from occupation A to B, plus those going from B to C, predict the wage change for workers going from A to C. This confirms that occupational fixed effects are a meaningful concept, regardless of whether they are interpreted causally.

3 Results

3.1 Occupational Fixed Effects

We estimate Equation 2 using OLS, and report the distribution of the estimated occupational fixed effects (denoted by λ_j) in Appendix Figure B.1. The distribution is fairly symmetrical and clustered around zero, but some fixed effects are economically significant: about 10% of workers are in occupations with fixed effects above 0.15 (indicating that switching to these

Table 2: Occupations with High and Low Fixed Effects Among Large Occupations

High Fixed Effect Occupations	**Fixed Effect**	**Low Fixed Effect Occupations**	**Fixed Effect**
Electrical engineers	0.203	Cooks	-0.135
Industrial engineers	0.201	Bakers	-0.146
Operations and systems researchers, analysts	0.193	Guards and police, except public service	-0.149
Mechanical engineers	0.190	Waiters and waitresses	-0.151
Chief execs, public admin, legislators	0.184	Gardeners and groundskeepers	-0.155
Engineers and other professionals, n.e.c.	0.182	Cashiers	-0.157
Computer software developers	0.175	Food preparation workers	-0.162
Computer systems analysts, computer scientists	0.172	Misc food preparation, service workers	-0.169
Chemists	0.170	Personal service occupations, n.e.c	-0.185
Physical therapists	0.163	Farm workers, incl. nursery farming	-0.193

Notes: Occupational fixed effects are calculated using Equation 2. Fixed effects for the occupations with the ten highest and lowest values are listed here. Only occupations above the median size are included.

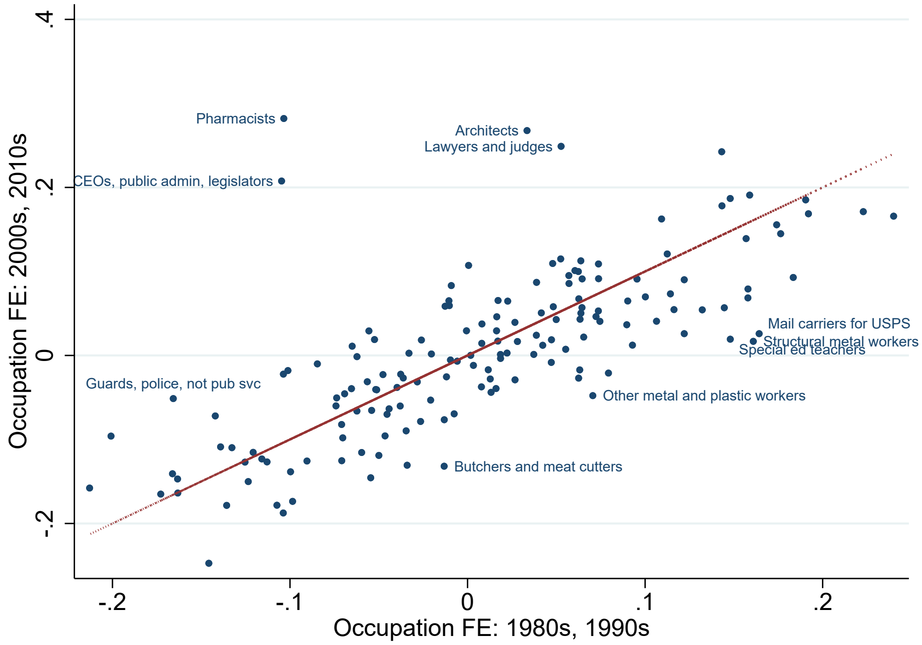
occupations from an average occupation is associated with at least a 15% wage gain), while 10% of workers are in occupations with fixed effects below -0.12. A strength of our setting is that we can report precisely-estimated fixed effects for each occupation (we further discuss precision below). Therefore, Table 2 lists the ten occupations with the highest and lowest occupational fixed effects.[9] The left side of the table shows the high fixed-effect occupations, which are associated with higher wages after controlling for individual characteristics. At the top of this list are electrical and industrial engineers, with fixed effects above 0.2. Other high fixed-effect occupations include chief executive officers and other types of engineers. The right side of the table lists low fixed-effect occupations. These occupations include farm workers and a variety of food service workers. Generally, we see high-skill occupations with high fixed effects, and vice versa; we return to this point below.

Figure 1 shows that these fixed effects are relatively stable over time.[10] Fixed effects along the x-axis are calculated using only data from the 1980s and 1990s, while those along the y-axis use data from the 2000s and 2010s. Each point corresponds to an occupation, with most clustered along the diagonal, for a correlation of 76%. Despite the overall stability, pharmacists and CEOs now see substantially higher recent fixed effects. On the other hand, structural metal workers and postal service mail carriers have seen their fixed effects decline since 2000. Although fixed effects changed little, these changes explain much of the between-occupation change in average wages. Occupation fixed effects increasing by 1 between the two periods is

[9]This and most other figures and tables that describe particular occupations are restricted to the largest 50% of occupations, where 93% of people are employed and estimates are most precise. All fixed effects, including those of smaller occupations, are shown in Appendix Tables A.5, A.6, A.7, and A.8.

[10]A similar graph for all occupations is shown in Appendix Figure B.2.

Figure 1: How Stable are Occupations' Fixed Effects?



Notes: One observation per occupation. Occupational fixed effects are calculated using Equation 2 two times: once including all observations through 1999 (as shown on the x-axis), and once including all observations in 2000 and later (y-axis). Only occupations above the median size are included. The red line indicates values where occupational fixed effects are unchanged (that is, the 45-degree line). Selected occupations are labeled.

associated with occupation average wages increasing by 0.609 (robust standard error .086), with an R^2 of .24.

To further explore which types of occupations have higher premiums, we regress the estimated fixed effects on various skill requirements from the O*NET database. Figure 2 illustrates the regression coefficients for different skills, with positive coefficients indicating that skills generally associated with "high-skill" work, like "Time Management", "Complex Problem Solving", and "Critical Thinking", are associated with higher occupational fixed effects. Conversely, skills such as "Operation and Control", "Equipment Maintenance", and "Repairing" are linked to occupations with lower fixed effects.

Notably, these patterns likely run counter to compensating differentials, if we believe the skill requirements associated with high fixed effects in Figure 2 are those that would make a job interesting and therefore desirable. A desire for "interesting" work, of course, may reflect our bias as economists. However, we also note that high fixed effects are associated with physically unpleasant work contexts, such as extreme temperatures, exposure to contaminants, and risk of minor burns and cuts. This suggests the labor market compounds rather than compensates for poor working conditions, with unpleasant occupations offering both worse amenities and lower wage premia.

3.2 Wages and Fixed Effects

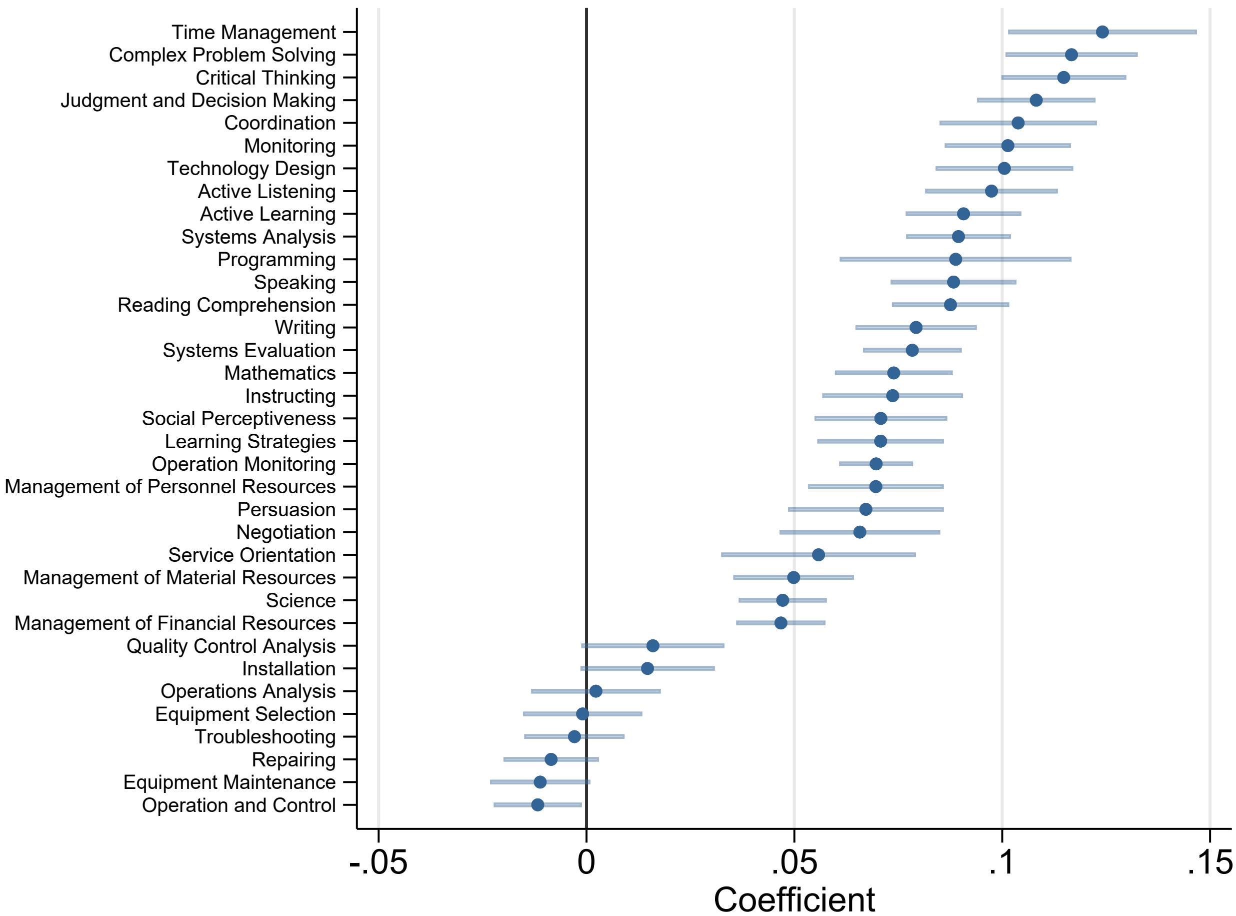
We have seen that high-skill occupations generally have high fixed effects; as we might expect, then, high fixed-effects occupations are associated with higher pay. The scatter plot in Figure 3 shows the strong positive relationship between an occupation's fixed effects and the occupation-wide average log real wage. However, note that high-wage occupations are also more likely to employ workers with high average individual fixed effects (red squares) than low average fixed effects (blue circles). Likewise, variation in occupation fixed effect is substantially smaller than variation in log wages. The best-fit line shown has a slope of around 0.31, indicating that workers moving to an occupation where average wages are 10% higher should only expect their own wages to increase by about 3.1%. We return to these points in Section 3.3. Despite the strong relationship between occupational fixed effects and wages, some occupations have average wages that are either above or below what would be predicted based on their fixed effects alone; Appendix Table A.1 shows the largest differences between a ranking based on occupational average wage and occupational fixed effect.

3.3 Variance Decomposition

To what extent do occupational premia account for overall wage inequality? To answer this, we run a simple decomposition exercise by estimating Equation 3. The variance in wages is decomposed into four main terms: one related to the dispersion of individual fixed effects, one related to the dispersion of occupational fixed effects, one related to the sorting of high-ability workers into high-effect occupations, and one capturing other variation, including noise. The results from four specifications are provided in Table 3.

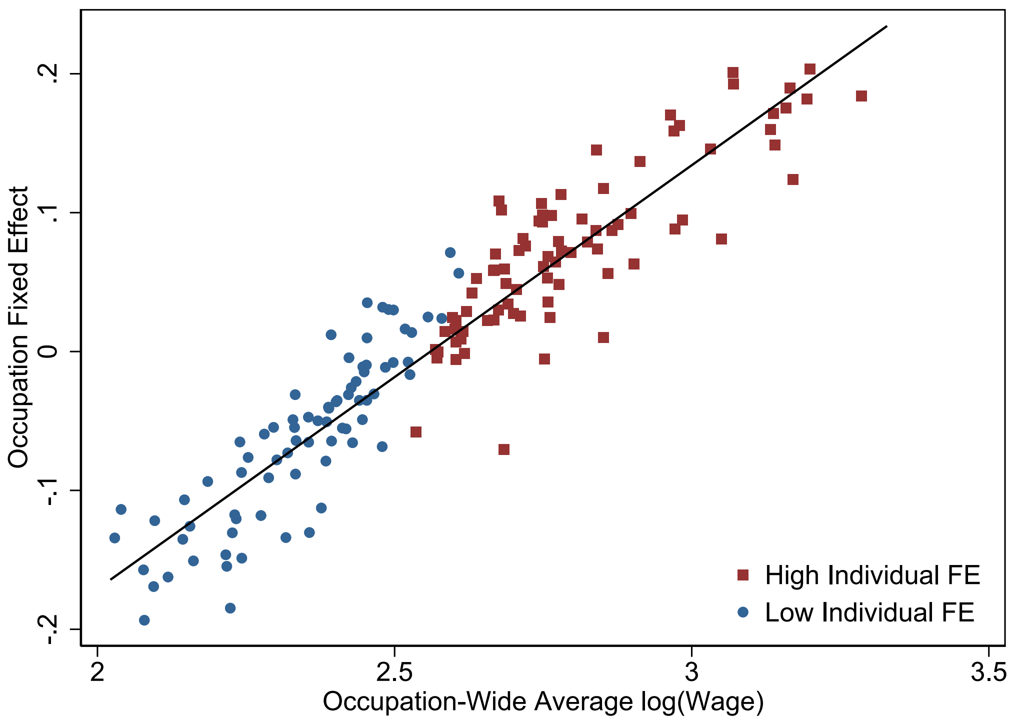
Panel A shows that, as expected, wages are fairly consistent over time for individuals: individual fixed effects, by themselves, explain 79% of overall wage variance. Occupations by themselves also have important explanatory power, explaining 29% of total variance. However, controlling for individual effects, occupations are much less important: in Panel C, we find that occupation effects only explain around 3.2% of the variance. The sorting component, which

Figure 2: Relation between Various Skills and Occupation Fixed Effects



Notes: Each point represents the coefficient from a simple regression with one observation per occupation. The dependent variable is the occupation's fixed effect; the independent variable is a standardized measure of an O*NET skill. Bars represent heteroskedasticity-robust 95% confidence intervals. Regressions are weighted by size of the occupation.

Figure 3: Correlation of Wages with Occupation Fixed Effects



Notes: One observation per occupation. The x-axis measures the average log real wage in the occupation; the y-axis measures the occupation's occupational fixed effect. Red squares are occupations in which average individual fixed effects are above the average of all occupations; blue circles are occupations in which it is below average. Only occupations above the median size are shown, but the best-fit line is based on all occupations, weighted by size. The coefficient (and robust standard error) on the best-fit line is 0.305 (0.009).

captures the selection of individuals into high- and low-premium occupations, explains a further 11.6%. Panel D shows that including control variables—our preferred specification—does not substantially change this result: occupations explain 3.1%, while sorting explains 11.3%. Overall, these results indicate that individual characteristics are the primary drivers of wage variance, including between occupations. In Appendix Table A.2, we find that occupation fixed effects also explain little of the increase in inequality over the past few decades. Comparing our results to those of Bonhomme et al. (2023), we find that occupational fixed effects explain substantially less variation than do firm fixed effects in the United States (around 5 or 6%).

Table 3: Variance Decomposition

	Var(Wages) =	Var(Ind. FE)	+ Var(Occ. FE)	+ 2 × Cov(Ind. FE, Occ. FE)	+ Other terms
			Panel A: Individual Fixed-Effects		
Component	.2513	.1978			.0535
Proportion	1	.7871			.2129
			Panel B: Occupations Fixed-Effects		
Component	.2513		.0727		.1787
Proportion	1		.2891		.7109
			Panel C: All (without controls)		
Component	.2513	.1629	.0079	2 × .0145	.0515
Proportion	1	.6482	.0315	2 × .0578	.2047
			Panel D: All (with full set of controls)		
Component	.2513	.1643	.0077	2 × .0142	.0509
Proportion	1	.6536	.0308	2 × .0565	.2026

Notes: Results of a variance decomposition based on Equation 3. "Var(Wages)" measures the overall variance of log real wages, with one observation per person, per employer. "Var(Ind. FE)" measures the variance of individual fixed effects. Var(Occ. FE) measure the variance of occupation fixed effects. "2 × Cov(Ind. FE, Occ. FE)" measures twice the covariance between individual and occupation fixed effects. "Other terms" includes the variance of the error term, as well as all terms related to other covariates, if they are included. All results weighted using SIPP weights. Panel A is based on a regression including only individual fixed effects. Panel B includes only occupational fixed effects. Panel C includes both types of fixed effects. Panel D adds year fixed effects, as well as controls for age^2 and age^3.

When there is insufficient movement of individuals between occupations or firms, fixed effects can be imprecisely estimated, leading to biased variances and covariances (Andrews et al., 2008). Because we see many movers in and out of most occupations, we would not expect limited mobility bias to be a major issue. Indeed, in Appendix Figure B.3 we show that estimated variance terms vary little when restricting to occupations of various sizes. To more formally adjust for limited mobility bias, Panel B of Table 4 applies the unbiased estimator proposed by Kline et al. (2020).[11] Although the variance of individual effects is substantially lower with the unbiased estimator (48%) than with the plug-in estimator (63%), our primary outcomes of interest are similar: variance of occupation fixed effects is around 3% and 2 times covariance is around 12% using both methodologies. This suggests that each occupation fixed effect—particularly that for larger occupations—is estimated precisely enough to be reported individually, as we do above.

Table 4: Comparison with the Unbiased Approach

	Var(Wages) =	Var(Ind. FE)	+ Var(Occ. FE)	+ 2 × Cov(Ind. FE, Occ. FE)	+ Other terms
Panel A: No controls, plug-in approach, unweighted					
Component	.2540	.1608	.0081	2 × .0149	.0555
Proportion	1	.6329	.0318	2 × .0585	.2183
Panel B: No controls, unbiased approach, unweighted					
Component	.2540	.1209	.0075	2 × .0152	.0952
Proportion	1	.4760	.0295	2 × .0598	.3749

Notes: See notes for Table 3. All results are unweighted, with no additional control variables, because the Kline et al. (2020) estimator does not allow for weights or control variables. Panel A is based on an OLS regression; Panel B is based on variances and covariances estimated following Kline et al. (2020).

Our results are robust to a variety of other specifications. First, our usual methodology reduces spurious moves by restricting to those when workers also change employers. However, this restriction ignores within-employer occupation changes, and may still include spurious occupation changes. In Appendix D, we instead examine occupation changes that are reported within an interview regardless of employer change; after the Kline et al. (2020) correction, we estimate similar variances. Second, we may worry that occupational licensing can restrict which occupations workers can enter, thereby biasing our results; however, in Appendix Table A.3, we find that removing occupations that require licensing barely changes our estimates. Third, asymmetries could complicate our analysis—for example, if our sample consists mainly of high-ability workers moving up to better occupations, whose gains aren't the opposite of those who move down. We therefore conduct the variance decomposition separately for two subsamples: workers who moved to an occupation with (1) higher average wages, and (2) lower average

[11] Panel A shows the unweighted decomposition using the usual OLS ("plug-in") approach without any controls, while Panel B shows results after applying the unbiased estimator. We display these results separately from Table 3 because our usual procedures weight estimates using the SIPP's individual weights, while the Kline et al. (2020) does not allow for weights. We therefore do not use weights in either panel of that table.

wages. In Appendix Table A.4, occupation fixed effects explain 4.4% of wage variance among upward movers and 2.4% among downward movers—a modest difference, with our main result near their average.

3.4 Incorporating Past Occupations

Despite occupations' modest role in explaining present wages after accounting for individual heterogeneity, we might expect that some occupations could propel workers to higher-wage jobs in the future—for example, by giving workers the skills they need to succeed in other jobs. To explore whether past occupations account for current wages, we perform a decomposition that incorporates both current and previous occupation fixed effects. The results are presented in Table 5; each panel uses a sample that is restricted to those who are observed changing employers twice to allow for computation of both current and previous occupation fixed effects. Panel A shows that in this sample, similar to our main one, current occupation alone explains about 26% of wage variation; Panel B shows that previous occupation has a similar explanatory power, explaining over 20% of variation. However, in Panel C, we see that past occupation essentially proxies for individual attributes: including individual effects reduces the explanatory power of previous occupations to below 1% of the variation. Inclusion of past occupation fixed effects has little influence on our baseline results: in Panel D, current occupation explains around 3% of variation, while past occupation still explains under 1%. In all, past occupation explains very little about current wages.

3.5 Wage Gaps

To examine how occupational premia account for various wage gaps, we decompose wage differences across several observable characteristics that are often associated with wage disparities: gender (men vs. women), education level (college degree or higher vs. high school or less), age group (45-65 vs. 25-44), and race/ethnicity (White non-Hispanic vs. Black or Hispanic). Results are shown in Table 6. The first row of that table shows the raw log wage differences, which are substantial across all categories (for example, 0.177 for men vs. women, 0.417 for college vs. high school education).

In the second row, we calculate the average log wage for each worker's occupation; we then compare the average of that statistic between groups and report both the occupational wage gap

Table 5: Variance Decomposition with Previous Occupations

	Var(Wages) =	Var(Ind. FE)	+ Var(Occ. FE)	+ Var(Prev. Occ. FE)	+ Other terms
Panel A: Occupations Fixed-Effects					
Component	.2459		.0637		.1822
Proportion	1		.259		.741
Panel B: Previous Occupations Fixed-Effects					
Component	.2459			.0503	.1956
Proportion	1			.2046	.7954
Panel C: Previous Occupations + Individual Fixed Effects					
Component	.2459	.2228		.0019	.0212
Proportion	1	.9058		.0077	.0862
Panel D: All					
Component	.2459	.1839	.0082	.0016	.0522
Proportion	1	.7478	.0334	.0064	.2123

Notes: See notes for Table 3. "Var(Prev. Occ. Fe)" measures the variance of the fixed effect for previous occupation. "Other terms" includes all covariance terms, terms related to other covariates, as well as the variance of the error term. Panel A includes only the current occupation fixed effect. Panel B includes only the previous occupation fixed effect. Panel C includes both individual fixed effects and previous occupation fixed effects. Panel D includes individual fixed effects as well as fixed effects for both current and previous occupation. All results only include those included in the sample for Panel D—that is, those with non-missing current and previous occupation, in a connected set including both variables.

and the fraction of the total wage gap explained by these averages. These gaps are substantial, except in the case of age; it is these gaps that previous literature has measured to suggest that occupations have important explanatory power for wage gaps. For example, men's wages are about 19.4% ($= e^{0.177} - 1$) higher than than women's; 42% of this gap is accounted for by average occupational wages, because men are in occupations that pay about 7.7% more than women's occupations. Naively, one could therefore argue that the gender wage gap would be cut in half if men and women worked in the same occupations. However, as we have shown above, workers switching to a different occupation might not gain the difference in average wages. Thus, in the third row, we compare the average fixed effect for occupations held by each group and report both the gap in fixed effects and the percent of the total wage gap explained by fixed effects. This final row is conceptually similar to what Card et al. (2016) refer to as sorting in the context of firms and gender wage gaps. These gaps are all substantially smaller than the difference in average log wage. Thus, for example, switching men and women to be in the same occupations would reduce their wage gap by approximately 1.2 percentage points, or 6.8% of the total gap. Similarly, occupational averages explain more than half of the wage gap by education and race/ethnicity, but occupational fixed effects account for less than 20%

of each gap.

Some care is needed in interpreting these gaps, because worker fixed effects include anything fixed about the worker at the time of the move. This includes their underlying skill level, but also their education, any discrimination they face, and so on. For example, our results imply that moving men and women to the same occupations would have little effect on the wage gap if these moves are restricted to those workers may actually make. However, it is possible that the effect would be greater if there were changes in the occupations available to different types of workers, such as due to changing education patterns or a reduction in discrimination.

Table 6: Comparison of Wage Gaps Across Different Groups

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Male vs Female		**College vs ≤HS**		**Age 45-65 vs 25-44**		**White NH vs Black, Hisp.**	
	Gap	%	Gap	%	Gap	%	Gap	%
1. Wage gap	0.177	100.0	0.417	100.0	0.120	100.0	0.196	100.0
2. Occupation wage gap	0.074	42.0	0.243	58.3	0.010	7.9	0.135	68.7
3. Occupation FE gap	0.012	6.8	0.071	17.1	0.003	2.2	0.039	19.8

Notes: Each odd-numbered column reports gaps in wage between two demographic groups: men versus women; those with a college degree versus those with no more than high school; those aged 45-65 versus those aged 25-44; and non-Hispanic White workers versus Black and Hispanic workers. Row 1 shows the raw difference in average log wage between the groups. Row 2 shows the difference between the average log wage in occupations held by each group. Row 3 shows the difference between the average fixed effect for the occupations held by each group. Even-numbered columns report the percent of the total wage gap (from row 1) that is explained by the wage gap reported in the previous column of the same row.

4 Discussion

Research has long shown that workers in some occupations are paid substantially more than others. In this paper, however, we show that workers moving between occupations close little of this gap: moving to an occupation where average wages are 10% higher is associated with a wage gain of approximately 3.1%. This modest gain means that occupational premia only explain about 3% of total wage variance in the US labor market. Because the premia are relatively small, they do little to explain gaps in earnings by observable characteristics like race and gender. Nevertheless, the remaining occupational premia are economically meaningful, and tend to be highest in high-skill jobs.

Ultimately, we estimate occupational premia with enough precision that they can be useful for job seekers looking to predict what occupation will lead to the largest wage gain. Our finding that occupational wage averages do not reliably predict wage gains also means that researchers should use caution when using occupations as an outcome as a proxy for wages, as is common in economic history research. Additionally, our analysis sheds new light on the consequences of occupational mobility, and the relative importance of fixed individual and occupational characteristics in shaping the occupational wage structure. The results can serve to constrain structural models of occupational mobility, which should incorporate the modest but systematic wage changes we see when workers move occupations. Future work should improve this constraint by attempting to understand why some occupations systematically pay the same worker more than others; given that seemingly more desirable occupations tend to have higher fixed effects, the answer likely goes beyond compensating differentials. For example, is the difference due more to the characteristics of the workers who switch, the employers who choose their wages, the nature of the job, or something else? Such work can help researchers, policymakers, and workers have a better understanding of the consequences of occupational changes.

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Occupation Changes and Wage Changes

Online Appendix

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A Additional Tables

Table A.1: Differences between Fixed Effects and Average Wages

Occupation	Average ln(Wage)	Wage Rank	Fixed Effect	FE Rank	Diff
Panel A: High Wage, Low Fixed Effect					
Surveryors, cartographers, mapping scientists/techs	2.684	58	-0.071	129	71
Subject instructors, college	2.851	26	0.010	83	57
Fire fighting, fire prevention, and fire inspection occs	2.752	43	-0.005	92	49
Auto body repairers	2.536	86	-0.058	121	35
Electricians	2.762	39	0.025	72	33
Automobile mechanics and repairers	2.479	96	-0.068	128	32
Purchasing managers, agents, and buyers, n.e.c.	2.859	24	0.056	53	29
Financial service sales occupations	2.903	20	0.063	47	27
Recreation facility attendants	2.357	123	-0.130	146	23
Management analysts	3.050	12	0.081	35	23
Panel B: Low Wage, High Fixed Effect					
Supervisors of food preparation and service	2.333	128	-0.031	104	-24
Actors, directors, and producers	2.747	47	0.107	21	-26
Administrative support jobs, n.e.c.	2.498	91	0.030	65	-26
Payroll and timekeeping clerks	2.490	93	0.030	64	-29
Correspondence and order clerks	2.480	95	0.032	63	-32
Machine operators, n.e.c.	2.393	116	0.012	82	-34
Management support occupations	2.594	79	0.071	43	-36
Roofers and slaters	2.454	98	0.035	61	-37
Postal clerks, exluding mail carriers	2.680	59	0.102	22	-37
Insulation workers	2.676	60	0.109	20	-40

Notes: Only the 159 occupations above the median size are included in this analysis. For each occupation in the table, we present, for the workers in that occupation in our sample, the average of the natural log of real hourly wage; the rank of that wage, where 1 indicates the highest wage; the occupation's fixed effect; the rank of that effect, where 1 indicates the highest effect; and the difference between wage rank and fixed effect rank. All 159 occupations are sorted by this difference; the top 10 and bottom 10 are included in this table.

Table A.2: Variance Decomposition: Different Time Periods

	Var(Wages) =	Var(Ind. FE)	+ Var(Occ. FE)	+ 2 × Cov(Ind. FE, Occ. FE)	+ Noise
Panel A: All (with full set of controls)—before 2000					
Component	.2358	.1582	.0079	2 × .0124	.0448
Proportion	1	.6708	.0337	2 × .0527	.1901
Panel B: All (with full set of controls)—2000 and later					
Component	.264	.1653	.0095	2 × .0152	.0587
Proportion	1	.6263	.0361	2 × .0575	.2225

Notes: See notes for Table 3. Panel A includes only observations from before 2000; Panel B includes only observations from 2000 and later.

Table A.3: Variance Decomposition Without Universally Licensed Occupations

	Var(Wages) =	Var(Ind. FE)	+ Var(Occ. FE)	+ 2 × Cov(Ind. FE, Occ. FE)	+ Noise
Panel A: Excluding universally licensed occupations					
Component	.2496	.1627	.0074	2 × .014	.0515
Proportion	1	.6519	.0297	2 × .056	.2063

Notes: See notes for Table 3. Occupations identified as universally licensed by Johnson and Kleiner (2020) are not included in this analysis.

Table A.4: Variance Decomposition For Upward and Downward Movers

	Var(Wages) =	Var(Ind. FE)	+ Var(Occ. FE)	+ 2 × Cov(Ind. FE, Occ. FE)	+ Noise
Panel A: Upward movers					
Component	.2428	.1516	.0108	2 × .0135	.0535
Proportion	1	.6245	.0444	2 × .0554	.2202
Panel B: Downward movers					
Component	.2496	.1751	.0059	2 × .0103	.0481
Proportion	1	.7015	.0235	2 × .0412	.1926

Notes: See notes for Table 3. Panel A includes only upward movers (individuals who move to occupations with strictly higher average wages); Panel B includes only downward movers (individuals who move to occupations with strictly lower average wages).

Table A.5: Statistics on Each Occupation, Part 1

Code	Occupation Title	Average Wage	Fixed Effect
4	Chief executives, public administrators, and legislators	3.286	0.184
7	Financial managers	3.031	0.146
8	Human resources and labor relations managers	2.900	0.093
13	Managers and specialists in marketing, advert., PR	2.972	0.088
14	Managers in education and related fields	2.866	0.087
15	Managers of medicine and health occupations	2.913	0.137
18	Managers of properties and real estate	2.579	0.024
19	Funeral directors	2.779	0.098
22	Managers and administrators, n.e.c.	2.824	0.079
23	Accountants and auditors	2.841	0.074
24	Insurance underwriters	2.883	0.104
25	Other financial specialists	2.876	0.091
26	Management analysts	3.050	0.081
27	Personnel, HR, training, and labor rel. specialists	2.709	0.073
28	Purchasing agents and buyers of farm products	2.601	-0.048
29	Buyers, wholesale and retail trade	2.601	0.016
33	Purchasing managers, agents, and buyers, n.e.c.	2.859	0.056
34	Business and promotion agents	2.466	-0.067
35	Construction inspectors	2.847	0.048
36	Inspectors and compliance officers, outside	2.745	0.058
37	Management support occupations	2.594	0.071
43	Architects	2.970	0.159
44	Aerospace engineers	3.319	0.237
45	Metallurgical and materials engineers	3.092	0.168
47	Petroleum, mining, and geological engineers	3.263	0.188
48	Chemical engineers	3.160	0.200
53	Civil engineers	3.132	0.160
55	Electrical engineers	3.199	0.203
56	Industrial engineers	3.069	0.201
57	Mechanical engineers	3.166	0.190
59	Engineers and other professionals, n.e.c.	3.194	0.182
64	Computer systems analysts and computer scientists	3.138	0.172
65	Operations and systems researchers and analysts	3.070	0.193
66	Actuaries	3.229	0.022
68	Mathematicians and statisticians	2.872	0.242
69	Physicists and astronomists	3.078	0.111
73	Chemists	2.964	0.170
74	Atmospheric and space scientists	2.907	0.292
75	Geologists	3.023	0.062
76	Physical scientists, n.e.c.	3.016	0.204
77	Agricultural and food scientists	2.604	-0.044
78	Biological scientists	2.769	-0.037
79	Foresters and conservation scientists	2.820	-0.038
83	Medical scientists	2.946	0.035
84	Physicians	2.984	0.095
85	Dentists	3.329	0.091
86	Veterinarians	3.029	0.276
87	Optometrists	2.982	0.025
88	Podiatrists	2.823	-0.068
89	Other health and therapy occupations	3.025	0.312
95	Registered nurses	2.852	0.118
96	Pharmacists	3.171	0.124
97	Dieticians and nutritionists	2.710	0.023
98	Respiratory therapists	2.794	0.062
99	Occupational therapists	3.091	0.239
103	Physical therapists	2.979	0.163
104	Speech therapists	2.937	0.203
105	Therapists, n.e.c.	2.648	0.089
106	Physicians' assistants	2.747	0.033
154	Subject instructors, college	2.851	0.010
155	Kindergarten and earlier school teachers	2.386	-0.051
156	Primary school teachers	2.705	0.045
157	Secondary school teachers	2.743	0.094
158	Special education teachers	2.758	0.068
159	Teachers, n.e.c.	2.667	0.058
163	Vocational and educational counselors	2.685	0.059
164	Librarians	2.691	0.034
165	Archivists and curators	2.567	-0.026
166	Economists, market and survey researchers	2.997	0.106
167	Psychologists	2.758	0.053
169	Social scientists and sociologists, n.e.c.	2.771	0.062
173	Urban and regional planners	3.095	0.148
174	Social workers	2.638	0.053
176	Clergy and religious workers	2.569	0.001
177	Welfare service workers	2.241	-0.107
178	Lawyers and judges	3.140	0.149
183	Writers and authors	2.828	0.058
184	Technical writers	2.997	0.223
185	Designers	2.712	0.025
186	Musicians and composers	2.667	0.126

Notes: Statistics presented for all occupations, regardless of occupation size. "Code" and "Occupation Title" are based on codes created by Autor and Dorn (2013). All statistics based on our primary sample. "Average Wage" is the average log real hourly wage for each occupation. "Fixed Effect" is the occupation's fixed effect, calculated as described in the text.

Table A.6: Statistics on Each Occupation, Part 2

Code	Occupation Title	Average Wage	Fixed Effect
187	Actors, directors, and producers	2.747	0.107
188	Painters, sculptors, craft-artists, and print-makers	2.700	0.044
189	Photographers	2.571	0.043
193	Dancers	2.598	0.203
194	Art/entertainment performers and related occs	2.517	-0.026
195	Editors and reporters	2.840	0.145
198	Announcers	2.401	0.078
199	Athletes, sports instructors, and officials	2.601	0.071
203	Clinical laboratory technologies and technicians	2.630	0.042
204	Dental hygienists	2.890	0.114
206	Radiologic technologists and technicians	2.781	0.072
207	Licensed practical nurses	2.529	0.014
208	Health technologists and technicians, n.e.c.	2.465	-0.031
214	Engineering technicians	2.721	0.076
217	Drafters	2.749	0.093
218	Surveryors, cartographers, mapping scientists/techs	2.684	-0.071
223	Biological technicians	2.446	-0.035
224	Chemical technicians	2.729	-0.000
225	Other science technicians	2.593	0.122
226	Airplane pilots and navigators	2.914	-0.069
227	Air traffic controllers	3.025	0.008
228	Broadcast equipment operators	2.727	0.153
229	Computer software developers	3.158	0.175
233	Programmers of numerically controlled machine tools	2.758	0.003
234	Legal assistants and paralegals	2.670	0.070
235	Technicians, n.e.c.	2.764	0.098
243	Sales supervisors and proprietors	2.603	0.022
253	Insurance sales occupations	2.656	0.022
254	Real estate sales occupations	2.751	0.061
255	Financial service sales occupations	2.903	0.063
256	Advertising and related sales jobs	2.668	0.059
258	Sales engineers	3.068	0.078
274	Salespersons, n.e.c.	2.758	0.036
275	Retail salespersons and sales clerks	2.377	-0.113
276	Cashiers	2.078	-0.157
277	Door-to-door sales, street sales, and news vendors	2.355	-0.047
283	Sales demonstrators, promoters, and models	2.309	-0.113
303	Office supervisors	2.688	0.049
313	Secretaries and stenographers	2.447	-0.011
315	Typists	2.329	-0.049
316	Interviewers, enumerators, and surveyors	2.389	-0.041
317	Hotel clerks	2.078	-0.138
318	Transportation ticket and reservation agents	2.526	-0.017
319	Receptionists and other information clerks	2.281	-0.059
326	Correspondence and order clerks	2.480	0.032
328	Human resources clerks, excl payroll and timekeeping	2.518	0.011
329	Library assistants	2.366	-0.075
335	File clerks	2.334	-0.064
336	Records clerks	2.441	-0.035
337	Bookkeepers and accounting and auditing clerks	2.454	0.010
338	Payroll and timekeeping clerks	2.490	0.030
344	Billing clerks and related financial records processing	2.423	-0.004
346	Mail and paper handlers	2.770	0.092
347	Office machine operators, n.e.c.	2.311	-0.046
348	Telephone operators	2.352	-0.072
354	Postal clerks, exluding mail carriers	2.680	0.102
355	Mail carriers for postal service	2.749	0.098
356	Mail clerks, outside of post office	2.302	-0.078
357	Messengers	2.384	-0.079
359	Dispatchers	2.446	-0.049
364	Shipping and receiving clerks	2.389	-0.040
365	Stock and inventory clerks	2.333	-0.088
366	Meter readers	2.473	-0.036
368	Weighers, measurers, and checkers	2.436	-0.014
373	Material recording, sched., prod., plan., expediting cl.	2.598	0.025
375	Insurance adjusters, examiners, and investigators	2.615	0.014
376	Customer service reps, invest., adjusters, excl. insur.	2.498	-0.008
377	Eligibility clerks for government prog., social welfare	2.560	0.101
378	Bill and account collectors	2.427	-0.026
379	General office clerks	2.403	-0.035
383	Bank tellers	2.254	-0.076
384	Proofreaders	2.310	-0.063
385	Data entry keyers	2.355	-0.065
386	Statistical clerks	2.401	0.023
387	Teacher's aides	2.231	-0.118
389	Administrative support jobs, n.e.c.	2.498	0.030
405	Housekeepers, maids, butlers, and cleaners	2.096	-0.122
408	Laundry and dry cleaning workers	2.029	-0.134
417	Fire fighting, fire prevention, and fire inspection occs	2.752	-0.005
418	Police and detectives, public service	2.815	0.095

Notes: See notes for Table A.5.

Table A.7: Statistics on Each Occupation, Part 3

Code	Occupation Title	Average Wage	Fixed Effect
423	Sheriffs, bailiffs, correctional institution officers	2.608	0.056
425	Crossing guards	2.302	-0.242
426	Guards and police, except public service	2.243	-0.149
427	Protective service, n.e.c.	2.374	-0.189
433	Supervisors of food preparation and service	2.333	-0.031
434	Bartenders	2.227	-0.131
435	Waiters and waitresses	2.162	-0.151
436	Cooks	2.144	-0.135
439	Food preparation workers	2.119	-0.162
444	Miscellanious food preparation and service workers	2.095	-0.169
445	Dental Assistants	2.394	-0.064
447	Health and nursing aides	2.242	-0.087
448	Supervisors of cleaning and building service	2.412	-0.055
450	Superv. of landscaping, lawn service, groundskeeping	2.435	-0.022
451	Gardeners and groundskeepers	2.218	-0.155
453	Janitors	2.234	-0.120
455	Pest control occupations	2.338	-0.218
457	Barbers	2.212	-0.133
458	Hairdressers and cosmetologists	2.275	-0.118
459	Recreation facility attendants	2.357	-0.130
461	Guides	2.238	-0.095
462	Ushers	2.336	-0.134
464	Baggage porters, bellhops and concierges	2.379	-0.124
466	Recreation and fitness workers	2.422	-0.031
468	Child care workers	2.156	-0.126
469	Personal service occupations, n.e.c	2.224	-0.185
470	Supervisors of personal service jobs, n.e.c	2.407	0.001
471	Public transportation attendants and inspectors	2.753	-0.025
472	Animal caretakers, except farm	2.274	-0.197
473	Farmers (owners and tenants)	2.403	0.001
479	Farm workers, incl. nursery farming	2.079	-0.193
488	Graders and sorters of agricultural products	2.146	-0.053
489	Inspectors of agricultural products	2.645	0.008
496	Timber, logging, and forestry workers	2.402	-0.036
503	Supervisors of mechanics and repairers	2.797	0.071
505	Automobile mechanics and repairers	2.479	-0.068
507	Bus, truck, and stationary engine mechanics	2.573	-0.000
508	Aircraft mechanics	2.776	0.079
509	Small engine repairers	2.378	0.004
514	Auto body repairers	2.536	-0.058
516	Heavy equipement and farm equipment mechanics	2.603	-0.006
518	Industrial machinery repairers	2.667	0.023
519	Machinery maintenance occupations	2.589	0.019
523	Repairers of industrial electrical equipment	2.571	-0.005
525	Repairers of data processing equipment	2.776	0.048
526	Repairers of household appliances and power tools	2.443	-0.104
527	Telecom and line installers and repairers	2.780	0.113
533	Repairers of electrical equipment, n.e.c.	2.612	0.021
534	Heating, air conditioning, and refrigeration mechanics	2.617	-0.001
535	Precision makers, repairers, and smiths	2.467	-0.006
536	Locksmiths and safe repairers	2.514	-0.013
539	Repairers of mechanical controls and valves	2.504	-0.074
543	Elevator installers and repairers	2.879	0.149
544	Millwrights	2.727	0.000
549	Mechanics and repairers, n.e.c.	2.523	-0.008
558	Supervisors of construction work	2.839	0.087
563	Masons, tilers, and carpet installers	2.556	0.025
567	Carpenters	2.603	0.007
573	Drywall installers	2.621	0.029
575	Electricians	2.762	0.025
577	Electric power installers and repairers	2.776	0.065
579	Painters, construction and maintenance	2.449	-0.015
583	Paperhangers	2.661	-0.013
584	Plasterers	2.589	0.043
585	Plumbers, pipe fitters, and steamfitters	2.771	0.065
588	Concrete and cement workers	2.484	-0.011
589	Glaziers	2.602	-0.125
593	Insulation workers	2.676	0.109
594	Paving, surfacing, and tamping equipment operators	2.506	0.003
595	Roofers and slaters	2.454	0.035
597	Structural metal workers	2.898	0.099
598	Drillers of earth	2.572	0.094
599	Misc. construction and related occupations	2.518	0.016
614	Drillers of oil wells	2.472	0.039
615	Explosives workers	2.645	-0.001
616	Miners	2.683	0.050
617	Other mining occupations	2.509	0.042
628	Production supervisors or foremen	2.716	0.081
634	Tool and die makers and die setters	2.785	0.053
637	Machinists	2.612	0.009

Notes: See notes for Table A.5.

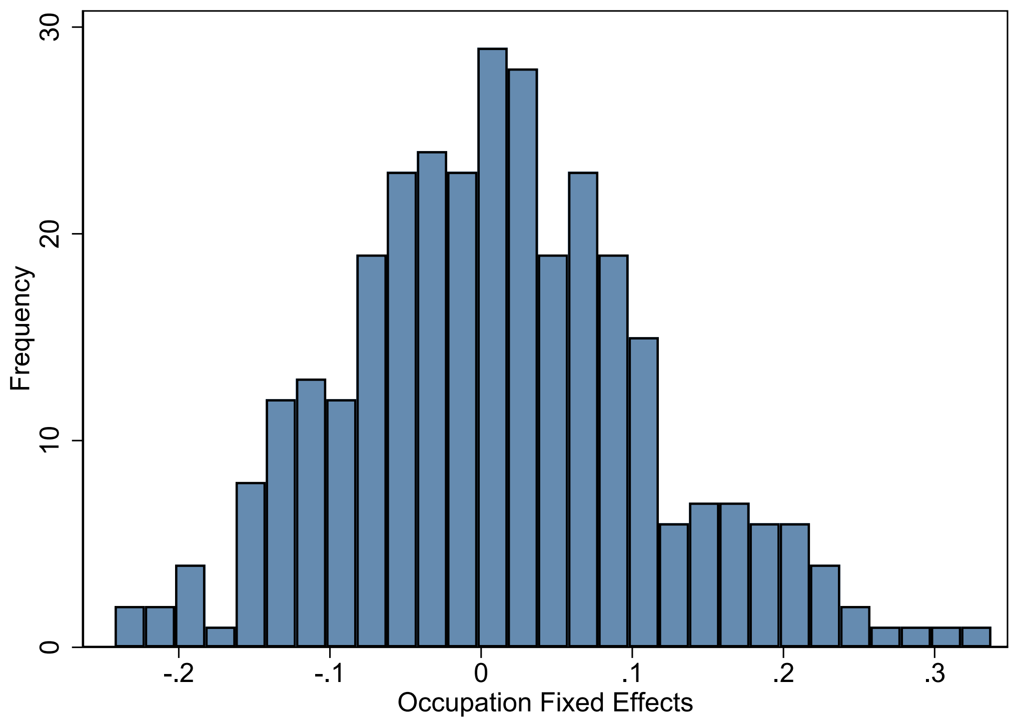
Table A.8: Statistics on Each Occupation, Part 4

Code	Occupation Title	Average Wage	Fixed Effect
643	Boilermakers	2.827	0.109
644	Precision grinders and fitters	2.573	-0.225
645	Patternmakers and model makers	2.739	-0.037
649	Engravers	2.561	-0.093
653	Other metal and plastic workers	2.700	0.028
657	Cabinetmakers and bench carpeters	2.413	-0.105
658	Furniture/wood finishers, other prec. wood workers	2.232	-0.092
666	Dressmakers, seamstresses, and tailors	2.198	-0.098
668	Upholsterers	2.560	0.053
669	Shoemakers, other prec. apparel and fabric workers	2.513	0.056
675	Hand molders and shapers, except jewelers	2.253	-0.019
677	Optical goods workers	2.390	-0.029
678	Dental laboratory and medical applicance technicians	2.467	-0.001
679	Bookbinders	2.294	-0.097
686	Butchers and meat cutters	2.320	-0.073
687	Bakers	2.216	-0.146
688	Batch food makers	2.198	-0.147
694	Water and sewage treatment plant operators	2.540	-0.084
695	Power plant operators	3.160	0.323
696	Plant and system operators, stationary engineers	2.778	0.075
699	Other plant and system operators	2.822	0.234
703	Lathe, milling, and turning machine operatives	2.427	-0.027
706	Punching and stamping press operatives	2.429	-0.066
707	Rollers, roll hands, and finishers of metal	2.290	-0.006
708	Drilling and boring machine operators	2.592	0.018
709	Grinding, abrading, buffing, and polishing workers	2.376	-0.011
713	Forge and hammer operators	2.437	-0.043
719	Molders and casting machine operators	2.370	-0.046
723	Metal platers	2.374	-0.036
724	Heat treating equipment operators	2.563	-0.046
727	Sawing machine operators and sawyers	2.263	-0.027
729	Nail, tacking, shaping and joining mach ops (wood)	2.395	-0.003
736	Typesetters and compositors	2.416	-0.083
738	Winding and twisting textile and apparel operatives	2.200	-0.059
739	Knitters, loopers, and toppers textile operatives	2.176	-0.030
743	Textile cutting and dyeing machine operators	2.288	-0.015
744	Textile sewing machine operators	2.040	-0.114
745	Shoemaking machine operators	2.068	0.003
747	Clothing pressing machine operators	2.022	-0.160
753	Cementing and gluing machne operators	2.066	-0.070
754	Packers, fillers, and wrappers	2.240	-0.065
755	Extruding and forming machine operators	2.337	-0.058
756	Mixing and blending machine operators	2.447	-0.002
757	Separating, filtering, and clarifying machine operators	2.736	0.151
763	Food roasting and baking machine operators	2.436	-0.120
764	Washing, cleaning, and pickling machine operators	2.450	0.003
765	Paper folding machine operators	2.407	-0.127
766	Furnance, kiln, and oven operators, apart from food	2.542	0.060
769	Slicing, cutting, crushing and grinding machine	2.296	-0.055
774	Photographic process workers t	2.302	-0.035
779	Machine operators, n.e.c.	2.393	0.012
783	Welders, solderers, and metal cutters	2.585	0.014
785	Assemblers of electrical equipment	2.332	-0.055
789	Painting and decoration occupations	2.419	-0.016
799	Production checkers, graders, and sorters in	2.453	-0.010
803	Supervisors of motor vehicle transportation	2.636	0.033
804	Truck, delivery, and tractor drivers	2.418	-0.056
808	Bus drivers	2.371	-0.050
809	Taxi cab drivers and chauffeurs	2.317	-0.134
813	Parking lot attendants	2.168	-0.207
823	Railroad conductors and yardmasters	2.830	0.119
824	Locomotive operators: engineers and firemen	2.845	0.179
825	Railroad brake, coupler, and switch operators	2.778	0.236
829	Ship crews and marine engineers	2.594	-0.021
834	Miscellanious transportation occupations	2.851	0.118
844	Operating engineers of construction equipment	2.675	0.030
848	Crane, derrick, winch, hoist, longshore operators	2.609	0.056
853	Excavating and loading machine operators	2.564	0.024
859	Stevedores and misc. material moving occupations	2.551	0.006
865	Helpers, constructions	2.289	-0.054
866	Helpers, surveyors	2.283	-0.048
869	Construction laborers	2.453	-0.035
873	Production helpers	2.286	-0.111
875	Garbage and recyclable material collectors	2.438	-0.094
878	Machine feeders and offbearers	2.237	-0.053
887	Vehicle washers and equipment cleaners	2.186	-0.094
888	Packers and packagers by hand	2.146	-0.107
889	Laborers, freight, stock, and material handlers, n.e.c.	2.288	-0.091

Notes: See notes for Table A.5.

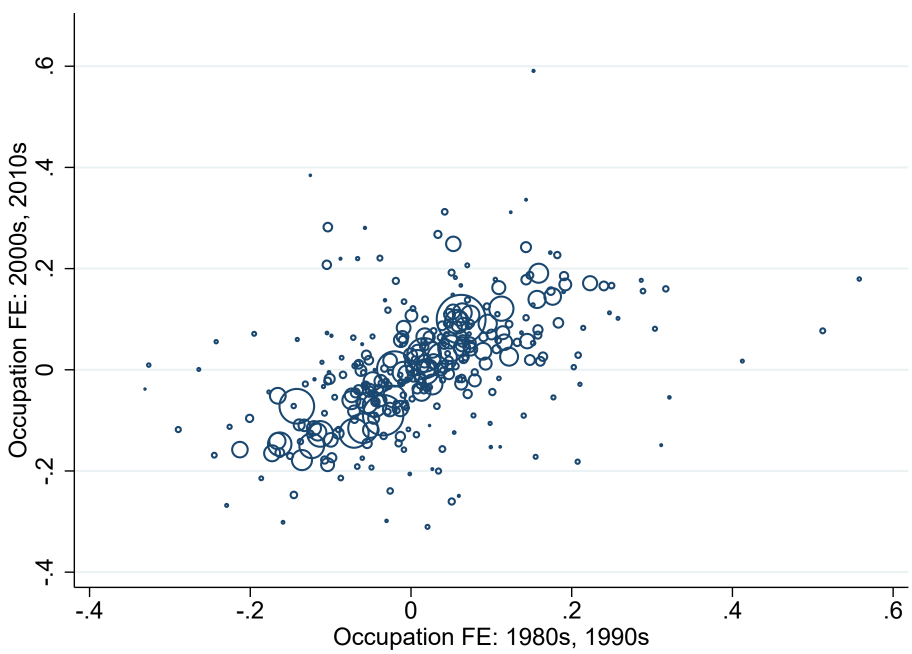
B Additional Figures

Figure B.1: Distribution of the Occupations Fixed Effects



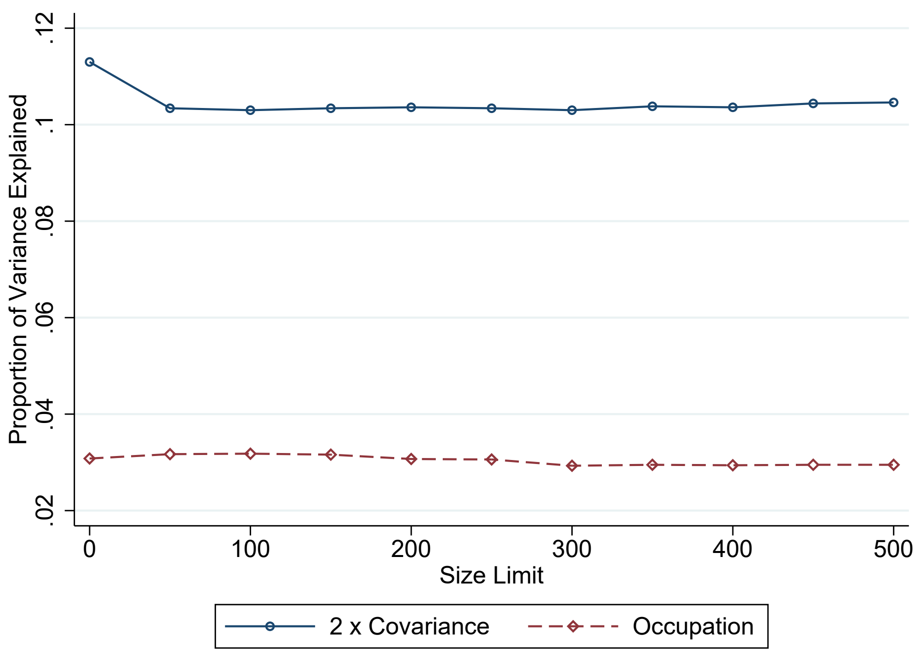
Notes: This histogram shows the distribution of occupational fixed effects. There is one observation per occupation, regardless of occupation size.

Figure B.2: How Stable are Occupations' Fixed Effects? All Occupations



Notes: Occupational fixed effects are calculated using Equation 2 two times: once including all observations through 1999 (as shown on the x-axis), and once including all observations in 2000 and later (y-axis). All occupations are included. Symbol size is proportional to occupation size.

Figure B.3: Do Fixed Effects Vary When Dropping Small Occupations?



Notes: Each point plots variance of occupational fixed effects (red diamonds) and twice the covariance of that with individual fixed effects (blue circles) in an AKM regression that includes control variables. At each value along the x-axis, the analysis is restricted to occupations for which our sample includes at least that many observations as the origin occupation, and also that many observations as the destination occupation.

C Validity of AKM Assumptions

In this section, we provide detailed results for the tests of the assumptions underlying the AKM analysis.

C.1 Tests from Prior Literature

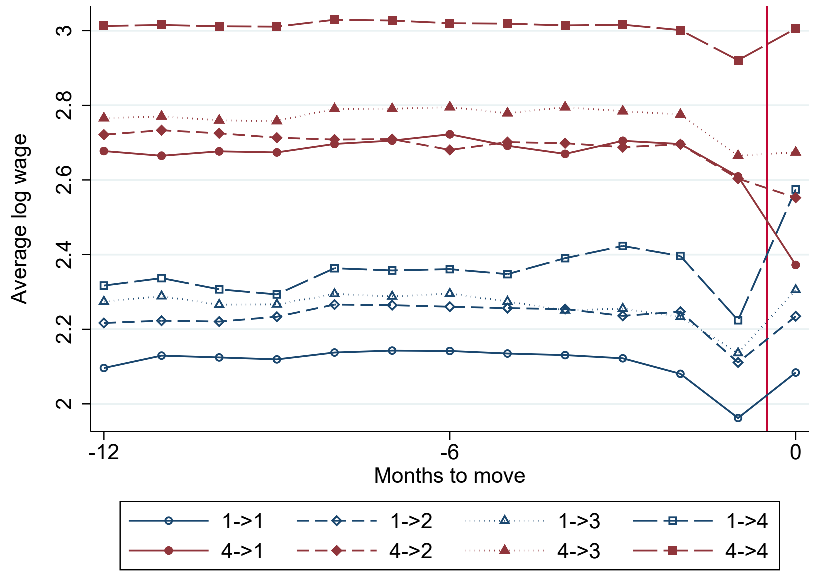
Pre-trends. Figure C.1 (based on a test in Card et al. (2013)) displays the average log wage trends for individuals across different occupation quartiles in the 12 months leading up to an occupational move. Post-move log wage, plotted at time 0, is the average wage at the destination occupation across the entire employment spell. These quartiles categorize occupations based on the average log wages, with the first quartile representing the lowest-paying occupations and the fourth quartile representing the highest-paying ones. Each line represents transitions between quartiles, with filled-in symbols for downward moves and hollow symbols for upward moves. The trends generally remain stable, indicating no significant differential pre-trends.

Post-trends. Figure C.2 shows the average log wage trends for the 12 months following an occupational move. Pre-move log wage, plotted at time -1, is the average wage at the origin occupation across the entire employment spell. Post-move trends also remain stable, suggesting no significant differential post-trends across quartiles. Although there are noticeable changes in wages immediately before and after the move, these are likely temporary adjustments related to the transition and are therefore excluded from the computation of the average wage for an occupation.

Symmetry. Figure C.3 (based on a test in Card et al. (2013)) shows the average log wage changes for individuals who either move up or down between quartiles before and after the occupational move. In general, the changes in log wages appear to be relatively symmetric for both upward and downward movers. In Card et al. (2013)), the equivalent of Figures C.1, C.2, and C.3 are together in one graph. We separate them out to increase sample sizes: many moves we observe to not include 12 months of pre-move wages and also 12 months of post-move wages.

Residual errors. Figure C.4 (based on a test in Card et al. (2013)) plots the actual versus predicted log wages across deciles of occupational fixed effects. The figure shows that the actual and predicted wages align closely across all decile pairs, indicating that the residual errors do not systematically vary with the level of occupational fixed effects. Figure C.5 (based on a test in Card et al. (2024)) plots the change in residuals against the change in occupational fixed effects

Figure C.1: No Differential Pretrends



Notes: Occupations are divided into four quartiles on the basis of their average log wage. For selected origin-destination pairs of quartiles, we plot the average log wage of everyone who makes such a move, in the 12 months before the move, among all movers who have data for all 12 months. Post-move log wage, plotted at time 0, is the average wage at the destination occupation.

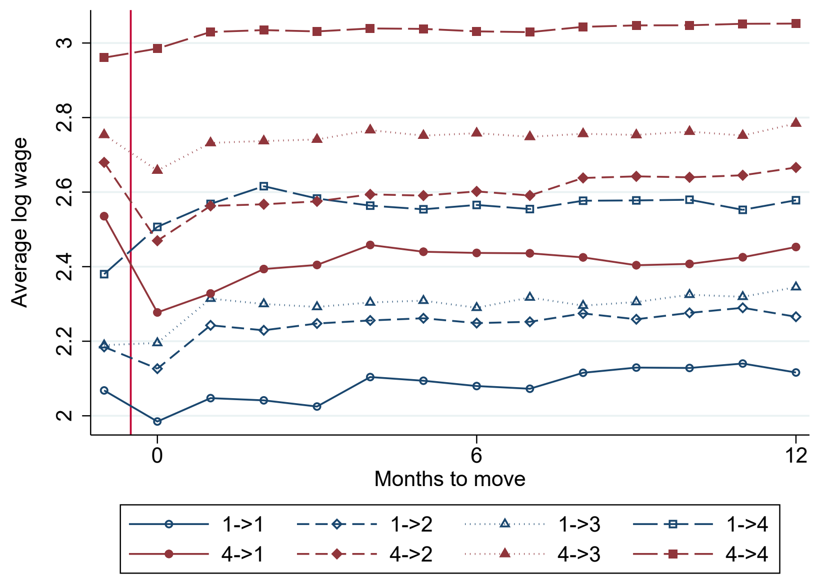
for individuals who switch occupations. The lack of a clear pattern or systematic relationship suggests that changes in the residuals are not correlated with changes in occupational fixed effects, supporting the assumption that up-movers and down-movers have similar error terms.

C.2 Out-of-Sample Test

A key assumption in our analysis is that fixed effects represent the effect of changing occupation on wages among those for whom these occupations are in their choice set. However, note that the difference in fixed effect between any pair of occupations A and C is based not just on moves between A and C, but also on moves from A to another occupation B, plus moves from B to C. In order to argue that the fixed effect difference estimates how much workers' wages would change if they were able to make a move, it must be that fixed effect differences predict changes in wages out of sample.

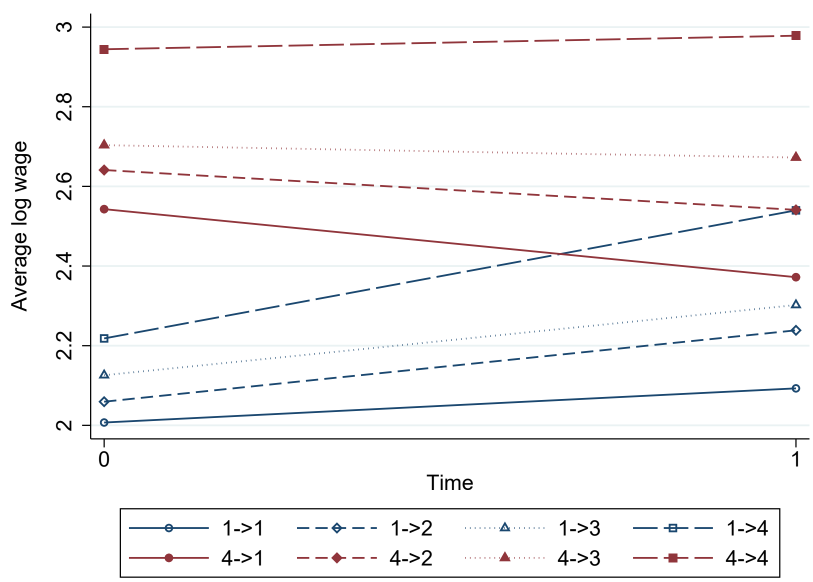
Figure C.6 illustrates our approach using three occupations: janitors, truck drivers, and sales supervisors. Among workers who are, at different times, both janitors and truck drivers, their log wage averages .078 higher as truck drivers; among those who are both truck drivers

Figure C.2: No Differential Posttrends



Notes: Occupations are divided into four quartiles on the basis of their average log wage. For selected origin-destination pairs of quartiles, we plot the average log wage of everyone who makes such a move, in the 12 months after the move, among all movers who have data for all 12 months. Pre-move log wage, plotted at time -1, is the average wage at the origin occupation.

Figure C.3: Symmetry Between Upward and Downward Movers

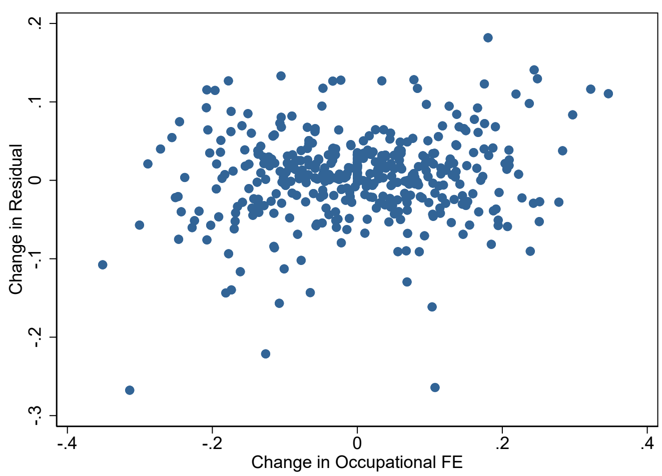


Notes: Occupations are divided into four quartiles on the basis of their average log wage. For selected origin-destination pairs of quartiles, we plot the average log wage at the origin (in time 0) and average log wage at the destination (in time 1).

Figure C.4: Estimated Errors

Notes: Individuals are divided into 10 deciles based on individual fixed effect, and occupations are divided into 10 deciles based on occupational fixed effect. The graph shows one point for each of the 100 decile pairs. The x-axis shows the ordinal occupational fixed effect, with 1 indicating occupations with the lowest fixed effect and 10 the highest. The y-axis for solid blue circles shows the average log wage within each decile pair; the y-axis for empty red diamonds shows the predicted log wage, if wage was equal to the sum of individual and occupational fixed effects.

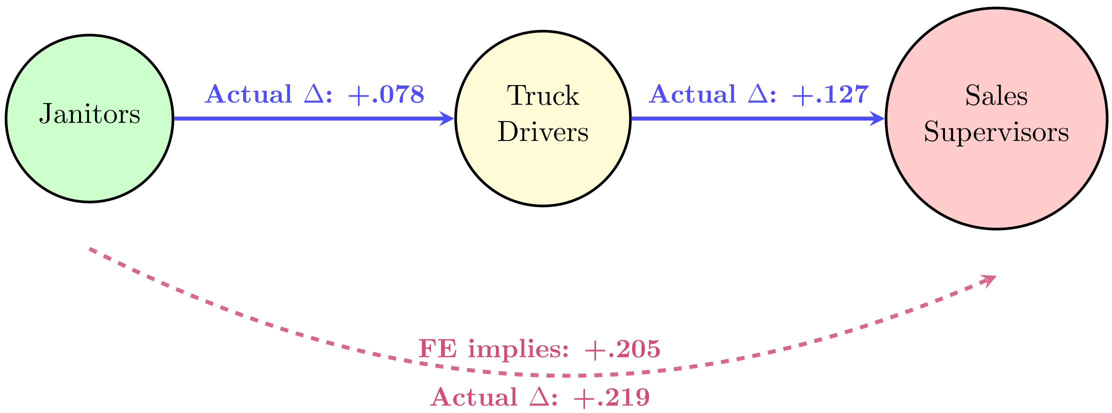
Figure C.5: Error Terms for Upward and Downward Movers



Notes: Occupations are divided into 20 ventiles based on their occupational fixed effect. For each origin-destination pair of occupational ventiles, the x-axis shows the difference between the destination ventile's average fixed effect and the origin ventile's average fixed effect. The y-axis shows the difference between the average AKM residual among workers in the destination ventile minus residual among those in the origin ventile.

and sales supervisors, their log wage averages .127 higher as sales supervisors. Therefore, if we estimated Equation 2 using only these movers, we would find that the fixed effect for sales supervisors is .205 (=.078+.127) higher than that for janitors. In fact, those who have both jobs have average log wages a similar .219 higher as sales supervisors. Therefore, for this triplet, estimates of Equation 2 predict out-of-sample wage differences well.

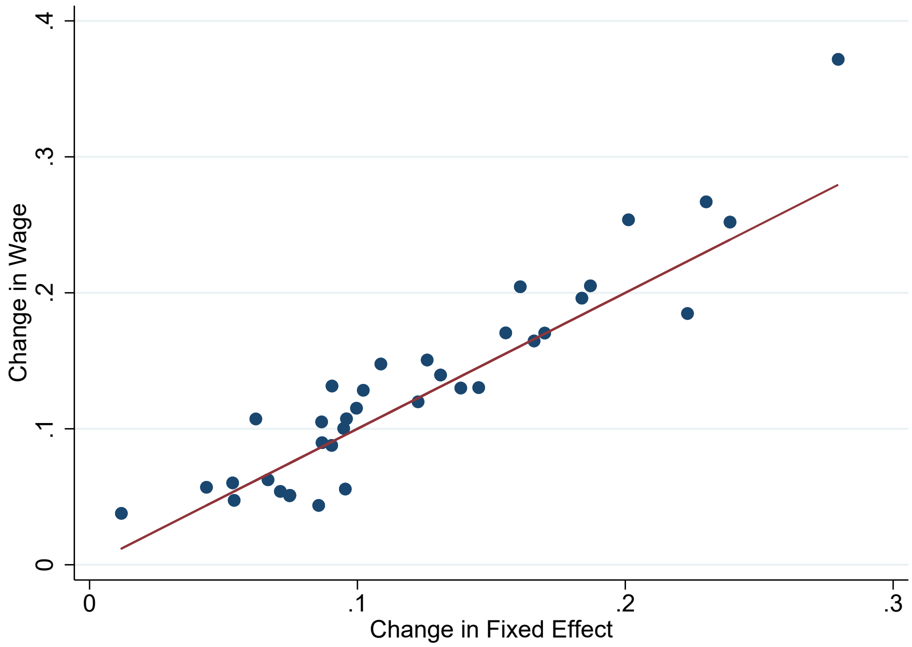
Figure C.6: Intuition for the Out-of-Sample Test



Notes: This figure shows the difference in log wage among those who held two of the listed occupations: those who were both janitors and truck drivers had log wages .078 higher as truck drivers; those who were truck drivers and sales supervisors had .127 higher as sales supervisors; and those who were both janitors and sales supervisors had .219 higher as sales supervisors. The first two differences imply that an AKM analysis involving only the first two types of moves would estimate a fixed effect difference between sales supervisors and janitors of .205.

To make this test systematic, we extend it to all occupations in the data. We first divide all occupations into deciles by their estimated fixed effect. For every pair of deciles with at least one decile between them (for example, deciles 1 and 3, but not deciles 1 and 2), new occupational fixed effects are calculated using Equation 2 including only observations of occupations between those deciles—including observations of those deciles, but not for workers who are observed at both the top and bottom endpoint. Then, for workers who are observed at both the top and bottom endpoints, we calculate the change in this newly-calculated fixed effect, along with the actual change in their wage. The change in fixed effect and change in wage are plotted in Figure C.7; points lie mostly along the 45-degree line, and a the 95% confidence interval of regression on the points cannot rule out a slope of 1.

Figure C.7: Do Fixed Effects Predict Wage Changes?



Notes: Occupations are split into 10 deciles by occupational fixed effect. Each point represents a pair of deciles with at least one decile in between. For each such pair, new fixed effects are calculated that include all occupations between this pair, including the endpoints but not workers who move between the endpoints. The x-axis plots the average change in the newly calculated fixed effects for those moving from the lowest to the highest decile in the pair (and the negative of that for those moving in the opposite direction); the y-axis plots the change in average wage for the same group. The red line is the 45-degree line.

D Within-Interview Fixed Effects

As discussed in the main text, the literature has found that—in survey data like the SIPP—many occupation moves are spurious. In our main specification, we address this by only considering occupation changes that occur at the same time as employer changes. However, this has two possible drawbacks. First, occupation moves within a firm may be different from those occurring between firms, leading to different fixed effects. Second, some occupation moves that occur at the same time as employer moves may still be spurious, leading to noise in the estimation. To address both points, we introduce a new sample on which we will estimate fixed effects using Equation 2. This sample is motivated by noting that each time someone is interviewed for the SIPP, they report their work history over the previous 4 months. Thus, months 1 through 4 in the data are from one interview, months 5 through 8 on the next, and so on. If a respondent reports two different occupations within the same interview, that change is unlikely to be spurious.

In theory, we could use any within-interview change in occupation to help estimate fixed effects. However, recall that the first and last month in a job often see lower wages than other months. In order to estimate wage changes completely within the same survey, our within-interview sample therefore restricts attention to interviews that begin in month t where the person works at the same occupation A in month t and $t+1$, switches to a new occupation B in month $t+2$, and stays at occupation B in $t+3$; wages are measured in months t and $t+3$ only. This greatly limits our sample: only moves with no break in employment are included, and only one quarter of such moves will happen to occur after the second month of an interview. After adding our usual sample restrictions on age, hours worked, and outlier wages, we have 3,466 pairs of observations where the same individual is observed at two occupations in the same interview. This is an order of magnitude smaller than our usual employer-switcher sample, which is why we prefer that sample in our main analysis. Once we estimate Equation 2 with the Kline et al. (2020) bias correction, the within-interview occupation fixed effects explain 2.5% of total log wage variation—similar to our baseline results. The Kline et al. (2020) correction is particularly important here since there is more scope in this smaller sample than in our usual estimates for an elevated variance due to limited mobility bias.

E Cleaning Procedure

Occupations. Because occupational classifications change through time, occupations are harmonized and some are merged using a crosswalk mapping various occupational classifications to consistent occupational codes using data from Autor and Dorn (2013).[12] To integrate the O*NET variables, a comprehensive crosswalk was created to link the 1990 Census classification[13] with the Standard Occupational Classification (SOC) and O*NET codes.[14]

*O*NET variables.* In our final occupational classification, some occupations were merged to ensure consistency. When a single occupation, as defined by the Autor and Dorn (2013) classification, corresponded to multiple occupations in the O*NET framework, the O*NET variables were averaged across the corresponding occupations to create a unified representation.

Demographic variables. Demographic variables are harmonized using a standardized coding scheme. Education levels are categorized into two groups: high school or less, and college or more, based on respondents' highest reported educational attainment at the time of the questionnaire. Racial and ethnic classifications are consolidated into four mutually exclusive groups—White (non-Hispanic), Black (non-Hispanic), Hispanic, and Other—using reported race and ethnicity identifiers. For the wage gap analysis in Section 3.5, we further aggregate these into two groups, comparing White non-Hispanic workers to Black and Hispanic workers.

Individual weights. Individual weights are used as provided in the SIPP data to reflect the population-level representation of each observation. These weights are normalized where necessary, such as when computing occupation-level averages or aggregating across spells, to avoid double-counting individuals in repeated observations. For specific analyses, like the estimation of occupational fixed effects, adjusted weights are applied to observations from the second period of employment spells after a change in employer.

Calculating wages. Wages are calculated by dividing monthly earnings from the primary occupation by the product of weekly hours worked and the average number of weeks per month (4.345). Adjustments for inflation are made using the Consumer Price Index, with 2002 as the base year, resulting in inflation-adjusted wages. The average wage for a given employment spell is calculated over the entire spell, excluding the first and last months to avoid temporary fluctuations.

[12]Available at https://www.ddorn.net/data.htm#Occupation%20Codes.

[13]Available at https://www2.census.gov/programs-surveys/demo/guidance/industry-occupation/.

[14]Available at https://www.onetcenter.org/taxonomy/2019/soc/.

Employment spells. Employment spells are defined as continuous periods of employment with a single employer, identified using unique employer identifiers. A new spell begins whenever an individual switches employers. If an individual has multiple occupations within a spell, the spell's last occupation (leaving out the final month of data) is used. To avoid overcounting transitions, gaps between periods of employment with the same employer—whether due to temporary unemployment or unavailability to respond to the survey—are treated as part of the same continuous spell, provided the individual returned to the same employer or occupation.

Final selection. The final estimation sample is built from a harmonized panel of the Survey of Income and Program Participation (SIPP), including panels from 1984 and 2008, covering individuals observed between the ages of 25 and 65. Our main specification defines an occupational switch as one that coincides with a change of employer; we therefore retain only individuals who are observed changing employers at least once during the panel. Each employment spell begins when an individual starts working for a new employer, and we retain all spells for these individuals, including those where the worker does not change occupation. Within each spell, we restrict to full-time work by requiring an average of at least 20 hours worked per week and exclude spells in which the average hourly wage falls below the inflation-adjusted federal minimum wage. Observations with wages above the 99th percentile and below the 1st percentile are dropped. The resulting dataset comprises 184,987 individual-employer spell observations, corresponding to 74,658 unique individuals.